Norma R. A. Romm

Responsible Research Practice

Revisiting Transformative Paradigm in Social Research

Springer

Responsible Research Practice

Norma R. A. Romm

Responsible Research Practice

Revisiting Transformative Paradigm in Social Research

Springer

Norma R. A. Romm
Department of Adult Education
and Youth Development
University of South Africa
Pretoria
South Africa

ISBN 978-3-319-74384-4 ISBN 978-3-319-74386-8 (eBook)
https://doi.org/10.1007/978-3-319-74386-8

Library of Congress Control Number: 2018932178

© Springer International Publishing AG, part of Springer Nature 2018
This work is subject to copyright. All rights are reserved by the Publisher, whether the whole or part of the material is concerned, specifically the rights of translation, reprinting, reuse of illustrations, recitation, broadcasting, reproduction on microfilms or in any other physical way, and transmission or information storage and retrieval, electronic adaptation, computer software, or by similar or dissimilar methodology now known or hereafter developed.
The use of general descriptive names, registered names, trademarks, service marks, etc. in this publication does not imply, even in the absence of a specific statement, that such names are exempt from the relevant protective laws and regulations and therefore free for general use.
The publisher, the authors and the editors are safe to assume that the advice and information in this book are believed to be true and accurate at the date of publication. Neither the publisher nor the authors or the editors give a warranty, express or implied, with respect to the material contained herein or for any errors or omissions that may have been made. The publisher remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.

Printed on acid-free paper

This Springer imprint is published by the registered company Springer International Publishing AG
part of Springer Nature
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

Endorsement

Norma Romm provides excellent guidance through theory and practical examples for researchers who accept the challenge of working towards social, economic, and environmental justice for members of marginalized communities. She brings together the work of transformative and indigenous scholars to further understandings of the complex dynamics of contributing to positive social change through the use of innovative research strategies. The examples illustrate an emphasis on addressing social and ecological justice, along with the challenges that researchers encounter in this type of research. This book is an excellent contribution to understanding better how researchers can be responsive to the wicked problems facing the world.

—Donna M. Mertens, Ph.D., Professor Emeritus
Gallaudet University

Foreword

The book *Responsible Research Practice: Revisiting Transformative Paradigm in Social Research* articulates a new and diversified direction to research which uniquely shifts from the dominant ways of doing research, while drawing on and inflecting Mertens' exposition (e.g., 1999, 2009, 2014, 2016) of the transformative paradigm. It embraces the incorporation of an Indigenous lens to research in which care, relationality, and accountability are seen as cardinal. In working with and extending existing debates about some of the epistemic considerations relating to Indigenous world views as articulated by certain Indigenous scholars (e.g., Chilisa 2009, 2012; Kovach 2009; Murove 2005; Smith 1999), Romm recasts these and challenges researchers to further ruminate the principles of Relationship, Respect, Reciprocity, and Responsibility (4 Rs) in research design. In this way, the book not only advances transformative-directed research but extends the meaning and practice of it by forwarding suggestions for ways of actively exercising responsibility in research.

The African *Ubuntu* principle enunciated by Romm and research participants as set out in Chap. 2 offers suitable parameters for reshaping/rearticulating the discourse of race and class relationships with a consciousness that emerges from people affected by social marginalization. This creates the ambience to debate socio-colonially constructed forms of dehumanization arising from the intersection of racism, classism, and use of cheap labor (capitalist exploitation). The book showcases via various examples, which are examined in depth across the book, how research can contribute to nurturing alternative principles for humans relating to each other and to all that exists in the web of life, where research can contribute to transforming social rifts and deep emotional/psychological wounds, as well as facilitating natural resource management and ecological sustainability.

The book advocates for transformative researchers, academics, community-engaged practitioners, and social/environmental activists to step outside their comfort space and reexamine their practices insofar as knowledge production, validation, and distribution/dissemination pertain. It recognizes that researchers and

professional research communities have particular orientations which, if not carefully reflected upon, can mean that the impact that research might have on the social and ecological world becomes neglected. To safeguard against this, Romm, referring to the work of Gergen (1978, 2015), examines possibilities for generative research and theorizing in which the metaphor of research as *shaping* replaces the metaphor of research as *watching*. In so doing, the main theme of the book challenges all those involved in research to take more responsibility for the shaping effects of research. This is underscored by Romm's detailed rendition of examples of research in various geographical contexts across the globe in which active ways of doing research are said (at least according to the Romm's account/extrapolation) to have been used to shape in cooperation with participants (as coresearchers) possibilities for empowerment. Also, Romm's reexamination of the autobiographical life story of an environmental feminist in Chap. 6 brings to bear the parameter for remedial action by those who would otherwise feel less empowered to become leading agents of change in their society on ecologically related issues.

I found particularly insightful Romm's discussion on ethics in Chap. 8, where she takes on and reconfigures the principles of the Belmont report of 1979. She explores with sensitivity issues such as giving incentives and rewards to research participants, where this is not seen as compromising the research agenda. She also maintains that the Belmont report does not sufficiently cater for an appreciation of the contention around professional researchers seeking knowledge production with reference to the label of "doing science", which she argues can become problematic (when the *watching* metaphor is invoked). She suggests that the Belmont report, which has become the benchmark of most Institutional Research Ethic Boards (IRBs) across the world, needs careful reconsideration in the light of alternative ethical positions where ethics and epistemology are seen as inextricably connected. She supplies practical examples of ways of navigating these alternative positions.

Lastly, by reiterating the use of multiple theoretical and methodological approaches to inform and empower researchers and coresearchers, some of the discourses in the multiple and mixed methods research (MMMR) literature are discussed along with paradigmatic extrapolation that responsibly feeds into both conventional and nonconventional (Indigenous) research methods (see, e.g., Chaps. 1 and 9). And by foregrounding spirituality in the discussion, which she crafts with reference to interdisciplinary fields of study (such as philosophy of science, sociology, Indigenous orality, and education), Romm engages in the politics of disrupting the conventional approach to research and knowledge construction that traditionally excluded the spiritual dimension. In articulating the spiritual, Romm extends the debates on connectedness in terms of fostering diverse relationships such as human to human, human to animal, and human/animal to the environment—living and nonliving things (cf. Smith 1999; Wane et al. 2014) as operationalized by many Indigenous societies globally.

In my view, Romm has managed to meticulously infer from different examples of research conducted in diverse contexts, ways of doing transformative, empowering and responsible research for a world-shaping agenda. The book is an

empowering and transformative tool that will reverberate with academics, researchers, social and environmental activists, and civil society bodies in their continuous endeavor to make the world a better place.

Pretoria, South Africa
February 2017

Francis Adyanga Akena, Ph.D.

References

The Belmont Report: Ethical principles and guidelines for the protection of human subjects of research (1979). USA: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research.

Chilisa, B. (2009). Indigenous African-centred ethics: Contesting and complementing dominant models. In D. M. Mertens & P. E. Ginsberg (eds.), *The handbook of social research ethics* (pp. 407–426). London: Sage.

Chilisa, B. (2012). *Indigenous zesearch methodologies*. London: Sage.

Gergen, K. J. (1978). Toward generative theory. *Journal of Personality and Social Psychology, 36*, 1334–1360.

Gergen, K. J. (2015). From mirroring to world-making: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3): 287–310.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts.* Toronto: University of Toronto Press.

Mertens, D. M. (1999). Inclusive evaluation: Implications of transformative theory for evaluation. *American Journal of Evaluation, 20*(1):1–14.

Mertens, D. M. (2009). *Transformative research and evaluation*. New York: Guildford Press.

Mertens, D. M. (2014.) *Research and evaluation in education and psychology*. Thousand Oaks: Sage.

Mertens, D. M. (2016). Advancing social change in South Africa through transformative research. *South African Review of Sociology, 47*(1): 5–17.

Murove, M. F. (2005) *The theory of self-interest in modern economic discourse: A critical study in the light of African humanism and process philosophical anthropology*. Doctoral thesis, University of South Africa.

Smith, L. T. (1999). *Decolonizing methodologies: Research and Indigenous peoples*. New York: Zed books.

Wane, N. N., Akena, F. A. & Ilmi, A. (eds.) (2014). *Spiritual discourse in the academy: A globalized Indigenous perspective*. New York: Peter Lang.

Acknowledgements

In the first place, I wish to acknowledge with gratitude the University of South Africa, who offered me a research professor position starting in 2016, which enabled me to finalize the writing of this book. I also wish to express profuse thanks to the many people who contributed to this book in different ways (here laid out in alphabetical order):

- Francis Adyanga Akena (who later wrote the Foreword to this book) was a critical reader who read the book during its draft construction and contributed input along the way, where he located gaps (which I incorporated into the text and acknowledged as "personal communication"). He also helped me to hone certain sections of chapters which he felt were not sufficiently sharp in their rendition.
- Antonio Roman-Alcalá and I took part in a number of email exchanges regarding my rendition of his insider account of the "Occupy The Farm" event at the University of California at Berkeley (as a case study), after I sent him the relevant part of Chap. 6. He added some further analytic interpretations to the chapter, which became included in the text (again cited as personal communication).
- Catherine Chesterton organized the Table of Contents for me, which was very helpful.
- Moya Joubert professionally edited certain chapters, along with the reference list. I am very grateful for the conscientiousness and support which she displayed.
- Janet McIntyre-Mills read and contributed to clarifying various drafts of Chap. 6 in the making, including the sections where I offered an account of her research work initiated with the South Australian Local Government Association. In face-to-face and email conversation she furnished constructive comments, which became incorporated in the text as personal communication. She also read other chapters and endorsed that in her view, the book would be well received, and could function as a valuable resource.

- Veronica McKay inputted substantially (via track changes) into Chap. 3, especially with respect to the South African national project which I showcased therein. She helped to foreground in what sense the research can be said to have been "active". I have labeled her input in the text as "personal communication via email". Additionally, Veronica discussed with me how I might incorporate the suggestions of an anonymous reviewer of the book (and I also thank this reviewer for taking the care to offer thoughtful commentary and suggestions).
- Martin Mendelsohn (my partner and additional critical reader) looked painstakingly at Chaps. 1, 2, 7, 8, and 10, and assisted me to assess where more focus should be put in the arguments presented and how to better express them. He also applied his intellectual energies in considering how the tables could be better constructed. His input was invaluable.
- Donna Mertens allowed me to use in Chap. 8 some of my notes regarding her answer to one of the questions that I had asked of her at a workshop which she facilitated (in Durham, UK, 2016) on (mixed methods) research and social transformation. She expressed support for my endeavor to continue to think and write about these issues.
- Gerald Midgley kindly read the parts of Chap. 1 where I asked him for clarity on certain aspects of a project conducted under the auspices of the Institute of Environmental Science and Research (ESR) in New Zealand; and he clarified for me how the team of researchers from the ESR involved themselves with the community.
- Sarah Ssali took care to read and edit the section of Chap. 4 which I sent to her, in which her work with Sally Theobald in Northern Uganda vis ā vis post-conflict health reconstruction is showcased. She added some clarifying statements for the benefit of the chapter (also acknowledged in the text as personal communication).
- Oluyemi Stephens spent two long afternoons with me while we explored together his sense of responsibility in setting up the experiment in two Nigerian prisons, which is showcased in Chap. 5. He explained to me with passion how he viewed his involvement in this experiment. This became the basis of my account in the book of various aspects of this experiment, where I extrapolated it as being an example of responsible research experimentation (which I cited as based on our face to face conversations).
- Hendrikje Tuerlings, as assistant editor at Springer, was extremely helpful in always answering all my queries (and offering advice) and in organizing the process of publishing the book (right from the beginning of arranging the contract).
- Belete Woldegies read carefully the part of Chap. 4 which I sent to him in which my rendition of the research which he initiated with women in Ethiopia and their supporting families appears. He edited some parts to lend clarity to my wording, taking care to modify wording to better express certain concepts.

Thank you all for taking the effort that you did in various ways to offer support for this project!

Contents

1 Introduction to the Book: Activating Transformative Intent in Consideration of the Immersion of Research in Social and Ecological Existence 1
1.1 Introduction 1
1.1.1 Introducing Myself (An "Academic" Introduction) 1
1.1.2 Some Considerations Around the Transformative Paradigm 4
1.2 Central Concepts Associated with Positivist/Postpositivist, Constructivist, Transformative, and Pragmatic Research Paradigms 11
1.2.1 Mertens' Explication of the Transformative Paradigm in Relation to Alternatives 12
1.3 Reconsidering Our Language When Speaking About Research 17
1.3.1 Ontology: Web of Connections 18
1.3.2 Epistemology as Social Relations of Knowing: Self-in-Relation 21
1.3.3 Axiology: Pursuing an Inclusive Wellbeing 24
1.3.4 Methodology: Reflecting Responsibly on Research Design 25
1.3.5 Revisiting Boundaries Between Paradigms and Creating Space for Additions 27
1.3.6 Permeable Boundaries Between Paradigms 32
References 37

2 Active Focus Group Research with Follow-up Interviews/ Conversations and Actions: Responsibly Re-exploring Race(d) and Classed Relations 45
2.1 Introduction 45
2.2 A Personal Narrative: Being (Defined as) White in South Africa 47

2.2.1 Being White and Jewish in South Africa 51
2.2.2 (Different) Learning Through Lectures and Books at University (and Viewing the 1976 Soweto Uprising on TV) Plus Further Developments 52
2.3 Researching Raced and Classed Relations in Post-apartheid South Africa 55
2.3.1 Opening a (Cross-Racial) Conversation About Race and Racism in Post-apartheid South Africa 56
2.3.2 The Focus Group Meeting 60
2.4 Storying in Relation to the One-to-One Conversation with Lindiwe (Concerning Her Expression of Work-Related Difficulties) 63
2.5 Storying in Relation to Race, Status, and Class Positioning: Exploring Possibilities for Action 68
2.6 Further Storying: One-to-One Interview/Conversations with Koos and Lynette 73
2.6.1 Interview/Conversation with Koos (2007) 75
2.6.2 Interview/Conversation with Lynette (2007) 81
2.6.3 A Brief Note on Classed and Not Only Raced Relations: Black Poor People Often Devalued 85
2.7 Judging the Validity of This Research: Rethinking Validity 89
2.8 A Concluding Note 92
References 93

3 Active Use of Questionnaires Combined with Focus Group Facilitation: Responsibly Researching Options for Generating Educational Inclusivity 97
3.1 Introduction 97
3.2 Inclusive Education: A Brief Account (Contextual Background to in-Depth Discussion of Research Exemplars) 106
3.3 Some Detail on the Comparative International Project 109
3.3.1 How We Worked with the "Findings" in a Creative and Active Way (and How This Was Perceived by Participants) 112
3.4 Some Detail on the National 500 Schools Project 123
3.4.1 Designing the Questionnaire: Taking into Account Its Transformative Potential 124
3.4.2 Some Reflections on the Setting of the Final Question 127
3.4.3 Responses to Our Final Question 128
3.4.4 The Design of the Focus Group Sessions 131

3.4.5 Guiding Questions for the Four Sets of Participants (Per Sampled Schools) 132
3.4.6 Intervention Handbook and Intervention Visits 135
3.5 Summary of Transformative Research Intentions 144
3.6 Conclusion: Additional Post Facto Reflections 147
3.6.1 Questionnaires as Tools for Enabling Processes of Re-Reflecting upon Constructions 147
3.6.2 Enabling, but not Imposing, Processes of Re-Reflecting upon "Everyday" Theorizing 147
3.6.3 Crediting Otherwise Marginalized Views 149
3.6.4 Engaging with Care as a Research Ethic: Should This Be Regarded as "Universally" Applicable? 150
3.6.5 Revisiting Correlations and Attributions of Causality 151
3.6.6 Developing Emtic Relationships with a View to Action 152
3.6.7 Some Reflections on the Status of My Storying in This Chapter 152
References 154

4 Actively Facilitating Individual and Focus Group Narrations: Responsibly Foregrounding Gender Stereotyping While Stimulating (Contextual) Empowerment 159
4.1 Introduction 159
4.2 Economic Empowerment Through Income Generating Activities and Social Mobilization (Ethiopia) 161
4.2.1 Responsible Framing of Questions in the Interview Guide 163
4.2.2 Inviting Women and Supporting Family Members to Focus Group Discussions 171
4.2.3 (Responsible) Approach to Member Checking 176
4.2.4 Ethics of the Research 178
4.2.5 Responsible Involvement in Dissemination/Public Discussion 181
4.2.6 Validation and Generative Theorizing: Toward Transformation 183
4.3 Using Life Histories to Explore Gendered Experiences of Conflict in Gulu District, Northern Uganda: Implications for Post-conflict Health Reconstruction 186
4.3.1 The Interview Process: Ceding Power as Researchers While Listening to Participant Narrations 188

4.3.2 Analysis (and Attendant Recommendations): Highlighting Gendered Experiences While not Exacerbating Gender Stereotypes . . . 190
4.3.3 Dissemination to Stakeholders: Taking Some Responsibility . . . 192
4.3.4 Noting Challenges that Need Continued Discussion . . . 193
4.3.5 Ethical Approach and Defining the "Veracity" of Life Histories as Expressed . . . 194
4.3.6 Generative Theorizing . . . 195
4.4 Conclusion . . . 196
References . . . 199

5 Active Use of Experiments: Responsibly Inviting Participants and Others to Review Options for Agency . . . 203
5.1 Introduction . . . 203
5.2 Researching Effectiveness of Six Thinking Hats and Emotional Intelligence Training in Enhancing Creative Thinking and Emotional Intelligence Capacity Among Male Recidivists in Lagos Prisons, Nigeria . . . 208
5.2.1 The Set-up of the Experiment . . . 208
5.2.2 Procurement of Participants and Research Procedure . . . 208
5.2.3 The Training: Responsible Intervention . . . 211
5.2.4 Some Results of the Experiment: (Re)Interpretations . . . 221
5.2.5 Validation Reconsidered . . . 225
5.2.6 Approach to Ethics . . . 226
5.2.7 Generative Theorizing: Toward Transformation . . . 228
5.3 Debriefing in Deceptive Research: A Proposed New Procedure (Poland) . . . 232
5.3.1 Some Results of the Experiment: (Re)Interpretations . . . 235
5.3.2 Experiments as Potentially Educational . . . 238
5.3.3 Generative Theorizing: Toward Transformation . . . 239
5.4 Conclusion . . . 241
References . . . 244

6 Development-Oriented Research to Forward Social and Environmental Justice: Responsibly Strengthening Discourses and Actions Toward an Inclusive Wellbeing . . . 247
6.1 Introduction . . . 248
6.2 Deployment of Participatory Decision-Making Software (South Australia) . . . 252
6.2.1 The Research Design . . . 256

6.2.2 Ethical Approach: Taking Responsibility for Admittedly Values-Based Research 258
6.2.3 Results Interpreted in Terms of a Value-Based Starting Point: Taking Responsibility for the Potential Impact of Statements Made 262
6.2.4 Reporting: Taking Responsibility for Engendering Dialogue and Inspiring Re-creation of Caring Narratives 268
6.2.5 Validation 270
6.2.6 Generative Theorizing: Toward Transformation 273
6.3 A Case Study of Occupy the Farm (OTF) at the University of California at Berkeley 277
6.3.1 Purpose of the Write-up (As Expressed by Roman-Alcalá) 277
6.3.2 Research "Method": Interspersing Interviews and Conversations with Further Theorizing in the Construction of the Case Write-up 279
6.3.3 Ethical Position: Research-in-Action Toward "Just" Outcomes (Defined Locally, with Global Import) 282
6.3.4 Validation: Strengthening Possibilities for Creative Thought and Action 283
6.3.5 Generative Theorizing: Taking into Account Possible Ways of "Moving" Audiences 284
6.4 Unbowed: A Memoir by Maathai (Kenya) 288
6.4.1 Introduction: Some Reflections on Autoethnography 288
6.4.2 Maathai's Story of the Journey Setting up the GBM in Kenya 290
6.4.3 Ethical Position: Inquiry in Action into "What Can Be Done" 297
6.4.4 Validation: Appreciating an Author's Memory Work as Inspirational 298
6.4.5 Generative Theorizing: Theorizing Options for Empowerment in Practice 299
6.5 Conclusion 300
References 302

7 Responsible Generative Theorizing 307
7.1 Introduction 307
7.2 An Explication of Gergen's Account of Generative Theory and Theorizing 309

7.3 Theorizing Social Structure Toward Envisioning Possibilities for Reconstruction . . . 315
7.4 Retroductive/Abductive Logic as a Resource for Generative Theorizing . . . 322
7.4.1 Abductively Creating Analyses in Grounded Theory . . . 326
7.4.2 Abductive Inferencing as Potentially Linked to a Transformative Research Agenda . . . 335
7.4.3 Indigenous Epistemologies and Retroductive Logic . . . 338
7.5 Revisiting Imputations of Causality in Generative Theorizing . . . 341
7.5.1 Admitting a Plurality of Perspectives in the Imputation of Causality . . . 344
7.6 Conclusion . . . 351
References . . . 353

8 Practicing Ethical Responsibility: Reconfiguring the Belmont Model . . . 359
8.1 Introduction . . . 359
8.2 Reviewing and Reconfiguring the Belmont Report's Principle of Respect for Persons . . . 365
8.2.1 The Belmont Report's Exposition of the Principle of Respect for Persons and Its Application . . . 365
8.2.2 Further Deliberations . . . 369
8.2.3 Respect as Enabling Opportunities for Novel Modes of Research Participation . . . 371
8.3 Reviewing/Reconfiguring the Belmont Report's Principle of Beneficence . . . 387
8.3.1 The Belmont Report's Exposition of the Principle of Beneficence and Its Application . . . 387
8.3.2 Some Concerns Pertaining to the Belmont Discussion Around the Principle of Beneficence . . . 388
8.3.3 Beneficence as Co-considering the Consequences of the Research Approach . . . 390
8.4 Reviewing/Reconfiguring the Belmont Report's Principle of Justice . . . 399
8.4.1 The Belmont Report's Exposition of the Principle of Justice as Pertaining to Research . . . 399
8.4.2 Reconfiguring the Principle of Justice: Toward Active Research . . . 400
8.5 Ethical Practices and IRBs . . . 402

8.5.1 Reshaping a Milieu of Institutional (Dis)trust: Strengthening Trust Relationships 406
8.6 Conclusion 412
References 414

9 Practicing Multiple and Mixed Methods Research Responsibly: Some Paradigmatic Considerations 419
9.1 Introduction 419
9.2 Some Postpositivist-Oriented Deliberations on MMMR 421
9.3 Some Constructivist-Oriented Perspectives on MMMR 427
9.4 Some Transformative Perspectives on MMMR: Transformative Research Terrain 434
9.5 Some Pragmatic Perspectives on MMMR 439
9.6 Some Indigenous Perspectives on MMMR 444
9.7 A Proposal for Paradigm Stretching: Highlighting Options for Researcher Responsibilities 448
9.7.1 MMMR Discussed in Relation to Chap. 2 (Reviewing Raced Relations): Some Paradigmatic Considerations 451
9.7.2 MMMR Discussed in Relation to Chap. 3 (Dealing with Inclusive Education): Some Paradigmatic Considerations 453
9.7.3 MMMR Discussed in Relation to Chap. 4 (Highlighting Options for Gender Empowerment): Some Paradigmatic Considerations 457
9.7.4 MMMR Discussed in Relation to Chap. 5 (Locating Possible Options for Agency): Some Paradigmatic Considerations 462
9.7.5 MMMR Discussed in Relation to Chap. 6 (Researching Contexts for Vitalizing Social and Environmental Justice): Some Paradigmatic Considerations 465
9.8 Conclusion 469
References 473

10 Conclusion to the Book: Storying Our Co-responsibilities as Part of Methodological Write-up 479
10.1 Introduction 479
10.2 Reconsidering Designs Construed as "Finding Out" Exercises 484
10.3 Storying the Methodology of Research Discussed in Chap. 2 491

10.4 Storying the Methodology of Research Discussed in Chap. 3 . . . 493
10.5 Storying the Methodology of Research Discussed in Chap. 4 . . . 495
10.5.1 Storying the Methodology of Research Reported upon by Woldegies . . . 495
10.5.2 Storying the Methodology of Research Reported upon by Ssali and Theobald . . . 497
10.6 Storying the Methodology of Research Discussed in Chap. 5 . . . 498
10.6.1 Storying the Methodologies of Research Reported upon by Stephens and by Oczak and Niedźwieńska . . . 498
10.7 Storying the Methodology of Research Discussed in Chap. 6 . . . 499
10.7.1 Storying the Methodology of Research Reported upon McIntyre-Mills . . . 499
10.7.2 Storying the Methodology of Research Reported upon by Roman-Alcalá . . . 501
10.7.3 Storying the Methodology of Research Reported upon by Maathai . . . 502
10.8 Rendering Frequently Used Designs and Methods More Active . . . 502
10.9 Some Less Frequently Used Designs . . . 505
10.9.1 The Mmogo-Method™ . . . 506
10.9.2 Photovoice . . . 506
10.9.3 Performance Theatre and Use of Poetry . . . 507
10.10 A Concluding Note . . . 509
References . . . 510

Index . . . 515

About the Author

Norma R. A. Romm (D Litt et Phil, Sociology) is currently a Research Professor in the Department of Adult Education and Youth Development at the University of South Africa. She is the author of *The Methodologies of Positivism and Marxism* (1991), *Accountability in Social Research* (2001), *New Racism* (2010), *People's Education in Theoretical Perspective* (with V. I. McKay, 1992), *Diversity Management* (with R. L. Flood, 1996), and *Assessment of the Impact of HIV and AIDS in the Informal Economy of Zambia* (with V. I. McKay, 2008). She has coedited three books—*Social Theory* (with M. Sarakinsky, 1994), *Critical Systems Thinking* (with R. L. Flood, 1996), and *Balancing Individualism and Collectivism* (with J. J. McIntyre-Mills and Y. Corcoran-Nantes, 2017). She has published more than 100 research articles on the contribution of research to social development; the way in which research can be practiced accountably; Indigenous ways of knowing and living; transformative-directed research; community-engaged research; and the facilitation of adult learning. She has worked on a range of projects aimed at increasing equity for organizations such as the ILO, ADEA, and IOM.

Abbreviations

ABCD	Asset-Based Community Development
ADEA	Association for the Development of Education in Africa
AIATSIS	Australian Institute of Aboriginal and Torres Strait Islander Studies
BACUA	Bay Area Coalition for Urban Agriculture
CBPR	Community-Based Participatory Research
CIHR	Canada's Tri-Council Policy Statement created by the Canadian Institute of Health Research
CT	Creativity Thinking
DBE	Department of Basic Education (South Africa)
DBST	District-Based Support Team
DHET	Department of Higher Education (South Africa)
EI	Emotional Intelligence
ENEP	United Nations Environmental Protection Program
EQ Test	Emotional Intelligence Test
ERC	Ethics Review Committee
ESR	Institute of Environmental Science and Research, New Zealand
FG	Focus Group
FS	Food Security
GBM	Green Belt Movement
GSS	Gudjonsson Suggestibility Scale
GT	Gill Tract (at University of California)
ICAS	Ibadan Creativity Assessment Scale
ILO	International Labor Organization
ILST	Institutional Level Support Team (South Africa)
IRB	Institutional Review Board
MMMR	Multiple and Mixed Methods Research
MMR	Mixed Methods Research
NARCH	Native American Research Centers for Health
NCWK	National Council of Women of Kenya
NGO	Nongovernmental Organization

OTF	Occupy The Farm
OWS	Occupy Wall Street
PAR	Participatory Action Research
PR	Participatory Research
REC	Research Ethics Committee
SACIE	Sentiments, Attitudes, and Concerns about Inclusive Education (Scale)
SALGA	South Australian Local Government Association
SBST	School-Based Support Team
SD	Sematic Differential
SESS	State Self-Esteem Scale
SGB	School Governing Body
SM	Social Mobilization
SMT	School Management Team
TEIP	Teacher Efficacy for Inclusive Practices (Scale)
TVED	Technical Vocational and Enterprises Development Office
UNICEF	United Nations Children's Fund
WWWCYA	Wadla Woreda Women, Children and Youth Affairs Office

List of Figures

Fig. 2.1 Depiction of the setting. 57
Fig. 6.1 Three scenarios located. 253

List of Tables

Table 1.1 Language commonly associated with major research paradigms 8
Table 1.2 Indigenous paradigmatic pathways alongside the (current) big four 30
Table 3.1 Feedback from focus groups: summary of responses 116
Table 5.1 A depiction of the experimental design 209
Table 5.2 A depiction of this experimental design 235
Table 6.1 Weightings of perceived areas of concern 264
Table 8.1 Reconfiguring the Belmont model 403
Table 9.1 Paradigms and paradigm stretching: revisiting use of MMMR 449
Table 10.1 Active development, and justification, of research designs 490

Chapter 1
Introduction to the Book: Activating Transformative Intent in Consideration of the Immersion of Research in Social and Ecological Existence

Abstract Mertens regards the transformative paradigm as an extension of critical and emancipatory traditions in social research. Researchers classed as working within the transformative paradigm, she notes, consciously tie the research enterprise to the furthering of social justice concerns. This chapter revisits her account of this paradigm and its relationship to "other" paradigms. After introducing myself and my concerns as a researcher, I discuss her understanding of the axiological commitments which guide research work within the transformative paradigm, commitments which she believes inform epistemological, ontological and methodological considerations. While looking at her arguments (and some variations in them in different writings of hers), I add what I see as additional angles, primarily with reference to a number of authors advocating critical systemic thinking-and-practice and advocating Indigenous systemic approaches. I focus on considering how research as an endeavor carries specific responsibilities, arising from our recognition of the involvement of social research in shaping the social and ecological worlds of which it is a part.

1.1 Introduction

1.1.1 Introducing Myself (An "Academic" Introduction)

For readers to place my discussions on research in this book, I begin by presenting a construction of the research trajectory that has led me to the writing of this book. I use the word *construction* deliberately, to indicate that I concur with Pushor and Clandinin that narrative accounts are necessarily incomplete, fluid, multiple and changing—but they serve to show what people (narrators) value and what they find important to highlight (at some point in time) as part of their storying about their lives (2009, p. 296). Kenny elucidates too that stories told offer openings for describing and inviting co-reflection with others on values and critical themes (2002, p. 28). It is in this light that I would like my story of my academic trajectory to be understood.

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_1

In this chapter I start by presenting myself in terms of my "professional" life, beginning with my University education. In Chap. 2, I offer a more personal story, beginning with my childhood and my socialization into Whiteness (being defined as White), and my experience of deconstructing this (which also informed the "active" research outlined in my first example in Chap. 2).

At University, during my undergraduate studies I majored in Sociology and Philosophy, and went on to an Honors degree in Sociology (1981). I then chose as the topic for my Master's degree (1982) Habermas's and Marcuse's (Marxist-oriented) critiques of positivist Sociology and the positivist striving for objectivity as the basis for a scientific approach (where researchers are urged to try to divest their own values from the knowing enterprise). For my doctoral degree, obtained in 1986, while I was employed at the University of South Africa (Unisa, 1982–1991), I critically discussed Althusser's conception of Marxist methodology as intent on developing "scientific" discourses, albeit less empirically based ones than urged by positivism. In 1991, my book—*The methodologies of positivism and Marxism*—was published. In other publications (e.g., Flood & Romm, 1996; Hölscher & Romm, 1989; Romm, 1990, 1996a, 1996b, 2001a, 2001b), I considered options for reworking the Marxist argument that social inquiry should be linked to social transformation, by considering the mediating role of researchers in organizing dialogically interventionist research.

During a period of working in Swaziland (as Dean) in the Faculty of Social Sciences (1991–1993), I became involved in a project called Women and the Law in Southern Africa, where the team conducting the research consisted of seven women who were exploring inheritance issues in Swaziland as part of a larger research program in Southern Africa. My involvement in the study was as a (so-named) consultant to the researchers in Swaziland at a certain stage of the research process (as requested by the Swaziland co-ordinator). The team consisted of some lawyers and sociologists, and some non-professionals. (All of the researchers were women, although in other national teams—in other countries in Southern Africa—the research teams included men.) The idea, as detailed in Aphane et al., was to create a research project which could be considered as activist in that it "aimed not only at bringing change at a personal level [in aiding people to envisage options for action in relation to inheritance issues], but at the formal level as well, e.g., on policy, law and administration" (1993, p. 7). In Swaziland, we considered how the research remit could incorporate such an activist agenda.

While working as a researcher in the Centre for Systems Studies at the University of Hull (1993–2003), as part of my reflections on research activity, and as part of my involvement in a variety of projects (including aiding master's and doctoral students from around the globe who came to study at Hull), I concentrated—with colleagues and with students—on examining how researchers' knowing efforts have social repercussions, which I argued need to be accounted for. In 2001, I produced a book called *Accountability in social research* (2001a). I identified seven different positions that researchers might take to render themselves accountable, and I finally argued for what I named a trusting constructivist

position. "Trusting constructivism" calls on researchers to defend their positions discursively as part of a process of earning trust.

From 2003 to 2004 as Dean of the School of Humanities and Social Sciences at Cyprus College (now European University of Cyprus), I met colleagues concerned with using research—which I now call active research—to bridge gaps generated by the historical divide between Greek and Turkish Cypriots; and I also learned from certain colleagues how experimental research in studies of racism on "implicit bias" could become the basis for people/research participants rethinking their ways of orienting to others.

From 2005 onwards, besides continuing to contribute to edited books (e.g., 2006a, 2006b, 2007, 2010) I collaborated with researchers from various countries (e.g., South Africa, Kenya, and Australia) in doing research for organizations such as the International Labor Organization (ILO), the Association for the Development of Education in Africa (ADEA) and the United Nations Children's Fund (UNICEF). In all of these projects I contributed primarily to the formulation of the methodological and ethical foundations for the projects—in the proposals and in conducting the research itself. We also were "active" in working with and capacitating local researchers (new graduates from the universities), exploring with them ways of relating to research participants so as to make them feel, and be, part of the research endeavor.

In 2010, another book of mine was published, namely *New racism* (2010). This work considers how one can investigate arenas of racism which are often not visible or concretizable, and are less overt than traditional forms of racism. I drew out different ways of seeing the value of critical race theorizing, and offered suggestions for proceeding from a "dialogical standpoint epistemology" (linked to a constructivist approach).[1] In the book, I also illustrated how questions around epistemology relate directly to issues of racism. For example, (new) racism manifests itself, inter alia, when Indigenous epistemologies are afforded less status than Western-oriented —supposedly more rational—styles of knowing. Via the book I was hoping to (re)credentialize what Ladson-Billings (2003) calls "ethnic epistemologies", by offering detailed discussions around their principles and how they can be activated in accountable social research. (Like the word "ethnic", the term *Indigenous* can be used here to denote "Indigenous peoples and culture" in different contexts, as Kovach, 2009, p. 20, explains. Notably, however, authors such as Ladson-Billings and Kovach do not consider "culture" as harboring monolithic meanings, but rather as harboring symbolic expressions which can form a basis for continuing conversation around the symbols.) In any case, the book addresses the oft-unrecognized privileging of Western-oriented knowing, while calling for explicit reflection on this privileged position, as part of the discussion around racism.

[1]A standpoint epistemology as posited by certain Marxist and feminist-oriented authors suggests that research always begins from a standpoint, which needs to be acknowledged. A dialogical standpoint epistemology focuses on continued dialogue around initial standpoints/positions/perspectives. (See Romm, 1997, paras 6.1–6.5.)

In recent articles, written from my base again at Unisa (since 2012), I explore anew current issues in the terrain of research (and theorizing): for example, how one can link "retroductive" reasoning to a dialogical standpoint and at the same time undercut social dominance theory (Romm, 2013a, 2017a); how one can design and use questionnaires within a qualitative constructivist approach (Romm, 2013b); how one can facilitate focus groups in an active manner (Romm, Nel, & Tlale 2013); how storytelling can be interpreted as an expression of "agency" (McKay & Romm, 2015); how one can organize focus groups to take into account Indigenous ways of knowing (Romm, 2015a, 2017b); how an Indigenous (relational) ethic can be brought to bear on the practice of community operational research (Romm, 2017c); how one can theorize literacy initiatives as an adult educator in view of the principles of Ubuntu (Quan-Baffour & Romm, 2015); how one can use evaluative research on literacy to strengthen development initiatives (Romm & Dichaba, 2015); and how one can foreground Indigenous ways of knowing to open up options for redirecting the Anthropocene, that is, our human involvement in the ecological environment (Romm, 2014a, 2017d). These articles all encapsulate my continued efforts to explore new ways of effecting researcher accountability. (See also Romm, 2014b, where I explain how, in my inaugural lecture at Unisa in November 2013, I spoke about my suggestions concerning active and accountable social inquiry.)

This book now takes further my "interest" in thinking through how we (the research community and all those concerned) can justify a kind of inquiry where inquirers do not shy from seeking opportunities to stimulate constructive change via the research endeavor. In order to forward these considerations, I engage with, and try to expand upon, the meaning and practice of what Mertens in her extensive (and eminent) writings came to call a transformative paradigm for guiding social inquiry.

1.1.2 Some Considerations Around the Transformative Paradigm

This book concentrates on what it might mean to speak about the active role of "transformative" researchers. I focus on offering in-depth discussions, with reference to examples, of how indeed transformative activity can be forwarded in a variety of ways via the research process. I shall indicate why in transformative research, the very term *research* has become reviewed, to highlight more collaborative relationships in the research enterprise. As Midgley et al. state (2007, p. 242), the redefinition of research is especially important in the light of a (long) history of experts treating as objects of study the research participants, under the gaze of would-be scientists. Cram and Mertens (citing Cram, Chilisa, & Mertens, 2013) refer specifically to research involving Indigenous communities, where "the 'bad name' that research has within Indigenous communities is not about the notion

of research itself; rather it is about how that research has been practiced, by whom, and for what purpose that has created ill-feeling" (2015, p. 94).

Midgley et al. make the point that in the case of a research project in which they were involved with a Māori community (where the research was linked to exploring options for safe drinking water) a display of "professional identity" on the part of researchers from the Institute of Environmental Science and Research (ESR) in New Zealand, would have created distrust (2007, p. 244). If they had emphasized their professional qualifications, track record in research and research experience, this would have been viewed as problematic by community participants, as if these researchers were imposing their expertise on the community (in colonizing style) (p. 244). Researcher "identity" thus had to be reconsidered. In this case, the team of researchers consisted of mainly non-Māori researchers, and the issue of "identity" as researchers needed to be negotiated so that consultation with the community was seen as paramount. Put differently, as explained to me in personal communication by email with Midgley, 28 July 2016, the ESR researchers "needed to be acutely aware of the implications of their [professional] identity from other perspectives and act accordingly". Ultimately, Midgley et al. (2007) report that their association with a trusted group of people with "strong community networks" enabled what they call a "blurring of the boundary of the ESR team's 'outsider' identity in favor of a partnership identity" (p. 242).

As it happened in this research project, Midgley et al. (2007, p. 244) remark that at some stage, certain "weight" (importance) did come to be assigned to aspects of the ESR researchers' professional identity, namely at the point at which the Community Health Trust, in conjunction with a local Māori committee, wished the research results to be presented to a "whole of government" seminar hosted by the Office for the national community and voluntary sector—part of the Ministry of Social Development. However, this was in order to assign credence to the research (for the government audience): it did not imply that the research process as it had been conducted was a process of "experts" framing the questions to be explored and defining ways to go about exploring them (without involving participants in these processes). On the contrary, the way in which the ESR researchers liaised with members of the community at all points could be said to have been a mutual encounter.

The intent to develop a collaborative type of involvement with participants in communities which is at the same time meaningful to them (see also Midgley, Johnson, & Chichirau, 2017) is accompanied by definitions of validity that reflect this intention. Gonzalez et al. suggest that the definition of "valid" research becomes broadened so as to accommodate an "expectation that the research question itself is 'valid', in the sense of coming from, or being meaningful to, the involved community" (2011, p. S166). This tallies with Mertens' suggestion that when following a transformative research agenda, researchers should, inter alia, make data collection decisions with community members and involve them in interpreting and considering the use of "information" collected, so that the research is likely to forward "the goals of social change and social justice" (2009, p. 313). Mertens refers, by way of example, to research work organized by Chilisa and

Tsheko (2014) aimed at developing an enriched "understanding of the meaning of HIV/Aids in the context of Botswana" (2016, p. 9). She explains this as follows:

> In Botswana, sex is regarded as a taboo topic, thus by using proverbs and other indirect ways to discuss the topic, the researchers were providing a safer way for the youth to discuss their beliefs, attitudes and behaviors. (Mertens, 2016, p. 9)

What became identified as significant in this process was that "youth wanted relationships and they wanted a future" (Mertens, 2016, p. 10). Mertens cites Chilisa and Tsheko's elucidation of how interventions aimed at envisaging new futures henceforward became facilitated via the research exploration, seen as a collaborative encounter of the initiating researchers with "the researched" (as co-explorers):

> The researched reflect on their qualities and move toward self-discovery, as they dream and envision the best that they could be, dialogue on strategies to implement their dreams, and draw a plan to take them to their destiny. (Chilisa & Tsheko, 2014, p. 229, as cited in Mertens, 2016, p. 10)

Mertens also elaborates that as the project progressed, the initiating researchers continued to focus on relationship building in the community, so as to develop "coalitions, networks and connectedness with the community and parents of the youth who were involved as research participants" (2016, p. 10).

The idea of research being a collaborative, relationship-building enterprise is of course not new in the field of social research. Many researchers in the participative research tradition—which includes certain versions of qualitative research and versions of research underpinned by "critical theoretical" traditions—have focused on what Kovach (2009, p. 26) summarizes as "the intimate relationship between the researcher and what is studied". Such researchers have questioned the notion of research as setting out to seek "a singular static truth from an objective distance" (p. 26). This search for univocal "truth" is associated with what is called the positivist or postpositivist paradigm. The positivist/postpositivist philosophy of science is often regarded as suspect within postcolonial thought, because of its historical links to colonial practices of imposing ways of seeing "realities", which fail to do justice to alternative ways of engaging with "reality", and alternative possibilities for acting (Bishop, 1994; Bristol, 2012; Collins, 2000; Harris & Wasilewski, 2004; Lavia & Mahlomaholo, 2012; Ndimande, 2012).

I have used as my first example in this section the deliberations provided by Midgley et al. (2007) (who self-name their work as critically systemic), not only because of their redefinition of researcher identities but also because the example is set within a discussion of environmental concerns regarding water use in the community. The importance of incorporating ecological concerns into discussions of social justice is one of the themes that will be looked into in the book (with detailed examples provided in Chap. 6). The focus on social *and environmental* justice is regarded as particularly important by certain (if not all) Indigenous authors and others who regard our human connectedness with the non-human world, our spiritual being in relation with all living and non-living things, and our

(non-anthropocentric) stewardship role as crucial to bear in mind (cf. Cannella & Manuelito, 2008; Goduka, 2012; McIntyre-Mills, 2014a, 2014b; McIntyre-Mills & Binchai, 2014; Molefe, 2015; Murove, 2005, 2007; Quan-Baffour, 2017; Ross, 2010; Smith, 1999; Wane, Akena & Ilmi, 2014).[2] Smith suggests that a common thread that Indigenous authors stress, is the "importance of making connections and *affirming connectedness* …. Connectedness positions individuals in sets of relationships with other people and with the environment" (1999, p. 148, my italics). Lowan-Trudeau sums up that "inherent in most Indigenous worldviews is recognition of the inherent value, spirit, and interconnectedness of all people, living creatures, and bioregions" (2014, p. 356). McIntyre-Mills calls this a "systemic ethics" approach, where the systemic concept of co-determination implies that "the environment is a living entity which co-determines our very existence, not as a commodity from which to extract endless profit" (2014a, p. 133).

How then do these considerations tie in with what Mertens has called the "transformative paradigm"? Mertens suggests that paradigms "serve as … frameworks that guide researchers in the identification and clarification of their beliefs with regard to ethics, reality, knowledge, and methodology" (2010, p. 469). But she recognizes that paradigms can of course also be used to classify (place) *others*' research as falling more or less within the scope of a paradigmatic way of conceiving and conducting research (as defined by the paradigms that she distinguishes).

Kovach points to Mertens' account of "positivism/postpositivism, constructivism, transformative, and pragmatic [research] as each being a distinctive paradigm" (2009, p. 26). She argues that Indigenous methodologies can "find an ally", especially in qualitative/constructivist and transformative approaches (2009, p. 27). She also notes (2009, p. 27) that "the field of qualitative research is an inclusive space", which can include various understandings of what commitment to qualitative and transformative research might mean in practice. However, Kovach has also expressed some reservations with aligning (current views of) transformative and Indigenous paradigms too closely (pp. 176–177). As the book proceeds, I show in what respects Indigenous paradigms offer specific understandings of "being-in-relation" (see also Romm, 2015a, 2015b, 2017e; Romm & Tlale, 2016; Tlale & Romm, 2017), and specific understandings of what this implies for responsible research practice. In Table 1.2 (in Sect. 1.3.5), following Chilisa (2012, p. 40), I include a heading on Indigenous paradigmatic research to point to certain distinct

[2]Molefe (2015) explains that because African metaphysical systems generally construe reality in terms of all-inclusive relationships, he does not agree with certain (dominant) interpretations of African humanism (including that of Gyekye, 1995, 2010) where implicit dichotomies between "humans" and "nature" are posited. He sets out, as he puts it, "to challenge and repudiate humanism as the best interpretation of African ethics" (2015, p. 59). He considers that "a truly African ethics … demands that we accord moral status to some aspects of the environment, like animals, for their own sakes" (pp. 59–60). He favorably cites Murove's article (2007), where the concept of *Ukama* is seen as linked with that of *Ubuntu.*

Table 1.1 Language commonly associated with major research paradigms

Positivist/ postpositivist	Interpretivist/ constructivist	Transformative	Pragmatic
Experimental Quasi-experimental Correlational Reductionism Theory verification Causal comparative Determination[a]	Naturalistic Phenomenological Hermeneutic Interpretivist Ethnographic Multiple participant meanings Social and historical construction Theory generation Symbolic interaction	Critical theory Neo-marxist Feminist Critical Race Theory Freirean Participatory Emancipatory Advocacy Grand Narrative Empowerment issue oriented Change-oriented Interventionist Queer theory Race specific Political	Consequences of actions Problem-centered Pluralistic Real-world practice oriented Mixed models

Source Mackenzie and Knipe (2006, p. 195) (http://www.iier.org.au/iier16/mackenzie.html) Adapted from Creswell (2003), and Mertens (2005)

[a]Mackenzie and Knipe (2006) had an additional term "Normative" in this column, but I have removed it as I regard it as confusing. This is especially since they cite Mertens (2005, p. 8) as indicating that positivism suggests that "there is a method for studying the social world that is value free"; and one can add that postpositivism holds that scientists should still *strive* for value-freedom (objectivity). Hence normative considerations are not supposed to enter into one's efforts at "doing science"

ways of speaking about research that can serve to shift somewhat the language associated with the four paradigms identified, inter alia, by Mertens—see Table 1.1.[3]

Because Mertens is credited with naming the transformative paradigm as such, I consider in some detail her explication of it with reference to her views on ontology, epistemology, methodology and axiology, and the links between these notions within this paradigm. Mertens defines the transformative paradigm by drawing on and adapting various authors' typologies, such as those supplied by Lather (1992) and Lincoln and Guba (2003). In these typologies, distinctions are made between: *emancipatory* approaches (which Mertens, 1999, p. 4, and 2004, p. xix, re-names as *transformative*); *positivist/postpositivist-oriented* approaches; and *interpretive/ constructivist-oriented* ones, which Lincoln and Guba see as having some commonality with emancipatory/liberationist approaches (2003, p. 261). In addition,

[3]As noted by Mackenzie and Knipe, one can create groupings of paradigmatic positions by perusing the terms used in research texts to speak about research, and by looking at the meanings of the terms as used, so that one can then consider the language commonly associated with the "broad paradigm groups" (2006, p. 195). The terms are given meanings within the language register, seen as a system of concepts, which together construct a vision of "research".

Mertens singles out a *transformative* versus a more *pragmatic* use of "mixed methods", adding *pragmatism* to Lincoln and Guba's typology.

As the discussion proceeds, I reflect upon how a pragmatic epistemology, defined as a philosophical approach to knowing where knowing-and-acting are seen as indissolubly bound to each other (cf. Kvale, 2002; Morgan, 2014; Nyamnjoh, 2004; Ossai, 2010; Romm, 2010, 2014b; Romm Nel, & Tlale, 2013), can be considered as falling within the ambit of the transformative paradigm. I also explore in what respect the boundaries between constructivist-oriented and transformative paradigmatic research can be regarded as fluid; and I offer deliberations around ways in which certain postpositivist arguments can lend themselves to enabling researchers to take into account the ways in which research might impact on the "realities" being investigated. Finally, following and extending Chilisa's (2012, p. 40) location of an Indigenous research paradigm, I discuss and elaborate upon this paradigmatic orientation as an addition to the so-called "Big Four" (so named by Dillard, 2006, p. 65).

In essence, my argument draws on the systemic idea—emphasized within critical systemic thinking and practice, and Indigenous systemic worldviews across the globe—that knowers and knowing processes are not to be considered as separable from the continuing unfolding of social and ecological life. Such a systemic approach, where knowers are seen as *part of* rather than apart from the dynamics of life in motion, is explored in depth by, for example, Cannella and Manuelito (2008); Kuntz (2015); Midgley (2000, 2001); Midgley and Ochoa-Arias (2001); McIntyre-Mills (2008, 2014a, 2017); and Murove (2005).

Manuelito (in Cannella & Manuelito, 2008) expresses her view of how the image of "Changing Woman" can be interpreted as representing metaphorically (and pleading further to nurture) the notion of *life in the process of becoming*. Manuelito refers favorably to her being part of Asdzaan Nadleehi, where a girl's puberty ceremony is meant to celebrate "the girl representing and actually becoming Asdzaan Nadleehi, Changing Woman" (Cannella & Manuelito, 2008, p. 51). Cannella and Manuelito see this story as shared by Manuelito as expressive of

> the power of creation, transformation, equality, and life as bodily realm (an entirely different ontological and epistemological perspective than that demonstrated in the dualist notion of separation of mind/body, objectivity/subjectivity, male/female that dominants modernist Euro-American science). (2008, p. 55)

Cannella and Manuelito (2008) suggest that all kinds of dualistic thinking are challenged within what they call an "ecofeminist ethic of care", which tries to avoid separations, including any attempts to separate knowing endeavors (perceptions of the world) from their influence on the known (as "object"). They suggest that it is preferable to call upon our collective power to *create experiences that are regarded as worthy of creating* and that express possibilities in human and non-human life for "caring support" (2008, p. 55). This implies that researchers need to be aware, as Romm (1995) puts it, that "knowing" already involves an intervention in defining the direction that social (and ecological) systems might develop, with knowing

being part of these systems. Kuntz clarifies that "engaging in inquiry practices always affects the phenomena of interest—we can never *not* impact that which we study" (2015, p. 65). Once this is recognized, then "concerns about affecting data or findings" (2015, p. 65) can give way to a focus on responsibly considering—with others—how our knowing can be directed toward opening spaces for "creative capacity" to move the present into a (more socially and ecologically just) future that is "yet to be" (2015, p. 26). Kuntz cites DeMartino (2013, p. 489) as asserting that "knowing here is generative and constitutive—never merely descriptive". This, he notes, "presents an important ethical dimension for the critical inquirer: what we know, how we come to know, is never socially neutral, never absent the import of the ethical frame" (2015, p. 26).

In his book entitled *Systemic intervention*, Midgley (2000) indicates why such an epistemological (and ethical) argument can be considered as systemic at root—in contrast to a dualist perspective:

> The term "subject/object dualism" refers to the separation of the observer (subject/knower) and the observed object (or that which is being researched). In a dualist perspective the observer is somehow independent of the observed, standing outside of it, so she does not influence it in any way. (2000, p. 42)

He states that a dualism which posits a separation of observer and observed is regarded as problematic by systems thinkers (and certainly, by *systemic* thinkers). He argues that in a systemic (anti-reductionist) perspective, "*everything can be seen as interacting with everything else* (and boundaries are constructs allowing the inclusion and exclusion of elements in analysis, rather than being real markers of systemic closure)" (2000, p. 42, my italics). This means *that the observer will always be connected with the observed* (as they are all part of a system where parts can never be separated). He argues that quantum theory in the natural sciences also "challenges the conventional separation between the observer and observed by demonstrating that the former cannot help but influence the latter" (2000, p. 43). Bausch too affirms that it is now understood within quantum physics that "at atomic levels our very observation alters the processes we are trying to observe" (2016, p. 2). In quantum physics, this is called the Heisenberg uncertainty principle. Lincoln and Guba for their part propose that one can speak of the "social Heisenberg effect", where it is recognized that "the intrusion of the [social] inquiry into a local context will necessarily disturb and disrupt it". This they see as "an aspect of the social Heisenberg effect" (2013, p. 65).

In Sect. 1.3.4, I elucidate why I believe that an appreciation of our *potential influence in impacting (either through confirming and reinforcing "observed" patterns or in redirecting/transforming outcomes in the making) poses specific responsibilities for anybody involved in the practice of research*. But before embarking on this, in Sect. 1.2, I point to the conceptions of research associated with what are often understood to be the major paradigms for social research (see Mackenzie & Knipe, 2006, p. 195). These are set out in Table 1.1 above.

1.2 Central Concepts Associated with Positivist/ Postpositivist, Constructivist, Transformative, and Pragmatic Research Paradigms

MacKenzie and Knipe (2006) note that certain concepts/terms are commonly associated with research work classed under a particular paradigm. (See Footnote 3.) They summarize the differences between "major paradigms" as in Table 1.1 (by adapting the schemas of Creswell, 2003, and Mertens, 2005).

It is important to note that within this rubric, Mertens, along with certain other authors, prefers to speak of *post*positivist views, because these are more sophisticated arguments, which do not presuppose that any statements about reality can become verified—rather it is recognized that knowledge is always provisional.[4] Johnson indicates that if one wishes to apply a label, then *postpositivist* to characterize a researcher's work "is a kinder and gentler term" (2009, p. 450). He goes on to state that "the term postpositivism, rather than positivism, shows that a researcher is more cognizant of changes that have occurred over the past 75 years in the philosophy of social science and research methodology" (2009, p. 450). This characterization is consistent with Mertens' (1999, p. 4) view that nowadays more sophisticated epistemological arguments are presented, where the focus is not on being "objective", but on *striving for increased objectivity*, and the focus is not on aiming to verify statements, but on trying to get *closer to the truth* (to use Popper's vocabulary, 1969). (For a detailed exposition of how Popper's position infuses argumentation in postpositivist thinking see Romm, 2001a, pp. 20–25.)

Regarding the interpretivist/constructivist label, again there may be different arguments associated with interpretivism and constructivism. Interpretivism, as a position rooted in Weber's arguments about social science, often still exhorts would-be social scientists to strive for objectivity so as to develop plausible accounts of the motivating meanings that constitute social existence. That is, although Weber broke with positivist ontological assumptions (with his ontology of social reality as consisting of meaning-making of participants rather than of nature-like cause-effect relationships), he still accepted a positivist-directed search for value-freedom (objectivity) on the part of social scientists (see also Romm, 2013b, p. 655). Weber insisted (in line with positivist-oriented views of science) that in their attempts to understand social life, social scientists should be dedicated to the task of "recognizing facts, even those which may be personally uncomfortable, and to distinguish them from [their] own evaluations" (1949, p. 5). However, in terms of constructivist argumentation, as, for example, detailed by Guba and Lincoln (1989), Lincoln (2001a, 2001b), and Lincoln and Guba (2013), research processes include inquirers' values, concerns and emotions: the understanding is

[4]Mertens states that this (postpositivist) view can otherwise be called the "scientific method paradigm" (1999, p. 4). This definition of postpositivism as associated with a particular understanding of the scientific method, recognizes that proponents hereof deny that scientifically produced statements can ever be verified (as in earlier positivist positions).

that both within everyday social experience and in research work (where people re-look at issues of concern), our value-laden concerns will affect the manner in which our constructions of reality are developed (Lincoln & Guba, 2013, pp. 39–41). What is important in terms of Lincoln and Guba's argument is that "the various constructions held by individual knowers must be confronted, compared, and contrasted in an encounter situation" (2013, p. 40). This is what renders the constructions less "subjective"—because they become rooted in a dialogical engagement with others.

I now turn to Mertens' explication of the transformative paradigm and its relation to these various positions.

1.2.1 *Mertens' Explication of the Transformative Paradigm in Relation to Alternatives*

In discussing the distinctiveness of the transformative paradigm as she sees it, Mertens indicates that "transformative scholars assume that … an important purpose of knowledge construction is to help people improve society" (1999, p. 1). She points out that she understands "transformative theory" as an umbrella term that encompasses paradigmatic perspectives that are meant to be emancipatory, participatory, and inclusive (1999, p. 4). As she puts it:

> The transformative paradigm is characterized as placing central importance on the lives and experiences of marginalized groups, such as women, ethnic/racial minorities, people with disabilities, and those who are poor. (1999, p. 4)

She indicates that when embracing this paradigm, inquirers endeavor to "link the results of social inquiry to action, and [to] link the results of the inquiry to wider questions of social inequity and social justice" (1999, p. 4).[5] In later work, she explains that "the transformative paradigm provides an umbrella for researchers who view their roles as agents to further social justice" (2012, p. 811). Here she suggests that what merits the label of *transformative* to research work is that the researchers must consciously consider themselves as being "agents" of justice. This does not mean that it is always clear what this amounts to. This book is aimed at offering further deliberations (with further examples) around what indeed it might involve. Mertens poses certain questions for us to consider, such as the following:

> If the researcher is not a member of the community, what are the measures that are needed to establish a trusting relationship? Are there circumstances in which a trusting relationship is not advisable and could even be viewed as harmful to the conduct of valid research [for

[5]Here, Mertens is placing her discussion in the context of doing research toward program evaluation—but her statements can be seen as applying to all forms of research as she questions the distinction between "evaluation" and "research" (1999, p. 5). That is, in inquiries guided by the transformative paradigm, whatever form of inquiry is being undertaken, the values of those involved cannot be bracketed out of the study.

> example, research (re)considering elite presentations of the world]? Whereas the transformative paradigm raises many questions, answers to some of these questions await further work in this area. (2012, p. 811)

One of the aims of this book is to offer such "further work", while also further exploring, and in the process revisiting, the tenets of the transformative paradigm. In this chapter, I do this by engaging with Mertens' account of the underlying ontological, epistemological, methodological and axiological orientations associated with work in the transformative paradigm. In engaging with her account, I also at times shift or re-work some of her articulations.

Mertens (1999) states that within a transformative paradigm, the ontological question, "*What is the nature of reality and by extension, truth*?" is not answered (as in postpositivism) by assuming that we have some access to it via processes of science; nor is it answered (as in constructivism) by asserting that there are multiple realities that are constructed in processes of living and knowing. Rather, the ontological question is answered by "placing various viewpoints within a political, cultural, and economic value system to understand the basis for the differences"—so as to understand how certain perspectives on reality become privileged over others (1999, p. 5). In other words, how specific constructions of reality come to be given more status in society and *how researchers can serve to undercut undue privileging of views, is the (ontological) concern of the transformative researcher.* In some of her writings, Mertens suggests that although we cannot ever claim to have access to reality (because humans have access only to perspectives or visions), we can still posit that "there is one reality about which there are multiple opinions" (2010, p. 470). I have argued elsewhere (Romm, 2001a, 2001b, 2002a, 2002b, 2010, 2015a) that there is no purpose in positing such a reality, because as knowers we can never claim that we have access (or methods of gaining closer access) to an externally posited reality.[6]

In what I call a trusting constructivist position (2001a)—which includes concerns with transformation—the focus is on considering (with key participants) what definitions/constructions are likely to have the most leverage in effecting change toward social and ecological justice. The focus here is on "discursive accountability" (cf. Romm, 2010, pp. 361–362). The problem with positing "one reality" of which there are different opinions, is that communicators (in scholarly debates and everyday life) still become inclined to try to justify their viewpoints by claiming a better way of accessing, or getting closer to accessing, the (one) reality. This also has implications for the manner in which recommendations for action—based on so-called "better knowing processes"—become presented as if based on more "informed" understanding. This is itself not without consequence in the social

[6]As with other authors, positions held by authors at points in time can become re-developed. In response to an article of mine (2015a), in which I expressed some reservations about Mertens' ontological position, she mentioned via email (12 January 2016) that she would say that "there are versions of reality and some sustain oppression and some lead to increased social justice". (I expand upon this in Chap. 9, Sect. 9.4.)

world because people come to forget the *influence of ways of framing realities* on the "information" that has become accrued in the first place (Romm, 2002a, p. 459). The "information" then becomes unduly constrictive of possibilities for acting.

The advantage of a constructivist position as I see it is that research becomes directed at relooking at constructions (as advanced within academic discourses and as influential in everyday life), with the intention that processes of learning can be set up as people re-develop what are felt to be restrictive constructions. And as Lincoln and Guba note, one motivator to unhinge constructions (and inquire further) is "personal or cultural disharmony, including social injustice" (2013, p. 50). This is also the position taken by Mugadza when she suggests that in designing our relationship with the world, the quest for truth which is defined as representing more or less accurately some posited world, becomes sidestepped because the aim is rather to work (with others) toward activating future better possibilities (2015, p. 3).

As far as *epistemology* goes, Mertens (1999) argues that within a transformative paradigm, the question of what "knowing" amounts to is not answered by claiming (as in postpositivism) that the quest for objectivity—observing from an ideally dispassionate standpoint—can lead to increased knowledge; nor, she suggests, is it answered by an acknowledgement that the interaction between researchers and participants generates the constructions that are developed in the research process (as understood in some forms of interpretivism/constructivism). In the transformative paradigm, *the manner in which researchers relate with research participants (or participant researchers) such that a fair understanding of key viewpoints is created and such that power dynamics in the research enterprise are taken into account, is deemed crucial* (1999, p. 5). Mertens lays the emphasis on the problem of power relations in the research process and argues that it is crucial to have an interactive link between researchers and participants (2007a, p. 216). She qualifies this statement by noting that when involving participants in the research process, transformative researchers take care to consider as key participants those who are dealing with the brunt of "issues of discrimination and oppression" (2007a, p. 216). This links up with her suggestion that the research process can become used (within a transformative paradigm) to "redress inequalities by giving precedence, or at least equal weight, to the voice of the least advantaged groups in society" (2007b, p. 86). In other words, researchers have a responsibility to gear the research process toward disrupting discourses and actions which arguably contribute to perpetuating inequality.

Methodologically, Mertens states that the postpositivist paradigm is "characterized as using primarily quantitative methods that are … decontextualized"—as the dominant methods to which it accords most status (1999, p. 5). The interpretive/constructivist paradigm is "characterized as using primarily qualitative methods in a hermeneutical … manner" in order to aid the interpretation of meanings as expressed by participants (1999, p. 5). These are its dominant methods, which are given more attention in the research endeavor to add depth to the investigation. She suggests that what is specific about the transformative paradigm is that it might

involve quantitative, qualitative, or mixed methods[7]—but the community that is most impacted by the research needs to be "involved to some degree" in the methodological decisions (1999, p. 5). She therefore argues that in research/evaluation[8] underpinned by a transformative paradigm, it is regarded as crucial to confer with key participants in defining which method(s) to use (and how). She highlights the following as being important for methodological considerations:

> A researcher can choose quantitative or qualitative or mixed methods, but there should be an interactive link between the researcher and the participants in the definition of the problem, methods should be adjusted to accommodate cultural complexity, power issues should be explicitly addressed, and issues of discrimination and oppression should be recognized. (2007a, p. 216)

It is worth mentioning that in terms of a forward-looking research enterprise (as advocated in this book), imagining what *could be* (or what potential can be activated for generating more just social and ecological practices) should also inform methodological considerations. Hayes, Sameshima, and Watson express this sentiment when they note that rather than trying to "establish the reality of an already existing phenomenon", play (or use of the imagination) allows those involved in research to "imagine ways in which the signs constructed from everyday experience can be rearticulated into promise and possibility" (2015, p. 42). This is similar to Gergen's treating the research enterprise as *future-forming*, through its impacting on everyday constructions and actions (2015, p. 287). That is, "science/research" has a performative role in that use of sense-making concepts and explanations proffered as part of inquiries are always "performative" utterances: they invite actions that become implied by the statements.[9] *All research thus has an influence in shaping society, and the question then becomes how this can be taken into account*, so that researchers (all those involved in inquiries) can be said to be acting responsibly. Using similar reasoning Kuntz calls for "engaged methodologists" who acknowledge that "our methodological work is … performative" to "refute the reductive positioning of methodologist as technocrat" (seeking efficient means to study the world) (2015, p. 138). A recognized performative inquiry stance implies for Kuntz

[7]Mertens qualifies this by stating that "mixed methods designs that use both quantitative and qualitative methods can be used in any paradigm; however, the underlying assumptions [that researchers are bringing to bear] determine which paradigm is operationalized" (1999, p. 5).

[8]The distinction between research and evaluation can be regarded as fuzzy once we recognize that research and evaluation both involve working with values. (See Romm & Dichaba, 2015, p. 224, and see also Footnote 6.)

[9]Gergen (2015, p. 291) notes in this regard that Austin (1962) famously drew the "distinction between constative and performative utterances", where the former refer to statements of fact, essentially (presumably) falsifiable through observation, and where the latter refer to statements made with the intent to effect changes in existing states of affairs. However, Gergen states that Austin "was himself quick to illuminate the impurities of the distinction, in that so-called constatives also can implicitly contain calls to action" (2015, p. 291). Denzin too points to the performative power of language when he states that "words matter" (2001, p. 24). (See also Denzin's suggestion, 2003, pp. 25–26, for consciously embracing performativity.)

"a refusal to recreate *what is to be as what has always been*" and to invoke a more forward-looking research agenda (2015, p. 138).

In the chapters that follow I offer some examples, which I discuss in depth, of ways of taking into consideration the shaping effect of research practices. In the course of my discussions I suggest that a form of "retroductive inference"—whether used implicitly or explicitly—can offer scope for what Cannella and Manuelito (2008) call activating the potential *becoming* of social and ecological life (as expressed in their image of Changing Woman). The form that I propose is also the approach taken by theorists and practitioners associated with the Future Worlds Centre and the Institute for 21st-century agoras, where retroductive inference is seen as a necessary counterpoint to inductive and deductive forms of logic, and where it is tied to imaginative reasoning. [See the logical and epistemological axioms as expressed at https://dialogicdesignscience.wikispaces.com/Axioms+(7).] Briefly put, I suggest that we can consider that a kind of retroductive logic is being invoked when inquirers/co-inquirers create inferences which admittedly do not relate in any direct logical way to "empirical evidence", but which make sense of interpreted evidence/experience in ways which are, in turn, inspiring of constructive action.

Returning to Mertens' argument, following Lincoln and Guba (2003), Mertens adds as an important element in the understanding of different paradigmatic positions *the question of axiology* (alongside ontology, epistemology and methodology). The axiological question, as she notes, "relates to the nature of ethics" (2007a, p. 215). She posits that in relation to ethical issues, the transformative paradigm exhorts researchers to make "an explicit connection … between the process and outcomes of research and furtherance of a social justice agenda" (2007a, p. 216). She argues indeed that "the axiological belief is of primary importance in the transformative paradigm and drives the formulation of the three other belief systems (ontology, epistemology and methodology)" (2010, p. 470). Or, otherwise put, "the axiological assumption provides a conceptual framework from which the other assumptions of the paradigm logically flow" (2012, p. 811). This axiology calls for researchers to *actively pursue social justice* as part of their research remit and is similar to Kuntz's point that "engaged methodologists" admit that their inquiry is "governed by ethics: an open-ended ethics toward necessary change" (2015, p. 138).

In considering the axiological basis for mixing methods in mixed methods research (MMR), Mertens makes the point that although some authors associate MMR with a "pragmatic paradigm", where the emphasis is on finding appropriate designs that "work" in practice to study the phenomena at hand (2014, p. 36), she prefers to use a transformative paradigm as her grounding for mixed method use. This then, in line with a transformative axiology, requires researchers to consider how the use of mixed methods might serve the ends of social justice (2014, p. 8). Mertens' suggestion is thus that *an axiological basis where an ethic of justice prevails* would provide the grounding for deciding—with participants—choices of method and how to use the different methods. She contends that MMR work fits the transformative paradigm insofar as "researchers adhere to the philosophical beliefs

of that paradigm more strongly than to those of pragmatism" (2014, p. 35). This is based on her perception that pragmatists tend to shy away from "(irresolvable) philosophical discussion" (2014, p. 36).

Nonetheless, in (re)considering the value of adopting a pragmatic stance, Morgan makes the point that it is possible to name what he calls a "philosophical pragmatism" (2014, p. 1045). He avers that such a pragmatism can pay "particular attention to issues of … justice as a broad agenda for social research" (2014, p. 1045). In the chapters that follow, I offer illustrations of how one might defend such a pragmatism.

Meanwhile, Mertens suggests that the overall philosophical basis that informs the work of those whom she characterizes as working under the umbrella of the transformative paradigm, is that they try to encourage the use of the research to "stimulate social action" which is likely to enhance social justice (2012a, p. 809). Another way of expressing this is to indicate that the research should be directed in such a way that social wellbeing is likely to be enhanced as a result of the research process. She argues that it is important to "include reciprocity in the design, that is, ways to leave the community better off than when they began the research study in terms of increased knowledge, capacity, or changes in policies and practice" (2011, p. 196). This implies a definition of justice that is linked to (marginalized) people's experience of wellbeing in terms of what Letiecq and Schmalzbauer phrase as "social justice principles we value, including: human dignity; non-discrimination; civil and political freedoms; economic, social and cultural rights; and solidarity" (2012, p. 247). That is, justice would need to be defined primarily with reference to the views of those most marginalized. As stated earlier, Mertens leaves somewhat in abeyance the issue of *environmental* justice, but this is not excluded in her approach; and with Cram (2015, p. 96), this concern is entered explicitly into the argument, with reference to authors such as Chilisa (2012) and Wilson (2008).[10]

1.3 Reconsidering Our Language When Speaking About Research

I have intimated in Sect. 1.2.1 that it is possible to revise our understandings of ontology, epistemology, methodology and axiology while still accepting Mertens' notion of transformative researchers as those "who view their roles as agents to further social justice" (2012, p. 811). I have suggested that what this "agency" implies as far as research work is concerned, is yet to be further explored. I now put forward that in conceptualizing responsible and active inquiry, certain revisions of

[10]In response to my article in which I explored Mertens' position in terms of a critical systemic and Indigenous lens (2015b), she mentioned to me (12 January 2016) that "I agree that I should include ecological justice as well; that is important"; and she referred me to her writing "with Fiona Cram on the intersection of transformative and indigenous paradigms".

the terminology as used by Mertens may be in order, especially if one wishes to more fully embrace Indigenous ways of thinking in understandings of research (re-exploration) as an enterprise linked to the development of social and environmental justice.

I start with the notion of ontology, and then move on to discuss other notions. I discuss them in the order of ontology, epistemology, axiology and methodology, as I regard the handling of methodological decisions as a product of ontological, epistemological and axiological considerations.

1.3.1 Ontology: Web of Connections

When discussing the notion of ontology from the perspective of "postcolonial Indigenous research paradigms" (2012), Chilisa explains that *a relational ontology* (understanding of the nature of existence) recognizes that people are situated in a web of connections. She elucidates this as follows:

> The thrust of the discussion is that among indigenous people, in the colonized and former colonized societies, people are beings with many relations and many connections. They have connections with the living and the nonliving [which includes the ancestors], with land, with the earth, with animals, and with other beings. There is an emphasis on an I/We relationship. (2012, pp. 21–22)

She suggests that in terms of research relationships, this implies that "the researcher becomes part of circles of relations that are connected to one another and to which the researcher is accountable" (p. 113). Murove similarly expresses this worldview, which he conceives to be harbored within African traditions (and attendant moral orientation):

> Human existence and life in general are meaningful in the context of relationships. These relationships are not only about things in their concrete, but they involve the past, the present and the future [e.g., future generations whose wellbeing also has to be borne in mind]. It is possible to extend the common good into the future if our present existence fosters inclusive wellbeing among all that exists. The actions that are done for the good of all will also promote the good of all into the future. Such a paradigm of a holistic ethic can only be plausible on the grounds that we start by affirming relationality as an inescapable framework of everything that exists. (2005, p. 208)

The "reality" that Chilisa (2012) and Murove (2005) posit here is thus a world in which all things (including knowers/people as part of the web) are seen as inextricably connected, and where people orient themselves toward nurturing these connections among "all that exists". This worldview also underpins Cannella's and Manuelito's proposal for using the research process to create possibilities in human life and also in relation to non-human life for "caring support" (2008, p. 55).

A critique of the dualism between "subject" (researchers) and "object" (under study, namely, "the world") as advanced by these authors leads us to appreciate that the "realities" that are being explored can be taken to be *unfolding processes*, with

researchers impacting on these processes through the way in which the research is organized and research results developed. Once we accept that research is itself implicated in the unfolding of (emergent) relations, then this places definite responsibilities on all those involved in the research enterprise to consider their obligations by considering (with others) the potential impact of the research. To the extent that research processes reinforce constructions that are debilitative (such as in deficit accounts of people and communities, as noted by Cannella and Manuelito, 2008, p. 48, and Chilisa, 2012, p. 174), research is complicit in stabilizing the constructions and the actions based on these constructions. (See also Romm, 2001a, p. 234.) To the extent that research serves to challenge constructions (and social actions) that are arguably oppressive in their social and ecological consequences, the research makes a difference to the quality of our existence—in the direction of increasing wellbeing. An understanding of "reality" as being made up of a web of connections, with researchers being part of this web, thus forms the basis for a new understanding of responsibility, as the *obligation to consider our involvement,* via *research, in the potential development of "the world".*

In order to illustrate how the manner in which research is undertaken influences perceptions/constructions and outcomes "in reality", I refer to an example of research involving what the researchers in this case call local (Indigenous) and Western-styled knowing and accompanying versions of reality (Kaschula, Twine, & Scholes, 2005). They refer to a project dealing with coppice harvesting of fuelwood species on a South African Common—where they indicate that those who were party to the research exploration sought to engender a community-based natural resource management process (Kaschula et al., 2005, p. 388). They furthermore affirm that many (human ecological) researchers

> have in recent decades emphasized the wisdom of Indigenous cosmologies which treat plant, animal, and human interactions as a single spiritual, moral, and regenerative system, and energetically sought to incorporate these cosmological traits into management policy. (2005, p. 388)

They caution, however, that when investigating ways of managing the environment, researchers following Western-oriented "scientific" research protocols have tended to disregard "Indigenous knowledge that fails to meet their rigorous scientific standards" (2005, p. 391). Tengo, Brondizio, Elmqvist, Malmer, & Spierenburg. (2014) likewise express concern that within an "integrative" approach to integrating different styles of knowing, there is a tendency to credentialize Indigenous ways of knowing *only when these can be brought into alignment with methods for scientific validation* (as in laboratory trials). By contrast, a *parallel* approach, as they call it, tries to assign equal status to different ways of knowing, viewing each as "legitimate in its own right" (legitimated by invoking different criteria of validation in each case) (2014, p. 582). Tengo et al. also refer to a third approach to working with different styles of knowing (and conceptions of its validation), namely "the *co-production of knowledge*" (2014, p. 582, my italics), which they regard as preferable. It is against this historical background of possibilities that both Kaschula et al.

(2005) and Tengo et al. (2014) assert that it is important to move outside of the "simplistic Western/traditional dualities" (of so-called "scientific versus Indigenous knowledge").

In the case discussed by Kaschula et al. (2005), the focus was not on validating one or other vision of "realities" regarding the sustainable utilization of woodland resources—but rather, on *findings ways forward that would express a sustainable utilization in the context under consideration*. As they note, the research question was,

> given the minimal resources available to local communities in terms of controlling harvesting technique and nutrient replacement programs, how can we manage a coppice harvesting program that encompasses local understanding of tree vegetative regeneration and harvesting practices and a consideration of biological processes involved in coppice regeneration? (2005, p. 393)

According to Kaschula et al., the research effort in this project was able to link biological data relating to coppicing (i.e., vegetative regenerating) to an appreciation of how local communities "understand, identify with, interpret, make use of, and manage their own natural resources" (2005, p. 393). Those organizing the research did this not by trying to integrate "scientific" versus "local" data sets, but by creating space for appreciating the Indigenous knowing as had been expressed via semi-structured interviews as well as focus group sessions in the community, which enabled participants to outline and discuss issues connected with coppicing.

Kaschula et al. comment that the continued discordance between the two data sets could be interpreted in the following way:

> Whereas scientific knowledge is inferred from the isolation and quantification of single variables, Indigenous knowledge tends to focus more on the bigger picture, bracketing plant responses into general trends and "rules of thumb" rather than specific categories. (2005, p. 410)

Additionally, they point out that while the "scientific method" necessitated working at a "miniscule scale" (to isolate particular variables), the focus on sustainability demanded working "at the broadest systems-level scale", to encompass multiple levels—including social and political and not just biological levels" (Kaschula et al., 2005, p. 410).

Kaschula et al.'s (2005) consideration of the inputs of different approaches to seeing the world, resonates with Smith's suggestions for "making science work" in a way which is not hostile to Indigenous ways of seeing and knowing (as has historically been associated with colonialism). Smith points out in relation to Western science and technology that there are

> huge debates within the scientific community about the nature of science …. This debate is over the notion of constructivism, and concerns the extent to which knowledge is socially constructed or exists "out there" as a body of knowledge …. The development of ethno-science and the application of science to matters that interest Indigenous peoples such as environmental and resource management or biodiversity offer some new possibilities for Indigenous people to engage with the sciences which they decide are most relevant. (1999, p. 160)

In the case of the research reported upon by Kaschula et al., they posit that an attempt was to move beyond the apparent discordance between local and (purportedly) scientific paradigms, by appreciating Indigenous knowledge "as an indicator of the symbolic and cultural values attached to natural resources" (2005, p. 410). They recap that the purpose of the research was not to look for a way of establishing a congruent account of "the realities", but to *find workable social and political solutions to environmental management issues in the fluid situations which were being explored, and which the research was contributing to create*. I have used this example to illustrate how researchers can make an entry into the patterning of the web of human and natural relations *by consciously being part of its way of developing*. This would also be consistent with Smith's focus on the project of *co-creating* as part of our being-in-the-world (1999, p. 158).

1.3.2 *Epistemology as Social Relations of Knowing: Self-in-Relation*

Kovach underscores the way in which an Indigenous epistemology "references within it the *social relations of knowledge production*" (2009, p. 21, my italics). Such an epistemological stance appreciates that knowing takes place in a social context, with "selves" constructing meanings *in relation* (i.e. in connection with one another and with "the environment"). She indicates that the term *epistemology* as she uses it "most closely approximates the *'self-in-relation'* … aspect inherent in Indigenous knowledges" (2009, p. 21, my italics). She states that she does not wish to create unhelpful binary oppositions between this and Western-oriented approaches to knowing, which tend to be more individualistically oriented (in the sense that knowing is seen as somehow resting on individual thought patterns). She acknowledges that "Western thought is not monolithic or static" and that there will be forms of Western thought—such as Denzin and Lincoln's (2003) constructivist position—which emphasize the socially constructed character of knowledge constructs and which acknowledge the transactions between knowers in particular contexts of knowledge production (2009, pp. 21–22).

What is specific to an Indigenous relational approach to knowing, as noted also by Chilisa (2012, p. 116) is that collectively-generated knowing (by selves in relation) is expressly sought. Collins expands upon this epistemological notion by offering the metaphor of "the polyrhythms of African American music, in which the relationships between the different beats becomes focused upon at the same time as individual uniqueness is acknowledged" (2000, pp. 262–263). Harris and Wasilewski, in offering their account of Indigenous ways of knowing and relating with reference to their involvement in the organization founded by Harris, called Americans for Indian Opportunity (AIO), explain that one of the characteristics of dialogue as understood within Indigenous thought (as they see it) is that those engaging in the dialogue appreciate that

> our strength is increased by sharing [in the process of developing communal wisdom]. We can affirm our view, expand our view, or sometimes alter or even give up our current view when we encounter a new one. We can also allow others to have contrastive views as long as they do not impose their views on us and vice versa. (2004, p. 498)

Harris and Wasilewski are concerned that in the current arena of globalization, the potential for "positive relationships" where people are oriented to recognize that our fates are interconnected with one another and with Mother Earth, has been unduly threatened by the forces of "power" and "profit" (2004, p. 498). Hence they exhort that "we need to establish respectful, caring relationships of responsibility with each other" (p. 499). For them, ways of knowing (and dialoging) are linked to the development of an orientation of caring, so that epistemology and ethics are not seen as separable. This is discussed further below, when I consider what Chilisa calls a relational axiology as an ethical approach (2012, p. 117).

However, before proceeding with this, I wish to re-iterate that the emphasis in a relational epistemology as defined here is clearly not on seeking objective "truths" about some (posited) world, but on what Roos calls "co-constructing and sharing meanings that may be regarded as an embodiment of human experience" (2008, p. 659). Roos refers to the Setswana word "mmogo", which means "relatedness, co-ownership, togetherness, co-construction and interpersonal threads" (2008, p. 660)[11]. She indicates that this has implications for epistemology, as follows:

> "Mmogo" acknowledges that meaning-making is derived from collective interactions within multiple social systems ranging from micro-systems to macro-sociocultural contexts … As such, the contexts are also constructed as symbols of meanings in their [research participants'] lives. (2008, p. 660)

What further follows from this is that the "insights" that become developed as people co-explore their contexts, can be regarded as having the status of *stories*, rather than of being more or less true statements about a reality that can be pinned down. Considering the features of African Indigenous Knowledge Systems (AIKS), Ossai highlights the notion that oral storytelling is paramount in this style of knowing, and also that such knowing takes into account long-term cycles of feedback, rather than being focused on short-term analyses (2010, p. 10). One of the qualities of stories is that they are "living"—they express people and life in *processes of development* (rather than as static). Pinnegar similarly points to her understanding of how "living processes of inquiry" (as expressed and/or documented through narratives) "make a commitment to each moment since we recognize that each moment holds all possible futures" (2007, p. 249).

Pinnegar suggests that narrative inquirers' accounts, and indeed any accounts that are expressed as narrative, "do more than create examples [of ways of living]—they *create realities*" (2007, p. 249, my italics).[12] This ties in with the point I made

[11]Setswana is one of the Indigenous languages of southern Africa.

[12]Pushor and Clandinin indicate (2009, p. 292) that "not all approaches to narrative inquiry see this connection to growth and change". Some researchers may see their work as "descriptive or analytic and they do not regard the research in and of itself leading to change" (p. 292). Pushor and

earlier that *research can be regarded as reality-forming* in the sense that what becomes expressed via the research process, already makes a difference to the continuing development of life. Even macro-sociocultural contexts as interpreted can be regarded as "living" in the sense that their patterning becomes either stabilized or altered *through the way in which they are lived*, and through the way we articulate and share our constructions. Bowers aptly expresses this concept when he discerns (citing Troncale's unpublished lectures, 2004–2007) that "in relative terms 'structure' is slow 'process' and 'process' is fast 'structure'" (2011, p. 545).[13] The implication here (for me) is that what might appear as fixed/stable structures can be reconceptualized as *processes of living that have been slowed down, but nevertheless can be stirred into new configurations if we envisage and create new ways of living*. Kuntz elucidates that "an ongoing engagement with process brings with it the concomitant valuing of fluidity over fixity, movement over stasis" (2015, p. 76). In light of this, my suggestion is that considering our "knowing" as being an involvement in *creating future-directed stories*, can enable us to develop more generative accounts, which are generative in the sense of serving to stimulate movement toward a future "yet to be" (as Kuntz, 2015, p. 26 puts it). (See also Romm, 2010, 2013a, 2017d.)

Once we consider research as consciously future directed, it means that the criterion of "catalytic validity" becomes a central guide for transformative researchers (cf. Chilisa, 2012; Gergen, 1978; Lather, 1986; Lincoln, 1995, 2001a, 2001b; Lincoln & Guba, 2013; Mertens & McLaughlin, 2004). The knowing endeavor is then not aimed at trying to "map" reality (as if this is possible), but at stimulating action (toward the ends of justice).[14] Kvale suggests that this implies the adoption of a pragmatic approach to knowing, where "truth" is defined as

---

Clandinin cite Atkinson's approach (1995) as an instance of this view. However, the argument in this book is that whether or not the research endeavor sets out to be implicated in change, the research is likely to have some effect on the lives of the people being studied (and in the broader community and society)—an effect that needs to be accounted for.

[13]Bowers' (2011) account of *process-structure* as a way of conceptualizing systems seems to be similar to the position suggested by Giddens (1984) when he develops the concept of *structuration processes* as applied to society. As Bowers explains it, "process–structures are coupled transforms. That is, as structure changes, process changes and vice versa" (2011, p. 545). In Giddens' theory of structuration, the structural properties of social structures exist only in their instantiations in social practices; hence social actors through their practices can exercise some degree of agency in their involvement in society (1984, p. 17). In addition, Kuntz argues that we need to emphasize, in accord with poststructuralism (as a theoretical position), that agency is *relational,* so that we do not posit "subjects" as somehow separable from the multiple and varied contexts in which they are immersed (2015, p. 51).

[14]If one still wishes to use the terminology of "mapping", it can be shifted in connotation to imply that one is mapping (with others) a way forward—see, for example, Bausch and Flanagan (2013), Christakis (2004), Flanagan and Christakis (2009), Laouris (2014), Laouris and Michaelides (2017), Shiakides (2010), for accounts of how maps can be developed with this intention. See also White's (2003) discussion of how maps can function to envision leverage points for action.

"whatever assists us to take actions that produce the desired results. Deciding what are the desired results involves values and ethics" (2002, p. 319). There are various ways in which researchers can take responsibility for consciously taking part in catalyzing actions, informed by axiological considerations (such as those detailed below). My argument in this regard is that researchers should not shy away from recognizing their influence in shaping the world of which they are part (and not apart): it is this recognition that should prompt them to try to energize action (their own and that of others) in a responsible way, rather than denying that research is already—wittingly or unwittingly—an impactful event. This of course has implications for our way of understanding axiology, as explained in the next section.

1.3.3 Axiology: Pursuing an Inclusive Wellbeing

According to Chilisa, in terms of a relational axiology, research should be guided by "the principles of accountable responsibility, respectful representation [of viewpoints and experiences], reciprocal appropriation, and rights" (2012, p. 117). A relational axiology expects researchers to operate in terms of a sense of connectedness with communities, the wider society and the whole network of relations in which they are involved. It is this sense of connectedness that, as Chilisa puts it (2012, p. 13) "invites researchers to interrogate their roles and responsibilities as researchers". Or, in the words of Kovach, "there is an ethical responsibility to not upset a relational balance". Otherwise put, there is a responsibility to make a positive difference to the "relational balance" and to "work toward eradicating egregious research practices" (2009, p. 178). How then, can these responsibilities be enacted? Kovach argues that there are no recipes for this because "specific responsibilities will depend upon the particular relationship" (2009, p. 178). However, what can be said is that "responsibility implies knowledge *and action*. It seeks to genuinely serve others, and is inseparable from respect and reciprocity" (2009, p. 178, my italics).

Kovach suggests that this axiological basis calls upon inquirers to seriously consider ways of activating "inherent stewardship responsibilities" and to "demonstrate how research gives back to individual and collective good", as defined through processes of inquiry (2009, p. 174). She states that:

> Research in service of social and ecological justice is inseparable from this value [of giving back]. Global warming, oil wars, religious dogma, poverty, isolation, unhealthy coping—there is evidence everywhere of a struggling world, angry and fragmented. The research community is becoming mindful of its complicity in this disorder. A desire to take responsibility is growing incrementally, albeit slowly, and in its wake is an appetite to learn of knowledge-seeking systems premised upon stewardship. (2009, p. 174)

Instead of denying that research can become complicit in social and ecological disorder, researchers who subscribe to a relational axiology hold it important to (re)generate knowing systems so that research efforts can embrace a stewardship

responsibility, mindful of the way in which, in "both its procedure and consequences", research is not innocent in its practice (2009 p. 174).

When Kovach suggests that responsibility implies *knowledge* (which she sees as co-constructed) as well as *action*, she is writing in a context in which researchers—especially non-Indigenous ones—often associate their sole responsibility as seeking to advance "knowledge". She wishes to accentuate that our responsibilities as researchers (professional researchers and others) extend to considering how research can, in Mertens' terms, act as a stimulus for constructive action (Mertens, 2012, p. 809). This is what I call a commitment to "active research", which I discuss further as the book unfolds, and which I relate to a conception of research as consciously world-forming.

1.3.4 Methodology: Reflecting Responsibly on Research Design

Mertens summarizes the methodological proposal of the transformative paradigm:

> The transformative methodological belief system supports the use of a cyclical model in which community members are brought into the research process from the beginning and throughout the process in a variety of roles. (2010, p. 472)

This proposal is consistent with Chilisa's suggestion (2012, p. 118) that a relational axiology requires a participatory methodological approach, in which participants (especially marginalized ones) are part of the research in all its stages. This, for Chilisa, means that they participate, inter alia, in: the framing of research questions; the choice and use of methods; the way in which findings are drafted and discussed; and in the way that meetings are held with key participants and targeted audiences to review the findings and to consider their import in terms of possibilities for action. Cram, Chilisa, and Mertens, in their introduction to the edited book, *Indigenous pathways into social research,* cite in this regard the "growth of community-based participatory research in the United States and participatory action research in other parts of the world" that is inspired by Indigenous worldviews/paradigms (2013, p. 18).

While Cram, Chilisa, and Mertens seem to advocate an engagement with community members for a prolonged period in order to define the contours of research processes and their emerging development, Knudson raises the question of what she calls "time constraints versus relationality" (2015, para 21). Lincoln has similarly argued that prolonged dedication to a community may not be possible for many/most professional researchers/academics, especially those who are associated with university institutions (2001a, p. 130) She cites time pressures such as "professional meetings, 'keeping current in one's field', teaching, advising students, overseeing dissertation research, and intra-institutional government responsibilities" (2001a, p. 130). Furthermore, the requirement to "see change through" (as in

catalytic validation) implies what Lincoln calls "heavy responsibilities in the face of institutional demands on time and energy" (2001a, p. 131). However, she notes that despite this, certain people "do pursue deeply engaged forms of community work within university structures, and doubtless have strategies for doing so that others may be able to adopt" (2001a, p. 130).

I have suggested in some of my methodological writings (e.g., McKay & Romm, 2008; Romm, 1999, 2002b, 2014b, 2015b; Romm & Adman, 2004; Romm & Hsu, 2002; Romm & Tlale, 2016) that engagement can take the form of *active research*, and that there are creative ways in which (professional and other) researchers can involve themselves in using the research process as a vehicle for change that is felt to be constructive. I suggest that if this is not done consciously, it does not mean that the shaping effects of the research are absent. Research can all too easily function, through self-fulfilling effects, to entrench social patterns that are supposedly observed and explained (see also Gergen, 1978, p. 1352). This is what Kovach (2009, p. 174) implies when she avers that research can become complicit in the perpetuation of a "struggling world" (Sect. 1.3.3). What is therefore important for researchers wishing to incorporate a transformative remit for which they can account, is that they work, with others, toward impacting responsibly, *taking into account the possible consequences of the ways of doing research*.

This book spells out options for proceeding accordingly in terms of an appreciation that research is never neutral in its social and ecological impact. This appreciation creates a basis for choosing ways of proceeding and trying to find opportunities to make research "beneficial" in desired directions. As Shaw indicates, this involves considering "immediate beneficiaries" (research participants) as well as "collective benefits of research" on a wider scale (2010, p. 408). I have placed the word "beneficial" in quotation marks here in line with Murove's considerations that "benefit" need not and should not be defined as if it is somehow linked to *self-interest*, because in terms of a relational ethical paradigm, which "emphasizes the interconnectedness of everything that exists", the notion of "self-interest becomes illusory, if not pathological" (2005, p. 4). Murove argues, favorably citing Polanyi's (1944) and Robertson's (2001) critical analyses, that it is regrettable that certain "early modern economists attempted to divest economics from morality and in this process the "idea of sympathy as social logic" was "hardened into a theory of self-interest" (Murove, 2005, p. 154).[15]

The question to which I now turn is how we should regard the relationship between different paradigms which might inform social research and how the contribution of Indigenous pathways to research extends the discussion of paradigms.

[15]This can indeed be seen as an example of how ill-reflected upon theorizing (as undertaken by certain scholars) which comes to imbue common-sense perceptions can function as what Gergen calls a stranglehold in its view of "human nature" or human potential (1978, p. 1354).

1.3.5 *Revisiting Boundaries Between Paradigms and Creating Space for Additions*

As indicated in Sects. 1.1.2 and 1.2, McKenzie and Knipe (2006) offer a presentation of the four paradigms (postpositivist, constructivist, transformative and pragmatic) that have served as a reference point for a myriad of discussions around paradigms in the research literature. I have tried to demonstrate in this chapter that reflections around paradigmatic assumptions, which serve to direct research endeavors, are worthy of undertaking; and, in line with Kovach's conception of the transformative paradigm as offering an "inclusive space" for furthering discussion (2009, p. 26), I have included this in the subtitle of this book to signal its significance. Nevertheless, Kovach also points to the value of outlining the quality of Indigenous paradigms (worldviews) as offering a distinct approach, with associated language (not provided for in the four-columned typology as presented by Mackenzie and Knipe, 2006, adapted from Creswell, 2003, and Mertens, 2005).

Dillard—as an African American woman—clearly expresses her reservations about her approach being subsumed under the "big four". She indicates that she wishes to embrace a paradigm that "resonates with my very spirit and provides some congruence and support for the work that I do, as an African-American woman scholar" (2006, p. 65). She expresses that:

> Rather than subvert the Big Four (or worse yet, create a replicated "sub-version" of the same), I seek to embrace and create a paradigm that embodies and articulates a coherent sense of life around me, as an African-American woman. (2006, p. 65)

Dillard (and many Indigenous researchers) prefer to add additionals to the "big four" so that their positions (including their specific understandings of spirituality and what it means to do research with a spiritual focus) are not subsumed under one of these four, which are still seen as overly Western oriented. Koitsiwe, writing from the context of South Africa, too suggests that "a new paradigm and epistemology in research is important because the global knowledge economy is based on new and diverse ways of generating and developing knowledge for sustainable livelihoods" (2013, p. 274). Furthermore, Wilson and Wilson, both from the Opaskwayak Cree Nation in Canada, indicate that "the highlight of our careers was to finally realize an initiative that reflected an Indigenous paradigm; one that honored relationships in all their many forms" (2013, p. 340). And Smith (1999, p. 186) notes that many Māori researchers have suggested that so-called "critical" approaches to research as developed by Western academics in the critical theoretical tradition often fail to cater for the specific issues "of communities such as Māori" and also tend to assume that "oppression has universal characteristics which are independent of history, context and agency" (1999, p. 186). Smith takes the position that although Māori struggles may be based on "the specificities of our history and our politics, this does not "preclude those who are not Māori from participating in research that has a Kaupapa Māori orientation" (1999, p. 186). (The

research referred to by Midgley et al., 2007, which I cited in Sect. 1.1.2 is a case in point.[16])

Smith poses the question of "whether Kaupapa Māori research is its own paradigm"; and she states that as she sees it, "it is both less than and more than a paradigm" (1999, p. 190). It could be defined as a paradigm in the sense that it "defines what needs to be studied and what questions need to be asked" and also has a "set of assumptions and taken-for-granted values and knowledge [or modes of collective knowing], upon which it builds". It is, however, "more than the sum of those parts" (1999, p. 190): Kaupapa Māori research is a social project and it "weaves in and out of Māori cultural beliefs and values" (p. 191). By defining it as a social project, she shows that "research" itself must be redefined as more than just research (i.e. insofar as research is considered as separable from the involvement in social projects). This means that she does not necessarily concur with those who wish to offer Māori research as presenting a "new paradigm" (which indeed could become restrictive).

In her tabling of different paradigmatic positions, Chilisa identifies the research orientation which she considers as associated with an "Indigenous research paradigm". This orientation includes commitments such as:

- operating in terms of a relational accountability;
- challenging deficit thinking (while "carrying hope");
- being informed by Indigenous Knowledge Systems and various modes of critical theorizing; and
- recognizing the social construction of multiple realities (as well as humans' multiple connections with the environment, cosmos, living and non-living things). (2012, p. 40)

But Chilisa also indicates that her locating of an Indigenous research paradigm need and should not imply adopting "an either-or approach", where, in the discussion of "Euro-Western paradigms and postcolonial Indigenous paradigms, these paradigms become essentialized, compelling thought along binary opposites" (2012, p. 25). What a postcolonial framework offers is an "articulation of a postcolonial Indigenous research paradigm informed by a relational ontology, epistemology, and axiology" (2012, p. 39). Chilisa, Major, and Khudu-Peterson refer to the call for

> a fifth paradigm (Buntu, 2013; Chilisa, 2012; Romm, 2015a; Russon, 2008; Wilson, 2008) to add to the typology of the current four Euro-Western paradigms: postpositivist, constructivist, transformative and pragmatic. (2017, p. 2)

[16]Midgley clarified in an email to me (28 July 2016) that the research was undertaken by a team of mainly non-Māori researchers from the Institute of Environmental Science and Research (New Zealand), with one member of the team being Māori and another having married into a Māori family. He also explained that "from a Māori perspective, you can say you are Māori if you can identify even one distant Māori ancestor; but then if you want to be an actual part of the Māori community, you have to learn their culture and traditions".

What might be suggested in conclusion, is that it is important to include Indigenous paradigms in paradigm typologies in order to create a space for further conversation between the (Euro-Western-oriented) "big four" and more Indigenous-oriented pathways (or projects as Smith, 1999, p. 191, terms them). In view of the ways of speaking about research that have been provided by Indigenous authors as well as by those who could be called Indigenous-oriented authors sympathetic to Indigenous worldviews, as discussed above (where I would "place" myself[17]), I provide a summary tabulation for now in Table 1.2. (In Chap. 9, I offer a further expanded version of this table with reference to discussions in Chaps. 2–8, and with reference to more detailed examination of various philosophical underpinnings of multiple and mixed methods research, i.e., MMMR: see Table 9.1.) As far as considering myself as Indigenous-oriented is concerned, what I mean is that I identify with the values which I see and draw out from authors writing about Indigeneity, including values which I draw out from my interactions and conversations with people Indigenous to Africa in particular. As can be gleaned from my personal narrative in Chap. 2, Sect. 2.2, I became increasingly aware, as I grew up, of injustices levelled against Black people in South Africa and of the importance of establishing connections (human connectivity). While at Unisa from 1982–1991 (see Sect. 1.1.1 above) I had the opportunity to interact in depth with Indigenous South African colleagues as we explicitly explored together what it means to be "human". (Even during the apartheid era Unisa employed Black staff and most of our students were Black.) At the University of Swaziland (1991–1993) I interacted with colleagues from many African countries and learned more about African Indigenous styles of thinking and being. And again at Unisa (from 2012 onwards) I worked in close collaboration in research projects with colleagues/friends Indigenous to Africa. (See, for example, the research projects detailed in Chap. 3.) Part of the process of designing research together included speaking about the worldview of "Ubuntu" and how we could incorporate this in our research. As I explored in Romm (2017e), when we engage with traditions (such as traditions of Ubuntu) we can seek interpretations that offer options for revitalizing dialogue around the values by which we wish to live together with others (and with all life on the planet).

[17]In the process of our writing a joint article (published in 2015), where we deliberated around this, Kofi Quan-Baffour suggested to me that Indigeneity is not about "skin color", but about where ones sympathies lie (in terms of self-identification). But, of course, as Cram, Chilisa, and Mertens point out, "the recognition of who is Indigenous is fraught with tensions related to ethnicity, race, colonization, and culture" (2013, p. 13). I am aware that I have occupied and continue to occupy a very privileged position by virtue of being seen as "White" in terms of racialized categorizations—see Chap. 2, Sect. 2.2. Nevertheless, I would like to class myself as Indigenous oriented. Readers can listen to a podcast conversation held in September 2016 between Francis Akena and myself, which refers to the notion of being Indigenous oriented (and offers Akena's interpretation of this in the context of our discussing the foregrounding of Indigenous worldviews and ecological practices): https://archive.org/details/NormaRommAndFrancisAkena. I also refer to this notion in Romm, 2017d, p. 4.

Table 1.2 Indigenous paradigmatic pathways alongside the (current) big four

Positivist/ Postpositivist	Interpretivist/ Constructivist	Transformative	Pragmatic	Indigenous and Indigenous oriented
Experimental Quasi-experimental Correlational Reductionism Theory verification [or falsification[19]] Causal comparative Determination	Naturalistic Phenomenological Hermeneutic Interpretivist Ethnographic Multiple participant meanings Social & historical construction Theory generation Symbolic interaction	Critical theory Neo-marxist Feminist Critical Race Theory Freirean Participatory Emancipatory Advocacy Grand Narrative Empowerment issue oriented Change oriented Interventionist Queer theory Race specific Political	[Attending to] Consequences of actions Problem-centered Pluralistic Real-world practice oriented [as experienced[20]] Mixed models	Culturally & relationally accountable (& participatory) Challenging deficit discourses Nurturing of reciprocal research relations Transformative toward (contextually considered) social & ecological justice, & spiritually symbiotic

Adapted from Mackenzie and Knipe (2006), as portrayed in Table 1.1, with qualifications in square brackets and with additional 5th column (drawing upon and extending Chilisa, 2012, p. 40)

[a]In his famous books, *The logic of scientific discovery* (1959) and *Conjectures and refutations* (1969), Popper argues that science relies on creating claims which are falsifiable with reference to evidence. If such claims cannot be refuted after repeated attempts at falsifying them, they can become tentatively corroborated (but never verified or confirmed). See also my detailed discussion hereof in Romm, 2001a, pp. 20–24

[b]As I explain more fully in Chap. 9, there are various versions of pragmatism, but usually the focus is on not trying to access some externally posited reality; hence I have added the word "experienced" here in order to highlight this point. (See Chap. 9, Sect. 9.5.)

On seeing my initial Table 1.2 in draft form, critical reader Francis Akena suggested that it is necessary to add the last phrase in the last column (namely, "spiritually symbiotic") because, as he stated (in a comment to me) "spirituality is the foundation upon which the wellbeing of most Indigenous societies are anchored". He indicated—in a personal communication, July 2016—that in Indigenous worldviews, domains of knowledge (as tied to practices) are all conceived in relation to spirituality, that is, in relation to sacred, metaphysical aspects of life. In other words, a spiritual approach infuses all aspects of knowing and existence. When I asked him if he could offer more detail, he inserted the following in this text (22 July 2016):

The economy is conceived and practiced in relation to spirituality in that holistic economic growth is considered in relation to spiritual awareness as expressed in songs, rituals, incantations and prayers. The idea among most African indigenous communities is that a value-based and well-packaged economic system is attainable when emphasis is laid on the welfare of the collective living and nonliving things in the physical and metaphysical world. Not only is the land meant to provide for human/animal/plants' wellbeing but the reverse is also true. Hence the economy is conceived as embracing the earth biodiversity—human to human, human to plants, human to land, human to animal, and human to metaphysical relations. Political and social aspects of existence are understood in relation to spirituality for both are believed to collapse in the absence of divine intervention. Divine blessings are the pinnacle of success for social and economic maneuvers and humans' relation with the (natural) environment is anchored in the spiritual. That has implications for ecological justice, in that the spiritual emphasizes the welfare of human and the natural environment. Usage of natural resources such as land and forests is predicated on continuity of ecosystems. Elsewhere (see Akena, 2017), I have argued that among most African traditional societies, land was treated as a bank account in which the account holder only withdraws profits accumulated without depleting the principal. Among the Acholi people of Uganda, identity is tied to land. The living and the spirits of the dead both live on the same land and have a responsibility to guard it against misuse. Likewise, all specific knowledge systems such as geography, mathematics, etc., have a spiritual undercurrent. That is, spirituality is central in imparting any of these disciplines to the young generation. In geography, for example, the spiritual component is espoused in how the invincible supernatural historically guided the ancestors of the present generation to where they are settled and adequately provided for them, and what it means to treat others and the environment with utmost dignity for fear of evoking the anger of the supernatural. Spirituality hence weaves through all social, economic, political and ecological aspects of life with their interdependency anchored on the spiritual.

Akena, with Wane and Ilmi (2014), is concerned that the spiritual aspects of life tend to be silent in dominant academic discourses. In their conclusion to their edited book *Spiritual discourse in the academy* (2014), Wane, Ilmi, and Akena argue that "conventional academic structures" have been set up to prevent spiritual conversations from taking place. This means that "Indigenous epistemologies grounded in spiritual ways of knowing" are discouraged from entering the academy (2014, p. 239). Resuscitating spiritual discourses is thus important to counteract paradigms founded on "principles of domination of non-Western ways of knowing" (p. 239). They see the project of encouraging spiritual conversation in the academy as part of "anti-colonial politics" (p. 239). Bongmba, as one of the authors of this edited book

(2014), underscores that spirituality is not to be considered as being linked to any particular religious tradition, but is more, as he sees it,

> a disposition toward the cosmos which exceeds one particular religious tradition. In the main, spirituality remains the deployment of thought and action toward all forms of relations between the individual (or community) toward the entire cosmos even though those actions take place in a concrete historical world. Spirituality (thought and action) is a way of life that is not determined by rules but guided by a good sense of the self, others, and the universe. (2014, p. 33)

Bongmba makes a case for regarding spirituality as an *intersubjective practice*, where the focus is on strengthening relationality:

> First, at the horizontal level, spirituality is an intersubjective engagement with other members of the community. Second, at the vertical level, spirituality is an intersubjective relation with ancestors and divine beings. Spiritual activities whether carried out in individual and communal rituals create spaces for participants to connect with one another. (2014, p. 36)

Regarding spirituality in this way means that space is indeed provided for intersubjective conversations—in the academy and in working with others "outside" the academy—around what it may mean to practice a way of life that is infused with a sense of connectivity. As far as research ethics is concerned, it would mean that all research work if understood (and practiced) as a spiritual practice, would try to encapsulate what Bongmba calls "an orientation taken by individuals and communities toward justice through a lifestyle that imagines the common good" (p. 33). He elaborates that "ethics [and I would add research ethics too] has come to mean a way of life that that has sought justice in a world where many have been oppressed, and humanity has ignored, or abused, the environment" (p. 33). Research practiced in terms of such a view of ethics would thus, in the terms described in the last column of Table 1.2, become "transformative toward (contextually defined) social and ecological justice, and spiritually symbiotic".

1.3.6 Permeable Boundaries Between Paradigms

Considering the boundaries between the various positions which she outlines, Mertens wishes to distinguish the transformative paradigm from, say, postpositivism and constructivism; but she also points out that boundaries between paradigms may be permeable (2014, p. 21). This is especially insofar as proponents exhibit a propensity to communicate and learn from one another. I have therefore used double dotted lines between the columns in Table 1.2 to denote the somewhat tenuous "separation" between paradigmatic markers. (And the whole table border is dotted, to denote space for others to consider further additions.[18])

[18]In regard to paradigmatic placement of the "new materialist" position which Kuntz explicates in his book on the *Responsible methodologist*, he indicates that although this approach to responsible

As far as the presentation of paradigmatic positions in Table 1.2 is concerned, I have argued in Sect. 1.3.2 that the transformative paradigm may benefit from appreciating the constructivist insistence that all that researchers can ever "find" are *ways of constructing visions of realities*. And constructivist-oriented authors can benefit from the axiological tenet that Mertens offers as a basis for deciding to which constructions researchers should feel allegiance to give more "voice", namely *marginalized ones* in the social fabric. This is also consistent with Indigenous authors' insistence on researchers being culturally accountable (to different voices and cultural forms of expression). (I have refrained from using the term "culturally sensitive", following Hart, who avers that when working with Indigenous participants one needs to move beyond "sensitivity" and to take into account "how research can be based in approaches and processes that are parts of our [Indigenous] cultures" (2010, p. 1).)

Meanwhile, Scott attempts to open a space for postpositivist-inclined researchers to communicate with more constructivist-oriented ones when she remarks that:

> There are very few adherents to epistemologies of objective knowledge. Quantitative researchers are not naïve positivists. They acknowledge the role of social construction in measures and are wary of quantification being seen as the equivalent of scientific reasoning. They know better than most that "statistics can lie". (2010, p. 233)

Once postpositivist-inclined authors adopt this position (regarding measures as constructions that contain certain presuppositions and decisions as to what to isolate for attention), there is clearly room for further discussion around the tenets (and application of tenets) of postpositivist- and constructivist-oriented paradigms (see also Romm, 2013b).

In reviewing the conduct of (post)positivist research in Māori research contexts, Smith makes the point that a positivistic-oriented research task (exploring connections between variables) is not precluded within a Kaupapa Māori framework (1999, p. 190). As long as the accountabilities of researchers to communities being served have been brought into consideration and to some extent "transformed the approach to research", one can admit such an approach into the research repertoire. This would also mean, for example, making efforts to render open to discussion and review within communities the findings/results, on the understanding that this

---

inquiry includes assumptions about how we "live, know and come to know", he is not intent on trying to "capture or otherwise render the totality of a new materialist paradigm" (2015, p. 82). He makes the point that in "true new materialist form" (which focuses on how the world is always open to new contexts and meanings), he recognizes that "paradigmatic boundaries are never fixed nor fully developed" (2015, p. 82). His intention in outlining a new materialist position (as an alternative paradigm) is to "point out how the recent embrace of new materialism in some circles presents methodological possibilities that cannot be ignored" (p. 82). In this book I have not focused on the distinctiveness of this position as a paradigm, but have cited his work as it relates to the various paradigms that I do discuss. In the way in which I see it, his work resonates with the sentiments of transformative paradigmatic positions. And I concur with his call for responsible methodologists not to become fixated on methodology as "procedure". I explore this further in Chap. 10.

discussion can contribute to defining the import of the constructions, albeit that this is not normally admitted within "positivist" research reporting. This implies a stretching of that which is normally associated with positivism/postpositivism.

Regarding her location of an Indigenous research paradigm in relation to "other" research orientations, Chilisa reminds us that we need not conceive paradigms in such a way that they compel thought along what seem to be exclusionary opposites (2012, p. 25). The dotted lines in Table 1.2 also emphasize this point. In short, I suggest that one does not need to see oneself as working *within* paradigms, but can also work *with* them as part of the process of defining socially relevant ways of proceeding.[19] In this way, if we wish to consider our research (and social) agenda as "transformative" we can still reconsider various paradigmatic views for developing pathways into responsibly practicing social research. (My Table 9.1 in Chap. 9 offers an extended presentation of how various paradigmatic positions, when stretched, can make provision for more responsible practices.)

To proceed with my reflections around responsible research practice in this book, I do this with reference to some research projects in which I have been involved and also with reference to examples and interpretations of others' work. The book can be seen as a follow-up of Mertens' suggestion (2009, p. 42) that there are few texts that explore in depth the research implications of the transformative paradigm (which she sees as bearing a close relationship to Indigenous research agendas). Mertens furthermore suggests that in order to promote research that can help address problems that "involve multiple interacting systems, [and] are replete with social and institutional uncertainties" (which have been named as "wicked problems" by Webber & Rittel, 1973), "the researcher's role *needs to be expanded in terms of social responsibility*" (2016, p. 15, my italics). In this book, I am exploring this idea from the angle of focusing, with reference to a detailed examination of chosen examples, on how research can be practiced responsibly.

For my examples, I selected research cases with the intent to illustrate how the initiating researchers could be considered as operating (with research participants/participant researchers and others involved) in terms of an "expanded" sense of responsibility, which I highlight and further extend as I engage with their texts and other communications. Nearly all of the examples which I have used included some personal communications—face to face and/or virtual—with these researchers in relation to their research involvements, in order that I could offer the detail that I felt was needed. Also, readers will notice that I have chosen as examples research that draws on "commonly used" research methods. This is so that the book can be of relevance for the many researchers using such methods. My aim is to offer illustrations of how these methods can be re-tuned creatively to make provision for an extended conception of researcher responsibilities. (In my concluding chapter I do, however, also refer to the use of some other methods.)

[19]This terminology of working *with* rather than *within* paradigmatic boundaries has been proposed by McIntyre-Mills (2006, pp. 20 and 81).

Chapter 2 is devoted to the case of my involvement in setting up what would be called in the research literature a focus group session/talking circle with participants in an apartment complex in which I lived, followed up with further one-to-one and group conversations (starting in 2007). The topic of the (cross-racial) discussion was race(d) relations in South Africa. I concentrate on how I interpret this research —via my post facto reflection now—as viewed through the lens of responsible research practice.

Chapter 3 refers to two examples of research concerning inclusive education. One of these is set in an international context (involving six countries, including China, Finland, Lithuania, Slovenia, South Africa and the United Kingdom, 2012–2014). The other was a national South African project (2013–2015). In both cases, teamwork was involved, with my being part of the team (of university-based researchers); and in both cases, questionnaires and focus groups were employed at different stages of the project. I use these examples to deliberate upon how questionnaires as well as focus group sessions might be deployed responsibly and actively with transformative intent, with participant researchers and stakeholders becoming part of the process.

In Chap. 4, I discuss two examples of research where gendered relationships were explored in what I call an active manner. One example (reported upon by Belete Woldegies, 2014) is set in Ethiopia; and one example (reported upon by Ssali & Theobald, 2016) is set in Uganda. I use the first example to point to the roles assumed by Woldegies and participant researchers (women and men), which included using active interviewing and focus group sessions toward trying to shape emergent possibilities for women's empowerment. I examine the second example, which involved interviewing participants (again both women and men) in relation to their experiences of the 20-year war in Northern Uganda, in order to draw out features of responsible research practice not explored in the first example. (In both cases I had contact with the authors; in the former case, with Woldegies, this was via email and Skype, and in the latter case, mainly with Ssali, this was via email.)

In Chap. 5, I turn to two examples of research experiments, where "intervention effects" were assessed via questionnaires. The first example is Oluyemi Stephens' experiment undertaken with recidivists in a number of Nigerian prisons. Although Stephens' text (2012) is organized to display his work as "scientific" in terms of a genre of postpositivism, in-depth conversations that we had face to face showed (as I interpret them) that he saw his responsibilities more broadly. I use the example to extrapolate on how experimentation can be practiced in a responsible way, with a view to recognizing the shaping effects of social research. The second example which I discuss in this chapter is the experiment (2007) by Oczak and Niedźwieńska, set in Poland, which involved their specific way of debriefing participants after the experiment (indeed as part of the experiment). I use this example to reflect on responsible debriefing practices. (Since I was unable to make contact with either Oczak or Niedźwieńska, my deliberations are based solely on my reading of their text.)

In Chap. 6, I discuss three examples of research that were explicitly oriented to addressing ecological justice issues alongside social justice. The first one was

initiated by McIntyre-Mills in South Australia (2011), with a view to putting on the agenda in local government and more widely, the possibility of shifting "business as usual" discourses in relation to environmental concerns. She calls this a participatory action research project, aimed at generating transformative action via the research. I discuss how she used a computer software package (designed with participants and stakeholders in an earlier project and extended to this project) to organize virtual and face to face research processes, meant to be change oriented toward increased social and environmental justice. (I had many personal communications with McIntyre-Mills in relation to this project.) I then turn to the write-up of Antonio Roman-Alcalá in relation to the activist attempt in California (2012) to "Occupy the Farm (OTF)" which was part of the University of California, so that it could be used for sustainable food security purposes. His article write-up (e.g., 2015) and other processes of dissemination offer his way of analyzing the OTF initiative, based also on his discussion with a number of other activists. Roman-Alcalá also provides—at the end of the section of the chapter where I detail my interpretation of the case—some extra analyses/commentary which he constructed for the purposes of this book (after I had sent via email to him my draft of my discussion of his work). Finally, I turn to the autobiographical account of the late Maathai (2006) entitled *Unbowed*, centered around the setting up of the Green Belt Movement as initiated in Kenya. I consider this memoir—where she narrates her research-in-action with others—by characterizing it as responsible autoethnographical narration.

Chapter 7 offers additional deliberations on all of the examples discussed in previous chapters, with an in-depth focus on how generative theorizing can be practiced responsibly. I develop further Gergen's conception of this by linking it to retroductive inferencing, which I explore with reference to, while extending, a number of other authors' conceptions of this. I discuss links between generative theorizing and an active (practically oriented) "grounded theorizing", where validity becomes defined in transformative terms (in keeping with the notion of theorizing as world-shaping). I locate links between retroduction and Indigenous styles of knowing.

Chapter 8 expands the discussion on "ethics" in previous chapters, and offers additional deliberations on practicing ethical responsibility. I organize the chapter around a discussion of the USA-commissioned Belmont report (1979), which offers three principles to guide the practice of research ethics, and attendant suggested applications. This report has been highly influential in the way in which Institutional Review Boards (IRBs) or ethics review committees worldwide review/regulate the work of researchers proposing to conduct any kind of research involving human participants. In the light of this, I re-look at the principles and at certain authors' proposals for extending/revising the norms embodied in the report. Further to this, I offer my own reconfiguration of them, with detailed reference to Chaps. 2–6, which I suggest all can be used to illustratively draw out various nuances of the reconfigured principles. I also offer my own story of my attempt to raise issues at the University of South Africa connected to the "regulation" of researchers by IRBs (called ethical review boards at the university), which includes my later becoming a member of a particular review board.

In Chap. 9, I focus on justifications for practices of combining methods in multiple and mixed methods research (MMMR). I pay attention to various paradigmatic positions that can be invoked for such justification. In the course of discussing paradigmatic positions, I point to how paradigms in any case are neither unitary nor static: I offer suggestions for the potential stretching of all the paradigmatic views that appear in Table 1.2, in view of a focus on responsible research practice. I then re-look at Chaps. 2–6 in the light of paradigmatic considerations around the meaning(s) of MMMR and of paradigmatic debates more generally. At the same time, I put forward suggestions for how "research questions" can be constructed such that responsibility and co-responsibility for the consequential character of research can become better provided for.

Finally, in Chap. 10, I conclude the book by suggesting that in explanations of research endeavors undertaken (for readers reading accounts of the research process and its justification), more attention should be focused on storying the interactions between the various parties involved in the research inquiries, and to how they might have exercised a sense of responsibility and co-responsibility, in the spirit that I have elucidated throughout the book. I submit that action-oriented inquiry, or what I call *active research*, can be accommodated in all research designs, which do not have to be conceived (and subsequently written up) as "finding out" exercises. I thus suggest that we can reconfigure what Creswell (2003, 2014) (and many before him) notes are "frequently used" designs and methods in social science research; again I provide practical illustrations with reference to Chaps. 2–6. I also offer some brief deliberations around less frequently used research options than ones that are currently popular.

References

Akena, F. A. (2017). Customary land tenure and ecological sustainability in Acholi land, Northern Uganda. In J. J. McIntyre-Mills, Y. Cocoran-Nantes, & N. R. A. Romm (Eds.), *Balancing individualism and collectivism: Social and environmental justice* (pp. 221–237). New York: Springer.

Aphane, M. J. D., Dlamini, T., Magwaza, M., Manzini, N., Mkhonta, F., Mthembu, L., & Vilakazi, M. L. (1993). *Inheritance in Swaziland: Law and practice*. Report co-ordinated by M. J. D. Aphane. Swaziland: Social Science Research Unit, University of Swaziland.

Austin, J. L. (1962). *How to do things with words*. Oxford: Clarendon.

Bausch, K. (2016). *Back stories for robust postmodern living*. Litchfield Park, AZ: Emergent Publications.

Bausch, K. C., & Flanagan, T. R. (2013). A confluence of third-phase science and dialogic design science. *Systems Research and Behavioral Science, 30*(4), 414–429.

Bishop, R. (1994). Initiating empowering research? *New Zealand Journal of Educational Studies, 29*(1), 175–188.

Bongmba, E. K. (2014). Spirituality: An intersubjective practice. In N. N. Wane, F. A. Akena, & A. A. Ilmi (Eds.), *Spiritual discourse in the academy* (pp. 33–50). New York: Peter Lang.

Bowers, T. D. (2011). Toward a framework for multiparadigm multimethodologies. *Systems Research and Behavioral Science, 28*(5), 537–552.

Bristol, L. (2012). Postcolonial thought: A theoretical and methodological means for thinking through culturally ethical research. In J. Lavia & S. Mahlomaholo (Eds.), *Culture, education, and community: Expressions of the postcolonial imagination* (pp. 15–32). New York: Palgrave Macmillan.

Cannella, G. S., & Manuelito, K. D. (2008). Feminisms from unthought locations: Indigenous worldviews, marginalized feminisms, and revisioning an anticolonial social science. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and Indigenous methodologies* (pp. 45–59). London: Sage.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., Major, T. E., & Khudu-Petersen, K. (2017). Community engagement with a postcolonial, African-based relational paradigm. *Qualitative Research*, 1–14. https://doi.org/10.1177/1468794117696176.

Chilisa, B., & Tsheko, G. N. (2014). Mixed methods in Indigenous research: Building relationships for sustainable intervention outcomes. *Journal of Mixed Methods Research, 8*(3), 222–233.

Christakis, A. N. (2004). Wisdom of the people. *Systems Research and Behavioral Science, 21*(5), 479–488.

Collins, P. H. (2000). *Black feminist thought: Knowledge, consciousness and the politics of empowerment* (2nd ed.). London: Harper Collins.

Cram, F. (2015). Harnessing global social justice and social change with multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 677–687). Oxford: Oxford University Press.

Cram, F., Chilisa, B., & Mertens, D. M. (2013). The journey begins. In D. M. Mertens, F. Cram, & B. Chilisa (Eds.), *Indigenous pathways into social research* (pp. 11–40). Walnut Creek, CA: Left Coast Press.

Cram, F., & Mertens, D. M. (2015). Transformative and Indigenous frameworks for multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 91–109). Oxford: Oxford University Press.

Creswell, J. W. (2003). *Research design: Qualitative, quantitative, and mixed methods approaches* (2nd ed.). Thousand Oaks: Sage.

Creswell, J. W. (2014). *Research design: Qualitative, quantitative, and mixed methods approaches* (4th ed.). Thousand Oaks, CA: Sage.

Denzin, N. K. (2001). The reflexive interview and a performative social science. *Qualitative Research, 1*(1), 23–46.

Denzin, N. K. (2003). *Performance ethnography: Critical pedagogy and the politics of culture*. Thousand Oaks, CA: Sage.

Denzin, N. K., & Lincoln, Y. S. (2003). Introduction: The discipline and practice of qualitative research. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (pp. 1–45). Thousand Oaks, CA: Sage.

Dillard, C. B. (2006). When the music changes, so should the dance: Cultural and spiritual considerations in paradigm "proliferation". *International Journal of Qualitative Studies in Education, 19*(1), 59–76.

Flanagan, T. R., & Christakis, A. N. (2009). *The talking point: creating an environment for exploring complex meaning*. Charlotte, NC: Information Age.

Flood, R. L., & Romm, N. R. A. (1996). Emancipatory practice: Some contributions from social theory and practice. *Systems Practice, 9*(2), 113–128.

Gergen, K. J. (1978). Toward generative theory. *Journal of Personality and Social Psychology, 36*(11), 1344.

Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.

Giddens, A. (1984). *The constitution of society: Outline of the theory of structuration*. Berkeley, CA: University of California Press.

Goduka, N. (2012). Re-discovering Indigenous knowledge—ulwazi Lwemveli for strengthening sustainable livelihood opportunities within rural contexts in the Eastern Cape province. *Indilinga—African Journal of Indigenous Knowledge Systems, 11*(1), 1–19.

Gonzalez, P. A., Minkler, M., Garcia, A. P., Gordon, M., Garzón, C., Palaniappan, M., et al. (2011). Community-based participatory research and policy advocacy to reduce diesel exposure in West Oakland. *California. American Journal of Public Health, 101*(S1), S166–S175.

Guba, E. G., & Lincoln, Y. S. (1989). *Fourth generation evaluation*. Thousand Oaks, CA: Sage.

Harris, L. D., & Wasilewski, J. (2004). Indigeneity, an alternative worldview: Four R's (relationship, responsibility, reciprocity, redistribution) vs. two P's (power and profit). Sharing the journey toward conscious evolution. *Systems Research and Behavioral Science, 21*(5), 489–503.

Hayes, M. T., Sameshima, P., & Watson, F. (2015). Imagination as method. *International Journal of Qualitative Methods, 14*(1), 36–52.

Hölscher, F., & Romm, N. R. A. (1989). Development as a process of human consciousness. In J. K. Coetzee (Ed.), *Development is for people* (pp. 108–136). Johannesburg: Macmillan.

Johnson, R. B. (2009). Comments on Howe: Toward a more inclusive "scientific research in education". *Educational Researcher, 38*(6), 449–457.

Kaschula, S. A., Twine, W. E., & Scholes, M. C. (2005). Coppice harvesting of fuelwood species on a South African common: Utilizing scientific and indigenous knowledge in community based natural resource management. *Human Ecology, 33*(3), 387–418.

Kenny, C. (2002). *North American Indian, Métis and Inuit women speak about culture, education and work*. Canada: Status of Women Canada's Policy Research.

Koitsiwe, M. (2013). Prospects and challenges of becoming an Indigenous researcher. In D. M. Mertens, F. Cram, & B. Chilisa (Eds.), *Indigenous pathways into social research* (pp. 261–275). Walnut Creek, CA: Left Coast Press.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.

Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.

Kvale, S. (2002). The social construction of validity. In N. K. Denzin & Y. S. Lincoln (Eds.), *The qualitative inquiry reader* (pp. 297–322). Thousand Oaks, CA: Sage.

Ladson-Billings, G. (2003). Racialized discourses and ethnic epistemologies. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 398–432). London: Sage.

Laouris, Y. (2014). Teams construct knowledge during project-driven social interactions. *Educating in Dialog: Constructing Meaning and Building Knowledge with Dialogic Technology, 24,* 111–113.

Laouris, Y., & Michaelides, M. (2017). Structured democratic dialogue: An application of a mathematical problem structuring method to facilitate reforms with local authorities in Cyprus. *European Journal of Operational Research*, 11–12. https://doi.org/10.1016/j.ejor.2017.04.039.

Lather, P. (1986). Issues of validity in openly ideological research: Between a rock and a soft place. *Interchange, 17*(4), 63–84.

Lather, P. (1992). Critical frames in educational research: Feminist and post-structural perspectives. *Theory into Practice, 31*(2), 87–99.

Lavia, J., & Mahlomaholo, S. (2012). Introduction: Imagining the postcolonial. In J. Lavia & S. Mahlomaholo (Eds.), *Culture, education and community: Expressions of the postcolonial imagination* (pp. 1–13). New York: Palgrave Macmillan.

Letiecq, B., & Schmalzbauer, L. (2012). Community-based participatory research with Mexican migrants in a new rural destination: A good fit? *Action Research, 10*(3), 244–259.

Lincoln, Y. S. (1995). Emerging criteria for quality in qualitative and interpretive research. *Qualitative Inquiry, 1*(3), 275–289.

Lincoln, Y. S. (2001a). Engaging sympathies: Relationships between action research and social constructivism. In P. Reason & H. Bradbury (Eds.), *Handbook of action research: Participative inquiry and practice* (pp. 124–132). London: Sage.

Lincoln, Y. S. (2001b). Varieties of validity: Quality in qualitative research. In J. Smart & W. Tierney (Eds.), *Higher education: Handbook of theory and research* (pp. 25–72). New York: Agathon Press.

Lincoln, Y. S., & Guba, E. G. (2003). Paradigmatic controversies, contradictions, and emerging confluences. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 253–291). Thousand Oaks, CA: Sage.

Lincoln, Y. S., & Guba, E. G. (2013). *The constructivist credo*. Walnut Creek, CA: Left Coast Press.

Lowan-Trudeau, G. (2014). Considering ecological métissage: To blend or not to blend*?*. *Journal of Experiential Education, 37*(4), 351–366.

Maathai, W. (2006). *Unbowed: A memoir*. New York: Anchor Books.

Mackenzie, N., & Knipe, S. (2006). Research dilemmas: Paradigms, methods and methodology. *Issues in Educational Research, 16*(2), 193–205.

McIntyre-Mills, J. J. (2006). *Systemic governance and accountability: Working and re-working the conceptual and spatial boundaries*. New York: Springer.

McIntyre-Mills, J. J. (2008). Systemic ethics: Social, economic and environmental implications of eating our yellow cake in South Australia. *Systems Research and Behavioral Science, 25*(2), 225–248.

McIntyre-Mills, J. J. (2014a). *Systemic ethics and non-anthropocentric stewardship*. New York: Springer.

McIntyre-Mills, J. J. (2014b). *Transformation from Wall Street to wellbeing*. New York: Springer.

McIntyre-Mills, J. J. (2017). *Planetary passport: Representation, accountability and re-generation*. New York: Springer.

McIntyre-Mills, J. J., & Binchai, N. (2014). Being, having, doing and interacting: Toward ethical democracy, governance and stewardship. In *Proceedings of the 57th Annual Meeting of the ISSS-2013*, Hai Phong, Vietnam.

McKay, V. I., & Romm, N. R. A. (2008). Active research toward the addressal of HIV/AIDS in the informal economy in Zambia: Recognition of complicity in unfolding situations. *Action Research, 6*(2), 149–170.

McKay, V. I., & Romm, N. R. A. (2015). Narratives of agency: The experiences of Braille literacy practitioners in the Kha Ri Gude South African mass literacy campaign. *International Journal of Inclusive Education, 19*(4), 435–456.

Mertens, D. M. (1999). Inclusive evaluation: Implications of transformative theory for evaluation. *American Journal of Evaluation, 20*(1), 1–14.

Mertens, D. M. (2004). *Research and evaluation in education and psychology* (2nd ed.). Thousand Oaks, CA: Sage.

Mertens, D. M. (2005). *Research methods in education and psychology* (2nd ed.). Thousand Oaks, CA: Sage.

Mertens, D. M. (2007a). Transformative paradigm: Mixed methods and social justice. *Journal of Mixed Methods Research, 1*(3), 212–225.

Mertens, D. M. (2007b). Transformative considerations: Inclusion and social justice. *American Journal of Evaluation, 28*(1), 86–90.

Mertens, D. M. (2009). *Transformative research and evaluation*. New York, NY: Guilford Press.

Mertens, D. M. (2010). Transformative mixed methods research. *Qualitative Inquiry, 16*(6), 469–474.

Mertens, D. M. (2011). Mixed methods as tools for social change. *Journal of Mixed Methods Research, 5*(3), 195–197.

Mertens, D. M. (2012). Transformative mixed methods: Addressing inequities. *American Behavioral Scientist, 56*(6), 802–813.

Mertens, D. M. (2014). *Research and evaluation in education and psychology* (4th ed.). Thousand Oaks, CA: Sage.

Mertens, D. M. (2016). Advancing social change in South Africa through transformative research. *South African Review of Sociology, 47*(1), 5–17.

Mertens, D. M., & McLaughlin, J. A. (2004). *Research and evaluation methods in special education*. Thousand Oaks, CA: Sage.

Midgley, G. (2000). *Systemic intervention: Philosophy, methodology and practice*. New York: Kluwer Academic/Plenum.

Midgley, G. (2001). Systems thinking for the 21st century. In G. Ragsdell & J. Wilby (Eds.), *Systems thinking for the 21st century: Understanding complexity* (pp. 249–256). New York: Kluwer Academic/Plenum.

Midgley, G., Ahuriri-Driscoll, A., Foote, J., Hepi, M., Taimona, H., Rogers-Koroheke, M., et al. (2007). Practitioner identity in systemic intervention: Reflections on the promotion of environmental health through Māori community development. *Systems Research and Behavioral Science, 24,* 233–247.

Midgley, G., Johnson, M. P., & Chichirau, G. (2017). What is community operational research? *European Journal of Operational Research* (in press).

Midgley, G., & Ochoa-Arias, A. E. (2001). Unfolding a theory of systemic intervention. *Systemic Practice and Action Research, 14*(5), 615–649.

Molefe, M. (2015). A rejection of humanism in African moral tradition. *Theoria, 62*(143), 59–77.

Morgan, D. L. (2014). Pragmatism as a paradigm for social research. *Qualitative Inquiry, 20*(8), 1045–1053.

Mugadza, G. (2015). Systems thinking and design thinking: Complementary approaches. *Systems Thinking World Journal: Reflection in Action, 4,* 1–7.

Murove, M. F. (2005). *The theory of self-interest in modern economic discourse: A critical study in the light of African humanism and process philosophical anthropology*. Doctoral thesis, University of South Africa, Pretoria.

Murove, M. F. (2007). The Shona ethic of Ukuma with reference to the immortality of values. *The Mankind Quarterly, 48,* 179–189.

Ndimande, B. S. (2012). Decolonizing research in post-apartheid South Africa. *Qualitative Inquiry, 18*(3), 215–226.

Nyamnjoh, F. B. (2004). A relevant education for African development: Some epistemological considerations. *Africa Development, 29*(1), 161–184.

Oczak, M., & Niedźwieńska, A. (2007). Debriefing in deceptive research: A proposed new procedure. *Journal of Empirical Research on Human Research Ethics, 2*(3), 49–59.

Ossai, N. B. (2010). African indigenous knowledge systems (AIKS). *Simbiosis, 7*(2), 1–13.

Pinnegar, S. (2007). Starting with living stories. In D. J. Clandinin (Ed.), *Handbook of narrative inquiry: Mapping a methodology* (pp. 247–250). London: Sage.

Polanyi, K. (1944). *The great transformation: The political and economic origins of our time*. New York: Farrar & Rinehart.

Pushor, D., & Clandinin, D. J. (2009). The interconnections between narrative inquiry and action research. In S. E. Noffke & B. Somekh (Eds.), *Handbook of educational action research* (pp. 290–300). London: Sage.

Quan-Baffour, K. P. (2017). A systemic view of the value of environmental conservation: The case of Bono Takyiman, Ghana. In J. J. McIntyre-Mills, Y. Cocoran-Nantes, & N. R. A. Romm (Eds.), *Balancing individualism and collectivism: Social and environmental justice* (pp. 211–220). New York: Springer.

Quan-Baffour, K. P., & Romm, N. R. A. (2015). Ubuntu-inspired training of adult literacy teachers as a route to generating "community" enterprises. *Journal of Literacy Research, 46*(4), 455–474.

Robertson, A. F. (2001). *Greed: gut feelings, growth, and history*. Oxford: Blackwell.

Roman-Alcalá, A. (2015). Broadening the land question in food sovereignty to northern settings: A case study of occupy the farm. *Globalizations, 12*(4), 545–558.

Romm, N. R. A. (1990). A humanist analysis of Marxism. In C. J. Alant (Ed.), *Sociology and society* (pp. 23–33). Johannesburg: Southern.

Romm, N. R. A. (1995). Knowing as intervention: Reflections on the application of systems ideas. *Systems Practice, 8*(2), 137–167.

Romm, N. R. A. (1996a). Critical theory as a way to understand development. In J. Coetzee & J. Graaff (Eds.), *Development and reconstruction in the new South Africa* (pp. 160–195). Halfway House (South Africa): International Thomson.

Romm, N. R. A. (1996b). Systems methodologies and intervention: The issue of researcher responsibility. In R. L. Flood & N. R. A. Romm (Eds.), *Critical systems thinking: Current research and practice* (pp. 179–193). New York: Plenum.

Romm, N. R. A. (1997). Becoming more accountable: A comment on Hammersley and Gomm. *Sociological Research Online, 2*(3) (Art. 2).

Romm, N. R. A. (1999). Exploring links between action research (AR) and "alternatives". In A. W. Castell, A. J. Gregory, G. A. Hindle, M. E. James, & G. Ragsdell (Eds.), *Synergy matters: Working with systems in the 21st Century* (pp. 199–204). New York: Springer.

Romm, N. R. A. (2001a). *Accountability in social research: Issues and debates*. New York: Springer.

Romm, N. R. A. (2001b). Critical theoretical concerns in relation to development. In J. K. Coetzee, J. Graaff, F. Hendricks, & G. Wood (Eds.), *Development theory, policy and practice* (pp. 141–153). Cape Town: Oxford University Press.

Romm, N. R. A. (2002a). A trusting constructivist approach to systemic inquiry: Exploring accountability. *Systems Research and Behavioral Science, 19*(5), 455–467.

Romm, N. R. A. (2002b). Responsible knowing: A better basis for management science. *Philosophy of Management, 2*(1), 57–72.

Romm, N. R. A. (2006a). The social significance of Churchman's epistemological position. In J. J. McIntyre-Mills (Ed.), *Rescuing the Enlightenment from itself* (pp. 68–92). New York: Springer.

Romm, N. R. A. (2006b). An exploration and extension of Churchman's insights: Toward the tackling of racial discrimination as a world problem. In J. J. McIntyre-Mills (Ed.), *Rescuing the Enlightenment from itself* (pp. 289–331). New York: Springer.

Romm, N. R. A. (2007). Issues of accountability in survey, ethnographic, and action research. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa: Ethical issues* (pp. 51–76). Addis Ababa: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York: Springer.

Romm, N. R. A. (2013a). Employing questionnaires in terms of a constructivist epistemological stance: Reconsidering researchers' involvement in the unfolding of social life. *International Journal of Qualitative Methods, 12,* 652–669.

Romm, N. R. A. (2013b). Revisiting social dominance theory: Invoking a more retroductively-oriented approach to systemic theorizing. *Systemic Practice and Action Research, 26*(2), 111–129.

Romm, N. R. A. (2014a). Indigenous ways of knowing and possibilities for re-envisaging globalization: Implications for human ecology. *Journal of Human Ecology, 48*(1), 123–133.

Romm, N. R. A. (2014b). Active and accountable social inquiry: Implications and examples. *Participatory Educational Research, 1*(2), 13–20.

Romm, N. R. A. (2015a). Reviewing the transformative paradigm: A critical systemic and relational (Indigenous) lens. *Systemic Practice and Action Research, 28*(5), 411–427.

Romm, N. R. A. (2015b). Conducting focus groups in terms of an appreciation of Indigenous ways of knowing: Some examples from South Africa. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research, 16*(1) (Art. 2).

Romm, N. R. A. (2017a). Social dominance theory. In B. S. Turner (Ed.), *Encyclopedia of social theory*. Chichester, England: Wiley.

Romm, N. R. A. (2017b). Conducting focus groups in terms of an appreciation of Indigenous ways of knowing: Invoking an Indigenous-oriented paradigm. In P. Liamputtong (Ed.), *Doing cross-cultural research in health social sciences*. New York: Springer. (in press).

Romm, N. R. A. (2017c). Reflections on a multi-layered intervention in the South African public education system: Some ethical implications for community operational research. *European Journal of Operational Research*, 1–13. https://doi.org/10.1016/j.ejor.2017.02.022.

Romm, N. R. A. (2017d). Foregrounding critical systemic and Indigenous ways of collective knowing toward (re)directing the Anthropocene. In J. J. McIntyre-Mills, Y. Cocoran-Nantes, & N. R. A. Romm (Eds.), *Balancing individualism and collectivism: Social and environmental justice* (pp. 1–17). New York, NY: Springer.

Romm, N. R. A. (2017e). Researching Indigenous ways of knowing-and-being: Revitalizing relational quality of living. In P. Ngulube (Ed.), *Handbook of research on theoretical perspectives on indigenous knowledge systems in developing countries* (pp. 22–48). Hershey, PA: IGI Global.

Romm, N. R. A., & Adman, P. (2004). Exploring the complexity of human dynamics within 360-degree feedback processes: The development of (active) qualitative inquiry. *Journal of Business and Society, 17,* 170–189.

Romm, N. R. A., & Dichaba, M. M. (2015). Assessing the Kha Ri Gude Mass Literacy Campaign: A developmental evaluation. *Australian Journal of Adult Learning, 55*(2), 220–241.

Romm, N. R. A., & Hsu, C.-Y. (2002). Reconsidering the exploration of power distance: An active case study approach. *Omega, 30*(6), 403–414.

Romm, N. R. A., Nel, N. M., & Tlale, L. D. N. (2013). Active facilitation of focus groups: Exploring the implementation of inclusive education with research participants. *South African Journal of Education, 33*(4), 1–14. (Art. 811).

Romm, N. R. A., & Tlale, L. D. N. (2016). Nurturing research relationships: Showing care and catalysing action in a South African school research-and-intervention project. *South African Review of Sociology, 47*(1), 18–36.

Roos, V. (2008). The Mmogo-method™: Discovering symbolic community interactions. *Journal of Psychology in Africa, 18*(4), 659–667.

Ross, E. (2010). African spirituality, ethics and traditional healing: Implications for Indigenous South African social work education and practice. *South African Journal of Business and Law, 3*(1), 44–51.

Scott, J. (2010). Quantitative methods and gender inequalities. *International Journal of Social Research Methodology, 13*(3), 223–236.

Shaw, I. (2010). Ethics and the practice of qualitative research. In P. Atkinson & S. Delamont (Eds.), *Qualitative research methods* (pp. 401–427). London: Sage.

Shiakides, S. (2010). *Practical rationality with a practical intent: A revised Habermasian conception centered on communicative negotiation*. Cardiff: Cardiff University Press.

Smith, L. T. (1999). *Decolonizing methodologies: Research and Indigenous peoples*. New York: Zed Books.

Ssali, S. N., & Theobald, S. (2016). Using life histories to explore gendered experiences of conflict in Gulu District, Northern Uganda: Implications for post-conflict health reconstruction. *South African Review of Sociology, 47*(1), 81–98.

Stephens, O. A. (2012). *Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, Nigeria*. Doctoral dissertation, Lagos State University, Lagos, Nigeria.

Tengo, M., Brondizio, E. S., Elmqvist, T., Malmer, P., & Spierenburg, M. (2014). Connecting diverse knowledges for enhanced ecosystem governance: The multiple evidence base approach. *Ambio, 43,* 579–591.

Tlale, L. D. N., & Romm, N. R. A. (2017). Systemic thinking and practice toward facilitating inclusive education: Reflections on a case of co-generated knowledge and action in South Africa. *Systemic Practice and Action Research*. doi:10.1007/s11213-017-9437-4.

Troncale, L. (2004–2007). *Courses in systems science at the California State University at Pomona for students of Comparative Systems Analysis*. Unpublished lectures.

Wane, N. N., Akena, F. A., & Ilmi, A. A. (2014). Introduction. In N. N. Wane, F. A. Akena, & A. A. Ilmi (Eds.), *Spiritual discourse in the academy* (pp. 1–11). New York: Peter Lang.

White, L. (2003). The role of systems research and operational research in community involvement: A case study of a health action zone. *Systems Research and Behavioral Science, 20*(2), 133–145.

Wilson, S. (2008). *Research is ceremony: Indigenous research methods*. Halifax: Fernwood.

Wilson, S., & Wilson, A. (2013). Neyo way in ik issi: A family practice of Indigenous research informed by land. In D. M. Mertens, F. Cram, & B. Chilisa (Eds.), *Indigenous pathways into social research* (pp. 333–352). Walnut Creek, CA: Left Coast Press.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.

Chapter 2
Active Focus Group Research with Follow-up Interviews/Conversations and Actions: Responsibly Re-exploring Race(d) and Classed Relations

Abstract In this chapter I consider some possibilities for responsible research practice geared to reviewing race(d) relations (and their link to classed relations). I discuss in depth an example of research in South Africa which involved initially setting up a cross-racial focus group, which was then followed up with one-to-one interviews/conversations and group conversations. The research is set in Margate, and was initiated by me. Because this example concerns issues of "race" and racism in South Africa, I open the chapter with a narration of my learning the meaning of Whiteness as a child in the context of apartheid South Africa, which forms a backdrop to my further storying in this chapter. Through this example, I wish to show how the banner of research (re-exploring issues not normally discussed in everyday life) allowed discussions to become generated and actions to become pursued that (arguably) otherwise would not have occurred if the research had not been embarked upon. I also show in this case how I tried to "give back" as part of my responsibility to nurture a relationship of reciprocity, while also being involved with participants in a wider effort to create forward-looking stories (for readers to engage with).

2.1 Introduction

This chapter explores in depth an example of employing in an active manner focus group (FG) research, combined with follow-up interviews/conversations. The example is set in Margate, South Africa—a seaside town which is popular as a holiday destination, with much business being directed to this end. The example presented in this chapter is based on research that I initiated to generate cross-racial discussion around "race" (as a construct) and racism (where the participants

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_2

strongly foregrounded class relations too).[1] I have chosen to start this as my first example in the book as it offers readers a glimpse of my attempts at "being-in relation" during this research process, so that readers can also better understand my way of treating and interpreting the examples in the rest of the book in terms of relational thinking and practice.

In regard to the appropriateness in this context of beginning the research process with a FG-type session, this was in keeping with Chilisa's indication of the value placed on "talking circles" within relational perspectives as developed, inter alia, in parts of North America, Canada and Africa (2012, p. 121). Kovach also refers to "research-sharing circles" as a method of "engendering story", in which participants can "share their story in a manner that they can direct" (2009, p. 124). She argues that many Indigenous authors prefer not to label such research as FG research, but indeed as *research-sharing circles*, especially insofar as they invite the sharing of stories, which is based on Indigenous cultural traditions for "gathering group knowledge" (2009, p. 124).

In discussing this first example, I pay attention to my role in setting up the FG/research circle meeting and in inviting follow up interviews/conversations, and I show the "negotiations" that were involved in this process. This concurs with Clandinin and Caine's point that narrative research—with the emphasis on the storywork of both the participants and the initiating researcher(s)—"begins with negotiation of relationships and the research puzzles to be explored" (2008, p. 542). Smith (1999, p. 159) indicates that "Indigenous rules of negotiation usually contain both rituals of respect and protocols for discussion". The negotiations with potential participants in the research process offer an indication of how respect was instantiated in this case, based also on a process of (what I experienced as) trust building.

During the research process I was careful to define the inquiry as being a collective undertaking of us all re-exploring the issues together. Because the research concerned issues of racism it can be said to have placed, in Mertens' terms,

> central importance on the lives and experiences of communities that are pushed to society's margins, e.g. women, racial, ethnic minorities, people with disabilities, those who are poor, and more generally people in nondominant cultural groups. (2009, p. 48)

But besides focusing on those "pushed to society's margins" through the South African apartheid system, I offer an indication of how the research process allowed me to critically engage with those from less socially marginalized groups (the White participants in this case). In this regard I consider Springwood and King's question (2001, p. 406) in their article "unsettling engagements" regarding how one might

[1]As Schutte (2002, p. 2) indicates, although the scientific use of the concept "race" has been discredited (hence its placement here in quotation marks), as a social construct it "still has relevance and consequences for those so labeled". Following authors such as Omi and Winant (1986, 2002), Schutte refers to the process of racialization as that of attributing essentialized racial characteristics to a group (or people presumed to be members of a group) on the basis of characteristics which are "presumed by the attributors to be unchanging" (https://www.academia.edu/11961305/The_Racialized_Stranger_Reflections_on_the_Insider_as_Outsider).

simultaneously "engage in cultural critique" while engaging with participants. I reflect upon how I worked with Springwood and King's appreciation that "field work spaces are not populated by a singular or bounded people, culture, interest, experience or profession" and with their question as to how one can "collaborate with these different "informants"" (p. 410). I answer/reframe this question by suggesting that rather than seeing people as *informants*, one can see people as potential co-learners in a mutual research/re-exploration exercise. (This is consistent with the arguments of, for example, Douglas, 1998, 2002; Gregory & Romm, 1996; Holstein & Gubrium, 1995, 2003; Kovach, 2009; Magcai, 2008; Mitropolitski, 2013; and Russell & Kelly, 2002, who all consider research as implying a potential for co-learning.)

I shall also show how I interacted with the (historically marginalized) Black participants in follow up interviews/conversations in a manner that tried to be cognisant of my historical power and privilege while also trying to shift the relationship toward a more egalitarian one in the research encounter. I suggest that in this way I was trying to destabilize the unequal patterns of social relationship—and was recognizing (and trying to activate) the understanding that cultural practices are, in Springwood and King's terms (2001, p. 410), "constantly in (re)production"—with the focus in this case being on re-working such practices rather than further sedimenting past patterns. Furthermore, I refer to the types of "action" for which I took some responsibility (as a way of giving back as part of a relationship of reciprocity as discussed in Chap. 1—see, for example, Chilisa, 2012, p. 117; Cram, 2009, p. 317; Mertens, 2009, p. 52; McIntyre-Mills, 2014, p. 2). Finally, I suggest that besides the "immediate beneficiaries" (insofar as people could be said to have benefited from the encounters) less immediate beneficiaries can perhaps benefit from my attempt, with participants, to develop what Gergen (1978) calls a "generative theorizing"—as a theorizing which invites us (all those concerned) to review, in this case, classed and raced relations such that these can be practiced differently.

Because the first example concerns issues of "race" and racism in South Africa, I open the chapter with a narration of my becoming socialized into the meaning of Whiteness as a child in the context of apartheid South Africa. This forms the backdrop to my account of the setting up of the FG meeting and interviews (and ensuing action), in a way that I call active.

2.2 A Personal Narrative: Being (Defined as) White in South Africa

I was born in 1957 in South Africa (in a Jewish, English speaking family). Pampallis explains that in 1948, the National Party was elected to power (by the White electorate) using the slogan of "apartheid" (2008, p. 5). Under the apartheid system, as he further indicates social segregation in accordance with racial

categories became enforced in that "Blacks living in close proximity to White residential and business areas were forcibly removed to segregated townships on the outskirts of the cities" and the "political, social, and economic rights of all Blacks, and especially those of Africans, were severely restricted" (Pamphallis, 2008, p. 5). As a child, I was not aware at all of the enactment of such policies. So how did I learn the social meaning(s) of Whiteness?

I recall that as a 3-year old I used to sit in the kitchen with the "nanny"—whom my mom also termed "the girl", but sometimes also called her by a name ("Precious"). I had no awareness that the name used was not the name that had been given to her by her parents and that it was a name constructed (and chosen by her) for the convenience of White people to pronounce and remember easily. I never learned her real name. I can recall sitting on the kitchen floor while Precious prepared the food or washed the dishes. I have strong memories of her at the kitchen sink singing songs (in some African language). I enjoyed sitting there, listening to the sound of her singing. (Much later in life I heard those songs again—and realized that they were political ones about African workers and their struggles.)

I also recall (this was at the age of 4 or 5) being taken to the park by her with another (White, male) child. He and I would play while she watched us. I do not recall whether I noticed that the benches at that park were labeled with signs saying "Whites only" or in Afrikaans "slegs Blankes". But I know now that they were labelled that way. Nonetheless, I don't believe that I even needed to see these signs in order to have internalized the social hierarchy through, for example, noticing that in my home and in all my (all-White) friends' homes and in all my aunts and uncles' homes the "girls and boys" as they were called when speaking about them —sometimes also called "the servants"—were all Black.

Much later in my development (I think it was in my teens) I became angry when I heard my parents use these diminutive terms ("girls" and "boys") and I openly challenged the language. I think thereafter in front of me they chose their words more carefully, using the names such as Precious for the domestic worker and Lucky (as I recall the name of one gardener).

I also have vivid memories of my mom giving the workers metal mugs and plates from which to drink and eat—that were "their" plates reserved for them (different from the crockery we used). And I recall that they did not eat with the rest of us (there were 5 of us in the family, including my sister and brother)—but ate at different times. I cannot recall if they used the same table; but it is possible that they did not. In any case, the social distinction was easily being imbibed by me. Workers in our home were Black, as were all the workers in my friends' and aunts and uncles' homes, as were the garbage collectors (from the municipality), and also the women who sold mealies (corn) by walking along the streets shouting "mealies"! I never saw, nor read about, Black people being, for example, doctors or engineers. But I did not notice this lacuna at all. It was simply normal and "real" for me (at the surface level of "fact") that Black people were in lower social positions in social life. I did not then know about the color bar that had been enacted as law, where Black people were not allowed to occupy skilled positions in the economy, but were to be kept at the level of manual labor. And I did not know that the schooling

system (with separate schools for Black children) was specifically geared to educate them for manual labor in "White South Africa".

I do recall that at some point—I think it was in my teens—I was upset when I heard my mom say to someone "we must not spoil them" when talking about the domestic workers' wages. (Perhaps one of the workers had asked for more and my mom was discussing this with someone.) When I queried her on this, she said that the neighbors would not be pleased if we gave too much in the way of wages—more than had been "set" in the neighborhood. I remember saying that we could and should be giving the person what their labor was worth to us—or in any case more than they were getting. I don't recall if she ever acted on what I suggested. But looking back, I realize that I still was taking it as more or less for granted that "they" occupied these kinds of jobs—rather than, say, the jobs of professionals.

Philomena Essed starts one of her articles (2005) with a "guessing game" that used to be played at a Dutch high school (in the Netherlands) that she attended. The game—as I recount it below—points to the way in which gender categories and their social significance have/had become deeply internalized. Essed further shows how the game points to the racialization of social categories—but not drawn attention to by the school children playing the game (or trying to solve the following riddle):

> A father and a son are in a car accident. The son, who needs to be operated upon immediately, is taken to the hospital by an ambulance. When the stretcher is pushed into the emergency room the only surgeon available takes one look at the patient and objects: "I can't operate on him; he is my son". Question: How is this possible? (2005, p. 227)

Essed recounts that:

> We would go at length, trying to explain that the father in the accident was not the real father, or the son not the real son, or something weird with the surgeon … . Then came the 'ha 'ha got you' moment for the story teller: The surgeon was his mother! (2005, p. 227)

Essed points out that not recognized then (by the game players) were the

> racial dimensions involved. No doubt I was not the only one who imagined the surgeon to be a *White* man [albeit that Essed herself is Black]. And once we found out that the surgeon was a woman, she was surely not imagined Black African either. (2005, p. 227)

This game pointed to by Essed reminds me how obvious it was to me too during my upbringing that certain social positions simply were associated with Whiteness (and as Essed notes, also with gender). I did not notice—till very much later in my development (say, in my twenties or thirties)—when I was reading stories or watching TV that the images that were being displayed were producing (and continually reproducing) my sense of the normality of the social world as divided in terms of color.

Our home (and the broader social environment) enacted the color distinctions and the different statuses accorded Black and White people, such that the first time I heard the riddle recounted by Essed above (I think I was in my twenties) I also could not solve the riddle on my own. And I know that when I heard the word "doctor" in the riddle, a White male came to mind. Looking back, I also realize that

I do not recall coming across so-called "Indian" or Colored" people in my childhood—this must have been due to the segregation of neighborhoods. In any case, I did not register such differences in my childhood—but do remember registering White and Black as significant.

One distinction that was consciously made by my parents as significant (that they wanted me to understand as significant) was that we were Jewish. I was told that I was Jewish. (I was never told that I was White.) As far as Jewishness was concerned, we practiced traditions like lighting candles on Friday nights; and my father often went to the synagogue. (I went on specific Jewish holidays.) I knew that my father did charity work collecting money for Jewish old-aged homes and Jewish institutions for disabled people. It was evident to me that Jewish people for the most part married other Jewish people. (I could see this from my aunts and uncles who were all Jewish.) I recall that sometimes during my childhood my parents mentioned friends whose children had married "out of the faith" and these parents were not easily able to cope with this. I was expected also to marry a Jewish person; and my parents were not pleased when I chose a non-Jewish partner—although they later did accept this (mainly I think because of the "liberal" attitude of my aunt—my mom's sister). I understood that sometimes Jewish people did marry non-Jewish people.

I remember that when at the age of about 11 or 12 I was sent from my grandmother in America my first Barbie doll, with the accompanying booklet of Barbie's wardrobe of clothes and also of her boyfriend, Ken, the picture of Ken (as White) was completely obvious to me. The idea of a partner to Barbie being not White would have been inconceivable to me at the time.

While I was clearly internalizing strong distinctions of Black and White, I also have memories of taking walks in the neighborhood with my father (also at the age of about 11–12) in which he began conversations in Zulu with Black people whom we met in the street. In choosing to learn Zulu for purposes of communication (he did not need the language for his work), he was going against the current of the time. (No-one else whom I knew was able to speak any African language.) I do not, however, recall anyone making comments—negative or positive—about his choosing to learn Zulu. I (too) never asked him about the meaning that this might have had for him.

I still have images of him sitting for hours at a time at the dining room table with the Old Testament in Zulu in front of him, studying it carefully, with his fingers going over the words. He had in his youth become very conversant with the Old Testament in Hebrew and he knew pieces of it more or less by heart. (He had studied it at a Jewish Yeshiva.) Hence he was able by looking at the Zulu version to make out the meaning of the words and also to get a sense of the grammar. He learned the language in this self-taught way.

When we met with Black people on our walks, he started up conversations with them and managed to hold an interchange for some time; and this was something that was very pleasing to them—as I suppose it showed that he had taken the trouble to learn enough to communicate with them. I remember that he always got an excellent reaction—huge smiles and friendliness. It created a very good energy. I enjoyed noticing that human exchange of energy. Perhaps through these childhood walks with my father I was getting a sense that things could be different (and

better)—and that the quality of social life was diminished by the way that things "normally" were in terms of social relations between Black and White people.

However, I was still not cognisant that Black and White were social constructions, rather than referring to "natural" divisions between groups of people.

2.2.1 Being White and Jewish in South Africa

In Sect. 2.2 I opened my personal narrative with the title "being (defined as) White in South Africa" to emphasize that being White is a matter of social definition. Interestingly I have friends (and also neighbors) from Lebanese background who told me that before 1948 in South Africa (that is, before the Nationalist apartheid government was elected) they were considered as non-White in South Africa. This was by virtue of the Middle Eastern origin of their families. (They mentioned this to me knowing that I was exploring issues of racism in the context of a book I was writing, namely the book that I published in 2010 on *New Racism*.) It was only in post-apartheid South Africa, they tell me, that they were classified as White—apparently because the Nationalist Party was attempting to shore up the numbers of the "White" population.

Mackintosh (2010) suggests that after 1948 the Jewish Board of Deputees (SAJBD)—supposedly acting to represent the "Jewish community" in South Africa—was trying to persuade the government that "Jewish enterprises in light industry opened up new employment opportunities for poor whites". However, Mackintosh argues (critically) that as he sees it, the primary aim of the Jewish community at large, and hence the Board, was to ensure that Jews were included as members of the racially defined privileged White class. According to him, then, the potentially tenuous position of the Jewry in South Africa meant that the Board focused on efforts to define Jewish people collectively as "one of us" (that is, of the White group). Mackintosh suggest that the Board could have taken a more moral stand against apartheid, but chose to argue that its remit was to define its interests as those affecting Jews as Jews, while allowing individual Jews to decide for themselves as "matters of conscience" in regard to National Party race policy.

I recall that as a child I had the knowledge that my parents, and also the relatives and friends with whom they discussed for whom to vote, were going to vote United Party (UP) at election time. (This party was not advocating legalized apartheid.) I did not know why they were choosing this. I just registered that their party of choice was UP. But now looking back I think it was because it was understood in the Jewish community that the Nationalist party may have elements of anti-semitism. I don't know if it was also that my parents (and the relatives and friends voting UP) were trying to take a stand against apartheid as such. I do not recall stories of relatives in the family or friends of the family choosing to actively struggle against the system. But I know that later my parents were proud of the work of Helen Suzman (from the Progressive Party), when she began to obtain social power and international recognition for her anti-apartheid work, struggling for Black people's rights.

But actually we never discussed why they were pleased with the work that she was doing. Nor did we ever discuss race issues at all in the home.

2.2.2 *(Different) Learning Through Lectures and Books at University (and Viewing the 1976 Soweto Uprising on TV) Plus Further Developments*

As mentioned earlier, the books I read at school served to reinforce my sense of the naturalness of the racially divided world in which I lived. It was only when I reached University in 1975 (just after TV was introduced in SA) that I was offered some new language by which to "see" the social world as constructed. Berger and Luckmann's famous book *The social construction of reality* (1966)—which was introduced to me when I was studying *Sociology 1*—was one of the books that I found highly meaningful. They illustrate how methods of legitimating the symbolic universe in society may be employed to protect the "official" universe from being undermined. They also argue that social change is characterized by new ways of seeing the world. Their focus on how the social world comes to take on a factual character for members socialized into it (although members can also resist the messages being transferred) gave me a way into understanding how the existence, and ranking, of "race groups" can be made to appear normal. Also while studying Sociology, I read various texts written by Marxist-oriented authors. Looking back, I realize that most of the authors I studied were White—but at the time this did not occur to me. The fact that the theorists/thinkers that were recommended in Sociology to read were mainly (if not all) White only struck me when I later read Black authors criticizing the dominance of academia by "the West" (for example, Collins, 1990, writing largely with reference to the context of the USA). But in any case, at university the Marxist texts that I read suggested that the emergence of the concepts of race and race groups could be seen as tied to the development of capitalist structures (structured around obtaining pools of cheap labor)—in South Africa as elsewhere. This left an important impression on me.

What also left an impression was the idea (offered by my Sociology lecturers) of the apartheid social system as being structurally violent. The apartheid government allowed academics to make these kinds of claims: it seems that the government did not expect that academics could have much influence in the broader body politic. The lecturers were often highly critical of the violence of the state, not only through acts such as the police battering (and shooting) of those who tried to resist its policies (for example, the students of Soweto in 1976, pictures of which were shown on TV), but also through the apartheid policies such as the color bar (forbidding Black people to do certain categories of work), and the Land Act (forbidding the owning of land in "White South Africa"), etc. I think it was my seeing the physical violence of the police on TV (and also hearing about farm workers being beaten by their employers for minor so-called errors that they might have made) as well as learning about the concept of "structural violence" of policies and laws, that made me more aware of

my privileges by virtue of being defined as White (and of the social consequences of the apartheid system for Blacks and Whites).

At University, I joined the "Institute of Race Relations"—which only later in life I thought could have better been called the Institute of Race*d* relations, to highlight how social relations across the globe historically became race*d* and thus could be transformed. This language of "raced relations" I picked up later from reading the work of more Black authors—such as Alleyne's (2002) *Radicals Against race*, which concerned political activities in Britain.

As part of my work for the Institute, I organized charity events to collect money for their advocacy; and I also did volunteer work (for 10 years) for an organization called Sached (to help Black students whom by then I realized had been disadvantaged via the apartheid system!) (My intellectual history as I narrated it in Chap. 1, follows from this period.)

To "close" my narration around my upbringing (with my immediate significant role models in my mother and father) I want to move forward to 30 years later (2000–2009) when my mom was living in an old-aged home, being cared for by carers who were all Black. Although my mom at first felt uncomfortable with some of the carers, as time went on she used to tell me about the hard lives of her friends as she now called them (the carers). Besides my visiting her regularly in Johannesburg, we also spoke on the phone as I was living in Margate. When I asked her what she and the carers spoke about, she used to say "all sorts of things", including the hard lives of the carers (whom she named). My mom also said to me that we must both pray for children who are living in poverty (she meant Black children—and this must have been the influence of the carers telling her stories of this). I felt relieved that my mom found it interesting and meaningful to gain some insight into the carers' personal lives. (I don't recall that she had previously shown interest in the lives of "the girls" who did domestic/childcare work for us when I was being brought up.) I was also relieved that she felt sufficiently concerned now to want to pray for (Black) children living in poverty—and even urged me to do the same. She passed away quite soon after our conversations had shifted into this gear.

Meanwhile, at the time when I had been going to the old-aged home to visit my mom, I had stayed for periods of up to two to three weeks with my aunt; and there I caught a glimpse of the relationship between her and the domestic worker (Tshimangodzo Mphilo). (I have used Tshimangodzo's real name here, as she wished to be acknowledged.[2]) It struck me when Tshimangodzo told me (we had long talks while, for instance, I was eating my breakfast and she joined me) that when she heard my aunt on the phone calling her "the maid" she expressed to my aunt that she must

[2]As will be seen in my discussion of Woldegies's interaction with research participants, they too wished their names to be acknowledged. (See Chap. 4, Sect. 4.2.4.) Chilisa makes the point that from a "relations with people perspective" or relational ethical framework, "the information imparted, or story offered, would lose its power without knowledge of the teller" (2012, p. 119). And it may well be that the research participants/co-researchers do not wish to be anonymized. This therefore needs to be checked with participants. See also my discussion on this in Chap. 8, Sect. 8.2.3.3.

not call her that. She said to my aunt that she must either call her by her name or else "the helper" or "the domestic worker". For Tshimangodzo the word "maid" was associated with having a servile position in the apartheid days when Blacks were always servants—and she told me she refused to allow her dignity to be affronted by the use of this term. She iterated to my aunt that she wanted to be recognized as a person who was helping my aunt (and on whom my aunt depended). In terms of the (new) language Tshimangodzo was using—and that I too was learning from her—she was trying to envisage a different relationship: instead of focusing on the inequality in position, she wanted to focus on their status of mutual dependence. My aunt was sufficiently adaptable to "go with" the shift in language—although I do not know if she fully understood that for Tshimangodzo the term "maid" (associated with "servant") carries all sorts of connotations and that by the use of the new language Tshimangodzo was envisaging a different social relationship.

Tshimangodzo also explained to my aunt (in strong tones) that it was due to apartheid that she did not get educated and is now relegated to be a domestic worker—but that had the history been different, she could have become properly educated and would not have to do this kind of work. This was a way of pointing out to my aunt to the privileges of my aunt's social position vis-à-vis her own. My aunt did not try to deny this. Perhaps she was indeed learning about Whiteness and oft-unnoticed/taken for granted privileges.

In the example of the research discussed in this chapter, my concerns with the exploration, and the shifting, of race constructions and racism (and in particular their intersections with class) were clearly "present" in the research enterprise that I initiated—of which I provide my story below. I call my account of this research a *story*, and I refer to my *storying* around the issues, in line with authors who argue that the word *story* (and the verb *storying*) expresses that the exposition has been written by a (concerned) author, and is not meant to offer an "objective" statement of "what happened": it is rather to be understood as an account that invites further engagement by others, who can "write into" the story (see, for example, Collins, 2000, p. 38). Sikes suggests that presentations of research encounters as "storying" mean that through one's stories one tries to offer a sense of the "feel and place" of the context of the research and a sense that the writer "has 'been there' and that they [the reader] could have been there too" (Sikes, 2005, p. 79). At the same time the word "storying" contains an admission that "researchers' personal identities and the perspectives, understandings and knowledges, [including] the beliefs and values that go with them, shape all aspects of the research process" (Sikes, 2005, p. 90), as expressed also by Sikes and Goodson (2003). It therefore invites readers into a re-reading of the text, so that additional meanings can be drawn out from it.

Viewing qualitative research (and indeed all research) as a matter of storying and re-storying the social world can thus be regarded as offering an alternative epistemology to dominant (hegemonic) conceptions of what is properly involved in understanding/researching the social world. This alternative approach to "knowing" is, as Ladson-Billings suggests, "well developed" and can serve to de-privilege "the hegemony of the dominant paradigm [of what knowledge production amounts to]" by offering a viable alternative (2003, p. 398).

2.3 Researching Raced and Classed Relations in Post-apartheid South Africa

In this section I offer my account/story of the cross-racial conversations which I initiated in 2007 in a building in which I was living—residential and holiday complex—in Margate. (Prior to 2006 I had been living in Swaziland, the United Kingdom, and Cyprus, having left South Africa to take up a position in Swaziland in 1991.) My story here begins with how I tried to create an opportunity to generate co-inquiries with people around topics that I felt were more or less social taboo in everyday conversations—especially cross-racial conversations. These are topics connected with race and racism. Trimikliniotis, Gordon and Zondo likewise observe the failure to speak openly around such issues, stating that "it seems that 'race' remains a taboo subject in South Africa" (2008, p. 1331). Jansen (2011) for his part offers an account of his efforts to address taboos around exploring race and racism in his book entitled *We need to talk*. The silence in everyday conversation around the meaning of "race" and implications of this for experiences of racism has also been remarked upon in other contexts—see, for example, Tatum (2003) and Sue (2005) commenting on this in the context of the USA.

As I indicated by way of my personal narrative in Sect. 2.2, I take the view, along with a myriad of other authors/actors (which informs my account too) that the categories Black and White are social constructions, where "race" is considered as referring to the social processes through which the idea of race is used (cf. Ansell, 2007, p. 329; Ashe & McGeever, 2011, p. 2009; Kiguwa, 2006, p. 113). It is for this reason that I have referred in the heading of this section to race*d* and class*ed* relations, using language that draws attention to the socially relational character of "race" (and class).

What I highlight in this chapter is the way in which—during the FG and in further conversations—many people, especially Black people, expressed concerns around race and racism in relation to issues of exploitation and devaluing of Black labor. This is set against the history of South Africa where apartheid was linked to the cheap labor-power of Black workers, as well as to the attendant calculations of their low standards of living and needs in order to calculate the minimum wage required by African workers (cf. Wolpe, 1972). Marxist-oriented authors such as Wolpe suggest that the link was not accidental and that it served the development of capitalism from the start. Bonacich, Alimahomed and Wilson (2008) place this in the context of what they call the continuing racialization of global labor, where they offer a (Marxist-oriented) structural outlook on racism on a global scale. They suggest that

> the entire global system of racialized exploitation of labor depends upon racial understandings—that the lives of some people, and some workers [from racially subordinated social groupings], are less important than those of the dominant group (typically White and Western). (2008, p. 352)

The question of what might be involved in trying to transform sedimented raced and classed relationships is taken up when I proffer some "generative theorizing" in Sects. 2.4–2.6. For the moment I take a preliminary definition of racialization as found in Lewis (speaking in the context of the USA):

> Racialization is an ongoing process that takes place continually at both macro- and micro-levels and involves questions of who belongs where, what categories mean, and what effect they have on people's life chances and opportunities. (2003, p. 285)

Lewis takes the position—also supported (and fostered) by the research in this chapter—that institutions and people are "forces in the reproduction and transformation of race". In this way she highlights theoretically the possibility of "transformation" (2003, p. 284).

2.3.1 *Opening a (Cross-Racial) Conversation About Race and Racism in Post-apartheid South Africa*

As mentioned above, in 2006 I set up permanent residence in an apartment in a complex in Margate (South Africa). I wondered about the possibility of trying to organize a FG discussion with the workers in the building (all of them being Black) and the few other permanent residents (my neighbors) who were English/Lebanese speaking, while also involving some other acquaintances (Afrikaans speaking Whites).

Two thirds of the apartment block was owned by (nearly all-White) Timeshare owners—with an office that caters for the Timeshare guests. The office supported all workers (cleaners, including someone handling the linen ironing, maintenance staff, etc.) who attended to this part of the complex. There was furthermore a gardener and a cleaner who attended to the block in general. One third of the apartment complex was owned by sole owners—with three of such owners (me and my neighbors) living on the premises.

Most of the workers recognized me as a resident by sight and we often waved greetings at each other. Our contact was infrequent, though, because they worked in another area of the complex. In contrast I used to see the gardener and general cleaner of the building more regularly. We exchanged friendly greetings when we saw each other on the grounds (but we had never spoken about race issues).

Below is a pictorial representation of the players in this narration. These are mainly pseudonyms, excepting the one case where the participant (Tracey) wished to be referred to by her name.

At the time when this story as recounted in the chapter occurred, a White woman managed the Time share support office that caters for the Timeshare guests. However, a Black person (Asanda), involved mainly with supervising workers, was sometimes called upon to fill in for her, such as when she was on leave. Duties included attending to the clients and administering the process of booking them in. (This manageress was later replaced by another White woman, and Asanda began to work more frequently in the office as support.)

I had known Asanda since I bought my apartment more than 15 years before, in the sense that I used to see and greet her on the premises during holidays there while I was living in the UK. I was also aware that her job outside of the office involved overseeing/supervising some of the staff. With this in mind, I approached her in the office in May 2007. I asked her what she thought about the feasibility of

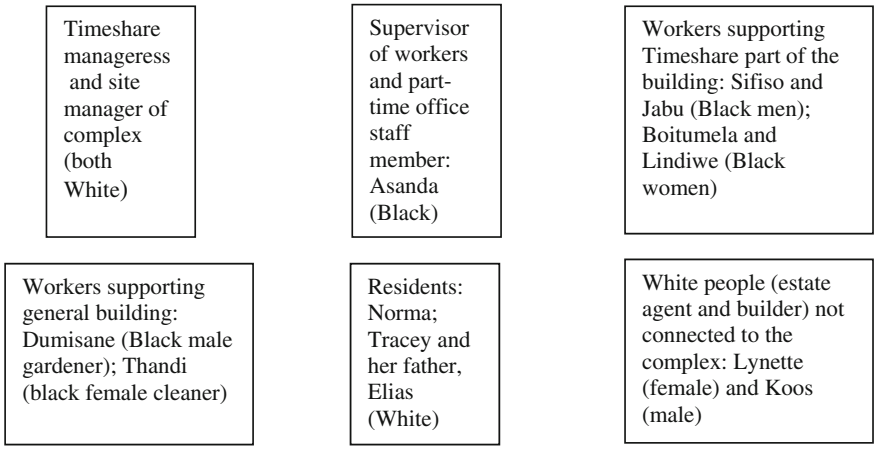

Fig. 2.1 Depiction of the setting

our organizing a discussion group around meanings of "race" and questions related to this in post-apartheid South Africa. (I did not call it a FG as such; to Asanda and others it was called a meeting where we could focus on this topic.)

I indicated that I had in mind that the participants in this group meeting could include workers in the building (cleaners, etc.), the live-in residents—me and two White neighbors[3]—and also some (White) linked people, namely an estate agent and a builder whom we all knew (as they sometime came to the premises). I mentioned that I was considering writing a book exploring issues of racism (to share with audiences) and that we might all have an interesting conversation between ourselves as we talk about this together.[4] I suggested that such a

[3]These were the neighbors whom I mentioned in my personal narrative, who informed me that prior to 1948 they—as part of the Lebanese community—were classified as non-White; but in 1948 the Nationalist party made the ruling that people of Lebanese origin become classified as White (in order to expand the White population).

[4]As it happens, the book that I had hoped would be co-authored as a cross-racial encounter between myself, Carlis Douglas and Susan Weil did not come to fruition, as the co-ordination across continents proved to be too difficult (with Carlis in the UK and Susan in Italy). This is the first time that this material is being used. It is also worth mentioning that since the time of this research I have also become more sensitive to the issue of how one can "give back" to participants who contribute to books that become written. At the time, the idea of writing a book (which became an opening to set up the research with the participants) served the purpose of opening an arena for cross-racial discussions amongst the participants around racism. But should I have thought of offering the participants more in terms of financial benefit? Although I did not think of this at the time (possibly because none of my academic books have ended up with anything in the way of financial benefit to me after covering my paper and printing costs, etc.), it is something to bear in mind. In effect, as a side-effect of the research, I became known as the person in the complex to whom staff could come for loans (which money I decided not to retrieve when I moved to Pretoria, knowing that it would not be at all easy for staff to pay back). See also my discussion of how Woldegies (2014) handled the issue of financial assistance in Chap. 8, Sect. 8.2.2.3.

conversation would also help us get to know each other better as persons, since we sometimes bumped into each other on the premises but rarely engaged with each other. At the time I did not think of how else the discussion might benefit the participants. However, as seen below, the way in which the research could become "useful" emerged during the building of the relationships. Chilisa asserts that "Indigenous-driven research methodologies can enable research to be carried out in respectful, ethical ways, which are useful and beneficial to the people" (2012, p. 100). Although I was aware of the principle of reciprocity as highlighted by Indigenous authors and also as felt by me to be important, it was only in the process of engaging with participants as the research proceeded that suggestions for usefulness, and participant expectations in this regard, became defined in practice.

I was pleased to hear that Asanda considered the setting up of a meeting to be a good proposal. She suggested that we act quickly (while the current White manageress was away on holiday, as she was not sure about her reaction to such a meeting). I asked Asanda if she could approach the workers to ask if they would agree to join a discussion around race/culture/and racism. I considered that because she was their supervisor, it would be helpful if they knew that she would not be against their taking an extended lunch time for this. (I mentioned that I would supply food and cold drinks.) Incidentally, along with her instincts not to involve the White manageress, I also did not suggest inviting any of the Timeshare owners, as the Black workers might consider this a constraint on their wishing to talk, feeling that perhaps some of the people in the FG would have undue power over them, and that what they said therein may be held against them later. Already asking people in the complex to engage in a cross-racial conversation of some sort around race and racism was something that I understood as being outside of convention.

A few days later Asanda told me she had managed to organize a few people but that I needed to go personally to speak to the rest of them. She explained that they were not sure about the purpose of the meeting and therefore did not want to commit to attending. I therefore approached these cleaners, maintenance workers and gardener, and suggested that it may be of value now that we are in the new South Africa to speak together about issues of race and racism. I also suggested that readers of the book (that I mentioned I was involved in writing) could find interesting our story about what it was like to talk together and express views. I felt that proposing the idea of talking as part of an *exploration project* for the project of book writing, would support us all in seeing this as an arena to just air and hear and exchange views. I did not name the process as research, as this would not be a term familiar to them—but I used words like discussion/exploration/book to signal the idea of searching together.

As part of my trying to create a feeling of "safety", I assured the (potential) participants that I would be facilitating the discussion during the meeting and that there would be no pressure to speak: it was up to them. This was in response to some of them seeming reluctant to attend on the grounds that they were not sure what contribution they might make. In this way the prospect of us all attending a meeting was "negotiated" as they could choose to define how they would participate in the meeting.

I was still not sure if all the workers would attend, but I felt that the numbers would be sufficient so that I could arrange further with my White neighbors (whom

I had sounded out previously), and with the other White people with whom I had explored the possibility of attending a cross-racial discussion about race and racism to be held in the apartment complex. As it turned out, all the workers did in the end attend the meeting, which was arranged for lunch time two days later. Before proceeding to recount its progress, it is worth mentioning already here how one of the Black workers—the gardener (with pseudonym Dumisane)—later reported to me how he had experienced its social importance. He stated to me that he considered it had been a very good idea to get White and Black people to talk about the kinds of issues we spoke about in the meeting. As he put it: "especially White people must talk about these kinds of topics". Upon seeing this expression of Dumisane, critical reader of this book Francis Akena added the following—as a comment alongside the text, July 2016:

> Norma: I find this participant's voice so powerfully asserted that in extension, I add: "because they have benefited from White privileges for years both consciously and unconsciously. The conversations to unpack this ringfenced (and sometimes undeserved) privileges serves as foundation stone in the long and challenging healing process to physical, emotional and spiritual injuries that might have been inflicted".

Similar to Dumisane's experience, there had been an air of "positivity" after the meeting: many positive utterances about it were exchanged between the participants.[5] In Chilisa's terms (2009) the process of talking in the meeting can already be said to have had a healing effect—healing some of the wounds of apartheid in the various participants' experience. One of the ethical principles to which Chilisa (2009, p. 408) points, is the notion of researchers taking some responsibility for "healing" (of whatever historical wounds have been created). In this regard she speaks about "the responsibilities of researcher as a transformative healer". Giri too speaks about "cultivating transformative reconciliation" and she considers some of the ways in which people can take some responsibility for this in South Africa (as in other countries). She cites favorably Sitas, Latif and Loizou's argument (2007)—applied to South Africa compared with Cyprus—that apart from trying to address "hard variables" such as class, one should also consider "soft variables" such as …. "border-crossing meetings which 'swing disposition toward reconciliation, co-existence and forgiveness'" (Giri, 2011, p. 606). As I shall expand upon below, the FG meeting in this case became a "border-crossing" encounter—based on people together re-exploring (researching) issues connected with "race".

[5]I also observed the high spirits in the expressions of smiles of the participants, that is, via their non-verbal expressions. Onwuegbuzie, Dickinson, Leech, and Zoran (2009, p. 2010) advise that researchers should not ignore the importance, inter alia, of non-verbal expressions when analyzing FG material. In June 2017, I showed Tracey some parts of the draft of this chapter and she recalled (from her interpretation as remembered of the body language of the participants) that especially the staff "were happy to have been included in this research" —even though after the focus group they would be back working on their jobs, but now with a feeling that "at least some people had taken time out to consider their feelings" (about racism). She stated that with the "sharing of food and in a relaxed atmosphere they had felt very comfortable to open up and talk".

2.3.2 The Focus Group Meeting

As arranged, we met in a roofed seating spot in the complex, around a table with a wooden bench meant to cater for guests. I was happy to see everyone at lunch time walking toward the table!

I acted as scribe in the meeting (as people spoke) as well as facilitator. Soon after the meeting while my memory of it was still fresh I filled in the details. These notes form the basis of my account here. The following people were present in the meeting. (They wrote down their actual names during the meeting, which I have anonymized here.)

ASANDA	The Black female supervisor of other staff (and some-time worker in the office), with whom I had initiated the organization of the meeting by previously speaking to her in connection with organizing the group discussion. She was in her early 50s.
SIFISO	A Black male window cleaner and sweeper, who was not literate, so Asanda wrote his name down for him. He was in his early 30s.
JABU	A Black male maintenance worker, in his late 40s.
DUMISANE	A Black male gardener, in his mid-50s.
BOITUMELA	A Black female cleaner supporting the Timeshare part of the building, in her late 40s.
THANDI	A Black female cleaner supporting the general building, in her early 50s.
LINDIWE	A Black female handling primarily the linen ironing for the Timeshare—in her early 50s.
TRACEY	A White (Lebanese) resident in the building, in her late 30s.
ELIAS	The father of Tracey, in his mid-60s. (Tracey and Elias had previously mentioned to me that they became classified as White in 1948, with the apartheid government coming into power.)
LYNETTE	A White female (Afrikaans speaking as her home language) who was an estate agent. She was in her early 50s.
KOOS	An Afrikaans White male who was a builder—in his late 60s. (Note: Both Lynette and Koos had expressed interest to me in talking about the issues when I said I was involved in writing book(s) on racism.)

In the section which follows I do not record the totality of the meeting (due to space limitations) but only the beginning part thereof, where we were exploring the

meaning of "race" and where the intersection of race and class as experienced primarily by Black people arose, although they did not label it as such.[6]

Norma Thank you for all coming to this meeting, where we can begin to look at questions connected with the meanings of race and also racism in our context—as Asanda mentioned to some of you we would be talking about today and I mentioned also to you. I know that some of you were not sure of attending, but I am hoping that you will find this a worthwhile experience. Please feel free to join in the conversation in whatever way you wish. But as I said to you earlier, you do not need to talk if you do not wish to. I hope you enjoy the lunch too—help yourselves to the food and here also are some cold drinks—fruit juice and coca cola.[7]

I will be taking notes as you speak so that we have some record of the conversation. Please can you firstly write down your names here [passing around the notebook]. This will help me later when I see my notes to make sense of them because I will be able to see what each of you said. In the book-writing your actual names will not appear (as you have requested). But readers will be able to see what we have been talking about and will be able to see us exchanging ideas together and exploring the topic. I am seeing this meeting as a way for us just to begin to hold a talk around this topic of race and racism as we see this—and in the process also to get to know each other a bit better and learn more about each other. I am very interested to hear how the conversation develops between us. We have only an hour, but in this hour we can see where our conversation leads.

Anyway, who wants to begin the discussion? If you want to speak, just go ahead and when someone else wants to speak you can signal this to me and I will keep things going that way. I may also from time to time add my thoughts as the discussion proceeds—so that you will also get to hear how I am seeing things. I had mentioned to Asanda earlier that I am not sure about the meaning of speaking about "race" in the new South Africa.

[6]The term intersectionality as used by various authors—and as used here—can be seen as a useful term to try to situate social categories such as gender, race and class in terms of their embeddedness in multiple systems of dominative relationships (cf. Collins, 1999; Crenshaw, 1991; Essed, 2001; Romm, 2010; Valluvan & Kapoor, 2016).

[7]As I indicate in Chap. 8, the sharing of food is regarded as crucial by certain authors writing about the conduct of FGs/talking circles. See for instance, Kovach (2009, p. 124) and Liamputtong (2011, p. 139).

Boitumela I will begin by saying that I myself do not believe in "race", but I think that it is more culture that we need to speak about. What is unfair is what happened to Black people in apartheid South Africa and that is what it means to speak of racism.

Asanda What I want to add is that racism never stops and even since post-1994 with the change to democracy I experience racism in the office. Also I have a worry that when White people see me they identify me as a "thief". This is just like I have noticed that when people see my grandfather they consider him as a "thief". They apply a label based on seeing the color of the skin.

Norma So we can summarize then that Asanda is suggesting that it is a racist approach when people make conclusions about others on the basis of their physical appearance—color in this case. (*I noticed that many people nodded when I said this. I was also pleased that people seemed to be giving me encouragement*—via *their nodding—to take part in the conversation. Thandi then proceeded to explore this further.)*

Thandi Also, when someone employs a Black person it is a cue that they can give them less money. When you employ them, you can give them a lesser salary than a White person would get. There is still this privilege associated with being White—in that you get better salaries. On this basis we can say that racism is still here today.

Lynette This is an abuse of people—that is, when some people are given less salary as in this account. But these days when I phone for a job, it is understood that the job is now for Black people in South Africa. Also adverts in newspapers are geared for Black people. So I myself have not been able to find work this way.

Asanda We must remember that before 1994 White people were employed even if they did not have qualifications. Now more Black people are becoming more educated. But, for instance, I myself am behind in my education. I did not get good qualifications in good time because of the apartheid system. Therefore, I now need to be given a chance because I am Black —because I did not get that chance before. Also we must remember that more White people have got money to provide for their kids in terms of quality education. This is also an important factor. So the government is trying to make things more "even" now.

Lynette I can understand this, but I am wondering when the process is likely to end.

Asanda One day it will stop but we just don't know when. (*I became somewhat uneasy when Lynette initially made her statement—regarding affirmative action—and I was hoping that one of the*

people in the group would place the policy in historical context in terms of how Black people had been disadvantaged for so long. I felt that Asanda's way of explaining this in a simple to understand way and with reference to her own experience was helpful. The way in which relationships can be built across different understandings of affirmative action is an issue that I explored in my book on New Racism, 2010.)

The rest of the conversation proceeded along lines of, for example, issues of crime and (White) people's fear of Black people, including discussions regarding the meaning of the Rainbow nation in terms of what it means for different cultures to co-exist as well as interact. I will not here continue with those themes, but will now move to certain sections of conversations held with people after the FG meeting (at different points in time). These conversations form the basis of my storying below.

2.4 Storying in Relation to the One-to-One Conversation with Lindiwe (Concerning Her Expression of Work-Related Difficulties)

One or two days after the meeting I saw Lindiwe—the woman who was doing the ironing for the Timeshare business at the complex—as she was walking toward the shops. I asked if I could walk with her and at the same time talk about her experiences of the meeting and anything else she wished to speak about. (I had some paper and pen in my bag for taking notes, which I did in shorthand.) After our initial introductions she began as follows:

Lindiwe	OK we can talk as we walk. My main concern that we did not cover in depth in the meeting is with the wages that I get and also with the loads of work that I am expected to do. The White manageress [she mentioned her name] as well as my supervisor Asanda expect me to do very much work. The work is very difficult here. I am ironing sheets all day and I am having to do this for the whole of the Timeshare units. I am getting a sore arm and shoulder and I have to take pain killers for this. No-one—neither the White manageress nor Asanda—seems to be interested in this problem.

Norma	Have you spoken to them about the heavy loads of work?
Lindiwe	I have asked the manageress if some of the other cleaning staff can be asked to help with the load. Sometimes the others do help a bit. But this is done very informally and they hardly help at all. They think it is my job and therefore they feel they are not obliged to do any of the ironing. If I try to ask them to do it, this causes friction between us. I am finding it very difficult. I also told Asanda, but she also does not seem to understand that I am carrying such a load nearly all by myself. And Asanda also does not officially ask any of the others to aid with the ironing. I think that the manageress and Asanda have the impression that others do in fact help, but they do not realize how little the others actually do. As long as the work gets done, neither the manageress nor Asanda is concerned. Their concern is only with seeing that the work gets done.
Norma	Would it be of any help to you if I tried to speak about this to Asanda, with whom I have a good relationship, to correct her impression that the others are helping you with the ironing?
Lindiwe	No, you must not raise this issue. That will only create further difficulty if it is known that I am complaining. This is not the reason why I am mentioning this to you.
Norma	I am wondering then if you maybe could try what I do with my computer when I get arm strain—that is that I change hands for moving the "mouse" on the computer and that does not put all the strain on one arm. So you could try to switch arms and see if this works (with a little practice). Could you maybe use your left hand to do the ironing from time to time? That might give a chance for your right hand to get some rest. Maybe you do not need be so concerned with ironing everything perfectly straight always with your right hand. Even if it is not done so well with your left hand probably they will not notice.
Lindiwe	That could be a good idea. (*Actually, I had forgotten what Lindiwe had responded to me initially in 2007 when I made my suggestion to her concerning switching arms—and I had not recorded it in my write-up of my notes. Four years later, in March 2011, I decided to ask Lindiwe if she remembered our initial encounter. She*

	had remembered it very well—which was the beginning of us exchanging warmer greetings over the years and opening up more conversations. She reminded me that in that first encounter I had mentioned to her that I used this strategy on my computer. I asked her how she had felt when I said this to her at the time; and she said that she had found it a good idea to try, and had done it a little bit—but soon thereafter a new manageress was appointed and they introduced a system of outsourcing the ironing and she became an additional cleaner.)
Lindiwe continued	I need to get back to work now as I have a lot to do!
Norma	Yes, I can see that that is the case. I am pleased we managed to talk while you were on your way to the shops and back!
Lindiwe	And I have spoken to you about this also because we see each other on the premises and we know each other and so I wanted you to know something more about my feelings.
Norma	As you say, it is important that we have made a connection through this conversation. [And indeed subsequently we greeted each other more warmly and asked each other with more depth of feeling "how are you today"?]

From this encounter we can see that I was not trying to hold back from being "active" in the conversation: I was aware that I had a part, with Lindiwe, in co-constructing the meanings that ensued as we spoke. In this regard I was following Gubrium and Koro-Ljungberg's proposal that we can treat interviews as an "event" wherein both "interviewer and interviewee 'feed off' each other as they co-construct data" (2005, p. 711). Or, as Kuntz explains, the focus was on our together "*coming to sense*" (meaning-making) as part of our relational encounter (2015, p. 87). I was furthermore trying to express that I cared about her and about what she told me.

As stated above, Lindiwe had asked me not to mention her plight to others, as this may be construed as her complaining and might jeopardize her position further. However, shortly thereafter, her job position changed when the ironing was outsourced and she became an additional cleaner. A few years later I asked the new manageress who had been appointed (White), whether Lindiwe's workload had been taken into account when this decision was made; and she said that she had been unaware that this was an issue, but had she been aware of it they could have organized a rotation of the ironing and cleaning tasks.

This story I see as related to Essed's suggestion that "the protection of dignity is one of the most important challenges people face in organizations [in all parts of the

world]" (2010, p. 144). She cites Hodson (2001) as identifying different challenges connected with employee dignity, namely: mismanagement and abuse; overwork; limits of autonomy; and expectancy of harder work and more commitment without offering job security.

Considered in this light, Lindiwe's sense of being overworked and uncared for as a worker can likewise be seen as a violation of her dignity. Essed pleads for leaders to understand the deep relevance of what may be seen as "micro inequities" and to take them seriously (2010, p. 144). She notes furthermore that while workers across the board may face these challenges, it is more often than not "women, immigrants, ethnic minorities, people with disabilities and other members who are not considered part of the norm … who are more vulnerable" (2010, p. 144). The new manager having indicated to me that had she been aware of Lindiwe's plight she would have found a way of acting (to at least show some sense of care) is to me an example of what could be and what might have been—and is thus in some sense encouraging.

Did my own indication of care in my asking Lindiwe if I could do something in the situation (such as acting as a mediator with Asanda) make any difference to the quality of her (and my) life? It could be said that we both felt some need to create more "connection" and our relationship henceforth improved (as part of a warmer relationship). In keeping with her request I did not mention her plight to anyone; but I use this story now as part of my sharing this with wider audiences to show that the new manageress did see the lack of care as a problem and would (on her account) have acted if she had known about this. This to me points to a potential to address what Essed (2010, p. 144) calls the "micro-inequalities" as they manifest in work situations. This does not mean that people cannot also operate to try to shift the "slower" structures (cf. Bowers, 2011, p. 545) that affect the life chances of those born in poverty (and still in South Africa, being largely Black). This would connect to what Giri (2011, p. 606) calls the "hard variables", excepting that they can be seen as less "hard" depending on how people relate to them (and depending on how "hard to alter" they are seen by those concerned with such alteration).

In relation to this story (or rather, as part of it), Sitas casts a particular angle on Lindiwe's indication that she was not being cared for as a human being when he refers to the ethic espoused and practiced by Mandela in terms of his disposition toward others based on what Sitas calls "moral rectitude" (2011, p. 571). Sitas argues that in the context of South Africa, and indeed resonating across the globe, an ethic can be detected which "navigates dispositions and practices despite dominant constellations of power" (2011, p. 157). Sitas sees this as a post-Second War and post-colonial phenomenon, although he admits that it may have earlier roots (in "ancient sources"). One of the features of this ethic is that "the other" is not regarded as "eliminable surplus"; furthermore, "the other" is regarded as non-enslavable and non-exploitable; and in addition, "the other" is *not* a "non"—and therefore is "non-excludable nor marginalizable" (Sitas, 2011, p. 572). Sitas emphasizes that dispositions of people in various geographical contexts not to think in purely "instrumental/rationalist" terms *do exist* and he cites a range of trends that bear witness to people's expressions of them, namely,

> … [alongside the] Gandhians there are "Freireans" as much as "Nyererians" as much as "Aung Saan Suu Kyi'ians", "Kibbutzim-ians", "Quaker-ians", "Neo-Khaldunians", "Ngugi-ans", "Ethiopians/Garvey-ans", "Sufi-ans" (in West Africa) and "AfroEuro-feminists". (2011, p. 574)

It is to these kinds of dispositions (and their strengthening) that Essed too is appealing (above); and I have suggested that the new manageress's response to me when I asked about Lindiwe's work offers a glimpse of a less instrumentalist vision of work relationships. Although this in itself does not solve the problem of the cheapness of Black labor as an index of exploitation (a concern raised by many participants), or the problem of continuing patterns of White privilege across the board, it would have made the quality of Lindiwe's life different if she had experienced a sense of care in the workplace at the time.

Interestingly, the same (new) manager who had mentioned to me about the outsourcing of the ironing stated to me that she believed that the decision not to continue to handle the laundry in the complex (and rather employ Lindiwe as an additional cleaner) might have been made even if it resulted in less profits for the owners of the Timeshare business. (The decision had been made due to weather constraints that made it difficult to organize the drying of the laundry.) Her understanding was that profit maximization was not the only consideration for those running the business. Whether her estimation of the dispositions of those running the "business" did resonate with their conceptions, we will never know—but at least this manageress recognized that profit maximization need not be the sole concern of business. This to me means again that there is scope for re-organizing economies if enough people engaged in businesses enterprises were to take on board such considerations. (I return to this issue in Chap. 6, when I refer to research geared to re-generating notions of wellbeing that question economic "rationalism", as explained in, for example, McIntyre-Mills, 2014.)

As Murove indicates, it is possible to offer a critique of economic structures that are premised on the idea of humans as essentially selfish, by pointing to the historical character of the structuring/patterning of the economy along these lines. (See Chap. 1, Sect. 1.3.4.) Murove treats Polanyi's (1944) outline of differing economic principles as suggesting that "economic relations that were based on greed or selfishness were actually invented in the same way that they were invented in the western societies during the era of early modernity" (Murove, 2005, p. 72). Murove proposes that the liberating effect of this account of Polanyi's is that we can understand that the patterning of economic relations along the lines of greed (and seeking to maximize profits at the expense of other considerations) is *capable of being shifted* and that this is not out of line with our human potentialities. Murove could be argued to be offering what Kvale (2002, p. 320), following Gergen (1978), calls a generative theory which points to different envisaged possibilities.

I wish to point out briefly here that the kind of "logic" used to create generative theorizing is neither "inductive" nor "deductive"—but can be called retroductive. (See my mention of this in Chap. 1, Sect. 1.2.1; and see my more detailed account in Chap. 7, Sect. 7.4.) Retroductive inferencing according to the exposition which I offer in relation to examples in my book on *New Racism* (2010) allows one to move

from "instances" back to some "hypothesis" that helps to explain the instances (and make some sense of them), while also invoking (together with others) the power of imagination. It is thus not a matter of deductive hypothesis-testing (criticized also by Gergen, 1978, p. 1352), nor a matter of "inducing" (which does not make provision for creative imagination). I offer much further detail on this point in Chap. 7, which is devoted to a discussion of responsible generative theorizing and how this can be supported by a form of retroductive inferencing. (See Chap. 7, Sects. 7.3–7.5.)

2.5 Storying in Relation to Race, Status, and Class Positioning: Exploring Possibilities for Action

To continue my storying in relation to this research, I now turn to the gist of a small group meeting that was arranged between Asanda, Boitumela, Thandi and me—which we held over lunch a few days after the FG meeting. The gist of my reporting on this is based on my shorthand note-taking and checking my notes with them from time to time as the conversation proceeded. (Markedly, Asanda had said to me that they would like to have a *group* follow-up rather than individual conversations with me. This request may have had to do with a language issue, namely, that Boitumela and Thandi did not feel fully comfortable conversing in English; and they felt that Asanda could act as translator when they were lost for the English word, as she had done from time to time in the FG meeting. It was also only later that I came to understand what Ndimande emphasizes—the importance of making provision for mother-tongue expression when researching with Indigenous people—Ndimande, 2012, pp. 216–217.)[8]

After Boitumela and Thandi explained to me why they had felt shy in the FG meeting (due to their not being sure how they might contribute and also due to language barriers), Thandi remarked that she had been pleasantly surprised that "the White man [Koos] wanted to learn about different cultures and wanted to know how

[8]This also came to light when I later facilitated workshops (2012) for researchers at Unisa on the conduct of FGs (as part of a project called the 500 schools project which I discuss in Chap. 3): it was mentioned by various Indigenous participants in the workshop that we needed to make provision for mother-tongue expression—hence we ensured during that research project that the team who would be conducting FGs in various South African schools must consist of some mother-tongue language users, and must make efforts to invite people in the FGs to speak in their home language if they so wish. I myself was not conversant in Zulu (the language used by the workers in Margate in this complex): Although at various times I tried to learn enough to converse, I realize (based on my own challenges) that when I was young would have been the best time to learn languages; and African languages were not offered as a choice of subject in my school (although I have heard from other White people that they were offered this choice). At this juncture (2016) what is called SAL (Second Additional Language) where White children will have to learn an African language, is being introduced as policy in South Africa.

to behave when for instance he enters a room, whether he must greet first or wait to be greeted".

Our conversation then turned to their indicating that they felt they were not sufficiently respected by both the Black and White staff when they went to the local hospital for their medicines. They expressed their feeling that White patients were treated better—due to the legacies of apartheid. I then asked the following:

Norma	So what do you all think may have changed post-apartheid?
Asanda	One thing is that there are far more grants from the government for things for Black people—such as child care grants and also housing and water than in the past. But there is still a lot to be done in the society. And poverty is still there. But sometimes people complain as if all problems are produced by the government. For example, if people build their houses too close to the river they cannot really blame the government if there is a flood. But there is a lot of blaming of the government for things that do not go well.
Boitumela	What for me is a problem is the low wages.
Thandi	I think this is the main problem—the low wages that I get. This makes it very difficult for me to manage my finances. And one cannot borrow money from a bank unless one can get a White person to stand for you as testimony of your being able to pay back.
Norma	Is it possible that maybe the Black people wanting a loan have not worked for Black people before and therefore they need a White person to give testimony as to trustworthiness at the bank?
Thandi	There is also a problem that Black people who have money do not treat other Black people so well as if they are rich. That is, if you are poor then often you are not treated well by Black people. They regard you with disrespect if you are poor. So if you are poor you are in a very difficult situation.
Norma	The other day I had heard Mbeki [the then President] talk of the issue of some Black people showing off their wealth and creating a stigma against those who do not have money. What do you think? Do you think this seems to go against the spirit of Ubuntu? (*The participants would recognize my reference to this as it is common parlance in South Africa. When acting in the spirit of Ubuntu, as* Chilisa (2012, p. 22) *citing South African Bishop Desmond Tutu, notes, people recognize that they belong to a greater whole and that they themselves are "diminished when others are … treated as if they were less than they are".)*

Asanda	In my experiences if you are poor and Black you can easily get treated with disrespect by Black people —they may even treat you worse than White poor people.
Boitumela	This is what I had meant when I gave the example in the beginning of our discussion of the person in charge of the clinic at the hospital—where I obtain very disrespectful treatment when I need my medicines.
Norma	Our lunch break is coming to an end. But maybe I can sum up that it seems to me that that is why you had said to Asanda yesterday that issues of racism are very complex in South Africa and it is difficult to cover the question of the workings of racism without a lot of discussion. (*Asanda had mentioned this to me the previous day as thoughts that Boitumela and Thandi had expressed*.)
All in chorus	But what we have discussed so far has been worth talking about.

Regarding this meeting, it is important to note that besides their mention of disrespectful treatment by certain Black people whom, they felt, were looking down on those who are poor, the issue of their positioning as Black workers was re-iterated several times in the meeting. The issue was furthermore raised by the gardener (Dumisane) when we spoke together in one-to-one conversation. Dumisane and I continued to discuss in various conversations later these issues that he raised—which could also be interpreted as an informal "member checking"[9]—but below I record the relevant extract of the conversation held a few days after the FG discussion. This conversation also shows how I took into account what he said about his low wages and how I interjected to ask him what, if any, support I might offer. This interjection in the conversation can be seen to be in keeping with Kuntz's plea for researchers to reject the "methodological fixation on maintaining the distance of research" (2015, p. 18). As Kuntz notes, "a differently engaged orientation of *working with* re-imagines responsibility as contextually situated and relationally aligned" (2015, p. 18). In terms of considerations such as these, I chose to mention the low wage issue as an issue of concern at a Trustee meeting when I became a Trustee for the body corporate—as I explain below.

Our conversation began with reference to some of the themes that had been raised in the FG discussion, especially in relation to the issue of White people fearing Black people (an issue that had been given some attention in the meeting, with different people expressing different views). Then Dumisane changed the subject:

[9]Member checking done in situ during FG sessions and in other conversations can consist of researchers/facilitators summarizing the gist of the discussion from time to time, also enabling "members" to add more depth (or modify) the discussion as summarized: see also Romm, Nel, and Tlale (2013, p. 9).

Dumisane	I would like to now talk about the issue of wages. I have had no increase in my wage—and it is a very low wage. I do many jobs at the building. I do painting as well as gardening—but I do not get paid for the kind of work I am doing. I am being paid as if I am only a gardener but in fact I am also doing painting jobs inside the flats that the White manager [he mentioned the name] asks me to do.
Norma	We could phone the Department of Labor to ask about this.
Dumisane	Yes, that would be good. Can you do this? (I later phoned on his behalf but was surprised to hear that the minimum wage in Kwa-Zulu Natal even for the jobs Dumisane was doing was very low—and that his wage was in fact legal.[10])
Dumisane continued	It is possible that the site manager respects White people more. I do not think that he would ask a White person to do all the jobs with not being paid enough. It is still expected of Black people to do lots of work for little money.
Norma	What you are saying makes a lot of sense to me: White people's work is often seen as more valuable—in South Africa and also elsewhere.

In relation to the problem of the cheapness of Black labor as expressed so many times by the participants (in the FG meeting, the small group meeting, and the one-to-one conversations), part of my storying around this is to mention that when I became a Trustee (2009), I was able to bring this up in discussion at a Trustee meeting (for the body corporate of the complex). In that meeting, the figures for workers' salaries were distributed. I used this as an opportunity to suggest that due to the different kinds of work being done by the workers (who all did multiple tasks) and the importance of the work for the complex, we should pay them more than the legally stipulated amount. (I explained that they were saving us from bringing in contract workers such as painters, etc. I also mentioned that once the

[10]Despite this legality, it is evidently possible for people who are determining people's wages to go beyond what is legally required, as my continued storying below will attest to. In addition, the Congress of South African Trade Unions (COSATU) has been struggling for what is called "decent wages" for workers. In my view, the question of minimum wages has to be coupled with steps to put a cap on maximum salaries of CEOs, etc. As I indicated in Sect. 2.4, examining and acting on these options is a slow process as these ideas continue to be marginalized in the society as being "unrealistic".

wage is divided it makes very little difference to each owner.) I was relieved that the other Trustees felt the same; and so this became implemented.[11] Dumisane did not get the benefit of this, because by then he had become ill and sought early retirement (and soon after that sadly passed away). But the other workers did, and continue to do so. I am recounting this in my story here to point to a principle—that each of us can operate in our various "fields of influence" to try to make some difference to the quality of human living in society.

Critics may of course argue that this gesture on the part of the Trustees (based, they might say, on a sense of pity bordering on patronage) is not the same as advocating for socio-economic justice. It might be argued that equity and socio-economic justice requires transformation at the structural level of macro-economic policy which would require, *inter alia*, White people to relinquish a significant portion of their wealth. As I mentioned in the Introduction to this chapter, advocating for structural change need not be mutually exclusive from appealing to people (whatever elites in the society) to act in terms of a sense of care and interconnection with others. While one can try to work for/with "immediate beneficiaries" of the research in concrete ways, one may still seek other avenues for "action", for example, by participating in debates about the economy on other levels. In 2012 the Minister of Finance (Trevor Manuel) set up a digital forum for people to discuss options for the economy, and in that debate I suggested that in order to ensure more decent wages as the "minimum" wage, one should introduce a "maximum" salary that can be earned—as a function of the minimum wage, for instance, not more than 30 times the lowest person's wage. This kind of thinking has not gained fast currency in South Africa although some unions too have been suggesting this (alongside calls for shifting patterns of White ownership of assets).

I now close my story in this regard by touching on some questions regarding Mandela's considerations around capitalism as a foundation for organizing economic relationships. Nash (1999) indicates that Mandela's shifting position on economic policy since his release from prison (1990) is well known. On the one hand, he considered that a form of socialism may be needed in South Africa "to overcome the legacy of poverty". On the other hand, he recognized the need to co-operate with business people and to not ignore their perceptions. In a lecture delivered in Singapore in March 1997, Mandela proclaimed that "South Africa was neither socialist nor capitalist, but was driven rather by the desire to uplift its people". Nash comments that Mandela's "ideological legacy" is "startlingly complex" (https://monthlyreview.org/1999/04/01/mandelas-democracy/).

In this chapter I have, needless to say, only touched on possibilities for keeping alive values and practices in South Africa that question the operation of an unbridled capitalism. But I tried to show how one can take some responsibility in

[11]Due to the legacies of apartheid, the Timeshare owners and also other owners are largely White, although this is now (slowly) changing. In the trustee meeting referred to, all the people in attendance were White.

various ways, by creating, and participating in, forums to research (explore together) possible options for re-thinking and re-evaluating the fabric of social and economic relationships.

2.6 Further Storying: One-to-One Interview/ Conversations with Koos and Lynette

In this section I consider my way of treating the one-to-one conversations with Koos and Lynette, as these illustrate by way of example how one might bear in mind Mertens' requirement to "give more weight" to the voice of the most marginalized (in this case the Black workers), while still engaging with more privileged participants. Mertens asks the question whether there are circumstances in which "a trusting relationship is not advisable and could even be viewed as harmful to the conduct of valid research". She mentions that "whereas the transformative paradigm raises many questions, answers to some of these questions await further work in this area" (2012, p. 811). That is, she invites further deliberation on the issue of "collaboration" with privileged participants. Kvale (2007, pp. 68–77) offers one avenue for (re)considering this when he presents his account of different kinds of interviewing ranging from more empathetic to more "confrontational". As will be shown in further chapters, empathizing with marginalized participants does not necessarily mean that one should desist from "confrontation" or what can be called critical questioning as in the case, for instance, of the research initiated by Woldegies (see Chap. 4). But in this context I felt more inclined to use an empathetic style of interviewing with the Black participants—in order to shift the sedimented patterns with which they were familiar of White people often not according them the appropriate level of dignity, that is, the same as they would accord White people. The Black participants (as expressed by some of them) were pleasantly surprised by Koos in the FG showing an interest in their cultural practices—and this too had what can be called a healing effect. But how did I choose to handle the interviews with Koos and Lynette? (Kovach emphasizes, 2009, p. 178, that there are no recipes for defining how to handle research with responsibility when trying to put more "balance" into human relations, as a relational exercise as part of the research.) In this section I narrate my choices.

As I mentioned in Chap. 1, it is understood by many researchers (working in and across many paradigms) that people's everyday language can serve to entrench relations of domination. An example can be taken from the arena of employer-domestic relationships as explored by various researchers across the globe, which also arose as an issue in conversation with both Koos and Lynette. For example, Nadasen researches the way in which domestic work in the USA—and by implication elsewhere too—is often regarded as "unskilled", thus legitimizing its low status. She makes these observations in the context of considering, with workers, the union organization of domestic workers in Atlanta (USA). She points out that

> despite the immense knowledge necessary to care for small children and maintain a household, most people judge domestic work as unskilled labor—and it is this which "legitimizes" for them the low status position of those in this occupation. (2010, p. 209)

In addition she points to the historical connection in the USA between domestic work and slavery, noting that even in regions of the country where slavery was not widespread, servants are overwhelmingly immigrant women and women of color (2010, p. 209).[12] She remarks that domestic work is also degraded due to its association with unpaid labor in the household (traditionally done by women and rendered more or less invisible). Interestingly, she notes that despite the occupation's low public standing, most domestic workers whom she interviewed "didn't have a negative view of household work. They didn't see it as an inherently degrading occupation, but took pride in the work they did" (2010, p. 210). She summarizes their stance: "In combination with improved wages, hours, and benefits, they sought individual empowerment and both public and private displays of respect" (2010, p. 210). This resonates with the research of Ally in South Africa, who makes similar observations (2010).

How then can (professional) researchers relate their critical interpretations—which arise out of their comparisons between different participants' views, out of their own observations of their relationships, and out of an evaluation of other researchers' conceptions—back to the "voices" of various participants? This section offers an indication of how I tried to critically engage with, and shift, certain "everyday perceptions", while still establishing a cordial (trusting) relationship with Koos and Lynette. I did this by way of organizing interviews/conversations and then later creating a follow up opportunity in which I offered the chance (by email) for them to consider my critical reflections.[13] That is, I took the opportunity a few years later, knowing that Koos and Lynette had access to email, to pursue this email option, after phoning to check if this was acceptable.

In Sect. 2.6.1, I first offer an indication of how the conversation with Koos proceeded in 2007 (shortly after the FG session). I present a brief narration of this, based on my shorthand note taking. In Sect. 2.6.1.1, I proceed to report upon our email encounters; and in Sect. 2.6.1.2, I present some of my further reflections. Then in Sects. 2.6.2, and 2.6.2.1, I turn to the (similar) engagements with Lynette.

[12]On reading this statement as expressed in this passage, critical reader Martin Mendelsohn commented (in face-to-face communication, 23 March 2017) that the link here can be detected/inferred via retroductive logic as he interpreted my earlier account of this logic. That is, sequences of historical events (such as created by the system of slavery) can become generative mechanisms that generate further social outcomes: slavery can be seen as forming the background to the way in which domestic labor of women of color became socially devalued.

[13]Nyamnjoh comments that with "the advent of the internet" forms of communication are opened up for obtaining electronic feedback during the research process (2007, p. 2).

2.6.1 *Interview/Conversation with Koos (2007)*

Koos started the conversation with me by indicating that he identified with Black people who stood up to their oppression. He said that had he been oppressed in the way the Black people were during apartheid, he too would have acted politically. He in fact told someone one day in a club (long before 1990) that "one day the Blacks will take over because a small minority cannot overpower a majority". With this beginning, the interview/conversation proceeded along the following lines:

Norma	How did people react to you in the club after that statement?
Koos	They ignored me after that.
Norma	So they were not impressed by your mention of the unsustainability of upholding the existing hierarchy and privileges assigned to Whites.
Koos	No; they were not interested in such a conversation.
Norma	I would like to hear a little about your childhood experiences.
Koos	I was brought up on a farm and on the farm I played with the Black kids. I did not notice their color.
Norma	Were you completely unaware of this?
Koos	Somehow I knew in the atmosphere that there was the idea that Whites were superior. But this did not affect me when I played with the kids. And I felt that Maria—the Black maid—was like a mother to me.
Norma	And how did other people in the house regard her?
Koos	We respected her and accepted her. We appreciated her very much. She would tell me not to do things and I would listen to her. Maria would also take my mother and my sisters and brother to her room—away from my alcoholic father. My mom used to hide out in Maria's room away from my father. My mom and Maria were very close. She confided in her and likewise Maria confided in my mom and they were friends. Maria would take me away from my father if he wanted to hit me unnecessarily, while my father calmed down. My younger brother who was not at school Maria also looked after. Maria was a carer. I used to go to her to sort out my problems.
Norma	Did she have the power to sort them out?
Koos	She sorted these out to the best of her ability. But she was restricted in what she could do to solve problems. For example, if the neighbor's kids gave me and my brother a hard time, she could not easily interfere—but she tried

	speak to them. She did not shout. But she reprimanded them. She was like a schoolteacher person. She also worked out the menu for the household—what we would eat. She cooked for us too. At lunch we all ate a sandwich together—but at night time after cooking she went to her room to eat.
Norma	Did people in the household make an effort not to make a mess or dirty up the house that Maria would then have to deal with?
Koos	I would pick up my clothes and put them in the washing basket and I would wipe my feet before I came in the house. This was something I knew to do. My mom was also neat, so she trained me to be neat and to realize that the place needed to be clean. She made it clear that once it is clean, you must not mess it up. If we did, Maria would reprimand us in a nice way. She was young and was very neat. But I did not make my bed or contribute to cleaning the rooms because I had to go to school—so Maria sorted that out. When I was at a young age—just after I had come first in class—my father chased me out of school and said I must go and work. I would have become a prefect. I saw myself as like youth in the Black community where young boys also had to go out to work. My father said to me, "it is time that you start looking after yourself as I have looked after you for long enough". I fainted when my father gave me this message.
Norma	This sounds sad indeed. What was your parents' political position?
Koos	They voted for the UP (United Party). (*He then explained to me, which I knew as my parents also voted UP, that UP stood for equal rights and an improvement in Black people's living conditions*.)
Koos continued	My parents did not talk much about politics but I knew that they opposed the NP (Nationalist Party). Because I heard a bit from them through the way they spoke against the NP, this influenced me too.

Harding (2006) points out that when encouraging people (during interviewing) to "remember the past", we can take it that memory is "a multi-authored, textual and contextual event"—and is not simply a recollection of "what really was" (as if there is one truth about "it" to be told). I concur with her that the process of

"re-membering the past and figuring one's place in it, is vitally caught up with processes of identity formation and transformation of self in the present and future" (2006, para 2.4). I interpret this also as implying that visions of possible ways of being-in-the world are brought to bear in the process of re-membering during interview encounters. Koos was forwarding this "way-of-being" in relation to Maria as a way of interacting with her that he remembered as "positive" (and/or that he wished to portray to me as positive). As Kuntz likewise suggests, when interacting with research participants in interview encounters, we do not need to posit a "core self" (in this case the self-definition of Koos) as an entity that "can be known and remains unchanged across time and space" (2015, p. 51). Nor do we need to posit that a "voice" of participants is something there to be retrieved during interview encounters independently of the relational context of meanings being constructed (2015, p. 51).

As indicated above, a few years later I presented Koos via email with some additional considerations of mine (further to the interview) to comment upon. I presented him with an opportunity to reflect on aspects of the interview by my injecting some additional thoughts which I presented to him—in which I also referred to some literature to open horizons for thinking which exceeded "everyday" thinking.

2.6.1.1 Some Reflections Presented to Koos as Considerations upon Which to Comment and His Response

I phrased my additional thoughts to Koos via an email message, in which I offered him a draft write-up of my considerations as in the following box:

> Considering some of the literature based on (other) research on domestic work, it is possible to suggest that although Koos was able to have a friendly and warm relationship with Maria, he may have learned during his upbringing to accept as "normal" the privileges of White people (what Spyros Spyrou, 2006, calls "entitlement"). Also, considering Koos's recounting of the position of Maria in the family—for example, as being a friend of the mother—many researchers have referred to the problem of domestic workers having to forego living with their own families and having to find other women to "stand in for them". In relation to this research was Koos perhaps taking it for granted that Maria's life had to be organized around this (White) family, with all her emotional work and problem solving in the home being directed toward *them*. Also, regarding Maria's going to eat in her room after cooking the evening meal, was this a mechanism for (re)-enacting status differences?

On seeing these remarks, Koos, commented upon them (by email) indicating that he did not get the feeling that he was entitled to all of Maria, and that in any case the issue of her not living with her own kids did not arise, as she did not have children as yet. Furthermore, he did not feel that her going to eat in her room at night was a mechanism for creating social distance on the part of the White family. This is how he stated his position (using the third person):

> What has been left out of this story is that Maria was very young, when she worked for Koos's family and was not married yet. Maria had a room on the property and worked only week days from Monday to Friday and her hours were from 7 a.m.–4.30 p.m. As soon as the Mother returned from work Maria went off. But Maria would then first get her food and retire to her own room where she wanted to go and eat her meal and do her own thing. That was her own preference and she could eat in the main house if she wanted to. But she considered it a natural thing to live like that—that is, not to eat the evening meal in the main house as she learned this also from her parents.
>
> Maria was a very close friend to the family but was not taken for granted as mentioned in the reflections.
>
> Maria was also a great believer and she use to pray daily as the Mother did and the children were also God-fearing people (Christians). Now in this home the Father was an alcoholic and if he got to drink too much, he got abusive toward the Mother and the children and that is when they would go to Maria's room that was like a small flat. Then they would wait for the Father to go to sleep and then they also went to their own bedrooms in the house.
>
> As far as Maria being a family counsellor, this did not mean that too much emotional work was required of her. She was not compelled to do everything that was mentioned [during our interview in 2007].
>
> In addition, Maria was one of the best paid servants in the area. When the Mother went on leave Maria also went on holiday. [Koos was unaware of the language of *domestic worker* or *helper to* describe a different relationship than one of servility—hence his use of the word "servant".]
>
> These people [the whole family] had a very good relationship with her so much so that after we moved from Welkom to Klerksdorp, Maria wrote to the Mother to ask her if she could come and work for them again. And she did for another year then she got married and went back to Welkom, but they used to write each other ever-so-often if there was news available.

2.6.1.2 Additional Analysis/Reflections Based on Considering This Response

Looking at Koos's response it could be said that in some way Koos was engaging with my reflections by having to reconsider his responses, but in another way he was more interested in offering me additional information to clarify his understanding of the relationship with Maria, rather than to review it (or his memory of it) in any substantive fashion.

Despite Koos's understanding of the "good relationship" one could of course argue on the one hand, following authors who have stressed the asymmetry in social positioning, that the notion of Maria being a "family counsellor" *hides the asymmetry in the relationships with the White family* (so that even to Koos it became invisible). As an anonymous reviewer of this book suggested, "helpers always know that there is a power difference between themselves and the people who hire them". The reviewer justly indicated that "the perception of the Whites may not be shared by the domestic helpers". This point is worth stressing. On the other hand, one can consider that the language of intimacy as used by Koos (and Koos's appreciation of Maria and all her kindness) harbored the seed for him—despite the racialized social order—to see Black people, and specifically Black workers, in human terms. Also, his mother teaching him (as he expressed in the original interview) that he must not mess up the house once it is cleaned (so as not to make more work for Maria) was perhaps a message that penetrated his consciousness so that now it is indeed normal to him that one does not see domestic workers (or any workers) as infinitely exploitable in terms of extracting as much from them as possible (for the least possible salary). He also takes cognisance that Maria rightfully needed to develop a relationship with her own children (once she had them) and that it would have been unfair practice to expect all her emotional work to be directed toward serving the (White) family or to have expected her to give more of herself than she chose to do (in terms of solving problems within the family).

One could say that in his childhood home as recounted by Koos it seems that some consideration was given to the domestic worker not becoming overworked.[14] Hence Koos was not exposed to a situation of witnessing extreme class exploitation on this score. Regarding the question of job security, as indicated earlier, pre-1994 this was not a realm that had any legal protection; and as Ally indicates (2010), the fact that some job security became legalized post-1994 was regarded by many domestic workers as important to their dignity. It seems that in Koos's home this was not an issue that came up—as Maria stayed with them for a long period and the question of her being summarily dismissed did not arise (given the closeness that became established in their relationships and the sense of their being mutually interdependent).

[14]Critical reader Mendelsohn reminded me (pers. comm., 23 March 2017) that I should emphasize here that this was the recollection put forward to me by Koos, where he may not have wished to remember, or to recount to me, elements of racism as operative in the home. That is, he may have edited these out for the purposes of the conversation with me. Participant Tracey (see Fig. 2.1) too indicated to me (in June 2017 when I showed her some parts of this draft chapter) that she felt from the FG discussion that Koos may have been wanting to portray a "non-racialist" self in this research. Nonetheless, I would suggest that his portraying what he considers important, at least points to a potential way of operating which he expresses that he values and which he implies is a valued way of being (as also pointed out by Harding, 2006, para 2.4).

Can we then suggest that in this home—as with some of the cases cited by Spyrou (2006, 2009) in his exploration of children's relationships with Asian domestic workers in Cyprus—we have some guidelines for shifting dynamics of racialized and classed relations toward what Spyrou calls "intercultural intimacy and relation building" (2009, p. 156)? Spyrou explores how 13 Greek Cypriot children whom he interviewed (as part of a research project) understood their relationship with the Sri Lankan and Filipino domestic workers in their homes. In remarking upon the orientations of the 13 interviewees as a totality as interpreted by him, Spyrou highlights that a "particular understanding of Self and Other emerge that suggest both cultural distancing and closeness" (2009, p. 170). He indicates that "all the children (with the exception of one boy) said they would be sad if the domestic worker were to leave" (2009, p. 170). Children described this closeness between themselves and their domestic worker in different ways: Some children, he notes, "saw the domestic worker as being almost a family member, a member of the house, a friend, or more like a mother—though not all children felt this way" (2009, p. 170).

But along with this sense of closeness, Spyrou points out that at the same time these children often "suggest a sense of superiority and power over these women" (2009, p. 170). Despite his pointing to ways in which children exercise their felt authority, Spyrou also highlights the "more positive" aspects of relation building that he sees the research as also bearing witness to. As he puts it,

> the young are implicated in the social and cultural production and reproduction of problematic human phenomena like racism and nationalism, but [the research] also can inform our understanding of more positive human conditions like intercultural intimacy and relation building. (2009, p. 156)

Spyrou clarified this in an email to me on 23 April 2015 as follows:

> For me, coming across these contradictions/ambiguities in research reveals the complexities of social life where individuals and groups are caught between conflicting discourses and possibilities. Acknowledging this complexity and recognizing that the world we live in is anything but straightforward and singular or clear and consistent, allows us to move beyond simplistic identity labels (i.e., these kids are racist, etc.) and to reflect on the tensions which emerge and, as you say [referring to my draft write-up of this], offer possibilities.

Linking this with Koos's comments on his childhood (aspects of it which he chose to recount to me)—can we perhaps suggest that the encouragement of reflection around relationships between employers and domestic workers in homes can constitute an arena for (adults and children) learning about mutually respectful human relationships? Could it be proposed that there is value in setting up research FGs and one-on-one conversations with adults (employers and domestic workers) and children using an "active" approach—to open up more conversation as part of a research effort exploring what can be done to make homes sites for learning such

patterns? This would require that researchers—professional and others—initiate more discussion forums and other (informal) interviews/conversations with a view to raising the level of conversation around this in society. This would be a way for us to try to contribute to interrupting dynamics in which homes become sites where (more or less) unmindfully racialized and classed relationships become further entrenched.

Upon seeing these deliberations of mine—in the draft chapter—Spyrou commented that:

> At one level, it is what you suggest about this [the interviewer as a transformative tool] offering avenues for critical engagements in the domestic sphere and rethinking relations with others. At another level, it also offers much food for thought for rethinking research, reciprocity, and ethics. Your thinking suggests moving away from a model of simply recording what is out there and toward another one where the researcher is fully engaged in a critical dialogue with research participants without pretentions to neutrality and emotional distance. I think this offers much food for thought as far as research and its transformative power is concerned. (Spyrou, via email, 23 April 2015)

Spyrou expressed this in another way in personal conversation between us (in Cyprus[15]) as follows: He suggested that in the more conventional conception of interviewing,

> you take away your responsibility for being part of the meaning of the interview. It is as if you are outside of it, whereas you are co-constructing the interview you are carrying out; whether you recognize this or not, it is there. It is a question of how far you take it. In the picture of the neutral interview your impact is camouflaged. (Spyrou, pers. comm., 27 April 2015)

It is in this light that we can read my interview/conversation with Koos and my email invitation to him to reflect later on his statements. In Sects. 2.6.2 and 2.6.2.1, I report upon my encounters with Lynette in similar vein.

2.6.2 Interview/Conversation with Lynette (2007)

I start this section with the interview/conversation that transpired between Lynette and me (again with an exposition based on my shorthand notes) very soon after the FG discussion in 2007.

[15]We agreed to meet as I was coming to a conference in Cyprus (Limassol) and we had earlier (12 years prior to this) worked together at the European University of Cyprus (EUC). We met for an hour at the EUC; and right at the end of our chat, he made this point, which I jotted down on paper in shorthand. I told him it was well expressed! He said he was re-iterating my position on this as he understood it from having spoken to me 12 years ago and also from having read the draft chapter I sent to him to look at/skim on 22 April 2017 (to which he replied by email on 23 April). I said to him that these words of his were still well chosen, and an interesting way of expressing this idea.

In the course of the conversation, in which I asked her about her upbringing, she indicated that she was brought up on her uncle's farm in the Free State (a province of South Africa). She then moved on to speak about her relationships with Black people. (This must have been because she considered that the research was about the dynamics of White/Black relationships.)

Lynette	I had a close relationship with Anna—the maid—as well as playing with the Black children of the farm workers. Anna had a relationship with my mother such that she could even stop my mom from hitting me if my mom was cross with me. Anna would step in and prevent this from occurring. (*Similarly to Koos, Lynette referred to the domestic helper as "the maid", unaware that this wording in South Africa—and elsewhere—is being shifted in certain circles*.)
Norma	When you played with the Black children, how did your parents respond to this?
Lynette	Although my parents were NP [Nationalist Party], they would not try to stop me from playing with the Black children.
Norma	Did your parents offer any indication to you in regard to White people's marrying of a Black person? Would your parents condone this?
Lynette	My parents would suggest to me that I must not marry a Black person. But as far as being together on the farm, all the children on the farm [White and Black] tended the sheep together, wiped the lambs' bum together, stole and ate biltong together—and when we were caught out we all got a hiding [beating] together. Speaking about Anna, she came twice a week to look after the house. My mom cooked most of the time and Anna cooked some of the time. Anna sat at the same table as the rest of the family at eating times. I considered Anna to be like a second mom to me. I liked having a second mom in the house and I saw Anna as protective of me. I felt very close to Anna and kept in contact all the time through my adult years with her—including when she came to live in Margate.
Norma	What did Anna's work consist of in your childhood home?
Lynette	During my upbringing both my mom and Anna worked together to sort out the household chores—looking after the clothes, cleaning the house, polishing the floor, etc.

	My mom did not sit while Anna worked. And if someone died in the family of Anna, they would all sit together and cry together—and our family would try to help her financially.
Norma	Was your mom considerate with Anna in telling the children not to make a mess that Anna would then have to clean up?
Lynette	There was no chance to make a mess because my mom was very strict. So no extra work was created.
Norma	Do you think it was partly your mom's motive in telling the children to keep the place clean so as not to create additional work for Anna out of a sense of consideration?
Lynette	Yes, if any one did make a mess they would have to do the cleaning up—not Anna.
Lynette continued	I called her "ou Anna" [old Anna] out of respect for her because old people are regarded with respect. I used to hug her a lot too.

In short, Lynette wanted to emphasize to me that in her household there was no hostility toward Black people—but there was an understanding that they must not get close enough to marry.

2.6.2.1 Some Reflections (Presented to Lynette to Comment upon)

In 2011 (around the time that I also re-approached Koos in this regard) I presented Lynette with my reflections via email (having first phoned her to establish how she felt about seeing them). I presented her with my interpretation/analysis as follows:

> Overall, in relation to this story, it seems that Anna was being seen as a *person*, and not just "the means to an end" in the household. But it is possible that although Lynette was able to have a friendly and warm relationship with Anna, she may have learned during her upbringing to accept as "normal" the privileges of White people (what Spyros Spyrou, 2006, calls entitlement). As one researcher (Carlis Douglas) stated to me (in another context which I am relating to this one) "it seems that people can develop warm relationships in the presence of huge inequalities".[16] In view of

[16]Carlis made this observation during a Skype conversation (2008) where she, Susan Weil, and I were talking about the complexity of racism as part of our venturing to write a co-authored book in which we held cross-racial discussions between ourselves around the dynamics of everyday racism. However, with Carlis being in the UK and Susan being Italy (and my being in South Africa) the co-ordination of our discussion became too time-consuming: once the publishing deadline was reached with no possibility of yet another extension, we decided that the material that

> this observation of Carlis, was the warmth in this case perhaps obfuscating (in Lynette's consciousness/sensibility) the fundamental inequalities perpetuated by the apartheid system?

Upon seeing this reflection Lynette did not comment further on the perpetuation of fundamental inequalities, but drew my attention to the fact that after the family moved away to another town both the parents and the children kept up telephonic relations with Anna—including very recently she mentioned they had still been in contact. She emailed to me in narrative form as follows:

> Anna the maid used to work for the family for many years and then she went to stay with her children. When Anna got older she did not need to work anymore and in any case our mother passed away and all the children got married and had their own houses, but Anna used to keep in touch with the oldest daughter [of the family] who was also called Anna. For years and years every Christmas she used to phone and wish them a Merry Christmas and a happy New Year. This daughter used to keep also in contact with Anna because she was very special to the family. I too used to keep in touch with Anna the maid. Because Anna [the maid] had a great relationship with my mother and then I would go off scot-free, I also always shared my sweets with Anna.

Lynette felt that all this was some indication of the strength of the warmth which was circulating in the household and she wanted to alert me to this. In an attempt to "sum up" here I would say that Lynette was expressing her sense (as remembered) of the contours of a more humanly connected and mutually respectful relationship than would be expected at the height of apartheid, especially with parents voting for the Nationalist policy. And perhaps Lynette having to reflect again on this through this research process (of us together making this more visible as a possibility), could influence her relations with the current domestic worker in her home? While it is not possible to say whether the research had any effect, at least the way in which I framed it (to make visible this potential) could have had a positive (if any) effect—in terms of human relational possibilities.

Nevertheless, as with my commentary on Koos, I suggest that research needs to continue in imaginative mode to open up more conversation (e.g., via FGs) with adults (employers and domestic workers) and children around what could be done in homes in order to make them sites for establishing more relational connection—much along the lines expressed to me and to my aunt by Tshimangodzo (see my personal narrative in Sect. 2.2).

I had produced in relation to the South African research could be distributed in a book which I would write (while Carlis would, time permitting, would take the lead with organizing the rest of the material, toward another book). Some of our deliberations on Whiteness can be found in the International Sociological Association newsletter (2011, pp. 11–14): http://www.isa-sociology.org/pdfs/rc05newsletter_october2011.pdf.

2.6.3 *A Brief Note on Classed and Not Only Raced Relations: Black Poor People Often Devalued*

In another conversation with Asanda (one of several that we had after the FG opened the possibility for talk around race and class), she volunteered some information regarding Black affluent people not respecting those who are poorer. She gave me an example of a Black friend whose house she had recently visited, who allowed her children to dirty the house with their muddy bicycle on the understanding that the (Black) domestic worker would have to clean up the mud and dirt. She said to me that Black people are not good to their staff and she herself would not want to work for a Black boss. I asked her if this is not prejudicial and she said that this was based on her experience of Black people not treating well those whom they regard as "below" them—especially if you are Black. She agreed with me that she may be generalizing too much based on what she thinks she has observed, but she also said this behavior toward (poor) Black people may be a continuation of the apartheid system, where Black labor became devalued. Asanda suggested that the legacy of apartheid-styled racism (with Black labor being undervalued) remains in that the labor of Black people is not respected as valuable (especially if they are workers) and is degraded as work. In other words, Black labor is still assigned a specific meaning, rendering Black laborers specifically vulnerable to being treated with disrespect.

When reading this anecdote with reflections as offered by Asanda, critical reader Francis Akena made the point that he felt that "it is important to let this conversation transcend the geographical boundary of apartheid and post-apartheid South Africa and to draw examples from other societies". He noted that through offering his own experience (as detailed in in the narration below) he wished to "build on what your participants have said and also to show that maltreatment of poor black domestic workers by rich black families is not just a legacy of apartheid". He suggested that "more research needs to be conducted to understand and unravel this injustice".[17] He suggested that his own narration may help readers to "start questioning and also challenging this kind of injustice that in most cases takes place quietly in the backyard of rich black families". He recounted one of his experiences (by adding to my draft text in July 2016) as follows:

> If my experience can weigh in: as a teenager, I was a domestic worker in an affluent neighborhood in Entebbe, a city located about 45 km outside of

[17]In other parts of Africa too, this orientation to (Black) domestic labor has been recorded. For instance, Miles (1996), citing Maziya (1993), notes that in Swaziland "domestic work is … characterized by low pay, long working hours, poor living conditions, dependency, invisibility, exploitation and even abuse" (Miles, 1996, p. 82). And Nyamnjoh (2006) discusses the relations between madams and maids in the context of xenophobia in Botswana—where Black people regarded as "other" become targeted for maltreatment.

Kampala, Uganda's capital. After completion of High school in the year 2000, I decided to look for a temporary employment as I waited for University admission to Makerere University since I had passed well in the national examination and was optimistic of being taken in on government scholarship. My cousin who was also a domestic worker (in Entebbe) had travelled to Kitgum district (a small town where I lived with my mother and siblings) about 450 km north of Entebbe for a short vacation. When I told him that I was searching for a job in town, he offered to take me to his boss in Entebbe who was looking for an additional "houseboy/girl"—as they are called in Uganda (domestic workers) and that the boss would be pleased to employ an educated person like me. Since I was coming from an economically improvized family, this idea was "God's blessings" coming at an appropriate time when my mom could not afford to pay for the extra requirements for university education. My salary from the "houseboy" job would take care of that. Upon arrival at my employer's home, I discovered that my cousin was not living in the same house with my boss's family. He was posted to the family's other rental property about 3 km from where I would be living. The boss introduced his family (wife, teen daughter and two twin sons—my contemporaries). I was given the terms of contract: do house chores such cooking/cleaning dishes (I had never cooked in my life before), cleaning the house, lawn mowing, taking care of the family two dogs, doing family laundry and ironing, washing the three family cars, securing the gates at all times from intruders etc. My monthly pay was 70,000 Uganda shillings (about $28 USD by then).

I was excited about the pay and didn't so much think about the amount of work involved. Then the reality struck in. I realized that I had to wake up every day at 4 a.m. to start preparing breakfast for the family who leave the house by 5:30 a.m. to drop the teenagers at school before proceeding to work. I would then spend the entire day working in the house and also compound. When they return from work/school, it was my job to offload whatever was in the car as the kids dashed to their rooms or watch TV in the living room. I had to cook dinner, serve the family and only eat after they were done. Besides, I was to make sure that all the dishes were washed crystal clean before retiring to bed. Hence every night, I went to bed between 11:30 p.m. and 12 a.m. very exhausted and yet had to wake up at 4 a.m. to start another day. To make matters worse, the boss (husband) treated me with contempt. He often yell/threaten me for minor mistakes. It was mostly the intervention of his wife who would remind him that "Francis is still new, please treat him calmly and give him time to learn" that breathed a sigh of relief in me each time I errored in his presence. Life was a nightmare in this rich black family home. The mistake I made was that I trusted them when I arrived from my village and told them everything about my family. They knew that I was raised by a single mother (my father died when I was young) who was not formally educated and that my family was living below poverty line and hence I was

vulnerable. After three weeks, I could bear it no more and waited for an opportunity to escape. That golden opportunity came on Friday of the third week in the home. One of the twin boys had stayed home due to reasons unknown to me. Because he was my contemporary, I lied to him that I was having a terrible headache that comes occasionally around that time of the year. I told him that it could only be cured by one herbalist in my village in Kitgum district and so I have to leave immediately to go back to my village and that I would return after getting well. His pleas to me to wait for his parents to return and give me transport money fell on deaf hears. I parked my belongings, showed the contents of my tiny bag to him and walked out of the family gate at about 10 a.m. so that they don't accuse me of theft. That was the last time they saw or heard from me. I didn't get even my first pay cheque. A week earlier, I had later learnt (from one of the twin sons) that this family had moved to Uganda from Rwanda during the 1994 genocide and became Ugandan citizens by naturalization.

In sharp contrast, about 3 km away my cousin (the one who brought me from the village) who was working as a domestic worker (house boy) for a white family renting my boss's property had dignified treatment. He worked less hours with less workload compared to mine. He could go to bed early and wake up late because the white family made their own breakfast and sometimes left home when my cousin was still in bed. They allowed him to have Sundays off and occasionally took him out on family picnics. He was entitled to two weeks' vacation with pay each year. He felt at home away from his real home. I didn't enjoy any of those privileges and every day I missed home which now seemed like a million miles away. Life was a miserable tragedy. My morale to raise money for university education was crushed and my spirit went into silence. Though my salary looked fat to me because I didn't know how much domestic workers were paid, I couldn't understand why I was being treated like a mule. Today, I still believe that I would have had a better and rewarding "houseboy" experience if only I was employed by a white family.

Further to my seeing his story as he recounted it in the passage above, Akena and I had a conversation where we tried to further theorize/make sense of his experience as compared with that of his cousin. I asked him whether possibly his class position is what was paramount in this story, and he replied that the family in which he was working felt a sense of superiority toward him by virtue of their knowing that he came from an impoverished family. They saw him as inferior and also as being vulnerable; and they capitalized on his position, where they felt that he could exercise no agency. Therefore, they exploited him to the full. (When the time came he was able to exercise some agency in running away, and he found a teaching job at a school when he returned to the village.) Akena stated in addition that they were hypocritical Christians, in that the father was an elder in the church,

espousing values such as love and treating people with dignity, while he did not practice these at all.

In contrast, he noted that the (rich) White family, practiced a genuine Christian approach (they belonged to another church) and therefore treated his cousin "like one of their own"—caring for him and ensuring that they did not mistreat him. He felt that the White family cared less about his cousin's (low) socio-economic status and focused instead on him as being a person: therefore, they treated him with dignity and respect. In other words, we (Akena and I) summarized together that this (White) family could be said to have had less of a sense of their own "class" position and were able in some sense to work outside of "classed" relationships in their treatment of his cousin.

The moral of the story which we decided here is that being rich in resources does not imply that one is bound to mistreat others; although they could have exploited his cousin's vulnerable position in the class hierarchy, they did not. Instead they chose to treat him as part of the family. Akena attributed this to their different spiritual orientation to the world (than that of the rich Black family). As Akena stated to me "they believed in their faith".[18] This is in keeping with Akena's understanding that it is important that people infuse their being in the world with a (genuinely) spiritual orientation and also that in the academy more room is created for conversations around spiritual matters (see Chap. 1, Sect. 1.3.5).

This would also tie in with Sitas's considerations (2011, p. 574) around using the research space to highlight orientations, including spiritual ones, which are not purely "instrumental/rationalist" (see Sect. 2.4). Readers of this chapter here are meanwhile invited, as Collins (2000, p. 38) advises, to write further into the story that I have presented (which I have tried to write in a style that invites further "writing into", and which Francis Akena chose also to write into). In the next section I turn to some considerations concerning ways of judging the validity of the research as presented.

[18]Interestingly, when reading this analysis (March 2017), critical reader Mendelsohn offered an alternative/additional interpretation, namely, that just as in the French expression "nouveau riche" (to refer to people newly rich in different contexts), Black people who have historically been underprivileged may now be more class conscious, that is, more aware of class positions. So the attitude on the part of the White employers versus that of the Black employers recounted by Akena could perhaps be attributed (in part) to this.

2.7 Judging the Validity of This Research: Rethinking Validity

In remarking upon invocations of notions of validity to judge qualitatively-oriented research endeavors, Connelly and Clandinin express that as they see it the concept of "validity" has no place in qualitative inquiry. Citing Van Maanen (1988) they suggest that, "like other qualitative methods, narrative inquiry [as a form of qualitative research] relies on criteria other than validity, reliability and generalizability" in order to judge its quality (1990, p. 7). However, other authors suggest that it is still meaningful to talk of validity in qualitative research, *as long as the term is reworked* to do justice to the distinctiveness of what is being, and potentially can be, offered in such inquiry. (See Chap. 1, Sect. 1.2.)[19] In this vein, Collins indicates that she does not wish to "duplicate the positivist [realist] belief in one 'true' interpretation of reality", but wishes to acknowledge that the statements made by her in relation to "the world" relate to a world that becomes presented by virtue of the interpretations, values, and feelings that are brought to bear in its appearance (2000, p. 297). This means that there is no attempt to uncover (or even get close to uncovering) a non-perspectival or value-free "truth".

An option for still judging quality as identified by Collins (1990, 2000) and further explained by Kvale (2002) is to invoke what Kvale summarizes as "communicative validity". (See also Gregory & Romm, 1996, 2001, 2004; Romm, 2001; Romm & Adman, 2004; and Romm & Hsu, 2002, for additional accounts of mine, with reference to examples in different contexts, of ways in which research can be consciously directed toward generating increased societal reflection and dialogue.) Kvale explains that:

> Communicative validity involves testing the validity of knowledge claims in a dialogue. Valid knowledge is not merely obtained by approximations to a given social reality; it involves a conversation about the social reality. (2002, p. 313)

For instance, as shown in this example, conversations around the meaning of racism and links to cheap labor were explored with the various participants. We were together theorizing around the meanings of race and racism and their historical connection with "class" (albeit that the participants did not use this word). We were exploring perspectives on the (perceived) links, also with view to considering what may be possible leverage points for change (See Chap. 1, Sect. 1.3.1.) The spirit of this research approach can ideally also serve to unseat theoretical conceptions (theorized in scholarly literature and in everyday life) which suggest that social

[19]Very few researchers justifying their qualitative inquiries speak of *reliability* in the sense of consistent "results", because it is generally recognized that researchers' way of speaking with participants necessarily affects the data that comes to the fore in qualitative research. Hence reliability as consistency of results collected at different time instances (either by the same or different researchers) is not regarded as a requirement in qualitative research (Ngulube, 2015, p. 152). Nevertheless, some criteria of *validity* are normally advised, albeit that these need not be linked to requirements of "accuracy" (see, for instance, Cho & Trent, 2006).

dominance, as manifested in forms of oppression and exploitation around social categories of race/ethnicity/gender and class relations, is ubiquitous across all societies and hence incapable of change. (See Romm, 2013.)

As we engaged together in speaking about racism and worker experiences, our theorizing can be said to have sprung out of what Onwuegbuzie and Frels (2015), following Onwuegbuzie (2012) call an *emtic* relationship between researchers (or research facilitators) and research participants/participant researchers, in which the viewpoints expressed in the encounters are "maximally interactive" (2015, p. 162).[20] Onwuegbuzie and Frels suggest that FG sessions can become inquiry arenas in which research facilitators "co-construct knowledge" with research participants: the emergent theorizing is grounded in everyday dialogues as well as extended viewpoints that become co-constructed as part of the research process. Onwuegbuzie and Frels indicate that this

> transforms the research from representing an etic perspective [in which researchers assume an "outsider" vision in offering interpretations] or … an emic perspective [grounded in insider initial views] to an *emtic* perspective (i.e., "representing the place where emic and etic viewpoints are maximally interactive"). (Onwuegbuzie & Frels, 2015, p. 162, citing Onwuegbuzie, 2012, p. 205)

Onwuegbuzie and Frels consider that it is important for researchers as research facilitators to contribute viewpoints in a process which is indeed interactive so that they do not assume the role of "outsider" analysts (the so-called etic view) or of simple recorders of research participants' "insider" (emic) perspectives. The idea is to try to generate interactive encounters in the field, in which those involved in the research (including the research facilitators) can all participate in an exchange.

The emergent theorizing can become validated in terms of a "pragmatic validation" (Onwuegbuzie & Frels, 2015, p. 162). As I indicated in Chap. 1, (Sect. 1.3.2), Kvale describes the notion of pragmatic validity as follows:

> To pragmatists, truth is whatever assists us to take actions that produce the desired results. Knowledge is action rather than observation; the effectiveness of our knowledge beliefs is demonstrated by the effectiveness of our action. (2002, p. 316)

Kvale cites Marx as stating in his second thesis on Feuerbach that the question of "whether human thought can lead to objective truth is not a theoretical, but a practical one. Man [sic] must prove the … power [or worth] of his thinking in practice" (2002, p. 316).

---

[20]The terms *etic* and *emic* (which Onwuegbuzie combines into *emtic*) are associated with a distinction that can be found in the social science literature following Pike's original coining (1954) of the words in linguistic theory. Ways of understanding these terms, as with all scientific ones, vary; and many authors have noted that they function more to signal a continuum than to locate exclusive categories (cf. Franklin, 1996; Morris, Leung, Ames, & Lickel, 1999; Olive, 2014). What I wish to highlight in what Onwuegbuzie (2012) calls an *emtic* relationship is that in practice so-called etic perspectives (introduced by "outsider" researcher understandings) and emic perspectives (generated by participant "insiders") can be *interactive* during the research process, as explored by Onwuegbuzie and Frels (2015).

Kvale proposes that one of the advantages of a "pragmatic knowledge interest" is that it can "counteract a tendency of social constructionism to circle around in endless interpretations" (p. 317). I have suggested in Chap. 1 (with reference to discussion on the transformative and Indigenous paradigms) that if one "gives more weight" to the voices/concerns of those most marginalized in the social fabric, one is not, in Kvale's terms, "moving in circles"—but is taking some responsibility for moving toward a more just social order. For example, the many participants having told me about the poor wages that they received (which they saw as the devaluing of Black labor) was an invitation for me to try to address this in some way in my "field of influence", which I did attempt at a Trustee meeting when I became a Trustee. This opened the possibility of my noticing (in action) that the Trustees did not object to offering a higher wage on human grounds. It also opened the possibility for some "generative theorizing" around issues of raced and classed relationships in this, and other, contexts. This can be conceived in terms of mitigating what Essed (2010, p. 144) calls the micro-everyday inequalities in social and economic relations, as well as in terms of macro levels of what Giri (2011, p. 606) calls the "hard variables" related to the structuring of life chances.

As I indicated in Chap. 1, one can link pragmatic validity to what Lather (1986) and Lincoln and Guba (1989, 2013), and various authors taking up their suggestions call "catalytic authenticity". Here research is judged in terms of attempts made consciously (and actively) to render the research "useful", where one can at a later stage ask for testimonies/reports from participants as to any felt benefits of the research so as to assess the catalytic validity of the research (Frels & Onwuegbuzie, 2012, p. 6; Mertens & McLaughlin, 2004, p. 108). As far as asking for "testimonies" of this from participants is concerned, I did not seek these as such, but Dumisane expressed that he found that the cross-racial process of "just talking" was already beneficial. Furthermore, one of the benefits was expressed in the three-personed follow up after the FG session, when Thandi voiced surprise that Koos was interested in hearing about their cultural heritages (and about their politeness protocol, so that he could act accordingly in other contexts too). And I did manage to catalyze action (with other stakeholders) based on my speaking at the Trustee meeting about the matter that concerned many of the participants—their wages. In Mertens' terms, the community (and specifically those most marginalized) can arguably be said to have been left "better off" (2011, p. 196) than prior to the research. And hopefully wider communities can benefit from the deliberations as I have presented them through my storying around the various relationships (and through my highlighting their historicity).

In line with a variety of suggestions that have been proffered by a range of authors to judge validity in terms other than a correspondence theory of truth (where one is seeking to develop "correspondence" between statements and some posited outside reality), I have pointed to other ways in which the research as presented can become justified/accounted for. My overall narrative with reference to the mini-stories as told here requires further input by readers who are invited to reflect upon their content. Collins explains in this regard that there is much to be gained by allowing for a story to become built up "from a multitude of different

perspectives"—accepting that different authors can contribute "missing parts to the other writer's story" (2000, p. 38). It is important here also, as Collins notes, not to concentrate only on accounts written up in books and journals—but to include as credible those accounts that may be expressed in any communications shared with others (2000, p. 253). I have therefore tried in my (small) way through this chapter to undercut the dominant credentializing processes that have thus far led to the undermining of "unconventional" approaches in our quest to (co-)explore, while also potentially make a difference to, our social milieu. (Through my dialogical view of "generative theorizing" I have also attempted to leave openings for audiences to engage further with different ways of seeing the research and its possible implications for action on their own part and/or on others' parts.)

2.8 A Concluding Note

Looking back now, and considering Cannella and Lincoln's injunction (2007, p. 316) that researchers (that is, those initiating research processes) need to ask questions such as "what is my privilege (or power position) in this research?", I would suggest that I tried to take into account how my status (as "inquirer" initiating an exploration) would be perceived. Clearly my being White (and also not a worker/laborer) meant that in historical terms I would have been seen as being more powerful. I made an effort to shift this perception somewhat by trying to be culturally responsive, such as in the way the "meeting/research circle" was set up (taking into account the views of participants as to how they may or may not participate). I tried to develop a friendship/informal type of interaction instead of a researcher versus researched one. (Ndimande, 2012, p. 216, notes that FG research is specifically suited to Indigenous "informality".) And I also sought feedback on various occasions from participants as to how they were experiencing the endeavor.

Signaling explicitly to participants (as I did from time to time) that this is an exploration exercise in which we were all co-exploring or re-looking at issues seemed to me to be a feasible way of shifting power relations. Nevertheless, readers may argue that the status which I had—as White and as a "researcher/bookwriter"—could not easily be nullified by my stating to participants that we were in a process of co-exploration. Be that as it may, some cross-cultural goodwill was, I suggest, at play as we all tried to converse around the issues. Sikes proposes in regard to researchers and participants "othering" one another, that (professional) researchers should at least try to do their best to mitigate against this in their own conduct, while recognizing that there is of course still scope for continued reflection around what this may mean in practice (2005, p. 90). In this chapter I have offered some deliberations relating to my way of trying to act responsibly in practice, taking into account the importance of trying to nurture more connectivity as part of the setting up of the research space for co-exploration of issue of concern.

References

Alleyne, B. (2002). *Radicals against race: Black activism and cultural politics*. Oxford: Berg.

Ally, S. (2010). *In service of racial dystopia: Anxiety and fantasy in the figure of the "maid"*. Harold Wolpe Memorial Trust 92nd Lecture, 7 September, Cape Town.

Ansell, A. E. (2007). Two nations of discourse: Mapping racial ideologies in post-apartheid South Africa. In R. D. Coates & R. M. Dennis (Eds.), *The new Black: Alternative paradigms and strategies for the 21st century* (pp. 307–336). Amsterdam: Elsevier.

Ashe, S. D., & McGeever, B. F. (2011). Marxism, racism and the construction of "race" as a social and political relation: An interview with Professor Robert Miles. *Ethnic and Racial Studies, 34*(12), 2009–2026.

Berger, P. L., & Luckmann, T. (1966). *The social construction of reality: A treatise in the sociology of knowledge*. New York: First Anchor.

Bonacich, E., Alimahomed, S., & Wilson, J. B. (2008). The racialization of global labour. *American Behavioural Scientist, 52*(3), 342–355.

Bowers, T. D. (2011). Toward a framework for multiparadigm multimethodologies. *Systems Research and Behavioral Science, 28*(5), 537–552.

Chilisa, B. (2009). Indigenous African-centered ethics: Contesting and complementing dominant models. In D. M. Mertens & P. E. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 407–426). London: Sage.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Cho, J., & Trent, A. (2006). Validity in qualitative research revisited. *Qualitative Research, 6*(3), 319–340.

Cannella, G. S., & Lincoln, Y. S. (2007). Predatory vs. dialogic ethics: Constructing an illusion or ethical practice as the core of research methods. *Qualitative Inquiry*, *13*(3), 315–335.

Clandinin, D. J., & Caine, V. (2008). Narrative inquiry. In L. M. Given (Ed.), *The Sage encyclopedia of qualitative research methods* (pp. 541–544). London: Sage.

Collins, P. H. (1990). *Black feminist thought: Knowledge, consciousness and the politics of empowerment* (2nd ed.). London: Harper Collins.

Collins, P. H. (1999). Producing the mothers of the nation: Race, class and contemporary U.S. population policies. In N. Yuval-Davis (Ed.), *Women, citizenship and difference* (pp. 118–129). London: Zed Books.

Collins, P. H. (2000). *Black feminist thought: Knowledge, consciousness and the politics of empowerment* (2nd ed.). London: Harper Collins.

Connelly, F. M., & Clandinin, D. J. (1990). Stories of experience and narrative inquiry. *Educational Researcher, 19*(5), 2–14.

Cram, F. (2009). Maintaining indigenous voices. In D. M. Mertens & P. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 308–322). Thousand Oaks, CA: Sage.

Crenshaw, K. (1991). Mapping the margins: Intersectionality, identity politics, and violence against women of color. *Stanford Law Review, 43*(6), 1241–1299.

Douglas, C. (1998). *From surviving to thriving: Black women managers in Britain.* Doctoral thesis, University of Bath, Bath, UK.

Douglas, C. (2002). Using co-operative inquiry with Black women managers: Exploring possibilities for moving from surviving to thriving. *Systemic Practice and Action Research, 15*(3), 249–262.

Essed, Ph. (2001). *Toward a methodology to identify continuing forms of everyday discrimination*. Retrieved June 5, 2008, at: www.un.org/womenwatch/daw/csw/essed45.htm.

Essed, Ph. (2005). Gendered preferences in racialized spaces: Cloning the physician. *Racialization: Studies in theory and practice*, 227–247.

Essed, Ph. (2010). Social justice and dignity. In K. Hannum, B. B. McFeeters, & L. Booysen (Eds.), *Leading across differences* (pp. 139–145). San Francisco, CA: Pfeiffer.

Franklin, K. J. (1996). *KL Pike on etic vs. emic: A review and interview*. Glendale, CA: Summer Institute of Linguistics.

Frels, R. K., & Onwuegbuzie, A. J. (2012). Interviewing the interpretive researcher: An impressionist tale. *The Qualitative Report, 17*(30), 1–27.
Gergen, K. J. (1978). Toward generative theory. *Journal of Personality and Social Psychology, 36*(11), 1344.
Giri, A. K. (2011). Cultivating transformative reconciliation and striving for peace: Compassion, confrontation and a new art of integration. *Current Sociology, 59*(5), 604–609.
Gregory, W. J., & Romm, N. R. A. (1996). Toward multi-agency dialogue: Facilitation as fair education. In W. W. Gasparski, M. K. Mlicki, & B. H. Banathy (Eds.), *Social agency: Dilemmas and education praxiology* (pp. 323–351). New Brunswick, NJ: Transaction.
Gregory, W. J., & Romm, N. R. A. (2001). Critical facilitation: Learning through intervention in group processes. *Management Learning, 32*(4), 453–467.
Gregory, W. J., & Romm, N. R. A. (2004). Facilitation as fair intervention. In A. Ochoa-Arias & G. Midgley (Eds.), *Community operational research* (pp. 157–174). New York: Kluwer Academic/Plenum.
Gubrium, E., & Koro-Ljungberg, M. (2005). Contending with border making in the social constructionist interview. *Qualitative Inquiry, 11*(5), 689–715.
Harding, J. (2006). Questioning the subject in biographical interviewing. *Sociological Research Online, 11*(3).
Hodson, R. (2001). *Dignity at work*. Cambridge, UK: Cambridge University Press.
Holstein, J. A., & Gubrium, J. F. (1995). *The active interview*. London: Sage.
Holstein, J. A., & Gubrium, J. F. (2003). *Postmodern interviewing*. Thousand Oaks, CA: Sage.
Jansen, J. (2011). *We need to talk*. Northcliffe and Northlands, South Africa: Bookstorm Pan Macmillan.
Kiguwa, P. (2006). Social constructionist accounts of intergroup relations and identity. In K. Ratele (Ed.), *Inter-group relations: South African perspectives* (pp. 111–136). Cape Town: Juta.
Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.
Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.
Kvale, S. (2002). The social construction of validity. In N. K. Denzin & Y. S. Lincoln (Eds.), *The qualitative inquiry reader* (pp. 297–322). Thousand Oaks, CA: Sage.
Kvale, S. (2007). *Doing interviews*. London: Sage.
Ladson-Billings, G. (2003). Racialized discourses and ethnic epistemologies. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 398–432). London: Sage.
Lather, P. (1986). Issues of validity in openly ideological research: Between a rock and a soft place. *Interchange, 17*(4), 63–84.
Lewis, A. E. (2003). Everyday race-making: Navigating racial boundaries in schools. *American Behavioral Scientist, 47*(3), 283–305.
Liamputtong, P. (2011). *Focus group methodology: Principles and practice*. London: Sage.
Lincoln, Y. S., & Guba, E. G. (2013). *The constructivist credo*. Walnut Creek, CA: Left Coast Press.
Mackintosh, D. (2010). Speaking out against injustice? Re-examining the SA Jewish Board of Deputies' response to apartheid, 1948–1976. *Writing Rights*, 15 September. Retrieved January 2, 2009, at: http://www.writingrights.org/2010/09/15/the-south-african-jewish-board-of-deputies-and-apartheid/.
Magcai, D. M. (2008). *Black farm workers' beliefs on HIV and AIDS*. Master's dissertation, North-West University, Potchefstroom, South Africa.
McIntyre-Mills, J. J. (2014). *Transformation from Wall Street to wellbeing*. New York: Springer.
Mertens, D. M. (2009). *Transformative research and evaluation*. New York: Guilford Press.
Mertens, D. M. (2011). Mixed methods as tools for social change. *Journal of Mixed Methods Research, 5*(3):195–197.

Mertens, D. M. (2012). Transformative mixed methods: Addressing inequities. *American Behavioral Scientist, 56*(6), 802–813.
Mertens, D. M., & McLaughlin, J. A. (2004). *Research and evaluation methods in special education*. Thousand Oaks, CA: Sage.
Miles, M. (1996). For the sake of the children: Coping strategies of women in Swaziland's domestic service sector. *GeoJournal, 39*(1), 81–88.
Mitropolitski, S. (2013). Interactive interview: A research note. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *16*(1), Art. 8.
Morris, M. W., Leung, K., Ames, D., & Lickel, B. (1999). Views from inside and outside: Integrating emic and etic insights about culture and justice judgment. *Academy of Management Review, 24*(4), 781–796.
Murove, M. F. (2005). The theory of self-interest in modern economic discourse: a critical study in the light of African humanism and process philosophical anthropology. Doctoral thesis, University of South Africa, Pretoria.
Nash, A. (1999, April). Mandela's democracy. *Monthly Review, 50*(11). Retrieved January 2, 2009, at: http://monthlyreview.org/1999/04/01/mandelas-democracy/.
Ndimande, B. S. (2012). Decolonizing research in post-apartheid South Africa. *Qualitative Inquiry, 18*(3), 215–226.
Ngulube, P. (2015). Qualitative data analysis and interpretation: Systematic search for meaning. In E. Mathipa & M. Gumbo (Eds.), *Addressing research challenges: Making headway for developing researchers* (pp. 131–156). Johannesburg: Mosala-Masedi.
Nyamnjoh, F. B. (2006). *Insiders and outsiders: Citizenship and xenophobia in contemporary Southern Africa*. London: Zed Books.
Nyamnjoh, F. B. (2007). Toward a predicament-oriented approach to social research ethics. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa: Ethical issues* (pp. 1–10). Addis Ababa: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).
Olive, J. L. (2014). Reflecting on the tensions between emic and etic perspectives in life history research: Lessons learned. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research, 15*(2), Art. 6.
Omi, M., & Winant, H. (1986). *Racial formation in the United States*. New York: Routledge.
Omi, M., & Winant, H. (2002). Reflections on "racial formation". In Ph Essed & D. T. Goldberg (Eds.), *Race critical theories* (pp. 455–459). Oxford: Blackwell.
Onwuegbuzie, A. J. (2012). Introduction: Putting the *mixed* back into quantitative and qualitative research in educational research and beyond: Moving toward the radical middle. *International Journal of Multiple Research Approaches, 6,* 192–219.
Onwuegbuzie, A. J., Dickinson, W. B., Leech, N. L., & Zoran, A. G. (2009). A qualitative framework for collecting and analyzing data in focus group research. *International Journal of Qualitative Methods, 8*(3), 1–21.
Onwuegbuzie, A. J., & Frels, R. K. (2015). A framework for conducting critical dialectical pluralist focus group discussions using mixed research techniques. *Journal of Educational Issues, 1*(2), 159–177.
Pampallis. J. (2008). Introductory essay. In *Submissions on apartheid in education* (pp. 4–11). Johannesburg: STE Publishers (on behalf of the Department of Education).
Polanyi, K. (1944). *The great transformation: The political and economic origins of our time*. New York: Farrar & Rinehart.
Romm, N. R. A. (2001). *Accountability in social research: Issues and debates*. New York, NY: Springer.
Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York: Springer.
Romm, N. R. A. (2013). Revisiting social dominance theory: Invoking a more retroductively-oriented approach to systemic theorizing. *Systemic Practice and Action Research, 26*(2), 111–129.

Romm, N. R. A., & Adman, P. (2004). Exploring the complexity of human dynamics within 360-degree feedback processes: The development of (active) qualitative inquiry. *Journal of Business and Society, 17,* 170–189.

Romm, N. R. A., & Hsu, C.-Y. (2002). Reconsidering the exploration of power distance: An active case study approach. *Omega, 30*(6), 403–414.

Romm, N. R. A., Nel, N. M., & Tlale, L. D. N. (2013). Active facilitation of focus groups: Exploring the implementation of inclusive education with research participants. *South African Journal of Education*, *33*(4), 1–14, Art. 811.

Russell, G. M., & Kelly, N. H. (2002). Research as interacting dialogic processes: Implications for reflexivity. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research, 3*(3), Art. 18.

Schutte, G. (2002). *The racialized stranger: Reflection on the inside as outsider*. Paper presented at the XV World Congress of Sociology, Brisbane, Australia, 8–13 July. Retrieved December 15, 2014, at: https://www.academia.edu/11961305/The_Racialized_Stranger_Reflections_on_the Insider_as_Outsider.

Sikes, P. (2005). Storying schools: Issues around attempts to create a sense of feel and place in narrative research writing. *Qualitative Research, 5*(1), 79–94.

Sikes, P. & Goodson, I. (2003). Living research: Thoughts on educational research as moral practice. In P. Sikes, J. Nixon, & W. Carr (Eds.), *The moral foundations of educational research* (pp. 32–51). Buckingham: Open University Press.

Sitas, A. (2011). Beyond the Mandela decade: The ethic of reconciliation? *Current Sociology, 59* (5), 571–589.

Sitas, A., Latif, D., & Loizou, N. (2007). Prospects of reconciliation, co-existence and forgiveness in Cyprus in the post-referendum period. *PRIO Cyprus Centre Report*, *4*.

Smith, L. T. (1999). *Decolonizing methodologies: Research and Indigenous peoples*. New York: Zed Books.

Springwood, C. F., & King, C. R. (2001). Unsettling engagements: On the ends of rapport in critical ethnography. *Qualitative Inquiry, 7*(4), 403–417.

Spyrou, S. (2006) *Research report on Greek Cypriot children's responses to Sri Lankan and Filipino women*. Research conducted with support from the Bi-communal Development Program funded by USAID and UNDP and executed by UNOPS.

Spyrou, S. (2009). Between intimacy and intolerance: Greek Cypriot children's encounters with Asian domestic workers. *Childhood, 16*(2), 155–173.

Sue, D. W. (2005). Racism and the conspiracy of silence. *The Counseling Psychologist, 33*(1), 100–114.

Tatum, B. D. (2003). *Why are all the Black kids sitting together in the cafeteria?*. New York: Basic Books.

Trimikliniotis, N., Gordon, S., & Zondo, B. (2008). Globalization and migrant labor in a "Rainbow Nation": A fortress South Africa? *Third World Quarterly, 29*(7), 1323–1339.

Valluvan, S., & Kapoor, N. (2016). Notes on theorizing racism and other things. *Ethnic and Racial Studies, 39*(3), 375–382.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.

Wolpe, H. (1972). Capitalism and cheap labor-power in South Africa: From segregation to apartheid. *Economy and Society, 1*(4), 425–456.

Chapter 3
Active Use of Questionnaires Combined with Focus Group Facilitation: Responsibly Researching Options for Generating Educational Inclusivity

Abstract This chapter discusses two examples of research in which I was involved, both of which made use of questionnaires and follow-up focus groups in the quest to explore/regenerate certain aspects of inclusive education. The first project was an international one comprising of six countries, namely, China, Finland, Lithuania, Slovenia, South Africa and the United Kingdom and was aimed at organizing a comparative analysis of teachers' roles in inclusive education. The team facilitating the focus groups in South Africa (Norma Nel, Dan Tlale and myself) twisted the original research remit to expressly include "intervention", which evolved also in response to expectations expressed by participants in focus group sessions in a number of schools in Atteridgeville. The second project was a national one (aimed at improving prospects for underperforming schools in South Africa). It was designed from the start to include intervention as part of the research remit. This was built into the project through a number of mechanisms aimed at stimulating creative reviewing of the notion of underperformance, while locating, with participants and stakeholders, leverage for improvements.

3.1 Introduction

In this chapter I look in-depth—with a focus on researcher responsibilities—at two examples of research in which I was involved, both of which used questionnaires with follow-up focus group (FG) sessions to explore/regenerate certain aspects of inclusive education. The first project was an international one comprising of six countries, namely, China, Finland, Lithuania, Slovenia, South Africa and the United Kingdom. It was entitled "*A comparative analysis of teachers' roles in inclusive education*". The purpose of this project (as set out in the proposal developed by the cross-country team and presented to various University Institutional Review Boards) was to organize a comparative study of teachers' understanding of their roles in inclusive education. For this purpose, a mixed method design was proposed to be executed in sequential fashion, with questionnaires preceding the FG discussions. The rationale for the mixed method design, as explained in the proposal,

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_3

was that the questionnaires would offer statistically analyzable data, while the FGs could delve more fully into participants' experiences of teaching in inclusive classrooms in the various country contexts. This was also in keeping with Sharma, Loreman, and Forlin's (2012, p. 17) recommendation to include a qualitative component to supplement their scale for measuring Teacher Efficacy for Inclusive Practices (TEIC)—which was one of the scales used in the research.[1] The scale has three sub-dimensions: efficacy in using *inclusive instruction*; efficacy in dealing with *disruptive behaviors*; and efficacy in *collaboration* as explained in Sect. 3.3.

> Sharma, Loreman, and Forlin state that the TEIC is limited in that it cannot provide researchers with an in-depth understanding of teacher efficacy and about the factors that may contribute in the formation of their efficacy as well as those that may sustain high level of efficacy amongst teachers. (Sharma, Loreman, & Forelin, 2012, p. 17)

They therefore suggest that "qualitative data will also be useful in making sense of the teacher efficacy construct in different cross-cultural contexts … and thus contribute to the further validity of the TEIP scores" (Sharma, Loreman, & Forelin, 2012, p. 17). In other words, they recommend that additional qualitative research enables researchers to better appreciate culturally specific meanings associated with teachers' conception of their efficacy to implement inclusive education in their contexts, and thereby adds depth (and validity) to the information derived from their questionnaire. As will be shown in Sect. 3.3.1, the South African team handling the FG sessions (the point at which I entered the project) saw the validity of the enterprise in a somewhat wider fashion, in line with Gonzales et al.'s (2011, p. S166) suggestion to broaden the definition of validity to include an intent to develop a "partnership ethic" with participants and stakeholders. We tried to activate this idea of validity in our specific way of handling the FG sessions and subsequent meetings with certain stakeholders.

Apart from measuring teacher self-efficacy to address inclusive education via the TEIP scale, the questionnaire was also aimed at measuring teachers' attitudes to inclusive education. To this end a scale called SACIE (Sentiments, Attitudes and Concerns about Inclusive Education) was also built into the questionnaire.[2] Altogether, the intention of the quantitative component of the research (the first phase of the project) was to investigate

[1]The notion of efficacy derives from Bandura's (1977) establishment of the concept of "self-efficacy", which is defined as a judgement on people's part of their "capability to execute a given type of performance" (Malinen et al., 2013, p. 35). The TEIC consists of 18 statements which are assessed on a Likert scale with 6 response anchors—strongly agree, disagree, disagree somewhat, agree somewhat, agree, and strongly agree Malinen (2013, p. 55) indicates that this instrument is a relatively new one and that the use of it in this project was the first time that it was used. Although the instrument (which consisted of 18 items) offered 6 response anchors using a Likert scale, Malinen suggests that in practice it worked more like a 4-point scale as respondents seldom used the lowest two points.

[2]This scale consists of 15 statements assessed by using a Likert scale with four response anchors.

- whether there are differences between the different countries in their profiles of self-efficacy for inclusive practices (in terms of the TEIP scale);
- whether there are differences in their profiles of attitudes toward inclusive practices (in terms of the SACIE scale); and
- whether self-efficacy for inclusive practices in its various dimensions correlate with attitudes toward inclusive education and, if so, which dimension of self-efficacy (treated as an independent variable) is the best predictor of attitudes toward inclusive education (Savolainen et al., 2012b, p. 55).

In administering the questionnaires across the six countries, only minor modifications for country-context were introduced into the questions, so that analytical comparisons could be made in relation to teachers' sense of efficacy as measured by the TEIP scale and their attitudes as measured by the SACIE scale.

However, the second phase of the project, the FG inquiries, allowed some scope for using the cross-country pre-set questions/guidelines for the FG sessions across the various countries as a springboard to develop a more fluid conversation around the issues and the challenges experienced by the teachers. The South African team involved in this phase converted the FG "interviews" into discussions in which all participants, including the facilitator, *could add input to enrich the dialogue*. This way of handling the FGs was country-specific and was based on our philosophy that we brought to the project, namely, that we could use the research process in the hope that participants as well as we ourselves could become enriched from having taken part in the research. We also took initiative in soliciting feedback from the participants in regard to how they experienced the facilitation style that was adopted during the FG sessions. (We facilitated FG sessions with teachers in three different schools in Atteridgeville, schools which were historically disadvantaged.)

Clearly, not all the people involved in the research in the various countries (the university-based researchers and the participants) had the same or even similar visions of what the researchers' roles might be. Reed discusses how one can treat differences between positions that might arise in the research process as being multi- and poly-vocal, as well as possibly conflicting and contradicting (2000, para 3.4). We took our cue from this and did not try to "merge" our views with all the researchers involved in the various countries, albeit that we did mention to the project leader—Petra Engelbrecht—that we hoped to introduce a more "transformative" agenda into the way we handled the FG sessions. In our approach we adopted a position somewhere between a "pragmatic" and "transformative" outlook (Romm, Nel, & Tlale, 2013, p. 2). This position allowed us to treat the questionnaire results, combined with the FG discussions, as an opportunity to invite additional discussion with further stakeholders regarding the implementation of inclusive education (as requested by some of the FG participants).

What is also important to mention is the way in which we (the South African team) organized our own cross-cultural interactions in discussing how to proceed with facilitating the research with FG participants (all 20 of whom were Black) in a way that could be said to be culturally appropriate. This is not to say that I wish to treat "race" as in apartheid-given categorization as if "races" are real and given in

some natural sense—see my discussion in Chap. 2, Sect. 2.2. Nevertheless, Erasmus (2010, p. 394) makes the point that we need to recognize the importance of naming the continued social effects of race categorization, while not assuming that people's racialized or cultural identities are fixed. In the context of the research our intention was to recognize that people with different cultural heritages can bring different modes of knowing and being into their social interactions. To effect what Kovach calls "cultural responsiveness", she suggests that "nonIndigenous scholars who wish to engage with Indigenous knowledges [and styles of knowing] need to connect with Indigenous scholars, people and communities". She adds that "those who try to sidestep the relational work will forever be frustrated by Indigenous knowledges, research and methodologies" (Kovach, 2009, p. 172).

In this case Norma Nel and myself are historically defined as White in terms of apartheid social groupings and socialization (see my personal narrative in Chap. 2, Sect. 2.2); and Lloyd Tlale (a doctoral student of Norma Nel at the time of the research) is Black. We had many conversations prior to "going into the field" where we did what Kovach might call relational work in deciding together our joint philosophy that we would take with us into the field. We agreed that we wished the process of facilitating the FGs ideally to be enriching to the participants. We also agreed that we would try to handle the sessions (and present ourselves to the participants) in such a way in which they would feel that they were very much part of a research process, where we were all co-exploring the issues together. We furthermore agreed that we would ask for their feedback on how they had experienced the session, so that we could learn about our way of handling the facilitation (as seen from their perspectives). This was part of showing them that we respected their understandings of what the research meant to them. Shumbamhini suggests that feedback practices are a way of deconstructing researcher power by taking into account participants' understandings of the research (2005, p. 14). She illustrates that in the case of her co-exploration with Shona widows of their experiences, the feedback that she sought "assisted me to face squarely the moral and ethical implications of my practices" (2005, p. 15).

In Sect. 3.3.1, I point to feedback received from the FG participants, which signaled to us that they trusted us as to carry their voices (as generated in the FG discussion) further. We did not want to betray their trust; and so indeed we took it upon ourselves to arrange for a meeting with the district officer in the area and the teacher-participants—a meeting which was held at the University of South Africa (Unisa). The district officer (Shila Mphahlele) suggested to us that she would also involve Head Office; and so this was arranged. In this respect, we could be said to be acting to some degree as what Bishop, citing (Beverley, 2000), calls an "interlocutor to bring their [participants'] situation to the attention of an audience to which he or she would not normally have access" (Bishop, 2012, p. 126). We chose to handle this by setting up a meeting that would not normally have been arranged (without our taking some responsibility for this).

All in all, by responding to the FG participants' expressed hope/expectation that we would "carry the baton" further (as one of them expressed it), we believe that we

in some way developed a sense of a community of people concerned with implementing inclusive education, where the question of all of our initial background cultures was less important than that we were all concerned to address the issues which we discussed as being at stake (in relation to the implementation of inclusive education). I would argue that it was important that as researchers we displayed ourselves as trying to explore, with participants, ways of serving the values of inclusive education (as, for instance, incorporated in the Department of Education's White Paper 6, 2001), while listening carefully to the challenges as expressed by the teachers, and also making inputs regarding possible ways of handling these. It was important that we showed that we cared, which was appreciated by the participants as evident in their feedback responses. (See Table 3.1 in Sect. 3.3.1.)

Mapotse (2012, p. 78) makes the point that in a research approach which involves co-exploring issues with participants, coupled with a display of caring, it can be said that researchers shift their role (in their own and in research participants' perspectives) from being an "outsider professional" (the so-called *etic* approach) toward an "insider's participation and understanding" (an *emic* approach). This would tally with Smith's suggestion that Indigenous (and Indigenous-oriented) researchers are well equipped to straddle "outsider/insider" boundaries (1999, p. 137). We were conscious that from the start we did not want to act as outsider research professionals, but were trying to create a relationship with participants such that we would to some extent become "insiders" concerned with similar issues. Mapotse classes the research in which he was involved with participants as action research because it involved a number of cycles of trying out actions with (underqualified) teachers of technology via spiraling cyclical processes of reflecting on practice, taking an action, reflecting, and taking further action to develop a sustainable way of teaching of technology (2012, pp. 78–80) (see also Mapotse, 2015). Our research cannot be said to have been action research in the (traditional) sense of developing cycles of planning, acting, observing, and reflecting (cf. Bless, Higson-Smith, & Sithole, 2013, pp. 93–95; Dick et al., 2015, p. 35; Niculescu, 2014, p. 30; Sung-Chan et al., 2008, p. 200). But it can be called *active* in that the intention was still to "make a difference" to the quality of participants' (teaching) lives and beneficiaries such as learners, as a result of the research. Young, McGrath, and Adams summarize that this model of research, which, they note, Romm has "coined 'active research' … differs from archetypes of action research that involve cycles of inquiry that may in fact not match with the needs and availabilities of co-participants" (2015, p. 63). They remark too that active research does not involve recipes, but looking for opportunities in situ to make a difference to the quality of life in the setting.

I now turn to introducing the second project—called the 500 schools project—in which there were 46 researchers (all University of South Africa staff) and many research assistants. This project was a national one, with a representative sample of 500 primary schools in South Africa selected from five provinces most affected by learner "underperformance" being invited to participate in the project. We decided to add as a subtitle "making school better" so that participants would be aware that school improvement was the purpose of doing the research. I was one of the

principal researchers and contributed to designing the research.[3] The research project was classed by our university as a Community Engagement project and it was funded from the university's Special Project fund. It was run under the project leadership of Soane Mohapi who worked closely with the person in charge of the community engagement portfolio in the College of Education, Veronica McKay, who recognized the importance of the project and the need to set it up. McKay had previously been seconded to the National Department of Basic Education (DBE), to set up an adult mass literacy campaign (Kha Ri Gude) and further to this, to organize the development of school workbooks for children (with a team of people working in 11 languages).

The 500 schools project was set up to examine in what were called underperforming schools the reasons for learners not achieving the required learning outcomes, with the attendant hampering of their further life chances, thus perpetuating (racialized) cycles of poverty. As McKay underscored when writing into this chapter text (March 2017):

> The legacy of apartheid appears to have endured. The former racially separate education departments therefore remain important categories for analysis. Not only have the large performance gaps between former black schools and former white schools prevailed, but also the relationship between former education department classification and socio-economic status. (McKay, pers. comm. via email, 10 March 2017)

The 500 schools project was aimed at exploring (and trying to shift) the dynamics leading to students' poor results in the areas of Languages (Home Language and First Additional Language), Science, and Mathematics, with the intention to develop ways of offering support to schools deemed as requiring such support. Although not named as examining "inclusive education" alone, the intent was, inter alia, to generate more educational inclusivity in terms of the government's White Paper 6 (2001) which addresses inclusive education. (See Sect. 3.2 below on this.) 500 schools (100 each from five different provinces) were sampled for participation in the questionnaire phase of the research using a sampling frame based on the Department of Basic Education's 2011 results of the Annual National Assessments which drew attention to schools that had performed poorly. The project was also designed so that in (sub)sampled schools certain groups of participants would participate in FG sessions, and these schools would in addition be visited by staff from the university in subsequent "intervention visits".[4] Our intervention visits

[3]Veronica McKay was director of the Community Engagement portfolio in the College of Education at Unisa, and liaised with the Department of Basic Education (DBE) in setting up the project and also worked with researchers in the Department in constructing most of the questionnaire items, while considering their value in relation to the overall design. Others who assisted in developing some of the items from their discipline-based backgrounds were: Prem Heerelal, Luckson Kaino, and Mapheleba Lekhetho.

[4]The process worked as follows: for FG interviews, random sampling to select 10% of participating schools was done. From these, the same 5–6 teachers who filled in the questionnaires were requested to be part of the FG; 5–6 learners taught by each of the participating teachers were selected to be part of the FG (this was supposed to be random but some of us suspect that

were one way of "giving back"—by giving specific attention to those who spent time participating in the FG part of the research. The interventions were geared ideally to make a contribution to, as Kuntz puts it, "generating new meanings" and "new ways of engaging within the world" (2015, p. 28).

The project involved the provinces of: KwaZulu-Natal; Limpopo; Free State; Eastern Cape; and Mpumalanga. Permission had been sought from the respective provincial education departments to conduct the research in terms of a permission letter explaining what the research was about and elucidating the research design. These letters were followed by letters to the principals of the sampled schools, after which, if granted permission, the research assistants went out on visits to further explain to the principals and teachers what the research entailed, and to proceed with the first phase of the research (the questionnaires), where they would assist those requiring help in filling in the questionnaire.[5] Prior to their going into the field to assist with the questionnaire administration, the fieldworkers were trained by the project leader and community engagement portfolio director.

Apart from the questionnaire administration (to principals and teachers of sampled schools), as noted above a second stage of the project involved setting up focus group sessions with various groups of participants. In the design of the project, FG sessions were to be held with four sets of participants in three schools in each province, viz: the School Management Team (SMT); the parents, including members of the SGB (School Governing Body); teachers (of Grade 3 and Grade 6 learners); and learners. As McKay puts it, "we factored into the design that we would use the responses in the questionnaire as prompts for the follow up FGs, so that deeper non-superficial data was envisaged" (pers. comm. via email, 10 March 2017).

Prior to the 46 researchers from the university going to the various provinces to conduct FG sessions, all of these researchers were required to attend a full-day workshop, which was facilitated by Brigitte Smit and me, so that we could all discuss how we would handle the FG sessions in the various (sub)sampled schools. A guide on facilitation in general, and specifically the handling of this in this project, with the introductions that should be used as guides at the start of the sessions, was also created (Smit & Romm, 2012) and became a starting point for discussions in the workshop.

---

principals and teachers asked the relatively good performers; nonetheless this could still be made to work in practice (as, for example, when I asked them whether they used a buddy system to help their classmates). 5–6 parents of the participating learners were also requested to be part of the FG sessions. The SMT FGs were composed of principals and heads of departments of the subjects concerned.

[5]The breakdown per Province of the schools which granted permission for us to conduct the research was as follows: EC: 87%; FS: 96%; KZN: 61%; Limpopo, 49%; Mpumalanga: 26%. In Eastern Cape province 74 principals and 315 teachers from 87 schools were surveyed via the questionnaire. In Free State province 80 principals and 370 teachers from 96 schools were surveyed. In Kwa-Zulu Natal province 55 principals and 218 teachers from 61 schools were surveyed. In Limpopo province 32 principals and 181 teachers from 49 schools were surveyed. In Mpumalanga province 16 principals and 86 teachers from 26 schools were surveyed. (This was the poorest participation of the 5 surveyed provinces.) Across the board, in the schools that did participate the response rate from principals and teachers filling in the questionnaire was high.

In the workshop while discussing how we would run the FG sessions, one of the issues that arose (brought up by some of the Indigenous researchers) was that many of the participants would not feel comfortable using English as the language in which to hold the FG sessions. This would be the case especially, for example, for learners—who could not be expected to express themselves well in English—and for many parents. But it was also considered that it would not be appropriate culturally to expect that the group discussions should be in English. Hence it was suggested that in all the groups facilitators should "sound out" how to deal with the language issue (and it was arranged that at least one native speaker would be present in the facilitation of each FG that was to be conducted) by recognizing the dominant languages of the specific provinces. This is indeed in keeping with Ndimande's stress (2012, p. 215) on showing respect for Indigenous languages when conducting research. (See also my account of this in Romm, 2015, para 55.)

Even though this meant that the data collected would not be able to be put through a software program such as Atlas.ti for analyzing them qualitatively (as the costs of translation into English could not be met by the project), we decided that an alternative would be for a transcription of the recordings to be made in whatever language they were in, and the team of researchers for each school would pinpoint issues that occurred in the groups on the basis of these transcriptions, highlighting also suggestions that participants offered for addressing challenges. As McKay (pers. comm. via email, 10 March 2017) affirms, "this meant that the researchers would use their judgements of what constituted the main problems and would then 'check' with the participants". The team who handled the FG sessions in each school thus would create a report based on their intersubjective checking with each other in relation to the data from their transcripts;[6] and this would form the basis for the subsequent visits to the schools—one visit of which would be for a kind of "member checking" of this report (so-called in the literature—cf. Lincoln & Guba, 1985, 2003, 2013), and two "intervention visits".

We also discussed in the workshop that we would ask of participants already during our first visit (to the selected schools) whether digital recordings would be in order (their consent could be either in written or oral form[7]). We clarified in the workshop that in our second visit to the schools we would check our interpretation of the gist of the earlier discussions (from the first visit) and would also focus on what we had identified from the first visit as being important to them (from the full set of FGs in the school). In these visits, we would *focus further on suggestions for possible improvement* and areas where they felt they may need support (and what

[6]In our field guide we had in any case (before the workshop) stated that it was important for all facilitators/moderators and assistant moderators/observers to bear in mind that they were required to make a transcript of the tape at the end of the session. We suggested that "it is best to do this as soon as possible after the session while it is still fresh in your memories, so that you can make a good transcript".

[7]Some of the participants, especially parents in rural areas, were not literate in writing; so oral consent to agree to be recorded was deemed to be in order here.

kind). Our "member checking" (in the second FG visit) was thus intended to be active in this sense.

The project was designed so that after the information from the questionnaire had been analyzed (for quantitative and qualitative content), and the draft FG (qualitative) reports had been "member checked", an "intervention handbook" would be created which could be of general assistance (across all provinces). The guide could also serve to support intervention visits that were to be undertaken. Also, draft results would be discussed in a meeting with the researchers from the Department of Basic Education (DBE) at national level.

Meanwhile in our initial construction of the questionnaire, we did not construct it in terms of our trying to hypothesize "causes" of underperformance as isolated from each other as in the positivist tradition; we worked (implicitly) with a more narrative account of causes, also seeking—via both the questionnaire and FG discussions—respondents/participants' understanding of the factors (and the relationship between factors) leading to school underperformance (in terms of student results), which, if addressed, would contribute to "making schools better". This is in accord with Ponti's understanding of how systemic accounts can be built up (2012, para 2). In Sect. 3.5, I elucidate how we sought such a systemic consideration of factors through both the questionnaire and FG sessions.

Because in this chapter I have focused on issues of educational inclusivity, I concentrate below on how these issues were dealt with in the questionnaire, FG discussions and intervention visits. As far as the intervention visits are concerned, I provide an illustrative example of how Lloyd Tlale[8] chose to handle the intervention remit in a particular school. There were many teams who visited many schools, but I focus on this one because of my detailed knowledge of how he chose to exercise his responsibility. What transpired in our co-reflections around how he handled his involvement with participants, was that he was operating in a spirit of caring, but had not recognized how this legitimately could be considered as linked to a "research" role, having been trained initially to see researchers as "outsiders" investigating "research subjects". I highlight the manner in which Lloyd (who also calls himself Dan) entered into relationship with the participants with the intention to inspire action. (This is further explored in more depth in Romm & Tlale, 2016.) I also highlight (as McKay expressed it when writing into this text) how via the 500 schools project considered as a whole:

> Teachers were able to identify where they required assistance, groups were trained, the manuals [intervention handbooks] were made available for wider dissemination and the research was active for Unisa staff members who participated. [In many instances], it induced them to reflect on the issues and to make their own teaching better as teacher trainers. The project was written up as a book for activating and alerting readers to some of the challenges in South African schools (see Magano, Mohapi, & Robinson, 2017) and ways in which they can be addressed. It opens up possibilities to explore how schools may be made better. (McKay, 10 March 2017)

[8]Lloyd Tlale, also calls himself Dan (his second name, and the one by which I call him); we came to know each other when we worked together in the other (international) project too.

In the course of the chapter I elucidate how both the international and national research projects can be said to have been conducted in line with a forward-looking agenda (although we had less "control" in the first project to do this, especially since the questionnaire items were taken from the cross-country administration of the TEIC and SACIE scales). Before I embark on the discussion, I provide in the next section a brief outline of some meanings associated with inclusive education. This is because my exemplars that I have chosen in both cases (Sects. 3.3 and 3.4 below) involve intervention toward increasing inclusivity, albeit that the 500 schools projects can be said to have been broader in scope than this, with a more general focus on "making schools better".

3.2 Inclusive Education: A Brief Account (Contextual Background to in-Depth Discussion of Research Exemplars)

Malinen et al. (2013, p. 34) indicate that the value of including students with diverse educational needs into the mainstream is a globally recognized value (as expressed in UNESCO, 2007). It is suggested in terms of this value that schools (including mainstream ones) have to become more geared to cater for students with different levels of ability, background, needs, etc. Sharma, Loreman, & Forlin explain that "under an inclusive philosophy, schools exist to meet the needs of all students; therefore, if a student is experiencing difficulties, the problem is with the schooling practices not with the student" (2012, p. 12). One of the principles guiding inclusion as a philosophy is that the onus is on schools (and wider communities) to cater for learners with different needs. Rather than regarding learners as if it is they who harbor deficits, the language of deficits is shifted to a language of commitment to meet learners' needs (Biraimah, 2016; Harry & Klingner, 2006; Kozleski et al., 2009; Paugh & Dudley-Marling, 2011). Miles and Singal make the point that the "mechanics of how to achieve inclusive education for all children" has thus far not been adequately addressed at theoretical or practical levels (2010, p. 8). Kozleski et al. similarly emphasize that:

> While there may be political consensus on the need to embrace a global inclusive education agenda, how it is accomplished and the degree to which a deep and sustained commitment to inclusiveness exists in policy and practice remains unexplored. Indeed, although there is growing consensus on a broad definition of inclusive education, this concept has complex local meanings that are shaped by historical, cultural, political, and economic forces. (2009, p. 1)

In the South African context, Tchatchoueng notes that the history of apartheid has had consequences in terms of the current handling of inclusivity issues. He states that in South Africa we need to bear in mind that:

> Unemployment, poverty, malnutrition, inadequate medical facilities and high rates of pre-natal and early childhood infections make the number of learners with special needs higher in this country than in many other countries with comparable levels of economic development. (2014, p. 217)

The policy on inclusive education in South Africa set out in White Paper 6 (2001), is directed at ensuring, as Tchatchoueng puts it, that,

> every learner, regardless of disability, socio-economic status, linguistic and cultural background, and regardless of competence in the language of learning and teaching, is able to acquire as much knowledge as possible, and to experience education success, friendship, and a genuine sense of belonging to the learning community. (2014, p. 219)

In terms of catering for disability, White Paper 6 suggests that it is important that

> a wider spread of educational support services will be created in line with what learners with disabilities require. This means that learners who require low-intensive support will receive this in ordinary schools and those requiring moderate support will receive this in full-service schools. Learners who require high-intensive educational support will continue to receive such support in special schools. (White Paper 6, 2001, p. 15)

As explored in Nel, Romm, and Tlale (2015), the current approach to staff development for inclusive education in mainstream schooling is multi-level classroom instruction, as the teacher is required to prepare differentiated lessons which will cater for individual learner needs (White Paper 6, p. 18). The Institutional Level Support Teams (ILSTs) that are supposed to be set up in the schools are also required to meet to consider if a child requires special support, and can refer cases for assessment of needs to the District Based Support Teams (DSBTs). However, as noted by Nel, Romm, and Tlale

> factors such as proximity of the school, available places at nearby full-service and special schools, as well as parents' wishes are also taken into account when deciding where children will be schooled. (2015, p. 37)

Swart et al. (2004, p. 80) point out that White Paper 6 "requires extensive changes in education, as the focus shifts from learners' adjustment to the demands of the system, to the system's capability to accommodate all learners' needs as inclusively as possible".

The Department of Basic Education's (2009) guidelines for what are called "full-service schools" indicates that ideally all "ordinary" schools should eventually become "full-service" in the sense of incorporating principles and characteristics of inclusivity. The guidelines document states that:

> Through these guidelines, the Department aims at setting criteria for schools, districts and provinces against which to measure their progress toward inclusion. The introduction of the guidelines (2009) is one of the first steps toward eventually making all ordinary schools full-service/inclusive schools.[9] (2009, preamble)

[9]The document makes provision (as in White Paper 6) for such schools to assess, in conjunction with parents/guardians and the affected learner, the value of placing a learner in a special needs school as follows: "In determining the placement of a learner with special education needs, the

The document suggests that the purpose of the introduction of the term "full-service school" in White Paper 6, was to "underline the important role mainstream schools play in developing an inclusive system and secondly, to clarify their role as levers of change" (2009, preamble). It is stated that to date:

> Even in the short term after 8 years since the publication of Education White Paper 6, it must be acknowledged that there are already many schools in the country that have adopted principles and practices which make them eligible to be considered inclusive schools. (2009, p. 1)

Key components (principles and characteristics) of full-service schools that are highlighted in the guideline document are:

- Philosophy and principles of inclusivity
- Promoting a culture that welcomes, appreciates and accommodates diversity
- Whole school development and management
- Collaboration and teamwork
- Professional development
- Provision of quality support
- Assessment of learner support needs
- Inclusive curriculum
- Flexible teaching and inclusive classroom practices
- Support on behavior
- Physical and material resources and transport
- Family and community networks
- Participation in the district support network for purposes of care and support (2009, p. 2).

In ordinary (mainstream) schools it is required that Institutional Level Support Teams (ILSTs), which are also called School Based Support Teams (SBSTs) are put in place in order to progress the incorporation of these elements of inclusivity. As Motitswe and Phala note:

> As mandated in the Education White Paper 6 … all schools are expected to have functional SBSTs to ensure an enabling environment for teaching and learning. It is the responsibility of the school principle to ensure that the school becomes an inclusive center of learning, care and support. (2017, p. 151)

The SBSTs in turn are expected to liaise with the DBSTs. This system of support at Institutional and District level is expressed in White Paper 6 as follows:

> At the institutional level, in general, further and higher education, we will require institutions to establish institutional-level support teams. The primary function of these teams will be to put in place properly co-ordinated learner and educator support services. These services will support the learning and teaching process by identifying and addressing learner, educator and institutional needs. Where appropriate, these teams should be

---

Head of Department and principal must take into account the rights and wishes of the parents and of such learner, taking into account what will be in the best interest of the learner in any decision-making process" (2009, p. 3).

> strengthened by expertise from the local community, district support teams and higher education institutions. (White Paper 6, 2001, p. 29)

Tchatchoueng points out that the spirit of White Paper 6 in its "inclusive vision" is that there needs to be a shared sense of responsibility:

> For the sake of learners, the inclusive vision and democratic leadership that the inclusive teacher fosters in the classroom is shared with and supported by other teachers, support staff, and members of the community. Each one of these groups of people bring something to the creation of the inclusive education classroom. (2014, p. 220)

Tchatchoueng adds that parents are also encouraged by the DBE to join in institutional level support teams and to "help in designing and implementing an individual support plan for their children participate in "support plans" (2014, p. 228). He summarizes, citing Swart et al. (2004, p. 81), that inclusive education in classrooms "demands the interactive participation of all role players, including teachers, managers, parents, learners, and community members" (2014, p. 229). The idea that collaborative efforts are required to make inclusive education work, was one of the findings that emerged from the quantitative component of the comparative international study, where as we shall see below South African teachers tended to score less than their country counterparts on this (collaborative) dimension of efficacy. This was further explored during the FG sessions in this project, which became an encounter where teachers could discuss issues concerning collaboration with other teachers and others, as well as constraints that they envisaged as needing to be addressed.

3.3 Some Detail on the Comparative International Project

The first phase of the international project exploring the role of teachers in inclusive education involved administering a questionnaire consisting of the 18 items that constitute the TEIP scale and the 15 items that constitute the SACIE scale (together with some demographic information that was also sought from participants). Following Sharma, Loreman, and Forlin's (2012) development of the TEIP scale, "self-efficacy" was regarded as being a multi-dimensional construct: Certain items were considered as measuring "efficacy in instruction" (that is, efficacy in directing their teaching toward learners' needs); certain items were considered as measuring "efficacy in managing behavior" (that is, confidence in being able to find way of handling, or preventing, disruptive behavior); and certain items were considered as measuring "efficacy in collaboration" (within the schools with colleagues and also with parents and partners/stakeholders outside of the school).

Regarding use of the SACIE scale, Savolainen et al. (2012b, p. 57) indicate that they drew on the SACIE scale as developed by Loreman, Earle, Sharma, and Forlin, which identifies three areas linked to the philosophy of inclusion (2007, p. 151). These areas are: Sentiments (or feelings) about people with "special needs" being taught in mainstream schools; Attitudes or beliefs regarding inclusion; and

Concerns (practical ones) about including people with "special needs" in their own classes. Savolainen et al. note (2012b, p. 53) that in the context of South Africa, the terminology of special needs has given way to concepts referring to learners' experiencing barriers to learning, where "disabilities, socio-economic deprivation, under-resourced schools and inappropriate language of instruction" are all recognized as constituting barriers (as expressed in the Department of Education's White Paper 6, 2001, p. 24). Nonetheless, the scale was still considered as able to "measure" attitudes in terms of sentiments, beliefs and concerns regarding inclusion. (The questionnaire was piloted with 20 teachers before distributing it, and certain wordings to questions were altered to modify what were felt to be ambiguous formulations. As I was not involved in the initial phase of this project, that is, the questionnaire phase, I am not aware of whether any persons with disabilities were consulted during the piloting phase. I am aware that springing from "results" of the questionnaire, three main FG questions to pursue further with selected teachers were identified by the researchers.)

This questionnaire was distributed in South Africa to qualified teachers enrolled for the Advanced Certificate in Education: Inclusive Education. In their letter to potential participants, the project team stated (as part of their informing participants about the purpose of the project and what would be expected from the participants if they agreed to participate) that the main purpose of this project is to "create a knowledge base that will shed light on the perspectives of teachers in the different countries regarding the development of inclusive education". They were informed that their participation involved filling in the questionnaire and that some of them would later be approached to participate in a FG further on in the year. And they were told that "participation is voluntary and you may withdraw your participation at any time". As far as the mechanics of the process is concerned they were informed as follows:

> Each participant will receive a participation number at the commencement of the research project that will serve as reference throughout the period of participation. This is to ensure anonymity and confidentiality. There will only be one list linking each participant to his/her participation number for the purpose of contacting participants for the FG discussion.

A questionnaire with accompanying letter was distributed to 605 students enrolled for the Advanced Certificate in Education: Inclusive Education. There was a 47.3% return rate, that is, 283 responses (Nel, 2014). Savolainen et al. state that within the sample, schools were located in well-to-do and advantaged urban areas as well as semi-urban areas characterized by poverty. They note, citing Engelbrecht (2011), that:

> Huge disparities in financial resources still exist between so-called advantaged and disadvantaged schools, especially those in semi-urban and rural areas. Poverty in all its manifestations can be singled out as the most salient characteristic of these school communities. (Savolainen et al., 2012b, p. 56)

They also note that demographically,

> schools in the higher socio-economic areas have learners from various ethnic groups and cultures, but the teachers are mainly white and Afrikaans- or English-speaking …. The more disadvantaged schools mainly consist of black learners and teachers. (Savolainen et al., 2012b, p. 56).

Ndimande refers to research indicating that the lack of resources in disadvantaged schools (especially in township[10] and rural areas) "has left these public schools dysfunctional and with poor academic results" (2012, p. 215). In view of this, we decided when organizing the sample for the FGs, to concentrate on teachers in so-called township areas (semi-urban). As Nel, Romm, and Tlale explain:

> In our locating of schools for this study, we wanted to explore the experiences of teachers in what are called "township" schools that were (under apartheid), and still are, particularly disadvantaged. (2015, p. 37)

The FG research questions were set by the larger international project team and consisted of three questions which were aimed at further exploring teachers' perceptions of, and practices relating to, "collaboration". This focus was because, as Nel (2014, p. 221) notes, "collaboration was identified in the quantitative part of the project as being pivotal for inclusive education. The FG guidelines for all countries consisted of the following questions:

1. How do you understand collaboration within inclusive education?
2. Tell me about your experience with collaboration in including learners who are experiencing barriers to learning at your school.
3. How do you see your role as a teacher within a collaborative network to support learners experiencing barriers to learning?

The South African researchers facilitating the FG sessions (Nel, Tlale and myself) chose to extend the sessions in the sense that we also asked participants in a feedback session directly after the FG session, whether they would have liked other questions to have been asked—so that they would have the opportunity to mention this to us and speak about their concerns.

In Sect. 3.3.1, I indicate how we worked creatively with the three pre-set interview questions stated above and also solicited feedback on participants' experience of the content and process of the FG sessions as well as on our facilitation style (which in effect was Nel's style, as she was the primary facilitator of the session). I also elucidate how we worked with our understanding of the "results" that had emerged thus far from the questionnaire analyses (where comparisons between South Africa, Finland and China had already been made—see Savolainen

[10]Ndimande points out that in terms of the Group Areas' Act of 1953, government officials were authorized to forcefully remove those classified as Black "from cities and metropolitan suburban areas that had been declared White. … This practice resulted in the establishment of tightly state-controlled Black settlements in the periphery of cities in what became known as township neighborhoods, which indeed became racially segregated to this day" (2012, p. 224).

et al. 2012a).[11] I spotlight how we tried to create a space for rethinking with the FG participants both the creation and the import of the findings. This is congruent with Kaur et al.'s point that empirical research data might operate as a "subject to work on" rather than as an "object of uncritical consumption and compliance" (2008, p. 246).

What emerged from the questionnaire analyses was that in the various countries, the findings showed that teachers felt themselves on the whole to be "self-efficacious" (with overall "positive scores" being obtained); and there were correlations between "self-efficacy" and "attitudes" (as had been expected, following authors such as Forlin et al., 2010). But the profiles on the subdimensions of the constructs differed somewhat. As Savolainen et al. note (2012b, p. 61), South African teachers had the strongest self-efficacy beliefs in managing behavior (mean = 4.87) and lowest in collaboration (mean = 4.33) relative to their scores on the other sub-dimensions of the construct (their sense of instructional capability and collaboration). In terms of the correlation between these subdimensions of efficacy and attitudes across the various countries examined, the results showed that self-efficacy in collaborating with other teachers and parents seems to be the best predictor of attitudes toward inclusive education. As Savolainen et al. put it:

> The first findings … show that in Finland, South-Africa and China teachers' attitudes are predicted best by teacher efficacy on collaboration and this finding is expected to replicate in England, Lithuania and Slovenia. (Savolainen et al. 2012a)

Interestingly, as far as attitudes were concerned (according to the study analyses), in South Africa the teachers were less concerned than were teachers in other countries that in terms of inclusive education requirements learners with barriers to learning would be included in their classes. This, Savolainen et al. suggest, could be attributed to the human rights discourse that is prevalent in South Africa (2012b, p. 53).

3.3.1 *How We Worked with the "Findings" in a Creative and Active Way (and How This Was Perceived by Participants)*

I now deliberate upon how we "worked *with*" and not only "*within*" the findings as had emerged from the questionnaire analyses that had been made, in order to cater for what we called our use of a transformative paradigm with pragmatic twist

[11]The analyses reported by Savolainen et al. (2012a) are based on the findings from three of the countries (China, Finland and South Africa); and in their article entitled "understanding teachers' attitudes and self-efficacy in inclusive education" (Savolainen et al., 2012b) they concentrate on comparison of data between Finland and South Africa. The analyses that had been made at this point are what informed our way of proceeding with the South African FG research (and subsequent meetings with stakeholders).

(Romm, Nel, & Tlale, 2013; Romm, 2014). But before proceeding with this I wish to highlight that our idea was, as Majumdar and Sowa (2009), advise, to be flexible enough to appreciate that there are a variety of ways of offering images of social realities, so that we did not try to authorize any particular claims. They state that "science" is inclined to work with "logic-based systems". But when people reason in practice, "they seldom carry out long chains of deductions, and they often perform a 'sanity check' to avoid obvious errors. If a conclusion seems odd, a prudent individual would ask for advice or try an alternative method of reasoning" (2009).

This is how we proceeded in that we used the FGs as an opportunity to organize "sanity checks". Also, one of the areas to which Savolainen et al. (2012b) draw attention is that although it seems theoretically feasible to suggest that self-efficacy predicts attitudes (based on regarding self-efficacy as an independent variable in the relationship with attitudes) one could also argue that the relationship works as well in the other direction—that attitudes "predict" self-efficacy. That is, they acknowledge that it is unclear clear "whether it is self-efficacy that predicts attitudes, or whether attitudes predict self-efficacy (2012b, p. 66). They indicate that in order to cast further light on this, the "future direction [for research] is to collect qualitative and longitudinal data on the development and change of teachers' sense of self-efficacy and attitudes toward inclusion" (2012b, p. 66). They point out that aside from longitudinal research that could be embarked upon in future, the next part of the existing research project was aimed at gaining more in-depth accounts of teachers' specific attitudes, their sense of self-efficacy in implementing inclusive education in their classrooms, and the role that cultural-historical factors play in this regard. This was planned to be achieved via the FG sessions.

They thus do not claim that they have developed clear-cut understandings based on the (quantitative) "evidence". But they suggest that despite this unclarity,

> it is clear that … more attention needs to be paid to teachers' perceived lack of confidence in establishing collaborative support networks and the role their contexts play in teacher education programs. (2012b, p. 67)

It is for this reason that the international team chose to concentrate on issues of collaboration during the FG sessions.

Boyask makes the point that it is important (in line with a sociological imagination) to grapple with "complex educational relationships where direct cause and effect is not discernable …, [with] complex situational influences, [and with] the impact of individuals and their paradigms on research findings" (2012, p. 27). I show below how we (the South African FG team) tried to take into account this notion as expressed by Boyask, and as also expressed by many Indigenous African authors.

Ossai, for example, writing with reference to African Indigenous Knowledge Systems (2010, p. 5) points out that Indigenous styles of knowing are intuitive in the sense of intuiting connections between phenomena and therefore are more holistic than (Western-oriented) analytic and reductionist approaches. Furthermore, the intuitions can arise out of oral storytelling, where people explore their stories

(experiences) together. In addition, in contrast to Western-oriented scientific approaches which he sees as "fast and selective" (for example, selecting specific factors/variables which are isolated for attention), insights are generated in a slower fashion, as people explore their views and experiences together via oral storytelling, in which "factors" are not separated out from each other. Rather, their mutual influence on each other and on other factors too are recognized (2010, p. 5). For instance, we could suggest in this case that teachers' perceived self-efficacy in collaboration may be related to their understanding of *how their efforts at collaboration are being received/responded to*. This might affect their sense of so-called "self-efficacy" (which after all also is relational in that collaboration depends on other parties). Such an interpretation (and shifting) of the meaning of "self-efficacy" to take this into account can be drawn out (inferred) from an examination of how many teachers to whom we spoke described their relationship with officials. Although they did not express their "attitudes" to collaboration in this way, it can be inferred (abducted) that a better quality of relationship with officials would make the world of difference to their own feelings/attitudes—with "quality of relationships" therefore being an additional "variable" to add into the discussion.

The abductive inferencing which I am presenting here tallies with Majumdar and Sowa's interpretation of Peirce's lectures on pragmatism (1903), where Peirce refers to abduction or retroduction as a logic of inquiry separate from inductive or deductive reasoning. Majumdar and Sowa define *abduction* as including "guessing" and "adapting" as a way of coming to terms with, but also making imaginative leaps from (perceived) phenomenal occurrences toward developing, actionable insights (2009, p. 11). For Ossai, these intuitive "leaps" can be seen as being a result of collective knowledge-generation processes which are linked to practical, and valued, ways of social living (2010, p. 10). And valued ways of living are not about "individual" skills, efficiencies, or attitudes, but require *collective discussions* (and ensuing actions).

We tried to highlight in the FG sessions that whatever results had come out of the analyses of questionnaires, were *provisional* in the sense that we were *seeking their views on "what it is like"* (whether they felt capable of addressing the mixed needs of learners). We shared with them that the questionnaire results had suggested that teachers in South Africa largely scored positively on the dimensions of efficiency in instruction and in managing behavior, and not so positively on the dimension of collaboration, but we expressed to them (in non-jargonized terms) that we sought their "qualitative" input and that the questionnaire results may sound "odd" when compared with their accounts of their experiences on the ground. We also were highlighting that such issues require a *discussion between people* (as the "answers" are not clear-cut). And we intimated that we were hoping that they could feel that they were gaining in some way from being part of the research. Hence we mentioned that we hoped it would be a learning encounter for us all. This is also consistent with Kuntz's point that the inquiries should not be geared to "extract" data from the social context of the research encounter, and then to treat "the data" as an "object of analysis" on the part of researchers. The purpose of research is to be

generative, generating new meanings which can be experienced as productive (2015, p. 56).

Space in this book does not allow for a detailed presentation of how the discussions in the FG sessions panned out; but Table 3.1 summarizes the feedback that we received from the participants, which indicates how they expressed what they felt about the sessions. (The table was designed by Nel, Romm, and Tlale as part of our preparation of an article which was published in 2015, pp. 49–51.) In the first FG session, each of us had separately spoken with two participants in order to elicit feedback. Hence in Table 3.1, FG 1 has separate columns with our initials on the top (NN, DT and NR). The semi-colons between statements in all cases indicate that a different participant's words are being used. In rows two and three under each question, we continue the Table by adding related issues that arose in the feedback with the other two groups (FG2 and FG3), where all three of us were present in these feedback sessions, using the same questions as had been prepared for FG1. (Participants in FG 2 and FG3 had requested that they offer feedback as a group.)

What came out strongly from the feedback sessions was that participants felt that they learned from colleagues about their frustrations (and it was a relief for them to realize that they were not "on their own" in experiencing frustrations). They also learned about some possible "tips" that they could share with one another. In addition, they felt that they had learned from the periodic intervention of Norma Nel during the FG discussion when she offered some ideas—for example, when she mentioned that they could ask for support from, for example, social workers and the District, and that they could think about using a buddy system whereby learners who are more proficient can help other learners. Some participants requested more ideas/interventions from the facilitators. I reminded participants (in the one group who raised this request—FG1) that the facilitator had made certain suggestions regarding the harnessing of support. Nevertheless, the participants here asked for more ideas on this (they felt that the facilitator could have extended her role to offer further input in this regard).

Overall, the participants expressed that they were pleased with the interview process, which turned out to be a discussion around issues raised: it allowed them to air and discuss their sense of their challenges, and they felt that the researchers gained valuable information. The feedback session also created an opportunity for participants to state what they would like from us—namely, as one participant expressed it, to carry "the baton" forward by carrying forward their concerns. As she put it,"it [your being here] shows that someone cares and wants to know what is happening. You have been listening carefully to us and we are happy about it. Maybe someone will [now] carry the baton".

Rather than merely "carry the baton" for them (in a "race" in which they would henceforth be excluded) we chose to arrange a meeting, via one of the district officers (a student of Norma Nel—Shila Mphahlele) with district officers as well as a person from Head Office, to which participants were invited. In inviting the participants to this meeting—with the invitation being sent by Shila)—the invitees were informed that this was to be a "feedback workshop" from our side, where we would offer feedback in relation to the research in which they had all participated in

Table 3.1 Feedback from focus groups: summary of responses

	NN	DT	NR
Question 1 *How did you experience the FG session?*			
FG1	– Opportunity to air views; – Say what we want to; – Learn from colleagues about others' problems	– We could listen to other people's views and experiences—fruitful, informative—and learn from each other; – Felt motivated (from being "very, very demoralized" before); – Expectancy—you guys are going to do something with the information you got from us	– Wish for more time to discuss; – We learned that we have the right to speak about policies
FG2	We are pleased we have found people to listen to us		
FG3	I feel personally we are happy to have you coming, that's why we don't mind sacrificing this time		
Question 2 *How did you experience the process of facilitation?*			
FG1	– Good questions, therefore the facilitators got most of information needed; – Process was that of probing questions	– Well conducted, professionally done; – Patient; – Good listener	– 100 out of 100; – Allowed us to express challenges we face
FG2	– You let us talk and vent; – You didn't tell us what to do. So you did not lead us with anything, we talked on our own		
FG3	– You are friendly; – There was good ice breaking. You explained that this was going to happen for me, this is what I'm going to be asked—and even the seating arrangement [sitting in a circle] helped the process; – The seating helped because we feel that we are the part of the research		
Question 3 *How did you feel about the facilitator's questions—did they make sense to you—did they make you think?*			
FG1	– Relevant and meaningful; – They allowed us to express our experiences in class	– Straightforward, understandable, open-ended; gave us a platform to say and relate our experiences in the classroom	– The questioning encouraged us to discuss practical examples, which was good; – Many of the questions were aimed at exploring our answers in more detail
FG2	– They opened up a ground for us to talk freely		

(continued)

Table 3.1 (continued)

	NN	DT	NR
FG3	– The questions were quite appropriate because they were sometimes very general so-so-so as to find out what is exactly happening; – They are appropriate at this stage in terms of finding out what is happening; – In terms of asking difficult questions when you just pose it, someone may keep quiet for a, for a long, long time. So that your facilitation to say individually, you know, how do you see this one? I think, I think that's very good		
Question 4 *Do you think you learned from the facilitators?*			
FG1	– Helped me think despite the challenges e.g., are we doing enough: we can try something else?; – We learned that we can take our concerns to the District	– Learned about interview skills; – Guidance from facilitator was helpful	– Need further ideas from facilitators' experiences; – You could offer something in terms of solutions to the challenges
FG2	– Fortunately for us, the way the facilitator did ask the questions allowed us to express ourselves: by answering those questions we come with, um, ideas and so on. (Here the facilitator summarized this as follows: "*So you're saying that just by asking the question it helped you to reflect on the activity*"—to which the participant replied: yes, to make us aware.)		
FG3	– We are getting a different perspective. The facilitator here probed this further as follows: "*Are you saying that just because we're coming in and asking questions from a slightly different angle that we are helping you to re-look at things?*" The participant replied: It's good that we, we also have, uh, people from a different background to ours coming and finding out, even just the questions, they were, the-the questions that you were asking. It's questions that we always discuss but if they come from another person it shows that we are not off track in discussing these. It is correct so that we also always try to get new ideas on this. There are things that we do every day, you know, some staff here they sit and complain. So we are at different levels but we need to discuss these crucial questions which make our day		
Question 5 *Did you think you learned from others in the group and can you give examples?*			
FG1	– Learned from examples that others offer about how they deal with situations (scenarios); – [For instance], like Killie was explaining that the learning problem with the learners' foreign languages—I didn't know that and the way that maybe a child doesn't understand when she's teaching she'll ask another to explain in mother tongue, aaa you know I've learned a lot	– Learned from each other, for e.g., learned that a colleague has got a difficult learner who needs her special attention	– Learned that colleagues are also frustrated; – District officials do not support us sufficiently; – Policies development and implementation differ; – Learned that our colleagues are trying to cope

(continued)

Table 3.1 (continued)

	NN	DT	NR
FG2	When we share as a group we gain from each other and can implement what we get from others		
FG3	We have learned a lot because some ideas we didn't have but we got some from other, uh, teachers as they were talking. So we have learned things like sharing ideas. Here the facilitator (F_1) asked: "*Right. So can you give me just one example?*" Below we record the extract that followed:		
	P_1 Like when maybe when, when Boitumela said they don't have food they take their own lunch box and give it to the learners		
	F_1 You didn't know that before?		
	P_1: I-I knew but…		
	F_2: It reinforces it again		
	P_1: It reinforces it, yes. And when the teacher says, uh, they take, they-they have a remedial book, I didn't know that. It's there in junior phase so now I have that idea, there's a book. So I will get that in the senior phase		
	P_2: Yes, you can use it in the senior phase. Because there's a lot of the children in the senior phase who are still functioning on this level. They're not functioning there		
	P_1: Yes. So I will just borrow it		
Question 6 *Would you have liked us to ask any other questions?*			
FG1	– The questions that you asked were relevant to our situation	– You could ask questions about teacher/learner ratio and work allocation; – Work allocation is really a burden, for example, if an assistant can concentrate on one child with the learning barriers one [other] can pay attention to the rest of the class	– You could ask questions about the GPLMS[a] so we can discuss how we are supposed to implement it; – We need to talk about there being so many different children with different needs and different types of support required for different situations
FG2	– They were not enough. There are some questions, questions like, for instance, time frames during tuition hours; – Also, we would like to talk more about the people coming from the District: This one from the District will say this; while the other one will say this. So there are so many ideas coming from there. And there are lots of questions to talk about still		
FG3	– I feel we need to ask questions on the system itself: The policies that are given to us. So I-I felt liked that, and if anything has to change in the system, it has to be involving educators; – We don't mean everybody must get into the hall, but at least select… and do genuine consulting; – The-the work is so much. It shows that the person who was writing the policy, they have not been sitting with a child. You are thinking of the teacher, not the child. That's too-too-too, just too much work for the little ones		

Source Nel, Romm, & Tlale (2015), pp. 49–51

[a]This is the Gauteng primary language and mathematics strategy, with Gauteng being the province in which the schools were situated

June on *A Comparative Analysis of Teachers' Roles in Inclusive Education*. We indicated that they would have a chance to discuss a (brief) presentation by Unisa staff with some additional participants from neighboring schools and also with some head office and district officers. We stated that "we look forward to your being there to engage your views once more. Refreshments will be served".

Incidentally, by arranging this meeting, it could be said that we were expanding our "sample" in an active way. Holstein and Gubrium, in their discussion of active interviewing, refer to active sampling as a process which is continuous, in that new participants (not previously envisaged) might be brought into the research process as it proceeds (1995, p. 74). We added an "action-oriented" twist to this mode of sampling by bringing to the table stakeholders who became participants, and could discuss the issues at stake with the (initial) participants. (In Chap. 7, Sect. 7.4.1 I elaborate on this point.)

The meeting was held at Unisa. It lasted about three hours. At the onset of the meeting (after the introductions), Norma Nel gave a brief overview of the research results from the questionnaire administered in various countries. She accentuated that these results suggested that:

> South African teachers do not collaborate the way they should—with one another, with principals, with the District Office, with professionals such as psychologists and speech therapists, and with "us at the university".

She then reported with reference to the FG sessions that the material from the FGs also pointed to the need for more collaboration on all scores, including with the District Office. This was her way of expressing the FG participants' concerns that they do not get sufficient support from the District. She stated that it seems that teachers (including School Based Support Team members) and district officers "are living in two different worlds and sometimes they do not even know each other". She mentioned that this meeting was an attempt to correct this, by setting up avenues for more communications.

After some discussion around issues that teachers raised, one of the district officers also mentioned *her* challenges in servicing the schools in that she has eleven people in her unit to service two hundred and sixty schools. Dan Tlale shifted the focus to the quality of the relationship. He stated that he was concerned that the attitude of the district officials sometimes worked against teachers feeling that they could develop a working relationship with these officials. This was a reference on his part back to what some of the teachers had expressed to us (although he stated it as a concern of his that he had). He asked the question: "How often do head office people or district officers go to schools to praise them about the good work that they are doing?" He suggested that it would be very motivating if people could be given positive reinforcement for the good job they are doing despite all the constraints and challenges. (This again was a reference back to the fact that participants had expressed to us that they felt motivated as a result of our being there, from a position of feeling very demoralized before this.) He stated that our discussions during the various FG sessions showed that people had become motivated by virtue of us having displayed an appreciation of their situation and

what they were doing—even in what he named as an "abnormal" situation (given all the resource constraints).

This statement helped to set the tone for the rest of the discussion, with district officials mentioning that "We normally say to teachers that 'you are doing a good job'"—trying to encourage them in this way. Whether or not they do "normally" say this to teachers, at least this session was an opportunity for them to express this!

After the "close" of the meeting (at the point at which people were taking refreshments), teachers, district officers, and the official from head office continued to hold long conversations amongst themselves. As part of this, further meetings were being set up between district officers and teachers, and between one of the district officers and the head officer. (Dan took Norma Nel and me aside and showed us how many people were engaged in conversations and indicated to us that he had heard them setting up further meetings.) We (Dan, Norma and I) were all delighted with this as this reflected our best hopes. Instead of the research merely having the outcome of suggesting that a sense of self-efficacy to collaborate as measured by the TEIP could make a difference to people's attitudes to inclusive education, or trying to work out whether the direction of causality might be in the other direction (or even that the influence was multidirectional), we *set up forums for people to discover how they could collaborate in practice in terms of an understanding of both self-efficacy and collaboration as a function of the quality of people's interactions—so that, in line with more Indigenous thinking, the "variables" become less individualized.* Furthermore, it was interesting to us that before the official start of the meeting of 10 December when I asked some of the teachers whether there had been any further collaboration between teachers following the FG sessions earlier in the year, one of the teachers said one of the issues where the ISLT team decided to speak further was around the question of poor parental involvement; they had started to discuss strategies as to how to get parents more involved—especially when their children are not achieving appropriately.

Considering Mertens and McLaughlin's (2004, p. 108), and Frels and Onwuegbuzie's (2012, p. 6) suggestions concerning asking for reports/testimonies on what kinds of actions have been set up as a result of participation in a research endeavor, this informal "reporting" is evidence of some catalytic process having been set off. And by collaborating in the meeting of 10 June with the District in ways not previously envisaged or practiced, participants experienced another way of developing more collaborative networks. This too can be treated as having been catalyzed via the research. Considering longer term implications, curricula emphasizing collaboration in inclusive education practices became discussed in our Unisa College of Education, including in the Centre for Professional Development at Unisa (part of the College).

In terms of contributing to "generative theorizing" (see Chap. 1, Sect. 1.3.2 and Chap. 2, Sects. 2.4–2.6), we might argue, along with Morrison (2012, p. 22), that "complexity theory" has much to offer in recognizing that (qualitative) change in social systems is "unpredictable". This means that the "predictions" generated by treating certain variables as independent ones that can predict outcomes, should be treated circumspectly. At the least they should be read as providing opportunities

for engaging initial participants and other audiences in discussions around the meaning of variables and their connections. In this case, I would suggest that some attention could be given (in self-reflexive fashion) to the way in which the questions in both the TEIC and SACIE scales are framed. This would involve reviewing the current concentration on asking teachers about their *individual* "abilities" and their feelings, beliefs and concerns—rather than, say, about how these *may be affected by their interactions with others* and how they become formed in processes of interaction.

For example, one item in the TEIC scale that is meant to measure "self-efficacy in instruction" asks teachers to respond on a Likert scale to the statement: "I am able to provide an alternative explanation/example when children are confused". But this way of setting the question (and indeed all of the questions linked to "instructional efficacy") leaves out the option of teachers involving learners' peers in helping each other to "provide an explanation/example". The question as set implies that teachers who wish to see themselves as adept in "instruction" consider that they are equipped to provide their own "alternative examples and experiences". However, Magano, Motitswe, and Tlale (2014, p. 24), referring also to Hay (2003) indicate that

> learners' peers can be powerful collaborators with the teacher to help support the learning … . Peer educators may be one of the teacher's sources of support as they [teachers] seek solutions and alternative strategies for working with their learners with diverse learning needs. (Magano, Motitswe, &Tlale, 2014, p. 24)

We see that Magano, Motitswe, and Tlale would consider that items measuring "self-efficacy in instruction" may be wanting if they do not already include possibilities for support from various places, including from learners' themselves. This would be consistent with Chilisa's statement that in Indigenous research contexts inquirers using "questionnaire-based methods" need to be wary of adopting or devising survey questions "using languages and Western-based conceptual and theoretical frameworks" (2012, p. 238).[12] Here Magano, Motitswe, and Tlale *express and endorse* a more social relational approach in considering any "variables". This would also be in line with Molefe's indication (2014, p. 129) that conceiving the world as relational through a relational ontology contains also a "normative load" from the start to *strengthen such relationality*.[13] Similarly, one of the inputs made by Norma Nel in her facilitation of one of the South African FGs in the

[12]To circumvent what she considers to be "adopting an inappropriate technology [in this case questionnaire-based method] from a developed country to use in a developing countries", Chilisa points to various ways of improving Western-styled questionnaire-based methods (2012, p. 238). She points to the importance, inter alia, of ensuring that "culture- and age sensitive local words, terms, and concepts become incorporated in the [construction of the] questionnaire" (2012, p. 238).

[13]As he expounds (2014, p. 129): "The fact that all being and reality is interrelated in the African traditions is not merely a descriptive fact but also one which entails a normative load. The normative package flowing from this view of reality is that, the interrelatedness of being is a state of being we *ought to strive to maintain*" (my italics).

international project (see Sect. 3.3) was to suggest the option to teachers of employing a "buddy system". This too could be seen as a way of constructing "self-efficacy in instruction" as a more *relational* "variable".

As far as the variable of "managing behavior" is concerned, the focus in the TEIP scale is on the degree of teachers' (individual) confidence to "prevent disruptive behavior" or "get children to follow classroom rules". Again, one can place a different emphasis here if one highlights "managing of behavior" as linked to an orientation of (relational) caring—which we shall see in the example below Tlale tried to inspire during his "intervention visit" in the 500 schools project as discussed in Sect. 3.4.6. This was in a context where he understood from prior FG sessions that he facilitated (with teachers, SMT members, parents and learners) in other schools that often teachers connect "managing behavior" with a form of punishment or threat of punishment. By shifting the orientation, he was trying to inspire what Morrison (and other complexity theorists) call an emergent (and different kind of) order.

From the research results, the variable that seemed to have the best predictive power to influence attitudes (as reported by Savolainen et al., 2012a, 2012b) was, as stated above, that of collaboration. The South African profile on this sub-dimension implies that South African teachers feel less confident (than the other country counterparts) to collaborate. But again one could argue that an item/statement (as on the TEIP scale) such as "I can collaborate with other professionals" does not provide for the *relational* aspect of collaboration (where people may become disappointed by the approach that the "other professionals" adopt when trying to advise/instruct them).

With reference to all of the above, I would like to re-iterate that ways in which respondents in questionnaires portray "attitudes" (as measured by questionnaire items) are also a *function of the manner in which questions are asked*: if questions gear respondents toward expressing feelings/beliefs/concerns at a point in time—separated out from the interactions in which they are involved—this *can well direct the respondents and also researchers interpreting the "results" (and wider audiences) not to concentrate on the quality of social interconnections in which people are enmeshed.*

Morrison's deliberations (2012) concerning "searching for causality in the wrong places" are again pertinent here. Morrison makes the point that according to (his interpretation of) complexity theory, "the interaction of elements/agents of, or in, a system gives rise to a new system that is qualitatively different from the elements that give rise to it" (2012, p. 24). He gives the example of a crowd: "we cannot explain the behavior of the crowd in terms of the individuals in it, but at the level of the crowd itself" (2012, p. 24). He goes on to state that

> to understand the processes causing emergent order requires a holistic rather than an individualistic view of system-level behavior … the interaction of the elements of the system [can] produce new, unprefigured, unpredicted and unpredictable, emergent order. (2012, p. 24)

This has implications for ways of conceiving, and gearing toward, emergent change. As Morrison explains:

> For such changes to occur requires systems and agents to be open to themselves, their internal and internal environments, for feedback, feedback loops, causality and causal influences to be multidirectional; everything influences everything (co-evolution). (2012, p. 24).

By "co-evolution" Morrison—following other complexity theorists—suggests that people (and the systems in which they are enmeshed) play a part in shaping their "environments". This understanding of the relationship between teachers and the larger "environment" is of course closely related to Indigenous understandings of web-of-life connectivity (cf. Smith, 1999; Kovach, 2009; Chilisa, 2012; Molefe, 2014).

What then does all this imply for the continued use of scales such as the TEIP and SACIE in terms of their becoming a basis for research that can lead to learning about inclusive education? It could be argued that the "individualized" focus of the scales was not sufficiently attuned to the more relational ontology of many of the respondents in South Africa (and perhaps in other countries too). Admittedly, because the study was meant to be internationally comparative, questionnaire items could not be modified radically to cater for the cultural context. Sharma, Loreman, and Forelin (2012, p. 17) suggest that follow up qualitative research can be an opportunity to correct for the omission of "cultural context". This qualitative work could indeed be one way in which the international comparative data could become *worked with imaginatively*, as we in the South African FG research team tried to encourage (during the FG sessions and the subsequent meeting and now in this book for wider audiences). This was our manner of taking some responsibility for (re)directing the research so that it could be contextually relevant.

I now proceed to discuss in some detail the 500 schools project and to draw out some additional lessons for responsible research practice.

3.4 Some Detail on the National 500 Schools Project

As one of the principal researchers in designing the research I was party to the study being explicitly intervention-oriented, with questions in questionnaires and also in FG interview guides being directed, inter alia, at asking respondents/participants about potential solutions to problematic issues as experienced. In constructing the questionnaire items, we (and specifically McKay) also took some cues from the DBE researchers with regard to pertinent issues to be explored in the questionnaire, based on gaps that they had identified in previous research.

3.4.1 *Designing the Questionnaire: Taking into Account Its Transformative Potential*

Before I begin to provide some examples of how we set up the questionnaire to invite suggestions from participants that could be helpful for our further interventions, I want to make a point about how we sought what we called (on the questionnaires) "School Profile and Biographical Information". In this section (for both principals and teachers) we sought information about the school that would allow us to identify it (including the district in which the school was situated, and the province). We also asked for specificities of the school's location as rural, urban, township or peri-urban. In addition, we sought some biographical information in relation to the age of the respondents, their gender, their position in the school, for how many years they had practiced as a principal or teacher (in the school or in another one), their highest qualification (and whether they had qualifications in the subjects they teach), and their home language. We intentionally did *not* ask them to specify whether they were "White", "Black", "Colored" or "Indian" (racial categorizations that are still used on certain forms in organizations, normally to be able to check for affirmative action requirements toward more equity). We were concerned that in asking principals and teachers to use these categories, it could serve to reify "race" as if it referred to an existing "thing" (biological or cultural) rather than being a social construct that becomes applied in race*d* relations. This was in keeping with my discussion in my book on *New Racism* (2010, pp. 62–66) where, I tried to show how researchers can unwittingly contribute to producing a reified understanding of "race" in respondents' consciousness by the way in which they treat it, for example, in questionnaires. In this case we felt that through the information that we requested in the profile of the schools, we would have sufficient data to "place" respondents' answers with reference to their background, given the way in which socio-economic positioning of schools is still racialized. However, on reading this text, McKay made the point that it is still debatable how we handled the question of "race" in this project. She pointed out that:

> 99% of the schools that underperform are black but since the focus was on the challenges it was not necessary to use race. However, in a way, we do them a disservice because 20 years after apartheid the same race groups have been underperforming which is an indictment. The system still privileges white learners and so naming the problem would have been better—but leave it as it is. There are times when race must be mentioned. (McKay, 10 March 2017 by email)

Here McKay suggests that the issue of when to mention "race" in research processes is far from clear-cut. But we made a choice at the time not to use this category when we engaged with participants (via the questionnaire and focus groups) in exploring with them their challenges and options for improving school performances.

To ensure that the participants (principals and teachers) would feel that they could offer suggestions for improving learner performances rather than being constrained by a purely closed-ended format in the questionnaire, we interspersed

open-ended questions throughout the questionnaire, which also offered them a chance to offer their reflections at various points in relation to the topic that the closed questions were introducing, such as language, mathematics, school attendance, and so on. (All in all there were 37 questions for principals and 76 for teachers, so I will only offer a few examples here, namely some of them pertaining to inclusive education.)

Regarding inclusive education, we asked some closed-ended questions, such as:

"What is the average percentage of children classified for special needs and/or with disabilities in your school?":

Between 0 and 1%
Between 1 and 2%
Between 2 and 3%
More than 3%
If more than 3% indicate approximate %……

We also asked: "Are any staff members in your school trained in inclusive education?". Here we offered a Yes/No response option, which was followed by a follow-up: If your answer is "Yes", what proportion of teachers are trained in the following (with response options being 1–24, 50%, and 25–49%):

Inclusion
Psycho-social needs and support
Emotional needs and support
Other (specify)
If other, please explain your answer
……

Our creation of the Psycho-social needs and support category in this question was to make provision for the understanding that many "barriers to learning" could be attributed to features in the social environment—such as children being orphaned, child-headed households, parents working at a distance, and poverty-related problems. And by speaking of "Emotional needs and support" (the third category above) again we wished to *highlight a non-deficit understanding of children* as needing support rather than as having "individual" problems.

We also thought it important to ask principals in the questionnaire whether there were challenges in the school regarding timeous coverage of the curriculum (with response options being "Yes", "No", "In Some Grades Only") and we asked them to offer commentary on their responses—thus inviting their suggestions. I mention this here because most principals (excepting in Mpumalanga) stated that there were general problems in covering the curriculum and many in their later commentary stated that some factors which hampered such coverage included their being expected to deal with "learners with barriers" or that "certain learners achieved in a

manner which was not compatible with the pace setters and teachers did not want to leave them behind".

Toward the end of the questionnaire for principals we posed the following open-ended question: "Do you have any suggestions for improving teaching in your school?"

…….

And finally we asked: "Is there anything you wish to mention at the end of these questions?" (See Sect. 3.4.2 for my account of the purpose of our asking this question and the kinds of responses it elicited.)

……..

In the questionnaire for teachers, as with that for principals, we interspersed closed and open-ended questions. As one example, we asked teachers how often they communicated with the principal (closed-ended) and, if they communicated, on what topics they spoke (open-ended). We also asked (open-ended) what additional support they would like from other bodies such as the School Governing Body (SGB) or other possible sources of support.

Concerning inclusive education, in order to match up our question on this that we asked of the principals, we asked teachers a closed-ended question about whether they had undergone any training in: Inclusion, Psycho-social needs and support, Emotional needs and support, and Other. And if Yes (to any of these), we asked them to fill in which institution trained them (e.g., provincial department, DBE, or a university, or other).

We also asked open ended questions regarding the implementation of inclusive education in the schools, viz:

What problems do you face in implementing inclusive education?
What support have you received?
What support would you like to receive?

As with the principals' questionnaire we asked: "Do you experience difficulties in covering the curriculum? ". Here we gave as response options "Yes" and "No" and we followed this with:

> If you experience problems, please state what problems you have? Or if you don't experience problems, say why you are successful.

And finally we asked of the teachers (as we had of the principals) the question: "Is there anything you wish to mention at the end of these questions?" This formulation had been "tested" for its friendliness as well as broadness on many past principals and teachers at Unisa—and it went through several iterations before we came up with this way of setting it. Nonetheless, as I argued in Romm (2017) it is possible that there are improved ways of asking for respondent/participant feedback vis-ā-vis their views on the questionnaire which they have just completed, and their experience of filling it in. Below I offer some reflections on our setting of this question (and responses elicited).

3.4.2 Some Reflections on the Setting of the Final Question

Although it could be seen as an oversight that we had not asked participants explicitly their views on answering the questionnaire, by our asking "Is there anything you wish to mention at the end of these questions?", we had hoped to provide them with the opportunity to comment generally on the questionnaire. Our final question was an attempt to give all the participants a chance to give comments of whatever nature they chose to do.

Readers may notice that I have tended not to use the word "respondent" when referring to respondents/participants. This has been to underscore that as far as possible we tried to engage the participants in being able to contribute their view of what was important to them. Chilisa notes that normally questionnaires are a "top down" method of data collection in the sense that researchers cannot check the construction of the questions or their meaningfulness with the large number of respondents/participants as required in a survey. As she puts it: "The questionnaire is a top down method of collecting data that mirrors the worldview of the researchers or their perceptions of the topic to be covered" (2012, p. 78). She cautions, along with Mukherjee (1995, p. 41), that this method, if used, should at least not be regarded as objective (or even close to objective) as in positivist or postpositivist conceptions of it (2012, p. 28). (She defines *post*positivism much in the same way as I described it in Chap. 1, Sect. 1.2, as adhering to the belief that "perfect objectivity cannot be achieved but is approachable"—2012, p. 28.) Nonetheless, she suggests that it may be possible to appropriately Indigenize questionnaires (in terms of Indigenous research ethics as discussed in Chap. 1) so that the concern would be with using them ultimately to enhance the quality of life of participants and other key beneficiaries (2012, p. 31).[14] Chilisa and Preece (2005, p. 110) propose that if questionnaires are used, they need to be used with an awareness of challenges in their use such as "how to construct questionnaires that are relevant to the purpose of the survey, sensitive to the culture, age and needs of the participants and inclusive of their worldviews".

Gboku and Lekoko (2007, p. 70) state that one of the strengths of a survey method is that "a considerable amount of information can be obtained from a large population or across numerous sub-populations". The major weakness of the survey approach, they note, is that it "may be superficial". We tried to mitigate the kinds of limitations identified by authors such as Chilisa (2012), Chilisa and Preece (2005), and Gboku and Lekoko (2007) in their quests to decolonize research. This was firstly by our offering many opportunities for open-ended responses, and secondly by our asking our "final question" leaving freedom to participants to express how

[14]This approach to questionnaires resonates also with Goff's (2017) account of her involvement in a sociocultural survey concerning the value of environmental water to Aboriginal populations living in Australia's Murray-Darling Basin. She cautions that it is important not to "maintain the power of interpretation of results in the hands of professional … researchers" such that processes of "synthesizing results become inscrutable" (to participants and stakeholders) (Goff, 2017, p. 188).

they felt about the questionnaire (and its meaningfulness or not to them). Markedly, answers to this final question indicated that many of the participants who answered it[15], urged us to relay the information to the DBE so that officials could be aware of this information for possible government action. (The DBE in this case was not our (direct) sponsor, so we were under no obligation to "satisfy" them; but we did regard them as a stakeholder, and we did regard them as a constituency with whom we could discuss results.[16])

3.4.3 Responses to Our Final Question

The "final question" at the end of both the principals' and teachers' questionnaire, elicited various comments expressing appreciation of our having included the participants in the research endeavor and also expressing that they wanted to get feedback on the research information which was being collected through the research. As one principal put it: "Researchers [should] help to keep us informed and not hesitate to do so". The persons compiling the responses to all the open-ended questions in the questionnaire (a team arranged in conjunction with the project leader) noted in regard to this "final question" that: "Respondents felt strongly that the researchers should give them feedback on the results of the research project". Below I offer a sample of some principals' as well as teachers' responses, starting with the principals.

Some principals stated that they were hoping that this research would lead to advising the DBE. One stated this by suggesting that the government could now become more equipped to do what is necessary toward "empowering" education: "It should be better if this task team advises the department of education to make everything to empower education". Or, as another stated it (as a plea): "Forward the information to the department for more support; what has been said here will hopefully be addressed as it will improve education especially in the rural areas". Such participants were clearly hoping that the researchers would be able to act as interlocuters in some way to "forward the information" to places where it could be actioned. Another stated this hope more strongly: "I am very much pleased to receive such a questioner that will have the school to expose the critical conditions which the school is working under". The same person commented that s/he found the questionnaire could be educative (for those seeing the results): "This has been an educative and constructive survey as far as schooling is concerned, through the information you will get in this form. We need help and support because educators were willing to do their best for improving teaching and learning". Here this

[15]The team who analyzed the answers to this question did not indicate how many people responded to it, but perhaps this is less important than the quality of the responses.

[16]McKay pointed out to me (pers. comm. via email, 10 March 2017) that the money we use in the university comes ultimately from government; but they were not "direct" sponsors of this project.

participant indicates that s/he is hoping that participation in the research will have some "impact" in making others aware of the plight of the school and their need for support.

Some of the principals took the opportunity to mention that they looked forward, ideally, to more field work with their school and more visits. For instance, one stated: "if the field [workers] can make follow up from those questionnaires, more could be achieved". This was a reference to the fact that the field workers had informed them that certain schools would be sampled for further visits (for FG session and intervention visits).

As far as teacher responses to this final question are concerned, these were also largely indicative that they appreciated the chance to participate in the research. Some made the point that when they have participated in other research projects there had been no follow up (either in the form of feedback to them or in the form of what has been done in other forums with the "results" to date). Thus, by way of answering this question, they requested that we should take a different line, so that they could see some utility emerging from the research project. I point to their responses below.

Some teachers mentioned that the questionnaire itself was already helpful to them in certain respects. One stated: "Appreciate this questionnaire, I gain a lot as it gives me the strategy on how to teach". This teacher felt that just by our way of phrasing the questions relating to teaching of different subject areas, this stimulated some learning about teaching. Another also commented in similar vein: "The questionnaire made me to know many things about my learning area". Or as another stated: "I learnt about lot of things". One stated that: "it was about our lives, it motivates us". And another stated this more strongly: "it was motivation itself, the questions were relevant to education". Other similar sentiments were expressed: "Questionnaires empower teachers about teaching and learning"; "Questions give us feedbacks to understand more about maths"; "The survey was very interesting and informative"; "This survey was interesting".

These are noteworthy responses in that they imply that a questionnaire that is well designed with a view to "helping" participants, can indeed perform such a learning/empowering function, because respondents become exposed to phrasings/terms that they may not have thought about before. McKay makes the point (pers. comm., 10 March 2017) that when she worked on the questionnaire "I did it almost as an aid memoire so that the questions themselves would alert them [the respondents/participants] to policy and practice. The tool itself is informative"". (See also McKay & Romm, 2008, and Romm, 2013a, for more detail on how questionnaires can function to stimulate thinking; and see Mbuli and Romm, 2015, for further discussion.)

Remarks on the way in which the questionnaire results could be used by the researchers were also very prominent in teachers' answers to this final question. The following is a sample of the types of comments received in relation to how the questionnaire results (after analysis) could possibly be useful in forwarding "advices" to those who could action them, and in "raising awareness". The semi-colons

between sentences/phrases below signify that a different respondent/participant is answering.

> "Department could implement all the recommendations and advices given in this survey"; "Feedback [to respondents] should come in a form of positive result [a number of respondents asked for such feedback]"; "Follow up on this research and provision of the school needs mentioned in this question paper [here the person is asking for impactful follow up]; "Give awareness about teaching and learning".

Some teachers were very explicit on their hopes:

> "Hoping this was going to bring an improvement in education and training"; "I appreciate this survey and I would like to continue it helping by the field worker"; "I hope this survey gets attention of people in charge"; "I should like the department to look at the recommendation that were given in this document or form"; "I wish the department will implement all the suggestions that have been provided in this form"; "I wish to request that higher institutions like Unisa should not just conduct research and abandon those schools; help us to improve qualifications attend courses"; "I would like to be developed (professional development) by the department of education"; "I need training in my learning areas if the paper work can be reduced"; "Unisa please next time visit us at school, otherwise your questionnaire was okay we understand it, we always find this questions from various field works [in other research projects] but there was no follow subsequent intervention flowing from it to help us make improvement in our teaching and learning".

In this last response, where mention is made about "subsequent intervention", the teacher is expressing that there have been too many prior research studies where nothing "flows from them". In this research we tried to take account of such prior experiences and to not continue a tradition where participants feel that there is no follow up. Hence we arranged (as planned) an intervention guide in which we drew on responses from both the questionnaire and FG sessions to create a booklet that could be of use for teachers across the country. And we also did visit certain sampled schools, as planned—namely, the ones where teachers (and others in the school) had participated in FG sessions, where we used the guide to support the intervention.

From some of the answers to our final question in the questionnaire it can be inferred that many felt that the questionnaire format (and time arranged for filling it in) was not sufficient for "discussion". (This serves to justify our earlier decision to organize FGs in addition to the questionnaire, albeit that the questionnaire had many open-ended questions.) The following are types of response that showed up some of the constraints that participants felt in filling in the questionnaire—especially as only one hour had been set aside for filling it in:

> "Give us time to discuss, time was only 1 h"; "It was very good but the time for filling not enough as it was very long". "Time was limited"; "Questionnaire takes too much time".

There were a number of suggestions to the effect that perhaps a quicker method [less extensive questionnaire] for soliciting response to the issues to do with learner performance could have been constructed. Yet, as indicated earlier there were also participants who felt that the (extensive) set of questions was helpful in enabling them to learn more about "teaching and learning" simply by virtue of filling in the questionnaire.

3.4.4 The Design of the Focus Group Sessions

With this background on the questionnaire and its place in the total design, I now turn to the design of the FG sessions (of which in total there were 60: four in each school, with three schools in each province, and across five provinces). The FG questions were created (by some staff members who also were involved in the construction of the questionnaire), in such a way that the two "instruments" would ideally complement each other. The FG guide had to be short, so that the discussions with participants would not be too protracted. This was in accordance with our indication to the provinces in our permission letter that teaching and learning programs would not be unduly interfered with through the research. So after some deliberations we decided not to explicitly include questions on inclusive education in any of our FG guiding questions (for any of the four groups of participants—SMT, parents, teachers and learners). But as it turned out, as Dan Tlale (who participated in FG session in a number of provinces) mentioned to me, the issues to do with inclusive education came out implicitly during the discussions. In Dan's words (pers. comm.): "it was not said as if it is inclusive education, but issues such as, discipline problems, child-headed households, being orphans, all are barriers to learning".[17]

The questions that were prepared for all 46 staff members to utilize in their visits to various provinces were given as part of the Focus Group Field Guide (prepared by Smit and myself, 2012)—which all researchers were given (in addition to attending a workshop, as indicated in the Introduction to this chapter). In the Guide it was mentioned that before beginning any of the session, facilitators should state the following (or handle the introduction to participants in this spirit):

> The idea [of this session/meeting today] is that together we can explore issues connected with teaching and learning more fully. This will supplement some of the data that we have obtained from questionnaires that have been filled in by a large sample of teachers across 5 different provinces. This will help the University (University of South Africa) to design an intervention strategy for improvement of schooling. All information in the interview will be treated confidentially by us. That means that no-one will know when we do our reporting who has said what. If we want to mention your ideas, we will use another name to record this so that no-one will recognize you in our report. Your answers are going to be used only for the purpose of us trying to understand more so that we can find ways also based on your input to "make schools better". In summary, although we have the permission from the Department of Education to do the research, we are not going to give any confidential information to the principal or the Department. Please note that if you are feeling that you do not want to answer some of the questions asked when you hear them, you can mention this to us—we do not want you to feel under any pressure here. But your answers will be helpful to us to gain a better understanding of what we are asking you about. You also may learn by hearing our questions and thinking about your answers to them. And hopefully you will learn from one another.

[17]Dan said this to me in the context of our preparation for out writing of our article called nurturing research relationships (Romm & Tlale, 2016).

At the end of each session we suggested that facilitators should say something like:

> We are interested to know how you all have experienced this FG discussion today. May we ask you to offer a few brief comments on this please, just mentioning how you felt about being involved in this group discussion today. Please feel free to say whatever comes to your mind about this. Who would like to start?

We suggested the following closure:

> Thank you very much for sharing your views with us! Is there anything else anyone would like to say or add? If there is anything else you would like to say to us after this session, here are my contact details (facilitators to supply) and the contact details of others in the project … ". Please contact us if you wish to mention anything further about your feelings about the issues we discussed or other issues that concern you in relation to this project.

Finally, we suggested that the green pens with Unisa's logo inscribed on them should be distributed to the participants as a sign of our appreciation for their having participated. (The project leader arranged that sufficient pens would be created for this purpose.) Interestingly, nearly all the participants in the (four) groups in which I was involved used this "closure" period to thank us for bringing along refreshments—fruit juice, biscuits, nuts, raisins, mint sweets—which we had (in the beginning) put on the table to share (also with a good budget from the project leader). The pens were also very well received. (This was important as it was an indication that we were not just "taking" from them: see also Smith's account on the giving of gifts, 1999, p. 105.)

3.4.5 Guiding Questions for the Four Sets of Participants (Per Sampled Schools)

The actual guiding questions for the four groups were of course different, but all were aimed at trying to elicit information as well as suggestions to "make schools better". For example, some questions for teachers were:

- Considering the ANA results of 2012, what do you think could be done to assist your learners to perform (even) better in ANA in future?
- What challenges do you face in teaching your subjects?
- What are the types of interventions that do *not* help in improving teaching and learning as you see it?
- What types of intervention *would help* in improving teaching and learning as you see it?

For the parents we included questions such as:

- In what kinds of ways do you think that you provide support to your children in helping them through the schooling and school work?

- What kinds of support would you need in order to feel that you can take more of a part in your children's schooling?
- Are there ideas for action that any of you can think of to make it easier for parents to take part in their children's schooling in some way?
- What would be needed for this? Let's all think together about possible ways forward.

For the SMT we included questions that asked them about their roles, challenges and options for improvement, such as:

- What kinds of roles are you expected to play in the management of your school?
- Do you feel that you can perform your roles as expected?
- What is the ideal situation you would like for your school in future?
- What do you think is mainly needed in order to get there?

And for the learners we tried to make the questions easy to relate to. So we guided the facilitators to pose questions as follows:

- What kinds of things that teachers do (when teaching the class) help you to learn best?
- [*Prompt*]: Let's talk more about this now. Let's think of examples.
- What kinds of things that teachers do (when teaching the class) make it more difficult for you to learn?
- [*Prompt*]: Let's all talk more about this now. What are you finding difficult to understand in the mathematics learning? What are you finding difficult to understand in the science learning?
- What are you finding difficult to understand in the First Additional Language Learning? [*for non-English medium of instruction*]
- What are you finding difficult to understand in the Home Language Learning?
- How are you finding the workbooks that are used in the class for different subjects? Do they help provide support for your learning?
- What makes you feel that you want to come to class? Is there anything that makes you feel you do not want to come to class?
- *Finally, we are interested to hear your feelings about being involved in this discussion today.*
- *[Prompt]: How did you feel about answering the questions?*
- *[Another prompt:] Would you want other questions also to be asked?*
- *[And another prompt]: Are there any comments any of you would like to make about the discussion today?*
  (*And we then suggested that facilitators move to the closure according to the format of the other sessions.*)

I have offered this outline of our way of structuring the four sets of FG sessions so that readers can gain a sense of how we tried to gear the FGs (similarly to the questionnaires) toward *generating ideas about challenges and possible ways of*

addressing them, to later share with other participants (in our "intervention guide") as well as with other stakeholders (such as the researchers in the DBE).

Due to space limitations I will not offer an analysis regarding the results that ensued from use of our "instruments"[18], but I will mention that our design of the project was such that when we went on the second FG visit as a kind of member checking event as well as for expansion of ideas, many of us chose to act as messengers of children's perspectives to the staff, SGB personnel and others who attended the second FG visit (which in some cases included the principal). We left some discretion to how the research team would handle the visit, but it was arranged so that we could offer feedback also to those with some "actioning power" in the schools. The visits involved two days of liaising with participants, normally in a "meeting form" where ideas from the various FG participants were put forward and discussed as a totality (with chances for modifications of ideas and further suggestions to be made). It thus became a sharing process with researchers presenting outlines of the research results, especially pertaining to the previous FGs at their school. In the process, issues that had been brought to light by learners were also given attention by researchers acting as "mediators" bringing issues to attention that learners may have felt difficult to raise in front of those with perceived authority such as teachers, etc. Hence, issues that many learners stated as in issue of concern (that some teachers too had mentioned to us in the first FG visit) were "relayed" (on their behalf) by the researchers during our second meeting to the school, such as:

- Learners learn best when teachers used creative teaching or humour when a detailed explanation was given or a dictionary was used.
- Learners learnt with difficulty when teachers shouted at them or failed to give detailed explanation for homework.
- Corporal punishment (which many children mentioned as problematic and certain teachers admitted was still taking place) made it hard for learners to understand. It also made them frightened to attend school and this was one cause of absenteeism.
- Failure from teachers to curb bullying made it difficult for learners to learn. (Learners in some FGs suggested that if teachers appoint learners' peers to watch out for and report bullying to the teachers, this would make it easier for the peers to do so.)
- Terminology in Science and Maths was difficult for learners and they lacked a detailed explanation from teachers.
- English vocabulary was a problem for both Grade 3 and 6 learners.
- Workbooks were helpful but sometimes teachers promote rote learning. Learners managed to read workbooks on their own at home.
- Learners enjoyed coming to school because they had goals in life or experienced exciting events at school.
- Learners felt that going to school was important since they were protected from drugs, rape, bullying and human trafficking.

[18]Some analyses and ways of engaging with them are presented in Romm (2017).

It was hoped that by raising these issues on behalf of the learners, we were doing justice to their agreement to participate in the project. When we (the researchers who had been involved in the FG sessions) later met to discuss both the first and second visits to the various schools, many researchers stated that they had indeed brought up the issue of "discipline" and tried to express to teachers that it would be better to create a classroom climate where the children were not shouted at or punished, but where teachers tried to motivate them by making classes more "exciting". And in the "intervention visits" this was also a theme carried forward by some of the researchers—as we had highlighted this for attention in the meeting of the researchers (after the focus group phase of the research) and had considered how to handle it. We decided to handle the issue carefully so that teachers and principals who had admitted that corporal punishment (which is illegal in South Africa) is still being used would not be "exposed". Nevertheless, we could make the discussion general enough for them to realize that it could generate problems in their school and that there could be better ways of encouraging learners.[19]

3.4.6 Intervention Handbook and Intervention Visits

The intervention handbook was organized to include four sections where it was felt that teachers and SMTs would most likely benefit from "assistance", based on the results from both the questionnaire and the FGs and based on "practical" know-how of the authors from their being involved practically in the areas covered. These areas were: school curriculum design and implementation; inclusive education; laying the basis of teaching mathematics; and parental involvement in education. The section on inclusive education (which I am concentrating on in this chapter) was co-authored by Magano, Motitswe, and Tlale (2014). Motitswe and Tlale used to work as district officers aiding schools to address issues to do with inclusive education before coming to work at Unisa. This served them in good stead to design what was intended to be practical ways to help teachers and others reading the handbook, based also on the authors' interpretations of the results from earlier phases of the research. This handbook, which was delivered to all the schools who had participated in the project as well as to the provincial officers in the department of education and the national office, was another way of "giving back" in a form that would be usable by participants/stakeholders.

The researchers compiling the handbook (all four sections) had conducted FGs in various provinces and had also seen the questionnaire results as well as FG summaries across the board that had been presented in the draft report that had been distributed to all staff involved in the project. In this sense the researchers' sharing

[19]Subsequent to the official close of the project, McKay, Mohape, Romm also prepared a chapter in a book that could be used as a resource for teachers (and others interested). The book—edited by Magano, Mohapi, and Robinson—is entitled *Re-aligning teacher training in the 21st Century*, and our chapter is entitled: "Rethinking School Discipline" (2017, pp. 250–270).

of their understandings as now proffered in the handbook could not be separated from their learning encounters that had ensued during the research process: the handbook could thus be regarded as a product of the relationship between the various (professional) researchers and research participants.

As far as inclusive education is concerned, the questionnaire responses indicated that most principles reported that less than 25% of their staff were trained in any aspects to do with inclusive education that we had mentioned in the questionnaire as options, viz: inclusive teaching; offering support for psycho-social needs; catering for emotional needs; and for "other" aspects. In the Free State and Mpumalanga an even larger percentage of principals reported that <25% of the teachers in the school were trained in "psycho-social needs and support", and likewise for emotional needs and support. Teachers commented in the "comment" sections provided in the questionnaire that inclusive education implementation problems arose partly through their own lack of training and also through the learner profiles—with different groups of learners, diversity of learners, and different behavior patterns. In regard to training, they mentioned the possibility of more workshops, training by experts from the department and district level, training on how to deal with "such situations" and training in "inclusive teaching". Some of them suggested bursaries for teachers to study and enhance their knowledge. And they suggested/pleaded that any kind of support and every kind of support—e.g., from teachers, from parents, from the DBE would be helpful. They also mentioned the need for support from the social welfare department, from professional people, from remedial teachers, etc.

Principals and teachers generally considered that inclusive education was a factor leading to difficulties in covering the curriculum (which the majority had said they found difficult and with 73% in the Eastern Cape stating that they found it difficult to cover the curriculum). Of course many other factors were mentioned too in the "comment section" (such as that it was a new curriculum that they had too much paperwork and administration to handle, that they were loaded with tasks, that there was a shortage of teachers, etc.).

The section in the intervention guide on inclusive education tried to encourage teachers by creating a module dealing with forms of support that teachers could try to harness (Magano, Motitswe, & Tlale, 2014, pp. 23–26). It was suggested how teachers could get support from for example, other teachers, from learners (as mentioned in Sect. 3.3.1), from trying to improvise (with materials), from taking part in ongoing professional training programs, and from working with the School Based Support Team (SBST). It was also suggested that SBSTs could, where appropriate, "be strengthened by expertise from the local community, district support teams and higher education institutions", and should collaborate with community members and NGOs in the area (2014, p. 26).

I now offer some reflections on Dan Tlale's intervention visit to the school in the Eastern Cape (a rural area called Idutywa), in which he used this intervention handbook as a starting point for interaction with participants. Some of these reflections can also be found in Romm and Tlale (2016), as cited below.

3.4.6.1 Intervention in Idutywa: Inclusive Education

The account as expressed in this section is based on my and Dan's listening to the tapes of Dan's visit (June 2014) so that we could reflect together on their significance; and some reflections are based on our discussing further the responses that he received (also taped) when he phoned the principal, SGB chairperson and senior teacher six months after his visit in June, in order to hear about their possible progress. (He had also spoken to the principal three months after his initial visit in June, to hear about progress.) Dan and I discussed this all in preparation for an article that we created entitled *Nurturing research relationships: Showing care and catalyzing action in a South African school research-and-intervention project* (2016). Below I offer some highlights and further deliberations (in the light of the subject of this book).

In the discussion I highlight how Dan can be said to have admitted into the research and intervention process what Magnat calls "spirit"—which she suggests "most scholars refuse to admit exists" (2014, p. 249). She states that when "spirit" infuses one's "scholarship" it offers routes to "insight, steadiness and interconnection". In their argument for integration of spirituality in the academy, Wane, Ilmi, and Akena state that spirituality (no matter what definition of it is used) has the potential to encourage notions of self-cultivation, self-reflection, community building, and being part of a collective" (2014, p. 238).

Developing "Interconnection" in the Research Space Dan's visit to the school in Idutywa in June 2014 was the first time that he had been to this school, due to the project design in which many different staff in the College were deployed to facilitate FG sessions, do "member checking" and arrange subsequent "intervention". Although this was the third time now that staff from Unisa visited this school it was Dan's first time. Present in the meeting room on this occasion were the principal, the members of the SMT, various teachers, members of the SGB (including the chairperson) and the district officer for that District. The project leader—Soane Mohapi—had suggested to provincial team leaders (from the College) for the intervention visits that they should arrange with the schools to invite "outside" stakeholders to join the intervention visits—and in this case the principal invited the district officer.

The principal began the session by welcoming Dan (as well as another team member, Cheryl Ferreira who as part of the 2-day visit would assist the group in considering ways of helping students struggling with English as a first additional language). As seen from the extract below, straight away after being welcomed Dan referred to the continuing of the relationship that had been established by the other staff on the project in previous visits:

> **Principal of School**: You are welcome to participate in this beautiful project. I will not take over from what you are going to ask from us. But this is to say you are welcome.
>
> **Dan**: Thank you very much ladies and gentlemen. We are from the 500 Schools Project which is aimed at increasing performance in our schools. This is the third time that we are visiting this school. We would like to establish a relationship. When you are in a relationship you have to make sure that you have to come now and again.

Here Dan indicated that he does not want the participants to feel that having participated in the questionnaire and FG phases, they are now being "abandoned". He furthermore noted that Unisa had constructed a handbook to aid the participants in challenges with which they are dealing. The chairperson of the SGB asked Dan how the school should make use of this handbook and Dan replied: "This should be your guiding question: Does it suit me or not? If something in there does not suit you, then leave it out". Here Dan indicated (by implication) that his relationship with participants was not meant to be a hierarchical and authoritative one based on "scholarship", but one which they should experience as suiting them in their contexts (Romm & Tlale, 2016, pp. 23–24).

Dan then mentioned that he would start the day by offering some of his views on the principles underlying inclusive education in South Africa. Thereafter he suggested that the participants should sit in groups to discuss and make notes of what they think are issues that need to be further covered today. This would be followed by a "plenary session" in which they could all participate (Romm & Tlale, 2016, p. 24).

Presentation Intended to be Inspirational As had been agreed by all those involved in intervention visits for inclusive education in various provinces, Dan began the presentation with a quotation from Ginnoth in his well-known book *Teacher and child*:

> I [as a teacher] … possess a tremendous power to make a child's life miserable or joyous. I can humiliate or humour, hurt or heal. In all situations it's my response that influences whether a crisis will be escalated or de-escalated, a child humanized or dehumanized (Ginnoth, 1975, as cited in Magano, Motitswe, & Tlale, 2014, p. 5)

Dan then said: "This is what we are talking about in inclusive education. If you are looking for money as a teacher, you are in the wrong business. It is important to have passion!" He went on to explain that inclusive education is about "being willing to include diversity without any hesitation. Inclusive education means reducing exclusion in education". He stressed that "inclusive education is not about addressing disability". It is not like this because "this places the problem with the learner who is seen as 'disabled'". But inclusive education "works from the social point of view, from the societal point of view". He continued: "In inclusive education we are moving away from saying a child has a problem and we say the system has a problem.….. If the child cannot see, the system must provide Braille services; if the child cannot walk, the system must provide a wheelchair, so that the child can be at the school".

He explained that there are various barriers to learning, but it is important to "make sure that you do not undermine the children". He also referred to various obstacles in children's lives such as being AIDS orphans. He stated, for example, that HIV and AIDS have made serious damages to our society, which makes it difficult at school for many children. On this and other bases, he stated that "we cannot have an inflexible curriculum, but we need one that can be adapted to suit a particular child". He referred in addition to various barriers such as psychological

problems, emotional difficulties, learning difficulties etc., that all imply a need for a differentiated curriculum. And he concluded that "it helps to know how learners *feel* and how to deal with them".[20] (All of these statements are recorded in Romm & Tlale, 2016, p. 24.)

Dan also discussed how issues to do with "discipline" or "behavior problems" often could be attributed to the social circumstances of the children, such as their being orphans, from child-headed households, from households with absent parents, etc. He suggested again that instead of locating the problem as "in" the child, it could be located in absent support structures. And he suggested that instead of taking a punishment approach to discipline, there was another way of approaching the question of how to motivate learners to behave (that is, to come to school, listen in class, do homework, etc.), namely, by explaining to them the importance of education for their lives.

When I later (during our reflections) asked Dan how he saw his function in making this presentation, he said that he was trying to make the various participants aware that "if you don't care for a child he may become a criminal and not [Nelson] Mandela because you never cared for the child". He tried to inspire them by saying that "in one of the kids you might miss Mandela". I asked him if his way of speaking about inclusive education reflected an African systemic worldview and he said that the point of inclusive education (as in the White Paper 6) is that you must "adjust the system to accommodate the child". He pointed out to me that this is consistent with the African saying that "it takes a village to raise a child": everyone is involved in raising a child. He explained further to me that this African perspective would be present in this village, which was a traditional Xhosa village. He remarked: "I capitalized on that to these people and said that everyone must be involved". He also said to me that people (the participants) seemed to be demoralized and he wanted to breathe life into them. After the whole day, he could see that they were "full of life". This to Dan is a testimony to the importance of his showing care as a route to "moralizing" people (in Dan's terms).

Group Work and Feedback After Dan's presentation, as planned, various groups (teachers, SMT, SGB with the district officer too) met to have a discussion around the challenges facing them and to report to a plenary session. Feedback from the teachers (as presented by one of them during the "plenary") is indicative of how they had engaged with Dan's presentation. The teacher reported as follows:

> We must work as a team: when we lack something [as an individual] we must encourage each other to work together as a team how to solve the lack…. We also discussed in our group how inclusive education is important and we discussed how to appreciate the context

[20]In effect, as McKay notes (pers. comm., 10 March 2017) Dan was pointing to ways of actioning aspects of the government's White Paper 6 (and its spirit of inclusivity). This could also be considered as what Liamputtong (2010, p. 81) calls sharing of information which is "useful to the participants" as a way of interacting with participants, so that they can in turn find beneficial the relational encounter with the (professional) researchers.

> of the misbehavior of some of the kids when they are staying alone and are orphans. We also discussed that we need to encourage the kids to learn not by saying "if you don't study you will go into punishment" but by explaining to them the value of learning for them and for their future. (Teacher, as cited in Romm & Tlale, 2016, p. 25.)

The SGB chairperson also provided feedback indicating that they had experienced Dan's presentation as inspirational. The following extract offers a gist of his view (expressed on behalf of the SGB members):

> Thank you so much! Education is a societal matter. … . We hope to take the school forward to make education a priority. I believe as a parent that team work is important. This is not just about the teacher or the learner but it is about making sure we all amalgamate our efforts to deal with challenges. (SGB chair, as cited in Romm & Tlale, 2016, p. 25)

This reporting offers a glimpse of how participants expressed that they felt inspired to "take the school forward" in whatever way they can, also by working in teams. The idea of team work and of harnessing more people to work on the teams was taken further up by Dan with all the participants after the next speaker (the district officer) presented her feedback. The district officer was very explicit that she felt that she herself had become changed as a result of the inspiring session. In her words: "Some of the things that cropped up here I did not know. It is an eye opener. It is good to be here. I am not the person I was before I came here" (district officer, as cited in Romm & Tlale, 2016, p. 26).

The participants (with Dan as a participant too) then went on to discuss possibilities for creating a school based support team (SBST), which they did not yet have in place (although they had an SMT). It was decided that an SBST committee could be formed to consist of teachers, SMT members, volunteers and other significant people like a social worker, the police (especially from a child care/protection unit), a nurse, local business and non-governmental organizations. They could also harness support from the district officer, who had expressed her willingness to help.

Follow up Telephonic Calls I now move to the follow-up phone calls that Dan made 6–8 months later (December 2014 and February 2015), which were taped (with the agreement of the participants of course). Dan and I discussed the kind of questions that he could ask, and we also agreed that at the end of the call, as in our previous research in the international project, we would ask participants how they had experienced the interaction. Again, due to space limitations I will provide only some snippets. (Some of these can be found in Romm & Tlale, 2016.)

Snippets of Conversation with the Principal (December 2014)

Dan (after some initial introductions, in which he also asked about taping the conversation): How did you experience the whole process of our intervention?

Principal: Your intervention was very good.

Dan: Regarding the orphans whom you mentioned during our visit are often staying alone, did you maybe try to call the guardians or do some follow up?

Principal: Since your visit we have a project called CSLT for learner support.

Dan: What does CSLT mean?

Principal: It means care and support for teachers and learners—it is a program by the Department of Education: not all schools are involved but we are—an agent comes to the school to try to assess problems that learners are facing and it is helping us very much.

Dan inquired about what had led to this agent becoming involved in their school and the principal explained that it was due to help that had been sought by the SGB. (See the relevant snippets of conversation with the SGB chairperson which I present later.)

Regarding the formation of an Institutional Level Support Team (ILST), Dan asked:

Dan: Remember we spoke during my visit about the formation of an SBST [School Based Support Team] … has anything happened since then?

Principal: With respectRespect to the SBST we have established a committee—with the chairperson of the SGB taking the lead in this.

Dan: Do you have a teacher also on this team?

Principal: Yes, we do have an SMT person as part of the team.

Dan I am very pleased to hear about this.

Principal: We are very happy that you came here and we are not relaxed on our side: we are trying to make things work.

Dan: Yes, now that you have an SBST this should help to make things operate more smoothly.

Principal: Thank you that you saw it important to phone me. I felt blessed because I was not expecting this.

Snippets of Conversation with SGB Chairperson (February 2015)

The conversation with the SGB chairperson took place in February 2015 (arranged via phone calls in December 2014.) Soon after the conversation began the SGB person mentioned to Dan that:

SGB: Last year after you left we created a team like you said consisting of a person from social development [social worker], the school principal, the SGB, some teachers and a learner. Interventions have already been made, especially for learners coming from poor families. We managed to make arrangements with the Department of Social Development and the Department of Education. So this social worker comes to assess the children with problems. She is deployed by the Department of Education and comes to hold sessions with learners in the afternoons to check how they are dealing with their personal situations at home and at the school. On top of that we filed a proposal to Telkom who will offer assistance with a laboratory for e-learning. Because we wrote letters to sponsors and others to assist us, we are closing gaps.

The SGB chairperson proceeded to speak about current teacher-learner relationships:

SGB: After you left you left us a huge food for thought and we realized how the school depends on us to develop. Your critiques were constructive and your advice has also motivated the teachers that it starts with them. Everyone has become awakened that it starts from here; and that is what you did; and we are proud and hoping you could come back again. We would appreciate that.

Dan: I am happy to hear that; this makes us motivated too. Our main aim [as the research-and intervention team] is to show you how you can fish—not to give handouts—and you are fishing now for the future of your kids.

Toward the end of the call Dan solicited the SGB person's experience of the phone call by asking:

Dan: How did you experience this phone call?

SGB: It was very conducive and it shows that you care. It shows that you did not plough and then leave the plants unattended. You sprinkled it [what you ploughed] and irrigated it; you nurtured it with your care-giving. It is now growing what you ploughed.

Snippets of Conversation with Senior Teacher (February 2015)

The conversation with the teacher proceeded along similar lines, with Dan asking what the teacher considered had occurred since the intervention visit and to what extent the involvement of Unisa was helpful.

Dan: Firstly, I would like to know, is there anything that you learned from our visit last year?

Teacher: Firstly, I remember that you spoke about the misbehavior of kids because of their staying alone and being orphans. You also suggested that we could form a School-Based-Support Team (SBST).

Dan: Has anything happened in regard to that and also in regard to your handling of discipline issues in the school?[21] How are the kids now?

Teacher: We have tried by all means to encourage the learners to do the work they are given; we tried to encourage them and since you came we have assembled them and reminded them that you were here. We told them that we are one of the schools targeted and we want to make sure we are on par with what is required because we were visited by the university. We explained to them that Unisa is hopeful that we will reach another tier. So we explained to them the importance of education.

After some further conversation in which the teacher mentioned the (positive) outcome of all the letter writing to the DBE, the Provincial Department and the Department of Social Development, Dan moved to closure as follows:

Dan: Just to close, how did you find this conversation between us? How did you experience it? I could not wait for when we visit next so I decided to phone to ask how you are doing.

Teacher: I find it very interesting. This is at least a memory of us talking face to face. I am on my side and you are on your side of the cable, but we are here.

Here the teacher expressed that he experienced the call as a continuation of the relationship/connection and, as the teacher commented, it was as if Dan had been there like in the face to face visit.

[21]Dan here was referring to the idea that discipline could have been associated with punishment and sometimes corporal punishment—his statement was a reference to another way of seeing discipline that he had tried to suggest during the intervention visit; and the teachers had indicated that "we need to encourage the kids to learn not by saying 'if you don't study you will go into punishment".

During Dan's and my reflections on all these expressions from participants, Dan remarked to me that it was crucial that he had helped them to appreciate the importance of setting up the School Based Support Team (SBST). As he said, "if they had not set up the team, they would not have searched for help: they developed an organizing committee and roped in the relevant people". He commented that "6 months down the line now things are starting to happen". (In Chilisa's terms, 2012, p. 174, they were bringing to the fore, in action, a non-deficit view of how "assets" in the community could be harnessed to develop a better quality of life for participants, with special attention to those most vulnerable—in this case the learners.)

What I consider particularly noteworthy is that the SGB chairperson saw a connection between Dan's somewhat challenging stance (which he indeed called critical) and his sense of caring. In other words, he lauded Dan's offering what the SGB chairperson considered as "critiques" and Dan's advice to teachers to "recognize that it [changing the system] starts with them". He felt that this amounted to constructive criticism which inspired action. Dan reflected on this (during our discussions) by noting that he had highlighted that if kids are seen as "misbehaving", there could be a reason for this "whole set of behavior of the child"—and that teachers needed to consider this seriously. It could be, for example that the children were staying alone, or that there was no parent figure—therefore one needed to speak to officers such as social workers, rather than seeing that the child is naughty. In this way Dan challenged the teachers to reconsider their ways of relating to the children.

This critical/challenging yet supportive approach in working with the participants (as used by Dan and appreciated by the SGB chairperson) is consistent with Mertens' (2012a, p. 807) favorable citing of Dillard's account of an "endarkened feminist epistemology". Mertens notes that Dillard (2000) offers a research paradigm in which it is understood that researchers have "a role as a *supportive, reflective activist who works [with others] to challenge the status quo*". In Dillard's terms, the researcher who brings a spiritual focus into his/her work does not try to distance him/herself from the arena of encouraging transformative action. Transformative researchers advocate for research to be empowering to the study community. In essence, research should not and must not simply lead to a publication of books, journal articles, or a thesis but should leave the community improved socially, economically and/or in all aspects including "spiritual, physical, emotional and psychological welling for the betterment of the collective in society"—as critical reader Francis Akena (pers. comm., July 2016), put it to me. (See also Chap. 1, Sect. 1.3.5.)

It should be noted in conclusion (as a possible limitation that needs to be considered and taken up in further research) that Dan and participants did not as such discuss in detail the heterogeneity in the community of students with disabilities. They did not, for example, discuss what might need to be done to assist a student with a learning disability, which is, of course, different from working with a student who is, say, hard of hearing. These details were not given attention, other than to set up the SBST whose remit was to consider individual learners identified

as needing attention and to hold meetings to consider options for action. Dan later explained to me that he was concerned that often students become "referred" to special schools (and paper work is filled in toward this end), which goes against the grain of trying as far as possible to accommodate learners in mainstream schools by, if necessary, locating support from the DBST and/or by teachers' pursuing specialized training. In the case of this school, currently the school could accommodate learners who were hard of hearing as this barrier was not "severe"—and the teachers found that seating such students in the front of the class was a solution. Likewise learning difficulties as experienced by learners were not regarded as needing referral. Dan iterated to me that inclusive education as he presented the spirit of it to the participants is based on the idea that learners with barriers to learning should ideally not be segregated by being placed in "special schools". He felt that the White Paper 6 policy embraces the fact that a learner with disabilities (as long as these are not "severe") may benefit from being in a general education classroom, both academically and socially (see also Tlale & Romm, 2017). What I have tried to focus on in this chapter in addition is that, especially in the context of South Africa with its apartheid past, the focus of inclusive education initiatives, as Dan also stressed, goes beyond "ability" and "disability" and embraces a concern with a wide range of social/systemic factors that constitute barriers to learning.

3.5 Summary of Transformative Research Intentions

In this chapter I pointed, inter alia, to some possibilities for employing questionnaires in a novel way so that their use is not necessarily "top down". I suggested that they can be (re)tuned to take into account some of the ethical issues that I raised in Chap. 1, Sect. 1.3.3, in relation to my discussion of transformative research intent.

With reference to the international project on inclusive education, I illustrated how one can in practice adopt a stance toward research findings that recognizes that they require further working upon (by various constituencies, in terms of "sanity checks" and imaginative inferencing around the meaning of the results) rather than treating them as objective or close to objective accounts of realities (as in traditional understandings of positivism/postpositivism). I also suggested that in the follow up FG sessions and the meeting with district officers and the Head Office, the "attitudes" of teachers need not be treated as stable feelings/views, but can be treated as malleable and indeed as *being formed through the way in which people interact with others in the social arena (including the researchers).* This abides by the view of Cannella and Manuelito (2008, p. 55) that I explained in Chap. 1, that reality should be treated as being in the *process of becoming*. We sought to provide for this in the international project by trying to *generate a climate conducive to "positive" attitudes toward inclusive education* (in the FG sessions and further meeting with participants).

With respect to the national project, I indicated that already in the construction of the questionnaire, we tried to take care to break with deficit perceptions of learners' emotional, psychological and social wellbeing, so that our questionnaire construction would not reinforce an "individualized" view of the location of problems, but would incorporate a relational and systemic perspective. This was a way of taking into account, and highlighting further, the African traditional view that "it takes a village to raise a child", so that solutions too could be geared accordingly. This meant that we took some responsibility for our framing of the questions so that respondents/participants responding to our questions, and also other audiences, would think about the web of connections in which they too are part of "making schools better". In addition, we continued to develop the transformative agenda through our FG discussions which were geared to looking jointly for solutions to issues of concern, and through the intervention handbook and intervention visits. In the construction of our report, the draft report that was discussed with researchers from the DBE (from the directorate of strategic planning and research co-ordination) contained information that was largely descriptive; however, McKay was also requested to "hand over" the descriptive data from the project so that the DBE in conjunction with a government Program to Support Pro-poor Policy Development (PSPPD) could synthesize and develop further statistical correlations as well as qualitative syntheses arising from various researchers' research inputs as part of a larger study on poverty.[22] (McKay's relationship with the DBE from the start of the project, and ongoingly, was crucial here—see also my account hereof in Romm, 2017). But meanwhile, we also tried to serve those whom we considered as being more immediate beneficiaries, via our intervention handbook and our intervention visits (to those who had spent extensive time with us in our earlier FG visits).

Readers may ask the question whether my discussion of the national research-and-intervention community engagement project is a fitting example of how research might responsibly be undertaken in relation to communities. They may ask whether the explicit intervention component is a *separate component* that indeed is fitting only to "community engaged research". My answer to this is that any researchers recognizing a responsibility (with others) for the impact that the research endeavors necessarily make in the social world, *should in any case consider, with others, opportunities for transformative action arising from involvement in the research.* My philosophical starting point as explained in Chap. 1, is that *research always makes an intervention* (which cannot be avoided); and it is a *matter of degree* how much and what type of conscious intervention ensues. In Chap. 8, I offer more deliberations on the links between "active" and "community engaged" research in terms of ethical considerations around researcher responsibilities.

[22]The data set from the 500 schools project was requested by the program manager of the Program to Support Pro-poor Policy Development, which is a research and capacity building program in the Presidency.

As far as use of questionnaires in an active way is concerned, I tried to show in this chapter (via the national example) that already in the construction of the questionnaire items, care can be taken to *consider the likely impact of the way of framing questions on the participants and on wider audiences reading about the research*. Hence we tried to consider carefully how not to reify "race" in people's consciousness (while admitting an apartheid legacy) and also not to introduce language that might unduly individualize (and de-prioritze) the social aspects of learner "underperformances".

In terms of generative theorizing (which looks forward to new futures), in this chapter I followed through the argument that I offered in Chap. 2 regarding the treatment of social structure as "slow process" (Troncale, 2004–2007), with research being part of the process of either reifying unnecessarily and hence reinforcing the patterning of social life (and people's life chances) or of desolidifying (rendering more fluid) the structures that restrict life chances. I used this chapter to illustrate how research can consciously be geared toward generating a more inclusive education system, which treats barriers to learning as systemic features for which actors (including collective actors) can nevertheless bear some responsibility. In this case I focused on barriers that might exclude people, such as poverty, resource-constrained schools, etc. As pointed out by Ndimande (2012, p. 224), such barriers are still racialized in South Africa, with some previously designated apartheid "races" still bearing the brunt of apartheid-based discrimination (as recognized also in White Paper 6). I offered an option for how research can contribute to trying to shift such dynamics, which can indeed be seen as dynamic so that there is room for "action" on the part of actors, who together can locate leverage points for change. This was highlighted, for instance, in Dan's engagement with participants during his "intervention visit" in the 500 schools project, where the participants indicated (through later feedback) that they widened their perceptions of how they could work within their fields of influence to capitalize on opportunities for action.

I did not concentrate in this chapter on exploring gender as one of the intersecting bases of social discrimination, as this was not the research focus in the two examples to which I refer. (The data that we sought on the gender of teachers and principals in the national project, showed that most principals were men, which points to inequalities along gender lines. But I did not concentrate on this in this chapter.) As I explained in Chap. 1, primarily with reference to Midgley (2000), any research project has to draw some boundaries, and what is important is that these are recognized, while trying to be as "comprehensive" as possible in the space of a project. But this of course does not preclude addressing "excluded" (or less foregrounded) issues in further projects, as undertaken by oneself or others. In the next chapter I focus more on how gender issues specifically can be approached in an active way. But first, before I close this chapter, I offer some additional post facto reflections with reference to the following (somewhat contentious) issues in the research literature.

3.6 Conclusion: Additional Post Facto Reflections

3.6.1 Questionnaires as Tools for Enabling Processes of Re-Reflecting upon Constructions

I have suggested in this chapter that instead of being (unduly) distrustful of questionnaires as "top down" research instruments, it is possible to *treat questionnaires as creating possibilities for people's understandings to become enriched* through their engagement, inter alia, with the questionnaire items (as I also explored in Romm, 2013). I have suggested that responsibly handled, questionnaire construction can enable transformative-oriented discourses to become encouraged in that it can facilitate participants' renewed reflection on what might be called constrictive discourses. As I indicated in Sect. 3.4.3, some participants in the national project (where we elicited feedback) indicated that they found the questionnaire to be a learning exercise; but even if they do not see it this way, the manner in which questions are designed will (arguably) not leave participants unchanged. I suggested that the questionnaire development in the national project took into account the (Indigenous) view of people existing "in relation", so that questions were not focused on "self-efficacy" so much as on *ways of functioning in a systemic whole*. In this way the team designing the questionnaire items tried to take some responsibility for the framings of items so that these would be more likely to encourage systemic thinking, rather than encouraging those filling in the questionnaire and readers of reports to conceptualize issues in terms of isolated categories/variables, isolated from social contexts of application. We thus tried to inspire a more relational/holistic mode of theorizing than might otherwise be encouraged in questionnaire construction (with its isolation of variables), with a view to *rendering visions of, and actions in, "reality" more relational* (through people's continued way of engaging with the issues identified).

3.6.2 Enabling, but not Imposing, Processes of Re-Reflecting upon "Everyday" Theorizing

With reference to the two projects discussed in this chapter, I have pointed out how the FGs were treated, alongside questionnaires, as opening research spaces for people to redevelop constructions and to re-reflect upon "everyday" (more or less unreflected-upon) theorizing which informs their practices. Nonetheless, Lincoln and Guba (2013, p. 68) warn that attempts by "academic" researchers to stimulate deconstruction of hitherto less reflected-upon constructions in the social fabric, can all too easily become elitism, thereby undermining what they call "the negotiation process". They define deconstruction as aimed at

> disassembling the discourse structures, reifications, myths, metaphors, artifacts and practices found in a construction in order to lay bare the assumptions which undergird its production, employment and deployment. (2013, p. 68)

And they emphasize that the aim of what they call "hermeneutic dialectic inquiry" is ideally to set up a

> series of encounters between and among inquirers and … inquiry participants, focusing on their initial constructions and aimed at developing reconstructions by a process in which they all share equally. (2013, p. 65)

Here they echo the concerns of Indigenous-oriented methodologists to undercut colonial (undemocratic) processes of knowledge production and to shift the social relations of research production (cf. Adshead & Dubula, 2016; Chilisa, 2012; Lavia & Mahlomaholo, 2012; Mihesuah & Wilson, 2004; Wilson, 2008; Smith, 1999). How then might it be said that we tried to effect research relations which took into account such a decolonizing agenda through the way the "methods" became used in the projects discussed above?

In both the international and national projects admittedly the (university-based) researchers exercised a privilege of handling the main framing of the questionnaire items before these were administered. We tried to shift this approach somewhat in the second phase of the international research (via FG discussion in which participants were involved also in defining questions that they found important to raise and discuss). We furthermore tried to de-objectify the "results" that had emerged from the questionnaire by considering, with participants, if the results resonated with their experience as discussed during the FGs and the subsequent meeting held at Unisa. And we were sensitive to doing "relational work": between the three researchers (Norma, Dan and myself), between ourselves and the FG participants, and between ourselves and the wider stakeholders (who became participants via our "active sampling"). In both projects we sought feedback from participants as to how they were seeing the research. We also tried to establish a community of people concerned with inclusive education—drawing on our various cultural and professional backgrounds.

In the national project, many of the questionnaire items were co-constructed with researchers from the DBE, and in this sense they were not a product of only academic researcher "interests". Furthermore, the many open-ended questions offered scope for respondents/participants to express perspectives in relation to the questions that were posed (which were also meant to encourage their reflection on various issues). Regarding the setting up of the FG questions and suggested style of facilitation, the 46 Unisa staff (some White and mostly Black researchers in terms of raced categories) also did relational work such as in our workshop where we considered how we would handle the FG sessions. The full-day workshop offered a chance for the researchers to gain some understanding of how other researchers were viewing the project and its purpose. And we also at the same time considered how we would handle the relationships with participants so that they would be encouraged to feel that they were indeed participating in co-exploring (between

themselves and with the Unisa researchers) issues of concern to them, and so that they would (hopefully) feel that a relationship of reciprocity was being established.

The way I have written up the research offers an example of the positive feedback that some of us received from the participants. I chose the example of Dan's intervention visit by way of illustrating "what is possible" in terms of working with research participants and at the same time inspiring their action. Even though Dan was quite challenging in his way of addressing the participants, they interpreted this as inspiring rather than as debilitating, because he inspired their hope that constructive action was feasible and could "make their school better". The location with participants of leverage points for action was considered as indeed empowering for them and also for the benefit of learners, which is why Dan called it "learning to fish now for the future of your kids" when he summarized the stance that the SGB chairperson had expressed in the telephonic feedback (Romm & Tlale, 2016, p. 29).

3.6.3 Crediting Otherwise Marginalized Views

In terms of the questionnaire construction in the international project (using the TEIP and SACIE scales) it is possible that the research results, unless treated circumspectly, might have inscribed and reproduced a view of people as needing to feel *independently self-efficacious* (on various dimensions) in their teaching roles in order to feel comfortable with inclusive education. This is somewhat different from adopting a view of people (and of their attitudes) as *inextricably relational*. The use of the questionnaire items in the *international* project (with very little modification for context) could thus become a way of discrediting, say, the African view that "it takes a village to raise and child" (as Dan feels is prevalent in South Africa, especially in certain rural areas). In our way of speaking to participants in the FG sessions in both projects and in our whole way of handling the national project, we tried to make provision for *crediting Indigenous systemic worldviews*; and in the write-up in this chapter I too am drawing attention to this, as a counterpoint (or counter-narrative) to less relational ontologies and epistemologies.

With reference to the comparative work in the international arena, Malinen et al. admit that in the inclusive educational arena, "the number of cross-cultural investigations of teacher self-efficacy is currently very limited" (2013, p. 36). And they further admit that "teacher self-efficacy is context dependent, and it is likely that different educational systems also pose dissimilar requirements for the work of teachers" (p. 36) But they do not hereby draw the conclusion that in cross-cultural investigation the scales that are used to define and measure such efficacy may need radical alteration. For instance, it is possible that even in China, some alteration of

the TEIP scale would have been in order, so as not to reproduce Western-oriented approaches to teacher self-efficacy with their focus on individual mastery.[23]

Does this mean that I am hereby in my write-up expressing an allegiance to more non-Western (that is, more relational) views? The answer to this is that I do see myself as more aligned to the project of furthering a relational social existence; and I do wish to undercut the dominance of narratives that affirm "individualism". But as Molefe notes, ways of creating distinctions need not be seen as "either/or" options (2014, p. 98). For example, it can be argued that people become more "self-efficacious" to the extent that they bring in more people into their arena for action (as, for example, expressed by Magano, Motitswe, and Tlale when they speak of organizing peer learning, 2014, p. 24). People can be both "personally efficacious" and "inextricably relational" in their outlook. But the point of my critique of the international questionnaire construction as it stood (and still stands as a scale) is to render more visible a *relational aspect*, which otherwise might be overlooked (and indeed rendered marginalized in our research practices and discourses).

3.6.4 Engaging with Care as a Research Ethic: Should This Be Regarded as "Universally" Applicable?

The research examples as I have discussed them in this chapter can be seen as expressing a passion-full mode of researching (co-inquiring) in relation to concerns located and developed via the research. In this respect McLaughlin (2001) suggests that she has no qualms in treating such an (engaged and caring) orientation to research as being a "universal" which may, however, become practiced differently by different researchers in negotiation with research participants/co-researchers. Does this mean that I am forwarding passion-imbued research as opposed to the striving (which some authors still advise) for a more "scholarly" (and somewhat distanced) approach to research and theorizing? I would suggest, with Denzin (2001, p. 24), Gergen (2015, p. 291), and Kuntz (2015, p. 138) that the problem with an attempt at scholarly research devoid of passion and caring is that the *research still impacts on social life*, but in less recognized ways. The supposed distance hides the performative effect of all constructions/theorizing that becomes generated via the research process (2015, p. 291). Cram (2010) and Magnat (2012) favorably cite Gergen's forward-looking stance, where performativity (toward

[23]Although researchers urging the use of similar scales in comparative research suggest that changes in the scales (adapted to context) might make analytic comparisons more complex, one could suggest that this can still be built into the analyses, as the chosen measures can still be taken as contextually sensitive measures of "similar" constructs (which indeed are more meaningful in the contexts in question).

creating a better quality of life) is consciously embraced.[24] My suggestion in this regard, following the views of those upholding Indigenous research approaches, is that for research to be relevant and meaningful a human relationship with (key) participants needs to be engendered in a spirit of caring for a "better" future, which is itself up for discussion during the research. (I deliberate further upon this in later chapters and especially in Chaps. 8 and 9.)

3.6.5 Revisiting Correlations and Attributions of Causality

I have suggested in Sect. 3.3.1 that possibly the way of handling the correlational work (and attribution of causality) in the analyses supplied in the international project, could fail to do sufficient justice to relational thinking as advocated by many Indigenous authors. The acceptance that research can be geared to "testing" predictive models with independent and dependent variables isolated, is already somewhat exclusivist and based on Western-oriented notions of "science". Lincoln cautions that this can destroy "possibilities for community, except for that community dedicated to looking at the world in objectivist, abstract … and analytic modes" (1995, p. 281).

In terms of what could be said to be more systemic thinking (as discussed in Chap. 1, Sect. 1.3.1) some provision was made for "correcting" this via the FG discussions, and this required in addition treating the "abstract analysis" (based on isolating variables) more circumspectly than in conventional accounts of the value of "science" in offering information. In this way the participants/stakeholders could more readily be encouraged to participate also in redefining the import of any presented "results" rather than feeling excluded by virtue of the initiating researchers' (supposed) professional research status. This is the point also made by Acquah (2007) when he suggests that the results of survey research need to be subjected to community testing, to see if the results resonate with how various participants experience the causes of issues which they find problematic, and to further discuss "causes" via group discussions. (See also my account in Romm, 2013, p. 658). Again, as with McLaughlin's (2001) suggesting that she does not apologize for upholding these kinds of decolonizing research practices where professional researcher control (including analyses of patterns and attributions of causality) become minimized, I would concur with this—as I further elaborate in Chaps. 7 and 8.

[24]Cram (2010, p. 1) refers to Gergen as "an eminent American social psychologist" and indicates that the "Taos Institute, a North American non-profit organization headed by Kenneth Gergen, … is dedicated to the development of social constructivist theory and practices for the purpose of world benefit" (2010, p. 1). Magnat, in the course of discussing Wilson's (2008) book *Research a ceremony*, favorably cites Gergen's (2009) views on relational being (Magnat, 2012, p. 168).

3.6.6 Developing Emtic Relationships with a View to Action

As far as the "active" component of the research is concerned, I suggested in Chap. 1 that in active (transformative-directed) research one tries to gear the research process so that the research is more likely than not to contribute to leaving communities "better off" (as experienced). Mapotse (2012, p. 78) calls this moving from an etic to an increasingly emic position as researchers become involved with communities and their struggles.

Lincoln and Guba make the point that a somewhat etic position, where researchers may bring theoretical ideas (from outside the participant community) into the arena for discussion means that the etic/emic distinction is still a useful one, but ideally through the research process we "move from a we-they (inquirer-subject) polarity to an etic-emic fusion from which all parties [can] profit" (2013, p. 81). This could also be seen as activating what Onwuegbuzie (2012) calls an *emtic* position, where participation by involved parties in the (re)-construction of realities is encouraged, with an intention to pursue with participants and stakeholders social justice principles in practice (Onwuegbuzie & Frels, 2015, p. 162). (See my reference to Onwuegbuzie's term *emtic* in Chap. 2, Sect. 2.7.)

I tried to illustrate in this chapter how "follow-up" research processes (in these cases via FGs and other meetings subsequent to the administration of the questionnaire) could be geared to develop a community of people—including the research facilitators—concerned with advancing a more inclusive educational system. In Chap. 7, Sect. 7.4.1, I elaborate further on what this might mean in terms of conceptualizing research as a future-forming enterprise.

3.6.7 Some Reflections on the Status of My Storying in This Chapter

The purpose of my storying here is to provide a sense of how the research process can involve people-in-relation taking some responsibility for generating a more justice-infused quality of being, in this case in the educational arena. I am hoping via my admittedly evocative story to, as Ceglowski puts it, "evoke [emotive] responses from the reading audience" (1997, p. 194). Ceglowski notes that in creating evocative stories rather than trying to create "rational" statements that appeal to people on the level of thinking alone, the "distinction between fictional writing and social science writing is fuzzy". And she also notes, citing Denzin (1989) that fiction bears some relation to "truth" (widely defined) in that it is "fashioned out of something that was thought, imagined, acted out, or experienced" (1997, p. 194).

By referring to (my understanding of) some of our research experiences as reported in this chapter, I am fashioning a story that I hope will further inspire audiences to imagine how one can indeed take some responsibility (as part of a

collective responsibility) for using the research process to activate imagination and action. Of course, as indicated above, readers could argue that the intent to generate/catalyze action in the national research project was predicated on the fact that this was "officially" a community engagement project (classed as such within our university). But one could just as well argue that *all research should be undertaken in the spirit of inquirers taking some responsibility for energizing transformative action,* taking cues from the domain of what is called community engaged research, as I elaborate further in Chap. 8.

Finally, I have suggested in this chapter that even when researchers do *not* regard the research remit as including any concerns with facilitating intervention, this does not mean that they are avoiding intervening in the shaping of "realities". I have tried to show via this chapter that *just by virtue of posing questions in questionnaires and in interviews/FGs, for instance, some agenda is subtly introduced*. This is bound to have what Lincoln and Guba (2013, p. 65) call a social Heisenberg effect (see also Chap. 1, Sect. 1.1.2). For instance, had we posed a question in the questionnaire in the national project in a manner which supports an individualist interpretation of "efficacy", this would surely re-inscribe this conception in the minds of (many) teachers and principals taking part in filling in the questionnaire, and in those reading the reports; or had we asked teachers during FG discussion how they ensure that "disciplinary" processes are followed in their schools, this would lead them to believe that this is largely what is at stake, rather than, say, inspiring learners via their teaching practices and their stance. This too would not be without consequence in the social fabric.

But aside from being aware as an "active" researcher of one's potential influence, I have suggested that one can and should also *seek opportunities to engender dialogue* around "received" notions/conceptions, where possible, as well as seek opportunities to *catalyze action which is sensitive to social justice concerns*. I have highlighted this by extrapolating the manner in which such ways of approaching the research as reported upon in this chapter could be seen as having been implied (and now drawn out/rendered more explicit for the purposes of this chapter).

In this chapter, as in Chap. 2, I admittedly did not touch on ecological justice issues. Nevertheless, it is worth noting that McKay continued to work on leading teams in constructing workbooks for a national DBE project (which was underway previously to her liaising with the DBE around the setting up of the 500 schools project). Further to the 500 schools project, alongside continued work on the upgrading of the workbooks, she was involved in developing readers for children (again across 11 languages), taking into account some of the problematic issues that had been raised by participants in the 500 schools project. These workbooks and readers included, inter alia, values such as ecological sensitivity in what McKay calls a "parallel curriculum" (2016, 2017). Questions of how research endeavors can contribute to ecological justice are given specific attention in my Chap. 6, with reference to a number of examples.

References

Acquah, B. K. (2007). Community involvement in social research in Botswana. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa: Ethical issues* (pp. 127–132). Addis Ababa, Ethiopia: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

Adshead, M., & Dubula, V. (2016). Walking the walk? Critical reflections from an Afro-Irish emancipatory research network. *Educational Action Research, 24*(1), 115–133.

Bandura, A. (1997). *Self-efficacy: The exercise of control*. New York, NY: Freeman.

Beverley, J. (2000). Testimonio, subalternity, and narrative authority. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (2nd ed., pp. 555–565). Thousand Oaks, CA: Sage.

Biraimah, K. L. (2016). Moving beyond a destructive past to a decolonized and inclusive future: The role of Ubuntu-style education in providing culturally relevant pedagogy for Namibia. *International Review of Education, 62*(1), 45–62.

Bishop, R. (2012). Indigenous methods in qualitative educational research. In S. Delamont & A. Jones (Eds.), *Handbook of qualitative research in education* (pp. 126–140). Northampton, England: Edward Elgar.

Bless, C., Higson-Smith, C., & Sithole, S. L. (2013). *Fundamentals of social research methods: An African perspective*. Cape Town: Juta.

Boyask, R. (2012). Sociology of education: Advancing relations between qualitative methodology and social theory. In S. Delamont & A. Jones (Eds.), *Handbook of qualitative research in education* (pp. 21–32). Northampton, England: Edward Elgar.

Cannella, G. S., & Manuelito, K. D. (2008). Feminisms from unthought locations: Indigenous worldviews, marginalized feminisms, and revisioning an anticolonial social science. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and Indigenous methodologies* (pp. 45–59). London: Sage.

Ceglowski, D. (1997). That's a good story, but is it really research? *Qualitative Inquiry, 3*(2), 188–201.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., & Preece, J. (2005). *Research methods for adult educators in Africa*. Cape Town: Pearson.

Cram, F. (2010). *Appreciative Inquiry. MAI Review, 3,* 1–13.

Denzin, N. K. (1989). *Interpretive interactionism*. Newbury Park, CA: Sage.

Denzin, N. K. (2001). The reflexive interview and a performative social science. *Qualitative Research, 1*(1), 23–46.

Department of Basic Education, South Africa. (2009). *Guidelines for full-service/inclusive schools*. Pretoria: DBE.

Department of Education, South Africa. (2001). *White Paper 6: Building an inclusive education and training system.* Pretoria: DoE.

Dick, B., Sankaran, S., Shaw, K., Kelly, J., Soar, J., Davies, A., et al. (2015). Value co-creation with stakeholders: Using action research as a metamethodology in a funded research project. *Project Management Journal, 46*(2), 36–46.

Dillard, C. B. (2000). The substance of things hoped for, the evidence of things not seen: Examining an endarkened feminist epistemology in educational research and leadership. *Qualitative Studies in Education, 13*(6), 661–681.

Engelbrecht, P. (2011). Equity in inclusive education in South Africa. In A. J. Artiles, E. Kozleski and F. Waitoller (Eds.), *Equity in inclusive education: Examining equity on five continents* (pp. 147–160). Cambridge, MA: Harvard Education Press.

Erasmus, Z. (2010). Contact theory: Too timid for "race" and racism. *Journal of Social Issues, 66*(2), 387–400.

Forlin, C., Cedillo, I. G., Romero-Contreras, S., Fletcher, T., & Rodriguez Hernández, H. J. (2010). Inclusion in Mexico: Ensuring supportive attitudes by newly graduated teachers. *International Journal of Inclusive Education, 14*(7), 723–739.

Frels, R. K., & Onwuegbuzie, A. J. (2012). Interviewing the interpretive researcher: An impressionist tale. *The Qualitative Report, 17*(30), 1–27.

Gboku, M., & Lekoko, R. N. (2007). *African perspectives on adult learning: Developing programs for adult learners in Africa*. Hamburg: UNESCO Institute for Lifelong Learning.

Gergen, K. J. (2009). *Relational being: Beyond self and community*. New York, NY: Oxford University Press.

Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.

Ginnoth, H. (1975). *Teacher and child*. New York, NY: Macmillan.

Goff, S. (2017). Statistical variation versus nation cohesion: Contesting truth tests in competing socio-ecological realities. In J. J. McIntyre-Mills, Y. Cocoran-Nantes, & N. R. A. Romm (Eds.), *Balancing individualism and collectivism: Social and environmental justice* (pp. 181–197). New York, NY: Springer.

Gonzalez, P. A., Minkler, M., Garcia, A. P., Gordon, M., Garzón, C., Palaniappan, M., et al. (2011). Community-based participatory research and policy advocacy to reduce diesel exposure in West Oakland, California. *American Journal of Public Health, 101*(S1), S166–S175.

Harry, B., & Klingner, J. (2006). *Why are so many minority students in special education?*. New York, NY: Teachers College Press.

Hay, J. F. (2003). Implementation of the inclusive education paradigm shift in South African education support services. *South African Journal of Education, 23*(2), 135–138.

Holstein, J. A., & Gubrium, J. F. (1995). *The active interview*. London: Sage.

Kaur, B., Boyask, R., Quinlivan, K., & McPhail, J. (2008). Searching for equity and social justice: Diverse learners in Aotearoa New Zealand. In G. Wan (Ed.), *The education of diverse student populations* (pp. 227–251). Dordrecht: Springer.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.

Kozleski, E. B., Artiles, A. J., Fletcher, T., & Engelbrecht, P. (2009). Understanding the dialectics of the local and the global in education for all: A comparative case study. *International Critical Childhood Policy Studies, 2*(1), 1–29.

Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.

Lavia, J., & Mahlomaholo, S. (2012). Introduction: Imagining the postcolonial. In J. Lavia & S. Mahlomaholo (Eds.), *Culture, education and community: Expressions of the postcolonial imagination* (pp. 1–13). New York, NY: Palgrave Macmillan.

Liamputtong, P. (2010). *Performing qualitative cross-cultural research*. Cambridge, England: Cambridge University Press.

Lincoln, Y. S. (1995). Emerging criteria for quality in qualitative and interpretive research. *Qualitative Inquiry, 1*(3), 275–289.

Lincoln, Y. S., & Guba, E. G. (1985). *Naturalistic inquiry*. Beverly Hills, CA: Sage.

Lincoln, Y. S., & Guba, E. G. (2003). Paradigmatic controversies, contradictions, and emerging confluences. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 253–291). Thousand Oaks, CA: Sage.

Lincoln, Y. S., & Guba, E. G. (2013a). *The constructivist credo*. Walnut Creek, CA: Left Coast Press.

Lincoln, Y. S., & Guba, E. G. (2013b). *The constructivist credo*. Walnut Creek, CA: Left Coast Press.

Loreman, T., Earle, C., Sharma, U., & Forlin, C. (2007). The development of an instrument for measuring pre-service teachers' sentiments, attitudes, and concerns about inclusive education. *International Journal of Special Education, 22*(1), 150–159.

Magano, M. D., Mohapi, S., & Robertson, D. (Eds.). (2017). *Re-aligning teacher training in the 21st Century*. Hampshire, England: Cengage.

Magano, M. D., Tlale, L. D. N., & Motitswe, M. J. (2014). Inclusive education. In S. J. Mohapi (Ed.), *Handbook for primary school teachers: 500 schools project* (pp. 3–32). Pretoria: University of South Africa.

Magnat, V. (2012). Performative approaches to interdisciplinary and cross cultural research. In L.-H. Smith & A. Narayan (Eds.), *Research beyond borders* (pp. 157–177). Plymouth, England: Lexington.

Magnat, V. (2014). Performance ethnography: Decolonizing research and pedagogy. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry outside the academy* (pp. 235–252). Walnut Creek, CA: Left Coast Press.

Majumdar, A. K., & Sowa, J. F. (2009). *Two paradigms are better than one, and multiple paradigms are even better*. Retrieved February 4, 2015, at: http://www.jfsowa.com/pubs/paradigm.pdf.

Malinen, O. P. (2013). *Inclusive education from teachers' perspective: Examining pre- and in-service teachers' self-efficacy and attitudes in mainland China*. Doctoral thesis, Ita-Suomen Yliopisto University of Eastern Finland Joensuu, Finland.

Malinen, O. P., Savolainen, H., Engelbrecht, P., Xu, J., Nel, M., Nel, N., et al. (2013). Exploring teacher self-efficacy for inclusive practices in three diverse countries. *Teaching and Teacher Education, 33,* 34–44.

Mapotse, T. A. (2012). *The teaching practice of senior phase technology education teachers in selected schools of Limpopo province: An action research study*. Doctoral thesis, University of South Africa, Pretoria.

Mapotse, T. A. (2015). *Processes in congruence to emancipate teachers: Technology education and action research.* Proceedings of the innovation challenges in multidisciplinary research and practice (ICMRP-2015) conference. Kuala Lumpur: Global Illuminators.

Mbuli, F., & Romm, N. R. A. (2015). Development of technological efficacy via an e-learning programme: South African parliament employees. *Participatory Educational Research, 2*(3), 1–11.

McKay, V. I. (2016). *Podcast on "the parallel curriculum"* (in conversation with J. J. McIntyre-Mills). Retrieved September 9, 2016, at: https://archive.org/details/veronica2016.

McKay, V. I. (2017). Introducing a parallel curriculum to enhance social and environmental awareness in South African school workbooks. In J. J. McIntyre-Mills, N. R. A. Romm, & Y. Corcoran-Nantes (Eds.), *Balancing individualism and collectivism: Social and environmental justice* (pp. 97–122). New York, NY: Springer.

McKay, V. I., Mohapi, S. J., & Romm, N. R. A. (2017). Rethinking school discipline. In M. Magano, S. Mohapi, & D. Robertson (Eds.), *Realigning teacher training in the 21st Century* (pp. 250–270). Hampshire: Cengage.

McKay, V. I., & Romm, N. R. A. (2008). Active research toward the addressal of HIV/AIDS in the informal economy in Zambia: Recognition of complicity in unfolding situations. *Action Research, 6*(2), 149–170.

McLaughlin, J. M. (2001). *Accommodating perceptions, searching for authenticity and decolonizing methodology: The case of the Australia/Papua New Guinea Secondary School Students' Project.* Paper presented at the 29th ANZCIES Conference, Curtin University of Technology, Perth, 6–8 December.

Mertens, D. M. (2012). Transformative mixed methods: Addressing inequities. *American Behavioral Scientist, 56*(6), 802–813.

Mertens, D. M., & McLaughlin, J. A. (2004). *Research and evaluation methods in special education*. Thousand Oaks, CA: Sage.

Midgley, G. (2000). *Systemic intervention: Philosophy, methodology and practice*. New York, NY: Kluwer Academic/Plenum.

Mihesuah, D. A., & Wilson, A. C. (2004). Introduction. In D. A. Mihesuah & A. C. Wilson (Eds.), *Indigenizing the academy: Transforming scholarship and empowering communities* (pp. 1–15). Nebraska, NE: University of Nebraska Press.

Miles, S., & Singal, N. (2010). The Education for All and inclusive education debate: Conflict, contradiction or opportunity? *International Journal of Inclusive Education, 14*(1), 1–15.

Molefe, M. (2014). Critical reflections on Gyekye's humanism: Defending supernaturalism. In E. Imafidon & J. A. I. Bewaji (Eds.), *Ontologized ethics: New essays in African meta-ethics* (pp. 121–131). Lanham, MD: Lexington.

Morrison, K. (2012). Searching for causality in the wrong places. *International Journal of Social Research Methodology, 15*(1), 15–30.

Motitswe, J., & Phala, T. A. L. (2017). Re-aligning the role of the school-based support team for addressing barriers to learning. In M. Magano, S. Mohapi, & D. Robertson (Eds.), *Realigning teacher training in the 21st Century* (pp. 149–161). Hampshire, England: Cengage Learning.

Mukherjee, N. (1995). *Participatory rural appraisal and questionnaire survey*. New Delhi: Concept.

Ndimande, B. S. (2012). Decolonizing research in post-apartheid South Africa. *Qualitative Inquiry, 18*(3), 215–226.

Nel, N. M. (2014). Applying a transformative-pragmatic paradigm to engender collaboration in educational processes. *International Journal on New Trends in Education and Their Implications, 5*(2), 220–229.

Nel, N. M., Romm, N. R. A., & Tlale, L. D. (2015). Reflections on focus group sessions regarding inclusive education: Reconsidering focus group research possibilities. *Australian Educational Researcher, 42*(1), 35–53.

Niculescu, G. (2014). New considerations about action research. *Annals of the Constantin Brâncuşi, University of Târgu Jiu, Letter and Social Science Series*, (4), 28–33.

Onwuegbuzie, A. J. (2012). Introduction: Putting the *mixed* back into quantitative and qualitative research in educational research and beyond: Moving toward the radical middle. *International Journal of Multiple Research Approaches, 6,* 192–219.

Onwuegbuzie, A. J., & Frels, R. K. (2015). A framework for conducting critical dialectical pluralist focus group discussions using mixed research techniques. *Journal of Educational Issues, 1*(2), 159–177.

Ossai, N. B. (2010). African Indigenous Knowledge Systems (AIKS). *Simbiosis, 7*(2), 1–13.

Paugh, P. C., & DudleyMarling, C. (2011). "Speaking" deficit into (or out of) existence: How language constrains classroom teachers' knowledge about instructing diverse learners. *International Journal of Inclusive Education, 15*(8), 819–833.

Ponti, M. (2012). Uncovering causality in narratives of collaboration: Actor-network theory and event structure analysis. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *13*(1), Art. 11.

Reed, K. (2000). Dealing with difference: Researching health beliefs and behaviors of British Asian mothers. *Sociological Research Online, 4*(4).

Romm, N. R. A. (2013). Employing questionnaires in terms of a constructivist epistemological stance: Reconsidering researchers' involvement in the unfolding of social life. *International Journal of Qualitative Methods, 12,* 652–669.

Romm, N. R. A. (2014). Exploration of transformative paradigm with pragmatic twist to contribute to educational change. *International Journal on New Trends in Education and their Implications, 5*(2), 134–144.

Romm, N. R. A. (2015). Conducting focus groups in terms of an appreciation of Indigenous ways of knowing: Some examples from South Africa. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *16*(1), Art. 2.

Romm, N. R. A. (2017). Reflections on a multi-layered intervention in the South African public education system: Some ethical implications for community operational research. *European Journal of Operational Research*. https://doi.org/10.1016/j.ejor.2017.02.022(pp.1-13).

Romm, N. R. A., Nel, N. M., & Tlale, L. D. N. (2013). Active facilitation of focus groups: Exploring the implementation of inclusive education with research participants. *South African Journal of Education*, *33*(4), 1–14, Art. 811.

Romm, N. R. A., & Tlale, L. D. N. (2016). Nurturing research relationships: Showing care and catalysing action in a South African school research-and-intervention project. *South African Review of Sociology, 47*(1), 18–36.

Savolainen, H., Engelbrecht, P., Alisauskiene S., Gaber, S., Nel, M., & Jiacheng, X. (2012a). *A comparative analysis of teachers' roles in inclusive education*. Paper presented (in roundtable discussion) at European Conference on Educational Research (ECER), held in Cádiz, Spain 17–18 September, http://www.eera-ecer.de/ecer-programmes/conference/6/contribution/16816/.

Savolainen, H., Engelbrecht, P., Nel, M., & Malinen, O. P. (2012b). Understanding teachers' attitudes and self-efficacy in inclusive education: Implications for pre-service and in-service teacher education. *European Journal of Special Needs Education, 27*(1), 51–68.

Sharma, U., Loreman, T., & Forlin, C. (2012). Measuring teacher efficacy to implement inclusive practices. *Journal of Research in Special Educational Needs, 12*(1), 12–21.

Shumbamhini, M. (2005). *Storying widowhood in Shona culture.* Master's dissertation, University of South Africa, Pretoria.

Smit, B., & Romm, N. R. A. (2012). *Focus group field guide: 500 hundred schools project*. Pretoria: University of South Africa.

Smith, L. T. (1999). *Decolonizing methodologies: Research and Indigenous peoples*. New York, NY: Zed Books.

Smith, L. T. (2012). *Decolonizing methodologies: Research and Indigenous peoples* (2nd ed.). New York, NY: Zed Books.

Sung-Chan, P., Yuen-Tsang, A., Yadama, G. N., & Sze, A. (2008). Action research and social development in China: Creating synergy between marginalized populations and government officials. *Action research, 6*(2), 193–212.

Swart, E., Engelbrecht, P., Eloff, I., Pettipher, R., & Oswald, M. (2004). Developing inclusive school communities: Voices of parents of children with disabilities. *Education as Change, 8*(1), 80–108.

Tchatchoueng, J. (2014). *Academic (institutional) support to first year students who have limited proficiency in English as language of learning and teaching at a South African university: Lecturers' experiences*. Master's dissertation, University of KwaZulu-Natal, Durban, South Africa.

Tlale, L. D. N., & Romm, N. R. A. (2017). Systemic thinking and practice toward facilitating inclusive education: Reflections on a case of co-generated knowledge and action in South Africa. *Systemic Practice and Action Research.* doi:10.1007/s11213-017-9437-4.

Troncale, L. (2004–2007). *Courses in systems science at the California State University at Pomona for students of Comparative Systems Analysis*. Unpublished lectures.

Wane, N. N., Ilmi, A. A., & Akena, F. A. (2014). Conclusion: An invitation to continuous dialogue on Indigenous spiritual ways of knowing. In N. N. Wane, F. A. Akena, & A. A. Ilmi (Eds.), *Spiritual discourse in the academy* (pp. 237–241). New York, NY: Peter Lang.

Wilson, S. (2008). *Research is ceremony: Indigenous research methods*. Halifax, Canada: Fernwood.

Young, J., McGrath, R., & Adams, C. (2015). Developing and refining a participatory educative model with Health Science students: A case study of practice. *Participatory Educational Research, 1,* 61–73.

Chapter 4
Actively Facilitating Individual and Focus Group Narrations: Responsibly Foregrounding Gender Stereotyping While Stimulating (Contextual) Empowerment

Abstract In this chapter, I look at two examples of research which bear on gender relations and which try to contribute to shifting stereotypical conceptions thereof (Woldegies in Economic empowerment through income generating activities and social mobilization: the case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia, 2014, Woldegies in South Afr Rev Sociol 47(1):58–80, 2016; Ssali & Theobald in South Afr Rev Sociol 47(1):81–98, 2016). In the first example, the research concerned women's empowerment via income-generating activities and social mobilization in a region of Ethiopia. In the second example, the research was around gendered experiences of the civil war in Northern Uganda and post-war reconstruction. I discuss in some detail Woldegies's account of research exploring (and trying to strengthen) women's experiences of empowerment. I draw out and extrapolate from his exposition the responsibilities he assumed in trying to make a constructive difference to the life-worlds of participants (in their communities) and to wider audiences via the research. I concentrate on how one might judge research in terms of considerations of the involvement of the research endeavor in the becoming of the "realities" being explored. I move on to discuss (in less detail) Ssali and Theobald's research—in this case, drawing out additional issues relating to the responsibilities of researchers, especially when conducting research asking people to recall and recount traumatic experiences.

4.1 Introduction

In this chapter I discuss, with a focus on researcher responsibilities, two examples of research which in different ways try to shift gender stereotyping and gendered forms of felt discrimination. As I examine the research examples, I offer my interpretations of the texts of the authors, deliberately drawing out aspects of researcher responsibility that I feel can be extrapolated from my reading of the texts. These are ultimately my extrapolations, but are based on some further personal communication with the authors too. The purpose of my exposition is to invite

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_4

readers of my text to consider the options for responsible research practice that I feel are illustrated—albeit sometimes only implicitly—in the examples.[1]

The first example I discuss in depth (Woldegies, 2014) is set in Ethiopia. The research focuses on considering women's empowerment via participation in income generating activities (IGAs) and in collective forms of social mobilization (SM). The roles that men sometimes play in supporting their partners and women more generally are also investigated and highlighted by Woldegies. I concentrate on the responsibilities that I consider Woldegies assumed in trying to make a constructive difference to the life-worlds of participants (in their communities) and to wider audiences.

The second example is set in Uganda (Ssali & Theobald, 2016). I discuss this research in less detail, as my purpose in the discussion is to draw out features of and decisions about responsible research not explored in the first example. Ssali and Theobald's research was directed at exploring (evidently traumatic) memories of both men and women regarding the 20-year old civil war starting in the late 1980s between the government's National Resistance Army (NRA) and the Lord's Resistance Army (LRA)—with a focus on possibilities for post-conflict health reconstruction in a region of Northern Uganda. The research points to the gendered experiences of the conflict, while suggesting avenues for post-conflict healing.

As I proceed in the chapter, I suggest that in their research write-ups (Woldegies, 2014, 2016; Ssali & Theobald, 2016), the researchers tend to downplay their sense of responsibility (as researchers) in being active in trying to make a difference to the fields they were co-exploring with participants. Through my reading of their work —and through some personal communication[2] I had with them—I believe that their conceptions of their research roles were not limited to conceptions of research as being a "finding out" exercise. However, this to me is not sufficiently detailed in their own write-ups. I suggest that this is probably because research roles in generating "knowledge" are still considered to be separate from concerns with avoiding stultifying restrictive patterns of thought and action and trying to shift these via the

[1]In any case, I do not believe that any interpretation of a text—or conversation—can refer to some "real" meaning, independently of the interpretive effort (cf. Romm, 1997, 1998). What I did was check with Belete Woldegies and with Sarah Ssali and Sally Theobald whether they wished to modify any of my interpretations of the work. Woldegies indicated in reply to my draft account that "I like the way you presented the research in your book entitled 'Responsible Research Practice'" (pers. comm. via email, 10 January 2017). Ssali, who replied on behalf of Theobald too (10 October 2016), clarified for the research ethics section that they had had counsellors on board in case this was needed, as required by their Institutional Review Board.

[2]I had been mentor to Woldegies for some parts of his studies under the leadership program at the University of Antioch in USA (under the direction of Philomena Essed, whose position on race and gender I pointed to in my narrative in Chap. 2, Sect. 2.2). Subsequent to his handing in his doctorate, I suggested to him that he could submit a paper toward a special issue of the *South African Review of Sociology,* of which I was guest editor (vol. 47, 1, 2016), and so we continued our virtual interactions/discussions around the value of his research. In the case of Ssali and Theobald, our communications began when they submitted (unsolicited) an article for the special issue of the *South African Review of Sociology*.

research process. I explain in this chapter how researchers can legitimately indicate, both to themselves and others, that they are taking some responsibility (with others) for the way in which the research might be impactful.

In both cases, the research was admitted by the researchers to be focused on "increasing knowledge" about people's lived worlds, with a view to making recommendations for action (e.g., to NGOs and government policy makers). In the first case, I suggest that Woldegies also bore in mind that during the research process itself, he needed to take into account that the way of framing the questions (in the interview and focus group context) would make a difference to the manner in which participants engaged with the issues. I discuss in detail how I believe Woldegies made provision for this in what I call a responsible way, which is not as yet sufficiently highlighted in his write-up (2014, 2016), but which can be drawn out.[3]

In the second case, the research write-up implies that the research could be empowering and transformative. I indicate how I see that the researchers made choices in this regard, especially given that it was research based on asking people to recount their (traumatic) experiences in their memories of their life history, with special reference to memories of the Northern Ugandan conflict and its aftermath. I point out (again with some extrapolations that I make based on their text) how Ssali and Theobald could be said to have linked their "research role" to an intent to take some responsibility for the social change directions that might ensue from the research process.[4]

I now proceed to examine the two research projects.

4.2 Economic Empowerment Through Income Generating Activities and Social Mobilization (Ethiopia)

This research, as explicated by Woldegies (2014[5]), was aimed at co-researching with participants (ten married rural women and their supporting family members) issues relating to experiences of women's empowerment through participation in IGAs and social mobilization (SM). The participants were sampled from a region of Ethiopia—Wadla Woreda. Woldegies indicates that Wadla Woreda is located in the

[3]This became better elucidated in the article he wrote toward a special edition of the *South African Review of Sociology* (vol. 47, 1, 2016), of which I was guest editor. See also fn 5.

[4]Ssali and I had some personal communication via email, sparked by her sending an abstract for her proposed paper for the special issue of the *South African Review of Sociology*, which I was guest editing. Subsequent interaction followed in which I suggested how she might direct the paper in order to make it suitable for the issue. Further to this, I interacted with her in regard to the reviews on the article that had been submitted by her and co-author Theobald. One of the reviewers asked the question, "how can life history interviewing be transformative?" This directed Ssali and Theobald to consider this question in more depth.

[5]A summary account, drawing out the implications for research directed toward social development, is provided in Woldegies (2016).

North Wollo Zone, Amhara Regional State, Ethiopia. It is "predominantly agrarian and the population produces mainly subsistence food crops with small amounts of cash crops" (2014, p. iii).[6] Woldegies was involved in this woreda and also headed (as founder CEO) an NGO aimed at nurturing women's participation in economic development—Nurture Education Development (NED)—which was set up in 1999.[7] At the time of the research (2014), which Woldegies was organizing toward obtaining his doctoral degree (in a leadership program), the women sampled for the research had already been part of a number of projects spearheaded by NED. NED had supported these women with a project entitled "Capacity building to improve the life quality of women" from 2010 to 2014. By 2014, the project had been completed/phased out, and the women were running their activities on their own strength, without any external support from NED (Woldegies, 2014, p. 94).

Woldegies indicated to the women whom he was requesting to participate in the research that the interviews that would be conducted were aimed at increasing knowledge of "economic empowerment through income generating activities (IGA) and social mobilization (SM)." He also explained this in a similar way to those who participated in the focus group discussions, namely the same women, this time with their supporting family members, that is, 25 of the latter (2014, p. 243). In Woldegies's recruitment of the various participants, and their agreement to participate, he can be said to have followed what Kovach calls a *relational* sampling approach, where sampling "is not just a matter of the researcher choosing participants" (2009, p. 126). She indicates that in doing research in Indigenous communities "the process is more reciprocal" and is based on the felt "trustworthiness of the researcher" (2009, p. 125).

Woldegies explained to the participants that as he foresaw it, the research would serve to

> provide additional information to policy makers, planners and practitioners to develop sound strategies that could be used to scale up women's level of economic empowerment and participation through income generating activities and social mobilization, by eliminating the existing barriers and resistances. (2014, p. 227)

In Woldegies's explanation to participants of the potential significance of the research, he was clearly displaying to them his own commitment to women's empowerment. But how did he try to organize the research with this commitment in mind? I explore this with detailed reference to the following issues:

---

[6]Wollo is one of the 11 zones in the Amhara Regional State and is situated in the Northeast of the region. The North Wollo Zone has 13 administrative Woredas, nine rural and four ketema woredas: Wadla, Dawnt, Meket, Bugina, Lasta, Gubalafto, Habiru, Giedan, and Kobo are the rural woredas; and the ketema administrations are Woldiya, Lalibela, Kobo, and Mersa (Woldegies, 2014, p. 32, citing Amhara Regional State, 2010).

[7]Nurture Education and Development (NED) is a not-for profit, non-partisan and indigenous non-governmental organization officially established in 1999 and re-registered by the Charities and Societies Agency on October 7, 2009, according to the New NGO law of Ethiopia (Woldegies, 2014, p. 16).

1. Responsible framing of questions in the interview guide
2. Inviting women and supporting family members to focus group discussions
3. Responsible approach to member checking
4. Ethics of the research
5. Responsible involvement in dissemination/public discussion
6. Validation and generative theorizing: Toward transformation.

4.2.1 *Responsible Framing of Questions in the Interview Guide*

The interviews with the women were organized according to an "interview guide". Woldegies indicates that although he followed a semi-structured interview format,

> nevertheless, it was still important that a degree of flexibility and spontaneity be retained so that interviewees had ample opportunity to lead the discussion. Consequently, the interview tools were not adhered to too rigidly; in this way room was made for unexpected insights to emerge. (2016, p. 66)

The first question of the guide consisted of asking the women "what is your story" about their activities prior to their involvement with NED and other similar organizations, and subsequent to this. This was followed by questions about issues such as

- their experiences in starting up their business;
- opportunities for them and others in the village for IGAs;
- who supported them from their family and community in carrying out the IGAs (and how often are/is the person(s) assisting them);
- whether they considered themselves to be successful entrepreneurs and if they could advise other women, what advice they would give;
- whether they felt empowered through their activities and if so, how and why;
- whether they felt they acted as a role model for other women;
- whether they had ever organized women's groups to change themselves and to give a voice for themselves and others;
- whether they had developed assertive behavior in dealing with and resisting male domination and other social barriers;
- whether they had developed social skills to effect change for their family and the community;
- whether they had sent their daughter(s) (if any) to school;
- what their relationship had been with their husband before and after NED/similar NGO's
- project engagement;
- what their level of engagement had been in mobilizing the community to have a louder voice for the protection and fulfillment of the rights of women;

- whether they could cite sayings (proverbs) in their community that encourage women to take risks and challenges;
- whether they could cite sayings (proverbs) in their community that epitomize and glorify the success of women, particularly positive deviances;
- what challenges they had ever faced in their community during their participation in decision-making processes or IGAs? And did they overcome the challenges? If yes, how? If no, why?

Via these kinds of questions, it seems to me that Woldegies was consciously using the interview as, inter alia, a forum for revitalizing the women's sense of agency (to make a difference to their own and others' lives) and their sense of how they could participate in activities that offer a "louder voice" for women.[8] According to Gergen (2015), research can be regarded/treated not as a world-mirroring, but as a *world-forming* process. Or as Cannella and Manuelito (2008, p. 51) explain, here one is working with an understanding of the world in the process of becoming; and as a researcher (researching "the world" with participants) *one contributes to this process of becoming* (in what one considers, with participants, to be desirable directions). Shumbamhini (2005, p. 12) indicates how the use of "narrative as guiding metaphor" (for her research, in this case with widows) opened

> space for new knowledge, which would evolve from the story told so that the storyteller and listener are involved in the construction of new meaning and understanding within a particular context. Stories both describe and shape our lives. (2005, p. 12)

I consider that Woldegies operated somewhat implicitly in terms of this understanding that the interview encounter would be generating meaningful information that would not be leaving the participants (the research participants and Woldegies) and wider audiences "unchanged" via the encounter. Furthermore, I regard Woldegies's choice of questions created in his interview guide as involving what Chilisa and Ntseane (2010, p. 620), favorably citing Marshall and Young (2006, p. 65), call a "vigilant monitoring" of how questions are asked. They concur with Marshall and Young that researchers have a responsibility to be vigilant in asking questions in such a way as to express a "sharp attention to the power of dominant values". I elaborate more on this below, with reference to a detailed examination of the questions.

But firstly it is worth mentioning that owing to the cultural context, Woldegies felt it important to include a female assistant (Ms Abeba Tekle) who would be present in the interviews and also take notes (2014, p. 243). Her presence, Woldegies indicates, would make participants feel "not alone" (2014, p. 243). She

[8]Puebla, Faux, and Mey aver that "most published interviews do not reflect the amount of effort and thought that went into it" (2004, para 31). I suggest that indeed a lot of (hidden and not reported-upon) effort and thought went into the construction of these interviews (as I also am cognizant of, from mentoring Woldegies at an earlier stage of the project, when I was asked to act as mentor for him on behalf of Antioch University).

would also help to record the beginning of the interview, as it was deemed more culturally appropriate to record only the beginning. Woldegies explains that:

> Permission was also obtained from individuals and groups in order to take notes during the conversation and take short introductory audio recordings of conversations to comply with the local cultural beliefs (not to still the voice of the participant or destroy their voice if recorded for a long time). (2014, p. 28)

I now look closely at the questions that Woldegies asked in the interviews, and I offer an extrapolation of their content in terms of researcher responsibilities. As I see it, Woldegies proceeded with an awareness that question-framing is not neutral in its consequences and that he had to take some responsibility for the way in which the interview/conversation itself could impact on the participants and others (when the "results") became shared. Let us consider the questions in turn.

The first question asked the women to "tell their story" about their experiences in starting up their business—this question was a route into asking them further questions. Already, it can be said that asking them to "tell their story" regarding their starting of their own business, urged them to express possibilities for what is called "positive deviance"—that is, deviating from what might be considered as normally "expected" conduct in terms of certain cultural prescriptions for women's roles. Woldegies elucidates that his ideas about positive deviance and the need for rural women to "share their stories" as a vehicle for inspiring other women, were developed in 2010, when, as part of his activities at NED, he had been involved in a project entitled "Women's Entrepreneurship Development Program with Nutrition Diversification in Rural Merhabete, Amhara Region, Ethiopia" (Woldegies, 2010). In this year, he notes, "a number of positive deviance women who became successful in this project inspired me to show their exemplary work to the larger segment of rural women" (2014, pp. 14–15). In similar vein, via this research (2014), Woldegies was hoping to access more "positive deviance" (PD) stories, which could be spread (via community organizations, including NED) to other women.

Woldegies indicates that his understanding of PD was attained from his reading of literature (e.g., Positive Deviance, 2004), which suggested the need to highlight and strengthen PD in a community so that "new behaviors" could be more widely practiced (2014, p. 199). In regard to gender relations, he notes that "chauvinistic attitudes that hindered women acquiring legitimate rights of power and socioeconomic status" are pervasive in Ethiopian communities, but are not the only prevailing attitudes (2014, p. 199) What is important, Woldegies suggests (drawing on PD theorizing around possibilities for appreciating "new behaviors"), is for both men and women to recognize that women can be "integrated in each activity in the society" to the benefit of a general wellbeing in community life. In terms of PD theorizing, certain women can be singled out as being "uncommonly successful". This meant that they "have the potential to solve their problems exceptionally, better than their peers". By looking at such women appreciatively, one can see successful women as role models (2014, p. 199). As I explain below, looking at how women are "uncommonly successful" (in terms of PD theorizing) does not

exclude exploring how they, together with peers in the community, were able to organize into groups and clusters so they could better find ways of becoming successful—for example, through self-help group savings schemes that they had set up and other ways of working together in clusters by forming economic associations.

Woldegies notes that the theory of PD can be enhanced by linking it to what is called an "Asset-based Community Development (ABCD)" approach. This approach extends the PD approach by foregrounding the importance of community networking as a way of generating community assets. Woldegies clarifies that such theorizing as developed by, say, Mathie and Cunningham (2003) "takes as its starting point the existing assets and strengths of a community, particularly the strengths inherent in community-based associations and other social networks" (Mathie & Cunningham, 2003, p. 1). He goes on to explain, referring to Mathie and Cunningham, that ABCD

> concentrates on identifying the capacities of individuals, associations, and local institutions, and rests on the principle that recognition of strengths and assets is more likely to inspire positive action for change in a community than an exclusive focus on needs and problems. Accompanying this approach are a set of methods that have been used to mobilize communities. (Woldegies, 2014, p. 83)

He remarks that PD and ABCD are theories that have been "used in the women's advancement program, particularly by NED and ORDA[9] to facilitate their empowerment programs to meet their practical gender needs" (2014, p. 83). In other words, the theories were found in other contexts to be inspirational. How then, did this prior understanding of these theories affect his way of conducting the interviews in the case of the 2014 research? Redman-MacLaren and Mills (2015, p. 6), following Charmaz (2006), make the point that "theoretically sensitive researchers" can "incorporate personal insight and intellectual history" into the research process in that they are able to "see possibilities, establish connections, and ask relevant questions". This does not preclude "generating and analyzing data in partnership with research participants"—who are involved in co-searching the issues (2015, p. 6).

As I shall elaborate further below, I believe that Woldegies did not pretend to be entering the interview context without any prior theorizing or viewpoints which admittedly would influence how the interview/conversation proceeded. Like Ssali and Theobald (2016), he was concerned not to exercise undue "power" as a researcher in framing questions; and he therefore started the interview with an invitation to participants to "tell their story". His intention furthermore was to be able to highlight (for the women themselves who become affirmed through telling the story and for others who can be inspired) what it takes to become "successful" (against the barriers of chauvinism). In this way, he took into account the impact that his questions might make on the women themselves and the wider community,

[9]Organization for Rehabilitation and Development in Amhara (ORDA). This was one of the charity organizations with which Woldegies worked closely.

by focusing on "what is possible" (despite the odds against success). This sensitivity to impact can be seen (or rather, is an interpretation that I offer) by looking at his framing of subsequent questions, as elucidated below.

Woldegies followed his first question with a question concerning opportunities for the participants and others in the village for IGAs. This was supplemented by asking them about family and community support that they could identify/remember. Again, we can see that here, Woldegies was influenced partly by the literature that refers to ABCD, which offers what Chilisa (2012, p. 17), following others (e.g. Smith, 1999), calls a non-deficit view of opportunities. This is a choice that researchers can make, rather than focusing on shortcomings of participants/communities. Chilisa states that it is important that researchers (with participants) try to undercut discourses which are imbued with deficit thinking. (See also Chilisa Major & Khudu-Petersen, 2017, pp. 10–11.) This involves a process of deconstruction of deficit discourses and reconstruction (of new ways of seeing, which concentrate on assets). The research process should thus be used in order, inter alia, to "interrogate distortions of people's life experiences, negative labeling, deficit theorizing, genetically deficient or culturally deficient models" (Chilisa, 2012, p. 17). Chilisa elaborates as follows:

> The researcher has to move the research from deficit- and damage-centered research to investigation that builds communities and restores hope and belief in their capacities to resolve challenges that they encounter. (2012, p. 174)

The idea behind this is that researchers' discourses, developed in part by asking questions and discussing issues with participants, are bound to make a difference to the way in which participants and wider audiences conceive possibilities for the exercise of agency. Focusing on "positive labelling"—which explores and thereby strengthens people's sense of opportunities—is preferable to focusing on deficits. A research orientation that becomes focused on locating deficits can all too easily contribute to making people—research participants and/or others who are party to their accounts—feel hopeless. This means that a (witting or unwitting) location of deficiencies at individual and community level is not neutral in its social consequences; and researchers acting responsibly in considering the consequences of their way of framing questions will *take care not to reinforce such discourses*. This is not to say that problems and barriers which minimize the life chances of, in this case, women (who clearly suffer various forms of discrimination as outlined by Woldegies) should be ignored. But it is to say that in locating these barriers, options for ways forward as identified by participants and others, also need to be highlighted and discussed.

Tanggaard (2009, p. 1498) makes the point that the research interview can be considered as "a dialogical context for the production of social life and personal narratives". As a dialogue, ways of speaking are generated during the interview so as to "produce" narratives (as a product of the interview) and accounts of social life. She emphasizes that "interviews are inevitable, dialogical social events based on repertoires of socially and culturally embedded and constantly changing words and discourses" (2009, p. 1498). This implies that the interview/conversation does not

need to be interpreted as generating a static account of how the interviewee sees his or her life in community, but can express shifts of discourses as the interviewee sees different angles evoked by the interview questions. This would also be in line with Holstein and Gubrium's account of what they call active interviewing (1995) and postmodern interviewing (2003), with the interview being considered as an encounter in which meanings can become *developed* (as I elaborate upon in Romm (2010, pp. 239–240; 2014b, p. 15). It is also consistent with Kuntz's assertion that interview encounters express a process of "coming to sense" rather than retrieving a pre-given meaning (2015, p. 57).

Woldegies's next question asked the women whether they considered themselves to be successful entrepreneurs and if they could advise other women, what advice they would give. This question was meant (as I see it) to enable women to reflect upon whether they regard themselves as successful and also how they might advise others on the basis of their experiences. In this process, Woldegies was also *generating information regarding advice that could be given to (and shared) with other women*, either by the participants themselves or through his subsequent dissemination of the stories in various venues (such as women's organizations). Already asking women what advice they might give others, was a catalyst to later stages of the project where such advice was spread via women's associations. (During the interview, some women explained the advice that they had to date been giving, such as advising other women to take seriously the need to educate their female—and not only male—children, to participate in viable IGAs and savings, and to try to practice equal participation with men at household and community levels.)

The question put to women whether they feel empowered through their activities and if so, how and why, was meant to provoke their self-reflections around how they saw themselves, if at all, as exercising more agency/power to improve their life chances and to act in the community—and how they identified the nature of this "empowerment". Besides offering the women the chance to reflect on this question (which itself might help them to affirm their agency) this would later contribute to Woldegies's generative theorizing to be shared and discussed with targeted organizations in the community and with wider audiences. For example, Woldegies highlights the fact that some of the ways in which women expressed their empowerment was, for instance, their indicating that they had coordinated environmental conservation activities in the community and had become role models in operating IGAs, adopting health packages and practicing improved agricultural practices. Some had formed a Women's League to use their collective voice for equal power with men at home and at community levels. Woldegies especially chose to document the interviews in the form of individual narratives of what each of the women shared with him (together with the assistant), so that their inspirational stories could readily be shared in the wider community (via various women's associations and organizations). I offer more detail on this in the section on dissemination (Sect. 4.2.5).

The specific question that Woldegies then posed concerning whether and how they felt they acted as a role model for other women again gave women a chance to ponder on this issue, and to recognize (more fully) their function that they could play as role models, rather than being shy to display their "success". Perhaps just by asking the question during the interviews about whether they felt they could act as role models, inspired them to see themselves as being role models. This would tie in with Chilisa's point about "positive labelling" and how the research process, if exercised responsibly, can contribute to feeding into people's sense of self-worth and worth in the community. (This sense of self-worth appears to have been expressed during the interviews, judging from the content thereof as recorded by Woldegies, where women—even in a context of discussion with men—expressed how their bargaining power had increased both within and outside the home.)

Woldegies also asked the question whether they had ever organized women's groups to change themselves and to give a voice for themselves and others. This was in a context where, as Woldegies understood it, "women's political representation is low and the women's movement is weak and voiceless" (2014, p. 24).[10] Woldegies felt, as one of his starting assumptions/concerns, that it was essential to strengthen the "collective voice of women to overcome resistance"—that is, the resistance of a predominately patriarchal system of privileges (2014, p. 32). Hence he asked the women to reflect on what it might mean to be (more) vocal. (These kinds of reflections that he invited were also followed up in a later question asking them to reflect on their level of engagement in mobilizing the community to have a "louder voice".)

Woldegies furthermore asked them to recall incidences of dealing with male domination and other social barriers (and what kind of assertive behavior they may have utilized); to consider whether they had developed social skills to effect changes in the family and community; whether they had sent their daughters to

[10]This is of course not the only context in which the weak voice of (especially rural) women has been noticed. Wang, Burris, and Ping note that "in developing countries, rural women are often neither seen nor heard, despite their extra-ordinary contribution to the labor force (1996, p. 1391). In view of this, they developed a method which was used initially in Chinese rural villages to encourage Chinese village women to become, as they put it, "visual anthropologists". The research process involved "putting cameras in the hands of rural women and other constituents who seldom have access to those who make decisions over their lives" (1996, p. 1391). It was hoped that in this way the women could develop a louder voice in policy-making circles. Wang, Burrie and Ping contend that the "photonovella" research approach has the potential to be impactful in that "through exhibitions in public spaces, their photographs and voices reach broader audiences in general, including policy makers in particular" (1996, p. 1393). Nonetheless, subsequent research on how such photovoice research might influence policy making suggests a variety of mechanisms that need to be put in place such as (a) longstanding relationships between researchers and community; (b) intensive training to build community capacity; (c) an iterative cycle of community documentation and critical dialogue; and (d) multilevel outcomes including engaging community members in action and advocacy, enhancing understanding of community needs and assets, and facilitating individual empowerment (see Catalani & Minkler, 2010, p. 448).

school; and whether they found that their relationships with their husband had changed as a result of their IGA engagements. Besides offering "information", these questions might stimulate the participants to think about the issues raised in ways that they might not have thought about before—that is, to reflect on changes in gender relations.

And he asked questions about whether they could cite sayings (proverbs) in their community that encourage women to take risks and challenges; and whether they could cite sayings (proverbs) in their community that epitomize and glorify the success of women. These questions could help participants to recall such sayings, both for the benefit of reminding themselves of this (which they could carry into their everyday interactions) and for the benefit of obtaining a collection of sayings that could be distributed among women's associations. NED itself could (and did) also distribute these positive proverbs and idioms about women that were collected during the research.

Finally, Woldegies asked what challenges they have ever faced in their community during their participation in decision-making processes or IGAs. And did they overcome the challenges? If yes, how? If no, why? Here the participants could express barriers, which would become a source of information to be shared with other stakeholders/organizations and the government. Woldegies notes that the government already had in place a number of programs for North Wollo. By identifying barriers as expressed by women, Woldegies could liaise with non-governmental and governmental stakeholders to consider how to offer starting support to women attempting to become involved in IGAs. (Some of the challenges they mentioned were, for instance, the gaps they face because of lack of capital, place of work, and access to credit. These were also reiterated by them in the focus group discussions, and in Sect. 4.2.2, I provide some detail on these.)

Clearly, Woldegies had in mind a goal of learning together with the women about possibilities, as well as the continued challenges they identified. A further aim was to explore with them avenues to strengthen empowerment options for women (such as his liaising, partly on their behalf, with non-governmental and governmental organizations). As Dzapasi notes (2015, p. 36), besides individuals becoming more empowered to act and make choices, institutional empowerment means identifying the needs of individuals, so that they *can be supported* to "do a lot for themselves" (my italics). This means opening options for them to act deliberatively and responsibly.

I now proceed to offer my interpretation of the import of the focus groups in terms of exploring and supporting empowerment initiatives. In the course of my pinpointing Woldegies's approach in organizing the focus groups (and indeed in arranging for both women and men to participate in discussions), I spell out how I see the "active" role that Woldegies fulfilled in relating with the various participants and with additional stakeholders to gear the research toward generating women's empowerment (not necessarily at the expense of men).

4.2.2 *Inviting Women and Supporting Family Members to Focus Group Discussions*

As part of the research design, Woldegies arranged with participants for three focus group discussions (FGDs), which all included women and supporting family members. Woldegies used a broad introduction to spark off the discussion in all three focus groups (in Talit, Meley and Gashena respectively), by stating the following to the participants in the sessions:

> The FGD is prepared to obtain information or experiences from concerned individuals (women, husbands of women and other adult family members) about economic empowerment through income generating activities and social mobilization in the context of married Amhara women in Wadla Woreda. (2014, p. 231)

He then proceeded to ask one general guiding question to focus the discussion, namely:

> How much emphasis is given by stakeholders including women to social mobilization in order to increase the income of women and what should be done to engage women in IGAs to make them individually more effective and productive? (Woldegies, 2014, p. 231)

Woldegies notes that this broad introduction from his side elicited a range of stories from the women and their supporting family members concerning ways in which women were already participating in IGAs, how they obtained support from family members, what their understanding of social mobilization was and how it was practiced, and what further support (from other stakeholders) was needed.

Woldegies does not say how the actual discussion proceeded during the focus groups, as he concentrates on the content of what was said. Woldegies's summary of the outcomes of the discussions is set out below.

4.2.2.1 Focus Group: Talit

In Talit, Woldegies notes that

> the understanding of the participants on the concept of social mobilization [SM] was "living a better life." Their engagement in the IGAs and awareness about their rights is good, and they said that previously they didn't consider their rights. (Woldegies, 2014, p. 199)

Regarding SM, Woldegies reports that the women participated in SM through "women's associations, women's leagues, women's federations, women, children and youth affairs, youth associations, youth leagues and youth federations, and kebele and woreda councils" (2014, pp. 199–200). The women became members of different organizations according to their interests and also of organizations that give them opportunities to defend themselves from forms of gender-based violence (2014, pp. 202–203). Woldegies states that:

> They organized themselves in women's organizations which gave them integrated power. Both the husband and the wife have equal power concerning their properties. However, since most of women are not educated, they do not have adequate knowledge and legal literacy to claim their rights and to respect others' rights. Due to their participation in IGAs and SM there is gradual improvement both in the political, economic and social sectors through their organizations at various levels of the family, kebele, etc. (2014, p. 200)

As far as IGAs are concerned, the supporting family members realized that the women were contributing income to the family and were therefore supportive of their wives. The men explained during the FG sessions in what ways they are supportive of the women (task wise and in respecting their activities) and that they were sharing tasks in the home normally assigned to women. Woldegies summarizes that "according to this study the women usually get assistance from their supporting family both at home and on the farm" (2014, p. 200).

The women mentioned (as in the other focus groups) that, as part of running their businesses, they save their profits in what is called *equib*. Different women's *equib* groups initiated by NED had been organized.[11] The participants also mentioned that "various government and non-government organizations tried to provide minimum support to improve the life quality of women in Wadla Woreda" (2014, p. 200).

The participants also made recommendations (probably realizing that Woldegies could act as a conduit/mediator for them and also because his general question asked them to consider "what should be done"). They suggested the following:

- There is a need to provide workplaces and initial capital for their business activities.
- The women also requested project support by the government to increase the current profit and to provide special support by the government for single women who are not married and live alone, and women who are living far from roads.
- There is a need for professional support and operating women's organizations and associations through full-time professionals by allocating the needed budgets.
- It is important to undertake further studies concerning the sexual abuse and harassment against beautiful ladies and old women.

The participants thus used the focus group discussion as a mechanism to identify requests to be followed. In other words, they treated the focus group session as an occasion not only to share experiences, but also to discuss recommendations for needed additional support from stakeholders. (The same occurred in the other focus groups, which suggests that the participants all felt that the sessions could and should be used to collectively discuss recommendations.)

[11]This is used as a form of collective savings and capital accumulation (Woldegies, 2014, p. 111).

4.2.2.2 Focus Group: Meley

In Meley, Woldegies notes that the women

> understand the concept of SM as organizing family, neighbors, and the community and mobilizing them to stand for specific purposes in a united manner in the local developmental association and to change their lives. (2014, p. 201)

They mentioned, for instance, that they are "organized under the Women's Development Committee and they are mobilizing the local community to protect its area from natural disasters and drought and they are participating to protect the environment" (2014, p. 201). They also were participating directly in politics by organizing themselves under the Youth and Women's League (2014, p. 201). Woldegies explains that:

> The women have a very good relation with different organizations and the Women's League and Youth League have strong affiliations with the ruling party, and are obtaining more support and benefits than the other women. The stakeholders such as the Woreda and Kebele Women's Affairs, the Women's League, ORDA, Nurture Education and Development have been supporting them by training the women, and introducing modern procedures for them. One of the governmental offices, the TVED supports them by evaluating their job creation plans and preparing business plans for them. (2014, p. 202)

As far as stakeholder support is concerned,

> various stakeholders supported them to increase the income of women: ORDA and NED, WWWCYA, TVED, local councils, woreda and kebele cabinets, police offices, and justice offices are supporting them in various ways. (2014, p. 203)[12]

Woldegies notes that from the discussion it was clear that "the support of stakeholders is better in Talit and Meley, whereas it is poorer in Gashena"—the other focus group context (2014, p. 203).

In addition, they were organizing themselves in Meley under associations such as the Cotton Spinning Association, and this has become a source of good income for the women. As in Talit, the women save a lot of money through *equib* (used as a collective means of savings and capital accumulation in order to run their businesses).

The women here also emphasized that "their bargaining power continuously developed both within and outside the home". The supporting family members in the group iterated too the kinds of support that they offer (Woldegies, 2014, p. 203, 2016, p. 68).

As far as recommendations go, the women in Meley underscored the need for

- places to work and loans with a minimum interest basis;
- reduction of their work load; and
- some technical support from governmental offices.

[12]The acronyms of WWWCYA and TVED refer respectively to the Wadla Woreda Women Children and Youth Affairs Office and the TVED Technical Vocational and Enterprises Development office.

4.2.2.3 Focus Group: Gashena

In Gashena, in relation to the issue of SM, participants made the point that SM has to do with "the changes which are brought in their lives—genuinely supporting one another and working together" (2014, p. 203). But Woldegies remarks that women's participation in SM in Gashena is too weak because of the lack of efforts in organizations, unlike in Meley and Talit which are stronger than Gashena. This, he argues is "due to weak and fragmented efforts" (2014, p. 203).

He suggests that in regard to "IGAs to make women more effective, there is a failure to organize under an association and to work in unity" (2014, p. 204). It is clear from his analysis (comparison with Talit and Meley) that he finds this problematic as he believes—from his experiences and from his also speaking to the women and men in Talit and Meley—that collective organization is a route to economic and social empowerment of women.

The suggestions that the women in Gashena made were that they need

- follow-ups of professionals to offer advice as to how to handle their production activities;
- initial capital loans which are free from interest;
- a place to work which they lack since they live near the town of Gashena where land is expensive; and
- the Woreda management and the Kebele administration should pay attention to organizing them.

What I would like to highlight here, which has not been foregrounded in Woldegies's discussion of the various FGs is that the FGs seemed to have taken the form of what Magnat (2012, p. 168) calls a kind of "theatre", where men and women were able to participate in ways which did not reinforce gender stereotypes. They were in this way exploring together as well as strengthening in the process "unusual" gender relations—for example, by the men mentioning that they provide some support on a labor and emotional level in the home and in the activities in which the women were involved. (It is not clear from the discussion whether all the men expressed this and whether they mentioned the extent of their support.[13])

I would suggest that by virtue of holding these FG sessions, new patterns of relationship became recognized as legitimate—as new stories which express these styles of relationship were fashioned via the discussion. The focus groups also served as a forum for the women and men to experience ways of speaking together, where women could be affirmed publicly (in this forum already) by family members. Furthermore, Woldegies's choice (discussed with participants) to disseminate the "results" of the discussions in the form of narratives that could be shared with various organizations and stakeholders was also a way of strengthening "unusual" discourses about gender relations where men support women at home and also

[13]Woldegies states that, as he interprets it, "the research benefited the women and their supporting family members to enhance their awareness of women's rights and gender equality" (2016, p. 68).

respect their mobilizing in the community. In this sense, what Delgado (1989) and Delgado and Stefancic (2001) call counter-narratives, which express alternative images that go against stereotypical responses, are presented, and these images become rendered more accessible to people.[14] The discussion of non-stereotypical gender relations and the dissemination of these in various forums could thus be said to have been significant in *rendering these accessible as options for thinking and for conduct*.

Of course, the ways in which continued SM needs to occur and the benefits of such mobilization were also iterated; and again one can say that the focus groups served the function of alerting women (and men) to the importance of such mobilization. Meanwhile, the stories about new gender relations—and benefits for men too (as women play a role of partnership in marriages)—could become "spread" via NED and other NGOs which Woldegies targeted for dissemination.

In short, I would say that the focus groups were responsibly designed (perhaps somewhat unconsciously/intuitively) so that women and family members could

- not only discuss but also strengthen via the discussion the connection between family members;
- consider options for increased SM (in the community); and
- discuss together options for how they could harness support from other stakeholders.

Woldegies chose to invite women and their supporting family members to the same focus group discussion, with the help of a female assistant to offer emotional support for women. A mixed session (arranged in this way) enabled the women to feel that they could express themselves in a discussion, which at the same time could serve to loosen stereotypical conceptions of gender roles in the focus group and in the wider community (when the results were disseminated).

This concurs with Magnat's account of trying, as an initiating researcher, to seek to develop/arrange research forums which "provide women with creative agency beyond the limitations placed upon them by conventional psychological realism", where the latter tends to "naturalize the normative gender roles" (regarded as normative in a society) (2012, p. 172). The purpose, as she expresses it, is to *alter the limitations of life as lived* (p. 172)—so that the research in this way becomes transformative. Ultimately, she argues, following Wilson (2008), that research becomes a ceremony for "making connections and strengthening them" (2012, p. 168). Here she refers to connections between human beings and between them and all that exists, including the natural world. Interestingly, some women in the focus groups referred to their efforts to protect the environment as part of sustainable living. Again by mentioning this in the forum, it may have helped to strengthen some participants' sense of the importance of respecting nature as part of

[14]They discuss this in the context of racism and racial stereotypes, but the general discussion of stereotypes and of the importance of generating counter-narratives is relevant here. See Romm (2010, p. 169) for a fuller account of this argument.

relational living. And as Woldegies indicates (2014, p. 235), one of the interested stakeholders in this regard (to whom the report would also be disseminated) was the Association for Combating Desertification (ACD).

Finally, I would like to reiterate that the focus group method was used by the participants in this research not only to generate stories about the lives of the women and their supporting family members, but also to generate a discussion around recommendations. And evidently Woldegies understood that he should take some responsibility for taking these forward. (He notes that he had become friends with many of the stakeholders through his previous and current involvements in NED; so it was feasible for him to indeed try to take recommendations forward—2014, p. 14.)

4.2.3 (Responsible) Approach to Member Checking

Woldegies comments that the concept of "member checking", as it is used in much of the literature on qualitative research, was developed in order to encourage researchers to "allow participants to critically review how the researcher has represented their words" (2014, p. 101). In the light of such literature, Woldegies explains his way of proceeding:

> Since I made minimal use of the tape recorder I summarized the notes made from the interviews and focus groups, often immediately after the sessions, so that they could add corrections. I also [later] made additional inquiries about some of the information I received during the interviews. (2014, p. 101)

He indicates that one of the elements of the process of member checking involved encouraging the women during the course of the interview to express themselves in more detail at various points. He did this by ensuring that he used easy communicable words to encourage them to express themselves:

> During the interview process, I probed for additional information … . I also got additional information by further clarifying some of the terminology used in Amharic language by using easily communicable words, poems, action, history, etc. (2014, p. 101)

He states that during the interviews,

> I continuously checked the understanding of the phenomenon/the concept by paraphrasing and summarizing for clarification and engaging them to detect any distortion of information (particularly social mobilization is asked about in a different way in Amharic, via their organizing efforts such as women's association, Ikub, mahber, women league, council, etc.). (2014, p. 102)

Moreover, he chose to check/query when they offered what he considered to be "possible exaggerated generalizations" during the course of the interview. As an example, he notes that some of the women said—"No one gives his/her daughter for early marriage in Amhara". He indicates that when they offered such generalizations, he decided to "make further inquiries" (during the interview itself and

afterwards). This was so that he would not report on "general truths" which could not be authenticated (and which the participants themselves could recognize did not apply to *all* women). In this way, he exercised some responsibility in ensuring that the women's statements would not be regarded as uncredible, by giving them a chance to reconsider their statements (upon further reflection).

He also states (p. 94) that as part of the member checking process, he later afforded the women "the opportunity to review the transcripts that he had created and to provide clarity and accuracy, feedback that I incorporated into the findings" (as advised by, for instance, Clausen, 2012). (The target women were able to read and write since they attended Integrated Functional Adult Literacy through NED and similar NGO projects). The process of asking participants to check and review transcripts with a view to offering feedback took place at the point at which Woldegies, with the assistant who was present in the interviews, made visits to look at the products of entrepreneurship of the women. While offering what Woldegies calls "feedback", the women decided not only to clarify/extend their ideas as recorded in the transcript, but also to add additional points. Woldegies notes that they

- added some new ideas about idir, ikub, the women-concerned organizations' operation;
- suggested to take pictures of the money collection plates they use for equib (savings schemes); and
- offered new information about why the abuse of older women becomes common in North Wollo. (p. 102)

These additions offered by the women, indicates (to me) that the participants found ways of participating in the design of the research—by treating the "member checking" process as an opportunity to add ideas. They also suggested that pictures should be taken as another means for generating additional credible "data" about the activities—data that could be shared with others in the form of pictures. The women felt sufficiently comfortable to offer these suggestions to strengthen the research (as well as to aid the sharing process at the point of dissemination).

Additionally, Woldegies indicates that the focus group discussions (FGDs] were a further mechanism for women (along with supporting family members) to "clarify questions"; and the focus group sessions therefore provided an opportunity to further look into what had been said during interviews (p. 98). He states that participants in the FGDs were "able to provide information on actual cases of the successes and barriers of women economic empowerment"—thus expanding upon what the women had expressed in interview format (p. 98).[15] The focus groups became an occasion for the women to add more substance (now in another forum) to the material/narrative accounts as had been expressed in the interview format and

[15]He notes that with regard to the credibility of what was said in the FGDs, typically, focus groups have high validity which is due in large part to be the credibility of participants' comments (p. 90). This is especially so, because in this case, the "data" that were generated in the FGDs constituted an expansion of what had been said in the interview forum.

to open their cases to collective discussion. The participants also collectively reflected on the recommendations.

Finally, at the reporting stage, Woldegies's assistant, Mr Astewale, "who is from Wadla and who worked in the office of Agriculture and Food for Hungry International (FHI) helped to further authenticate the information". Woldegies indicates that FHI commented and validated the report, as he read it to them in Amharic, before translating it into English (p. 103). This, I would suggest, was part of an effort to augment the significance of what participants had said, by having it further "authenticated" by a helpful organization. This can also be regarded as an important activity on Woldegies's part on the level of dissemination—because one consequence of checking the narrative accounts by cross-checking the information with the FHI, was to alert FHI to the various participants' experiences as told in interviews and focus group sessions, including their expressed challenges. In any event, Woldegies felt that part of his role as a researcher was to use the research process to generate credible stories in such a way that the stories would be likely to "move" others (including key stakeholders) on the level of emotion and of action.

4.2.4 Ethics of the Research

Woldegies indicates that in organizing the research, he obviously had to take into account various potential ethical issues associated with conducting a field study with human beings (2014, pp. 28–29). His first consideration, he notes, was to "get informed consent from people to be researched". In this regard, "every interview and discussion was preceded by an explanation of what the study was for and a request for the respondents' consent" (2014, p. 28). He does not state whether the consent was attained in written or oral format; and in Chap. 8, I discuss this issue further. He specifies that not only were the participants happy to give their consent, but the women wished that pictures of them with their entrepreneurial work should be taken, as part of the research (p. 28). They did not wish their stories to be anonymized, as is often suggested in more conventional research ethics guidelines (2014, p. 99). He elaborates on this point as applied in this context:

> As the study of the women focuses more on the economy, they were happy and took it as an opportunity to use their pictures and real names to illustrate their engagement and to show their incredible work to the people, financial institutions, NGOs, government, academic institutions, etc., who might refer to the dissertation. (p. 99)[16]

Besides obtaining consent from the participants and group discussants, Woldegies also asked for written permission from the Wadla Woreda relevant government office in the woreda.

[16]Woldegies did, however, keep secret certain private information that was divulged in some cases, and did not report on it at all: "Information that needs to be kept private or confidential is not reported in this study report" (2014, p. 99).

Another ethical consideration to which Woldegies refers is, as he notes, "related to the protection of the research participants and their households from possible social, psychological, and economic harm (2014, p. 29). In order to "avoid potential marital and other social tension associated with individual interviews and group discussions, no adult other than the informants/group discussants were allowed to attend interviews or discussions" (2014, p. 29). In this way, Woldegies tried to address the question of minimizing/avoiding potential harm. Clearly, although not rendered explicit by Woldegies under his discussion of "ethics of the research" (2014, p. 28), he also manifestly took into consideration the additional ethical requirement to try to ensure that both the participants and the wider community would benefit from the research, with attention to forwarding women's empowerment within the communities involved. He found innovative ways (with participants) to use the research endeavor as an opportunity for furthering the wellbeing and development of individuals and communities.

As indicated in Chap. 1, many Indigenous authors writing about Indigenous pathways to research insist that researchers reflect (with participants) upon ways of contributing toward the quality of life of participants and others, with the focus on those most marginalized. (See Chap. 1, Table 1.2.) In this regard, Woldegies can be said to have exceeded the requirements of the Belmont principles as detailed in the USA-commissioned Belmont report (1979), which I discuss further in Chap. 8. Woldegies does not indicate explicitly that he extended the "ethics of research" as required by the (Western-oriented) Belmont report (which has come to inform many ethics review committee across the globe). However, I would suggest that his dissertation taken as a whole provides evidence of his considering his felt obligations as being wider than presumed in the Belmont report, in that he actively sought opportunities with participants and stakeholders to further women's empowerment in practice. Castro-Reyes et al. (2015) aver in this regard that the Belmont report is not well fitted to doing research that is directed at engaging communities in transformative work; while White (2009) likewise contends that ethical review boards which try to follow the advice of the Belmont report are not suited to understanding action-oriented research (which would include collaborative research with communities). While Woldegies does not discuss his research in relation to his efforts to widen narrow understandings of the "ethics of research", this, I believe, can be read off/extrapolated from his write-up of the work (2014, 2016).

Where Woldegies does explicitly indicate that he distanced himself somewhat from Western-oriented guidelines for ethical research is in his discussion of the importance in this cultural context of offering "incentives" to participants to take part in the research, which he notes, was culturally appropriate:

> Participants were provided with incentives, to conform with the existing culture. This is done usually to help build up the relationship and say "thank you" to them. This is seen as a sign of respect and part of a tradition, which recognizes their contributions to the research. Therefore, a financial incentive was offered to research participants as compensation since the women spent part of their working time in the research. I paid a per diem of about $3 per head to the 10 women and their support groups who participated in the study. In some

> cases, the researcher had to buy drinks (homemade brews, tea, coffee, etc.) for informants and had to provide financial assistance when an urgent need was noticed. (2014, p. 29)

Especially since Woldegies was due to obtain a doctorate on the basis of the research that was being undertaken at this point, it seems that he felt obliged to offer some "immediate" returns to the participants.

In detailing his commitment to the research in subsequent sections of the dissertation, Woldegies explains further his ethical approach, which included wanting to use the research process to advance women's empowerment. He explains that:

> Male involvement in the "affairs of women" is seen as very strange in the Amhara Culture in which I was brought up. I am a male researcher and I wanted to be taken as an example that engaging men in women's emancipation processes can help to change the deep-rooted gender inequality in our society. (2014, p. 94)

At the same time, he indicates that because of his work for NED, he had regular contact with the women and their families through the work of NED, and for this reason he

> expected the women to feel at ease with my presence. I do not represent any government authority, or police force whom rural people mostly feel intimidated by. I am working on behalf of a charity organization committed to supporting the poorest of the poor. I am usually dressed casually rather than formally. (2014, p. 94)

He therefore hoped that on this basis he could develop a relationship with the participants, wherein they would not feel "intimidated" by him (and his researcher status).

He also shares with us as readers that his choice of the research topic was shaped by his "professional and personal concerns about gender inequality" (2014, p. 94). He does not believe that this detracts from the value of the research work; on the contrary, it supports it. He cites Denzin and Lincoln in this regard: "As Denzin and Lincoln (2000) underlined, just as the artist is the primary instrument in painting, the researcher is the primary research instrument in qualitative investigation" (2014, p. 95, referring to Denzin and Lincoln). This implies for him that researchers' creative way of proceeding with the research endeavor will admittedly make a difference to the "picture" that is painted. But this (subjective) artistic involvement need not be a threat to the value of the work, because readers can judge the work on the basis of the way the research process proceeded (including his processes of "authenticating" the stories, as explained in Sect. 4.2.3 above).

What Woldegies does not delve into is the question of judging the quality of the research with reference to its attempts to "step beyond interpretation and Verstehen [understanding] toward social action" (Lincoln & Guba, 2003, p. 268). Lincoln and Guba observe that for some theorists/researchers "the shift toward social action came in response to widespread non-utilization of findings" and the desire to find "champions who might follow through on recommendations and meaningful action plans" (2003, p. 268). For others, they note that the importance of embracing forms of "action" as part of the research remit is seen as a "political and ethical commitment". In both cases, the "action" that researchers either inspire or undertake

with participants (who can be seen as co-researchers), is deemed "an important outcome of inquiry processes" (2003, p. 268). Indeed, it is not only an outcome but one way of validating the worth of the project—as expressed by the concept of catalytic validity, which I detailed in Chap. 1, Sect. 1.3.2. Lincoln and Guba go on to note that "many positivist and postpositivist inquirers still consider 'action' [as being] the domain of communities other than researchers and research participants: those of policy personnel, legislators and civic and political officials" (2003, p. 268) —and therefore do not concern themselves with this domain.

Lincoln and Guba (2003) assert that the "mandate for social action" can be seen as one of the main dividing lines between positivist/postpositivist research and new (er) paradigm inquirers—as I outlined in Table 1.2 in Chap. 1. They state that in "much new-paradigm inquiry work", designing the research so that it is likely to make a positive difference to the lives of participants, communities and wider audiences, is regarded as part of researchers' responsibilities (2003, p. 268). Put differently, in new-paradigm inquiry, researchers often deem it as *part of their responsibility to consider likely impacts* and do their best (in the circumstances) to contribute to fruitful discourses and actions. While not referred to explicitly by Woldegies in his discussion of ethical issues (2014, pp. 28–29), my reading of this work, supplemented by a number personal communications with him via Skype and email (at various points from 2012 to 2016) suggests that he took these responsibilities seriously as part of an ethical commitment. I discuss this further in Sects. 4.2.5 and 4.2.6.

4.2.5 *Responsible Involvement in Dissemination/Public Discussion*

Woldegies considered that one of the purposes of the study at the point of "dissemination" would be to circulate to women and to those around them (men included) messages concerning women's positive deviance in a context in which women are often victims of social discrimination. This was a purpose defined by him at the start of the study:

> Due to male domination, women are victims of social discrimination, gender-based violence, and other socio-economic barriers. In the woreda, women have limited access to resources. Their employment rate and representation in local government are low. Their economic status is marginal. At times, their income generation is negative, meaning their returns are less than what they invested, leading them into absolute poverty… . The purpose of the study is to add empirical evidence to existing knowledge on Income Generating Activities (IGAs) by identifying opportunities for women in the woreda and by sharing success stories of women's advancement while also identifying barriers. It is hoped that the results of this study will provide information to concerned stakeholders for scaling up IGAs and for enhanced social mobilization. (2014, p. iii)

Woldegies's statements about the discrimination against women in the woreda (as elsewhere in the region), could be argued to have been retroductively inferred by

him from his experiences over the years in trying to contribute toward women's empowerment, via his NED activities. Based on these experiences, he detects (postulates) "male domination" as it manifests in a variety of ways—see my further discussion in Chap. 7, Sect. 7.4, on retroduction as a logic of inference.[17] With this understanding, he hoped to organize a research study whose results would serve to generate an increased awareness of the issues at stake, for stakeholders also to take up. Woldegies explains his "hoped-for" benefit of the research as follows:

> Thus, the outcome of the study would provide useful information to attract the attention of local state and non-state actors to support the enhancement of women's empowerment initiatives in Wadla Woreda—Women, Children and Youth Affairs office, Ethiopian Orthodox Church, Development Interchurch Aid Commission (DICAC) and Organization for Relief and Development of Amhara (ORDA) are working in localities in the area, and they would be the primary beneficiaries of this research. Also, the results of the research would be relevant to other woredas in the region, since the majority of the women in the region live under comparable conditions. (2014, p. 6)

Woldegies took it upon himself to play an active role in the dissemination process, speaking to stakeholders, including NGOs and stakeholders in government, about the results and recommendations that were generated via the research (and also supplying his dissertation to some of them). Besides targeting policy makers in government, he also focused on organizations such as the following: the Consortium of Christian Relief and Development Association (CCRDA); the Rural Development Forum (RDF); the Ethiopian Nile Basin Dialogue Forum (EtNBDF); the Sustainable Land Use Forum (SLUF); the Poverty Action Network in Ethiopia (PANE); the Consortium for Integration of Population Health Environment (CIPHE); and the Association for Combating Desertification (ACD). His intention with these stakeholders was to alert them to the importance of supporting women in view of the women's contribution to sustainable development (as evidenced by the research). He felt that because the women chose to mention their activities in regard to mobilizing the local community to protect the environment in processes of development, this was essential for the stakeholders to take into account: such information could motivate their continued (and scaled-up) support for the women in this, as in other rural woredas across the country (2014, pp. 233–236).

Concerning the research reporting to the participants, the research was disseminated to the ten women and supporting family members, and to women-concerned organizations after translating the relevant parts into the local language (2014, p. 106). Woldegies indicates that:

> The narrative stories of the positive deviance have been replicated through their [women's] associations. The Amhara people have a number of positive and negative proverbs widely spoken in the community. The positive proverbs are more powerful to transform the status of women and indicate the paradigm shift. (2014, p. 225)

[17]In Chap. 7, I highlight how retroduction can be employed as a mode of inference, which proceeds from the interpretation of observation/experience to the postulation of structures/patterns that affect life chances and are considered to provide leverage for change.

Woldegies thus tried to ensure that at the level of everyday interactions, the research would have some power in transforming the status of women—via the dissemination of the narrative stories and the collected "positive" proverbs. The idea that the hearing of positive proverbs can make a difference to people's orientations, is also underwritten by Mayaba (2012), who used proverbs in the context of aiding vulnerable children in South Africa to increase their sense of self-worth, and to find sources of support from the community. Chilisa Major, & Khudu-Petersen, in a section of their article called "deconstructing, re-constructing and transforming identities through songs and proverbs" also indicate that "Chilisa (2012) has used proverbs to explore community constructed gender ideologies, while Musyoka and Mertens (2007) used proverbs to challenge stereotypes about people with disabilities" (2017, p. 10).

Overall, it can be said that Woldegies tried to act strategically in line with what Speer and Christens call "strategic engagement" (2013, p. 734). Speer and Christens suggest that to engage strategically means that researchers should bear in mind options for making impacts in various arenas—not only at the level of policy, but also within other arenas. This would also be consistent with Mertens' suggestion that when operating in terms of the tenets of the transformative paradigm, one sets out to actively seek collaborative relationships with those involved in the research, while also forwarding beneficence (promoting wellbeing) on several fronts (2009, p. 49). This advice is linked to Mertens' statement that in transformative research, the underpinning aim is to forward a connection between the process and outcomes of research and furtherance of a social justice agenda (2010a, p. 470).

4.2.6 Validation and Generative Theorizing: Toward Transformation

In discussing the question of the validation of the research findings, Woldegies notes that he believes that his close relationship with the women (who had been beneficiaries of NED) meant that both the reliability and validity of the "findings" were enhanced. As he puts it:

> Due to my close relationship with the participants of the pilot study, I had an opportunity to obtain information without reservation on the part of the respondents throughout the NED projects. This situation contributes to a high level of reliability and validity of the findings for my current research project. (2014, p. 14)

Woldegies does not define what he means by reliable findings, but in this context he seems to mean that other researchers, who too are sufficiently "close" to the participants, would be likely to obtain similar findings/stories. As far as his definition of validity goes, he states that "the validity of the research is determined by the degree to which women who will read the findings recognize themselves and their experiences" (2014, p. 89). Here he seems to be suggesting that insofar as the stories strike a chord with women readers as being credible (based on their

experiences of discrimination and of attempts at empowerment), the research can be deemed "valid". This implies that its validity is partly a function of how readers make sense of the research and can relate it to their experiences (in that it resonates in some way with them). Its validity therefore rests not on the text itself, but also on *how readers engage with it (and find it meaningful).* The research as presented thus invites further discussion and engagement, and is not finalized via the presented reporting. This is similar to Melrose's account of "naturalistic generalization", where, as she notes—drawing on Lincoln and Guba's explanation of this notion (1985, pp. 119–122)—readers are invited to enhance their personal understandings by building on their tacit knowledge. Melrose elucidates how "naturalistic generalization" leaves room for reader engagement:

> As readers recognize similarities in case study details and find descriptions that resonate with their own experiences, they consider whether their situations are similar enough to warrant generalizations. Naturalistic generalization invites readers to apply ideas from the natural and in-depth depictions presented in case studies to personal contexts. (2010, p. 191)

Flyvberg similarly notes that when he writes up case studies, he

> demur[s] from the role of omniscient narrator and summarizer. Instead, I tell the story in its diversity, allowing the story to unfold from the many-sided, complex, and sometimes conflicting stories that the actors in the case have told me …. . In this way, I try to leave scope for readers of different backgrounds to make different interpretations and draw diverse conclusions regarding the question of what the case is a case of. (2006, p. 238)

Clearly, Woldegies is sensitive to the fact that his way of writing up for certain readers, such as the women in the women's organizations to which he disseminated the narratives, and to other stakeholders, had to be accessible so that they would be likely to find these "meaningful". Flyvberg, following Peattie (2001), suggests that in any case, when writing up a "dense case study", where one refrains from excessive summarizing/analyzing, "*this is more useful for the practitioner* and more interesting for social theory than either [supposed] factual 'findings' or the high-level generalizations of [some] theory" (2006, p. 238, my italics).

Flyvberg emphasizes that this does not mean that those involved in case study research make no claim to the "generalizability" of the research (2006, p. 226). (Lincoln & Guba, 1985, p. 219, call this *transferability* to other contexts.) When speaking of "generalization", Woldegies likewise argues in his dissertation, following many authors writing about the qualitative tradition of research (of whom he cites Creswell & Plano Clark, 2007; Johnson & Onwuegbuzie, 2004; McMillan & Wergin, 2010), that "the degree to which results can be generalized is determined by the translatability, transferability and comparability of the findings to other populations" (2014, p. 89). He comments in his "rationale for the research methodology" (2014, p. 88) that because the sample is small, what he hoped to offer, via the research, was an in-depth way of

> understand[ing] women's experiences, each of which might be unique. The purpose is to find examples of how opportunities can be created and barriers crossed, which can be inspiring to other women, family, policymakers and other stakeholders. (2014, p. 89)

Woldegies thus asserts that one of the purposes of the research was to locate inspirational examples which can serve to stimulate action (on the part of the women involved as well as other women, family, policy makers and other stakeholders). In formulating this expressed purpose, he veers toward an inclusion of the criterion of catalytic validity to judge the worth of the research, as I explored in Chap. 1. In a nutshell, he wishes the significance of the research also to be tied to its being inspirational and stimulating future "action" on the part of a variety of practitioners, by their gaining increased insight into "how opportunities can be created and barriers crossed".

By focusing on the inspirational aspect of his research, he seems to be suggesting that any theorizing around women's empowerment should be what Gergen (1978, 2015) calls "generative"—that is, forward looking—in order to stimulate people's imagination of how indeed "opportunities can be created and barriers crossed". I return in Chap. 7 (Sect. 7.3) to how we might regard responsible generative theorizing as being at play in the research.

What I would like to share at this point is his conceptualization of the women in his sample's sense of empowerment and his understanding (in relation to their accounts) of the need for empowerment at institutional levels (what Dzapasi, 2015, p. 33, in another context, calls grooming a culture of empowerment). The understanding that he offers has been gleaned from the women's narrations and from the focus group discussions, as well as from his visiting of entrepreneurial sites, and from being familiar with policies. He avers that:

> The interviewees feel empowered because IGAs enable them to have equal opportunities with men. Currently, the women have equal decision making power at home with their husbands on matters concerning the family; most of them have been participating in "kebele" and "Woreda" level councils; they developed self-confidence in expressing themselves to actively participate in the community conversations. Their active participation in socio-economic activities inspired other women to follow their way to enhance the participation of women, unlike previous times when males were the only decision makers at home, "kebele", or "woreda" levels. (2014, p. 114)

Regarding participation at "kebele" and "woreda" levels, he states that:

> The "kebele" Women's Association, Women's League, Women's Federation, and Youth Association are working to ensure that "kebele" level decisions consider the benefits and equality of women. The women's concern organizations also have an umbrella organization known as the Women's Federation. The Women's Federation advocates and mobilizes the society to ensure gender equality. (2014, pp. 115–116)

Here Woldegies urges (also based on his noticing that in Gashena, the women's efforts at collective organizing were weak and fragmented, to the detriment of their felt empowerment) that women's empowerment is linked to their organizing in various associations and organizations.

Alongside these experiences of women feeling more empowered by virtue of their economic, social and political involvements, Woldegies refers to the importance of shifts at institutional levels toward being supportive of women:

> Regardless of the number of negatively reinforced sayings and proverbs still spoken in the society, recently there has been a slow change so that the community has started uttering proverbs that praise and admire women—which shows that there is a remarkable shift in the new generation in my community. This is the result of a number of interventions based on government policy and institutional arrangements that have been advocated by NGOs, the international community, the media, the church, and government structures like the Women, Children and Youth Affairs Offices and various women's concern organizations. (2014, p. 176)

Again these statements of his are inspiring, in that he points to ways in which cultures of empowerment can be generated.

Magnat states that the purpose of research as a ceremony is to contribute to "activate, sustain, and revitalize relationships to others, to the entire community and to the world at large" (2012, p. 168). One of the ways in which research can indeed contribute to "the world at large" is by offering what may be regarded as fruitful theorizing, which can serve to "revitalize relationships", such as, gender relations, by showcasing examples of how connections between people can be mutually supportive. Magnat also refers to links to the natural environment (2012, p. 168): one could say in this regard, that Woldegies's reported-upon research around women's empowerment indicates a way in which such empowerment can, in turn, impact on generating options for sustainable development and revitalization of human-nature connections. In Chap. 6, I focus more on the question of advancing "environmental justice"; but for the present I now turn to briefly consider Ssali and Theobald's research (2016).

4.3 Using Life Histories to Explore Gendered Experiences of Conflict in Gulu District, Northern Uganda: Implications for Post-conflict Health Reconstruction

Ssali and Theobald (2016) offer an account of their research using life histories to explore gendered experiences of conflict in Gulu district, Northern Uganda, with a view to spelling out the implications for post-conflict health reconstruction in the district. The research was financed by the Department for International Development (DFID) in the UK, which funds the ReBUILD Consortium, which in Uganda, is led by the Makerere University School of Public Health. Ssali and Theobald refer to their links with REBUILD as follows:

> Central to ReBUILD's operation are the networks with policy makers (local and national).[18] ReBUILD researchers are encouraged to compile a popular version of their research findings for uptake into policy. From these life histories, a number of policy briefs,

[18]When I sent the draft of this chapter to Sarah Ssali (and to Sally Theobald in October 2016), Ssali made the comment (10 October 2016) that upon reflection, the word "international" would also be fitting here "given the international nature of the consortiums and involvement with DFID UK". (Sally had asked Sarah to respond to my chapter on both of their behalves.)

> blogs and videos were produced and shared with local, national and international policy makers. (2016, p. 95)

Ssali and Theobald provide details (to a more academic audience, 2016) of the manner in which the study proceeded and how they regard its justification. They explain that it was undertaken in the Gulu district in Northern Uganda, because it was one of the districts that bore the "brunt of the 20-year old northern war" (2016, p. 84). The participants of the research were chosen from four sub-counties within the district: Paicho and Unyama sub-counties, which represented the rural sub-counties, while Layibi and Bardege represented the urban sub-counties. From these, four villages were selected, thus creating a "rural urban mix" of participants (2016, p. 86).

In the study, 47 heads of poor households were asked to recall significant life events over the three key phases before, during, and after the war, and to mark changes in their households' ability to cope with household health care costs over the three time periods (pre, during and post conflict). Of the households, 26 were females and 21 were male household heads (p. 86). Hence both genders were well represented in the study.[19]

Ssali and Theobald explicate that the "life history" approach to interviewing in this study meant that they

> left it to the respondents to determine for themselves what events they considered significant and to share these with us. Similarly, the choice of what determines health, treatment and cost of care were left to the respondents to define and articulate: this was considered more empowering than choosing for them. Having told us their life experiences, they went on to give us their expectations of what good health care meant and recommendations for reconstruction. (2016, p. 86)

Ssali and Theobald suggest that by not interjecting into the participants' stories while they were telling them, they "ceded some of our power as researchers, to participants to tell their own story other than just reproducing knowledge as part of the post war research industry" (2016, p. 86). They felt that it could be empowering for participants to be able to express themselves without interruption, in telling their stories. The researchers' starting assumption was that people "live and relive their lives through narratives" (p. 96). They wished to credentialize the "depth, detail and nuance" of the participants' stories as offering insight into the war experiences and into post-war possibilities for reconstruction. And, as also advised by Cannella and Lincoln, they evidently did not aspire to feed into (and reinforce) research agendas where "the use and acceptance of knowledge is legitimated only as applicable to market perspectives" (Cannella & Lincoln, 2007, p. 318). Instead, they wanted the research to potentially contribute to a process of wellbeing and healing that could be transformative of life quality in the society.

[19]The Consortium also indicates its commitment to pay attention to "gendered implications of policy and expenditure" (http://www.rebuildconsortium.com/).

4.3.1 The Interview Process: Ceding Power as Researchers While Listening to Participant Narrations

Ssali and Theobald note that there is currently debate in the social sciences concerning "power relations in research, … and, who is best placed to talk for the researched and how best to represent their multiple and evolving views, experiences and perspectives" (2016, p. 82). They indicate that the life history approach (in the way in which they used it) enabled the participants to express their evolving views as they recalled important moments in their life histories and decided what to share with the researchers.

In similar vein, Ssali indicated to me (in communication via email[20]) that their use of the life history method in the interviewing process meant that

> people are [were] encouraged to reflect on their lives in a more detailed and reflexive manner than would be the case in traditional interviewing. …. . In our case, as respondents reflected on what worked and what did not work, they were able to come up with recommendations for themselves and policy makers, regarding health care access. (Personal communication via email, 8 March 2015)

A glimpse of how the interviews proceeded is offered by Ssali and Theobald (2016) when they communicate to readers of their article the ways in which participants indeed shared their stories with them. For example, when Ssali and Theobald elucidate the gendered nature of the experiences, they offer some extracts from interviews, two of which I reproduce below.

> … when this war begun he [her husband] had just got a job and started working in Kampala. From Kampala he was posted to Khartoum [Sudan] but me I came home in Gulu. … It was a Red Cross job, so he stayed there for some time that we lost communication; he did not get in touch with me for a long time, for three years. I thought it was their kind of job, being field work he could be caught up where he could not communicate, but later on some man came from Khartoum and told me that my husband had died in Khartoum. At that time we were here with my in-laws and his parents. … My brother-in-law started mistreating me and the children because there was no one to defend us now. … They [in laws] became so greedy for the things that we had come back with from Kampala saying it was their brother's. So they started to remove the things from me. I tried to resist but being a man, he took it by force so it became too much for me. So, it was hard that I went to the office of the Red Cross in Gulu for help. I told them what I was going through and they asked about my home and where I come from (54-year-old woman). (Ssali & Theobald, 2016, p. 88)

This woman's reference to the fact that the brother-in-law "being a man" was able to take things by force, points to patterns of inheritance in the community where, after the man dies, the woman may not have a claim to what is considered to have been her husband's (see also Armstrong, 1992, who points to similar patterns across Southern Africa). The case underscores the vulnerabilities that this participant

[20]This was part of the process of my inviting texts for the special issue of the *South African Review of Sociology* which I was guest editing—and deciding whether submission would be suitable for the general or special issue.

experienced in this regard by virtue of "being a woman". It is this that ultimately, according to her story, rendered her destitute. But Ssali and Theobald also refer to the vulnerabilities experienced by some of the men:

> By 1989 my life was not easy … my wife had passed away, following my parents … I was just forced to marry … I went to the camp in Acet here in Gulu district, we were forced to go there by the government. 1995 was a difficult year. … It is painful to talk about death in this discussion … [Silence] In around 1995 my son died when he was abducted by the rebels … He was only 15 years old. We had just stayed for a year at Acet Camp. In that same year [1995] I also lost my brother (56-year-old man). (2016, p. 89)

This interviewee indicated how he is expected to be strong and to "withstand everything" by virtue of being a man (2016, p. 89). From the above extracts, we can see that the interviewers encouraged the participants to express themselves in depth. In the second extract, the participant indicated that he found it painful to speak about the events that he was recalling. Ssali and Theobald note that a silence ensued when the man stated, "it is painful to talk about death in this discussion".

In their article, Ssali and Theobald do not cover the question of how to deal with interview situations where people are recounting traumatic events. In my chapter on ethics (Chap. 8), I offer some further deliberations around this. It seems that in the case of this study, the interviewer here did not wish to interrupt the participant—hence there was no interjection at the point of silence in the interview extract above. Perhaps the participants in the study realized that they had a sympathetic listener and perhaps they did indeed feel "empowered" by virtue of being able to tell their story without interruption. Shumbamhini (2005, p. 2) states in this regard that from her experience as a pastoral therapist who had "assisted a number of widows therapeutically", she recognized "the need of widows to have their voices heard" (and hence encouraged this in her research entitled "storying widowhood in Shona culture"). However, Shumbamhini also tried to generate (based on her experience) what she calls a "therapeutic conversation" with research participants (2005, p. 12). This became part of her involvement in trying to help the participants—in this instance, widowed women—to come to terms with their felt experiences. She notes that she handled this process by recognizing that she was a "narrative researcher in training" and she therefore sought feedback from participants: "As a narrative researcher in training, feedback from the participating widows enabled me to know what sort of therapeutic interaction is helpful and what is not" (2005, p. 15). (She also cites White, 1991, p. 37 in this context.)

Perhaps the interviewers in the case of Ssali and Theobald's study could have made some remarks in interaction with participants that could have been experienced as "helpful", or, at some stage, could have advised on various options for seeking counselling of which the interviewer might have been aware. When I asked Ssali and Theobald via email (October 2016) if they would like to make comments on this draft chapter, especially on this part of the text, Ssali responded that

> To get ethical clearance, we had to promise to handle adverse events of this nature arising from our interviews. We had a list of counselling agencies in Gulu district. However, we never used any because we did not see the risk as being too adverse and requiring this.

> Otherwise it had been factored in in the study preparation and in securing ethical clearance. (pers. comm. via email, 10 October 2016)

From Ssali's perspective then, the researchers did not find that the risk (of harming participants) required that the counselling agencies from their list needed to be brought in for any of the cases, although this risk had been accounted for. I would suggest in this regard, following the cue of Shumbamhini (2005), that perhaps by asking the various interviewees at the end of the session how they had experienced the interview, including the relationship with the interviewer, might have been an opening for a more "personal" discussion around how the pain that they had expressed might have been dealt with (and what the participant might have expected from researchers listening to their traumas). Ssali and Theobald's article does not touch on whether the researchers perhaps felt uncomfortable in separating out their "research" role from a more personal encounter and palpable expression of caring, with participants recounting traumas at the moment of the interview, or how participants possibly felt about the way in which the interviews were handled.[21] (I discuss some further angles on ways of considering researchers' mode of engagement with participants in Chap. 8, Sect. 8.3.3.3.)

4.3.2 *Analysis (and Attendant Recommendations): Highlighting Gendered Experiences While not Exacerbating Gender Stereotypes*

Taken as a whole Ssali and Theobald state the following:

> From the different narratives of war illustrated in the life histories above, it is clear that people experienced suffering, death, vulnerability, humiliation, poverty, starvation and variations of helplessness. This suffering was not reduced to one gender. (2016, p. 91)

By examining during their analysis each of the participant's stories, they were aiming to "highlight gendered realities in a given context without exacerbating gender stereotypes" by showing that "both women and men experienced gendered violence, suffering and extreme violence", and by suggesting that this needs to be taken into account in "the post conflict reconstruction effort" (2016, p. 96).

Based on the life history accounts as offered by participants and on their suggestions for reconstruction, Ssali and Theobald identify a number of areas of which

[21]Cram (2009, p. 316) indicates that in their post facto reflections on research interviews conducted with Māori families that had lost a baby to Sudden Infant Death Syndrome (SIDS), Edwards, McManus, and McCreanor (2005) emphasized as part of their reflections "the important role played by the Māori SIDS Prevention (MSP) care workers who were present during the interviews. The MSP care workers were on hand to offer expert support and comfort to participants during and after the interviews, and their presence helped participants maintain their composure while talking about this grief-laden topic".

they believe "those constructing the health care system need to be cognizant". I summarize these below.

Firstly, they suggest that it is crucial to invest not only in hardware, but also in "software". They state that:

> In northern Uganda, the post war effort has arguably paid much more attention to the hardware of health infrastructure (building clinics) over the software of health approaches (supporting and retaining human resources for health and ensuring appropriate training to best support women, men, girls and boys traumatized by war). (2016, p. 92)

They clarify this as follows:

> However, the experiences and issues raised in the life histories clearly illustrate the need to go beyond constructing facilities. The post-conflict health intervention needs to be cognizant of what people suffered, how they suffered, their framings of suffering and best approaches to address pain and suffering. This calls for preventive and curative services, within both communities and health centers to enable community healing. (2016, p. 92)

Having underscored the need for preventive and curative services and centers to enable reconciliation and "community healing", this leads them to the next requirement, namely the importance of "stepping up" mental health services. They note that:

> Most post-war health interventions focus on rebuilding health services and the immediate causes of disease and mortality: there has been a lot of attention paid to curative and maternity services. … However, the accounts of many who experienced the war call for more attention to be paid to mental health services. (2016, p. 93)

They refer back to the 47 life histories, where they found that:

> From the 47 life histories, the most enduring memories from the northern Ugandan context were negative, ranging from death, to sexual and physical violence, sickness, suffering, impoverishment and humiliation. This raises the importance of mental health as a key component of post-conflict health reconstruction. (2016, p. 96)

They also suggest that participants' references during the interviews to their feeling "'foolish','emptiness', suffering unexplained joint pains, body weakness or illnesses that medics could 'not see' were indicative of people *somatizing distress*" (2016, p, 93, my italics). On these grounds they argue that "while the gains of maternal and preventive health are to be celebrated, there is need to step up mental health services" (2016, p. 93). They argue that this is especially the case, given that "there was never a truth and reconciliation commission" (as in South Africa after apartheid). (2016, p. 93)

They furthermore suggest that the current focus on women's health may be one-sided. They recommend in this regard that, in addition,

> there is a need to pay attention to men's health care needs. Most literature assumes only women have war-related health care needs, and this in turn underpins the emphasis on maternal and child health services. But the life histories illustrate the multiple impacts on men's health too. In addition to physical illnesses, such as bladder infections, HIV and wounds, there were clearly articulated psychosocial needs arising from the suffering and humiliation they suffered at the hands of different groups of combatants. (2016, p. 93)

They also comment on the problem of health care access:

> While the health care system has both public and private providers, money is required to purchase medicines and other requirements, even in public facilities. While the government is committed to providing free primary health care in its many facilities, these tend to be basic and drugs are often in short supply. (2016, p. 92)

Meanwhile, the facilities that are particularly "well stocked and with enough health care workers are the private, fee charging diagnostic facilities that flourished after the war" (2016, p. 92). Ssali and Theobald therefore suggest that to cope with the costs of seeking care "improved livelihoods are [one] key to health care coping" (p. 92). They indicate that presentations made by them ensuing from the research might also have been helpful in that "presentations regarding the need for sustainable livelihoods at household level, as a strategy for improving seeking health care, also attracted interest from the district leaders" (2016, p. 95).

In conclusion, they propose that a national dialogue that will also enable people to heal is called for. They emphasize that:

> In many of the life histories, the narratives of suffering were tied to their identity as a people, as Acholi people. Because the war was localized in the Acholi sub region, being the basis of the resistance struggles by both Alice Lakwena and Joseph Kony, most people associated their suffering with being Acholi. …. Therefore, for people to heal, there is need for community counseling, to take them through what they suffered, to show them that what happened to them could and has happened elsewhere and is not linked to an Acholi identity. This could be followed by a national dialogue on war, conflict and it's after effects. If put together with other wars that have occurred in Uganda, it would help the people of the north to know that what happened to them was not restricted to them. (2016, pp. 93–94)

Here they offer a suggestion for setting up a process for national healing as a way of helping those involved in the war (and in other wars in the country) to heal.

4.3.3 Dissemination to Stakeholders: Taking Some Responsibility

Ssali and Theobald did not initially plan to include in their (2016) article the question of to whom they were to address their "recommendations" offered above, other than to say that "those constructing the health care system" would do well to take heed of the recommendations. However, in some personal communications with Ssali in March 2015 (as part of our discussion about her potential submission to the *South African Review of Sociology*), and in response to reviewers' and my comments after the article had been submitted, the authors included more detail. In a personal communication via email in March 2015, Ssali provided the following information when I asked about this question:

> The constructed picture [of the various participants' lives] was shared with key informants (largely health care providers and district leaders) both during interview sessions and validation meetings. In the last validation, I presented these visuals to show how trajectories

> in people's lives affect their access to health care. The aim was to clearly make the message that health care reconstruction has to go beyond health to social reconstruction, an idea the policy makers bought and echoed throughout the discussions.

Somewhat in line with the idea of catalytic validity to which I referred in Chap. 1, Ssali suggested in this email that she had presented the visuals to policy makers as part of the "validation" of the research, as she had wished to make a clear message (based on the shared life histories) that health care reconstruction needed a wider social reconstruction. She went on to mention in her email that she and her co-author were currently "developing policy briefs around this to share with the ministry of Health and other relevant stakeholders" (March 2015); and this indeed then was shared with readers in the 2016 version of the article.

The fact that the authors were developing policy briefs means to me that they did not see the work as "done" after that the participants' stories and attendant recommendations had been recorded (in some report). Ssali mentioned to me (in the same email communication), that part of the value of the life history approach as used in this research was that the participants "reflected on what worked and what did not work, they were able to come up with recommendations for themselves and policy makers, regarding health care access". The process was geared so that they could reflect on this—and so that their reflections could, in turn, be conveyed (with the help of Ssali acting as one of the mediators) to "those constructing health care policy". I would argue that the responsibility taken in this regard by Ssali and Theobald, in conjunction with participants, is therefore well included in their article (2016). (I elaborate on this point in Chap. 10, Sect. 10.5.2.)

4.3.4 Noting Challenges that Need Continued Discussion

Ssali and Theobald conclude their article by stating that:

> The nuance and depth of life histories bring challenges that need discussing but also bring opportunities to inform the post-conflict reconstruction effort from the perspectives of women and men. Prominently it raises the need for us to focus on the software of health systems … to go beyond a focus on the physical rebuilding of health infrastructure. (2016, p. 96)

Based on, and extrapolating from, their discussion of the "software" involved in post-conflict reconstruction, it seems that they considered the research as a vehicle, through continued discussion around "results", to open deliberations in the public arena on health care (as care) rather than just on "infrastructure", so that systemic reconstruction could be geared toward comprehensive transformative healing. Such healing (as interjected by critical reader Francis Akena in this text, July 2016) could also be geared to accommodating community spiritual world views. Akena adds that:

> Among most African communities, the question of spirituality with reference to healing from traumatic experiences positions the community at the interior of two worlds: universal and metaphysical worlds. This signifies recognition of the present generation and departed ancestors' contributions as being cardinal in the comprehensive healing process of the land, people, other living and nonliving things because violent war has adverse ramifications on both. Among the community of northern Uganda especially the Acholi people, it is a question of embodied community identity in which the dead are present among the living and play an important role in the healing process from stressful experiences.

4.3.5 *Ethical Approach and Defining the "Veracity" of Life Histories as Expressed*

Overall, Ssali and Theobald argue that it was part of their ethical approach, following Mertens (2012), to try to be

> sensitive to people's cultures, power relations and building relationships of trust between the researchers and the researched …, and also paying attention to exploring how the research participants construct knowledge and what the significance of the constructed meanings is in the given context (Mertens, 2012). (Ssali & Theobald, 2016, p. 82)

They believe that by paying this attention, they could be argued to have organized a research process which could be used to "challenge the status quo and promote social justice", as defined by Mertens (2012).

As far as the "veracity" of the life stories goes, they admit, following Atkinson (1995), as cited in Shacklock and Thorp (2010), that life storying (as undertaken by research participants) "tends to be selective, contingent upon remembered events that are amenable to being told, be they fact or fiction" (2016, p. 83). They also cite Goodson and Gill's (2011) distinction of "life as lived and life as told". They comment in this regard that "how the story is told, then is at the discretion of the individual respondent" (2016, p. 83).

For them, whether the story as narrated by participants is fact or fiction (or something in between) is not the issue at stake: the issue is *how participants remember and decide to recount their life stories and its current impact on them (and how they see possibilities for ways forward*). This would concur with my suggestion proffered in Chap. 1 in interpreting Mertens' argument about what "realities" are being discovered/generated via a transformative-oriented approach. As I see it, firstly, participants need not be regarded as having static views to be "uncovered" (as Ssali and Theobald also note when they refer to the participants' "evolving views"). Secondly, views become generated during interview encounters and other conversations—and as Cannella and Manuelito indicate (2008, p. 55), *reality is in the process of being formed*. Therefore, the distinction between life as lived and told is not important for researchers researching with participants (co-researchers) how participants now (in the interview moment) wish to speak about their lives and possibilities for personal and social reconstruction.

4.3.6 Generative Theorizing

Ssali and Theobald (2016) do not use the word "theory" or "theorizing" in their article, but they do speak of the importance of building an understanding of possibilities for health reconstruction based on their interpretations of the import of the "experiences and issues raised in the life histories" as told/relived by participants involved in the 20-year conflict. They indicate that their study is set in a political context in which

> questions of legitimacy emerge regarding who rebuilds the health system, especially if those rebuilding it are foreigners. With what knowledge are they acting? And with what degree of sensitivity to the local context—are they simply transplanting standardized templates of post-conflict reconstruction or using templates tailored to context? (2016, p. 84)

The idea underpinning their approach was that it was crucial not to encourage a process of transplanting standardized solutions for rebuilding health systems, based on the kinds of knowledge that may be brought to bear by "foreigners" to the context. The idea was to generate knowledge/insights that could be specifically helpful *in this context* in terms of what was signified to be important for the participants with whom they interacted in depth during the study. Because Ssali and Theobald's study involved a relatively large sample of 47 participants, with in-depth life history interviewing so that experiences told in each case indeed had depth, they believe that the life histories as told could be argued to be "representative" of the voices of the participants and of the broader population in the (Acholi) region who experience the war. They therefore feel justified in making inferences relating to the Acholi community and that offer recommendations for action to those involved in health reconstruction in this region.

In their analyses of issues raised by participants, they have tried, as Cohen, Manion, and Morrison put it, to "keep text and context [i.e., the context in which words appear in participants' narrations] together" (2011, p. 553), while also illuminating how the stories as told express gendered life histories. What they tried to accomplish in this "analytic" process was to "highlight gendered realities in a given context without exacerbating gender stereotypes" (p. 96). Their theorizing around gender and how it intersects with other social forces which influence life histories (such as class/experiences of poverty), was based on their trying to "present the participants' stories while not 'othering' arguably vulnerable groups" (2016, p. 95).

They analyzed the life stories (admittedly interpreted by them) with the intent of creating contextually relevant recommendations for the purposes of policy making and of "revealing" the following to wider audiences (such as those reading their article):

> Both women and men experienced gendered violence, suffering and extreme violence; and we have highlighted this in our analysis but in particular showed the experiences of male vulnerability as these are largely absent in discourse, analysis and the post-conflict reconstruction effort and addressing this is arguably key to both women and men's health. (2016, p. 96)

They thus proceed to offer a counter-narrative to what they see as current discourses in health reconstruction in Uganda, and also in other contexts (where they refer critically to health reconstruction in line with "foreign" templates). Ssali and Theobald indicate that they interpret the transformative paradigm as laid out by Mertens (2012) as, an approach which, inter alia,

> calls for the researcher(s) to investigate meanings from the view point of the researched, exploring how the research participants construct knowledge, and what the significance of the constructed meanings is in the given context. It calls for the investigation of how this meaning varies across groups and how it is layered depending on the position of a given individual or group. (2016, p. 82)

What Ssali and Theobold did during their analysis was draw out, for instance, how the experiences of women and men in the war could be seen as gendered and as linked to (or layered in terms of) wider socio-cultural patterning in the social fabric. They cite the following example:

> For example, for women, widowhood compounded their war suffering, bringing upon them further loss of property, displacement and family disruption. Meanwhile, men highlighted their inability to take charge of their families and property as was customarily expected of them. (2016, p. 89)

Through their analyses of the life histories—as they portray them, and as grounded in participants' statements as made in narrating their remembered experiences—we thus get a glimpse of their theorizing around how "gender" intersects with, say, impoverishment caused by the war, and how this has differing implications for lives as lived by women and men. Women as war widows could in this social context be excluded from their husband's inheritance when greedy in-laws grab the resources; and meanwhile men as victims (a term used by Ssali and Theobald to highlight their victimhood) could feel humiliated by not being able to fulfil the social expectation of looking after their families. Ssali and Theobald's theorizing—which involves some deconstruction of views of *women* as being the primary victims in wars waged by *men*—is also meant to be forward looking (as Gergen, 2015 advises) in that they wish to undercut stereotypical thinking and indeed to point to possibilities for *social,* as well as health, reconstruction. In Chap. 7, I indicate how their theorizing (though implicit) can be said to have drawn on some retroductive inferencing, rooted in, but not confined to, an analysis of the stories as told.

4.4 Conclusion

In both the cases chosen for discussion in this chapter, I looked at/extrapolated the ways in which the researchers attempted to exercise some responsibility, in conjunction with participants, for using the research process in a way that would be empowering for participants. In both cases, the researchers recognize that life as lived and told is experienced differently by men and women, with women bearing much of the brunt of sedimented patterns of discrimination. But they also wish to

use the research process to stimulate possibilities of looking forward to "healthier" social relationships that express a more general wellbeing in terms of, inter alia, gender relations. The research initiated by Woldegies provides an example of what could be done through researching (with participants) women's participation in IGAs supported by family members and community processes. And Ssali and Theobald provide an example of how research might be responsibly practiced in the context of the traumas experienced by both men and women during the 20-year civil war in Uganda.

From the outset it can be said that the initiating researchers in both cases were cognizant that the way in which the research process was undertaken would make a difference to the continued unfolding of "events"—and could either reinforce deficit discourses or contribute to developing discourses that express possibilities for revitalizing life quality, including gender connections which surpass gender stereotyping.

Lincoln notes that research work can intentionally embrace "professional, personal, and political" stances in the quest to be "politically and ethically sensitive" (1995, p. 277). Such research, she suggests, would include

- a professional content (with researchers having some experience and skills that they bring to the research context);
- a personal dimension (based on personal concerns of those involved in the research, including the professional researchers); and
- a political dimension (which acknowledges that no research is neutral in its consequences).

Some readers may argue that the research work that I chose in this chapter in order to explore researcher responsibilities allowed too much lead on the part of the (professional) researchers to design the research process. Some of the authors whose work I discussed in Chap. 1, suggest that research participants should also play a crucial role in defining, for instance, the research topic, and what methods should be used and how. Others argue that the roles of professional researchers and research participants can become negotiated. It would be difficult to suggest that in the cases discussed above, the roles were fully negotiated. Nevertheless, it seems to me that the researchers tried to present themselves to participants as not having undue power to control the research process, and hence tried to shift the power relations via the research.

Redman-McLaren and Mills (2015, p. 6) suggest that those practicing research in line with principles as advocated by Indigenous research methodologists such as Chilisa (2012), Kovach (2009) and Smith (2012) should at least try to ensure that the research agenda is "agreed to" by research participants—even if the participants do not "determine" the agenda as such. In this way, they argue, the power differences between the researcher and co-researcher" still become reduced. What is important is that, throughout the research process, *attention is paid to the dynamics of power relations*.

My position on this is that professional researchers wishing to initiate a research process may need to decide in situ how much responsibility they should take in designing research that is likely to enable participants to indeed participate in co-exploring issues and reflect on possible recommendations for action. I would say that in both of these examples, the professional input of the researchers in setting up interviews (and FGD as in the first example) was fruitful in generating information/insight/reflections—and that participants recognized it as being helpful rather than restrictive. However, feedback from participants at various points could have been obtained, as part of sounding out with participants too how they were experiencing the relations with the professional researchers.

As indicated above, in the case of the research reported upon by Woldegies, the participants did at times make inputs into the research method(s)—for example, at member checking points, when Woldegies visited their entrepreneurial sites, they chose to make additions to their previously recounted stories and also additions on the level of suggesting that pictures of the equib plates should be taken as extra "data" about their activities. In the case of Ssali and Theobald, they state that they ceded power to the participants—but it would have been helpful to have had some indication of how the participants had felt in regard to whether the life history approach was deemed suitable to the task of exploring what was important to them. In any event, after the process, some feedback could have been sought and if any of the participants had felt that additional methods/ways of speaking could have been included, the researchers could have "gone back" to such participants.

What I have tried to draw out in this chapter from both examples is that in both cases, the researchers *did not shy from their responsibilities in recognizing that research processes can make a difference to participants' lives/conceptions, and that the way discourses are framed by researchers with research participants also makes a difference to stakeholders' and policy makers' considerations of the import of the research (insofar as stakeholders and policy makers become involved)*. Discourses become generated via the research processes (a process in which researchers are not innocent) and this needs to be borne in mind by those taking responsibility (admittedly with others) for research processes and products.

By my drawing out the various features of responsible inquiry that I read into the authors' texts (and by offering some suggested expansions), I have tried to illustrate how research can be practiced in a manner that is sensitive to the likely impact that its set-up and way of framing questions, etcetera, might have in the social fabric. In this way, I am hoping to inspire readers of my text to focus on a wider responsibility than professing to simply contribute to the "body of knowledge", as if "knowledge" can be treated as more or less neutral in content.

Before I close my discussion of the examples in the chapter I wish to make a final point about responsibilities as far as liaising with policy makers is concerned. In both cases discussed in this chapter, the researchers did try to make contact with some key policy makers. However, Minkler also cautions (2010, p. S85), citing Guthrie, Louise, and Foster (2006) that while community-based participatory research (CBPR) is directed at influencing policy making, "it is [often] difficult to sort out the distinct effect of any individual player or any single activity" in indeed

managing to influence policy decisions. She therefore suggests (following Guthrie, Louise, and Foster) that instead of asking the question "whether policy has changed" one can more fruitfully ask the following question: "How did the [partnership] work improve the policy environment on this issue?" (2010, p. S85). But she argues anyway that not all research claims to "translate findings into practice and policy", even though it could be considered to be desirable if research—especially CBPR—can work toward promoting change in policy or in policy environments" (2010, p. S85).

Woldegies and Ssali and Theobald made efforts to influence the "policy environment" by liaising with those whom they considered to be some key players. Of course, how much might be expected from (professional) researchers, is still open to discussion in specific research contexts—bearing in mind the fact that they are often part of academia with its manifold institutional demands (cf. Lincoln, 2001a, pp. 130–131; Gill, 2010, pp. 234–236). The examples discussed in this chapter provide what I consider to be inventive options for readers to reflect upon, in terms of expectations for inquirers to be more active than conventionally understood in taking into account the potential impactfulness in various areas (including the policy environment) of the research work.

References

Armstrong, A. K. (1992). *Struggling over scarce resources: Women and maintenance in Southern Africa*. Harare: University of Zimbabwe Publications.

Belmont Report: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research (1979). *Ethical principles and guidelines for research involving human subjects*. Washington DC: Office of the Secretary, US Department of Health and Human Services.

Cannella, G. S., & Lincoln, Y. S. (2007). Predatory vs. dialogic ethics: Constructing an illusion or ethical practice as the core of research methods. *Qualitative Inquiry, 13*(3), 315–335.

Cannella, G. S., & Manuelito, K. D. (2008). Feminisms from unthought locations: Indigenous worldviews, marginalized feminisms, and revisioning an anticolonial social science. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and indigenous methodologies* (pp. 45–59). London: Sage.

Castro-Reyes, P., Cooks, J., Drew, E., Edwards, K., Freeman, E., Isaacs, M-L., et al. (2015). *Research ethics reconsidered in the context of community-engaged research.* American Public Health Association (APHA) Annual Meeting and Expo, Oct. 31–4 Nov., APHA.

Catalani, C., & Minkler, M. (2010). Photovoice: A review of the literature in health and public health. *Health Education and Behavior, 37*(3), 424–451.

Charmaz, K. (2006). *Constructing grounded theory*. London: Sage.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., Major, T. E., & Khudu-Petersen, K. (2017). Community engagement with a postcolonial, African-based relational paradigm. *Qualitative Research*, 1–14. https://doi.org/10.1177/1468794117696176.

Chilisa, B., & Ntseane, G. (2010). Resisting dominant discourses: Implications of Indigenous, African feminist theory and methods for gender and education research. *Gender and Education, 22*(6), 617–632.

Cohen, L., Manion, L., & Morrison, K. (2011). *Research methods in education* (7th ed.). London: Routledge.

Cram, F. (2009). Maintaining Indigenous voices. In D. M. Mertens & P. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 308–322). Thousand Oaks, CA: Sage.

Delgado, R. (1989). Storytelling for oppositionists and others: A plea for narrative. *Michigan Law Review, 87*(8), 2411–2441.

Delgado, R., & Stefancic, J. (2001). *Critical race theory: An introduction*. New York, NY: New York University Press.

Denzin, N. K., & Lincoln, Y. S. (2000). *Handbook of qualitative research* (2nd ed.). Thousand Oaks, CA: Sage.

Deviance, Positive. (2004). A community based approach to solving community problems. *Positive Deviance, 1*(2), 1–8.

Dzapasi, A. (2015). Educational inputs for grooming a culture of empowerment. In O. Maguvhe & M. D. Magano (Eds.), *Disability in context: A socio-educational perspective* (pp. 33–46). Hampshire: Cengage Learning.

Flyvbjerg, B. (2006). Five misunderstandings about case study research. *Qualitative Inquiry, 12*(2), 219–245.

Gergen, K. J. (1978). Toward generative theory. *Journal of Personality and Social Psychology, 36*(11), 1344.

Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.

Gill, R. (2010). Breaking the silence: The hidden injuries of neo-liberal academia. In R. Flood & R. Gill (Eds.), *Secrecy and silence in the research process: Feminist reflections* (pp. 228–244). London: Routledge.

Goodson, I. F., & Gill, R. S. (2011). Understanding the life history research process. *Counterpoints, 386,* 35–54.

Holstein, J. A., & Gubrium, J. F. (1995). *The active interview*. London: Sage.

Holstein, J. A., & Gubrium, J. F. (2003). *Postmodern interviewing*. Thousand Oaks, CA: Sage.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.

Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.

Lincoln, Y. S. (1995). Emerging criteria for quality in qualitative and interpretive research. *Qualitative Inquiry, 1*(3), 275–289.

Lincoln, Y. S. (2001). Engaging sympathies: Relationships between action research and social constructivism. In P. Reason & H. Bradbury (Eds.), *Handbook of action research: Participative inquiry and practice* (pp. 124–132). London: Sage.

Lincoln, Y. S., & Guba, E. G. (1985). *Naturalistic inquiry*. Beverly Hills, CA: Sage.

Lincoln, Y. S., & Guba, E. G. (2003). Paradigmatic controversies, contradictions, and emerging confluences. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 253–291). Thousand Oaks, CA: Sage.

Magnat, V. (2012). Performative approaches to interdisciplinary and cross cultural research. In L.-H. Smith & A. Narayan (Eds.), *Research beyond borders* (pp. 157–177). Plymouth, England: Lexington.

Marshall, C., & Young, M. (2006). Gender and methodology. In C. Skelton, B. Francis, & L. Smulyan (Eds.), *Handbook of gender and education* (pp. 63–78). London: Sage.

Mathie, A., & Cunningham, G. (2003). From clients to citizens: Asset-based community development as a strategy for community-driven development. *Development in Practice, 13*(5), 474–486.

Mayaba, N. N. (2012). *Exploring the use of folktales to enhance the resilience of children orphaned and rendered vulnerable by HIV and AIDS*. Doctoral thesis, Nelson Mandela Metropolitan University, Port Elizabeth, South Africa.

Melrose, S. (2010). Naturalistic generalization. In A. J. Mills, G. Durepos, & E. Wiebe (Eds.), *Encyclopedia of case study research* (pp. 191–193). Thousand Oaks, CA: Sage.

Mertens, D. M. (2009). *Transformative research and evaluation*. New York, NY: Guilford Press.
Mertens, D. M. (2010). Transformative mixed methods research. *Qualitative Inquiry, 16*(6), 469–474.
Mertens, D. M. (2012). Transformative mixed methods: Addressing inequities. *American Behavioral Scientist, 56*(6), 802–813.
Minkler, M. (2010). Linking science and policy through community-based participatory research to study and address health disparities. *American Journal of Public Health, 100*(Supplement 1), 81–87.
Musyoka, M., & Mertens, D. M. (2007). Making research and evaluation our own: Ethical implications for strengthening the foundations for Africa-rooted and Africa-led inquiry. *Mosenodi, 13,* 104–113.
Peattie, L. (2001). Theorizing planning: Some comments on flyvbjerg's rationality and power. *International Planning Studies, 6*(3), 257–262.
Puebla, C. A. C., Faux, R. B., & Mey, G. (2004). Qualitative researchers—Stories told, stories shared: The storied nature of qualitative research. An introduction to the special issue: FQS interviews. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research, 5*(3), Art. 37.
Redman-MacLaren, M., & Mills, J. (2015). Transformational grounded theory: Theory, voice, and action. *International Journal of Qualitative Methods, 14*(3), 1–12.
Romm, N. R. A. (1997). Becoming more accountable: A comment on Hammersley and Gomm. *Sociological Research Online, 2*(3), Art. 2.
Romm, N. R. A. (1998). Caricaturing and categorizing in processes of argument. *Sociological Research Online, 3*(2), Art. 10.
Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York, NY: Springer.
Romm, N. R. A. (2014). Active and accountable social inquiry: Implications and examples. *Participatory Educational Research, 1*(2), 13–20.
Shumbamhini, M. (2005). *Storying widowhood in Shona culture.* Master's dissertation, University of South Africa, Pretoria.
Smith, L. T. (1999). *Decolonizing methodologies: Research and Indigenous peoples*. New York, NY: Zed Books.
Smith, L. T. (2012). *Decolonizing methodologies: Research and Indigenous peoples* (2nd ed.). New York, NY: Zed Books.
Speer, P. W., & Christens, B. D. (2013). An approach to scholarly impact through strategic engagement in community-based research. *Journal of Social Issues, 69*(4), 734–753.
Ssali, S. N., & Theobald, S. (2016). Using life histories to explore gendered experiences of conflict in Gulu District, Northern Uganda: Implications for post-conflict health reconstruction. *South African Review of Sociology, 47*(1), 81–98.
Tanggaard, L. (2009). The research interview as a dialogical context for the production of social life and personal narratives. *Qualitative Inquiry, 15*(9), 1498–1515.
Wang, C., Burris, M. A., & Ping, X. Y. (1996). Chinese village women as visual anthropologists: A participatory approach to reaching policymakers. *Social Science and Medicine, 42*(10), 1391–1400.
White, L. (2009). Challenge of research ethics committees to the nature of operations research. *Omega, 37*(6), 1083–1088.
Wilson, S. (2008). *Research is ceremony: Indigenous research methods*. Halifax, Canada: Fernwood.
Woldegies, B. (2010). *Organizational change project on women's entrepreneurship development program with nutrition diversification in rural Merhabete, Amhara Region, Ethiopia.* Unpublished paper. Yellow Springs, OH: Antioch University.
Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.
Woldegies, B. D. (2016). Research highlighting options for gender empowerment in Amhara Regional State, Ethiopia. *South African Review of Sociology, 47*(1), 58–80.

Chapter 5
Active Use of Experiments: Responsibly Inviting Participants and Others to Review Options for Agency

Abstract In this chapter I consider, with a focus on researcher responsibilities, two examples of research which involved experimentation as a research procedure (Stephens in Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, 2012; Oczak and Niedźwieńska in J Empirical Res Hum Res Ethics 2(3):49–59, 2007). In the first example, the research was aimed at trying to ascertain whether interventions which consisted of creativity training and emotional intelligence training for recidivists in Nigerian prisons (Lagos) had an effect on the creative thinking scores and emotional intelligence scores of participants. I draw out and extrapolate from Stephens' exposition the responsibility that he assumed, while also encouraging the responsibility of the recidivists to try to activate a potential for learning, while appreciating the limited life chances of prisoners before and after entering the prison system. I move on to discuss Oczak and Niedźwieńska's research—in which they offer an experiment to ascertain how their proposed new debriefing procedure following a deceptive experiment was received by the research participants. I explain how their debriefing was designed in an effort to generate beneficial and educative effects for participants (and others engaging with the research); and I underscore the importance of their eliciting participants' experiences of the research procedure as part of their study.

5.1 Introduction

In this chapter, I discuss two examples of research which involved different types of experimentation, each in its own way concerned with empowering participants in the study as well as others who might benefit from the study. I try to show with reference to these examples what might be involved in conducting experiments responsibly. When I look at the research examples, I offer my interpretations of the texts of the authors (supplemented with some personal communications with one of

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_5

the authors[1]), deliberately drawing out aspects of researcher responsibility that I feel can be extrapolated from the work. The purpose is to invite readers of my text to consider the options for responsible experimental research practice that I feel are exemplified (albeit sometimes only implicitly) in the examples as written up.[2]

The first example (Stephens, 2012) deals with the vulnerability of recidivists (prisoners/offenders who have been in and out of prison[3]) in two prisons in Lagos, Nigeria. The research—toward his Ph.D. thesis—focuses on setting up an experiment to ascertain whether the administration of creativity training (via Edward De Bono's "Six Thinking Hats" creativity approach) and "emotional intelligence training" (with reference to a number of authors in the field) had a positive effect on the creativity and emotional intelligence scores of these recidivists. I concentrate on the active role that Stephens fulfilled in trying to render the participants—and others reading his Ph.D. text—aware of certain options for agency, despite the dire conditions that the recidivists live under in the prisons and the wider social stigma as understood by Stephens. I elaborate on the various responsibilities that he assumed, while at the same time encouraging participants to participate in potentially self-transformative encounters, with implications for their ways of living. (His sense of responsibility and also encouraging others' responsibility was explained to me by Stephens more in personal conversation than is evident in the text of his doctorate. These conversations took place on 9 February and 18 November 2015, while he was in South Africa taking up a postdoctoral fellowship at Unisa.)

Stephens writes up the research as an exposition of his doing of science via experimentation. He indicates (2012, p. 147) that he wished to determine (at a 0.05 level of statistical significance) the effect of the independent variables—which he manipulated via his "treatment packages" (creativity thinking and emotional intelligence training)—on dependent variables (the creativity and emotional intelligence scores of the participants as measured by certain scales).[4] But he also at times admits that perhaps there were other factors at play—including the care that

[1]The communications were only with Oluyemi Stephens as I was unable to reach either Oczak or Niedźwieńska (the authors of the second example here). I communicated in-depth with Stephens while he was a postdoctoral fellow at Unisa (2014–2015), under the mentorship of Norma Nel.

[2]As indicated in Chap. 4, footnote 1, I do not believe that any interpretation of a text—or conversation—can refer to some "real" meaning, independently of the interpretive effort (cf. Romm, 1997, 1998).

[3]I use the terminology of "prisoners" and "prisons" as this is the terminology Stephens used (as used in Nigeria). In the context of South Africa, people who have offended are referred to as "offenders", and what were hitherto called "prisons" are called "correctional centers".

[4]Statistically this referred to the probability of not incorrectly accepting the null hypotheses that he constructed. He tested a total of five null hypotheses at a 0.05 level of significance (2012, p. 149). For example, his first null hypothesis was as follows: "There is no significant difference in the ideative flexibility score [a type of creative thinking score] of recidivists … in the two treatment packages 'Six Thinking Hats' and emotional intelligence training as compared to their counterparts in the control group" (2012, p. 179). The aim was to test (at a 5% risk of incorrectly accepting the null hypothesis) whether he could accept that "there existed a significant difference" in relation to the various null hypotheses (2012, p. 179).

he brought to the situation, which the prisoners understood. They would have understood this because of his previous and ongoing involvement in the prison in various capacities—such as being a (Nigerian) pastor ministering to them, and his involvement in an NGO, called Cottage of Hope, aimed at improving their welfare in the prisons.[5] *The recognition on Stephens' part that the participants might have been responding to his "person", leaves room for admitting that the researcher's approach, and the way he or she is perceived by participants are factors that may impact on the experimental results*.

My argument is that instead of treating researcher stances as confounding the results via so-called "experimenter effects", we can contend that it is more important that experimenters act responsibly, in recognition that *observed results may indeed be a product of the way the experiment is handled.* In this case, the experiment proceeded in such a way that a caring person (whom the recidivists understood cared for them) administered the intervention—namely the various trainings (with his caring for participants indeed being part of the intervention as I see it). Although the attitude that Stephens himself brought to the study is mentioned only briefly and momentarily toward the end of his thesis (while discussing recommendations, 2012, p. 186), I suggest that this, coupled with our personal communications (2015), indicates to me that he was aware that his specific way of handling the "treatment packages", and being able to solicit the recidivists' motivation made a difference to what was "found" via the study, in terms of the scores of the various participants as measured by the scales. I elaborate on the import of this in Sect. 5.2.4.

I explain how I believe Stephens legitimately recognized as part of his research role, that he was assuming some responsibility (with the recidivists) for the way in which the research could engender empowering impacts. When I asked Stephens about his sense of responsibility, he stated that it was crucial to have a "mutually beneficial relationship" with the participants so that they would feel they were benefitting from the study (18 November 2015). As I understand it, therefore, he was trying not only to theoretically examine the effectiveness of creativity and emotional intelligence training for recidivists (with implications for reducing rates of recidivism), but was hoping to empower them to think and feel differently, with less despair while in prison and more of a sense of how they might act once out of prison. In his thesis write-up of his findings (which I would say emerged as a product of his interaction with the recidivists), he makes a brief reference to empowerment when he states that "acquisition of various creativity skills such as the Six Thinking Hats also could be described as fostering positive life skills, which empower an individual to live a productive life" (2012, p. 182).

He also wanted to reach a wider audience via the study through his Ph.D. thesis, so that it could potentially impact widely across the globe. He explained this to the

[5]He indicated to me that the Cottage of Hope works mostly with personal funds from the board of trustees (some of whom are dedicated and some are less dedicated). The board of trustees and friends are the source of the funding.

participants. He told the recidivists that the thesis could "get to any part of the world because we are living in a global village" (pers. comm., 18 November 2015). And he was personally hoping that it would reach and "move" the target community to whom he felt specific allegiance as a trained counsellor himself—the community of counsellors across the globe. He was aware therefore that the discourses that he brought to the experiment and to his interpretation of results could be impactful and that how he framed "the problem" was not going to be neutral in its consequences. I discuss the implications of attention to framing of issues and of discourses by relating this to my discussion in previous chapters regarding the importance of trying to desist from reinforcing "deficit" discourses via the research process (e.g., the notion that recidivists have little potential to change their ways of thinking and feeling, as indeed was expressed to Stephens by some of the prison officials who told him that he was "wasting his time"—pers. comm., 18 November 2015).

I then turn to Oczak and Niedźwieńska's (2007) experiment, which was conducted at a Polish university. Oczak and Niedźwieńska sought to examine how, as a researcher, one might be able to mitigate against or remedy the possible harm or experience of uneasiness for participants who have been involved in deceptive psychological research (as reported upon by, for example, Brody, Gluck, & Aragon, 2000; Ortmann & Hertwig, 2002). In deceptive psychological research, research participants are told only later what the research was about, because it is considered that if they had known earlier, this would have affected their responses. Explaining to participants post facto the purpose of the experiment is known as "debriefing". Oczak and Niedźwieńska's experiment was geared to "examine the effectiveness of a new debriefing procedure designed specifically to address possible negative consequences of participation in deceptive research" (2007, p. 49). These researchers wished to examine if harm could be minimized and indeed if benefits could be accrued for participants involved in deceptive research (and for others concerned) if researchers *make an active effort* to try to ensure that the research becomes a learning experience via the debriefing process. (This is in line with the principle of beneficence as applied to participants and to the broader society.[6]) Since I was unable to contact either Oczak or Niedźwieńska, my extrapolations of their intention are based solely on my reading of their text.

Oczak and Niedźwieńska devised their new debriefing procedure to innovatively debrief half of the participants who had been subjected to a deceptive experiment, while the other half of the participants were debriefed in the "standard" fashion. The

[6]The Belmont report (1979), which I discuss extensively in Chap. 8, states that two rules need to be followed to comply with the principle of beneficence: The first is to "do no harm," and the second is to "maximize possible benefits [for individual participants and others in society who may benefit from the knowledge developed] and minimize possible harms" (http://learn.yale.edu/hsp/module_1/3_beneficence.asp). My interpretation of Oczak and Niedźwieńska's research is that they sought to consider how experimental research (especially research involving deception of subjects) could mitigate against potential harm and indeed create benefits for participants, while also contributing to knowledge about experimental designs for other researchers to take into account.

"standard" debriefing procedure amounts simply to explaining to the participants the true nature of the experiment—in this case, the mechanisms of persuasive suggestion that the experimenters had used (while ostensibly testing the participants' memories). The new debriefing procedure affords participants the opportunity to exercise their (new) knowledge about suggestibility mechanisms and how to avoid them, by inviting them to participate in a re-doing of the suggestibility test (2007, p. 51).

Oczak and Niedźwieńska's experiment was aimed at investigating whether participants who have been involved in a deceptive experiment and are debriefed either through a standard procedure or a non-standard procedure (their new devised one), experience their participation in the research differently (in terms of their mood, attitudes toward psychological research and self-appraisal after participating). The independent variable was thus the type of debriefing procedure (which varied between standard or non-standard), and the dependent variables were the mood, attitudes and self-appraisal of participants as measured by various scales.

What I find significant about the research is that the researchers were trying to see if participants' sense of agency against the suggestive power of the researchers' suggestibility mechanisms, could be increased by giving them the skills to avoid such power. If participants educated via the new debriefing procedure showed that they could more easily avoid suggestive cues, this result would be important to show that it is *possible to educate people so that their suggestibility from misleading cues on the part of "authorities" (or anyone trying to manipulate their responses) is diminished.* The research could thus be empowering and transformative for the participants who took part in the experiment (especially the ones who were subjected to the new debriefing procedure) and could offer hope too that possible tendencies to respond to misleading suggestive cues are alterable. *What this shows to me is that researchers can recognize that people need not be treated as "unchanged"* via *the research process*—and researchers can then consider their responsibilities in the light hereof. In the case of Oczak and Niedźwieńska's research I submit that they were trying not only to "find out" about prospects for diminishing suggestibility, but also to *empower the involved participants to recognize how they could respond differently* in the case of people wielding deceptive cues. (The experimental group was indeed afforded the opportunity to try out new responses, based on their learning from the debriefer.)

The researchers also offer some deliberations (2007, p. 52) around their asking participants to rate psychological experiments along various dimensions, including valuable/worthless; pleasant/unpleasant; meaningless/meaningful; important/unimportant (which was part of their experiment as "attitude toward [the] research" was one of their dependent variables). Clearly, the researchers wished to offer ideas for researchers regarding the worthiness of the new debriefing procedure (so that it can be used as an educative process from which participants can benefit) as well as the worthiness of seeking participant feedback on research participation. I offer further considerations along these lines while discussing their experiment.

I now proceed to examine the two research projects in some depth.

5.2 Researching Effectiveness of Six Thinking Hats and Emotional Intelligence Training in Enhancing Creative Thinking and Emotional Intelligence Capacity Among Male Recidivists in Lagos Prisons, Nigeria

5.2.1 The Set-up of the Experiment

This research, as explicated by Stephens (2012), was based on an experimental design, where participants—who were recidivists in two Lagos prisons and who agreed to participate in the study—were divided into three groups. The groups received different types of training (administered by Stephens) in both prisons. The first group (the first "treatment group" in the experiment) was given training across the two prisons in "creative thinking" (Six Thinking Hats training). The second treatment group received training in "emotional intelligence" (called EQ training). The third group in this study was meant to be a control group, as they were not given any training in line with the aim of the experiment, which was to determine the effects of the Six Thinking Hats training and emotional intelligence training on various dependent variables. They received "placebo" training in HIV and AIDS (2012, p. 115). All participants were asked to fill in various closed-ended questionnaires before and after the training, which were aimed at assessing their capacities in creative thinking (henceforward called CT) and in emotional intelligence (henceforward called EI). The design can be represented as follows (Table 5.1).[7]

5.2.2 Procurement of Participants and Research Procedure

The way in which Stephens procured the participants was to first obtain a list from prison officials of prisoners in the two prisons that were chosen as sites for the experiment (medium security prison, Kirikiri, and Ikoyi security prison). Prisoners were then categorized by the officials as non-recidivists or recidivists, and

[7]Stephens does not provide a table depicting the research design. However, in a table he supplies of the participants (2012, p. 127), he offers three column expressing the "personality type" of the 85 participants, namely Type A, Type B and Type C, as defined and measured in the psychological literature. He shows in the table how they were distributed in the various groups (the two experimental ones and the control group). The number of Type A personality participants were distributed as follows: twelve, twelve and ten in the Six Thinking Hats, Emotional Intelligence, and HIV training; Type B personality were distributed as ten, ten and eight participants in the Six Thinking Hats, Emotional Intelligence, and HIV training; and Type C were distributed as eight, eight and seven. However, I have not focused on this in my outline of the study. In Sect. 5.2.4 I return briefly to this aspect of the study.

Table 5.1 A depiction of the experimental design

Group	Participants	Scales administered pre-treatment and post-treatment
Treatment techniques (independent variable: type of training)	Recidivists in two Lagos prisons (medium security prison, Apapa, Kirikiri; and Ikoyi security prison, Ikoyi)[a]	Ibadan Creativity Assessment Scale (to measure hypothesized dependent variable of CT); and Akinboye EI Test (to measure hypothesized dependent variable of EI)
Group 1: Six Thinking Hats training	30	
Group 2: EQ training	30	
Group 3: Control group: "Placebo" training in HIV and AIDS	25	
Total	85	

[a]These were all males as there was only one female recidivist in the selected (nearly all-male) prisons, and she declined to participate. The experiment thus involved only male recidivists. Stephens later assessed (via a personality test) the "personality types" of the participants in all of the groups and found that they were more or less equally distributed across the groups. See footnote 7

subsequently certain recidivists were randomly chosen from a "random numbers table" to be invited for participation in the study. Stephens explains as follows:

> A list of all prison inmates was obtained from the record officers of each of the selected prisons. From the obtained list from the record officers, prison inmates were categorized [by the officials] into recidivists and non-recidivists. A random number table was prepared with the names of identified recidivists such that any inmate (recidivist) that falls within a specified number was selected for the study. (2012, p. 118)

The recidivists who were identified in this way in each prison were then invited to a hall so that Stephens could introduce himself and the research. Actually, the prisoners knew Stephens from his previous involvements in the prisons as a religious minister, a professional counsellor and the CEO of an NGO called Cottage of Hope—which assisted them in various ways (e.g., in arrangements for their schooling and in distance education university training). After a brief introduction about himself, he explained what this research was about. He indicated that he wished to conduct an experiment. The experiment, he explained, was aimed at finding out whether the provision of creativity and EQ training would increase the recidivists' CT and/or EI capacities (compared to a control group that would be offered training only in HIV and AIDS). He explained that this would mean ascertaining, via a questionnaire, which they would have to fill in, their scores regarding their CT and EI capacities both before and after they had undergone the training, as well as characteristics of their personalities. He told them that the results of the study would indicate with some confidence whether recidivists could be said to benefit from training, and if so, which type of training seemed to help better in

which respects. He explained that the research was toward his Ph.D. thesis, a thesis he hoped would be disseminated in other parts of the world because the world is a "global village".

Having explained that no-one should feel obliged to take part in the study and also that they could withdraw from it even if they had initially participated (and that their names would not be recorded in any reporting), the individuals were afforded the opportunity to decide whether or not to remain in the hall. Stephens estimates that in Kirikiri prison, about 30 recidivists left the hall and in Ikoyi about 20 left the hall (pers. comm., 18 November 2015[8]). In each venue, participants were assigned to the three groups (i.e., the Six Thinking Hats group, the EQ group, and the control group who would receive HIV and AIDS training) through balloting. Pieces of paper indicating the name of each group were rolled and dropped in a hat, and the participants were asked to pick one. The name of the group on the paper picked by the participants was the group that they would participate in.[9] Altogether, across both prisons, the number of participants totaled 85.

In the first week of the experiment, the pre-test scores were taken by asking all 85 participants before the training began to fill in questionnaires, which were designed to assess their initial CT and EI (as well as to assess their different personality characteristics). Stephens obtained the assistance of three research assistants to help with this task. He explains that:

> The services of the research assistants became necessary to assist in explaining the questionnaire to participants who may need further clarification and to guide on how the participants tick the responses for each of the item on the instruments. The research assistants who were graduate employees of the Nigeria Prison Services were trained on the rudiments of the study. (2012, p. 126)

The Six Thinking Hats, EQ, and HIV/AIDs training consisted of 12 training sessions across six weeks each (and was repeated in the two prisons). Each session lasted 1½ hours. The training in the prisons in each case took place in a classroom (2012, p. 126). A final week was used to obtain the post-test scores of the participants.

As far as the instruments to test the constructs of CT and EI were concerned, Stephens notes that for creativity assessments he made use of a scale called the Ibadan Creativity Assessment Scale (ICAS). The ICAS was devised by Nigerian researcher Akinboye (1976), and had been adapted by him from similar scales used by researchers in other geographical contexts. The ICAS was designed to tap—in a relatively simple manner—"certain creative patterns of behavior such as thinking, ability to generate new ideas, concepts and problem solving skills" (2012, p. 118).

---

[8]Stephens mentioned to me that one factor that might have caused them not to wish to participate was that some of them were not literate and may have wondered if they could participate in filling in questionnaires, etc., even though he mentioned that assistance would be available in this process (pers. comm., 18 Nov. 2015).

[9]When I sent Stephens the draft of this chapter in February 2017, he suggested (via an email reply) that this detail on the way of assigning the participants to the groups should be included.

The scale contains items in the form of "simple statements to which participants were expected to indicate on a five-point scale the extent to which he or she agreed with each statement" (2012, p. 119). Stephens did not administer the whole scale, but concentrated in the study on certain subscales which included the following:

- Section A: Ideative flexibility scale, which contained 21 items
- Section B: Ideative originality scale, which contained 25 items
- Section C: Ideative fluency scale, which contained 26 items
- Section D: Creativity motivation scale, which contained 17 items.[10]

The Akinboye Emotional Intelligence Tests (EQ Tests) developed/adapted by Akinboye from other similar scales were also used by Stephens (Akinboye, 2002).[11] To administer the EQ test, which was aimed at assessing EI, the participants were asked to read each of the statements (on the questionnaire) carefully and rate the items by circling the number that best reflected their feelings in relation to the statement (with options 1–5 being given as ratings). This test consisted of 40 statements. Apart from Stephens administering the ICAS and EQ tests in the pre-test week, they were administered post-test (after the training) in the eighth week of the study.

5.2.3 *The Training: Responsible Intervention*

Stephens indicates this was not the first time that he had tried in Nigerian prisons to organize "practical creativity and emotional intelligence education in remediating anti-social behavior of prison inmates" (2012, p. 7). In his master's thesis (in another prison, 2006) he had investigated this option. He also investigated with Badejo "the effectiveness of emotional intelligence training as an intervention strategy to remediate anti-social behavior of some prison inmates from dysfunctional homes" (2012, p. 8), as reported in Stephens and Badejo (2010). However, none of the studies was specifically targeted at trying to make the training relevant

[10]As far as validity of the Ibadan Creativity Assessment Scale (ICAS) is concerned, Stephens notes that Akinboye (1976) took various ideas from a number of authors (who could be considered as experts) in the field of creative intelligence testing, and designed simple items for measuring creativity using these as bases. The ICAS was tested on 200 participants in Nigeria. Akinboye also determined via Cronbach's alpha whether the items for each subscale of ICAS could be said to be tapping into the same construct by checking the degree of internal consistency between the items of the construct (Stephens, 2012, p. 124). Akinboye furthermore organized test-retest reliabilities with the 200 participants after a period of four weeks. That is, the tests were re-administered to the same participants to see if similar responses would be given. The test and re-test revealed acceptable reliability scores—as measured by Pearson's r (Stephens, 2012, p. 124).

[11]For the emotional intelligence testing, Akinboye (2002) also referred to authors in the field and adapted existing scales somewhat, while checking for convergent validity with the "Emotional Entrepreneurship Test" and for discriminant validity with the "Emotional Stress Test" (Stephens, 2012, p. 122).

in terms of reducing recidivism in Nigeria (2012, p. 8). This was the focus of the current study. He felt that this was important because, as he notes, referring to Messemer and Valentine (2004), the reduction of recidivism is "the long term goal of incarceration" (2012, p. 9).

Stephens entered the arena with the conjecture—which also guided his way of interacting with the participants—that, as he states,

> the two treatment package Six Thinking Hats and Emotional Intelligence training will equip the recidivists with skills which will improve their thinking ability, manage their emotions well, hence the ability to make good judgement, not to return to a life of crime, and this can make them useful to themselves and the society at large. (2012, p. 9)

He, in turn, took it as part of his responsibility to deliver the packages in such a way that they might indeed have some positive consequences for the participants and ideally for the society as a whole (considered broadly). That is, he geared the intervention to this end.

It should be mentioned that Stephens does not discuss in his account of the experiment his initial choice of intervention. It is not clear whether Stephens consulted participants about what interventions might be useful to them. While creative thinking and emotional intelligence were regarded as salient by Stephens, might the participants have chosen some other type of intervention if consulted? Or perhaps we can argue that due to his prior involvement with recidivists in the prisons as a pastor and counsellor, he was indeed being responsive to their considerations of what was important to pursue in the study. That is, he became aware that studying emotional and problem-solving skills was if interest not only to him but to the recidivists. What can be said is that there are of course ethical implications in the choice of intervention in experimental work, and ideally a responsiveness to participant concerns should be reflected in the choices. (This was pointed out as an important point to stress by an anonymous reviewer of this book.)

5.2.3.1 Six Thinking Hats Training Package

In his discussion of the "treatment package" which was aimed at creative thought training, Stephens notes that he drew on the approach to creativity developed by De Bono. De Bono himself emphasizes that people can be trained to be "more creative" (1982, p. 154). De Bono makes the point that "if one can develop skills in using information creatively, then one no longer has to rely exclusively on natural temperament" (1982, p. 212). Stephens took on board De Bono's advice (1985) on use of the Six Thinking Hats tool as a way to enhance creativity. In De Bono's schema, each of the Six Thinking Hats is denoted by color: white, red, green, yellow, black and blue. Putting on any of the thinking hats defines a certain type of thinking. Stephens considers the use of the Six Thinking Hats as being

> a perception-broadening creativity technique belonging to the creativity thinking framework group. … . This technique is specially designed to help people think by simplifying thinking so that a thinker is able to use one thinking at a time. It is a deliberate thinking

> technique aimed at liberating individuals and groups from confusion created by argument or doing all the thinking, emotional, information, logic, hope and creativity at once. (2012, p. 10)

The training on the Six Thinking Hats consisted of a build-up of sessions. In each session, Stephens advised the participants to take notes, and invited them to ask questions during the session and at the end (to which he gave responses). He also requested them to take part in homework in which they tried out the "thinking" that was being discussed in each session. He also indicates that in the first session, a contract was made with the participants in which they agreed that as long as they were part of the study, they would "co-operate fully, attend and be active in and out of the study. They would not miss any session. The participants were informed that they were free to opt out of the study once they felt like doing so" (2012, p. 128). Notebooks and pens were distributed to the participants. This was followed by an introduction about the Six Thinking Hats as developed by De Bono.

In session 2, Stephens explained to participants—with reference to De Bono's work—that "most people are used to poor thinking, hence the need to make conscious efforts to teach thinking". The participants were informed of the main aim of "Six Thinking Hats", which is to make the thinking process simpler, which in turn should be beneficial "to the thinker and the society at large" (2012, p. 129). He explained that as a thinking technique, Six Thinking Hats could be used on an individual basis or co-operatively in a group. Participants were afforded the opportunity to ask questions, and the various questions were answered. The participants were also given homework to do.

Stephens notes that in session 3, there was a brief discussion of the homework from session 2. However, he observed that "only a few participants carried out the assignment". He remarks that "those that did the assignment were commended and those who did not were encouraged to always do their assignment". (Stephens later explained to me on 18 November 2015 that if he had reprimanded the participants who had not done the homework, this would have demotivated them.) The meaning of the various Thinking Hats was then explained to the participants as follows:

- White hat—symbolizes the seeking of facts and figures or information
- Red hat—emotional expression
- Black hat—negative judgement, gloomy judgement, logical negativism
- Yellow hat—positive judgement, symbolizes brightness, hope
- Green hat—symbolizes productivity, seeking alternatives and creativity, generating ideas
- Blue hat—process control (thinking about one's thinking).

In this session, Stephens tried to highlight the "consequences of not considering any of the hats during a thinking process". He indicates that "the participants confirmed that they have never looked at thinking as a process and that going through this process will make taking decisions slower" (2012, p. 130). From this brief account, it appears that participants were already confirming that they were learning from the training—that is, they were learning to consider thinking as a process.

Sessions 4–9 were devoted respectively to the various colors of Thinking Hats (with participants and Stephens all wearing that color of hat, which were distributed at the beginning of the session). Stephens also tried to draw the participants into thinking how use of the hat under consideration might be valuable to them in considering "avoiding a life of crime". He notes that when he discussed the green hat with participants—namely, the thinking process that depicts productivity, alternatives, creativity and generating ideas—he again tried to show the participants the relevance of the hat to their situation. He tried to suggest via this hat that "there is always an alternative to any issue; this was used to debunk the view of some prison inmates who believe that there is no other way of life other than that of crime" (2012, pp. 131–132). He reports that "the participants showed their interest in the green hat as they identify the green hat as a possible way out of crime" (2012, p. 132). He also highlighted in the blue hat session that the idea was to use this hat to "arrange and monitor other hats, hence it could be referred to as the programming hat. The blue harvests the ideas generated by the green hat for use" (2012, p. 133). According to him, the participants recognized that this use of the blue hat was a tool for them to think about their thinking.

Session 10 was devoted to considering "merging of the Six Thinking Hats" (2012, p. 133). Stephens explains how he arranged this session:

> The participants were welcomed to another session; a review of the previous session was done. A diagram made up of the six colors in the "Six thinking hats" (Blue, White, Green, Red, Yellow and Black) was drawn on the board. The hats were also arranged on a big table. Efforts were made to summarize the roles played by each of the Six Thinking Hats. The opinion of the participants about the arranged caps was sought. (2012, p. 133)

Stephens remarks that the recidivists responded positively to this arrangement (2012, p. 133). Following this, the participants were requested to discuss "the likely effects, consequences and outcome of leaving out any of the thinking hats in the thinking process" (p. 134).

The purpose of the penultimate session (session 11) was to consider some further practical uses of the Six Thinking Hats. Stephens indicates that he arranged this session as a group session. He explains what happened in one of the prisons (Kirikiri) as follows:

> A combination of the blue hat (control) as well as the white hat was adorned and this statement was made [by one of the recidivists]: "political corruption is the cheapest/easiest way of making money". (2012, p. 134)

Below I present the totality of the way in which Stephens outlines that he proceeded to engage with the participants in the light of this statement:

> Some of the participants were asked [by Stephens] to put on the red hat and give the group red hat thinking.
>
> **Response**: Everybody is corrupt, there is nothing wrong in corruption, however (there were some negative feelings toward corruption but these were played down to bring out some things).
>
> A call for yellow hat thinking was made.

> **Response**: One can become a multi-millionaire in a day, all the big/rich men in Nigeria made their money through corruption, it's our money I should have my own share.
>
> A request for black hat thinking was made.
>
> **Response**: EFCC will catch up with you and prison will be your place of abode.
>
> The discussion was brought to a halt and a demand was made for the green hat thinking to consider alternatives.
>
> **Response**: One can make a difference in this corrupt ridden country: one can make money by identifying needs of people and meeting such needs that is, one can cultivate the habit of saving money, one can join a co-operative society to access cheap loan for business. (Stephens, 2012, pp. 134–135)

This reporting on session 11 in this prison ties in with Stephens's noting, in his (prior) theoretical discussion of his thesis, the importance of taking into account "environmental factors" when considering criminal behavior. He states that we need to recognize that "environment is one of the major contributory factors to criminality" (2012, p. 58). He notes that researchers in various geographical contexts have offered ideas about societal environmental factors, and some have referred specifically to the issue of corruption in society at large. As he puts it:

> Researchers have also identified poverty, education, parenting practices and family structure as family risk factors. Unemployment, illiteracy, affluence, greed, inequality in society, urbanization, political instability, corruption are other identified causes of crime. (2012, p. 58)

The issue of corruption also appears in one of the recommendations he offers at the end of his thesis. He poses this recommendation as follows:

> the government should also fight the incidence of corruption seriously as some prison inmates view the leaders as corrupt and if the leaders are siphoning our common wealth, they too should find a way of taking their own share by engaging in crime. On the other hand, less emphasis should be placed on materialism as wanton display of affluence in the midst of abject poverty could increase the incidence of crime and recidivism. (2012, p. 196)

This recommendation here suggests that Stephens was theoretically sensitive to the cultural climate in the society that might predispose people to crime (along with factors such as unemployment, poverty, etc.[12]). He comments that a culture of corruption combined with rampant materialism and high levels of inequality in a society can fuel criminal behavior (p. 196). This is consistent with the argument of Wilkinson and Picket in their book entitled *The spirit level*: *Why more equal societies almost always do better* (2009, p. 5). It is also consistent with McIntyre-Mills' argument that a global "corrupt market system" which in effect has "failed the majority of people and the planet" in the way in which it maldistributes wealth and concurrently damages our natural environment, has deleterious consequences in terms of both social and ecological justice (2014b, p. 114).

[12]When speaking about this factor, Stephens notes that Animasahun reported in 2011 that 75% of armed robbers in Abuja were university graduates belonging to the unemployed sector (2011, p. 2).

McIntyre-Mills argues that one solution to this is to set up narratives and practices which express a "post-consumerist world" where "wealth is re-evaluated and reconceptualized" in terms of human and planetary wellbeing rather than in material terms (2014b, p. 76). Regarding the question of unemployment and the emotions that can be generated by its, McIntyre-Mills refers to the emotional anger that can be triggered in the face of unemployment and poverty (2014a, p. 135). I return to these points in Sect. 5.2.7 and again in Chap. 6 (when I discuss McIntyre-Mills' attempts to contribute to the production of new narratives).

Stephens meanwhile also points to factors within the Nigerian prison system that have been located by a number of researchers (including himself), such as overcrowding in prisons, lack of basic rehabilitation infrastructure and demotivation of prison staff (2012, pp. 3–6), which are not conducive to rehabilitation. But with the recidivists, he tried to urge them to consider some angles of hope for what they might do, even in the face of the various "environmental factors". Interestingly, one of the options that arose during the group discussion in the Kirikiri prison was the idea of forming of co-operatives as a way of making money, with (some) recidivists opting not to think along the lines of contributing to "corruption-ridden behavior" to accumulate material goods (2012, p. 135). This is despite the negative role models that some of them observed to be abounding in the society.

In session 12, Stephens closed the training program on an approving note:

> The participants were commended for their consistency, cooperation and active participation thus far. A brief discussion on the homework took place, with the participants presenting what they had done. The participants were [then] asked to identify the specific problems that made them to recidivate and apply the Six Thinking Hats with the view to prevent a future occurrence … . An inspection of what the participants were writing down [in this exercise] was carried out after which the participants' notebooks were collected. [After this, Stephens made some notes for them in their individual books.] (2012, p. 135)

All in all, one could say that Stephens was aware of the impact that he could potentially be making in his relationships with the participants in the study (as well as in his write-up for wider audiences). He was careful to

- invite the participants to participate fully (to increase the likelihood of success of the interventions);
- show that he cared for them and their futures (through the questions that he set for them to reflect upon); and
- indicate to them that he did not have a "deficit" view of them as unable to benefit from the training: he considered them as able to learn and to develop themselves for their own sake and for others in the community.

One could thus say, that in his way of conducting the experiment he was not trying to operate as a neutral researcher with an intention merely to "find out" whether or not (and to what extent) Six Thinking Hats and EQ training make a difference to CT and EI scores; he was clearly hoping to draw out the recidivists' potential—and because he believed in this, this itself might have had constructively self-fulfilling effects in contributing to activate it.

5.2.3.2 EQ Training Package

According to Stephens, emotional intelligence is a relatively new concept which was coined by Salovey and Mayer (1990). He summarizes that they succinctly "describe emotional intelligence as intelligence driven by emotions" (2012, p. 10). Stephens indicates that Akinboye (2006) submitted that "emotions are central to effective human functions, also that our emotion affects our perception, motivation, cognition, decision making hence the need to pay attention to our emotions and the study of emotional intelligence" (Stephens, 2012, p. 10). He also cites Azeez (2007), as pointing to the qualities associated with EI—such as realistic self-confidence, personal integrity, knowledge of personal strength and weakness, resilience in times of change or adversity, self-motivation, perseverance and the knack for getting on well with others (Stephens, 2012, p. 10). Based on these various authors' work (and on his own counselling experience in Nigeria as a professional psychologist), Stephens arranged the sessions for EQ training.

In the first session, Stephens re-iterated to the participants what was involved in the study and also of its potential benefits to them. He explains how he spoke to the participants about this:

> The objectives of the study were highlighted and the participants were assured of utmost confidentiality of all information given in the course of the training program. The participants were also encouraged to express their views and opinion[s] freely, that one could also learn even through making mistakes. The benefits of participating in the program were highlighted; this included ability to have control over one's emotion, being able to establish and maintain relationship with others, improved problems solving skills/ capacity, improved ability to understand other people's emotion and use it for the benefit of both parties. The participants were encouraged to be actively involved during the sessions…. . The need to ask questions, for clarification and to contribute to the program was also discussed. (2012, pp. 135–136)

Again Stephens tried to encourage the participants to fully participate so as to derive maximum benefit from the training. (As with the creativity thinking sessions, this first session was also an occasion to distribute exercise books, biros and pencils to the participants, to enable the participants take down notes.)

Session 2 revolved around an explanation of emotion. He explicates that:

> Emotion was explained as the expression of such feelings that underlie, activate influence or direct behavior. The importance of emotion was also elaborated this include that emotion inform us about things that are of utmost importance to us such as the people, values, activities, our needs and wants. Emotion affects our thinking. Effects of negative emotions include stress, anger, depression, while positive emotions make us well when we are happy, joyous and loving. (2012, pp. 136–137)

In this session, Stephens hoped that participants would realize "the importance of knowing how to effectively use emotion, which emotional intelligence is all about". While he was speaking, the "participants took down notes and asked questions for clarification" (2012, p. 137).

Session 3 was devoted to an explanation of the concept of EI, which he explained as

> the ability to perceive emotions, generate emotions so as to assist thought, understand emotions and emotional knowledge and to reflectively regulate emotions so as to promote emotional and intellectual power. The elements of emotional intelligence include personal competencies—self-awareness, managing emotion, motivation and social competencies, empathy and social skills. (2012, p. 137)

Stephens worked with the starting assumption—which was also conveyed to the participants—that these competences could be learnt. He took care to invite the participants to consider how EI might be valuable for them in areas such as "recognizing their own emotions, behaving responsibly, focusing on the task at hand and paying attention as well as trying to generate positive feelings about self, family, others, and managing stress" (2012, p. 137). He indicates that the participants were actively involved in the process. They "took down notes, asked questions and made contributions during the discussion". He encouraged them to do this because this would "ensure that the participants were involved" (2012, p. 137).

Session 4 honed in on a "discussion on self-awareness". Stephens notes that first (as with other sessions) the homework was briefly discussed, and "participants who did the home work were commended" (as also with the creative intelligence training). In this session, he tried to impart some "skill indicators" which include "knowing when you are thinking negatively; when self-talk is helpful; knowing when you are becoming angry; and knowing the moment of your mood shifts" (p. 138). He also explained that:

> Self-awareness incorporates accurate self-assessment which is a key to realizing one's own strength and weakness. Some self-skills assessments were itemized, for example, do you recognize your feelings and emotions as they happen? Are you aware of how you speak to yourself? (2012, p. 138)

In the next two sessions, options for managing emotions were discussed. He described the concept of managing emotions as "the capacity to soothe oneself, to shake off rampant anxiety, gloom, despair or irritability"; and he highlighted various skills to try to manage one's emotions. The participants were also asked to think about how the various components of managing emotions were relevant to them in terms of their "personal feelings and experience in the world of crime" (2012, pp. 139–140).

Session 7 was about the emotional commitment to motivation. Skills indicators were addressed, such as the

> ability to regroup after a setback, ability to change and stop ineffective habits, being able to develop new and productive patterns of behaviors, and capacity to follow through words with actions. (2012, p. 140)

Stephens urged them to consider that "motivation is a major key in changing behavior including criminal behavior" (2012, p. 140).

Session 8 moved on to a discussion of social competencies, with particular attention to empathy, which he explained as follows:

> Empathy gives an individual an astute awareness of other's emotions, concerns and needs. The empathic individual can read emotional currents; pick up non-verbal cues such as tone of voices or facial expressions. (2012, p. 141)

He asked them to reflect generally upon how "sensitivity to others will reduce the incidence of crime against such people" (2012, p. 141). And as homework, the participants were asked to consider how empathy could help them to live a crime-free life. (Again, he was trying to help them to reflect on their own situations in the light of the training program.)

Session 9 was devoted to "relationship management" which was described as

> understanding others and relationship, resolving conflicts skillfully, being popular, outgoing, friendly, being concerned. The skill indicators include ability to accurately reflect back to others their feelings they are experiencing, helping others manage their emotions, recognizing when others are distressed, staying calm in the presence of others. (2012, p. 141)

As homework, "the participants were asked to identify how relationship management could assist them in living a crime free life" (2012, p. 142).

In the subsequent sessions, more practical work was done; and in the final session, the participants were commended for their consistency, co-operation and active participation. The participants were "asked of their opinion about the program" (p. 142). Stephens notes that "in responding they indicated that "their participation in the program had not been in vain" (p. 142). After this, the assignment that had been given to the participants during the previous session was discussed; and Stephens offered "ways of improvement and consolidation of materials learnt" (2012, p. 142). The participants, in turn, suggested that an emotional intelligence club should be formed as this could enable them to learn more and get more prison inmates to benefit.

The training session concluded with the participants being encouraged to continue applying all the techniques learnt for the remaining time in prison as well as after completing their jail term. As far as the club was concerned Stephens indicated to me (18 November 2015) that he found that he himself did not have the time to follow this through—and so this process was not in fact forwarded. He was concerned that if he asked the prison officials to allow this club to be formed (as their permission for this was needed) and he himself could not proceed with facilitating it in some way, it may not get off the ground. This, he felt, might have a negative effect in terms of the prison authorities' perceptions of the prisoners and the prisoners' self-labelling too. The amount of time that Stephens felt he could devote, given other commitments, of course relates to the question of the continued relationship with the recidivists. (See Sect. 5.2.6 below.)

What is noteworthy is that it was when Stephens asked the participants about how they had experienced the training, they came up with the idea of a club; this at least showed that they had felt that they had benefitted to some extent from the training and that other prisoners could too. What is also significant is that they knew that Stephens was asking for their feedback on the process—and that he was concerned to hear how they had experienced the training as a whole. (With the CT

group, he indicated to me, he had sought this feedback too, intermittently as he proceeded: pers. comm., 18 November 2015.)

When I checked with Stephens (18 November 2015) whether he believed that the recidivists had participated actively in the training programs because he had won their trust (from previous encounters with them in the prisons), he stated that he believed that one of the reasons they took the training seriously and were active in the classes, was because "they knew this person [he] was genuine". He said that he did not wish to "flatter himself" (for me), but that he did feel this was the case. He stated that "I don't know what effect this might have brought" to the study, but he admitted that it would most likely have had an effect on how the experiment (and the results) had panned out. He also hints at this when he states the following in his discussion in his thesis of the EQ treatment package and resultant scores:

> The attendant improvement of the participants' emotional intelligence score of the treated group could be attributed to the fact that they were motivated throughout the period of the treatment program and the environment was conducive, hence the participants were free to express themselves unlike the hostile predisposition of the prison officials. (2012, p. 188)

Markedly, in the write-up of the experiment, he speaks in the third person about "the instructor" (2012, p. 130); it was only in a personal communication with me (9 February 2015) that he mentioned that he was the one who did all the training. To me this is crucial, because the way in which the experiment panned out surely was also linked to the way that Stephens established a relationship with the participants (which was a continuation of his prior involvements and which also again evidenced his caring approach during the training). But he writes up the exposition in line with trying to offer a "scientific" exposition of the experiment in terms of a more positivist or postpositivist approach, where the researcher and his or her relationship with the participants should ideally not influence the results that will be obtained. Fitting in with the idea that "scientific inquiry" requires the researcher to adopt a distanced approach (see Chap. 1, Sect. 1.1.2), he chose to speak of "the instructor" who administered the training. This means that in the write-up of the work, the responsibilities that he assumed in treating the recidivists as if they were able to develop themselves, appreciating that his desisting from perpetuating deficit discourses (negative labelling) was likely to affect their way of seeing themselves, become more or less hidden (except in the admission that he makes in the above quotation). In effect, he was operating more in line with the injunction of Chilisa that:

> The researcher has to move the research from deficit- and damage-centered research to investigation that builds communities and restores hope and belief in their capacities to resolve challenges that they encounter. (2012, p. 174)

As I mentioned earlier and will also follow up in Sect. 5.2.7, the community in this case was in the first instance the community of recidivists, but Stephens also regarded as one of his prime "targets" the community of counsellors across the globe, whom he also considered might particularly benefit from the insights he shares in his text (pers. comm., 18 November 2015).

5.2.4 Some Results of the Experiment: (Re)Interpretations

In this section, I briefly present some of the results of the experiment as measured by the scores on the dependent variable scales. I offer an account of Stephens' (2012) interpretation of the scores, and I extend this account to suggest that one can interpret them in a less positivist-type fashion, without thereby devaluing the worthiness of the "scientific" endeavor. What is interesting to me is that on paper (as set out in Stephens' various tables), the scores appear to be measuring the result of "an instructor" giving training in terms of an independent variable (type of training) and computing the way recidivists' initial pre-test scores on the dependent variables changed or not (and how they changed if so). By way of example, I offer below some of the results in relation to the administration of the "creativity motivation scale"—one of the subscales of ICAS—and some of the results in relation to the EI scale. (The creativity thinking of the recidivists was measured in terms of four subscales of the ICAS, but below I am merely presenting an example of how Stephens looked at the results of creativity motivation.)

When discussing the improvements in "creativity motivation", Stephens specifies that:

> While the Y-mean scores of the treated groups increased … compared to the X-mean scores [pre-test], that of the control group did not indicate any increase … . This showed that the two treatment groups (Six Thinking Hats and EQ Training) benefited more from the training program than the control group. The "Six Thinking Hats" group showed the highest improvement in creativity motivation … followed by the emotional intelligence group … and lastly the control group. (2012, p. 172)

Stephens (2012) computed what this implies in terms of the statistically significant effects of the independent variables (the Six Thinking Hats and EQ training) on the hypothesized dependent variable (creativity motivation in this case) at a 0.05 significance level. He found that both treatment groups had significantly higher comparative pre-test and post-test score differences than evidenced in the control. The "personality type" of the various participants was also considered as being a possible mediating variable in influencing recidivists' learning, and was hence included in the computations. But personality was not found to be a significant mediator mediating the effect of the training on creativity motivation. What the statistical computations further indicated was that the Six Thinking Hats training improved the creativity motivation score of recidivists of different personality types more than the EQ training—although both types of training significantly improved the scores on creativity motivation (2012, pp. 177–178). That is, the Six Thinking Hats training was more effective than EQ training in improving the mean creativity motivation score, although both types of training had a significant effect. Similar effects were found for the other subscales of the ICAS, albeit that for some of these, personality type became a mediating factor in the relationships (2012, pp. 184–186).

Turning to Stephens' presentation of the findings relating to EI, he indicates that while the Y-mean scores of the treated groups increased, those of the control group

did not indicate any increase after the training. The results were found to be statistically significant in terms of the effect that both the Six Thinking Hats training and EQ training had on EI.[13] As far as EI was concerned, there was no difference in the effectiveness in the two treatment packages in terms of the improvement in EI. What I wish to draw attention to in this chapter is what I believe is lost in the various statistical inferences. I consider that at least three other possible ways of interpreting the "data" have become hidden behind the statistics.

1. Firstly, I suggest that the scores that the participants received post-test on the ICAS scale and scale directed at measuring EI, can be seen as a measure, not only of how they responded to the type of training (Six Thinking Hats or EQ), but also how they responded to the caring attitude of the "instructor" (and his messages of belief in their potential), which helped to keep up the participants' active participation and motivation.
2. Secondly, the responses the participants gave in the post-test could well be an expression of their wanting to present themselves in a positive light (also to please the "instructor"). Their filling in the questionnaire items in order to reveal themselves in the best possible manner cannot be outruled. This means that they may have learnt from the training what would count as their being creative and/or emotionally intelligent and could have answered the questionnaire items accordingly (whether or not they felt at the time that it quite applied to them or to what degree it applied). In this regard, Kinai notes that self-reporting "cannot guarantee a hundred percent honesty" (2013, p. 302). This concurs too with O'Sullivan's account of the "methodological morass in measuring emotional intelligence"—including the difficulty of assessing this via self-report measures (2007, p. 265).
3. If they were answering the questionnaire items in terms what they thought to be "good/expected" responses—given that they knew that they were supposed to have learnt from the training—they at least showed that *they had learnt from the training what would be indicators of CT and EI!* This means that one could suggest that they had learnt from the training how they were supposed to respond to the various items on the questionnaires. And they were learning further from now seeing again the various questionnaire items and relating this to their training (in order to fill in "appropriate" responses).

In short, what Stephens (2012) has not attempted to consider is how the participants' way of answering the questionnaire items could be seen as a *construction based on their decisions on how to interact with the questions*. But now we can ask —does it matter that the interaction between participants and research instruments makes a difference to what is "found"?

[13]Stephens also did an analysis of covariance to determine the degree of effect of his hypothesized independent variables on dependent ones, but for the purposes of this chapter, these details are not presented.

In terms of the idea that "science" must try to find out, to the best of its ability, *what is the case in reality independently of the influence of the observer*, this would indeed matter. But as I noted in Chap. 1 Sect. 1.1.2, science need not be directed in terms of a separation of "knower" and "known". In terms of this alternative understanding, we can re-look at what the results "show"[14]:

- Firstly, even if the responses as recorded by the participants, at the end of the training, were a response to their relationship with the particular instructor, this still shows that they had responded well to his caring attitude and had benefitted (or at least wished to express that they had benefitted) from having been exposed to his caring as well as the training.
- Secondly, even if (some of) the recidivists wished to please Stephens by putting themselves forward in the questionnaires in the best possible light, this suggests that they were concerned with wanting to please (and were being creative and empathetic in deciding how to read and respond to the questionnaire so that their expressions would reflect their wanting to please).
- Thirdly, even if they had learnt from the training how they were supposed to respond to the various items on the questionnaires in order to express increased CT and EI capacities, this showed that they had understood some of the messages that Stephens was trying to impart during the training regarding "good" CT and EI responses. And one could also say that by seeing the items in the questionnaire, their memories were being refreshed about the skills that Stephens was trying to impart during the training: The filling in of the questionnaire was thus performing a further educative function for them!

By affording the recidivists the opportunity to participate in the training and to learn more about CT and EI (and by providing the opportunity for them to fill in questionnaires trying to tap their potential capacities), the recidivists were not the "same" as they were before the experiment began; their filling in the questionnaires in the way that they did, showed that their exposure to the training during the study did make some difference to them. If the recidivists responded in a way that suggests they benefitted from the training, it means that we can infer that there has been some transformation, even though we may not be able to pinpoint its source through a linear logic that tries to work out the causal efficacy of intervention packages divorced from other factors in creating "effects". When I spoke to Stephens on 18 November 2015 and asked him what he had learnt from the study, he stated the following:

[14]Nassif and Quevillon (2008, p. 13) contend that one can apply the Heisenberg uncertainty principle (as it pertains in quantum physics) to the observation of participants' creativity. They cite Plucker's (1999) application of this principle to creativity research where Plucker states that "one cannot simultaneously observe and also account for creativity completely. The *modality in which creativity is being searched for necessarily determines the nature of creativity that becomes seen*" (2008, p. 13, my italics).

> One of the things I learned is that one must still give these people opportunities [to learn]. They should not be condemned, because they have got potential, if we give them a chance and expose them to correct interventions. Giving them a sense of responsibility also increases their hope. We need more research where people come in and show they care. There is a need for the whole society to rise up [to the challenge of giving recidivists hope].

He continued to remark that:

> If we can [e.g., through counselling combined with training interventions such as these] assist them to reflect upon their thought patterns and ways of managing their emotions, we are setting a good stage for effective rehabilitation. If thought is changed and EI increased everywhere [considering other factors such as environmental ones in immediate and wider environments] the rehabilitation process would be more effective.

Here Stephens suggests that training interventions that focus on improving both CT and EI can be used to draw out recidivists' potential and give them a sense of hope. As he sees it, the study can be interpreted as showing that recidivists do have potential, if given a chance, and that the stigmatization of prisoners (within prisons and the larger society) can be viciously circular. Interestingly, he suggests that the stance of caring on the part of researchers is an essential part of breaking cycles (and attendant discourses) of "condemnation".

One could argue that although Stephens in his write-up seems to rely on notions of (Western-oriented) science—also taught at Ibadan University—to develop and test hypotheses about the causal relationships between independent and dependent variables, ultimately he used a form of abduction or retroduction when speaking to me about the inferences that he drew from the study. His inferences as expressed above imply that he considers it reasonable to suggest that it is (at least partly) the lack of believing in people's potential that can make a difference in a self-fulfilling fashion to observed outcomes that ensue. Researchers therefore need to bear these possible consequences in mind—along with counsellors, welfare officers, etc. (In Chap. 7, Sects. 7.3–7.5, I continue my discussion on the responsible development of inferences via the research process, also with reference to this case.)

One could say at this point that based on his identification of supposed facts—as contained within the pre-test and post-test scores of the participants—Stephens offers a leap of imagination (as in abductive or retroductive logic) to argue that *the research results can be interpreted as reflecting the need to create a more caring social milieu,* in which people are given "more of a chance". In terms of abductive reasoning, Stephens' framing of research inferences to evoke sympathy for recidivists need not be value or emotion free: such logic can make provision for "leaps of imagination as part of value-and emotion-laden inquiries by a variety of stakeholders"—that is, those concerned with the issues at stake (cf. Romm and Kakoulaki at https://dialogicdesignscience.wikispaces.com/Axioms+%287%29). Stephens puts forward his understanding/interpretation for those concerned about the issues to which he has drawn attention, to engage with further.

5.2.5 Validation Reconsidered

Stephens mentioned to me (9 February 2015) that after the close of the experiment, he still sought feedback from the recidivists on their experience of the training:

> After the training every time I go [in various capacities] I still try to show the prisoners that they are appreciated and acknowledged and I ask them if they have been able to put into practice what they have learned (pers. comm., 9 February 2015).

He indicated that as far as managing emotion is concerned, some of them stated that "I used to get angry very easily but now I am better able to manage this". Or they commented: "Before now I used to feel very sad, but I have been trying to encourage myself. Now I look from different perspectives". He remarked to me that the recidivists saying that they are able to look from different perspectives matches the idea that he had been trying to impart during both the CT and EI training, namely that "one can look from different perspectives", and he found it encouraging that they were speaking to him in this way when he asked them about their well-being. But he mentioned to me too that when he asked them how they had found the experiment, some of them stated that "time was short and it was difficult to do the assignments because of the prison conditions". Nevertheless, they said that despite this they still felt that they had learnt (pers. comm., 9 February 2015).

These anecdotal accounts that Stephens received (i.e., which the recidivists shared with him when he asked them about their wellbeing and about whether the experiment had been helpful) lend some credence to the inferences he made (no matter what "logic" he applied) regarding the benefits that could be said to have accrued as a result of their involvement in six hats thinking and EQ training. As I indicated in Chap. 1, one of the ways in which one can argue that research becomes validated, is if it engenders some transformation. This is a form of what authors such as Lather (1986), Chilisa (2012), and Mertens and McLaughlin (2004) call catalytic validity. According to Mertens and McLaughlin, one way of trying to assess catalytic validity is through participants' self-reporting on outcomes as experienced by them (2004, p. 108).

But Stephens also shared with me (18 November 2015) that there are different conceptions of what "transformation" might mean. For example, he remarked to me that the welfare officials in the prison had the view that "these guys can't change". That is, they had "their own concept of change". Put differently, Stephens commented to me that "their understanding of change is different from my perception". Their perception was that "they [the prisoners] will be deceiving you that they can change: they will say this but they will be pretending; they will still go out and re-offend". As Stephens noted to me, "the element of doubt is strong". I agreed with him that it seemed that he and the welfare officers whom he was mentioning "have different concepts of transformation".

He went on to say to me that after the research experiment, he had continued to maintain the relationship that he had had with the prisoners who had been part of the research—and he did not see them as offenders but as "my friends"; "I call them

my friends". Could one say that his friends had changed subsequent to the research? As Stephens states, there are different perspectives on this, but he chooses to take the perspective that there was some transformation—as felt, experienced and shared by some of the participants with him. He therefore believed that the study had made a difference to their lives (if only their way of seeing themselves and their potential more positively). And of course, as I noted in the Introduction to this chapter, to the extent that people in society (such as welfare officers) have "strong doubts" about recidivists, this could easily have self-fulfilling effects, because then efforts are not made to try to channel forms of support for them and for their rehabilitation.

My conversations with Stephens around these issues showed up the subtlety of his conceptions of what the experiment had "found". He was aware that there may be a variety of interpretations, including the ones offered by the pessimistic welfare officers who had felt that even if the recidivists were pretending (e.g., in their filling in of the questionnaires and their reporting to Stephens) that they had learnt and changed, this was not "real change", because they would most likely "go out and re-offend". Choosing not to work with such deficit discourses in the process of the study and in the interpretation of the results, is thus a considered choice. I would conclude that Stephens chose to act in what he considered a responsible fashion, recognizing that how he interpreted the results (at least presenting the optimistic view alongside others that may be possible) could itself have an impact on the prisoners and on wider audiences. Otherwise put, he acted in awareness of the likely consequences of the impact of the research work (in all its stages) on others.

5.2.6 Approach to Ethics

Ethical considerations that arise in the research endeavor do not feature explicitly in Stephens' write-up—in that he does not devote any section of the thesis to talking about his ethical approach (indeed, the term *ethics* as pertaining to research is not used in the thesis). It is clear that he is cognizant of the need to mention in his thesis issues such as obtaining informed consent, assuring participants of their right to withdraw, and assuring them of anonymity and confidentiality in any reporting. This cognizance informed his exposition in his thesis of how he spoke to the potential participant when trying to recruit them for participation and also in the first session of each of the trainings. However, he glosses over other ethical considerations when organizing his write-up. This could be a function of his reliance on Western-oriented scientific expositions. In such expositions (as noted by Chilisa, 2009, p. 420), what is normally required is that researchers show that they have followed ethical requirements concerning obtaining informed consent, explaining to participants their right to withdraw, etc. This is what was mentioned by Stephens in his text (2012, p. 136). However, Stephens' way of handling his ethical responsibilities featured strongly in our personal communications. For example, it is here that he expressed the following:

- The research relationship as he saw it, needed to be mutually beneficial. In terms of benefits for all participants, he wanted to cater not only for the "treatment" groups whom he believed could benefit from the Six Thinking Hats and EQ training, but also for the "placebo" group—hence the fact that he organized HIV and AIDS training for them. (He indicated to me in conversation that he would not have had the time to later administer the treatment to the control group after the experiment had been completed, but that, time permitting, this too would be ethically desirable.)
- He felt it incumbent upon himself to address participants with a caring attitude (no matter that this might have had an effect on the scores, as he admitted).
- Participants needed to be treated with respect by asking them for feedback on how they experienced the training (their views were solicited along the way and at the end of the sessions).
- It was important to continue to follow up with participants after the official close of the study—after the post-testing had been done—and to continue to ask them if they felt that they had benefitted and how they were now feeling.
- After the experiment, he wished to "return" something to participants (time permitting), which he did do as part of his continuing involvement with them as pastor/minister and psychological counsellor, and his continued involvement in Cottage of Hope (the NGO aimed at giving them hope at various levels, in particular with their schooling, university education via distance education, and more recently, to cater for their health needs).[15]
- Apart from trying to make a difference via the research process to the immediate prison community, especially the people involved in the various "treatments", he also wished to target communities (such as the community of counsellors across the globe) who might be able through their reading of his results and interpretations to learn more about how their counselling could draw out the potential of recidivists.
- Given the hostile relationship that he discerned between prison officials and the prisoners, and the distrust, he did his best to at least show the participants that he believed in their potential (despite some of the officials telling him that he was wasting his time in conducting all the training and in continuing to offer them support). He was aware that his own approach, which he assumed (in the study and in other capacities) could make a difference to how the participants saw themselves, despite a more pervasive societal stigmatization.[16]

[15]The fact that he was a postdoctoral fellow in South Africa (starting from 2014) made his visits to the prisons less regular than before, but he still participated with them during his postdoctoral fellowship, for example, via the NGO Cottage of Hope and via his links with the National University of Nigeria.

[16]When I asked Stephens if he thought there were any options for addressing such stigmatization, he suggested that positive role models needed to be nurtured. He cited the case of an ex-prisoner who had studied (through the initiatives created by College of Hope) via the National University of Nigeria's distance education program. This person, after leaving prison, had gone to work for the university—and the prisoners, officials and of course university staff were aware of this. He constituted a positive role model in this regard (pers. comm., 9 February 2015).

What I would suggest is that in research write-ups for audiences, ways of treating these kinds of ethical questions should not be sidestepped. Researchers' reflections concerning their felt responsibilities in their ways of relating to, and potentially impacting on, the research participants and also making a difference to the wider discourses that have currency in the society, should be discussed in such write-ups. It should be of interest to readers to be given some idea of how researchers are choosing to exercise their responsibilities as they see them. I expand upon this point in Chap. 10, Sect. 10.6.1, in relation to this research.

5.2.7 *Generative Theorizing: Toward Transformation*

Gergen (2015) argues that in transformation-oriented research, researchers are acutely aware that research is not a matter of mirroring, but a matter of *forming realities in the making*. As I noted in Chap. 1, Cannella and Manuelito (2008, p. 51) likewise make this point by referring to the metaphor of "changing woman". This means that researchers have to be vigilant about how their theorizing, with participants, is likely to impact on the participants as well as wider audiences, and is likely to contribute to "practice forming" (Colliver, Goff, Reedy, & Vaartjes, 2015, p. 14).

I would submit that when Stephens was urging the participants to become more aware of different perspectives and different ways of addressing their emotions, he was co-involved with them in generating a theory about recidivists' potential to become motivated to learn and apply such skills. Together they were developing ideas about possibilities for rehabilitation (and were applying and developing the "theory" that CT and EI skills can be learnt and need not be seen as innate qualities). This is not to say that they were unaware of the problems and barriers that might mitigate against prospects for them practicing such skills and imagining and managing a crime-free life once out of prison; but it is to say that they were together defining ways of proceeding in the face of the barriers, and thus looking for and finding some hope. Stephens' contribution in this arena was to inject the language of "creativity thinking" and "emotional intelligence" to urge them to re-imagine possibilities.

As far as wider audiences are concerned, Stephens' write-up is intended to "move" audiences by pointing to the limited life chances of the prisoners before and after entering the prison system. He implicitly calls upon readers to consider that *blame can be attributed at the level of the larger social and cultural processes* that impinge on those who end up in the prison system. In similar fashion, Gergen makes the point that ways of explaining others' behavior as proffered by, for instance, social psychologists "may either sustain or alter the common attribution patterns of the culture [where people attribute causality] and thus the common patterns of blame and praise [in society]" (1978, p. 1349). Gergen refers to the case of theorists offering explanations for ghetto riots in the USA, and he notes that when scientific explanation operates at the level of focusing on "society's

oppression of the ghetto black [those marginalized in society] … such explanation shifts blame from the rioter to society" (1978, p. 1349). (Here Gergen can be seen as drawing on the concept of structural violence that I introduced in Chap. 2, Sect. 2.2.2.) One can see resonances with Stephens' effort, via this theorizing, to shift the blame somewhat "to society", so that people who are concerned can reflect on how social changes at this (societal) level might be accomplished.

At this point I would like to consider the question raised in earlier chapters about using during the research process apparently "neutral" language, which in effect carries with it cultural connotations. Was Stephens' use of the language of "emotional intelligence" and "creativity thinking" unduly steeped in the intellectual traditions (of the West) in which these concepts emerged? Davis raises this question in his reflections on "emotions as commentaries on social norms" (2012, p. 39). He notes that what we become ashamed, remorseful or proud of (as part of our emotional repertoire) is a feature of "what is important to us in our lives as subjects"—and this itself is not culturally universal. As he puts it, "the differential impact of social norms [which affect how we feel about ourselves and how we interact with others] … reflects different visions of the good life in society" (2012, p. 39). People can thus develop negative emotions which express "their failure to conform to social [cultural] norms of relationship, feeling and self" (2012, p. 39). What we become emotional about, in large part reflects "the normative order as well" (2012, p. 39). This would be consistent with Stephens' reporting that the pervasive images that express norms about what is valued in Nigerian society, can lead many people to feel disappointed with themselves and that they should by any means (e.g., even through crime) try to join the culture where the emphasis is placed on "materialism" (2012, p. 196).

However, one could suggest, as do many authors whom I cite in the next chapter, that this apparently prevalent cultural norm of materialism is not specific to Nigerian society, but is indeed a norm that has infused the globe as a "dominant" narrative. Cram and Mertens argue that both transformative and Indigenous paradigms of research and evaluation point to the need to sustain counter-narratives as part of the process of transformation toward increased social justice; and they suggest that dominant norms can become interrogated via the research/evaluation process (2016, pp. 172–174). Davis similarly argues that at the level of individual action (and emotion), people who have been exposed primarily to dominant normative orders are able to develop a "shift in normative frameworks" which can guide their thoughts and emotions (2012, p. 39). As I indicated in Chap. 1 (Sect. 1.1.2), research endeavors can be regarded as one way of bringing to the fore and developing alternative normative frameworks. Wane, Akena, and llmi (2014, p. 1) also emphasize this point. What is important is that researchers take into account how their framing of issues (with others), use of language and ways of engaging with research participants/co-researchers might make a difference to the discourses that become generated in the social arena.

Stephens (2012) himself arguably was "monitoring" (being self-aware of) his own way of injecting discourses into the social arena, through his way of addressing the recidivists and through his write-up, aware that the messages that he sent via his

way of speaking/writing could be impactful. He was working with an (emerging) theory on the effect of giving recidivists a chance by offering training programs that he conjectured could be helpful for them and could assist them to conceive new ways for viewing themselves and their options. In the group discussion to which I referred in Sect. 5.2.3.1, the group offered alternatives for theorizing their situation, such as recognizing that despite the various environmental factors, there was scope for the exercise of agency on their part (e.g., by forming co-operatives as a way of making money, in order to sidestep employers' stigmatization of them, and by understanding that in considering the "culture of corruption", they could still act in their own way, thus not contributing to this cultural repertoire). They were, in Magnat's terms, trying to find options for "altering the limitations of life as lived" (2012, p. 172). And Stephens, for his part, in his own contribution to the development of an ongoing generative theorizing, draws attention to possibilities for others (e.g., counsellors, employers and policy makers) to participate in altering the limitations which make it difficult for prisoners to manage in society once they have been released from prison. For instance, he suggests, in one of his recommendations, that

> employers of labor should be encouraged by the government to offer employment opportunities to ex-prisoners. Incentives such as tax rebate for any employer who employs an ex-prison inmate as introduced by the Mayor of Philadelphia, Michael Nutter who announced a $10,000 tax credit for any employer who employs an ex-prison inmate could be introduced by the Nigerian government. Recognition, national honors can also be given to such employers of labor as a form of motivation. (2012, p. 196)

He also proposes as a novel recommendation (again based on the theorizing around "giving prisoners a chance") that

> there should also be increased efforts to arrive at out of court settlement through mediation, reconciliation and compensation not only in civil matters but also in certain criminal cases as this will reduce the number of cases in the courts as well as reduce the prison population [given the problem of overcrowding in the prisons]. (2012, p. 193)

Furthermore, Stephens makes a number of recommendations regarding the counselling of prisoners (in Nigeria and also more broadly). He suggests the need, in Nigerian prisons and elsewhere, for a dedicated counselling unit to be established in prisons; and he feels that it is important to share with counsellors his inferences from the study, namely that prisoners have the potential to learn from creativity thinking and EQ training. He recommends that this is taken on board by counsellors in various contexts of application: "Since the study established that' Six Thinking Hats' and emotional intelligence can be learnt, counsellors should teach these skills in the school setting, work place, reformatory homes among others" (2012, pp. 189–190). He recommends too that "counsellors themselves should be exposed to Six Thinking Hats and EQ training through workshops, lectures, seminars, conferences and at meetings of professional bodies". And as professionals, he proposes that counsellors "can also conduct further research in the use of 'Six Thinking Hats' and emotional intelligence, as well as on recidivism" (2012, p. 191).

Owing to space limitations, I have concentrated above only on a few of Stephens' recommendations. I have highlighted those that relate back to his theorizing—with recidivists (in the sense that they participated actively in the experiment)—around their prospects for applying CT and EI so as to consider imagining and feeling motivated toward a crime-free life.

In his suggestions for further research, Stephens underlines the fact that there is never "finality in research" (2012, p. 197), and that additional researchers should further develop the theorizing. For instance, he suggests that other studies could be done "using other creativity techniques such as Brainstorming, Akinboye Practical Creativity at Work, Problem Solving techniques, etc." (2012, p. 197). He also proposes further studies looking into various "risk factors" that are predictive of recidivism. (One could argue here that although he uses scientific jargon such as "predictive of recidivism", he recognizes that a myriad of factors combined "cause" recidivism—including, as he mentioned earlier, a variety of "environmental factors". He would thus caution against a reductive account of the "causes" of recidivism.)

Stephens feels that what he has contributed theoretically is to "open a new chapter in research in psychology and counselling by using these intervention strategies rather than conventional counselling techniques" (2012, p. 199). He indicates in conclusion that he hopes that:

> The findings of this study will assist guidance counsellors, social and counselling psychologists respond more practically in resolving societal problems, [so as to be] more relevant and make the profession one to be reckoned with. (2012, p. 199)

Stephens' generative theorizing is thus intended to be helpful, inter alia, for this target audience (counsellors) whom he is hoping to "move" via his outline of the experimental process and its results (as interpreted), so that the "profession can be one to be reckoned with"—in Nigeria and in other parts of the globe. Altogether one might say that similar to Woldegies (2014, p. 89), as far as the issue of "generalization" and transferability of results to a variety of geographical and professional contexts is concerned, Stephens considers that this would be a function of the comparability of the findings to other contexts—as "translated" by those concerned (See also Chap. 4, Sect. 4.2.6.). The "external validity" of the findings would thus rest on (concerned) people deciding how and to what extent the findings/results could be transferred (or indeed altered somewhat to suit new contexts) in practice.

I would like to emphasize in closing (for now) that when I asked Stephens (18 November 2015) about whether he believed that any of his many recommendations directed toward, for example, policy makers in the government, societal citizenry, counsellors, and others, would have the intended impact, he replied that all that he could do for now would be "write them down" because of his lack of action influence in these circles. Even with the prison officials, trying to explain the import of the findings to them would "not make any difference" as he saw it—that is, according to him, they "will not do anything about it". Nevertheless, he has a good relationship with the prison officials in the sense that he has blanket permission to

conduct research in the prisons. And this at least allows him to conduct research such as the one reported upon here, which he hopes will have some influence in that people may read it and decide to engage with some of the suggestions. The thesis is currently in the University of Ibadan library, and has also been published by Lambert Press (2015). I suggested to him too that his saying to the recidivists at the start of the project that the results of the study could be read widely because "the world is a global village" might also come to fruition now partly through my showcasing the experiment in this book!

I now turn to the second example from which I wish to draw additional lessons regarding responsible use of experimentation as a research approach, by focusing on how experimental debriefing can be re-designed and on how feedback from participants can also be designed into experimental work.

5.3 Debriefing in Deceptive Research: A Proposed New Procedure (Poland)

Oczak and Niedźwieńska set up their experimental design with the intention of examining "the usefulness of the new debriefing procedure", which they devised following ethical principles of avoiding harm and seeking to promote wellbeing in relation to participants who participate in deceptive psychological research (2007, pp. 51–52). Oczak and Niedźwieńska wanted to examine how as a researcher one might be able to reduce/eradicate or reverse the possible negative consequences for participants who participate in research which involves some initial deception of them. The initial deceptive process in their experiment made use of the Gudjonsson Suggestibility Scale (GSS), designed by Gudjonsson (1984) to ascertain subjects' level of suggestibility to misleading cues administered by a researcher. The administration of the GSS is based on deceiving participants to believe that they are taking part in a memory test, when in fact the research process is meant to determine their suggestibility to cues offered by the researcher. The participants in this case were 60 students from various disciplines studying at Jagiellonian University who volunteered to be part of the study.[17] They were told that they were taking part in a memory test aimed at testing their recall abilities.

Before the GSS administration (as part of the pre-test for this experiment), their scores on three variables—which were the dependent variables in this study—were computed. Oczak and Niedźwieńska used what is called the "semantic differential technique" to measure participants' appraisal of three constructs, namely "mood", "attitude toward psychological research" and "self-esteem/current feeling about

[17]Fifty-three were women and six were men (2007, p. 53). Oczak and Niedźwieńska note in this regard that it is possible that "there are gender differences in the interpretation of information provided during both types of debriefing procedures" and therefore future research could look into this (2007, p. 56).

themselves". Oczak and Niedźwieńska piloted the appropriateness of the following dimensions of the three constructs and "ten dimensions of highest rank" (in the pilot) that were used for the study:

> *mood* (pleased/displeased, sad/happy, tense/relaxed, calm/anxious, care-free/worried, cheerful/gloomy, bright/depressed, bored/excited, bad mood/good mood, disappointed/satisfied), *psychological experiments* (valuable/worthless, pleasant/unpleasant, meaningless/meaningful, important/unimportant, useful/useless, unhelpful/beneficial, thoughtfully/thoughtlessly designed, harmful/harmless, interesting/boring, incompetently/competently conducted), *myself* (inefficient/efficient, forceful/timid, soft/hard, socially competent/incompetent, disreputable/respectable, confident/insecure, clever/mindless, weak/strong, intelligent/unintelligent, worthless/valuable). (2007, p. 52, my italics)

The dimensions were rated by participants on a seven-point scale, with "1" indicating the negative end, and "7" indicating the positive end (2007, p. 52).

Furthermore, in regard to the construct of self-esteem, Oczak and Niedźwieńska also applied the "state self-esteem scale" to measure "short lived (i.e. state) changes in self-esteem" (2007, p. 53). All participants filled in the three Semantic Differential [SD] scales and the State Self-Esteem Scale [SSES]" (2007, p. 53). The latter scale consisted of 20 items, each scored on a five-point scale (2007, p. 53).

The application of the GSS was then set in motion. The GSS research process began with the researcher narrating a short narrative paragraph about a fictitious robbery. Participants were

> required to listen to the story and were instructed to try to remember as much of the story as they could. After the story had been read to participants, they are asked to recall it. After a 20-minute delay, they were asked 20 specific questions about the content of the story, 15 of which contain misleading suggestive cues. (2007, p. 54)

One index of their suggestibility, as measured by the GSS, is called "Yield"—that is, to what extent they yield to the misleading suggestive cues offered by the researcher (and designed to mislead). Oczak and Niedźwieńska note the following:

> The number of times participants accept the misleading cues and recall material that was not in the story constitutes the first index of suggestibility, called Yield. The highest possible Yield score is 15, with a range of 0–15. (2007, p. 54)

In terms of the GSS suggestibility research, the researcher then proceeded as follows:

> After answering the 20 questions, participants were told that they have made a number of errors (even if no errors had been made), and it is therefore necessary to go through the questions once again. They are asked to be more accurate than before. The number of times participants changed their answers following negative feedback is called Shift. This is the second index of their suggestibility. (2007, p. 54)

In Oczak and Niedźwieńska's experiment, after the GSS research with this narrative had been administered to all the participants (with the GSS scores on Yield and Shift being attained), the participants were divided into two groups (30 each), with half of them being debriefed via the standard debriefing procedure, and the other half via the extended debriefing procedure.

The standard debriefing procedure involved explaining to the participants that this experiment had been measuring their susceptibility to suggestion. The "true nature of the [GSS] scale and mechanisms of suggestion" were given to the participants, along with their "results on all indexes together with means and standard deviations for this study" (2007, p. 54).

As part of this debriefing, the experimenter

> explained that a suggestive question indicates the wanted answer which, in all cases, is a wrong answer. Participants were informed that 15 suggestive questions introduced material that had not been mentioned in the story, so that the right answers would be: "I do not know"" or "It was not mentioned." It was pointed out that the test included two types of suggestive questions: (1) "false alternative questions" that implied two alternatives only, both of which were wrong, and (2) "leading questions" that included salient premises which created an expectation of a certain type of answer. (2007, p. 54)

The participants were also informed as part of this debriefing that when they had been asked specific questions for the second time during the GSS, this in fact

> was not connected with the number of errors participants made [as they had been told originally]. After having been given their results, participants were informed that the knowledge they had gained could be used to diminish their suggestibility and they might use it in the future. (2007, p. 54)

In the new (non-standard) debriefing procedure, the first part of it was the same as the standard debriefing. That is, participants were given all the information that was presented to the control group. Subsequent to this, however, in addition, they were told that they could *re-take the test*, this time with the new knowledge now at their disposal. The experimenter stated that:

> I would like you to see how you can apply the knowledge you have just acquired. Now, when you understand what suggestion, or a suggestive question is, I propose that you take a similar suggestibility test once again and see what your performance is this time. (2007, p. 54)

They were then afforded an opportunity to participate in what is called a parallel version of the GSS, which involved a different narrative, followed by the same kinds of misleading suggestive cues. After ten minutes, they are asked to recall aspects of the story, with the GSS being administered only in terms of their "Yield" scores. (This was to shorten the process.) They were then given their Yield scores, so that they could see to what extent they had benefitted from their learning about the mechanisms of suggestibility.

Subsequent to this, all the participants in the experiment were told that the study of suggestibility had been completed and that a new experimenter was interested in knowing "how people felt while participating in psychological research". The new experimenter told them that:

> This part of the experiment is independent of what you have been doing so far and it will not influence your results. We would like to know how people feel during psychological experiments that might be disagreeable and how they perceive various experimental procedures. Your honest answers will help us understand whether a given procedure is unpleasant or not. Thanks to your honest answers we will be able to modify experimental

Table 5.2 A depiction of this experimental design

Group	Participants	Scales administered before and after the GSS and the follow-up debriefing process
Type of debriefing (independent variable)	Students from various disciplines at Jagiellonian University	Three SD scales to measure hypothesized dependent variables of *mood*, *attitude to research*, & *self-esteem*; and one SSES scale to measure *state self-esteem (short-lived)*. In the post-test, participants were informed that the experimenters were interested in soliciting their feelings about participation in psychological experiments
Experimental group: Innovative debriefing	30	
Control group: Standard debriefing	30	
Total	60	

> procedures so that they will be more enjoyable for participants. Remember that whatever your answers will be they will not be treated as the evaluation of the person that conducted this experiment and they will not have any consequences for her. (2007, p. 53)

And having being told this, all the participants were now asked to fill in the SD and SSES scales again (for the post-test). The researchers were hence able to "compare changes in mood, self-esteem, and the perception of psychological research" in the two groups to see if there were significant differences between the groups on these (dependent) variables (2007, p. 51). The whole experimental design can be depicted as in Table 5.2 (my rendition of it based on my reading of Oczak and Niedźwieńska's text).

Oczak and Niedźwieńska explain as follows what their experimental design was intended to examine:

> This study examines the effectiveness of a new debriefing procedure designed specifically to address possible negative consequences of participation in deceptive research. The new debriefing includes an extended educational procedure that enables participants to gain insight into relevant deceptive practices and how to recognize and deal effectively with them, and thus end their participation with a positive and beneficial learning experience. (2007, p. 49)

Oczak and Niedźwieńska now look at the results to see to what extent their hypothesized positive relationship between the debriefing procedure (standard or innovative) and the variables of mood, attitude and self-esteem could be said to have been borne out by the results.

5.3.1 Some Results of the Experiment: (Re)Interpretations

Space does not permit a full account of all the results. However, I focus on a few of them as interpreted by Oczak and Niedźwieńska and then offer some additional interpretations.

Regarding the administration of the GSS, Oczak and Niedźwieńska's indicate that they had "hypothesized that participants who received the new debriefing would reject suggestive information during the second GSS administration and their suggestibility would diminish" (2007, p. 54). They state that, as expected,

> hardly anyone resisted suggestive questions and negative feedback in the first suggestibility testing. When the GSS was administered for the first time ($N = 59$[18]), only three individual Yield scores equaled 0. But there was "a substantial decrease in Yield scores after the extended [non-standard] debriefing procedure. (2007, p. 55)

After the non-standard debriefing procedure, in which participants were given a chance to make use of their newly acquired knowledge about avoiding suggestibility, they all scored lower on the GSS, and the scores of 87% of them decreased to 0 (p. 55). This indicates that indeed the extended debriefing, as expected, became a learning experience for the vast majority of the participants who were given the chance (via the non-standard debriefing) to try out their skills in not "yielding" to misleading suggestive cues.

Oczak and Niedźwieńska then examined the results for their three dependent variables, namely hypothesized changes in mood and beliefs, attitudes to psychological research and self-esteem.

As far as changes in mood are concerned, they note that:

> Planned statistical comparisons … revealed that the groups did not differ significantly at the pretest, but participants after the extended debriefing procedure expressed significantly more positive mood than participants after the standard debriefing procedure. (2007, p. 55)

In an analysis of "simple main effects" (of the independent variable), it was revealed that "the standard debriefing group showed the significant pretest to posttest *decrease* in mood (i.e., it became more negative), … while scores of the extended debriefing group significantly *increased*" (2007, p. 55, my italics). So while the groups did not differ significantly at the pre-test, participants after the extended (non-standard) debriefing procedure were found to "express significantly more positive mood than participants after the standard debriefing procedure" (2007, p. 55).

The researchers also found when examining the hypothesized variable of "perception of psychological research" that while "there were no significant pretest to posttest differences in research perception in the standard debriefing group … the extended debriefing group evaluated psychological experiments significantly better at the posttest". That is, "participants after the extended debriefing procedure evaluated psychological research significantly better than participants after the standard debriefing procedure" (2007, p. 55).

The researchers note that the results on these variables (mood and attitude to psychological research) turned out better than they had expected:

[18]One participant from the control group was eliminated on analysis because the researchers found that she had failed to fill in the various forms properly.

> Although we expected that the extended debriefing would simply return subjects to as good a condition as before the study, its effects turned out to be much better. After the GSS with the extended debriefing, the ratings of mood and attitudes toward research increased, i.e., participants showed better mood, and perceived psychological experiments in a better light than before the participation. (2007, p. 56)

As far as self-esteem ratings were concerned, the researchers note that there was also a

> tendency for self-esteem ratings to increase but this effect was not strong enough to result in significant group differences in the post-experimental self-esteem assessment (either on the Semantic Differential scale or the State Self-Esteem Scale). (2007, p. 57)

The statistical comparisons conducted on self-esteem ratings showed that "planned comparisons did not reveal significant differences between the groups either at the pretest or at the posttest". That is, there were no significant differences between scores on "self-esteem" (post-test) for the control or experimental groups. The researchers had not anticipated this result. The following are tentative explanations of these results (which I have numbered below):

1. Perhaps they [the participants who had participated in the standard debriefing and did not get a chance to try out anew their responses] attributed their unfavorable performance in the GSS to the experimental situation rather than to their own abilities or personal characteristics.
2. It is also possible that the participants in the standard debriefing group did not consider the experimental situation important enough to make any generalizations about themselves.
3. Alternatively, perhaps the research instruments did not successfully invite participants to express their felt moods/impressions (including negative ones).
4. In addition, it is also possible that by virtue of the researchers expressing interest in participants' feelings and perceptions during the experiment made them evaluate themselves more highly then participants usually do in susceptibility research (2007, p. 56).

Point 3. above as given as an option by Oczak and Niedźwieńska, indicates to me that they were sensitive to the possibility that the research instrument which was trying to tap into people's felt self-esteem (at that point in time) was not necessarily tapping into this. Participants may have decided to interact with the questionnaire—and the researchers, by implication—by not expressing "negative impressions". People may well have been responding to the fact that they did not wish to displease the researchers (with their instruments) who were asking about their felt self-esteem, assuming that the participants did feel some negative emotions at the time. In other words, the invitation to ask them about their feelings (an invitation which they may have appreciated) may have affected their decisions about what to express.

Furthermore, Oczak and Niedźwieńska postulate in point 4. above that possibly by virtue of the researchers "expressing interest in participants' feelings and perceptions during the experiment made them evaluate themselves higher than

participants usually do in susceptibility research" (2007, p. 56). That is, just because someone had expressed interest in their responses, might have made them feel more valued, and hence their self-esteem scores were higher than one would expect from participants involved in deceptive research. Again the influence of the observer (in this case showing interest in people's feelings) in generating the scores that become seen cannot be ruled out.

What we can also draw out especially from point 4., is that merely by asking people to offer responses on how they are feeling (in this case about themselves) may make them feel better about themselves—as this can be interpreted by participants as a sign of respect for them. This is therefore a good reason to indeed ask people, as it may well lead to people feeling more respectful of themselves (which manifests in the higher than expected scores, despite their having been subjected to a deceptive experiment in this case).

In any event, in whatever way we interpret the results above, the principle which states that observer and observed are connected (see again Sect. 5.2.4 above) may well apply. Now, just because one is aware that researchers' way of relating to participants—including asking for feedback on their experiences—could well make a difference to how they feel about participating in research, *one needs to be careful about how one interacts*. If possible (and it is indeed possible), asking them for feedback on how they feel—whether this is done through a questionnaire as in Oczak and Niedźwieńska's case or through other methods—should be considered a vital part of one's sense of responsibility to participants. This alone can mean that less "harm" is caused than might be the case if they believe that they have been "used" via the research process with no attention to their feelings. In Chap. 8, I offer additional options for seeking feedback, and in Oczak and Niedźwieńska's terms, "expressing interest in participants' feelings and perceptions" (2007, p. 56).

5.3.2 *Experiments as Potentially Educational*

Oczak and Niedźwieńska (2007) indicate that after the participants in the experimental group had been subjected to the new debriefing procedure, their GSS scores increased dramatically. This shows, according to Oczak and Niedźwieńska, that participants can learn to avoid and evade the misleading cues given by researchers (and indeed by others in society who may be trying to manipulate them). They draw the conclusion from this that experimenters should conduct extended debriefings which are both intended to be educative and to help participants to "try out" new skills for exercising agency in the face of (experienced) domination. They indicate how in this case the extended debriefing process enabled the vast majority of the participants to recognize (in theory and in practice) that they do not have to "yield" to suggestive cues which are subtly applied; and meanwhile, by participating in the learning exercise offered via the extended debriefing, the participants can feel that they have benefitted. Oczak and Niedźwieńska regard this as significant as a matter of ethics, as explicated below.

> Our interest in ethical problems emerged from our experience as researchers conducting studies on suggestibility and related issues. We have had the impression [from their survey of the research literature] that in deceptive studies investigating undesirable behavior the standard debriefing practice is unsatisfactory as it leaves participants with the feeling of being cheated and used, as well as provides them with negative feedback on their abilities [in terms of their scores on the GSS]. To prevent these undesirable effects of deception, we proposed the new debriefing procedure. (2007, p. 56)

They go on to argue that:

> [Because] our strategy turned out to be effective in the suggestibility study, researchers might consider applying debriefing with the extended educational procedure to various deceptive studies that investigate social influence. The general idea of the extended debriefing is that it must provide participants with an opportunity to learn how to master social situations in which others seek to mislead, manipulate, or coerce them. (2007, p. 58)

Oczak and Niedźwieńska aver that the extended educational function of the new debriefing approach, where participants are afforded the opportunity to learn in practice, implies that in conditions outside of the experimental situation (in everyday life), people will presumably be able to apply the skills they have learnt. *Or at least it is the responsibility of researchers to enable participants to better recognize social situations where others are trying to mislead them*—so that they can be more attuned to this. Even in the standard debriefing, they exercised responsibility in suggesting to the participants that they might be able to use information that they had obtained in the debriefing session to help them in the future (albeit that they did not have a chance to try this out). Clearly Oczak and Niedźwieńska feel that it is imperative to equip people to resist forms of coercion, where coercion is subtly applied by those who send out misleading messages designed to mislead. As this is a psychological experiment, Oczak and Niedźwieńska do not tackle other social conditions where people feel coerced to submit to authority; but they wish at least to demonstrate to people how they can resist thought manipulation.

5.3.3 Generative Theorizing: Toward Transformation

Based on the understanding that the participants in the experiment could learn from the experimental situation when debriefed about the mechanisms of suggestibility and also that the experimental group benefitted from the opportunity to try out their new skills in avoiding suggestibility, Oczak and Niedźwieńska make some generalizations to other similar research situations. As indicated above, they infer the "the general idea of the extended debriefing … [to] provide participants with an opportunity to learn how to master social situations in which others seek to mislead, manipulate, or coerce them" (2007, p. 58). They believe that people can become empowered to "resist the undesirable influence and manipulation [e.g., they resist the tendency to obey, comply, fulfill a request, or accept a suggestion" (2007, p. 51)]. Hence they advocate that other researchers—especially those organizing

initially deceptive research—should incorporate an educative function in the research process, including a practical learning exercise. One could even extend this to any experimental research where researchers can set up opportunities for people to learn. For example, in my book entitled *New racism: Revisiting researcher accountabilities* (2010, pp. 151–152), I pointed to research investigating people's "implicit" racism, where participants through the debriefing process were enabled to rethink the associations that they were making between "Black" and negative stereotypes and "White" and positive images of people. (See also Monteith, Voils, & Ashburn-Nardo, 2001, p. 39; Devine, Forscher, Austin, & Cox, 2012, p. 1267.)

What I would suggest is important as an insight arising from Oczak and Niedźwieńska's experiment as designed by them, is that one does not need to conceptualize people as automatically submitting to cues that are given by experimenters or others in society: people can become more conscious of their "automatic" responses (what Oczak and Niedźwieńska call their initial "tendencies") and can learn new responses. Now considering my point that researchers need to be mindful of the messages they send out when organizing research and that the very organization of it is not neutral either for participants or for the wider society, let us consider the following:

Assuming that experimenters involved in deceptive research—or any research looking at people's initial responses—do not organize extended educational debriefings such as undertaken by Oczak and Niedźwieńska (which show both the participants and wider audiences the potential for learning), people in society may well believe that *it is extremely difficult if not impossible to avoid responding to suggestive misleading cues*. This means that they accept that they will not easily be able to resist this. And just by believing this, *these patterns become reinforced* for participants as well as for audiences who read the experimental results and notice how responsive people are to misleading cues. This then becomes seen as a "normal" and hence acceptable response. But if the experiment is set up to draw out the potential for people's learning—for example, via the new debriefing procedure—then both the participants and wider audiences recognize that people are not naturally dupes (easily duped by misleading cues) and can resist and overcome potential manipulation. Here the self-fulfilling impact of the research is again at play. The understanding is that *reality is in the process of being formed,* and that experimental procedures contribute to the way it becomes formed, either by reinforcing people's conceptions of supposedly normal patterns (such as the normality of submitting to suggestibility) or by offering alternative repertoires for responses.

Oczak and Niedźwieńska do not explicitly state the import of their experiment in these terms, but what can be called their forward-looking generative theorizing, is their offering the "message" (to use McGregor's terminology, 2003) that people need not be inclined to automatically submit to misleading cues from authorities, if opportunities are provided for them to look back reflectively on their "immediate" responses. This could mean that if this message is spread in the society as something for audiences to consider, coercive practices that depend on subtle manipulation of people's thoughts and feelings, will more easily be circumvented by people (because they will be more aware of options for resistance).

It should be noted that in their conclusion, Oczak and Niedźwieńska indicate that there is still further research work to be done along these lines: they suggest for, example, that because the volunteers in this experiment were mainly women (53 out of 60) this could have made a difference to the results that were generated. They also posit that because the post-test questionnaires were administered closely after the experiment, this too could have made a difference to the results that appeared. They therefore suggest further research with more mixed genders, and also perhaps the contacting of participants "several weeks or months after their participation" (2007, p. 58). In the meantime, the post-test administration of the questionnaires was a relatively simple process to conduct as part of their experiment; and they recommend at least providing some opportunity for participant feedback regarding the research experience.

Overall, one could say that they conceptualized this research as a vehicle that invites further deliberations in the research arena on caring as researchers, making efforts to benefit the participants, and indeed trying to make a difference to people's possible patterns of response in society more generally (for those reading the disseminated results of the research).

5.4 Conclusion

In both the cases chosen for discussion in this chapter, I focused on drawing attention to how the researchers conducting the experiments attempted to exercise some responsibility for designing the research process in a way that would potentially be empowering and educative for participants, and that could open up fresh deliberations for audiences engaging with the work. In his study, Stephens (2012), wished to use the research arena not only to "find out", but also to try to *activate* the potential of the recidivists who were the participants in the study. This was through the interventions in which he did all that was possible (as he saw it) to organize the training and his relationship with the participants so that they would most likely benefit. Various scales were used to try to "measure" people's benefits derived from the learning (where it was hypothesized that those undergoing Six Thinking Hats and EQ training would derive specific benefits also as compared with the "placebo" control group). Stephens also continued to interact with the participants after the close of the experiment and received further anecdotal evidence of learning and of participants' appreciation for having participated in the experiment. And he offered novel "generative theorizing" meant to be forward looking (transformative) for those others too engaging with the research.

In Oczak and Niedźwieńska's (2007) study, the experiment was designed to introduce the new educative debriefing procedure in order to compare it with standard debriefing procedures. The researchers wished to see how participants in the experimental group responded to the opportunity to try out their new skills, and also to ascertain whether such participants' mood, attitudes to psychological research, and self-esteem (as determined by various scales) might have been

affected by this provided opportunity. The idea was that if the results turned out as hypothesized, this would suggest that participants, given knowledge about misleading suggestive cues, and afforded them the opportunity to try out their skills in avoidance hereof, could fruitfully learn, and also that they would appreciate this opportunity (as reflected in their mood, attitudes and self-esteem). As it turned out, self-esteem did not differ significantly for the experimental group as compared with the control group, but Oczak and Niedźwieńska offer one interpretation that this is because participants in the control group (as in the experimental group) might have felt respected merely by virtue of being asked about their feelings—hence their self-esteem was preserved. What Oczak and Niedźwieńska's experiment also revealed is that the new debriefing did significantly increase the experimental group's mood relative to the control group, and also that this group found the research to be more meaningful and valuable.

Oczak and Niedźwieńska encourage researchers setting up research designs (especially those setting up initially deceptive ones) to afford participants an opportunity to learn certain skills, which presumably they can carry over into their everyday life to the extent that they find these meaningful. Meanwhile, insofar as the results can be interpreted as that some learning has been achieved, other audiences reading the results can recognize that people can become empowered to consider wider repertoires of response than their initial ones. This means in effect that researchers can contribute to injecting discourses into the wider social fabric which are potentially empowering (and hope-forming) for participants as well as for other audiences.

As I indicated in Chap. 4 (Sect. 4.4), Lincoln (1995, p. 277) points to research which is "politically and ethically sensitive" as including:

1. a professional content (with professional researchers having some experience and skills that they bring to the research context);
2. a personal dimension (based on personal concerns of those involved in the research, including professional researchers); and
3. a political dimension (which acknowledges that no research is neutral in its consequences).

In terms of point 1., Stephens as well as Oczak and Niedźwieńska brought professional skills to bear as designers of the experiments. Indeed, they took the lead as professionals in the way the experimental procedure was used; but they also took care to design the research so that it could potentially be meaningful for the participants. Stephens' prior contact with the prisoners through, for example, his being a pastor to them, would have stood him in good stead to try to design an experiment that could be experienced as helpful to the participants. Oczak and Niedźwieńska's understanding from their review of the literature that participants can easily feel "cheated" after participation in deceptive experiments, also stood them in good stead to try to cater for this possible feeling (as reported by, for instance, Brody, Gluck, & Aragon, 2000; Oliansky, 1991; Ortmann & Hertwig, 2002). This sensitivity by both Stephens and Oczak and Niedźwieńska to how the

participants might feel from participating in research (even though formal consent had been sought) could be said to be in line with ethical principles as advocated by Indigenous research methodologists such as Chilisa (2012), Kovach (2009) and Smith (2012). Researchers propounding the importance of Indigenous research methodologies argue that often, when giving their "consent", participants still do not understand the implications of this for how they will feel after participating. They suggest that this is especially the case for marginalized participants in society. Hence researchers are obligated (as part of "professional" conduct) to attend to the question of the meaningfulness of the research for participants. (I follow up this point further in Chap. 8, Sect. 8.2.1.)

In terms of point 2., it is noteworthy that Stephens designed the experiment so that the two experimental groups could both benefit from the "treatment" training, and even the control group was given some "placebo" training, in an attempt to ensure that all groups could derive some benefit. Stephens clearly cared for the participants (as shown also by the care he took in all the training sessions) and of course his choosing to work with recidivists whom he notes are stigmatized in various ways in society, evidences his care to still try to offer them some hope. Oczak and Niedźwieńska show care for those research subjects who are subjected to deceptive psychological research and may feel used and cheated as a result of such participation, if they derive no (experienced) benefit from it.

In terms of point 3., both Stephens's and Oczak and Niedźwieńska's designs imply that research has a political dimension. The researchers acknowledge that the research is meant to make a difference toward empowering participants and others concerned. In Stephens' case, he was not only *investigating* the effects of certain types of training, but trying to *activate the potential for empowerment of recidivists* to find options for hope in the face of the odds against them. He was also attempting to set an example via his research of how one can "give a chance" to prisoners and interact with them beyond the more usual stigmatizing frameworks. In Oczak and Niedźwieńska's case they were trying to activate the potential of participants during the experiment, and by implication, outside of this, to resist misleading cues of those who use thought manipulation as a mechanism of control. They accept the responsibility of recognizing that research can make a difference at the moment of research to participants' lives/conceptions, and (more widely) to those of audiences through the way the research is undertaken. Oczak and Niedźwieńska also display what can be said to be a political allegiance to the vulnerability of research subjects when they urge researchers to take care to design research (even if it is initially deceptive) in such a way that it can be a learning encounter for participants. They thus clearly show that they are "on the side of" research participants who otherwise might feel that they have been used by researchers in a non-reciprocal relationship.

As far as theorizing on the basis of the research is concerned, both Stephens (2012) and Oczak and Niedźwieńska (2007) admit that a variety of interpretations of their results, as examined from a statistical reading of the pre- and post-test scores of the participants on the various scales, may be possible. They therefore do not present their research with any kind of finality. However, they do offer some generative theorizing (which I have further drawn out and extrapolated), based on

what I would call not only deductive but also abductive or retroductive inferencing. Here they make some leaps of the imagination in relation to the "facts" as presented by the scores, for readers to take into consideration as part of our continued engagement with their texts. (In Chap. 7, I follow up the issue regarding the logic at play in generative theorizing, which I suggest the authors are implicitly drawing upon.)

References

Belmont Report: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research (1979). *Ethical principles and guidelines for research involving human subjects*, Washington DC, Office of the Secretary, US Department of Health and Human Services.

Brody, J. L., Gluck, J. P., & Aragon, A. S. (2000). Participants' understanding of the process of psychological research: Debriefing. *Ethics and Behavior, 10*(1), 13–25.

Cannella, G. S., & Manuelito, K. D. (2008). Feminisms from unthought locations: Indigenous worldviews, marginalized feminisms, and revisioning an anticolonial social science. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and indigenous methodologies* (pp. 45–59). London: Sage.

Chilisa, B. (2009). Indigenous African-centered ethics: Contesting and complementing dominant models. In D. M. Mertens & P. E. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 407–426). London: Sage.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Colliver, R., Goff, S., Reedy, R., & Vaartjes, V. (2015). Systemic pedagogy: A design for action researcher collective self-development. *Action Learning Action Research Journal, 21*(1), 1–22.

Cram, F., & Mertens, D. M. (2016). Negotiating solidarity between Indigenous and transformative paradigms in evaluation. *Evaluation Matters—He Take Tõ Te Aromatawai, 2*, 161–189.

Davis, J. E. (2012). Emotions as commentaries on cultural norms. In A. M. Gonzālez (Ed.), *The emotions and cultural analysis* (pp. 31–49). Surrey, England: Ashgate.

De Bono, E. (1982). *Lateral thinking for management*. Harmondsworth: Penguin.

De Bono, E. (1985). *Six thinking hats*. Boston, MA: Little Brown.

Devine, P. G., Forscher, P. S., Austin, A. J., & Cox, W. T. (2012). Long-term reduction in implicit race bias: A prejudice habit-breaking intervention. *Journal of Experimental Social Psychology, 48*(6), 1267–1278.

Gergen, K. J. (1978). Toward generative theory. *Journal of Personality and Social Psychology, 36*(11), 1344.

Gergen, K. J. (2015). From mirroring to world-making: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.

Gudjonsson, G. H. (1984). A new scale of interrogative suggestibility. *Personality and Individual Differences, 5,* 303–314.

Kinai, T. K. (2013). Kenyan student-teacher counsellors' creativity and its relationship with their gender, age, and teaching experience. *US-China Education Review, 3*(5), 296–304.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.

Lather, P. (1986). Issues of validity in openly ideological research: Between a rock and a soft place. *Interchange, 17*(4), 63–84.

Lincoln, Y. S. (1995). Emerging criteria for quality in qualitative and interpretive research. *Qualitative Inquiry, 1*(3), 275–289.

Magnat, V. (2012). Performative approaches to interdisciplinary and cross cultural research. In L.-H. Smith & A. Narayan (Eds.), *Research beyond borders* (pp. 157–177). Plymouth, England: Lexington.

McGregor, S. L. (2003). Critical discourse analysis-A primer. *Kappa Omicron Nu FORUM, 15*(1), 1–15.

McIntyre-Mills, J. J. (2014a). *Systemic ethics and non-anthropocentric stewardship*. New York, NY: Springer.

McIntyre-Mills, J. J. (2014b). *Transformation from Wall Street to wellbeing*. New York, NY: Springer.

Mertens, D. M., & McLaughlin, J. A. (2004). *Research and evaluation methods in special education*. Thousand Oaks, CA: Sage.

Monteith, M. J., Voils, I. C., & Ashburn-Nardo, L. (2001). Taking a look underground: Detecting, interpreting, and reacting to implicit racial biases. *Social Cognition, 19*(4), 395–417.

Nassif, C., & Quevillon, R. (2008). The development of a preliminary creativity scale for the MMPI-2: The C scale. *Creativity Research Journal, 20*(1), 13–20.

Oczak, M., & Niedźwieńska, A. (2007). Debriefing in deceptive research: A proposed new procedure. *Journal of Empirical Research on Human Research Ethics, 2*(3), 49–59.

Oliansky, A. (1991). A confederate's perspective on deception. *Ethics and Behavior, 1*(4), 253–258.

Ortmann, A., & Hertwig, R. (2002). The costs of deception: Evidence from psychology. *Experimental Economics, 5*(2), 111–131.

O'Sullivan, M. (2007). Trolling for trout, trawling for tuna: The methodological morass in measuring emotional intelligence. In G. Matthews, M. Zeidner, & R. D. Roberts (Eds.), *The science of emotional intelligence: Knowns and unknowns* (pp. 258–287). Oxford: Oxford University Press.

Romm, N. R. A. (1997). Becoming more accountable: A comment on Hammersley and Gomm. *Sociological Research Online,* 2(3), Art. 2.

Romm, N. R. A. (1998). Caricaturing and categorizing in processes of argument. *Sociological Research Online, 3*(2), Art. 10.

Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York, NY: Springer.

Salovey, P., & Mayer, J. D. (1990). Emotional Intelligence. *Imagination, Cognition and Personality*, *9*(3), 185–211.

Smith, L. T. (2012). *Decolonizing methodologies: Research and indigenous peoples* (2nd ed.). New York, NY: Zed Books.

Stephens O. A. (2012). *Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, Nigeria.* Doctoral dissertation, Lagos State University, Lagos, Nigeria.

Stephens, O. A., & Badejo, A. O. (2010). Emotional intelligence training as an intervention strategy in remediating anti-social behavior of some selected prison inmates from dysfunctional families. *The Counsellor, 27,* 29–37.

Wane, N. N., Akena, F. A., & Ilmi, A. A. (2014). Introduction. In N. N. Wane, F. A. Akena, & A. A. Ilmi (Eds.), *Spiritual discourse in the academy* (pp. 1–11). New York, NY: Peter Lang.

Wilkinson, R. G., & Pickett, K. (2009). *The spirit level: Why more equal societies almost always do better*. London: Allen Lane.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.

Chapter 6
Development-Oriented Research to Forward Social and Environmental Justice: Responsibly Strengthening Discourses and Actions Toward an Inclusive Wellbeing

Abstract This chapter examines some examples of development-oriented research directed toward actively furthering social as well as environmental justice (McIntyre-Mills, Report prepared for South Australian Local Government Association. Flinders University, Adelaide 2013; McIntyre-Mills, Systemic ethics and non-anthropocentric stewardship. Springer, New York, 2014a; McIntyre-Mills, Transformation from Wall Street to wellbeing. Springer, New York, 2014b; McIntyre-Mills, Planetary passport. Springer, New York, 2017; Roman-Alcalá, Paper presented for discussion at an international conference held at Yale University entitled Food Sovereignty: a critical dialogue 2013; Roman-Alcalá, Globalizations 12(4):545–558, 2015; Maathai, Unbowed: A memoir, Anchor Books, New York, 2006). I start with McIntyre-Mills' facilitation of research (in conjunction with a local government association) in a region of South Australia. The research involved employing a software package with which participants were encouraged to engage in an interactive fashion to expand their reflections around social and environmental issues in relation to alternative development scenarios. I then discuss the deliberations of Roman-Alcalá (Globalizations 12(4):545–558, 2015) as he reflects as an insider on a case of "Occupy The Farm" on Earth Day at the University of California in 2012. Based on interviews and conversations with some of those involved (and his own theorizing) he explores the intention of the activists toward transcending economistic thinking within a global movement for social justice and deep ecological thinking. Finally, I turn to the autobiographical narration of Maathai (Unbowed: A memoir, Anchor Books, New York, 2006) in relation to her involvement in founding the Green Belt Movement in Kenya. I class this as an example of transformative autoethnographic research which expresses her learning and her critical spirit, while inspiring practices undertaken with a similar spirit of care for people and the land.

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_6

6.1 Introduction

In this chapter I discuss some examples of research aimed at making a difference to the enactment of social and environmental justice by tying research to (civic) action. By social and environmental justice, the authors whom I have chosen to showcase mean to include a concern not only with more social equality and improvement in quality of life for people on the planet as a development agenda, but also a concern with more compassionate relations with the natural world as in deep ecology movements (Maathai, 2006, p. 122; McIntyre-Mills, 2014a, p. vii; Roman-Alcalá, 2013, p. 25).

The first research example, which I discuss in detail in the chapter, is an example of research facilitated by McIntyre-Mills, as summed up mainly in her books entitled *Systemic ethics and non-anthropocentric stewardship* (2014a) and *From Wall Street to wellbeing* (2014b). She explains that the concept "Wall Street" is used to "symbolize economic rationalism"; and the notion of "wellbeing" is used to symbolize "a reframed approach to 'being interconnected' and … treading lightly [rather than heavily exploiting the earth's resources)]" (2014a, p. xiv). She points out, following authors such as Bostrom (2011), that the privileged lives of some could lead to existential risk for people and the planet (Bostrom 2011, as cited in McIntyre-Mills, 2014a, p. xiv). As with Chap. 5, when I scrutinized the research write-ups, I offer my interpretations of her texts (supplemented with a number of personal communications[1]), drawing out aspects of researcher responsibility that I feel can be further extrapolated from the work.

McIntyre-Mills states (2014a) that her starting assumption that a radical cultural transformation in dominant ways of thinking about "wealth" is needed was inspired by her many interactions with Indigenous people of Australia and of South Africa, who operated for many years as mentors to her in teaching her about living more elegantly (and simply).[2] She explicates (2014a, p. 143) that

> Indigenous people the world over do not draw the line so strongly between self, other and the [natural] environment. … . A spiritual connection with their environment gives a sense of awe and reverence. (2014a, p. 143)

She indicates that she has drawn inspiration, for example, from the work of Wangari Maathai "who founded the green belt movement by encouraging the simple act of planting trees in Kenya" and mobilized people "to take action through small steps toward green living" (McIntyre-Mills, 2017, p. 19). She admires

[1]Apart from working at Flinders University in South Australia (and being an adjunct professor at the University of Indonesia), Janet McIntyre-Mills attained the position of honorary professor at the University of South Africa (2014) and she has visited South Africa several times since then; we have thus had various occasions in which I have spoken to her to further explore her intentions.

[2]In a talk given in Cyprus in 2015 as part of a conference organized by the World Futures Centre, she noted that "as a social anthropologist and sociologist I have drawn inspiration from people with whom I have learned whilst undertaking field work in a range of contexts" (http://www.sddp-international.org/SDDP2015Conf/index2.html: see row two, column two).

Maathai's (2004, 2010) "humming bird approach", which suggests that "it is better to make a small contribution than to do nothing. … . In taking action we can inspire others to join us" (2017, p. 19). Similarly, the work of Shiva (2002) provides another source of inspiration for McIntyre-Mills. She draws from Shiva's work that "we are all seeds for change and we are reliant on water, earth and seeds for our survival". She indicates that Shiva has "also reminded us that we return to the soil and that we are thus connected to the earth worm and the organic life that is rooted in the soil" (McIntyre-Mills, 2017, p. 20).[3] And she comments that the work of Rose (1996) on multi-species ethnography and extinctions provides us with another way to help us think about the possibilities for non-anthropocentricism as an approach to caring (2017, p. 20).

These works in turn influenced McIntyre-Mills' development of a research project in a region of South Australia, initiated in 2011. The research was aimed at encouraging people (in this case the residents of a particular area with pseudonym Middleville) to reflect on their way of being in the world, and on how they can support and be supported by local government in enacting new ways of living. (This was a locality which was mainly what could be called a middle class area, but also residing there were some people who considered themselves as being poor or facing poverty—as mentioned to me in personal communication with McIntyre-Mills, 14 January, 2016.)

In my portrayal of this research I refer in some detail to the report (2013) which McIntyre-Mills constructed for the South Australian Local Government Association (SALGA) at the "end" of what was regarded as the pilot project, as well as to her various books that she constructed following this report (2014a, b, 2017). I concentrate on her account of how she conceives research in general and in particular this research endeavor which she facilitated. I use the word "facilitate" following Onwuegbuzie and Frels' suggestion (2015, p. 162) that researchers working in the tradition of a "critical dialectical pluralism" (a stance which valorizes a critical review of perspectives) should more aptly be named as "research facilitators". I see the label of "research facilitator" as indeed appropriately applying to the work that McIntyre-Mills undertook, which was based on deploying (and further developing) a software package with which participants could interact as part of reflecting upon and extending their views on lifestyles and implications for local government policy.

I spell out how (in McIntyre-Mills' account) the software package encouraged participation by residents in the community while McIntyre-Mills simultaneously urged them to review their life choices. The attempt was to try to encourage those accustomed to link status with material wealth to recognize that other choices can be made. (The software had earlier been co-designed for another project to explore

[3]She cites Shiva's account (2002) of the manner in which, as she summarizes, "global markets and mainstream research supported by corporate profits have disempowered local producers and in particular women farmers whose knowledge of plants was challenged by patriarchy and the 'enlightenment' during the witch hunts in Europe and America and then extended through the commodification [across the globe] of seeds through patents" (2014b, p. 123).

decision making to enhance wellbeing for Aboriginal service users and providers. It drew on the Indigenous philosophy that many interrelated factors together shape wellbeing. The software was designed to map pathways to wellbeing.[4] The software initiative to map pathways to wellbeing was then extended to the research coordinated with SALGA.[5])

The second example which I discuss is Roman-Alcalá's case study (written up as an insider) of his reflections on a case of Occupy the Farm (OTF). He presents it as a study of civil society tactics to introduce communal and collaborative ownership in the management of a 10-acre tract of land which had been owned by the University of California at Berkeley (UCB). As he expresses it,

> hundreds of participants (mainly made up of UCB students and alumni, organizers and participants from Occupy Oakland, and other urban farming advocates) marched from a nearby park, broke the locks on the Gill Tract's gates, entered, and proceeded to remove weeds and replace them with over 15,000 vegetable starts. (2015, p. 552)

The idea, as he interprets it, was to build "discursive legitimacy for food sovereignty" based to some extent on Food Sovereignty (FS) movements in other parts of the globe, and "reach for more transformative goals while achieving improvements locally, in the here and now" (2015, p. 550). The decision to occupy the land was taken after negotiations had been attempted with the university by a "network of 30-plus community-based and non-profit organizations", under the auspices of the Bay Area Coalition for Urban Agriculture (BACUA) which had been "attempting to integrate the local community's interest and pursuit of food security and economic justice with UCB's existing resources at the Gill Tract" (2015, p. 551).

BACUA and other groups had tried over a protracted period to negotiate with the university, to utilize the tract of land for educational sustainable urban agriculture. The university, however used it for research in molecular genetics with biotechnology applications—that is, a technology and capital-intensive production orientation, with focus on "product-outcome potential such as genetically engineered seeds" (2015, p. 551). The research at the Gill Tract had been linked to the use of

[4]McIntyre-Mills indicated to me by email (March 2017) that the original initiative was funded by the Australian Research Council, Neporendi Council and the (then) South Australian Department of Health. The software was funded together with her co-researchers "Assoc. Prof. Douglas Morgan, Senior Lecturer, Bevin Wilson, informatics designer Denise De Vries and several mentors including senior researcher Kim O'Donnel". Douglas Morgan was at the time also" chair of Neproendi, an advocacy organization with and for Aboriginal people living in the Southern Region and Bevin [from Flinders University] had been his senior mentor".

[5]McIntyre-Mills clarified by email (March 2017) that: "At this next stage of the research I worked with some other researchers, but Bevin remained mentor. The scenarios presented as a basis for the local government research flowed from what I had learned from these mentors and the many informants in Alice Springs where I had previously worked and where my mentors stressed the importance of wellbeing as the outcome of many interrelated aspects that were explained in dreaming stories, such as Mpantwe (Caterpillar Dreaming) … . The many other narratives by leaders helped inform the design of scenarios which were then explored in the next project [with SALGA]".

gene technologies, with university researchers benefitting from patents that became established. This was at a time when, as noted by Roman-Alcalá, the public was coming to demand changes in agricultural practices away from agribusiness (2015, p. 160). Meanwhile, the university was planning to pave over/develop the land, and this, according to Roman-Alcalá (pers. comm. via email, 3 July 2016) is what "precipitated the occupation".[6]

The intention of the activists (as gleaned from Roman-Alcalá's interviews and personal communications with many of them) was to institute research and action toward chemical-free agricultural systems not based on trying to maximize short-term profits, but based on deep ecological sustainability thinking and the production of "good food" (http://insearchofgoodfood.blogspot.co.za/), and also based on the idea of "collaborative governance" of this commons. I consider some of Roman-Alcalá's arguments regarding why this initiative in this case could be said to have "worked" in practice (in that a collaborative farm management scheme was set up) and how it can be treated as inspirational—through generating and strengthening narratives that run counter to state and/or market control principles—rather than as prescriptive of "what must be done" in other contexts.

Finally, as my third example, I look at extracts from the late Maathai's (2006) autobiographical account of her involvement in addressing gender and environmental issues in Kenya—that is, extracts in relation to her founding of the Green Belt Movement (GBM) in Kenya.[7] (In 2004 Maathai was awarded the Nobel Peace Prize Laureate for her contribution to sustainable development, democracy and peace.) She explains how being active in the National Council of Women of Kenya (NCWK) gave her a platform to found what later became called the GBM. She remarks that the leadership of the NCWK was "generally elite and urban" but "we were concerned with the social and economic status of the majority of our members, who were poor, rural women" (2006, p. 123). She narrates how her involvement in NCWK and other networks of women's organizations globally was linked to her concern with environmental issues (specifically desertification arising, *inter alia*, through deforestation linked to the pressure for "development" begun under colonial rule).

[6]As will be seen in Sect. 6.3, Antonio Roman-Alcalá inserted a number of additions at different places into my draft text (that I had previously emailed to him). This too was one of his insertions.

[7]In outlining the history of the Green Belt Movement she indicates that:

> The Green Belt Movement is a grassroots Non-Governmental Organization (NGO) that focuses on environment conservation and development. It does this mainly through a nationwide grassroots tree-planting campaign that is its core activity. Unlike many other organizations in Africa, it is not a branch of a foreign NGO but an indigenous initiative, registered and headquartered in Nairobo. (2004, p. 6)

She also remarks (2006, p. 329) that the GBM did not only plant trees, but planted *ideas*, such as ideas linked to empowering people. She sums this up as follows: "By planting trees, my colleagues in this grassroots movement and I planted ideas. The ideas, like the trees, grew" (2006, p. 329).

What I focus on in my account of her storying around the GBM, are the lessons that are threaded through her text—lessons that she learned along the way as she reflects on the actions of herself and others. Her research in action (as a reflective practitioner) involves her theorizing, grounded in practice, of what becomes possible if one searches, with others, for solutions to problematic issues seen from the angle of social and environmental justice in the context in question (in this case Kenya). I look at her text by considering how she writes up her learning journey, for example, her coming to recognize—based on her own and others' research—the multiple costs of the planting of exotic trees for the timber industry, combined with the cutting of indigenous forests to make way for tea and coffee plantations. I focus on her search with others—which can be regarded as research in action—for context-based solutions on a grassroots level.

6.2 Deployment of Participatory Decision-Making Software (South Australia)

As indicated in my Introduction this research was spearheaded by McIntyre-Mills in conjunction with the South Australian Local Government Association (SALGA) who funded the project. (See also footnotes 4 and 5 for detail on this project as an offshoot of earlier initiatives in other contexts in which the software became co-designed.) This was meant to be a pilot project (2011–2012) for use in local government and McIntyre-Mills notes that efforts to improve upon it and apply it elsewhere are still ongoing (2014b, p. 19[8]). In her report to SALGA McIntyre-Mills refers to the initial inspiration for the project:

> The philosophy underpinning the approach grew out of the research in Alice Springs and in the Southern Region with Aboriginal Australians who stressed that "we are the land". Olive Veverbrants, an Arrerente Australian, stressed "the earth is our mother" and we depend on it for our survival. (2013, p. 10)

In the report and in her books McIntyre-Mills emphasizes that:

> Our attitude to what constitutes normal usage of the planet's resources is unsustainable. Democratization to ensure a fairer use of the world's resources needs to ensure that a redesign of living standards occurs—and in the meantime governance controls need to achieve both the contraction of resources (Beck 2005) used by the rich and greater convergence across living standards for all. In a post consumerist world, wealth needs to be re-evaluated, because … we cannot solve the problems of today with the same ideas of *property and consumption* that created the problem of an *unsustainable* way of life. (2014a, p. 123)

[8]For example, in West Java (Indonesia) she has being working with collaborators to introduce a (revised) process (http://www.sddp-international.org/SDDP2015Conf/index2.html).

The purpose of the research was to inspire people to think about the consequences of their ways of being and acting in terms of their quality of their and others' lives and also consequences for planetary wellbeing. Based on the idea of stimulating reflection around ways of living, McIntyre-Mills together with SALGA arranged for focus group sessions to be held in the local library, where residents were invited to consider the parameters of three different scenarios, which were called:

- Business as usual
- Small changes
- Sustainable living

The following diagram depicts what were considered to be the essence of these scenarios (McIntyre-Mills, 2013, p. 6, reproduced in 2017, p. 181) (Fig. 6.1).

McIntyre-Mills explains the features of the scenarios—which appear as descriptions in the software package in audio as well as written form—as follows (2014b, pp. 51–52):

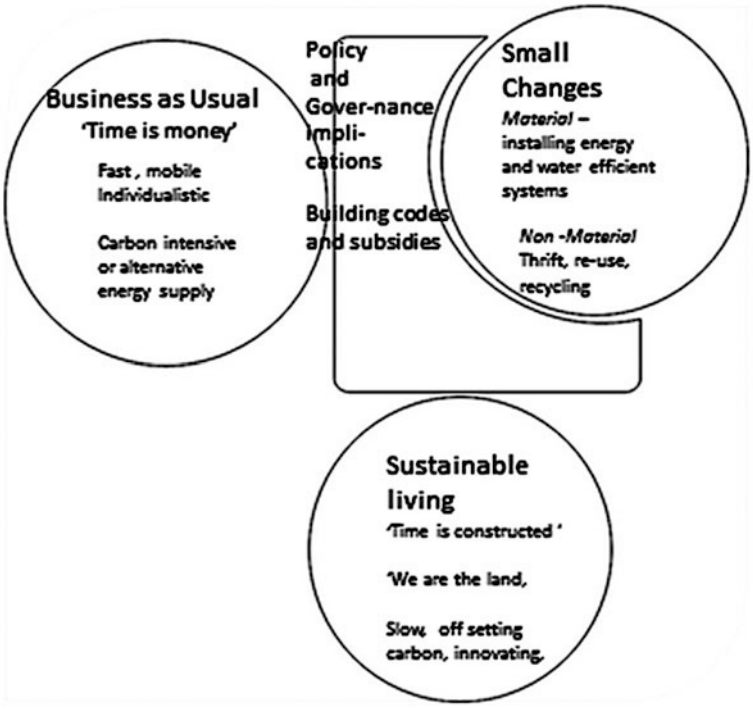

Fig. 6.1 Three scenarios located. *Source* McIntyre-Mills (2017, p. 181)

Business as usual

We continue to believe in economic arguments that others believe ignore the social and environmental dimension. We continue to think that our way of life is sustainable and are not prepared to manage the perceived risks of climate change by changing our way of life. We attribute drought, bush fires and floods to one-off unrelated events or natural cycles, and deny that climate change can trigger rising temperatures in some areas and plummeting temperatures in others as melting ice affects the ocean currents.

Small changes for the long-haul scenario

People make slow annual progress toward goals which they meet for the benefit of their children and grandchildren. People of all ages and from all walks of life who are able to join up the dots between the economic, social and environmental dimensions help to motivate movement toward a better future. We do not perceive these small changes as being too slow to sustain beyond our grandchildren, or we envisage that something else will happen by then to reverse the current trend. Governments and nongovernment organizations take the initiative. They hold workshops to demonstrate how people can make a difference. They listen to the people and help local groups to respond to local challenges. Together, they undertake model projects that demonstrate how it will be possible to live differently. They model different ways of thinking and through living the changes show that it is possible to balance individual and collective interests, because we are not selfish nor are we unable to create alternative ways of governing at a regional level.

Sustainable future scenario

We live in an environment that can support this generation and the next. Housing is affordable and made of sustainable materials. We have faced up to the convergent social, economic and environmental challenges and we are resilient, because we live in clusters of homes, share rain tanks and solar grids that are subsidized by local governments. Our living and working areas are powered by alternative energy. The new status symbol is the environmentally friendly lifestyle. Public transport is green. The green economy supports a vibrant job market spurred by subsidies to enable packaging goods, housing people, transporting people and educating and entertaining the public. The carbon economy is replaced through innovative inventions. All members of the public are encouraged to share their experiences and ideas for living sustainably. The future's market has been reconstructed to take into account the air, water and earth we need to grow organic, safe food. We have thought carefully about the implications of treating people, animals and the land as commodities and we strive to care for ourselves, others (including the voiceless) and the land.

The characteristics of these scenarios as fleshed out by focus group participants and written up with some extensions by McIntyre-Mills were then fed into the software package so that all the research participants interacting with the package could consider to what extent they identify with the scenarios as defined. The details of the scenarios can be listened to and also visually seen as one "opens" the package (this is after filling in some demographic data). The identification with a scenario which seems most fitting provides a starting point for people to consider further what they have, what they want, what they need, what they can discard, and what turning points may be possible in their lives, while also considering what services at local government level are available for more sustainable life styles and what additions may be needed (so as to inform local government decision making).

Having "clicked" an identification with one of the scenarios, participants can then move to create their own pathway, which is a reflection of their particular haves, wants, needs, possible points to discard in their lives, what would constitute a turning point for them, what the barriers are, and what actions they can envisage to make changes. Certain ideas of possible needs, wants areas to discard, turning points, and actions are available for participants to peruse in the populated software (populated with options), and if they wish they can click on these as part of their pathway; but they can also add content to their pathway. This added content is then available for others to access (in de-identified form). That is, along with clicking what they find as relevant in existing content, they can add some content. They can also at the same time express their perceptions and emotions—because the software allows a certain number of words to be added in each section (2013, p. 3). What people have added as they interact with the software becomes accessible to others who open the software at a later date. In this sense the software enables people to interact with the choices that were originally on the package and also with what others have added; and they can add options and ideas themselves so that others too can have the benefit of seeing their content added. For some people who did not do the "clicking" themselves, research assistants were available and McIntyre-Mills herself also took part in assisting would be participants to "fill in" the form by verbally expressing their views to the researchers, who then added the data in the package.

Services available in the community for operating a more sustainable lifestyle are also given as information in the software package (so that those interacting with the software package are aware of these, and they can also add other services made available by local government or by any others, such as those running community gardens, etc.). Under "actions" people can also state what additional services they may require. This is then useful information for local government to take into account. As McIntyre-Mills explains, "local government is able to address policy and governance based on an analysis of their [residents'] perceptions and choices" (2014b, p. 18).

McIntyre-Mills summarizes the way the software works:

> The software provides a way to enable people to explore the question, how should we live and what constitutes wellbeing? Participants are invited to consider what have, they need, what their turning points are for the better or the worse and what the barriers are. Services that help them meet their needs are detailed as well. The software updates as it is used. (2014b, p. 8)

The actual way in which participants are invited to engage with the software as expressed to participants when they open the package (after filling in some demographic information) is as follows:

> Please listen to three scenarios. Think which of the three scenarios you identify with. This is the entry point for the software. Please build your own unique choices and insights into the program by starting the pathways journey.
>
> Please choose one of three pathways. The software does not make decisions; it enables people to think about whether they identify with: Business as usual, Small changes or sustainable living that support wellbeing now and in the future. Participants are invited to consider their unique circumstances.
>
> Please consider: what material and non-material things you have in life—positive and negative—such as rented accommodation, a mortgage, a supportive family, a job? What you need—a well-insulated home, a bicycle, access to public transport? What you could add/discard—exercise/costly vehicle? What are the positive and negative turning points—adding solar panels, learning more about sustainable gardening? What are the barriers—lack of motivation, lack of subsidies for green technology? What local government services can be used and in what combination?
>
> Your own pathway to wellbeing can be printed out and kept for personal review. You can revise it on line. (McIntyre-Mills, 2013, p. 2)

As it turned out, the invitation for people to revise their initial responses did draw many participants to indeed do this. By July 2012, 81 participants had engaged with the software, and there were 350 pathways … which means that many of the participants had chosen to enter again and redefine their "pathways to wellbeing" (2014b, p. 61).

6.2.1 The Research Design

The research design consisted firstly in McIntyre-Mills' formation of a team to design the software so that it could function to enable people participating in the

research to reflect upon their lifestyle choices.[9] As indicated above, one of the features of the design is that it could update and grow as participants add new content into the system. McIntyre-Mills and the team also sought feedback from participants in Middleville regarding their experience of using the software so that it could be improved along the way to be more user-friendly. Various versions of the software in accordance with feedback received were produced.

The steps involved in the whole research design were as follows (as outlined by McIntyre-Mills, 2013, p. 1):

1. The gathering of data through focus groups discussing three scenarios and defining their contours and characteristics—so that these could be fed into the software for people to engage with.
2. The development and piloting of the software in the local area of "Middleville", with some residents offering ideas on how it could be improved to make it more user-friendly. The software was meant to be a pilot project which other local governments could use and which indeed could be scaled up to be used at regional, national and even post-national levels to invite participants to participate in "democracy from below". This was in terms of a model of democracy where people are mindful of their responsibilities to others and to the planet and express their views accordingly (McIntyre-Mills, 2014b, p. 75). The participants were recruited via what can be called convenience sampling (cf. Onwuegbuzie & Leech, 2007, p. 114): certain residents who responded to an advert sent out in the local newspaper by SALGA came to the library at set times to learn about how to use the software, so that they could participate; and McIntyre-Mills with assistants (e.g., Ph.D. students) also recruited people in shopping malls and in the community gardens venue. By July 2012, 81 participants had participated (2014b, p. 62). The remit of the project as far as her interaction with participants in the locality was concerned was to enhance local residents' awareness and to help local government decision makers to understand the needs and perceptions of local voters. The software was intended to "help with raising consciousness which can provide a step toward the creation of a new suburban culture" (2014b, p. 18).
3. Certain analyses were created automatically by the software which converted information into spreadsheets, for easy perusal by the administrators of the

[9]McIntyre-Mills states in this regard (2013, p. 6) that the research was funded by the Australian Local Government Association and managed by Janet McIntyre (Social and Policy studies) to conceptualize and implement the project with Dr. Denise de Vries (Informatics, Flinders University) and David Hope (accountant and risk management consultant for local government). She also acknowledges support from the following:

- Sun Binchai (Hons graduate, Informatics, Flinders University),
- Ivantia Mokoginta (Ph.D. student, Economics and Public Policy),
- Adib Mohamned Shomad (Ph.D. student, Public policy and management),
- Barbara Dickson (MA student, Public Policy and engagement specialist for an urban renewal department, public sector, Adelaide),
- Dr John Mugabushaka (Ph.D. graduate and public policy and youth justice officer, public sector, Adelaide).

system (who had access to the data as a totality). These spreadsheets of course had to be further interpreted. The interpretations which McIntyre-Mills shares with us in her books are also informed by dialogues around the "results" that were held with interested participants and with stakeholders from SALGA. A report to SALGA (2013) offers her summary of the interpretations as relevant for them but also includes what I would call some generative theorizing that she created to be further inspirational. (See also Sect. 6.2.3 below on "results".)

4. Ongoing development of software and invitations to people to join the project or set up similar initiatives and also scale up the pilot project were encouraged.

McIntyre-Mills wishes to emphasize that this is an ongoing project and she hopes it can be used by user groups in different geographical localities (pers. comm., 14 January, 2016). In her book (2014b) she explains that:

> The findings will be shared iteratively with the other groups, using the Delphi technique to build a sense of overlaps and differences. Organizations such as 350.org39 could be used to share the free software and by linking, for example, the work of Ph.D. students and graduates on the relevance of critical systemic thinking. Cultural narratives that foster sustainability are a starting point for democratic transition. … We welcome the opportunity to work with you to continue the research. (2014b, p. 63)

She also explains that she regards this research as fitting into broader initiatives that she or others can facilitate. She sees this research as a first step—much in the way she notes (2017, p. 19) is embraced in Maathai's (2010) humming bird approach, wherein it is understood that actions although small in themselves, can become significant, and that one should partake in action to the best of one's ability (https://www.youtube.com/watch?v=IGMW6YWjMxw). McIntyre-Mills elucidates:

> This research explores people's perceptions of what works why and how, but also assesses whether the process of engagement enables them to move from denial toward making changes toward a more sustainable future. Contradictions need to be identified and addressed by policy (supported by legal frameworks) at the local, national and post-national regional level to achieve transformation. The challenge is to enable people to become aware of the need for a cultural shift from a large carbon footprint created by "business as usual" and to move toward making logical steps toward sustainable living. (2014b, p. 18)

By offering what can be called some generative (forward-looking) theorizing as part of the "design" of the research (which I explore in Sect. 6.2.6) she also points in anticipatory fashion to new options for thinking (and feeling), urging people (those reading her reports and other material) to "connect the dots" and recognize our connectedness in the fabric of life (2014b, p. 2).

6.2.2 Ethical Approach: Taking Responsibility for Admittedly Values-Based Research

McIntyre-Mills explicitly brings into her engagement with participants and with wider audiences her concerns that we should be more mindful of our relationships

with others and with the environment, and should not be linking our status (sense of self-worth) with living "high-carbon lifestyles" (2014b, p. 19), but rather with living what she calls "elegantly". She notes that the research was not meant to "find out" about people's experiences and views *as much as to inspire them to reflect anew on their experiences as they think anew about "pathways to wellbeing"*. She explains her position thus:

> They are able to see the typical scenarios of what people have, need, are prepared to add or discard from their lives and what they think are turning points for better or worse. A sustainable local community is determined by a sustainable region in which food, energy and water supplies are considered as major determinants for wellbeing. No community can be expected to transform from a high-carbon lifestyle (or aspiring to this lifestyle) without feeling part of the design process and owning the decisions as to how resources should be used. The exercise of "thinking about their thinking' is an attempt to build new neural pathways in the mind—to rehearse a different way of "being in the world", based on thinking about the consequences for themselves, others and the environment. (2014b, p. 250)

The starting point of her research was that she believed (and hoped) that via this exercise of "thinking about their thinking" people can be enabled to "rehearse a different way of 'being in the world', based on thinking about the consequences for themselves, others and the environment" (2014b, p. 50). She operates on the basis of the assumption that people are not static—and that how they enter the research at the start is not how they *might be after participating* in the process of re-reflection (and building new neural pathways). She argues in this regard that "telling and listening to narratives [as part of a research process] does not necessarily enable people to see things differently, unless they are *asked to consider the implications of different scenarios*" (2014b, p. 89). Clearly, via this project, she wished people to start to see things differently as they participate in the research (which implies seeing things differently from "business as usual").[10]

It is worth noting that she does not define "thinking" as separable from emotional involvement—and indeed she urges that people combine thinking more carefully with compassion. It is this, she suggests that will enable people to "rebuild the connections with one another and the environment" (2014b, p. 89). This ethical position is similar to Molefe's interpretation of how the African conception of reality as consisting of relationships (being-in-relation) is as much a statement about how reality "is" as a statement about how we ought to act to shape "it" so that it becomes more connected (Molefe, 2014, p. 128). This ethical injunction to enhance connectedness implies certain responsibilities on the part of us as researchers/research facilitators. McIntyre-Mills indicates that:

[10]This is consistent with Salleh's point (2015, p. 1) that "climate politics will go nowhere as long as peoples' movements remain locked into debates over arithmetic. It is time to re-set the start line for climate struggles in a place that transcends the old episteme" (http://ppesydney.net/another-climate-strategy-is-possible/). McIntyre-Mills adds that besides being locked in disagreements about arithmetic, we also need to rethink outdated narratives.

> The responsibility to ensure that some do not prosper at the expense of others is a core challenge. It [the research] considers the potential of human beings to address social, economic and environmental factors that support or undermine adaptation to the challenges and mitigation of the effects of climate change. It evaluates the extent to which interactive engagement approaches enable the community to consider their perceived assets and risks and the implications of their consumption choices for developing wellbeing stocks. (2014b, p. 88)[11]

Like Molefe, McIntyre-Mills pleads for (and brings into the research agenda) a non-anthropocentric ethics, where human wellbeing is not sought at the expense of caring for non-human realities. She contends that it is important that we draw on a range of spiritual traditions which express awe for the natural world (including animals) and accordingly do not see it as exploitable for human use. As she states, the challenge is to "create new global narratives arising out of a cross-pollination of spiritual ideas from a range of religious and spiritual practices" (2014b, p. 7). McIntyre-Mills and Molefe are in agreement that a humanism which fails to express our connectedness with all living and nonliving things needs to be challenged. Molefe notes in this regard that aside from certain Western forms of humanism, there are versions of African humanism that can become too anthropocentric. As he states:

> I reject humanism as a basis for African ethics because it fails to capture some of the prevalent thoughts and intuitions we Africans typically have about our duties toward the natural environment—the idea that, in some sense, we are one substance with nature (Murove 2007) and that some aspects of nature matter for their own sakes to some degree. (2015, p. 59)

Molefe concurs with Murove (2005, 2007) that "a truly African ethics must cohere with a holistic and supernaturalist tenor that often characterizes African ontology, which in turn demands that we accord moral status to … aspects of the environment, like animals, for their own sakes" (Molefe, 2015, pp. 59–60). See also my discussion in Chap. 1, Sect. 1.3.1, concerning how Murove links this ethic with critiques of economic models based on (selfish) profiteering at the expense both of humans and of the environment. This is what McIntyre-Mills also stresses in her account of environmental justice as a caring for living and nonliving things not as resources to support human wellbeing, but "for their own sakes" (as Molefe puts it).

McIntyre-Mills openly admits that this is her ethical position and that she brings this into the research purpose, which is aimed at urging people to reconsider their life choices in terms of such an ethics. She does not believe that in this way she is trying to "universalize" her own position (and initial cultural heritage)—because she sees that this position can be regarded as springing from a cross-fertilization of a variety of heritages which all stress the importance of "connecting and engaging with people

[11]This is a reference to Stiglitz's (2010) account of wellbeing stocks, but McIntyre-Mills notes that one might need to be wary of this language as it still implies commodification of "goods" (2014a, p. 22).

and our surroundings" (2014b, p. 89). And she does not believe that by infusing this ethical position into the research design she is acting "unscientifically"—the purpose of the research is, after all, to try to foster more social and environmental justice (in line with those who argue for research to consciously include a transformative agenda). (See Chap. 1, Sect. 1.2.1, where I discussed arguments such as those of Chilisa, 2012; Cram & Mertens, 2015; and Mertens, 2007a, b, 2014.) Cannella and Lincoln explain why this purpose is important in order to offset a

> global environment [where] dominant discourses (re)inscribe all human activity as economic (e.g., even religion is tied to some forms of profiteering). The use and acceptance of knowledge is legitimated only as applicable to market perspectives. (2007, p. 318)

McIntyre-Mills' research (or rather her research facilitation) is aimed at contributing to a discourse that urges various "bottom lines" to be taken into consideration in our thinking, feeling and acting—rather than being focused on the economic bottom line (2014b, p. 89). Nevertheless, she also contends that any statements/narratives about possibilities, do need to be rooted in some (research) evidence which is "brought to bear on a topic" (2014b, p. 71). In this regard she argues that "different perceptions need to be explored, in order to cocreate a shared truth" based on different interpretations of what is possible (2014b, p. 135). She concedes that there will always be disagreement about specific forms of action; but this does not exclude the creation of collective narratives that invite new forms of action.

McIntyre-Mills argues that insofar as a postmodernist stance—as advanced by certain authors following Lyotard's (1984) *The postmodern condition*—becomes a philosophical approach which suggests that there is "no truth", she prefers to consider the research process as seeking "harmonized responses which are based on co-creating meaning" (2014b, p. 87). The point of the research undertaking with SALGA was to explore the potential for developing new ways of seeing and new ways of acting in the locality, based on how people participating in the research responded to the software where they could engage with others' statements and could reflect on their own ways of being. While McIntyre-Mills criticizes a relativist-inclined postmodernist approach which denies any truth-seeking, her approach can be said to be consistent with Gergen's (2001) affirmative interpretation of how we might operate in a postmodern context, by accepting that "concepts such as 'truth,' 'rationality', objectivity,' 'individual knowledge,' 'evidence,' and 'scientific progress,'" need to be reviewed, so as to create space for newly imagined possibilities (2001, p. 804). I return in Chap. 7 to a more detailed discussion of Gergen's position concerning how "postmodern thought can bring us into a new and positive space of understanding [and action]" (2001, p. 804). Briefly put, he suggests that when the postmodern context is seen *affirmatively*, provision is explicitly made for "intercultural dialogue, in which concepts of the person and of knowledge itself, along with methods [of science] and practices [of research] are appreciatively exchanged" (2001, p. 810). Such a dialogue is clearly also endorsed by McIntyre-Mills as part of a research process geared to activating hope in alternative futures that can be experienced as less oppressive and as enhancing an inclusive wellbeing.

6.2.3 *Results Interpreted in Terms of a Value-Based Starting Point: Taking Responsibility for the Potential Impact of Statements Made*

In looking at the results of the research (based on the 81 participants and 350 pathways to date, i.e., in July 2012), McIntyre-Mills indicates that the "business as usual" path was not chosen by many participants. For those who did choose it, "typically, informants stress that 'time is money' and life is considered to be stressful and competitive. Adaptation is through increased expenditure and technological innovation, rather than a willingness to give up their standard of living" (2014b, p. 61).

For those who chose the pathway of "small steps toward sustainability"—the majority of the participants—their pathways were characterized by: "making more use of energy-efficient appliances and cars and a 'less is more' approach to living elegantly and ethically" (2014b, p. 39). She also surmises that

> the increasing direct and associated costs associated with the with the use of energy and water will mean that local living will have increased benefits. The savings associated with living locally, engaging in local food production, sharing the resources and developing local recreation and volunteering opportunities will make investment of time in the community more attractive. [Nonetheless,] … for more wide-ranging changes to occur, there will need to be transformations that are driven from below and above. (2014b, pp. 59–60)

She indicates that the third scenario, "sustainable living"

> extends the less is more approach to supporting wellbeing for this generation and the next by being the change. Those without the means to install new technology stressed the need to provide subsidies and for building codes to ensure the quality of accommodation for all. Those who rent and are unable to afford to purchase their own homes are at a disadvantage. Few fit into the ideal of sustainable living. But the overlaps across all three [scenarios with which people might identify] include a willingness to save energy and water—driven by price, fines and user standards that are determined by the public sector. Installing energy-saving devices and expenditure on new technology is the main way in which the groups who favour business as usual adapt to rising temperatures and the rise in energy prices. (2014b, p. 57)

Generally, across all the participant responses, she remarks that

> People want to spend more time exercising, working in gardens, walking, saving money through living simply and slowly, but many who strive for middle and upper middle class lives work long hours and cannot afford to take time out of work. (2014b, p. 8)

McIntyre-Mills presents a table of results which express perceived areas of concern of the participants as a whole, based on the number of responses to various concepts, which the software excel sheets show up according to the categories of "business as usual", "small changes", sustainable future".[12] This offers some

[12]The weightings given to perceptions were obtained by analyzing data from a matrix spanning the following snapshot of factors downloaded from the publicly accessible software in June: For

indication of how many people who identified with a category (namely, one of the three scenarios) chose to rank particular concepts (the concept could have been expressed via the software as a *need* that they have, as a *turning point*, as a *barrier*, or as an *action*). Concepts most highlighted were: social inclusion; less waste and more recycling; energy awareness; financial and job security; greed and waste habits; spare time; sustainable transport; information and education on climate change adaption; food security; poverty; and housing quality. The weightings are reproduced below.

Space does not permit a full discussion of how McIntyre-Mills interprets the numbers as given in Table 6.1 below. But one feature of the table which I discussed with her in January 2016 and which I shall highlight here is that 63 of the participants who prefer a "sustainable future" focused on the need for more social inclusion (either expressed as a need or as a turning point in their pathway). Social inclusion, as stated by McIntyre-Mills implies a need of people to be included in decision-making at in this case local level, and it also refers to

> participation within their communities and what they are prepared to change in terms of their thinking and practice, in order to adapt to the effects [of climate change] and to strive to address the causes through mitigation. (2014b, p. 22)

Also considering those who identified with a sustainable future, 35 of them stated that spare time is problematic for them. McIntyre-Mills sees such results as a sign that people potentially do wish to make changes, and that a "slower" life is a preferred option for many people (this was also identified as problematic by 7 people identifying with the scenario of "small changes").

I asked McIntyre-Mills (January 2016) about whether she thinks the 35 people in the "sustainable future" category can imagine how to institute this radical shift, given the broader social and economic environment where capitalist-based organizations often demand that people spend vast amounts of time at work and under pressure (in order that firms can extract maximum profits). She responded that one of her suggestions (besides trying to institute a slower life within city life) is that options should be set up for more people to turn to agricultural work as a career. Such a lifestyle is "slower" and at the same time would help solve problems of food insecurity. A focus on agriculture and the development of rural communities is of course not incompatible with creating urban spaces which are more sustainable (e.g., through the various suggestions offered above for increased sustainability, including urban farming and community gardens). But she maintains that too many people come to cities partly because opportunities for agricultural careers are not supported well by the education system and partly because of other factors (pers. comm., 14 January 2016).

---

example, these are the findings on 1/07/2012, but the research is ongoing and the data set continues to grow as it is extended (p. 62). McIntyre-Mills commented to me in an email (March 2017) that "the spread sheets are actually just weightings of perceptions on what works, why and how …. we were subverting evidenced-based policy, which is all about evidence based on top down instruments for data collection. We were trying to get people to think and express emotions and ideas".

Table 6.1 Weightings of perceived areas of concern

	Social inclusion needs + turning point	Less waste and more recycling needs + turning point	Energy awareness as a need or turning point	Financial and job security as a basis for change	Greed and waste habits as a barrier	Spare time	Sustainable transport as a need	Information and education on climate change adaptation	Food security	Poverty as barrier	Housing quality
Weightings of perceived areas of concern based on the number of responses per category											
Business as usual	9	2	7	8	1	2	0	3	0	1	1
Small changes	9	6	28	4	3	7	5	11	7	0	1
Sustainable future	63	23	52	17	16	35	20	7	9	8	11
Total	81	31	87	29	20	44	25	21	16	9	13

Source McIntyre-Mills (2014b, p. 62)

McIntyre-Mills made her comment to me also in the context of a discussion we had had the previous day in Pretoria (13 January 2016) with Kofi Quan-Baffour about the relative dearth of educational facilities to support agricultural skills development in South Africa, and also of bursaries for those wishing to pursue a career in agriculture. Quan-Baffour linked the failure to turn to agricultural careers also to the low-status afforded to farming. McIntyre-Mills and Quan-Baffour agreed that the school educational system could already include some basic skills in farming, coupled with encouragement of this as having high value/status attached to it. As she indicated during the course of the conversation, at a policy level one would have to "put the right value on agricultural skills" and agricultural scholarships should be awarded for, example, students who do well at school. There would need to be policy shifts to support those who show an interest in agriculture to develop their skills in farming. (Interview/conversation with Quan-Baffour, 13 January, 2016: https://archive.org/details/KofiPartA20165; and: https://archive.org/details/Kofi24.)

McIntyre-Mills added in the course of the conversation with Quan-Baffour that she considers that this is crucial across the globe because at the moment we are "doing the wrong things right"—that is, we are valuing the wrong things as a society—and putting all energies into these, rather than into the "right things". This was a reference on McIntyre-Mills' part to Ackoff and Pourdehnad's (2001) account of misdirected systems which are directed toward "doing the wrong things right" and also a reference to Flood and Romm's book *Triple loop learning* (1996), where we discuss the implications of Schön's book *The reflective practitioner* (1983). Schön spotlights how single loop learning is focused on improving what people are already doing (in some social context), but does not step out of this to *re-examine what we are doing/valuing*. Double loop learning as elucidated by Schön is defined by the fact that we as learners can *reconsider together what we are doing as a collective*; and triple loop learning (as spelled out by Flood and Romm) emphasizes the *need also to take power relations into account* while bearing in mind the unequal life chances of certain groups of people in society as part of considerations of "what should be done".

McIntyre-Mills pleads for a cultural transformation toward valuing (on a global scale) relationships of caring for one another and for the planet, bearing in mind how the current powerful operation of the state, the market and social processes work against creating/encouraging such relationships. In her book (2014b) she notes that it is not surprising that participants in the research tended to show dissatisfaction with the fast-paced life styles, driven by a consumer-oriented culture, where status is tied to money. She argues that this is not surprising because most of the participants could be said to be middle class (in the middle income groups according to the demographic data) and not in the top 1% (where the Occupy Wall Street protestors noted that wealth is highly concentrated at the top). She adds that:

> But ironically, Monbiot raises the point that even those who are part of the 1% are dissatisfied, because wealth is a "marker or indicator of status". Everyone wants to be higher up on the scale, "money is just a way of keeping score," according to a banker, cited by Monbiot (2013). He also cites 'How much is enough?' The author shows that money is

> never enough unless you are at the very top of the 1%. The cost of profit is borne by those who work for the surplus value of corporations and shareholders. (2014b, p. 125)

McIntyre-Mills remarks that while many participants in the research did express some desire to make some changes to their ways of living, "the challenge is that most participants seem to combine a range of thinking and actions, some of which lead them toward sustainable living and some of which are unsustainable" (2014b, p. 80).

She explains the results by looking at some of what she calls the "non-material" responses to change:

> To date, a preliminary analysis of the factors shows that the following are the most frequently mentioned non-material responses to change: water conservation through introduction of rain tanks, installing solar panels and using air conditioning more responsibly. But the cost of making these changes are a barrier for some. Only those who are in full-time employment or with access to capital are able to make these changes. The more affordable options are using public transport more frequently and walking or cycling. The barriers are lack of time to spend waiting for public transport and concerns about safety—due to inadequate pathways for cycling or walking and inadequate public lighting. (2014b, p. 81)

She indicates that some of the barriers as mentioned by participants, such as the provision of more public transport and lighted cycling paths, can be taken up by local government by lobbying, for instance, for more state support for this. (She commented to me, pers. comm., 15 January 2016, that SALGA was able to use her report (2013) to lobby for funding for these endeavors, to some good effect.)

She goes on to indicate that aside from these options, which were most frequently mentioned by participants,

> other non-material changes are community-based gardening and home-based gardening leading to the sharing of produce. For some, this is about growing seeds and also transforming people's attitudes to one another and the earth. In the process of engaging in community gardening, they learn to share and to grow more connections with the land and one another. The keeping of livestock on a small scale was raised, in Middleville, a suburban area, but bylaws will need reconsideration in Middleville. (2014b, p. 60)

Meanwhile she notes that the community gardening approach became publicized during the research because participants were able to see that this is a resource already available. (This was organized by a private person who offered some of her land for this purpose and there is now a thriving community garden—pers. comm., 15 January 2016.) She interprets that participants' making use of this recognize that growing vegetables together also involves getting to know neighbors and participating in community life. Some of the participants expressly pointed to the value of

> a lifestyle of growing local food and developing greater concern for those beyond the immediate family to include the wider community and global concerns. An appreciation of the importance of kindness to others was emphasized and a few questioned their sense of entitlement to a standard of living that some can only dream about. (2014b, p. 60)

She sums up that

> changes required are social in order to free up time to spend on building the community and fostering links with others, the desire to build a creative, active and renewable lifestyle through growing vegetables, living locally and relying more on local produce. The processing of rubbish, reusing, recycling, growing vegetables, making compost and sharing community harvest are the basis for growing community in Middleville. A future is possible where local, sustainable living is a lifestyle choice will coexist with those who are forced to remain in one area as a result of poverty. (2014b, p. 60)

Here she posits that a different future is possible in the sense that many middle and upper income people recognize the damages that current lifestyles are causing not only to themselves but also to others and the planet. It is possible, she believes, to change neural pathways so that people can learn to better respect our relationships with others and the environment of which we are part (2014b, p. 119). She finds it important that the research showed up—as also expressed by Wilkinson and Pickett (2009)—that increased consumption as a value does not lead to greater health and happiness for those who manage to amass consumption goods. She adds (based on the findings as she interprets them from the research with SALGA) that greater levels of consumption can lead to exhaustion, lack of time, and the depletion of the planet's resources (2014b, pp. 157–158).

Of course, one could argue (critically engaging with McIntyre-Mills' interpretations) that people might have tended to offer what is called in the research literature "socially desirable" answers. (See, for example, Romm, 2010, p. 54.) That is, they might be be aware that it is socially acceptable to distance themselves for the most part from the "business as usual" scenario and to express allegiance to a simpler lifestyle, even though it is unlikely that they themselves will move towards this (even after reflection as inspired by the software package). Would the participants indeed be willing to change their behaviors? This is a question that requires follow-up research (which is an option not outruled by McIntyre-Mills).

For the moment McIntyre-Mills suggests that the findings of the research point to the possibility—if sufficient people are able to appreciate this—that cultural transformation can take place on a wider scale, coupled with new approaches to "the state" and "the market", supported by social movements on a global scale (2014b, p. 10). But she does express concern that the research results seem to indicate that, while the "desire to engage more in the community and to have more spare time is shared by the majority of participants … energy awareness does not however translate into a willingness to change greed and wasteful habits" (2014b, p. 61). This can also be seen from the relatively small amount of people who highlighted the concept of greed in Table 6.1, with none highlighting it in the "business as usual" category, 3 highlighting it in "small changes", and 16 highlighting it in the "sustainable future" category.

She also reminds us that although very few of the participants in the sample were poor or facing poverty, "poverty was considered a barrier by those without financial security or facing poverty" (2014b, p. 83). What she suggests is that the solution to poverty on a world-wide scale does not lie in "more of the same"—based on the dominant argument that growth in GDP also reaches the poor. She indicates that

this effect has not transpired if one looks at the way in which the capitalist market economy has functioned to maldistribute wealth (as also expressed in the Occupy Wall Street protests). Hence a radical shift is called for. She comments that the "wounding of the spirit" in modern times (as expressed by Ramphela, 2012) "is central as people strive to have more material resources, rather than being fully human through their relationships with others and the environment" (2014b, p. 8). She urges audiences of her books to consider how we, taken as a collective, have been wounded by the trajectory of globalization thus far. She conjectures that "the issue is that in a context of increasing risk the number of people who will be living with a sense of woundedness will increase"—unless a radical turn around can be effected (2014b, p. 75).

6.2.4 *Reporting: Taking Responsibility for Engendering Dialogue and Inspiring Re-creation of Caring Narratives*

As indicated earlier, McIntyre-Mills worked directly with SALGA in setting up the project as a pilot project to test and improve the software package. One of her principal lines of reporting was thus to SALGA. In her report she explains the whole research design (as I outlined it in Sect. 6.2.1) and she concentrates in particular on what came out from the research in terms of what she considers as "highlights for policy making"—which might be relevant for SALGA.

But before reporting to SALGA in 2013, as well as presenting the report to a conference held by SALGA thereafter, McIntyre-Mills invited to a workshop in August 2012 a number of participants to discuss the results. Those who attended were some participants who had created pathways using the software, an Aboriginal mentor for the project, and some of the research assistants who had been involved in aiding certain participants to find their way through the software package. Some of the points that were made by participants in this workshop were carried through into the research report (2014b, p. 63). McIntyre-Mills notes too that the workshop included "a diverse group of participants spanning diverse beliefs and cultural experiences in Australia, Indonesia, Africa and Europe". (The participants from places outside of Australia were mainly her Ph.D. and former Ph.D. students who had participated as research assistants in the project.) In other words, McIntyre-Mills took care that her interpretation (and what would be recommended to SALGA for further interpretation and utilization) was not based only on her own considerations, but also included discussions with some participants and research assistants in relation to what could be said to be the import of the research.

It is also worth noting that in terms of her research intention that the research might contribute to "making a difference" in as many ways as possible, she makes the point in the report (and in her books) that "the local prefiguring project will have little potential unless it is scaled up through other organizations in Adelaide and

through other participating local governments" (2014b, p. 63). And she highlights that an Aboriginal mentor for the research observed that:

> in the Southern Region (of Adelaide), partnerships have been created with Aboriginal people to achieve better wellbeing outcomes. The challenge is to match services to complex needs, but to [also] realize that the final determinant of decisions is "how sustainable is this for the land?" (p. 64)

This comment by this mentor was made in response to another participant's remarking that:

> Decisions need to ensure that ecological land value is placed foremost in decisions when undertaking urban planning. Our relationships with the resources that sustain us are critical to the long-term viability of our cities if we are going to learn from the past demise of great civilizations. Cities can only exist in harmony with the land on which they are dependent. (2014b, p. 64)

Taken together, these remarks point to the importance of sustainable urban planning and also to the importance of creating partnerships across local governments, which include partnerships in Australia with Aboriginal communities, who have always recognized that "dependency on the land needs to be recognized as a key determinant of policy decisions" (2014, p. 63).

Yet another of the participants in the workshop (one of her Ph.D. students who was doing research in the Democratic Republic of the Congo—which he argued had become a failed state) "stressed that social justice needs to be supported by the law—we do not have democracies without the rule of law" (p. 65).[13] This was a reference to the need to make laws not only at local but also at state level in order to support (and encourage) people to "do the right thing". In this context McIntyre-Mills made the point about scaling up democratic participation based not only on people expressing their rights, but also on being concerned with their responsibilities (to others and to the planet), so that laws can reflect this ethical approach.

In her reporting to SALGA, McIntyre-Mills included detail from the research "results" by presenting tables from the spreadsheets and by interspersing these with the interpretations offered by some research participants and other interpreters, and by herself. And the report was presented and discussed further at a conference organized thereafter by SALGA, in which local governments across South Australia were invited to attend (pers. comm., 15 January 2016).

This was not the only public venue by which McIntyre-Mills tried to disseminate and discuss the project and its results and engage in discussion around interpretations thereof. For example, the research process and results were presented at a Chinese Academy of Social Sciences (CASS) research conference held in Beijing in 2012, with the theme of sustainable environmental policy and global governance. And they were presented at the International Society for the System Sciences (ISSS)

[13]This student from the Congo had helped as a research assistant to gather some of the data during this research project and was now participating in discussing the project (pers. comm. via email from McIntyre-Mills, March 2017).

conference held in Berlin in 2015, where the theme of the conference was governing the anthropocene. The research was also presented at the 2015 World Futures Centre (WFC) conference in Cyprus, devoted to reconsidering dialogical design processes (http://www.sddp-international.org/SDDP2015Conf/index2.html).

I have focused above on drawing out core aspects of McIntyre-Mills' reporting process to highlight how I see "responsible" reporting as not shying from offering value-based interpretations (rooted in dialogue with participants and others) of the meaning of the research—with the intention to inspire action based on people in various contexts reflecting anew on dominant narratives. This can be said also to be a response to some of the participants who stated that "loss of inspiration" (as in non-inspiring narratives) was an issue that affected them adversely (2014b, p. 58).

6.2.5 *Validation*

In McIntyre-Mills' book (2014b) where she elucidates the research and its purpose, the word "validation" as applied to the research does not feature. In this regard she can be seen as drawing on a range of research paradigms in the social sciences which express wariness with using this concept, because of its traditional connection with seeking "accuracy" or "one picture" of social realities. Clearly, McIntyre-Mills is operating more in line with those qualitative and Indigenous-oriented methodologists who argue that this is not the purpose of social research. Wilson's account of *Research as ceremony* (2008) can be seen as aptly applying to McIntyre-Mills' research facilitation. As Wilson notes,

> validity; statistically significant; worthy or unworthy … lose their meaning. What is more important and meaningful is fulfilling a role and obligations in the research relationship—that is, being accountable to your relations. (Wilson, 2008, p. 77)

What can be said of McIntyre-Mills' stance is that she is trying to fulfil felt obligations to participants (in offering them opportunities to express their own pathways to wellbeing and later inviting them to participate if interested in interpreting the overall results) and she is also trying to render herself accountable to wider audiences such as local governments and any readers who might engage with the "results" (as interpreted). When engaging with audiences, she does not try to offer her deliberations as a finished account of what the research tells us—but rather as an opportunity to build connection with audiences, as part of a process of people creating new global narratives together. This would be in keeping with Wulff's comment (in his review of Wilson's *Research as ceremony*, 2010, p. 1291) that "the emphasis on relating to one's audience leads the Indigenous storyteller or Indigenous researcher to build connection, not as a device to convey packaged knowledge—rather, as a value in its own right".

The status of the statements made by McIntyre-Mills to audiences do not function as packaged knowledge, but as invitations for people to engage with the work to, as she puts it, cocreate truths as shared narratives that become

"increasingly harmonious songlines" (2014b, p. 39). While the SALGA research is set in what was mainly middle and upper middle income area (Middleville), the research is meant to inspire anyone reading the "results" to rethink their connectedness with others and with the planet, including involving compassion in this process. The story that McIntyre-Mills herself offers by way of deliberating on the research results and on interpretations of them is critical of narratives which cater for those striving for material wealth at the expense of the majority of people on the planet and the planet itself. She concurs with Sen's explicit critique of opulence (1997). And she tries to spotlight leverage points where cultural change and attendant structural changes at the local policy-making (and scaled up further) may be worked on. (This commitment is also expressed by her and Binchai, 2014, p. 4.)

Although she does not use the words *validit*y or *validation* in her various write-ups of the research, she does express in her report to SALGA what she believes the project achieved to date, namely:

- Created an interactive data collection tool;
- Collected user-centric data that can be used for evidenced based policy;
- Publication of general findings for use by SALGA [via the report];
- Presentation of de-identified process to various conferences and workshops [in different parts of the globe] (2013, p. 1).

As far as the second bullet point is concerned, I would add that because the "user-centric data" was collected through participants' filling in of the "data collection tool", the results of which are accessible to administrators of the system, there is a paper trail or rather a software trail of all the participants' responses (de-identified) to peruse in "raw" form. And in this sense the "data" are available. However, McIntyre-Mills does not quite stress in her report that the "data" are not meant to represent "facts" *independently of participants' ways of considering how to deal with the software*. (See also my comments regarding people's engagement with questionnaires/forms in Sect. 5.2.4 of Chap. 5.) Perhaps because key staff in SALGA were aware of how the package was set up, they would be aware that the spreadsheets expressing people's responses are to be understood *as a function of how people either online or face to face with research assistants responded to the outlay of the software.* Nonetheless, McIntyre-Mills could have added a paragraph to this effect in her report so that it would less likely lend itself to being interpreted in terms of what Kuntz (2015, p. 33) calls an "extractive logic", where data are considered as extractable and "found", rather than as being *generated* in material and relational contexts.

While this particular software package provided for an interactive relationship between participants and the software (which meant that the software could grow as people added new concepts and made unique comments), this does not mean that the results represent "facts" independently of the way the participants *chose to treat the opportunity to engage interactively* with the software and to express their needs, wants, barriers, etc., *in this context.* That said, and with this caveat, I would suggest that McIntyre-Mills has presented a story in her report and in her books, conference

presentations, etc., that is credibly linked to "the data" that was created as a result of participants' interaction with the software. And audiences can continue to engage with the story and find inspiration in it (in that she tries to offer a story explicating how change—and even radical alteration of dominant business as usual narratives and ways of being—is possible).

I would like to make a further point regarding the nature of the interactive tool and its purpose. McIntyre-Mills states in her report to SALGA (as elsewhere) that she hoped that the interactive tool would prompt people to "think about their thinking" (2013, p. 10). As she puts it:

> The paper [report] discusses the attempt to develop software to enhance the consciousness or awareness of people about the unfolding social, economic and environmental crisis. The research assesses the extent to which data shows that people see sustainable living as either ideal at best, or at worst unattainable, because the situation of climate change as beyond their control. … . Our design attempts to enable people to think through the "if-then" scenarios, not merely to "unfreeze" (to cite Lewin's (1947) concept), but to work toward sustainable praxis, based on an understanding of our interrelatedness. This requires ongoing changes to learning. (2013, p. 10)

Here she alludes to the idea that she hoped to use the research as a *reality-constructing rather than reality-finding mechanism*. (This would be in keeping with both the transformative and Indigenous paradigms as I outlined these in Table 1.2 in Chap. 1.) The idea was to enable people to think through "if-then scenarios" as a way for them to participate in the learning exercise, out of which they will (presumably) not emerge unchanged. But she does not specify how the software can be used by those looking at the results to study *if and to what extent people did find the exercise to be a learning one*. One could perhaps infer from the fact that 350 pathways had been created by July 2012 while there were only 81 participants at that date (2014b, p. 61), that many of the participants did choose to use the software as an opportunity to revisit their choices and their pathways—which means that they were indeed treating the software as a learning exercise to "think about their thinking". But I would propose that coupled with this way of inferring that people were learning, one could *ask them directly* some questions at the end of the form, regarding how they experienced their filling in of it and whether it aided their reflections in any way, and if so, how. This would be another way of substantiating whether the software can be regarded as a tool that aids people to rethink their pathways and it would also highlight for the researchers/research team (and others engaging with the research) what aspects can perhaps be strengthened in the tool to facilitate the process of such critical reflection further. In our conversations in January 2016 I suggested this to McIntyre-Mills and we devised some questions along these lines that she felt could easily be incorporated into the software design.[14]

[14]Bawden argues in this regard that it may be important not only to urge people to "think about their thinking" but *to think about the worldviews that underlie* their thought processes (2005, p. 152). One could perhaps argue that by offering different scenarios which in effect express worldviews, McIntyre-Mills was laying the ground for people to reflect on their worldviews. But

6.2.6 *Generative Theorizing: Toward Transformation*

As I have noted throughout this book, and also with reference to previous examples, many authors have argued that researchers have to be vigilant about how their theorizing, with participants, is likely to impact on the participants as well as wider audiences. The idea is to develop, as Goff puts it (2014, p. 113) a "co-production of theory and practice". One could argue that McIntyre-Mills' initial theorizing in relation to our potential to form "webs to wellbeing" (inspired as she notes from various theoretical and spiritual traditions) in turn formed a starting basis to urge participants in interaction with the software to develop ideas regarding possibilities for thinking about the consequences of living isolated lives, and of trying to secure goods at the expense of other people and the environment (2014b, p. 9). Her intention was to inspire the development of narratives that could express a more relational existence, rather than seeing the research process as (simply) a process of listening to and documenting existing narratives that people may tell and share. As I indicated in Chap. 1, (Sect. 1.3.3), research endeavors can be regarded as one way of *developing alternative normative frameworks*. What is important is that researchers initiating inquiries take into account how their framing of issues (with others), use of language, and engaging with research participants/co-inquirers, might make a difference to the discourses that become "currency" in the social arena.

McIntyre-Mills can be said to have been conscious that her way of injecting discourses of essential connectedness (also based on some focus group discussion with sets of participants in which she prompted them to consider a radically sustainable lifestyle as a "third" scenario) was not a neutral stance. Through the research, she offers her contribution to the construction of an ongoing generative theorizing, which is also developed/refined/reinterpreted in terms of the "data" (as interpreted by her and others) that emerged from the way the participants interacted with the software package. In abductive or retroductive fashion (see also Chap. 4, Sect. 4.2.6) she abduces that the "data" are not surprising if seen from a *perspective that human wellbeing can be regarded as a function of involvement in communities and reverence for nature*—and that human neural pathways can be created to enhance this form of wellbeing.[15] This is consistent with many other theorists who too place importance on community and communion with nature as a potential route to human wellbeing (as derived from practical experience and from theorizing

---

Bawden's suggestion is that thinking about thinking (or even about consequences of life choices) does not necessarily prompt people to reconsider their initial worldviews, By McIntyre-Mills asking a direct question about this to participants (as I suggested), this in itself could serve as a (further) stimulus to make them more aware of the need to reflect upon their worldviews. As Bawden puts it, "I believe that worldviews themselves need to be consciously developed" (2005, p. 152).

[15]Regarding our capacities to "make and remake neural connections in the brain," McIntyre-Mills (2014b, p. 10) refers to research work on the plasticity of the brain, as undertaken by, for example, Greenfield (2000).

of their own and others' experiences). Examples of such theorists—some of whom are mentioned by McIntyre-Mills in her various books—are: Binkley (2000), Harris and Wasilewski (2004), McCrea, Walton, and Leonard (2014), Murove (2005), Shiva (2002), Wilkinson and Pickett (2009)

McIntyre-Mills engages with these theoretical ideas, while offering her way of connecting the literature with the import of the pilot research (with mainly middle and upper income groups who were deliberately chosen in view of their heavy carbon footprints[16]). As she stated to me (pers. comm. via email, March 2017), "the point of the research was to explore whether this approach to public engagement could make a difference in enabling the heavy consumers to change by reflecting on their lives". She had been inspired by "earlier work on learning from Indigenous Australians who stress why treading lightly is important". The (largely) middle class area was now selected for this study "because the intention of the design was to look at excessive consumers with a view to narrowing the gap between those with heavy footprints and those with light footprints".

In her book (2014b), she notes that one "finding" that can be considered as having arisen from the research work in this project (which also lends substance to deliberations around wellbeing), is that:

> Poverty is not having time and social relationships. It is not knowing your neighbor and distrusting the quality of the food that we eat. It is competing for a step up the run of status, instead of trying to live lives that support a sense of wellbeing. (2014b, p. 124)

McIntyre-Mills' focus on a "sense of wellbeing" tallies with McCrea, Walton and Leonard's research which suggests that "relationships between an individual's objective circumstances, and their subjective satisfaction [sense of wellbeing] are weak" (2014, p. 272). They suggest furthermore that it is possible to develop ways of exploring community wellbeing in terms of quality of life at a community level, such as, for instance, by looking at "community cohesion" (2014, p. 272). McIntyre-Mills also notes that many authors have underscored the need to think more communally about our collective future and its possible contours; and when looking at authors who focus on "the common good" we do not need to attach the label of "communism" to their work, including their theorizing and their offering of solutions (2014b, p. 104).

As far as "popular" thinking in everyday life is concerned, she hopes that people in their everyday living can better develop

> critical systemic thinking skills so that one can read through newspapers and are able to locate the different arguments in commentary and editorials that are pro the zero-sum or containerist approaches, pro-market and unaware of or denying the interconnections or the impact on wellbeing. (2014b, p. 82)

[16]Their heavy consumption consists in their "driving big 4 wheel drives, running air-conditioning day and night and buying food out of season, pre-packaged, etc." (pers. comm. via email, March 2017).

She considers that the agenda of (transformative-oriented) researchers when generating theorizing processes with participants and when sharing reflections with audiences, is to try to enhance such systemic thinking skills. She suggests (2014b, p. 174) that in this process people would do well to recognize, as expressed by Brown (2012) that:

> The political debate of the 20th Century was polarized between capitalism and communism. It was about control of the economy in the narrow sense of material goods and money. [It was about] a free market versus state control … . In this 21st century the political debate is moving to a new arena. It is about whether we expend earth's natural capital as our population grows to 10 billion people in the decades ahead with average consumption also growing. (Brown, 2012, as cited in McIntyre-Mills, 2014b, p. 195)

Regarding the suggestions of some authors that families need to be controlled so that fewer children will be born, McIntyre-Mills contends on the contrary that

> empowering women [to reconsider their choices regarding numbers of children] requires gender mainstreaming through working with both men and women so that men understand the advantages to their households. Literate, numerate women can produce beyond the household and this provides a way out of poverty. (2014b, p. 174)

She continues that based on her reading of the literature coupled with her experiences of listening to women that,

> women and men who have other means to survive are more open to understanding that more children does not mean prosperity because eventually there will not be enough resources to maintain their health. This is quite different from the autocratic controlling of population that would be a negative sanction on the poor. Instead of negative sanctions, better quality of life needs to be achieved through development and education. This approach has better results in achieving a demographic transition. (2014b, p. 174)

She avers that "population control" can be regarded as autocratic and that it unduly sanctions the poor (who may tend to have more children in the belief that this provides a safety net for their old-age). On these grounds (where theorizing is linked to her ethical concerns) she proposes a different route to a "demographic transition", namely through empowering women. She also believes that would-be women farmers across the globe in particular need to be empowered to participate more fully in economic activity, and that this will go some way to solving global looming issues of food insecurity. This is consistent with the (theoretical and practical) approach taken by Woldegies (2014) as detailed in Chap. 4.

It is worth noting that although McIntyre-Mills is averse to applying legally binding controls toward "population control" as she believes that this unduly sanctions the poor, she is not averse to the application of controls at state level to try to ensure that corporations operate in terms of more socially and environmentally just laws. She believes that the dualism between "profit" and "economic development" is outdated and that laws need to be constructed to reflect this. States need to take into account that current models of economic growth are socially untenable (as growth is not accompanied by an increase in quality of life for the vast majority of

people) and environmentally unsustainable. On this, and other bases, she argues that laws at state level, infused with "ethical processes to support wellbeing at post-national level" (and in line with international treaties) need to be developed and enforced (2014b, p. 45). She explains (McIntyre-Mills & Binchai, 2014) that while the research with local residents in Australia points to possibilities for people changing their lifestyles away from "business as usual", this does not mean that "the burden of responsibility … can be shifted to locals without the private sector, welfare sectors and all levels of government playing an equal role" (McIntyre-Mills & Binchai, 2014, p. 3). She suggests that rational decisions as embodied in laws "need to also be guided by compassion for the [relatively] powerless" (2014b, p. 100).

As far as what she as a research facilitator can do so that collective responsibility for furthering social and ecological justice can be stimulated, she indicated to me (pers. comm., February 2014) that her strategy is to find locations in the world where it looks as if there is space for transformative-directed action, based also on communicating with her many present and past Ph.D. students from various parts of the globe and with other networks of actors—some of whom occupy strategic positions in governments or in non-governmental organizations. Upon seeing my account of her entire approach in this draft to date (27 August 2016) she made the comment that:

> I would explain that the on line engagement [as handled in the SALGA project] was one of many ways of engaging with people in the local government project and not the only or necessarily best way! I am preparing for the research in three placed in Indonesia now in urban Jakarta and regional Jatinagor and then a food growing area … possibly Cimis. The software will be ineffective if it is the sole means to gather data or engage. People who are poor or destitute will need to be interviewed or to take part in group conversations about what it means to live virtuously and well. Also when trying to understand what matters to people it is important to give them respectful attention during human interactions face to face. This is the only way that we can even begin the on conversations about spirituality. The quality of engagement and the resonance between people provides the context for discussions on spirituality … also the sense of place. … . Hence the importance of places of spiritual significance … natural and constructed.

McIntyre-Mills advises that we must suit "method" to context and that in certain contexts one-to-one and group conversations may be more appropriate than engaging people through a software program. She also points out that such a program may not be appropriate for opening up and deepening discussions on "spirituality". This example therefore must not be regarded as "transferrable" without an appreciation of context and of the purpose of the research.

6.3 A Case Study of Occupy the Farm (OTF) at the University of California at Berkeley

In this section I discuss Roman-Alcalá's case study write-up of a (fruitful) endeavor to Occupy The Farm (OTF) at the University of California on Earth Day in 2012. The text to which I primarily refer here is Roman-Alcalá's article in a special issue of the journal *Globalization*, entitled: *Food Sovereignty: Concept, Practice and Social Movements* (2015). The case of OTF as presented by him covers events in which he was a participant in the action, while he also interviewed and conversed informally (in 2012 and 2013) with the central organizers of the action and other participants (2015, p. 557). My discussion of this is somewhat brief (in relation to the first example): it is geared to illuminating via this example how case study research can become transformative insofar as it develops and strengthens new narratives—new in relation to "business as usual"—and new possibilities for action.

6.3.1 Purpose of the Write-up (As Expressed by Roman-Alcalá)

Roman-Alcalá states that one of the purposes of his write-up of the case is to illustrate that "land occupations in the North are potentially useful—but uncertain, and very context-dependent—tactics to promote land and food sovereignty" (2015, p. 545). He is thus not attempting to outline a set of tactics for others to follow, but to encourage people reading the text to reflect on their options toward creating more social and environmental justice[17], inspired by an understanding via this case of what it might mean to try to operate outside of state and market imperatives.

He indicates that the Food Security [FS] framework as developed and explored via this case study in "the North" (of the globe) relates to the FS project more generally: broadly stated, the FS political movement was founded on ideas connected to communally developing and managing "the commons", and it originated in grassroots agrarian movements in the global South (2015, p. 549). The write-up of OTF is meant to offer thoughts about the application of this (broad) FS framework in this case, with the hopes of improving the long-term vision of the potential for FS action across the globe. He explains the context as follows:

> Food sovereignty movements have proposed redistributive land reforms to shift existing land concentration and the monocultural industrial agricultures associated with it toward the food sovereignty ideal of small-scale, peasant, family, and collective production of high-quality products intended mainly for community consumption and local/domestic markets (Rosset, 2006). … . This vision is in turn supported by notions that community

[17]Roman-Alcalá indicated to me via email (3 July 2016) in responding to my draft Sect. 6.3.1 of this chapter that this is "exactly what I hope".

> management of resources is more environmentally sound [than] … industrial capitalist agriculture. (Roman-Alcalá, 2015, p. 546)

He cites various examples of activism in the FS movement. For instance, he cites the case of Brazil's Landless Worker's Movement (MST), which he sees as "perhaps the most emblematic instance of 'from below' land reform" (2015, p. 547):

> In the absence of meaningful redistribution of land "from above" [by the state], the MST occupied millions of hectares of land, converting the land into farm communities under the auspices of a clause in the Brazilian constitution demanding 'social use' of underutilized or neglected lands (Wright & Wolford, 2003). The … MST were able to successfully settle so much land, and gain legal titles to that land. (2015, p. 547)

He emphasizes that the FS movement is closely tied to land sovereignty (and contestations around right to the land, including contestation of capitalist-oriented notions of private property) and he adds that the worldviews that can become invoked in this movement are often linked to the cosmologies of Indigenous people:

> Land sovereignty addresses redistribution, for economic (but also gender) justice; recognition and protection of the land rights and cosmologies of Indigenous peoples; and attention to the intra-community inequalities around land that exist (Rosset, 2013). Land sovereignty's "access" orientation focuses on what works with regard to democratizing land resources. (2015, p. 547)[18]

He points out that although FS as a political movement originated in rural contexts of food production, it has "increasingly [been] adopted by consumer and urban populations"—including in the global North (2015, p. 546). But he also remarks that the FS movement currently has a weak status here. He elucidates his intentions in the writing of the article:

> The article attempts to … illuminate the challenges to and conditions surrounding land occupations as successful tactics for land and food sovereignty in the North. The case concerns a 10-acre parcel of land in Albany, California known as the Gill Tract (GT), which contains some of the last remaining Class-1 agricultural soils left undeveloped in the Bay Area of California. In 2012, the GT was occupied by Occupy the Farm (OTF), a direct action group that promotes "a future of food sovereignty" and the idea of community management of land. (2015, p. 546)

In the midst of the prevalence in the North of notions of individual property rights and the notion (which McIntyre-Mills too queries, 2014b, p. 122) that attempting to manage land through a collaborative effort is unlikely to work in practice, these OTF activists tried to give expression to an alternative in this context, as reported upon, for instance, in this case write-up—2015. (This was one amongst other avenues of reporting on the case—see Sect. 6.3.5 below.)

[18]This is consistent with Fenelon and Hall's point that "in recent decades, there have been indigenous movements that challenge globalizing capitalism. … . These movements and organizations represent indigenous peoples on social-group levels and collectively, with great variation in their approaches toward issues, resistance, and political participation" (2008, pp. 1870–1871).

6.3.2 *Research "Method": Interspersing Interviews and Conversations with Further Theorizing in the Construction of the Case Write-up*

Roman-Alcalá's article—written as an account by himself as an insider in the action—is built up of his deliberating around the context of operation of the OTF case, interspersed with citations from those whom he interviewed or held conversations with (2015, p. 557). For example, he notes that according to one OTF organizer whom he interviewed:

> Developed capitalist economies have almost completely divided us from land. … . Food sovereignty is coming out from people with preexisting relationships to land, not people creating that connection. This creates an interesting dynamic in our society, because how can we implement food sovereignty if people don't even understand the issue? … We had a lot of people at the occupation saying they had never done something like that before. That doesn't happen in other movements I imagine, and that's the difference between us and agrarian societies. Their tactic is the cultivation of a political process, community-based and revolutionary … changing the relationships of power and space through the control of land. One of our tactics and goals is to just reestablish relationships to land. (OTF organizer, as cited by Roman-Alcalá, 2015, p. 548)

He expands with his own thoughts on this statement by observing that it is set in a context in which:

> In general, US legal and cultural structures privilege private property rights to land, and systems of property enforcement are legitimized and functional. Private property is equated with individual and family liberty, and land property in particular has long been linked to the ideals enshrined in the US Constitution and the 'Declaration of Independence'. (2015, p. 548)

Nonetheless, he points out that alongside the strong discourse of the value of private property as endorsed in US legal and cultural structures, a food movement has developed in the USA, where alternative actions and alternative narratives have become created. He does not see that this is a homogenous movement, but he argues that:

> The US "food movement" is an amalgam of environmental NGOs, the small organic and family farm sector, voluntary initiatives like community gardens, consumer groups, and (social) entrepreneurial projects (see Allen, FitzSimmons, Goodman, & Warner, 2003, p. 64). (2015, p. 549)

His view is that these initiatives thus far have not been effective in creating shifts at the level of federal policy. This is due, he suggests, to their being "composed of diverse sectors that do not necessarily share the same goals, strategies, or campaign (p. 549). However, he argues that this is not to say that there have been no shifts in either discourses or material gains. He cites the OTF as an example:

> That said, there has been an increase in discussion on land reform recently. In April 2014, the NGO "Agrarian Trust" convened a symposium at University of California at Berkeley (UCB) called "Our Land", where the nation's "dilemma of farmland succession" was

> assessed through "historical, ecological and political economy perspectives" by a multitude of food movement actors. (2015, p. 549)

He posits that the process of "rethinking land relations" as conceived by OTF organizers and participants

> stems from a nationwide revival of interest in agriculture exhibited by young and urban agrarians … but also a general trend toward cynicism of capitalism and its norms of private property. This latter trend has been indicated (and perhaps deepened) by the 2011 countrywide Occupy Wall Street (OWS) movement, which opposed capitalism's effects on social equality and democratic functionality. (2015, p. 549)

In this way he links the OTF discourses to discourses which became invoked in the Occupy Wall Street (OWS) initiatives.

As far as the "success" of the OTF initiative goes, he indicates that in this article he does not wish to track only what he calls "material gains" (2015, p. 548). He suggests that "these are, of course, important, but not the only gains worth tracking". He explains his view of the link between the "real world"—which he places in quotation marks to indicate that there may not be a univocal interpretation hereof—and what he calls "discursive struggles", as follows:

> … land sovereignty is framed by the constant interaction of material and discursive conditions. Discursive changes may (though do not necessarily) lead to material changes, while material changes—by altering the material grounds on which discursive battles are fought—may open up opportunities for further material advances. (2015, p. 548)

He thus indicates that what he considers important to highlight in this case is the way in which "narrative resources" were created as part and parcel of the action. He cites one of the OTF participants on this score:

> The story of how there is a higher use of the [GT], that it is a common resource that should be governed together, that development was not ideal, and that we were not afraid of hard work, were key elements of broadcasting a new vision. (OTF participant, as cited in Roman-Alcalá, 2015, p. 552)

He elucidates that while occupying the GT, OTF was also striving to "create narrative resources for questioning land use, at the GT specifically and more generally in relation to 'misused' private and public properties". Part of the OTF strategy was therefore, inter alia, to arrange:

> Workshops and open meetings … on various topics relating to the land's history, UCB's collaboration with biotechnology corporations, and food sovereignty alternatives. Banners, interviews with media, slogans, and ceremonies at public events all contributed symbolic resources against UCB's development plans, and for a "community farm" alternative. When journalists asked how people could support OTF's efforts, organizers sometimes responded: "Take more land: there is a destructive development project planned on arable land somewhere near you," showing how OTF's actions sought material access to the GT, but also (in the words of one later participant) "focused on restructuring relations to property" in a more general sense. (2015, p. 552)

Needless to say, he notes that while OTF worked toward generating and explicating their narratives "of what the occupation was about and meant", many of the faculty

at the university were promoting opposing discourses—in which participants of OTF were "stigmatized, and demonized" (2015, p. 548). He remarks, though, that despite this stigmatization:

> Local television coverage was uncharacteristically positive (for coverage of illegal political actions) and supportive of OTF, stacking interviews with participants (2015, p. 552) … . Coming off of the heels of OWS, media interest in the action was strong, providing OTF the opportunity to link that movement to the world of food sovereignty. (2015, p. 552)

He also indicates that while many of the faculty at the University were clearly not in favor of OTF, the OTF organizers managed to seek out "sympathetic faculty", which brought them (anew) into a process of negotiation with UCB (University of California in Berkeley). Arising from this, in July 2013 the College of Natural Resources (CNR) offered

> a small piece of the north side of the GT ("Area A") for a "community-based" project. This project was spearheaded by professor Altieri and in it, community groups (including members of OTF) planted equal area plots and gathered data on output in order to assess the efficacy of various agroecological approaches. (2015, p. 553)

Attempts were made by OTF organizers to enlarge their area, and OTF tried further in this regard to "raise awareness, organize community opposition, and petition UCB authorities". But he notes that

> the struggle to produce "community" management of land comes up against the reality that neither Albany residents [in the area] nor Gill Tract Community Farm [GTCF] participants hold unified positions on the GT's development. (2015, p. 553)

He concludes on a note of optimism:

> Even considering these contradictions within "community" and the challenges of US [United States] conditions, the OTF action was successful in challenging cultural norms about property and achieving access, partly due to the occupation having foregrounded multiple appealing narratives that invited participation and wider support. These narratives included agroecology versus biotechnologies; community/public access versus privatization; participatory versus bureaucratic governance structure; and green space/food production versus urban development. Even considering observers' potential pro-private property biases and concerns for the action's illegal nature, these narratives helped legitimize the occupation and turn observers into supporters. Part of OTF's effectiveness was due to its use of context-appropriate frames to contest the GT; relating the existing conditions of the land discursively to material and immaterial ends regarding its social use. (2015, p. 554)

Roman-Alcalá wishes to clarify that he does not consider his write-up of the case as pointing in any way to a recipe for action, but rather as pointing to possibilities for action, and possibilities for hope that more social and ecological justice can be fruitfully striven for in other contexts too. He cites an OTF organizer explaining this point:

> The [strategy] is a creative and fertile space that I don't think we should presume to be able to trim neatly into a road map for change. There could be other avenues for the change energy a direct action cultivates. It may not be "revolution" but it may also be something other than "policy". … . [It is crucial] to hold first that undefined space, where new and

> creative social innovation that lies outside the spheres of state or market solutions can happen. (2015, p. 554)

Regarding processes of operating outside of "the state", Roman-Alcalá suggests that while some proponents of FS have claimed that one cannot work with "the state", others have suggested a contextually sensitive approach, where one can try "leveraging state-directed 'right to land' frames but also deploy communitarian, prefigurative ideals of direct action to achieve the same" (2015, p. 555). He refers to Guttal (2013) in this regard. Interestingly, other authors writing in the same special issue of *Globalization*, on FS (2015) in which Roman-Alcalá's article is placed, share this sentiment. For instance, Shattuck, Schiavoni and Van Gelder suggest that

> to advance the theory and practice of food sovereignty, new frameworks and analytical methods are needed to move beyond binaries—between urban and rural, gender equality and the family farm, trade and localism, and autonomy and engagement with the state. (2015, p. 421)

Also regarding engagement with the state, Schiavoni (in the same special issue) clarifies that we can conceive multiple areas of FS action "that cut across jurisdictions and scales" (2015, p. 466). In a global context where food sovereignty as a movement and a set of ideas "is increasingly adopted into state policy at various levels, [this calls for] state and societal actors to redefine their terms of engagement" (2015, p. 466).

I now turn briefly to some considerations of how Roman-Alcalá's case study (as presented) might be regarded as an example of action-directed research practice infused with the intention of stimulating activities toward increased social and environmental justice. I do this by extrapolating his ethical position, understanding of validation, and view of generative theorizing (as I read his case write-up).

6.3.3 *Ethical Position: Research-in-Action Toward "Just" Outcomes (Defined Locally, with Global Import)*

As an activist involved in the OTF case, Roman-Alcalá does not present his write-up of the case as an endeavor to offer a value-free account of "what happened". His ethical position influences the way in which he retrospectively engages with, while expanding upon, the statements of others which he presents—all the time infused with a sense of what might be regarded as more "just" outcomes on a global scale. He clarified to me that his "intention with these projects is essentially to 'think with' my informants/interviewees/movement partners, to better understand what we are doing, why we are doing it, and what can improve our action" (pers. comm. via email, 3 July 2016).

He presents his reflections/interpretations as offering a needed counterpoint to the discourses which buttress economist and single bottom line profit-oriented agribusiness, with all its damaging effects as he, and others in the FS movement, see them. By deliberating on the broader FS movement in which OTF was enmeshed,

he points to how OTF can be conceived as an effort to improve "material" and "discursive" outcomes toward creating social and ecological transformation. It is worth noting also that he does not believe that strategists (actors choosing ways of acting) need to choose which tactics to follow as "the best" ones, even in a given context, in order to further "just" outcomes. He argues that ultimately it is action in a range of contexts of action by networks of actors, which creates changes toward a better quality of life locally and globally (2015, p. 550). He added to me (pers. comm. via email, 3 July 2016) that "additionally, … an iterative approach will work best: instead of finding a tactic or belief and sticking to it, it behooves movement participants to question themselves and recursively rethink these tactics as they move forward".

6.3.4 *Validation: Strengthening Possibilities for Creative Thought and Action*

Roman-Alcalá considers his account of the OTF case—in which he was involved in acting and in reflecting (with others) on the meaning of OTF—as being a case of insider participatory action research (2013, 2015). His write-up is a retrospective account, with further deliberations, on the import of the OTF case, that is, of its import on both a "material" level (in the land-management regime) and a "discursive" level (in that narratives became generated, sustained, and shared in the process). He presents his storying, rooted in an engagement with quotations from organizers and participants, and coupled with his account of his way of experiencing the case, as being an effort to activate more democratic decision making over land issues and more collaborative use of land. And he leaves it to readers to consider the credibility of the account and to take from it something that hopefully will stimulate future possibilities in movements toward increased democracy in addressing FS issues. The validation of the research—although not named as such by him—rests partly on how his evocative write-up touches readers beyond an intellectual level and evokes possibilities for creative thought (development of narratives) and action (on any level) in this FS arena.

One place in which he does use the word "validation"—albeit in a footnote—is when he speaks about the validation of the multiple narratives that inspired the OTF actors, and that gained substantial community support of those in the community participating in the action or observing it, as well as in the media. He notes that while some comments on a blog called "Albany Patch" suggested that some residents considered OTF activists "as outsiders interloping into the local issue of the GT", this to him does not indicate a widespread lack of support from the community. He points to the way in which OTF's narratives indeed became widely validated "by news reportage, vocal resident support at public meetings, presence on the farm, and in organizing against the development" (2015, p. 557). This to him means that the occupation was "able to build or sustain local community support"

(p. 557); and in this sense the OTF initiative and the activists' way of acting and presenting their actions, could be regarded as validated. The validation of the case is thus at least partly a matter of the activists managing to gain community support and *managing to render more "in currency" narratives that run counter to dominant narratives which buttress forms of injustice* (as experienced on the ground). Roman-Alcalá points out that in

> assessing the fruits of an action toward environmental or social justice, expecting complete acceptance or validation by any one community would be unrealistic, so it is the relative acceptance, buy in, and repetition of OTF's narratives (across multiple communities, geographic and non-geographic) that indicate its "validation". (pers. comm. via email, 3 July 2016)

Roman-Alcalá himself takes some responsibility for the way in which his retrospective account might be heard by audiences—and in this regard he takes care to highlight and at the same time point to possibilities for developing what can be experienced as transformative discourses and transformative actions. He also takes care not to close the discussion about the import of the case for strategizing in either the North or other sites on the global map; but he uses the case as an opening for people to speak further around the issue of equitable and fair land use toward FS in a variety of social contexts. He indicated to me that to be "true to the role of the scholar, and to support FS more effectively", he tried to be circumspect in his account—for example, "not taking participant words or OTF narratives as simple and unchallenged 'truths'" (pers. comm. via email, 3 July 2016).

6.3.5 Generative Theorizing: Taking into Account Possible Ways of "Moving" Audiences

Roman-Alcalá presents theoretical deliberations around the possible import of the OTF case on both "material" and "discursive" levels, based on his involvement in the action. Through this action, he explicates that "what is possible" *became expanded beyond what might have been expected in the USA, with its history of strong narratives of private property and sentiments against communal management of land*. He offers a retrospective account, rooted in deliberations around the broader FS movement, of how the actors succeeded ultimately to occupy the GT and to manage it via a communal management scheme that was set up (with the university). His theorizing/reflections are forward looking of yet new possibilities (for other actors), while he also cautions that actions need to be context dependent rather than recipe following.

Again (as with other cases/examples which I have discussed in this book) one could argue that Roman-Alcalá is alert to the impact that his way of analyzing/presenting the case might have on audiences, and he takes this into account as part of his reflections. It is worth noting at this point that Roman-Alcalá tries to engage a range of audiences—and that this journal write-up is but one of such avenues.

Specifically, Roman-Alcalá sees as important "the trend of many younger people entering into academic contexts as a step toward being change agents in food systems" (pers. comm. via email, 3 July 2016). He feels that by writing this piece and having it "validated" through the peer-reviewed journal system, OTF and FS movement in the Global North have gained additional visibility and traction:

> Professors of food and democracy-themed courses now have an additional resource to assign which can help students contextualize their own experience and theorize and actualize their own activist goals and dreams. (pers. comm. via email, 3 July 2016)

Another avenue was a documentary (directed by Todd Darling) which was made on the case (http://www.imdb.com/title/tt3472754/)[19]. The idea of the participants in OTF was to use a variety of avenues to touch and move different sets of people who could possibly be (positively) impacted upon by hearing about, viewing visually, or reading the accounts of the case (and interpreting their import for their own situations). Roman-Alcalá indicates that this was part of the strategy of allowing the occupation to receive "ongoing attention" (2015, p. 557). Related films and articles were also created (cf. http://civileats.com/author/aromanalcala/#sthash.rePPvSg6.dpuf). Notably, neither the genres of journal writing nor documentary making as supported by Roman-Alcalá are meant to offer emotion-free accounts of cases described—they are meant to be inspirational of new possibilities of thinking and acting.

When I asked Roman-Alcalá (after I had prepared this piece for the book) to make comments on my draft account of his involvement in the case and also about what had taken place since the OTF initiative, he responded (besides making some extensions in the text) that he would like to offer some additional analyses. He proceeded to indeed create these for the purposes of this book. In the boxed section below I present his continued account.

OTF: Updates and Analysis (sent to me by email, 3 July 2016)

Staying engaged with Occupy the Farm (OTF) as an allied researcher has been an ongoing challenge. While I have not been as involved in OTF's actions as before (due to life circumstances and political preferences), I have followed developments, and continued to write in an effort to be constructively critical. What follows are a few observations about the continued story itself, as well as my participation as an activist-researcher.

First, since my original 2013/2015 articles were published on the case, 1.5 acres of the Gill Tract were put aside by the university for a

[19]When I asked Roman-Alcalá by email what his role in the organization of the film was, he replied (3 July 2016) that "I was interviewed for the film (but actually didn't end up being in it very much!) I also worked more with the filmmakers on the film Edible City (ediblecity.net) which I appear in much more. So … long story short: I was not central to that movie being made on OTF but certainly I support(ed) it".

"community-based" farm project. This "Gill Tract Community Farm" is co-managed by university officials and community members, including OTF organizers. The Tract's south side has continuously remained under development threat, and the university has (as of June 2016) begun construction on some parts of the Tract in order to develop for-profit housing and retail facilities. In the process they've cut down 100-plus year old trees, and paved over some of the land. This is a sad development for OTF and many allies.

My analysis is that the "success" of achieving the establishment of the Gill Tract Community Farm on the north side has actually ended up *demobilizing* those people who might have fought the south side development more forcefully. In fact, at an Earth Day celebration in 2016 taking place on the north side farm, a small handful of OTF organizers tried unsuccessfully to convince attendees (many sympathetic to the original 2012 occupation) to re-occupy and encamp the south side, in order to prevent continued development. Rather than risk arrest and be part of a difficult, uncertain campaign of occupation, many chose to participate instead only in the less dangerous and certain activities of the Gill Tract Community Farm.

Writing, Accountability, and Positionality

In addition, over the past two years, I have been attempting to write another article on the subject of race, gender, and class issues within the OTF organization and process. This has long seemed a subject that is implicitly important to OTF, but hasn't been overtly addressed publically. My sense, as a participant, was that racial, class, and gender-based structures, conflicts, and differences all have played a part in limiting the success of the OTF efforts. Because this piece is more critical of those who have been involved in OTF, it has been far more difficult to write and to finish (also, because the story continues to evolve and more data becomes available which is relevant to the issues covered within!). I have also wanted to remain accountable, ever since I conducted the first interviews with OTF organizers for my papers—I am writing *for* and *with* OTF, not just *about* them, and so I do not wish to write anything that is unproductively disparaging, or to write without having a dialogue with those I write about.

In the end, the point of writing such pieces is to help us all (as food sovereignty activists in the Global North) become more strategic, better organized, and more effective. As such, I have not finished the piece because I first need to share the working draft with OTF organizers and have a dialogue about how the piece can most effectively serve these goals. The point is certainly NOT to "call out" certain organizers, or to proclaim myself more pure and righteous in terms of these issues of race, class, or gender.

Roman-Alcalá indicated further (by email, 3 July 2016) that one of the issues that he thought should be foregrounded in further analysis, is that, as he put it,

> two of the main organizers of OTF are white, and I wished to argue that their particular leadership styles were inflected by white privilege, and thus did not do as good a job of leaving space for organizers of color to assert leadership.

When I asked him (via email) whether he regarded himself as a "person of color", that is, how, if at all, he would characterize himself (I suggested he could use a range of characterizations if he thought this appropriate), he indicated in reply (16 July 2016) that a piece on this can be found at: http://www.plantingjustice.org/resources/food-justice-research/concerning-the-unbearable-whiteness-of-urban-farming/ (which explains his anti-racist activism). He mentioned that:

> I am Latino/Chicano (i.e. a person of color) but am also light skinned, and have culturally "white" attributes, such as coming from a college educated background (on one side, my dad's/Mexican side, funny enough). So I don't feel perfectly at home in any category. I am taken as white by some, and Latino by others. I have white privilege, for sure, so I must accept and deal with that in my role as an activist who wants to challenge white supremacy. … .
>
> I also think that this focus on ethnic categories is unhelpful. My point in bringing up the two core organizers' ethnicity (as white), is to point out how their being white contributed —culturally, likely unconsciously—to a certain style of leadership, which may have compromised OTF's effectiveness in some ways. I don't mean to dismiss them as people, or say these problems are irredeemable—after all, they are both trying constantly to improve their organizing—I am just pointing to an unfortunate pattern in many food movement efforts in the USA, which is a structural issue of how white supremacy pervades organizing efforts and sometimes precludes more effective inclusion and broader solidarity. Just as racism isn't just one person's prejudice, but is socially created, so are the issues I'm talking about.[20] It's not about these two organizers being white, it's about their socially-constructed "whiteness" (or maleness in one of their cases) and how that affects the process of organizing. (16 July 2016)

He indicated (3 July 2016) that overall:

> I continue as a sympathetic but critical researcher, involved in and committed to struggles for change, but not interested in writing which is either uncritically fawning and promotional, or self-righteously disparaging or know-it-all. The delicate balance within, I think, is an important one, and similar to Antonio Gramsci's formulation of maintaining "pessimism of the intellect, [but] optimism of the will".

Roman-Alcalá's further analyses as offered above, point to the entanglement of various issues of concern and his recognition that any "research" as offered at a point in time (such as his account as offered in his 2015 article) leaves out issues that can be taken up at a later point in time (such as issues of anti-racism). Issues of social and ecological concern that are identified at a point in time (and co-explored

[20]This position if consistent with my discussion of the construction of race and raced relations which I detailed in Chap. 2, with reference to my personal story in the context of South Africa—see Chap. 2, Sect. 2.2.

with others) are connected, which means that as persons we can continue to explore their connections in further research work with others, as Roman-Alcalá indicates.

In the next section I proceed to explore possibilities for acting and writing in terms of what, as Roman-Alcalá, following Gramsci, notes, can be called an "optimism of the will". I do this with reference to Maathai's book called *Unbowed* (2006), which I treat as an autoethnographic account written in a storytelling genre of learning and critical reflection on her journey of involvement in setting up the Green Belt Movement (GBM).

6.4 Unbowed: A Memoir by Maathai (Kenya)

6.4.1 Introduction: Some Reflections on Autoethnography

I have chosen in this final section of the chapter to refer to Maathai's memoir recounting, inter alia, the setting up of the GBM in Kenya. In this section I am highlighting her autoethnographic writing as a genre of reflective research practice, where she illustrates how she operated with a critical as well as learning spirit in her journey with others in setting up the GBM.

Chilisa and Ntseane indicate that one of the features of "non-Western feminism" is that scholars in this tradition urge writers to

> find and highlight theory and theorizing in spaces perhaps not deemed "theoretical from a Western academic perspective" (Saavedra and Nymark 2008, p. 258). (Chilisa & Ntseane, 2010, p. 620)

Also citing Dillard (2008, p. 278), they suggest that the inquiry process can be used to explore "what Indigenous cultures can offer in terms of concrete ways to read/reread our current situations in the world" (Chilisa & Ntseane, 2010, p. 620). Although Maathai classes her book *Unbowed* on the book cover as "one woman's story" (2006), and does not use the word "autoethnography", I shall suggest in this section that it complies with what can be called *action-oriented autoethnography*, where she reflects upon her and others' transformative thoughts and actions in forming/shifting "current situations" (as experienced).

Ngunjiri, Hernandez, and Chang (2010) aver that "autoethnographic data provide the researcher a window through which the external world is understood" (p. 2). In this case of what I call action-oriented autoethnography, I highlight how Maathai sees the "external world" as a *formation-in-the-making*, to be formed and shaped through collective action. Ngunjiri, Hernandez, and Chang note furthermore, citing Ellis (2009), that autoethnographic writing offers access to "inner-most thoughts", which makes this research method "a powerful and unique tool for individual and social understanding" (2010, p. 3). Again, I underline how *Maathai develops her thoughts/theorizing in action*, as she takes into account, with others, what she considers as possibilities for action. Ngunjiri, Hernandez, and Chang (2010) make a further point that the *ethnographic* component of autoethnography

means that it is not written as a story of the "self in a vaccum", but as a story of the self operating in a *context of relationships with others and with cultural and social contexts* (as understood by the author). As they state:

> A variety of others—"others of similarity" (those with similar values and experiences to self), "others of difference" (those with different values and experiences from self), and "others of opposition" (those with values and experiences seemingly irreconcilable to self)—are often present in stories about self (Chang, 2008). This multiplicity of others exist in the context where a self inhabits; therefore, [autoethnography involves an] exploration of how the context surrounding self has influenced and shaped the make-up of self and how the self has responded to, reacted to, or resisted forces innate to the context. (Ngunjiri, Hernandez, & Chang, 2010, p. 3)

My focus here is on how Maathai presents her journey of "responding to, reacting, and resisting", with others, the forces that she found to be fostering poverty of people and of the environment (which she considers as interlinked).

Lemmer (2014), summarizing Ellis (2004), makes the point that autoethnography makes use of literary devices such as "scene setting, conversation/dialogue, characters, dramatic tension, and plot". Furthermore, the story that is created, as with other storywriting, includes emotion as the author reflects upon his/her feelings while s/he engages with the world. The degree of this display of feeling can vary across ways of practicing autoethnography, with some authors intentionally using "feeling" to in turn evoke emotional responses in audiences. Such autoethnographers are sometimes called "evocative autoethnographers" (cf. Bochner & Ellis, 2016; Dillow, 2009; Ellis, Adams, & Bochner, 2011; Ellingson & Ellis, 2008).[21] In the case of Maathai's writing, I suggest that she has taken responsibility for the kinds of emotions that her write-up may hopefully evoke—namely emotions of caring for, and appreciating the contributions of, marginalized rural women (such as those in her story), and the environment (which she regards as sacred rather than as a resource to be plundered).

Lemmer proposes that as a research inquiry we can regard autoethnography is "more than a method". Instead of seeing it as a method, it can rather be understood as the writer's "reflecting and engaging; making meaning; remaking; changing"

[21]This is distinguished from a more "analytic approach", in which as, Anderson (2006, p. 378) suggests, writers are more committed to offering "theoretical analysis" of the cultural and social mileux in which events being recounted are taking place (p. 378). But Denshire and Lee argue that while this "binary classification is useful as an initial way of making visible the variation in how autoethnographic writers integrate the strands of self and culture in their writing", it implies a separation between *self* and *culture* (2013, p. 222). In other words, holding onto a clear distinction between evocative and analytic autoethnographic work which is seen as *either* more evocative (personally-infused) *or* more analytic (offering more distanced analyses) fails to do justice to the way in which *selves becomes constituted in and through the social practices* in which they become involved (p. 222). Nonetheless, I would suggest in this regard that perhaps, in drawing a distinction between more evocative and more analytic approaches we can focus on the intentionality of the author (as understood), that is, on whether we interpret the (implicit or explicit intention) as being evocative or as presenting a stance of "analytic objectivity". In the case of, say, Maathai's writing (or my reading hereof), I consider her work as intentionally evocative.

(2014). This is how I would class Maathai's work, that is, as an effort to display her and others' remaking of meaning as part of the process of activating the GBM. Sykes (2014, p. 4) offers the label of "transformative autoethnography" when characterizing his own attempts to "develop a novel research strategy" by focusing on his lived experiences of trying to exercise transformation in an adult learning setting. Using this understanding of autoethnography, Maathai's reflective inquiry into her transformative-oriented action and learning in relation to the GBM can also be classed as "transformative autoethnography". All in all, through her write-up readers are in turn invited to engage with her storying, toward conceiving and imagining possibilities for what Bochner calls "better ways of living" (Bochner, 2000, p. 269).

6.4.2 *Maathai's Story of the Journey Setting up the GBM in Kenya*

I start this section with Maathai's account of her having graduated from high school in 1959 at a time when "the colonial era for most of Africa was coming to an end" (2006, p. 73). She notes that a newly independent Kenya "would need educated men and women to fill key positions in government and society once the British administration departed" (p. 73). To this end, the Kenyan politicians of the day encouraged contacts with the USA, to provide scholarships to study in the USA for promising students. She was chosen as one such student and went to study at a College in the USA and later (1964) at a University, where she studied for a master's degree in biology (2006, p. 92). She indicates that she had previously (in 1963) attended a six-week course on leadership at this same University (Pittsburgh); and her course paper was on "helping women in rural areas work together and promote developmental efforts". She states: "Little did I know that I would be putting theory into practice only a decade later, when I would be inspired by rural women to initiate the GBM" (2006, p. 92).

She remarks that when she left the USA five and a half years after arriving there "I was taking back to Kenya five and a half years of higher education, as well as a belief that I should work hard, help the poor, and watch out for the weak and vulnerable" (2006, p. 95). She sees the years in USA as having prepared her also to be confident not only in reclaiming her original name which she had lost during colonial times (as she had been called by other names than the one given to her by her mother), but to "critique what was happening at home, including what women were experiencing" by way of gender discrimination. This was because her years in the USA had overlapped with the beginnings of the women's movement. She states:

> Even though many women [across the globe] were still bound to traditional ideas about themselves at that time, I came to see that as an African woman I was perhaps even more constrained in what I could do or think, or even hope for. (2006, p. 96)

From her experience in the USA she considers that:

> It is fair to say that America transformed me: It made me into the person I am today. It taught me not to waste any opportunity and to do what can be done—and that there is a lot to do. (2006, p. 97)

Of course this learning of hers was her own response to what she was "taught" and reflects also her own belief in "doing what can be done" and doing this for the benefit of the most vulnerable (as she had mentioned earlier in her plotting of her journey).

She indicates that when she returned home she did not immediately get a university appointment although one had been promised to her. She considers this as being an outcome of ethnic as well as gender discrimination (2006, p. 101). However, she was fortunate to get work at a School of Veterinary Medicine connected to the University of Nairobi. She also registered for her Ph.D. (and later became the first women in Kenya to earn a doctoral degree). But she notes that after some further study in Germany, when she returned to Nairobi, she suffered discrimination as a woman at the University. As she puts it: "What I did not enjoy at the university was the discrimination I and my female colleagues faced" (in that conditions for female and male staff varied, with women receiving fewer benefits). She indicates that she and a female colleague "waged this first fight for equality together" (2006, p. 114). Via the Academic Staff Association, they managed to "be given what they were asking for" (2006, p. 116). But many women had refused to join her and her colleague in the struggle, as they had been "advised by their husbands not to be part of that struggle". She came to realize that:

> Fighting battles with women can be very difficult and sad, because both society and the women themselves often make it appear that most women are happy with the little they have and have no intention of fighting for their rights. (2006, p. 116)

But she also observes that despite this, since those early days at the University "things have changed quite a lot. Now there are quite a few women members of the academic staff at the university and the terms of service have greatly improved for everyone, including women" (2006, p. 117). Nevertheless, she states that she had not anticipated (when she got the initial university appointment) that

> I would be discriminated against on the basis of my gender as often as I was, or that I could be belittled even while making a substantial contribution to society. I did not want to accept that one human being would deliberately seek to belittle another, and I found myself challenging the idea that a woman could not be as good as or better than a man. (2006, p. 117)

She spells out some of the lessons she learned from this:

> What the struggle for equality at the university taught me was that sometimes you have to hold on to what you believe in because not everybody wishes you well or will give you what you deserve. (2006, p. 117)

She adds another side to her learning from the story:

> To their credit, I have never heard any criticism from the male colleagues with whom Vert [her female colleague in the struggle] and I raised these issues. Many of them were involved

> in our advocacy of a staff union and would have agreed that there was no reason on earth that I should receive less money than my technician. (2006, p. 117)

So we see that she expresses various human angles in her story (for us as readers to learn from/engage with). She notes that when faced with discrimination it is possible to act (especially in concert with others, such as through the staff association and the union) and that in her experience not all men try to deny women equal treatment.

6.4.2.1 The Beginnings of the GBM: A Confluence of Factors

Maathai describes her involvement in setting up the GBM as being the result of a confluence of factors, starting with small beginnings. She explains this with reference to a metaphor:

> A great river always begins somewhere. Often it starts as a tiny spring bubbling rom a crack in the soil, just like the little stream on my family's land in Ihithe, which starts where the roots of the fig tree broke through the rocks beneath the ground. But for the stream to grow into a river, it must meet other tributaries and join them as it heads for a lake or the sea. (2006, p. 119)

Her narrative account of the causes which led to the start of the GBM is thus expressed via this metaphorical account of tributaries joining toward a direction. She answers the question of why she became concerned with planting trees by noting that there are many answers, but "the essential one" was that:

> I reacted to a set of problems by focusing on what could be done. As it happened the idea that sprang from my roots [as in the fig tree roots] merged with other sources of knowledge and action to form a confluence that grew bigger than I could ever have imagined. (2006, p. 119)

One of the beginnings of the GBM was linked to her involvement at the university in a number of civic organizations, such as:

- The Nairobi branch of the Red Cross (of which she became director in 1973);
- The Kenya Association of University Women;
- The Environmental Liaison Centre, which was part of the United Nations Environmental Protection Program (UNEP), whose headquarters were in Nairobi (2006, pp. 119-120).

She indicates that most of the originators of the UNEP were from Europe, North America and Asia and felt it was important for local people (Kenyans) to serve as local board members. She was one of the few women members of the local board (2006, p. 120).

She recounts that since she had grown up "in a rural area where our daily lives depended on the health of the environment, the issues raised at the Liaison Centre were not completely strange" (2006, p. 120). Nonetheless, she was exposed to additional information through meetings at UNEP, and through books, articles and

discussions with people working in environmental NGOs in different countries (2006, p. 120). And the ideas resonated with her as they were about "dealing with natural sciences from a holistic perspective" (2006, p. 120).

Another stream contributing to her growing environmental awareness was through her post-doctoral research in various rural areas of Kenya (in an effort to keep domestic animals healthy and productive). While working in the rural areas, she explains:

> I noticed that the rivers would rush down the hillsides and along paths and roads when it rained, and that they were muddy with silt. This was very different from when I was growing up. "That is soil erosion", I remember thinking to myself. (2006, p. 121)

She noticed furthermore that:

> The people, too, looked undernourished and poor and the vegetation in their fields was scanty. The soils in the fields weren't performing as they should because their nutrient value had been depleted. Around the village, commercial trees had replaced indigenous forest. I noticed that much of the land that had been covered by trees, bushes, and grasses when I was growing up had been replaced by tea and coffee. I also learned that someone had acquired the piece of land where the fig tree I was in awe of as a child had stood. The new owner perceived the tree to be a nuisance because it took up too much space and he felled it to make room to grow tea. By then I understood the connection between the tree and water [with the tree root system serving to hold the water], so it did not surprise me that when the fig tree was cut down, the stream where I had played with the tadpoles dried up. … . Ironically, the area where the fig tree of my childhood once stood always remained a patch of bare ground where nothing grew. It was as if the land rejected anything but the fig tree itself. (2006, pp. 121–122)

She recalls from her childhood that:

> When my mother told me to go and fetch firewood, she would warn me, "Don't pick any dry wood out of the fig tree, or even around it." "Why?" I would ask. "Because that's a tree of God," she'd reply. "We don't use it. We don't cut it. We don't burn it."… I later learned that there was a connection between the fig tree's root system and the underground water reservoirs. (http://www.livelihoods.eu/wp-content/uploads/2014/05/1562423540.pdf)

Maathai surmises that it is possible that because the land itself understood (in some way) that the fig tree was sacred, it "rejected" attempts to replace the fig tree by those who did not revere it. And in her storytelling she indicates that she herself mourned the loss of the fig tree and the irreverent way of treating the environment as resources to be commercialized without a holistic understanding of, and concern with, consequences of such treatment (2006, p. 122).

In reading my account of Maathai's narrative, as I presented it in my draft of this chapter, (August 2016) critical reader Francis Akena considered that this connected with a recent example which he wished to share, to add richness to the text. This is expressed in the boxed section below:

> I connect [this narration] to an example in June, 2016 when I was conducting research in Mbuzini village, Mpumalanga Province—located at the confluence of South Africa, Swaziland and Mozambique. At the center of the

village sits a thick natural forest measuring about 400 square meters with tall and old indigenous trees species. The young man who was my translator in the village told me that "this is the King's forest. Traditionally and even to this present day and age, no one was and is allowed to collect fire wood, cut timbers or poles from the trees. It is only the King who can go into the forest on a few occasions each year to perform certain rituals". According to the young man, none of the villagers has ever entered into the forest and it's widely believed (by the villagers) that anybody who stubbornly ventures into the forest will not return. In his tone, I could read the sacredness (spiritual aspect) of the forest to this community. But gazing beyond the spiritual characteristic, I also attributed the restrictions levied on the forest to environmental protection because the mountainous community is heavily deforested with scarce trees spread across the hill/valley. The forest at the village center hence acts as a wind breaker from strong prevailing wind since the village is located on the western side of the mountain (windward side) and receives heavy down pour during rainy season. But population increase has led to pressure on the forest's coverage for farming and settlement. My conclusion is that preservation of the forest's portion in the middle of the village whether in the name of sacredness or not, could have been done for the betterment of the collective. If left to the mercy of the villagers, the forest would have been cut down for timbers/construction materials, fire wood, or cleared for subsistence farmland. This position does not of course dismiss the spiritual significance that the village so dearly attaches to the forest.

To return to Maathai's storying, she continues her nature-derived metaphor regarding the tributaries of knowledge that led to her setting up of the GBM in Kenya. One of these tributaries was, as she notes "the [rural] women themselves, who brought the urgency of the situation home to me" (2006, p. 122). One of the ways in which she came into contact with the women was through her being a member of the National Council of Women of Kenya (NCWK) founded in 1964 to "unify women's groups, both large and small, throughout Kenya, with membership drawn from urban and rural areas".[22] At a seminar organized by NCWK,

> a woman researcher presented the results of a study she had done in the central region of Kenya, which found that children in the central region of Kenya were suffering from diseases associated with malnutrition. This was an eye-opener for me, since that is where I come from and I knew from personal experience that the central region was one of the most fertile in Kenya. (2006, p. 123)

Maathai expresses how, due to the influence of colonialism, plantations of commercial trees now had replaced indigenous forest and tea and coffee plantations had also been set up, so that tea and coffee could be sold on the international market.

[22]She later became its chair: 1981–1987.

Cash crops were now "occupying land previously used to produce food for people to eat" (2006, p. 123). These facts troubled her, because they seemed so contrary to her experiences as a child—"when there was more than enough food" (2006, p. 123). She recounts how

> the colonial administration had cleared the indigenous forests and replaced them with plantations of exotic trees for the timber industry. After independence, Kenyan farmers had cleared more natural forests to create space to grow coffee and tea. Until now, however, I had not appreciated the multiple costs of these activities [in terms of their deleterious consequences]. (2006, p. 123)

She indicates that the leadership of the NCWK were concerned with trying to do something to ease the burden of these rural women, who were bearing the brunt of these "multiple costs" (2006, pp. 123–124). She also notes that in subsequent seminars organized by NCWK, other rural women confirmed the researcher's findings: "they did not have enough wood for fuel or fencing, fodder for their livestock, water to cook with or drink, or enough for themselves and their families to drink" (2006, p. 124). She remarks that "as I sat listening to the women talk about water, energy, and nutrition, I could see that everything that they lacked depended on the environment". Meanwhile, when the NCWK representatives who had gone to Mexico City for the first United Nations conference on women (1975) returned, they "carried the same message: we needed to do something about water and energy". The conference participants "had also concluded that the world needed to address the realities of rural women, their poverty, the overall lack of development, and the state of the environment that sustained them" (2006, p. 124).

She declares that now "it suddenly became clear" to her that something needed to be done at the level of finding a solution to the environmental degradation by seeking to address the root causes of this (rather than remaining at the level of symptoms). She explains:

> I didn't sit down and ask myself, "Now let me see; what shall I do?" It just came to me: "Why not plant trees?" The tree would provide a supply of wood that would enable women to cook nutritious foods. They would also have wood for fencing and fodders for cattle and goats. The trees would offer shade for humans and animals, protect watersheds and bind the soil, and if they were fruit trees, provide food. They would also heal the land by bringing back birds and small animals and regenerate the vitality of the earth. This is how the GBM began. (2006, p. 125)

She set about trying to secure funding. One avenue was through the NCWK, where she was elected in 1977 as a member of the executive committee and of the committee on environment and habitat. In this setting she proposed "planting trees as an activity the NCWK could take on to assist its rural members and so meet the woman's needs"—and this was agreed. In June 1977 Kenya marked the world-wide celebrations of World Environment Day with a procession of tree planting initiative organized by the NCWK's new initiative (2006, p. 131). She notes that subsequent to this there were other initiatives to plant trees in other parts of the country but "none of these projects lasted for long" (2006, p. 132). She learned from this that "if

you do not have local people who are committed to the process and willing to work with their communities, the project will not survive" (p. 132).

However, soon news of the tree planting initiatives started spreading through the NCWK networks and "soon farmers, schools and churches were eager to set up their own programs. This was the beginning of communities themselves taking ownership of GBM" (2006, p. 133). With the tree planting taking momentum, she recounts that she was constantly asking friends and others to sponsor trees; and they managed to get support from institutions too, such as from NCWK and also from the Canadian ambassador to Kenya and from Mobil Oil in Kenya (2006, p. 134). Now the tree planting initiatives became so popular that there was a shortage of seedlings to plant; and the solution that arose at this point was to "create our own supply of trees by starting a nursery". She continues her story:

> We organized meetings where foresters (from the Department of Forests) talked to the women about how to run their own nurseries. But these were difficult encounters. The foresters didn't understand why I was trying to teach rural women how to plant trees. (2006, p. 135)

She was told by the foresters that "you need a professional". They were suggesting to her that "you need people with diplomas to plant trees", so that the people could understand the gradient of the land and the entry point of the sun's rays, for example. She comments that "what the foresters were saying did not seem right to me". She did not believe that "the women needed all the technical knowledge the foresters were dispensing to plant trees successfully. All they needed to know was how to put the seeding in the soil and help it grow" (2006, p. 136). She points out that in any case these women were farmers so "they were putting things in the ground and watching them grow all the time" (2006, p. 136). So she told the women to "use your women sense. These seeds are very much like the seeds you deal with … every day" (2006, p. 136). She indicates that this practice took root (an apt metaphor that she often uses in her story) and "soon the women started showing one another". Tree nurseries "were springing up on farms and public land around the country. These were 'foresters without diplomas'" (2006p. 136). The NCWK also decided to give the women an incentive. So she told the women that "the movement will compensate you" (2006, 137). The women were compensated with a small amount—the equivalent of four US cents a tree—but it provided motivation. She notes that this is because:

> These were poor women who were working all the time—tending crops and livestock, gathering firewood, carrying water, cooking, taking care of children—had few options for paid employment. (2006, p. 137)

Based on the success of the tree planting in the areas in which they had started, she suggested to women to go to surrounding areas and convince others to plant trees. And she encouraged them to plant in rows to form green belts. This is how the name *Green Belt* came to be used (p. 137).

In my own display of her narration of her involvement in the GBM, I have concentrated on her mode of interacting with others—in ways which show how she,

in Ngunjiri, Hernandez, and Chang's terms, "responded to, reacted to, or resisted forces innate to the context" 2010, p. 3). For example, I have highlighted how she sometimes made decisions that went against what "did not seem right" to her—such as the professional foresters' feeling that one needed technical diploma-type knowledge to plant a tree (2006, p. 136).

Although I have focused here on how she managed to circumvent the ideas of those with whom she disagreed (while endorsing a non-deficit view of the "woman-sense" of the foresters without diplomas), I have not concentrated on her own suffering which she also records in her various books. This is sadly reported by Robinson in her Wangari Maathai lecture in 2013, in honor of the late Maathai who passed away in 2011. Robinson states, for instance, that

> the GBM grew rapidly, and Wangari refined her thinking by linking it to the global women's movement and the Nairobi Women's Conference in 1985. As the movement began to have more impact in Kenya and elsewhere in Africa, Wangari encountered serious resistance from the Kenyan Government, which, she felt, "killed creativity, nurtured corruption, and produced people who were afraid of their own leaders" [as she stated in Unbowed]. (Robinson, 2013)

Robinson notes that "through her outspokenness, she came into conflict with the government and she endured much personal suffering and vilification". Despite this, as Robinson (2013) observes, she managed to continue to exude her "spirituality and her indomitable spirit".

I have taken a slice out of her autobiographical writing to make a point about the treatment of it as active autoethnography, which expresses her account of her learning in action (with others and in social contexts). I now examine her narration by considering her ethical position, how her approach might be validated in terms of its quality of inquiry, and in what sense her narration can be classed as generative theorizing.

6.4.3 Ethical Position: Inquiry in Action into "What Can Be Done"

Maathai's autobiographical narration can be seen as a story of a person engaging with the world in order to, as she puts it, "do what can be done" (2006, p. 97)—which possibilities can only be discovered in action. She notes that already in her time in the USA she knew that she was going to try to do what could be done to help the "weak and vulnerable"; and that this is where her allegiance lay (2006, p. 95). Later, after returning to Kenya, as she hears about the research of others and relates this to her own research and her childhood and adult experiences it becomes clear to her that dealing with the problems as experienced by, for example, poor rural Kenyan women, is inextricably tied to dealing with environmental issues (and with adopting a stance of caring for the earth). In any case, she clearly is not trying to write her story by analytically taking distance from her involvement in "the

world" and its shaping. Her memoir is a reflection on the meaning of the past as involving her, along with others, facilitating the growing of ideas as well as trees to make what may have seemed impossible possible (2006, p. 329). She writes as a person who is committed to engaging with those whom she sees as vulnerable, and with the sacredness of the earth, to explore (in action) what could be done to increase human and planetary well-being/healing. The value of this autoethnographic work does not come from her hiding her ethical position, but from expressing herself as a person holding on to commitments toward seeking social and environmental justice (as defined in conjunction with vulnerable people.

6.4.4 *Validation: Appreciating an Author's Memory Work as Inspirational*

As stated by Ellis and Bochner (2000) and re-iterated by Ngunjiri, Hernandez and Chang (2010), when validating autoethnography as research one need not try to locate a specific method that has been used to generate the "data" as recorded in the author's narrations, in order to judge its quality. I would suggest that especially in what I call action-oriented autoethnography which highlights an author's reflections around "doing something" about issues experienced as problematic (in this case both socially and ecologically problematic), one is not on the look-out for the method through which "evidence" has been established. But one can (as reader) get a glimpse of an author's meaning-making memory work as s/he retrospectively considers, and makes sense of his or her learning processes and manner of ethically engaging with social and environmental contexts (as now remembered). In reading an author's account of a remembered past, which expresses how the author is now "rethinking, redescribing, and even refeeling the past" (Bochner, 2017, p. 73) we can get a glimpse of how an author is continuing to making sense of his or her learning process and engagement with the world, as these become turned into experiences "through the activity of remembering" (2017, p. 173).

The judgement (by others) of the quality of the practice-based data that is displayed in such an autoethnographic account is dependent on our considerations of how the researcher/reflective practitioner has re-looked at issues experienced as problematic and recounted discoveries made in action about what became possible. Ellis, in considering how one can substantiate evocative authoethnograpic work more generally, states that for her

> validity means that our work … evokes in readers a feeling that the experience described is lifelike, believable, and possible. You also can judge validity by whether it helps readers communicate with others different from themselves or offers a way to improve the lives of participants and readers—or even your own. (Ellis, 2004, p. 124)

Here Ellis suggests that one criterion of validation could be in the form of a judgement as to whether the work helps people (those who may have been part of the author's network of actors in the story, as well as other readers) to "improve

their lives". One can argue in addition that if it helps participants and readers/audiences to consider more carefully their involvement in acting as stewards in caring for the earth, this too is a consideration that would help to validate the research (as in the case of Maathai's work). When Ellis offers the practical criterion of validation as linked to "improving lives", she veers toward adopting a notion of catalytic validity which I outlined in Chap. 1, Sect. 1.3.2. Maathai's autoethnographic work can therefore be assessed in terms of our considerations of how she has expressed her engagements with others toward improving the quality of (social and ecological) life. To the extent that we as audiences feel—based on the "data" that she supplies via her narration—that her account is believable, and insofar as we ourselves become moved by its inspirational value, the work gains "validity" as transformative inquiry.[23] Furthermore, I would suggest that Maathai has taken specific responsibility to write-up her engagements in a way that can inspire others not only toward action (toward improving lives), but indeed toward *learning in action*—as reflective practitioners who learn from others whom they encounter, but who also can resist (and give reasons on an intellectual and emotional level) what "does not seem right".

6.4.5 *Generative Theorizing: Theorizing Options for Empowerment in Practice*

As I noted in Sect. 6.4.1, Chilisa and Ntseane (2010, p. 620), citing Saavedra and Nymark (2008, p. 258), point out that certain non-Western feminists have urged scholars to "find and highlight theory and theorizing in spaces perhaps not deemed 'theoretical from a Western academic perspective". In line with this, Maathai displays her research-in-action as *a person learning with others to "do what can be done" and to act in terms of the imagination of possibilities.* Her theorizing relates primarily to women's experiences (and to her own learning) to develop an understanding of, say, experiences of poverty as linked to environmental degradation, and of options for imagining and practicing change.

Although it can be said that Maathai's ways of theorizing women's poverty and options for empowerment to address social and environmental issues fits in with a less analytic notion of theorizing than may be offered with "Western" feminism, this does not mean that Maathai draws a strong line between different feminist agendas (or agendas in the global women's movement). She highlights, in what Bochner (2017, p. 173) calls "memory work" the "knowledge" that coalitions in practice with others from the women's movement—e.g., at international conferences—provided further inspiration and support for the efforts being undertaken in

[23]Finzer (2015) suggests in this regard that "powerful storytelling counts among the most effective forms of activism as speakers and writers use story to build empathy in audiences" (p. 2).

Kenya (which were later extended to other African countries and which she hoped to extend worldwide[24]).

Moreover, Maathai does not try to use a single "discipline" of thinking when conceptualizing the connections between factors leading to experiences of poverty and of social and environmental degradation of quality of life. Her knowing processes as she (now) shares them with us include her biological research background, her (qualitative) research with rural women, and her listening to their stories as told and explored together. She thus develops a mode of thinking that draws connections, just as advised by Sandoval (2000), as cited by Chilisa and Ntseane (2010, p. 620). What makes her theorizing—as learning with others—generative (forward looking), is her way of developing action-directed insight into the need for environmentally oriented solutions, a need which suddenly becomes clear to her (using a form of abductive/retroductive inference—see also Sect. 6.2.6) based on the confluence of factors that she mentions, and on her imaginative response. In terms of Ellis, Adams and Bochner's understanding of autoethnography (2011, para 6), she has provided new perspectives on her "remembered moments perceived to have significantly impacted the trajectory of [her] life … after which life does not seem quite the same" and she has expressed how she negotiated these remembered "intense situations". Through her text she in turn nurtures readers' imaginative capacities. As Bochner suggests:

> Alternative [transformative] ethnographers [and autoethnographers] usually want to produce interesting, innovative, and evocative texts, works that seek to nurture the imagination not kill it. (2000, p. 268)

In the next chapter, Sect. 7.5, I elucidate further why I consider Maathai's autoethnographic reflections as involving a form of imaginative generative theorizing.

6.5 Conclusion

In examining the research chosen for discussion in this chapter (in relation to authors' texts and sometimes further personal conversations), I focused upon how the research facilitators/reflective practitioners could be said to have used the research space (space for learning) in what I would call a responsible way, as part of a conscious effort of trying to activate and inspire social and environmental justice projects.

[24]Maathai states in this regard that "the challenges facing Africa, particularly the degradation of the environment, are facing the entire world. This is why the GBM International [another organization which she founded] was founded. Only by working together can we hope to solve some of the problems of this precious planet" (2006, p. 329). Here again she used the book as a platform to try to remind us of our connectedness to one another and to the planet.

In the first example, McIntyre-Mills used the avenue of research (via primarily an interactive software package) to engage a South Australian local government and its residents to "think about their thinking" and their ways of living in terms of consequences for themselves and others, and for the planet. By setting up the research in this manner, McIntyre-Mills herself takes some responsibility for the possible consequences of *spurring people to think about questions that may otherwise have not been raised or thought about in depth* (that is, without the stimulus of the research package). She accepts that one of the consequences—which indeed she hopes for—is that people will be spurred to rethink business-as-usual type thinking (and feeling). The research "evidence" in some way bore out that people (including people with middle class income levels as were the majority in this case) have the capacity to rewire their neural networks and reconsider their "pathways to wellbeing". McIntyre-Mills explores this human capacity with reference to this evidence and wider literature and her own inferences. Her own inferencing involves value-infused and forward-looking imaginative leaps concerning what might be possible, which she shares with audiences. Her hoped-for audiences include an array of people whom she hopes to inspire to do further research and action along similar lines (pers. comm. face-to face, 14 January 2016). McIntyre-Mills does not present the research "results" with any kind of finality in regard to implications for addressing local and global challenges. But she does not shy from offering some generative theorizing, which I have further drawn out and extrapolated, regarding the human potential to recognise our connectedness with others and with the planet, as a route to an inclusive wellbeing.

In the second example, Roman-Alcalá used the OTF case of insider-based activist research as an avenue to deliberate with other participant activists—via interview and other conversations—on the import of the OTF endeavour, and as an avenue to share with additional audiences his further theoretical deliberations. The research (re-looking at the case) involves his developing interpretations, which are based on his speaking with others and critically reflecting as reflective practitioner/activist around possible further action in the FS arena. He also expresses an awareness (in his write-up) that the way in which he speaks to people and writes up conversations is itself not without social consequences in that it *makes accessible—and strengthens—certain discourses and it points to possibilities for action which hitherto might have seemed unfeasible*. Again, as with McIntyre-Mills he does not wish in his write-up to offer any final interpretation of the meaning of the OTF action, but rather he wishes that people reading the text (and/or engaging with other reporting/documentaries etc.) can be "moved" thereby. For this reason I call it *action-oriented case study research* geared to stimulating further movement in terms of social and ecological justice goals.[25]

In the third example, I discussed Maathai's exploring via her autobiography/autoethnography how she was able—through involvements in various associations,

[25]Roman-Alcalá indicated to me (by email 3 July 2016) when he saw this characterization of his work that he: "Agreed! ☺".

organizations and movements—to generate collective action to turn around some of the ill-effects of colonization of the people and the environment in Kenya. She indicates how she learned that it was crucial to involve the communities in this process, and how the GBM depended on many, many acts by many, many people: the movement had to resonate with the concerns of people at grassroots level. It also depended on people who occupied (and volunteered to occupy) important positions in organizations (such as herself) not taking an elitist stance, but being willing to serve the "weak and vulnerable". Through her storying she emphasizes how with hope and perseverance (and imagination) it is possible to do what might seem impossible at first in order to improve the quality of people's lives and at the same time heal the land. Her inferences that she makes in the process of reflecting on her learning are not meant to offer "analyses" of the kind that Chilisa and Ntseane (2010, p. 618) indeed question as often being linked to Western-styled theorizing preferences. In Chap. 7, I explore further the issue of what it may mean to engage responsibly in generative theorizing, where theorizing is seen as linked to potentially shaping social and ecological outcomes.

References

Ackoff, R. L., & Pourdehnad, J. (2001). On misdirected systems. *Systems Research and Behavioral Science, 18*(3), 199–205.

Anderson, L. (2006). Analytical autoethnography. *Journal of Contemporary Ethnography, 35*(4), 373–395.

Bawden, R. (2005). Systemic development at Hawkesbury: Some personal lessons from experience. *Systems Research and Behavioral Science, 22*(2), 151–164.

Binkley, S. (2000). Kitsch as a repetitive system: a problem for the theory of taste hierarchy. *Journal of Material Culture, 5*(2), 131–152.

Bochner, A. (2000). Criteria against ourselves. *Qualitative Inquiry, 6,* 266–272.

Bochner, A. P. (2017). Heart of the matter. *International Review of Qualitative Research, 10*(1), 67–80.

Bochner, A. P., & Ellis, C. (2016). *Evocative autoethnography: Writing lives and telling stories*. New York, NY: Routledge.

Brown, B. (2012). Global democracy alone will save us from ourselves. *The Sydney Morning Herald*, 31 March, 2012. https://www.smh.com.au/politics/federal/global-democracy-alone-will-save-us-from-ourselves-20120330-1w3s9.html

Cannella, G. S., & Lincoln, Y. S. (2007). Predatory versus dialogic ethics: Constructing an illusion or ethical practice as the core of research methods. *Qualitative Inquiry, 13*(3), 315–335.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., & Ntseane, G. (2010). Resisting dominant discourses: Implications of Indigenous, African feminist theory and methods for gender and education research. *Gender and Education, 22*(6), 617–632.

Cram, F., & Mertens, D. M. (2015). Transformative and Indigenous frameworks for multimethod and mixed methods research. In S. Hesse-Biber, & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 91–109). Oxford: Oxford University Press.

Denshire, S., & Lee, A. (2013). Conceptualizing autoethnography as assemblage: Accounts of occupational therapy practice. *International Journal of Qualitative Methods, 12*(1), 221–236.

Dillard, C. B. (2008). When the ground is black, the ground is fertile: Exploring endarkened feminist epistemology and healing methodologies in the spirit. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and Indigenous methodologies* (pp. 277–292). Thousand Oaks, CA: Sage.

Dillow, C. (2009). Growing up: A journey toward theoretical understanding. *Qualitative Inquiry, 15*(8), 1338–1351.

Ellingson, L. L., & Ellis, C. (2008). Autoethnography as constructionist project. In J. A. Holstein & J. F. Gubrium (Eds.), *Handbook of constructionist research* (pp. 445–466). New York, NY: Guilford Press.

Ellis, C. S. (2004). *The ethnographic I: A methodological novel about autoethnography*. Walnut Creek, CA: AltaMira Press.

Ellis, C. S. (2009). *Revision: Autoethnographic reflections on life and work*. Walnut Creek, CA: Left Coast Press.

Ellis, C. S., Adams, T. E., & Bochner, A. P. (2011). Autoethnography: An overview. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research, 12*(1), Art. 10.

Ellis, C. S., & Bochner, A. (2000). Autoethnography, personal narrative, reflexivity: Researcher as subject. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (2nd ed., pp. 733–768). Thousand Oaks, CA: Sage.

Fenelon, J. V., & Hall, T. D. (2008). Revitalization and indigenous resistance to globalization and neoliberalism. *American Behavioral Scientist, 51*(12), 1867–1901.

Finzer, E. S. (2015). Putting environmental injustice on the map: Ecotestimonies from the global South. *Studies in 20th and 21st Century Literature, 39*(2), 2.

Flood, R. L., & Romm, N. R. A. (1996). *Diversity management: Triple loop learning*. Chichester, England: Wiley.

Gergen, K. J. (2001). Psychological science in a postmodern context. *American Psychologist, 56*(10), 803–813.

Goff, S. (2014). The generative and catalytic powers of creative, expressive arts in Action Research. *ALAR: Action Learning and Action Research Journal, 20*(1), 112–141.

Guttal, S. (2013). Porto Alegre, Nyéléni and Bukit Tinggi: Evolving views of agrarian reform. *Journal of Peasant Studies, 40*(4), 739–747.

Harris, L. D., & Wasilewski, J. (2004). Indigeneity, an alternative worldview: Four R's (relationship, responsibility, reciprocity, redistribution) versus two P's (power and profit). Sharing the journey toward conscious evolution. *Systems Research and Behavioral Science, 21*(5), 489–503.

Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.

Lemmer, E. (2014). *An introduction to autoethnography: Using yourself as data*. Workshop presented at University of South Africa, 8 October.

Lewin, K. (1947). Concept, method and reality in social science: Social equilibria and social change. *Human Relations, 1*(1), 5–41.

Lyotard, J. -F. (1984). *The postmodern condition: A report on knowledge* (G. Bennington, & B. Massumi, Trans.). Minneapolis, MN: Minnesota University Press.

Maathai, W. (2004). *The green belt movement*. New York, NY: Lantern Books.

Maathai, W. (2006). *Unbowed: A memoir*. New York, NY: Anchor Books.

Maathai, W. (2010). *I will be a humming bird*. Retrieved June 5, 2015, from https://www.youtube.com/watch?v=IGMW6YWjMxw.

McCrea, R., Walton, A. M., & Leonard, R. (2014). A conceptual framework for investigating community wellbeing and resilience. *Rural Society, 23*(3), 270–282.

McIntyre-Mills, J. J. (2013). *Development of participatory decision making software* (Report prepared for South Australian Local Government Association (SALGA)). Adelaide: Flinders University.

McIntyre-Mills, J. J. (2014a). *Systemic ethics and non-anthropocentric stewardship*. New York: Springer.

McIntyre-Mills, J. J. (2014b). *Transformation from Wall Street to wellbeing*. New York, NY: Springer.
McIntyre-Mills, J. J. (2017). *Planetary passport: Representation, accountability and regeneration*. New York, NY: Springer.
McIntyre-Mills, J. J., & Binchai, N. (2014). Being, having, doing and interacting: Toward ethical democracy, governance and stewardship. In *Proceedings of the 57th Annual Meeting of the ISSS-2013, Hai Phong, Vietnam*.
Mertens, D. M. (2007a). Transformative paradigm: Mixed methods and social justice. *Journal of Mixed Methods Research*, *1*(3), 212–225.
Mertens, D. M. (2007b). Transformative considerations: Inclusion and social justice. *American Journal of Evaluation*, *28*(1), 86–90.
Mertens, D. M. (2014). *Research and evaluation in education and psychology* (4th ed.). Thousand Oaks, CA: Sage.
Molefe, M. (2014). Critical reflections on Gyekye's humanism: Defending supernaturalism. In E. Imafidon & J. A. I. Bewaji (Eds.), *Ontologized ethics: New essays in African meta-ethics* (pp. 121–131). Lanham, MD: Lexington.
Molefe, M. (2015). A rejection of humanism in African moral tradition. *Theoria, 62*(143), 59–77.
Murove, M. F. (2005). *The theory of self-interest in modern economic discourse: a critical study in the light of African humanism and process philosophical anthropology* (Doctoral thesis, University of South Africa, Pretoria).
Murove, M. F. (2007). The Shona ethic of Ukuma with reference to the immortality of values. *The Mankind Quarterly, 48,* 179–189.
Ngunjiri, F. W., Hernandez, K. C., & Chang, H. (2010). Living autoethnography: Connecting life and research. *Journal of Research Practice, 6*(1), 1–17.
Onwuegbuzie, A. J., & Frels, R. K. (2015). A framework for conducting critical dialectical pluralist focus group discussions using mixed research techniques. *Journal of Educational Issues, 1*(2), 159–177.
Onwuegbuzie, A. J., & Leech, N. L. (2007). A call for qualitative power analyses. *Quality and Quantity, 41*(1), 105–121.
Robinson, M. (2013). *Wangari's inspiration on courageous leadership and climate justice*. Royal Botanic Gardens, Kew, London: Inaugural Wangari Maathai Lecture, 27 March.
Roman-Alcalá, A. (2013). *Occupy the farm: A study of civil society tactics to cultivate commons and construct food sovereignty in the United States*. Paper presented for Discussion at an International Conference Held at Yale University Entitled Food Sovereignty: A Critical Dialogue, 14–15 September (Paper 75).
Roman-Alcalá, A. (2015). Broadening the land question in food sovereignty to northern settings: A case study of occupy the farm. *Globalizations, 12*(4), 545–558.
Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York, NY: Springer.
Rose, D. B. (1996). *Nourishing terrains: Australian Aboriginal views of landscape and wilderness*. Canberra: Australian Heritage Commission.
Salleh, A. (2015). Another climate strategy is possible. *Progress in political economy (PPE), 23 December*. Retrieved September 6, 2015, from http://ppesydney.net/another-climate-strategy-is-possible/.
Sandoval, C. (2000). *Methodology of the oppressed*. Minneapolis, MN: University of Minnesota Press.
Schiavoni, C. M. (2015). Competing sovereignties, contested processes: Insights from the Venezuelan food sovereignty experiment. *Globalizations, 12*(4), 466–480.
Schön, D. A. (1983). *The reflective practitioner: How professionals think in action*. New York, NY: Basic Books.
Sen, A. (1997). *On economic inequality* (enlarged edition, J. Foster, & A. Sen Annexe). Oxford: Clarendon Press.

Shattuck, A., Schiavoni, C. M., & Van Gelder, Z. (2015). Translating the politics of food sovereignty: Digging into contradictions, uncovering new dimensions. *Globalizations, 12*(4), 421–433.

Shiva, V. (2002). *Water wars: Privatization, pollution and profit*. London: Pluto Press.

Stiglitz, J. (2010). *From measuring production to measuring wellbeing*. Paper presented in Melbourne, 29 July. Australian Productivity Commission Review of Government Service Provision.

Sykes, B. E. (2014). Transformative autoethnography: An examination of cultural identity and its implications for learners. *Adult Learning, 25*(1), 3–10.

Wilkinson, R. G., & Pickett, K. (2009). *The spirit level: Why more equal societies almost always do better*. London: Allen Lane.

Wilson, S. (2008). *Research is ceremony: Indigenous research methods*. Halifax, Canada: Fernwood.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia* (Doctoral thesis, Antioch University, Yellow Springs, Ohio).

Wulff, D. (2010). Unquestioned answers: A review of Research is ceremony: Indigenous research methods. *The Qualitative Report*, *15*(5), 1290–1295.

Chapter 7
Responsible Generative Theorizing

Abstract In this chapter I develop further my discussions of generative theorizing as introduced in earlier chapters and I explore in more depth what responsible generative theorizing might amount to. I link this to a discussion around the use of retroductive logic, which I argue (combined in some way with deduction and induction) offers a way of organizing inferences which include imaginative leaps and which can, in accordance with generative theorizing, be consciously forward looking. I suggest that a kind of retroductive logic is being invoked when inquirers/co-inquirers create inferences which admittedly do not relate in any direct logical way to "empirical evidence", but which make sense of interpreted evidence/experience in ways which are in turn inspiring of constructive action. I indicate that such a mode of inference therefore can become a resource for generative theorizing, which is *consciously geared to being future forming*. I offer examples of this by drawing on the detailed discussions which I introduced in earlier chapters. I also highlight (and draw out) associations between retroduction and Indigenous arguments regarding the importance of grounding knowledge generation (as a social practice) in an appreciation of our connectedness with others and with all that exists.

7.1 Introduction

In this chapter I draw on the detailed examples that I have introduced in earlier ones, to expand upon what might be meant by generative theorizing (as labelled and explored by Gergen, 1978, 2001, 2015). With reference to examples from Chaps. 2 to 6 (and my discussions of generative theorizing therein) I explore in more depth what responsible generative theorizing might amount to. I link this to a discussion around the use of retroductive logic, which I argue (combined in some way with deduction and induction) offers a way of organizing inferences which include imaginative leaps and which can, in accordance with generative theorizing, be consciously forward looking. I therefore suggest that retroductive inferencing can serve as a resource for responsible generative theorizing, which is consciously

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_7

geared to being future forming. I also suggest that for retroductive inferencing to fulfil the ethical principles that I have introduced in earlier chapters (and which are implicit in Gergen's relational account of morality and epistemology), it has to be grounded in dialogical reasoning, much in the way that Indigenous-oriented authors suggest is the key to collective knowledge generation and attendant action.

To structure the chapter, I firstly offer an in depth explication of Gergen's account of generative theory and theorizing (Sect. 7.2). I then discuss, drawing upon and extending his position, the following (under Sects. 7.3–7.5):

- Theorizing Social Structure Toward Envisioning Possibilities for Reconstruction
- Retroduction/Abduction as a Resource of Generative Theorizing
- Revisiting Imputations of Causality in Generative Theorizing.

In Sect. 7.3, I expand upon Gergen's suggestion that generative theorizing in relation to social structure points to possibilities for *re-envisaging structure as historically generated practices which are therefore open to intervention*. I illustrate how research as a process of re-viewing with view to transformative action can be accomplished, and I offer reflections on what this might mean in terms of researcher responsibility.

In Sect. 7.4, I offer some suggestions as to how retroductive modes of inference can function as a resource for such generative theorizing. I relate this to a discussion of what has been called "grounded theory", because certain proponents hereof refer to the utility of abduction (or retroduction) as a way of grounding our theorizing while not being bound to supposed "evidence" (Sect. 7.4.1). Some proponents whose arguments I discuss, also refer to possible pragmatic epistemological interpretations of the status of such theorizing. That is, the theorizing develops its status by grounding cognition in its helpfulness in contributing to addressing problematic situations (understood anew). I suggest that if we wish to advance a pragmatic vision of grounded theorizing as tied to a justice-oriented transformative agenda, the grounded theoretical conception of "theoretical sampling" as a way to enrich theoretical categories, can (and should) be shifted to a focus on "active sampling" (as explained in Sects. 7.4.1.1 and 7.4.2). I also show how retroductive reasoning can be regarded as fitting well (to use Hall and Fenelon's terminology, 2009, p. 38) with Indigenous styles of reasoning and acting (Sect. 7.4.3).

In Sect. 7.5, my focus is on revisiting the concept of causality as it features in social scientific research. I deliberate on the status which we might assign to any considerations of causality in processes of theorizing. I proceed to suggest a transformative-infused pragmatic account of how inquirers can proceed, in discussion and in action, to identify leverage points for altering felt-to-be restrictive patterns. I indicate that this is not predicated on people assuming or stating that their account of such leverage points (as inferred and found to be meaningful in action) mean that they have come to grips with the "real causes" of any experienced effects (as if this is the only way of regarding them). The circumspect treatment of the status of statements about causality, I suggest, is in keeping with Indigenous (and decolonizing) methodologies, which are characterized by the practitioners'

highlighting that stories about the world should not be posed as hegemonic, as, for example, urged by Collins (2000, p. 36).

In all of these sections, I offer examples with reference to, and now expanding upon, my earlier chapters. It should be noted that in my discussion running through this chapter of various self-named pragmatist's arguments I accord with Joas (1994, p. 506) who indicates that the everyday sense of the term "pragmatic" (as a kind of "muddling through" in relation to "immediate requirements") has contributed to misunderstandings regarding the disinclination in a pragmatic position to consider "theoretical or moral principles". In the course of the chapter I highlight Peirce's pragmatist understanding (as summarized by Joas, 1994) that knowledge is to be defined in terms of its being a vehicle for dealing with the world in which ways which also reflect the human capacity for "creative accomplishments" (1994, p. 506). Considered in this fashion, pragmatism as a position can be regarded as compatible with transformative paradigmatic tenets, as long as social and ecological justice issues are foregrounded in the position (and taken into account in research processes). To begin my discussion around what this all implies in relation to the justification of transformative-oriented theorizing, I refer to Gergen's account—which he links to "pragmatism with a conscience" (2015, p. 290)—of generative theory and theorizing.

7.2 An Explication of Gergen's Account of Generative Theory and Theorizing

In Gergen's article entitled *Toward Generative Theory* (1978), he argues that "much contemporary theory [in the social sciences] appears to lack generative potency, that is, the capacity to challenge prevailing assumptions regarding the nature of social life and to offer fresh alternatives to contemporary patterns of conduct" (1978, p. 1344). He locates a number of reasons for this, one of them being the injunction for scientists—especially those aligned to what he calls the "traditional positivist-empiricist paradigm"—to ensure that any theorizing "should ideally be premised on sound fact". He points out that this insistence on "establishing the facts" so that theorizing can ensue based on "the evidence", seems to be inimical to "some form of creative or intuitive act" which is the hallmark of new theories in both the physical and social sciences (1978, p. 1347).

Gergen avers that modes of inquiry which are based on developing hypothesized statements and trying to test them with reference to the facts (in order to support, adapt, or negate them as part of our theorizing processes) do not take sufficiently into account how "common sense" often serves to "unconsciously guide one's observations and hypotheses, and [resulting] theoretical models (1978, p. 1347).[1]

[1]Gergen notes that from a traditional scientific perspective (which would include traditional positivism and Popper's later postpositivist work on hypothetico-deductivism), "a close

He argues that in the realm of the social sciences this means that what is taken as common sense can all too easily become reinforced through practices of "science", which means that the science is "generating theory that fulfils itself" (p. 1347). As the theory becomes constructed, so it all-too-easily *perpetuates* common sense, and therefore does not play a part in shifting purportedly observed and theorized "patterns" (considered as connections between observables). Gergen laments that thus far "virtually unexamined by the field [of social science] is the potential of the science to shape the meaning systems of society and thus the common activities of the culture" (1978, p. 1349). This is what he calls the self-fulfilling property of science that is virtually unacknowledged as scientists go about the task of theorizing the social world.

He argues that from a positivist perspective (which we can say includes positivism and postpositivism and may also include those qualitatively-oriented researchers who try to minimize their impact on participants[2]) "one might view such shaping effects with dismay". This is because the traditional role of the scientist is seen as tied to "observation, description and explanation" (1978, p. 1349). But now, he argues, instead of viewing the shaping effects as "nettlesome" (as creating nettles in the practice of science) we can choose to *regard the shaping effects of science as an asset*. Social theorists can recognize that they can "directly alter patterns of social action as his or her mode of conceptualization is incorporated into the common understandings of the culture" (p. 1350). This means in turn that

> not only is the theorist urged to free him or herself from the shackles of prevailing conceptual agreements [which may seem to form common sense], but is asked to consider alternative social forms that may be created through theory. (1978, p. 1350)

It is this type of theory which Gergen calls "generative theory" (and the process of creating such theories can be called generative theorizing). He laments that currently (in 1978)

> we have [as researchers] too frequently stopped short of realizing the generative potential of present [research and theorizing] pursuits. At the same time we have little encouraged creative theorizing and have scarcely begun to take advantage of theory as a means of social reconstruction. (1978, p. 1358)

Gergen notes that some authors—mainly in the positivist/positivist tradition, but any authors insisting on the "empirical" basis of science—claim that if (generative) theories become conscious expressions of value (as they unashamedly advance ideas for better ways of living) they may lose their status in society as being "science" (1978, p. 1357). But he counter-argues that "it is simply not clear that society searches for dispassionate theoretical accounts" to be advanced by

---

relationship should ideally be maintained between theory and data" (1978, p. 1350) so that statements that are generated can be regarded as "ethically neutral" (1978, p. 1350). Gergen's account of this position would be applicable also to qualitative theorizing which assumes that inductive theory-building is a way of securing researcher neutrality.

[2]See my discussion in Romm (2010, pp. 241–243) regarding attempted minimization of what is sometimes called the reactivity effect.

scientists/professional researchers. He suggests that the question of what is expected of social scientists "is deserving of continued exploration" (1978, p. 1358).

In his entitled *From mirroring to world-making: Research as future forming* (2015), he remarks with some relief that, as Wertz (2011) has also remarked, one can suggest that in the social sciences presently there is emerging "a quite robust spirit of pluralism", with "all forms of research" being credentialized. He suggests that "it is indeed this spirit of pluralism that has fueled the enormous expansion in qualitative research practices"—a spirit which has also been inspired by Denzin and Lincoln's *Handbook of qualitative research* (1994). He sees one of the consequences of this as being that researchers for the most part now recognize that all research practices "can be legitimated in their own terms"; and he argues that the question then becomes one of outcomes:

> What does the research ultimately contribute to the world more generally? And this question is accompanied by a critical concern with politics and ideology. For whom are the outcomes useful, and in what way; who is benefited, who may be harmed; and who is absent from the discussion? We have, then, a pragmatism with a social conscience. (2015, p. 290)

However, he expresses concern that although it is often recognized that research is "socially inflected" (and not without social consequences),

> lingering in the wings, there is a dulling sense of the eternal return of the same. Traditional practices of research remain unchallenged; and while there is an impressive accretion in the range of practices, they do little to violate or challenge the basic structure of inquiry …. That is, the vast share of these research practices carry with them a dualist premise, distinguishing between the world on the one side and the observer on the other …. Metaphorically, then, the process of research is effectively one of watching, looking, or seeing. (2015, p. 290)

In contrast to this vision of "watching, looking and seeing" Gergen proposes the view of research as *world-forming*. He refers in this regard to what he calls "logics of reflective pragmatism" (2015, p. 287). In spelling out what might be meant by this, he refers to future possibilities:

> Now invited is a further exercise of the imagination, extending and expanding the vision of inquiry. There are promising possibilities, for example, in linking researchers with the creativity by design movement in the technological sphere. More broadly, one might explore the possibility of researchers working with governments to experiment with new and more viable forms of governance? Or, might it profit the world to have at its disposal a body of researchers whose talents were available to work creatively with conflicted parties? With education in the science of world making, and the broad dissemination of successful innovations, perhaps we could escape the logic of determinism and begin to realize the potential of collaboratively fashioning the future. (2015, p. 303)

Here Gergen is offering a range of possibilities to escape the "logic of determinism" (p. 303) where science seems to offer visions which often function to reproduce the existing patterning of social existence. In order for science to perform a different function, we need to unsettle the view of how theory is conceived. He notes that:

> Traditionally the chief functions of theory in traditional social science are primarily to integrate and synthesize findings, observations and interpretations from the broad domain

> of research; to provide a framework from which to mount fresh inquiry (bearing on the ultimate viability or truth value of the theory); and to offer the society an account of the world from which useful applications can be derived. (p. 304)

He contends that this understanding of science as directed toward (evidence-based) inquiries which then may result in "applications" is not sufficient if we are to capitalize on its "future making potentials" (p. 304).[3] He proposes that what is needed for a future-forming science is a "relative independence from what might otherwise be considered 'established fact' …. This liberation from the sedimented realities grants the theorist liberty to construct new and challenging intelligibilities" (2015, p. 304).

He avers that within such a conception of research and theorizing, what are traditionally considered as *ought* questions concerning the kind of future to which we wish to contribute *cannot be eluded*: "the traditional claim that science is concerned with what is, rather than what ought to be, is now reversed" (p. 306). He adds that researchers' involvement in making moral choices is always at the same time a social involvement and never simply "linked to the individual actor" (as in some traditional moral conceptions). He criticizes the individualist conception of the making of moral choices:

> The metaphor of the lone figure of Galileo heroically confronting the church, lends tacit support to an individualist view of moral decision-making. The question of "ought" is thus a personal one: "what future do *I* value?" However, there is an important way in which the logic developed here shifts the site of moral choice *from the individual to the relational sphere*. The key point is embedded in the earlier proposition that to know about a given form of behavior opens the possibility of its being done (or not). Inhering in this view is an understanding of human action as issuing from shared intelligibilities …. From this standpoint, activities become valuable, worthwhile, or *moral from within relational activity* …. What pass for individual decisions are essentially social actions removed from their birthplace in the swarm. (2015, p. 306, my italics)

Based on these deliberations, Gergen suggests that:

> Choices concerning the future building outcomes of research should not, then, be matters of personal integrity but of relational responsibility—responsibility to the social process out of which morality emerges. (2015, p. 307)

This conception of morality is clearly consistent with the axiological tenets referred to in Chap. 1, Sect. 1.3.3, where I noted how transformative and Indigenous-oriented paradigms in particular conceptualize inquiry processes geared toward addressing social and ecological justice as being *relational* (with relations including human

[3]Deetz clarifies that working with a view of generative theorizing (as developed by Gergen) means that people in communities are not urged to apply the theories—rather, as he puts it, the point is to engage in a *potentially inventive conversation* so that (professional) researchers can "develop a conversation with people in communities around their needs rather than our literatures and preferred topics of study" (2008, p. 290). He also concurs with Gergen (1978) that it is important to develop generative theorizing, which Deetz suggests can better be understood as *co-generative theorizing* (p. 289, my italics). Following Gergen, he considers that this involves as he calls it, "a compelling way to think and talk about the world" (p. 290).

awareness of our relations with what Chilisa, 2012, calls "living and nonliving things").

In keeping with this relational axiology, Gergen also refers to the importance of social theorizing not remaining "in the hands of the research community"—that is, professional inquirers in the academy. As he puts it:

> The potentials of inquiry into future building practices are enormous. However, one limitation of this genre lies in the fact that the capacity for creation remains primarily in the hands of the research community. Would it be possible, one asks, to enhance the potential of the populace to experiment more creatively in developing alternatives to unwanted conditions? (2015, p. 301)

He indicates in this regard that what is sometimes formally called *action research* as a genre includes efforts to develop modes of research where (professional) inquirers "work collaboratively with those outside the academy in achieving social change" (2015, p. 302). He notes that "much of this work shares in a conception of practice based—as opposed to propositional—knowing" (2015, p. 302). In other words, the criteria for quality knowing is not that statements as propositions gain their warrants (justification) by proponents claiming that they are more likely than others to closely "reflect" some posited reality, but by our tying knowing to action to *address experienced problematic conditions*. He links this to a dialogically-based epistemology which is operative in such a practice-based inquiry:

> Most interesting, however, is a shift within the action research community from a conception of knowledge as an individual possession, to knowledge as inhering within relations among people". (2015, p. 302)

Here Gergen refers to a relational view of knowledge similar to the relational understandings offered by the Indigenous methodologists whose arguments I introduced and threaded through the discussion in Chap. 1. Gergen touches on the issue of what the dialogue between people in such a collaborative knowing process can be seen as involving. He remarks that the term *dialogue* itself is not uncontested and that, "with constant repetition, the meaning of dialogue can shift; conversations once absorbing in their spontaneity now become programmatic and subject to strategizing" (2015, p. 302).

As I pointed out in Chap. 1, various Indigenous authors have offered a conception of dialogue as including the sharing of views/stories, with an orientation toward defining workable courses of action, which at the same time reflect principles of social and ecological justice. Exactly what is at play in this process, is a matter for those involved in situ to work out (as Gergen also notes). But the principle, as Collins elucidates in explaining her view of the dialogical approach endorsed within "Black feminist" thinking, is to refrain from linking dialogue to adversarial debate. More important is to generate via the process of dialogue a "call and response discourse" between speakers and listeners—where specific expressions of concerns are provided for (2000, p. 261). She exhorts furthermore that those engaged in dialogue (in academia and in everyday discourses and in connection between them) should not try to divest emotions and moral concerns from the discussion, as might be advised by those who believe in "rational" dialogues

rooted in "the facts". We see here how this concurs with Gergen's lamenting of knowledge generation being too firmly rooted in supposed "facts" (as if in any case these can be observed in theory- and emotion-free fashion). Chilisa makes a similar point when she asks: "by whose standards is evidence appropriate or accurate?" (2007, p. 206).

I have chosen to concentrate in depth on Gergen's account of research as future forming as it expresses how in terms of relational thinking, one can legitimately tie "research" to consciously—and dialogically directed—efforts at "world shaping". Gergen does not offer detailed examples of how this might ensue, but suggests that his arguments can form a new direction for research, where the metaphor of research as *watching* is replaced by one of *shaping*.[4] He argues that the time has come for this: "It is time for the social sciences to channel their substantial resources of intelligence and ingenuity into creating more viable forms of living together" (2015, p. 307). Deetz adds to this suggestion when he makes the point that

> when we [as inventive generative theorists engaged with others] gave a compelling way to think and talk about the world, the impact was widespread long before we had enough research to support the implied claims and implications. (2008, p. 290)

In other words, it is *only in action* that the creative potential of the inventive theorizing becomes realized and in this sense "supported" as people develop what Gergen calls "more viable forms of living together". Kuntz likewise expresses that an activist stance (including an activist approach to research methodology) can "nudge previously stable (and normatively reproducing) systems into a state of disequilibrium, thereby intervening in the logical production of the future" (2015, p. 135). Kuntz asks:

> How are we to productively engage in methodological work that, in turn, aligns with an activist vision of methodological responsibility to activate some element of social change? What, in short, does it mean to be a responsible methodologist? (2015, p. 137)

What I have been doing in previous chapters is offering examples of how research can be undertaken in the spirit of a sense of research as future forming, where some responsibility is taken for contributing to world shaping. I now consider in more depth various features which I see as associated with what can be called responsible generative theorizing.

[4]Gergen speaks of the "watching, looking and seeing" metaphor. I have focused on the *watching* part of the metaphor, as this to me implies a specifically passive approach in contrast to the active approach tied to recognizing how knowing contributes to world-shaping. I believe that *seeing* can be linked to "imagining new visions" (which can be *forward looking*) and that *looking* can be linked to *re-looking* and *reviewing* (as the term re-search might imply), including being *forward looking*. Of course these are just words, but as Denzin aptly puts it (2001, p. 24), *words matter* in the way in which they can point to possibilities.

7.3 Theorizing Social Structure Toward Envisioning Possibilities for Reconstruction

In line with his view of the shaping effects of inquiries, Gergen offers an example of how choices of explanation as given by, for instance, social psychologists "may either sustain or alter the common attribution patterns of the culture [where people attribute causality] and thus the common patterns of blame and praise [in society]" (1978, p. 1349). I pointed out in Chap. 5, when discussing the study initiated by Stephens in Nigeria (2012), that Gergen refers to the case of theorists offering explanations for ghetto riots in the USA. He notes that to the extent that scientific explanation operates at the level of focusing on "society's oppression of the ghetto black [people] … such explanation shifts blame from the rioter to society" (1978, p. 1349). In other words, science has the capacity to "shift the attributed locus of causality for a given range of activity and, in doing so, alter common reactions to such activity" (1978, p. 1349). This I argued was borne in mind by Stephens in his attempts to point, with prisoners, to the dearth of opportunities in the Nigerian social context for employment, coupled with an emphasis placed on "materialism", which assigns status to people in terms of their accumulation of wealth (2012, p. 196). As part of his developing theoretical interpretations (based also on what emerged in discussion with participants) Stephens highlights the violence of a social system that thus impinges upon people's life chances and routes to dignity, along with his exploring the causal relations between the variables that he chose to locate in the study (as the experiment ensued with reference to these variables). Although the experiment was geared to "testing" the possible effects of training (given to recidivists in emotional intelligence and creative thinking) on the dependent variable of learning on the part of recidivists, Stephens also offers further inferences at the level of social structural reasoning (so as not to further stigmatize the prisoners, whom he argues are often stigmatized).

Stephens' mode of theorizing, where he pleads for "the need for the whole society to rise up" to the challenges that he elucidates (pers. comm., 18 November 2015), can be seen as an example of generative theorizing. I argued in Chap. 5 that while his experiment did indeed isolate a number of variables in order to consider whether the experimental conditions could be said to have affected the dependent variables, the study (and Stephen's theorizing, also in some conjunction with participants) did not end with a statement as to what had been "found" on this level alone. Stephens also tried to tie these "findings" to making conjectures, with transformative intent, at the level of theorizing less visible social forces.

In considering the value of such theorizing I side with those who suggest that theory-generation cannot concentrate only on what some authors, following Merton (1964), have called middle-range theory. As Turner explains, according to Merton, theories of the middle range "reveal clearly defined and operationalized concepts [defined with reference to their empirical indicators] which are incorporated into statements of covariance for a limited range of phenomena", and which are testable in relation to these phenomena (1991, p. 80). Such theories "are not highly abstract

or broad" (1991, p. 80). Merton has suggested that the focus on developing theories of the middle range is a way of linking theorizing with empirical findings (as required by scientific investigation), and in future possibly the theories of the middle range could be consolidated in a broader theory (Turner, 1991, p. 80). Turner indicates that he does not believe that this mode of proceeding yields "theoretical payoff", but he notes that "Merton's advocacy has developed a wide following" (1991, p. 80). Thus, he remarks:

> Sociology has spawned a large number of theories about such specific empirical processes as juvenile delinquency, family conflict, race relations, social mobility in America, urbanization and other empirical events. I am not sure if Merton intended theories of the middle range to be so empirically grounded, but this was the outcome of his advocacy (1991, p. 81)

Looking critically from a Marxist-oriented perspective at Merton's proposals for researchers to direct their attention toward middle-range theorizing, Layder suggests that researchers then run the risk of "underestimating the influence of structural components [of society]" (1993, p. 159). He suggests that Merton's insistence that theorizing needs to be logically linked to the realm of what can be "observed" in order for it to warrant being called scientific, precludes assigning analytic weight to structural components which arguably operate "behind the scenes" (1993, p. 29). He suggests that whether engaging in so-called quantitative or qualities inquiries, Merton's focus on tightly connecting theories to empirical observation means that one may lose a sense of the mechanisms in society considered as a totality which contribute to the formation of inequalities of life chances and associated power relations (1993, p. 29).[5]

Layder refers to the example of Marx's theoretical ideas on class inequalities in society. As Layder sees it, the Marxist-oriented focus on social structures and their historical development cannot be translated into observable portions of reality in the way that Merton's (1964) conception of middle range theory implies (1993, p. 27). In similar vein, Kuntz refers to the importance of developing methodological strategies that link "macro-level discursive patternings (say, globalized neoliberalism) with more micro-level and localized practices" (2015, p. 75). Kuntz goes on to say that as a researcher,

> my research must attend to the many ways in which the values of globalized neoliberalism give a sense of meaning to my everyday practices … even as my localized activities reinscribe (or, perhaps, are assumed within) the manufactured values that are neoliberal in

[5]Layder makes this argument in the context of critically examining Hammersley's reliance on Popper's (1959, 1969) arguments on the logic of scientific discovery to justify certain ethnographic forms of theorizing—see Romm (2010, pp. 274–276). Layder compares Hammersley's argument concerning theory-testing as the hallmark of science (including in ethnographic work) with that of Merton, noting that both follow a Popperian position regarding the empirical basis of science. Turner for his part (1998, p. 22) notes that Merton hoped that eventually the various middle range theories that had been established could be consolidated into a more general (grand?) theoretical scheme, but this scheme would still have to be rooted in "empirical findings" (to be deemed scientific in his terms).

> order. As such, I perhaps have a responsibility [as inquirer] to locate and problematize such connections. (2015, p. 75)

Kuntz argues that inquirers need to connect daily/everyday social practices to larger social processes while at the same time challenging "common sense" insofar as it works against "social justice work" (2015, p 134). He refers favorably to Willis's (1977) way of exploring class relations in his book *Learning to Labor*, in a manner which makes manifest how "collective mobility" is not possible in the present system, albeit that "individual-level" working class mobility may mean something (2015, p. 126). Unless the research analysis moves across scales to focus on "the collective", this becomes lost in vision. As Kuntz summarizes, "cutting one scale from another (often in the guise of technical specificity) is shown [up] from the materialist orientation [which he supports] as methodologically limiting and phenomenally simplistic" (2015, p. 128).[6]

In Sect. 7.4, I return to the question of how "social structures" might be inferred with reference to, but also imaginatively leaping from, the realm of so-called empirical observation. I indicate how retroductive logic can be used to postulate structures for the pragmatic purpose of seeking leverage points that can be used to try to alter the basic institutions and patterning of social life, which are theorized as being unduly constricting of (collective) wellbeing. This is what is also pleaded for within a transformative paradigm and further emphasized by Indigenous and Indigenous-oriented authors whose theorizing includes counter-narrative creation to shift dominant modes of social being. As indicated in my Table 1.2 in Chap. 1, transformative-directed researchers (following critical theoretical schools of thought) do not shy from a certain amount of what might be called "grand theorizing" which exceeds postpositivist-styled understandings of the logic of science. The point that they emphasize is that critical theorizing must be rooted in practically-oriented dialogues regarding options for empowering action. In Sect. 7.4. I offer more detail on how I believe this can be accomplished.

Related to this point, Gergen remarks that much social theorizing "justifies by implicit assumption the hierarchical organization of society; such theories, he suggests, support the continued oppression of certain classes by others" (1978, p. 1355). Gergen is not specific about what theories he has in mind, excepting that his discussion is set in the context of a critique of positivist/postpositivist modes of inquiry, where it is forgotten that, as he puts it, theories "may favor certain forms of conduct at the expense of potential alternatives. As its implications and applications are borne out, every theory becomes [wittingly or unwittingly] an ethical or ideological advocate" (1978, p. 1354). Now Gergen suggests that one of the implications of a theory built on the assumption of the necessary "hierarchical organization of society" is that this way of seeing society infiltrates into (and supports) certain

[6]As I indicated in Chap. 1, Footnote 18, Kuntz advocates for a "new materialist" position, which draws on various (including Marxist-oriented) theoretical traditions to make the case that "matter" (of whatever kind) is *in the process of being formed*, and hence always has productive capacity toward a "yet to become" future state of being (2015, p. 83).

common sense views that hierarchies are inevitable and that a more relational existence is a chimera.

As I intimated in Chap. 2, Sect. 2.7, what is called Social Dominance Theory (SDT), as originally developed by Sidanius and Pratto (1999), can serve to endorse a view of hierarchy as ubiquitous and hence incapable of radical revision. In Romm (2013) I point to the way in which theorists aiming to empirically test/advance/refine SDT by deducing hypotheses springing from the theory, often proceed in "middle-range" fashion. I point to the possible self-fulfilling effects of this mode of theorizing. Pratto, one of the authors of the initial book (with Sidanius, 1999) on SDT concedes with colleagues (2014, p. 93) that it is important to bear in mind the self-fulfilling effects of SD theorizing which I located (2013). One way to indeed take this into account, is for theorists to consider that any repetitive links that are seemingly ubiquitous, are capable of transformation once the underlying structures/mechanisms forming the social context are transformed. Patel makes a similar point with reference to a consideration of Essed's (1991) suggestions for addressing (in theory and practice), "everyday racism". He notes (by referring to his own study in South Africa, 2015) that when, for instance, "everyday racism" as explored by Essed is recognized to be a "product of wider social forces" (2015, p. 75), this makes it more possible for people to consider ways of "actively opposing" it (2015, p. iii). That is, the social mechanisms which arguably function to reproduce these everyday instances of racism (albeit not immediately observable) are recognized to be capable of being opposed.[7]

In Chap. 2, I outlined how the discussions generated in the focus group and follow-up conversations around "race" pointed to its being historically linked to cheap labor (or *class* in scholarly discourse), with racism linked to exploitation of labor (class domination). It could be said that we (all those involved, including myself) were together fashioning options for thinking how such nodes of domination could be refashioned in practice. During the discussions (in the various forums) there emerged a recognition/highlighting of the historically-created forms of domination arising from the links between racism as practiced and what would be understood in more scholarly discourse as capitalist bases of exploitation (and in non-jargonized discussions as use of cheap labor). The participants pointed out how their life chances were affected by these wider forces in society, and by implication they referred to possibilities for some alteration as far as their and others' lives were concerned, suggesting that the economic and social hierarchies could be shifted (see Chap. 2, Sects. 2.5 and 2.6).

Considering the examples that I introduced in other chapters, in Chap. 3 I proposed that the survey research undertaken within the comparative international project on inclusive education could be "twisted" in order to rescue/nurture transformative possibilities in as far as social reconstruction is concerned. Instead of confining the research to examining and reporting upon relationships between

[7]In Romm (2010, pp. 219–241) I explore in depth Essed's methodological approach. See also http://www.encyclopediaofafroeuropeanstudies.eu/encyclopedia/philomena-essed/.

observed (operationalized) variables and their manifestation in different countries, one could make space for *theorizing anew the apparently "observed" patterns of links between, say, teacher's self-efficacy and other variables that were isolated for study*. In this way, one could invoke what Lavia and Mahlomaholo (2012, p. 6) call the postcolonial imagination by redefining, say, the variable of "self-efficacy" and by reconstructing questions asked to participants (in this case teachers) in the questionnaires. This might then expose them to the possibility of understanding and practicing "self-efficacy" in less individualistic fashion than currently provided for in what could be said to be Western-oriented scales. Meanwhile, we (Nel, Tlale and myself) tried to make some provision for exercising such imaginative theorizing (in conjunction with participants) in further discussion in forums such as focus groups, and in meetings with key stakeholders (see Sect. 7.5.1).

I suggested further in Chap. 3 that in the national 500 schools project most of us involved in the questionnaire construction tried to be sensitive to how questionnaire design and interpretation of findings (combined with focus group discussions) could be geared to imaginatively reworking socially-sedimented constructions. This is also what Dan Tlale encouraged in his liaison with participants during his "intervention visit" to the school in Idutywa, in which interpretations of findings were explored, further discussed, and placed in a systemic context. I suggested in this regard that Dan, admittedly himself inspired by other researchers focusing on systemic barriers to learning, in turn inspired participants/co-inquirers to reconsider the systemic quality of problems which were manifested in their experiences in the classroom. These could be recognized to be indeed systemic (and acted upon accordingly through collective action). This would be in keeping with Layder's plea for theorizing to draw attention to social forces that otherwise might operate "behind the scenes".

In Chap. 4, I suggested that Woldegies brought to the project a concern to theorize inequalities affecting women's life chances, while reviewing them as historically revisable. He expressed this concern in the research by posing questions that brought to the fore possibilities for rethinking culturally-sedimented discrimination and socially-sedimented lack of institutional opportunities for women to participate in economic and political life (while exploring options for unseating such sedimentations in practice). The initial theoretical lens with which he came to the project—which he attributes to his exposure to feminist discourses and to theories such as Positive Deviance Theory and Asset-Based Theory—sensitized him to "think", in conjunction with participants, at the level of social structure. At the same time, he encouraged them to consider how dents in repetitive oppressive patterns at the level of everyday life could be accomplished for them. In this sense, he could be said to be, in Kuntz's terms (2015, p. 128) "moving across scales", and putting into practice a conception of "structure" as "open to future possibilities" (Kuntz, 2015, p. 25)— a conception that could be applicable for others in Ethiopia, and perhaps in other contexts too, to the extent that others are inspired by the cases (see Chap. 4, Sect. 4.2.6).

This does not mean to say that the participants in the research self-identified as "activist feminists" or that they used the jargon of "positive deviance" or "assets" to

communicate their experiences and hopes. But their thinking and practice at the level of imagining and practicing new forms of social relations can be said to have been activated during the inquiries. Grey (2003), citing Udel (2001) notes in this regard that "the widespread and strategic use of alternative descriptors [than feminism] amounts to an intentional separation by certain Indigenous women from both the theory and practice of [a supposed universally appreciated] feminism". Grey also wishes to underscore that one of the reasons why "feminist ideology" may not be universally applied is that many Indigenous women (and men) are inclined to act to *create/restore a balance between the genders*, so that their transformative actions are tied to this balance-creating project.[8]

One could argue that Woldegies too was sensitive to the local context as advised by Grey, in which indeed he, as with the female and male participants, was not trying to empower women at the expense of men, but was trying to nurture and activate male support as well as wider community support. As part of his own interpretation of the relevance of the results to other contexts, he puts forward that perhaps the women's ways of handling their empowerment could also serve as an inspiration in other contexts. If readers reading his text (or through oral communication via the women's movement and organizations supportive of women) consider that it resonates with, and extends, their possibilities for thinking and acting in their own experienced situations, the ideas can stimulate such empowerment.

Woldegies's theorizing bears some similarity to the intention of Ssali and Theobald as expressed in their article on options for health and social reconstruction in Northern Uganda (2016). They make mention of trying to deconstruct gender stereotypes, insofar as these can be said to influence life chances and are experienced as oppressive. Their gendered analyses of the life histories of the (female and male) participants is made with the hope of restoring/activating more balance in gender relations, as indeed in other social relations (such as between Acholi and others, who all have suffered war-time experiences). I have extrapolated from their text that they could be said to be performing, based on their engagement with the life histories as expressed by participants, a generative theorizing which is forward looking at the level of social reconstruction (although they themselves do not use the word "theory").

With reference to Chap. 5, as noted above, one can extrapolate from Stephen's (2012) account and from my personal communications with him (2015), how he too could be said to have been theorizing at the level of structure. This was by his locating various points where societal "blame" (to use Gergen's wording, 1978) can

[8]Grey (2003), also citing Sunseri (2000, p. 146), states that in many "traditional" societies it can be said that "Indigenous women were central to family, community, political, social and cultural expression; all of which operated on foundational principles of balance and consensus". Grey argues that even if gender relations were "not ideal" there were, "more nuanced gender relations" traditionally in such societies than can be understood using terms such as "patriarchy" as employed in the language of many non-Indigenous/White feminists. This is also an argument offered by Hoppers (2015), which I further explore in Romm (2017).

arguably be attributed, so that the recidivists' behavior (in this and perhaps in other contexts) can be reconsidered in this light. And Oczak and Niedźwieńska too (2007) can be said to be exhorting researchers in the Polish and other contexts (where they believe their results might also be considered relevant) to recognize that submissive behavior that might be repetitively observable via experiments, can be shifted via the same kinds of experiments, but now differently practiced. (See Chap. 5, Sect. 5.3.4.) In this way what Gergen (1978, p. 1354) calls "the stranglehold of what people accept as 'human nature'" is re-theorized.

In Chap. 6, I used three examples which all point to possibilities for re-envisaging structure as historically generated practices which are therefore open to intervention. In the case of McIntyre-Mills' work, she tried to render visible to the participants and stakeholders involved in the selected locality in South Australia that "business as usual" is a historically contingent way of "doing business". As part of setting up the study she drew on what she calls "new global narratives arising out of a cross-pollination of spiritual ideas from a range of religious and spiritual practices" (2014b, p. 7), in an effort to show up how the current dominant narratives in the global system have deleterious consequences for human and planetary wellbeing (see Chap. 6, Sect. 6.2.2). Following various Indigenous authors and elders with whom she has come into contact (and sought out for her learning) she makes the case that Western and dominant individualistic views of humans as able to live more or less isolated lives, may not even hold in the case of this relatively well-off suburb of South Australia. Based on the way that participants interacted with the software package, on further discussions with some participants and key stakeholders, and on certain critical systemic literature on which she draws she highlights possibilities for shifting, through action, "business as usual" in this and, by implication in other, contexts.

I noted/extrapolated in Chap. 6 that Roman-Alcalá (2015) offers his analyses/generative theorizing to radically re-envision ways of organizing property relations. In this case his theorizing is based on his conversations and collaborations with participants in the Occupy The Farm (OTF) action in California. His analysis of how in this case capitalist-oriented private property relations were set aside by participants and by many supporters in the community, suggests that it is not only in "the South" (with traditionally-recognized communal land tenure systems) but also in "the North" that the restructuring of property relations to cater better for food security becomes a possibility. He is careful not to purport to be offering any recipes for what needs to be done to create transformative shifts in different contexts of action, but again—as with the other authors whom I explored through the book—he hopes that his analyses can become inspirational and can function as what Gergen calls a "resource" for others pursuing (similar) collective actions. Gergen explains what he means by this. He refers to

> a form of research in which knowledge is acquired through the complex and creative process of constructing a successful practice (see also Hassan, 2014; and Kahane, 2012). When such knowledge is shared, it becomes a resource for others. (Gergen, 2015, p. 301)

Gergen suggests that the practices which spring from imaginative generative theorizing and action can be said to "provide alternatives from which one can select as best fits local needs" (2015, p. 301).

As my last example in Chap. 6, I referred to how Maathai (2006) through her autobiography points, inter alia, to the colonial history in Kenya that she considers as connected with the desertification of the land. Through her narration she explores this as being a product of history—as she herself experienced through her life and as other women's stories bear testament to (see Chap. 6, Sect. 6.4.2). In this way she relates her own and others' life experiences and their experienced poverty to her interpretation of wider social forces. Her theorizing, in dialogue with other researchers and with other actors in her narrative, around what otherwise might have been less obvious or hidden "behind the scenes", now becomes seen/located as a basis for remedial action. She suddenly recognizes that planting trees might turn around some of the social and ecological effects that have been experienced by herself and others as caused by the historical process. She proceeds, also in collaboration with others, to try out this potential "solution" in practice.

Her autobiographical account is set in this context, but does not preclude—and indeed invites—people in other contexts to be inspired by her thinking and by her learning in action, to in turn draw relevant lessons as they engage with her work. She notes that she herself was inspired by her attendance of a six-week course on leadership at Pittsburgh, where her course paper was on "helping women in rural areas work together and promote developmental efforts". She states: "little did I know that I would be putting theory into practice only a decade later, when I would be inspired by rural women to initiate the GBM" (2006, p. 92). That is, others' thinking and action served as a resource for her; and she hopes that likewise her (autobiographical) work might constitute a resource for others to draw on.

The idea in generative theorizing is not for anybody to treat the theorizing as dominating/restricting their options for thinking and acting, but as offering a resource (Gergen, 2015, p. 307). This is also in accord with Knudson's (2015) summary of how "Indigenous knowledge systems are highly contextualized and emphasize story, local knowledge, and the experiential" (2015, para 1). This is so that space is at the same time made in the "knowledge systems" (or theorizing) for people to, as Collins puts it, contribute "missing parts to the other writer's story" (2000, p. 38).

7.4 Retroductive/Abductive Logic as a Resource for Generative Theorizing

One of the features of generative theory to which Gergen points is its being based on what he calls "logics of reflective pragmatism" (2015, p. 287). He suggests that in terms of such "logics", what is invited is a "further exercise of the imagination, extending and expanding the vision of inquiry". Although he offers some intimation

of how he conceives that research can be inventive and creative (see Sect. 7.2), he does not focus on the question of how the logic might be characterized. Nevertheless, he does contend that the logic of inquiry in a "future-forming science" is a logic based on what he calls is a "relative independence from what might otherwise be considered 'established fact'" (2015, p. 304). He also indicates that he subscribes to a form of pragmatism (pragmatism with a conscience) in his account of the future-forming potential of social inquiry. But he does not offer detail on the logic(s) which might be used in the process of pursuing an agenda of pragmatism with a conscience.

In order to proceed in this section, I start off with an interpretation of Peirce's identification of what he calls abductive or retroductive logic, which is linked to his particular pragmatic approach. Chiasson (2001, p. 1) remarks that Peirce's use of the different words *abduction* and *retroduction* seems to point to different foci; but Paavola (2004, p. 4) suggests that we can consider that Peirce used these terms interchangeably (to point to a form of logical inference distinct from induction and deduction). In my discussion I do not try to distinguish the two as the purpose of my deliberations is to draw out how Peirce's pragmatism invokes a different logic from induction and deduction.

As Mingers notes (2000, p. 1258), "Peirce developed a pragmatist theory of meaning such that the meaning of a concept was specified purely in terms of the actual practical effects that it would have". And as Joas too summarizes Peirce's model of pragmatism, "the semantics of pragmatism locates the meaning of concepts in the practical consequences for action resulting from their use" (Joas, 1994, p. 506). But how did Peirce see the logic involved in such sense-making/theorizing? There are of course many interpretations of Peirce's conception of the logic of inquiry supporting his pragmatic approach. What can be said, for the purposes of this discussion, is that in the first place Peirce draws a distinction between inductive, deductive and abductive or retroductive logic. For Peirce, following many philosophers of science, the logic of *induction* is seen as proceeding by making inferences from the particular to the more general (i.e., from observing empirical instances to making more general statements), while *deduction* works by deducing implications of more general statements for more particular occurrences (so that statements can be tested). Peirce now adds that the form of logical inference that he calls *abductive* or *retroductive* reasoning can and must operate over and above induction and deduction to "*open up new ground*" in processes of theorizing (1911, p. 2, my italics).

Peirce affirms that he regards "retroduction … to be the most important kind of reasoning, notwithstanding its very unreliable nature, because it is the only kind of reasoning that opens up new ground" (1911, p. 2). He defines retroduction as a type of logical inference that allows us to proceed from

> finding ourselves confronted by a state of things that, taken by itself seems almost or quite incomprehensible … to supposing that perhaps there is … another definite state of things … which would shed a light of reason upon that state of facts with which we are confronted, rendering it comprehensible. (1911, p. 2)

That is, by using retroductive logic, states of affairs (as observed/experienced) can be made sense of (rendered comprehensible) by inferring other "states of things". He remarks that the "characteristic formula" of the type of inference that he calls retroduction is that it involves reasoning from a *consequent* (observed/experienced phenomena that confront us) to an *antecedent* (i.e., a posited state of things that helps us to render comprehensible the observed phenomena). Or, as he otherwise puts it, retroductive logic can be considered as "regressing from a consequent to a hypothetical antecedent" (1911, p. 4).

Peirce's speaking of "states of facts" in the above quotations might imply that he considers that certain "facts" can be grasped independently of human meaning-making activities. And his reference to "states of things" which can be retroductively inferred (albeit as a conjecture) might imply that knowers should try to seek access to such "antecedents" which explain observables (via the use of retroductive logic). Some authors—such as Marsden (1998, p. 299)—have suggested that Peirce's logic is therefore consistent with a realist epistemological position, where the quest of inquirers is to set out to grasp as best as possible the hidden structures which give rise to the "consequents" which are observed. This is how Marsden explicates the structure of the logic as consistent with a realist perspective:

(1) Some surprising phenomena, P1, 2, 3 are observed.
(2) P1, 2, 3 would be explicable if H [a hypothesis about an unobservable state of affairs] were true.
(3) Hence, there is reason to think that H is true. (Marsden, 1998, p. 299)

Marsden indicates that in this formulation, although there is no logical necessity between P and H, conclusions about realities (in this case unobservable states of affairs) can be made, if only "problematically or conjecturally" (1998, p. 299). Marsden elaborates on this logic as part of his suggestion that Marx adhered to this manner of practicing social science, and that his work in *Capital* (1965) should be read and understood in the light hereof (1998, p. 299). Subsequent to this, many self-named critical realists (e.g., Bati, 2015; Bhaskar, 2008; De Souza, 2014; Kazi, 2003; Layder, 1993; Mingers, 2000; Pawson & Tilley, 1997; Sayer, 1999) have likewise argued that the logic of retroduction supports a critical realist position, in which hidden structures can and should be theorized by social scientists.[9]

But it can also be argued that Peirce's presentation of retroduction as a mode of inference implies, as Bendassolli (2013) suggests, that

> the choice of a theory may have nothing to do with the truth or the theory's approximation to the essential facts, but rather with its capacity to help us solve problems of practical interest. Therefore, the aim of a theory would not be "pegged" to the world, but would be designed to help us represent the world in aspects relevant to a proposed transformation of part of it. … This argument depends on the premise that we can never come to know the true nature of the world due to the existence of unobservable entities. (2013, para 22)

[9]This is normally tied to an argument to the effect that the logic of retroduction properly guides the study of both natural and social reality (see Romm, 2001a, p. 4).

Bendassolli suggests that just because retroductive logic posits that there is "no way to guarantee an essential, definitive connection between theory and any particular facts and [posited] properties of the world", realist interpretations of the use of retroductive logic face the problem of how to adjudicate between different theories which all arguably could be plausible (i.e., which all can make sense of apparently observed "facts", and which indeed all may see these "facts" differently in the first place). As Bendassolli notes, "the same phenomenon can be legitimately explained in different ways, using distinct theories and theoretical models" (2013, para 21). One solution to the problem of undetermination (the "problem" that theories can neither be induced nor deduced with reference to empirical observation), he notes, is to assume that theories have a "pragmatic value" (as in pragmatic epistemologies) (2013, para 21). This would mean then that theoretical webs, which order and re-order relationships among a number of key concepts, become judged in terms of how well they organize experience for the purposes of action (para 22). And as Gergen suggests, it is in choices of moral action (with others) that decisions around how to theorize the world so as to render it more "just" and develop "more viable forms of living", become created (2015, p. 307).

In Sect. 7.5.1, I return to the question of how a pragmatic account can accommodate the recognition of a plurality of ways of observing and theorizing (where observation is itself not seen as theory- or value-neutral). For now, I wish to introduce into the discussion how some other authors understand the manner in which retroduction exceeds hypothetico-deductive logic—the logic which is assumed in positivist and postpositivist discourses to be the "logic of science" (see Column 1 of Table 1.2, in Chap. 1). Following this, I refer to how some authors argue that retroductive logic exceeds qualitatively-inspired "induction", which is often assumed to be the logic governing qualitatively "rich" interpretations of people's life experiences—see Table 1.2, column 2, in Chap. 1.

Meyer and Lunnay (2013) compare abductive inference (which I mentioned earlier I do not try to distinguish from retroduction) with deductive logic, as follows:

> It is through abduction that new ideas are introduced …. When using deductive inference, the theory is proved or disproved. However, findings that are outside of the initial theoretical premise may remain unanalyzed. Fundamentally, abduction is a means of forming associations that enable the researcher to discern relations and connections that are not otherwise evident or obvious. This allows the researcher to formulate new ideas, think of something in a different context, and to "see something else" (Danermark et al., 1997; Meyer & Lunnay, 2013, para 2.5).

For instance, we saw in Chap. 3 (where I referred to examples of survey research) and 5 (where I referred to examples of experiments) how the generative theorizing that I argued was at play—or could be extrapolated to be at play—was not based on trying merely to *confirm*, *disconfirm,* or *modify our understanding of*, the relations between the variables isolated for attention. The idea was not to deductively connect the "findings" with initial theoretical premises concerning the links between the variables, but to "see something else", or rather I would say *infer something else* via the theorizing. Meyer and Lunnay suggest, citing Collins (1985), that just

because abduction is not logically rigorous like deduction, it can be used to "discover circumstances and structures … that are obscured in empirical data" (2013, para 2.6). This is why such logic can be directed to theorizing (more or less invisible) structure, as I noted in Sect. 7.3.

Meyer and Lunnay still, however, use the *watching* metaphor criticized by Gergen, rather than the *world-shaping* metaphor in their understanding of the value of abductive (and retroductive) logic. Nonetheless, they also cite Habermas (1972), whom they argue "suggests that abduction is a mode of inference used to broaden knowledge and stimulate the research process" (Meyer & Lunnay, 2013, para 2.5). I have argued elsewhere (1991, pp. 132–140, 2001a, pp. 65–68, 2001b, pp. 141–151) that Habermas's epistemological position can be characterized as nonrealist (and indeed as including a pragmatic dimension) in that he sees the function of theorizing as part of a social reconstructive project (Habermas, 1974, 1984, 1996). The "broadening of knowledge" in such an epistemological context is in the service of, and gains its validity through, its contribution to the project of social reconstruction. But in any case, Meyer and Lunnay draw attention to how abductive inference can move beyond the terms of the "initial theoretical premise" (2013, para 2.5) and in this sense enables more creative thought and imagination.

Considering the logic of retroduction, they define it (not dissimilarly to abduction on the creative score) as enabling "the researcher [to] move between knowledge and observable events, recognizing that knowledge cannot be reduced to observable events" (para 2.10). They point to the inferential process of transcending what is regarded as the realm of observation/experience. They suggest that "the term retroduction is an instinctive mode of inference" (2013, para 2.10). Considering the manner in which qualitatively-directed inquiries proceed, they make the point that many researchers writing about qualitative research find it "difficult to explain the processes of how qualitative analysis is actually conducted", and they suggest that an appeal to retroduction can be helpful in this regard—whether or not the research is "in line with critical realism" 2013, (para 2.10). They point out that in qualitative analysis "it is difficult to describe the process of developing an idea" (2013, para 2.10).

7.4.1 Abductively Creating Analyses in Grounded Theory

In her account of the process of creating qualitative analyses through what is called "grounded theory", Charmaz refers to abduction as being an important tool for grounded theorists (cf. Puddephatt & Charmaz, 2006, p. 15; Charmaz, 2009, pp. 137–138).[10] Strauss and Corbin for their part, in offering their overview of

[10]Clarke explains that "the very term 'grounded theory' means data-grounded theorizing …. The theorizing is "generated by tacking back and forth between the … specificities of empirical data and more abstract ways of thinking about them" (2009, p. 214). Like Charmaz (and others), she argues that this process is "called abductive reasoning" (2009, p. 214).

grounded theory methodology (as first set out by Glaser and Strauss, 1967), define grounded theory as being "a *general methodology* for developing theory that is grounded in [qualitative] data systematically gathered and analyzed" (1994, p. 272). They indicate that what this means in practice is subject to some "invention" on the part of grounded theory practitioners. They emphasize, though, that the defining feature of grounded theorizing is *not* that it is built primarily on induction—in this regard they suggest that Glaser and Strauss (1967) "overplayed the inductive aspects" (1994, p. 277).

When summarizing how she conceives grounded theory, Charmaz states that "grounded theory is a systematic method of analyzing and collecting data to develop middle-range theories" (2012, p. 2). She adds that "this method begins but does not end with inductive inquiry" (2012, p. 2). In considering the historical context of the emergence of grounded theory, she points out that "it served to legitimize inductive qualitative inquiry at a time that it was losing ground in the United States. Quantitative researchers saw qualitative research as idiosyncratic, impressionistic, unsystematic, biased, and impossible to replicate" (2012, p. 3). As far as theory development via the "grounded theory" method is concerned, she points out that

> grounded theorists' claims to constructing theory might be a little over-stated. Nonetheless, using grounded theory strategies fosters giving your work an *analytic edge*. What constitutes theory has neither been agreed upon nor codified. (2012, p. 3)

She suggests that the grounded theory method of collecting and analyzing data can be adopted by

> researchers who hold different theoretical perspectives, focus on various levels of analysis, pursue varied objectives, and address diverse areas—including social justice research, policy analyses, organizational studies, societal issues—and social psychology. (2012, pp. 2–3)

In her exposition above, although Charmaz considers that grounded theory can focus on different levels of analysis, she also purports that it is aimed at developing theories of the "middle range" (2012, p. 2). Whether grounded theorizing is able to develop generative theorizing of the kind endorsed by, for example, Gergen (2015) and Kuntz (2015), where social structure is theorized with a view to stimulating possibilities for reconstruction, is a matter that still needs to be considered. What I would suggest is that because Charmaz views abduction as being the logic that is used by grounded theorists, she makes room for the kinds of imaginative leaps not provided for in Merton's (1964) approach to middle range theorizing, which is more tightly tied to what is supposedly observed (as noted by Layder, 1993).

Charmaz suggests that grounded theorizing can be seen to involve a logic of abduction, more or less as defined by Peirce in his writings. She states that this abductive process involves a certain "theoretical agnosticism" in relation to the theoretical lenses that theorists bring (a priori) to their studies (2006), as they go about looking for new "interesting" categories and findings. During her interview with Puddephatt she explains:

> So, the notion of "theoretical agnosticism" is quite good, because you go in with that doubt, and you look for some kind of interesting category or finding, which is consistent with abduction, or abductive reasoning. (2006, p. 15)

In her discussion of the construction of grounded theories, she refers explicitly to Peirce's account of this logic:

> C. S. Peirce's (1935–1958) original conception of abductive inference rested on considering all plausible theoretical explanations for a scientist's surprising finding while conducting inductive research [with close reference to an examination of the "data"] …. Hence researchers form hypotheses for each possible explanation, and test these hypotheses empirically by [further] examining data to arrive at the most plausible explanation. (2012, p. 11)

She considers that the grounded theory approach draws on the abductive logic of examining "surprising" data from a range of perspectives, until categories which seem to offer the most plausible explanation emerge. She further explicates that in the grounded theory approach this implies a process of "gathering data to fill out the properties of a tentative category …. You keep gathering data until no new properties of your categories emerge" (Thornberg & Charmaz, 2011, as cited in Charmaz, 2012, p. 11).

According to her, this logic of inference enables grounded theorists to proceed to locate participants' subjective and collective experiences in "larger structures" that may not be immediately visible (2005, p. 508).[11] She argues therefore that grounded theory can enable the pursuance of a social justice agenda (either by researchers actively advocating this themselves or by inspiring other actors). She summarizes how this can be achieved:

> The critical stance in social justice combined with the analytic focus of grounded theory broadens and sharpens the scope of inquiry. Such efforts locate subjective and collective experience in larger structures and increase understanding of how these structures work (see also Clarke, 2003, 2004; Maines, 2000, 2003). Grounded theory can supply analytic tools to move social justice studies beyond description, while keeping them anchored in their respective empirical worlds. Not only are justice and injustice abstract concepts, they are, moreover, *enacted processes*, made real through actions performed again and again. Grounded theorists can offer integrated theoretical statements about the conditions under which injustice or justice develops, changes, or continues. (2005, p. 508)

Here she suggests that it is the inferential leaps of grounded theorizing that enables theorists to legitimately offer theoretical statements which can increase understanding of how structures work (and therefore how they might be altered). In this understanding of social structures, she invokes what Troncale (2004–2007) would call a conception of structure as "slow process" (see Chap. 1, Sect. 1.3.2). As she sees it, "social structure" is constituted "through [processes of] actions

[11]Strauss and Corbin likewise suggest that analyses can operate "at every level of scale from the most "macro" to the most "micro" (1994, p. 275). What I am proposing (following authors such as Gergen, 2015; Kuntz, 2015; Meyer & Lunnay, 2013) is that moving between scales also involves some imaginative leaps in order to point to the potential of (theorized) structures to become radically reconstructed.

performed again and again" (2005, p. 208). She suggests that grounded theorizing can offer insight into the structuring of social life as it becomes constituted.

It is important to note that Charmaz (2000, 2008) distances herself from an objectivist approach to grounded theory, which she understands Glaser (1978, 1998) as endorsing (2008, p. 400).[12] She states that her constructivist understanding of the grounded theory approach specifically recognizes that research involves an interaction between researchers and participants and that "the researcher and researched coconstruct the data" (2008, p. 402). She emphasizes that "researchers are part of the research situation, and their positions, privileges, perspectives, and interactions affect it" (2008, p. 402).

In terms of a constructivist stance, she forwards her way of conceiving the epistemological assumptions of such grounded theorizing, by emphasizing that

> what observers see and hear depends upon their prior interpretative frames, biographies, and interests as well as the research context, their relationships with research participants, concrete field experiences, and modes of generating and recording empirical materials. No qualitative method rests on pure induction. The questions we ask of the empirical world frame what we know of it. (2005, p. 509)

Johnson and Duberley's interpretation of her position is that she believes that grounded theorizing as developed via abductive logic can venture to exceed participants' initial understandings as expressed by them, but should not be proffered as if the "interpretive readings" are theoretically- or value-neutral. They explain (2015, p. 769) that she endorses "situational analysis of 'unstated or unrecognized influences'" (more or less invisible to the participants[13]), but would not be suggesting that researchers should present their theorizing as offering more or less objective accounts of the (theorized) influences/social forces. I have focused on the example of Charmaz's grounded theory approach in this book to make the point that in Charmaz's view (as interpreted here) the process of constructing grounded theories is recognized to be already imbued with theoretical lenses, which, however, she notes, can and should be reviewed as the research proceeds.

---

[12]Glaser (2002) in turn argues that it is "too simple" for her to suggest that she is adding another vision for future qualitative research, namely constructivist grounded theory [GT] (2002, para 7). Glaser believes that one can try within a grounded theory approach to minimize the constructed character of "data". For example, he suggests that "with the passive, non-structured interviewing or listening of the GT interview-observation method, constructivism is held to a minimum" (para 10). This contention of his (and efforts to desist from admitting that data involves an interaction between researchers and participants) would not be accepted by constructivist-oriented researchers.

[13]Clarke (2005, 2009) points out that through situational analysis one can explore various conditions of the situation which are "in the situation" (in the sense that they are felt as consequential). Such conditions include: discursive constructions of actions; organizational/institutional elements; major contested issues; local to global elements; symbolic elements; popular and other discourses; various empirical elements; spatial and temporal elements; human elements (individual and collective); nonhuman elements; and political economic elements (2009, p. 209). She states that the fundamental assumption of situated analysis is that "everything in the situation both constituted and affects almost everything else in the situation in some way(s)" (2009, p. 209).

In the examples that I used in earlier chapters, I tried to illustrate how this reviewing process can operate by (professional) researchers acting as research facilitators (to use Onwuegbuzie and Frels' terminology, 2015, p. 162) so as to *organize the strengthening of reflective meaning-making on the part of participants and themselves*. This means that research inquiry, practiced as a process of facilitating people's re-reflecting on experiences and views, *extends the initial constructions of participants (as participant researchers) as well as of the "research facilitators"*. Furthermore, I suggested that when researchers (anyone involved in systematic inquiry) operate with a pragmatic intent—or as what Gergen calls pragmatism with a conscience—they will *try to stimulate re-rethinking* in the direction of exploring options for activating more social (and often more ecological) justice. Seen in this light, grounded theorizing can move in the direction of generating modes of thinking and acting which are linked to transformative-oriented research remits. And indeed seen in this light any research which includes forms of co-inquiry with participants (and stakeholders) in future-forming research enterprises can learn from, but also extend, the abductive approach to theorizing as offered by Charmaz and other proponents of grounded theory.

To operate in this way, I suggest that some reworking of Charmaz's (and other grounded theorists') account of "theoretical sampling" may be needed (as I elaborate in Sect. 7.4.1.1.). Charmaz indicates that theoretical sampling as understood in the grounded theory approach is distinct from (initial) sampling which is aimed at "identifying initial research participants, documents, or field sites to study" (2012, p. 11). In theoretical sampling, which according to those propounding grounded theory should take place as the research proceeds, the focus is on trying to "saturate the *properties* of your *category*" in order to ground and strengthen the categories which are emerging as theoretically important as the research continues (2012, p. 11). The idea is not to continue asking the same kinds of questions in the collection of "data" (thus saturating "the data"), but now on the basis of newly developing theoretical ideas, to gather (and re-examine) data with a view to strengthening new theoretical understandings. In other words, the purpose of theoretical sampling is to sample "for development of a theoretical category" (2012, p. 3). Charmaz points out that "theoretical sampling encourages you to ask increasingly focused questions and seek answers as you progress through inquiry" (2012, p. 11). Theoretical categories which may have formed part of the theorist's initial theoretical lenses can thus be rethought as the research progresses.

This account would be in line with Meyer and Lunnay's point that abductive inference (which Charmaz ties to theoretical sampling), means that "findings that are outside of the initial theoretical premise" can become "analyzed" (2013, para 2.5). Or, as Clarke states, the theoretical premises/understandings become further developed as new theoretical explanations are explored in relation to "fresh data sources" that are sought out as research directions become clarified (2009, p. 211). But my concern is that theoretical sampling understood in this way puts the focus on developing the *theory*, rather than on sampling (and theorizing in situ) with a view to reviewing possibilities for action in dialogue with others. I therefore suggest

that we need to shift the grounded theorizing notion and practice of theoretical sampling toward a more active approach to sampling, as discussed in Sect. 7.4.1.1.

7.4.1.1 From "Theoretical" to "Active" Sampling: Extending Grounded Theorizing Toward a Transformative-Oriented Agenda

As I illustrated through my discussion in Chaps. 2–6, instead of putting the focus on asking questions so as to arrive at the most plausible explanations of perceived data (as Charmaz advises, 2012, p. 11), in a transformative- or pragmatically-oriented research approach one can *ask questions with a different intent. One can locate key participants and stakeholders and ask questions that are likely to be helpful and relevant to participants and others in the community so that as Mertens states* (2011, p. 196), the community and ideally the wider society becomes better off than before the research began. I would propose then that the "theoretical sampling" as conceived within grounded theory can be supplemented (or indeed shifted) by what Rivombo (2016), extending Holstein and Gubrium's suggestions (1995, p. 74), considers to be involved in "*active sampling*".

Active sampling as Rivombo understands it, supports research as an "edifying" process for the initiating researchers, research participants, and wider stakeholders, who become participants as the research proceeds (2016, pp. 41–42). In his article *Using active interviewing combined with lesson observation to engage adult educators in reflection around addressing diversity in Soshanguve, South Africa* (2016), he explains how he conceived the sampling process so that initially 16 adult educators could engage with him in reflection around addressing diversity challenges in their classes in Soshanguve, South Africa. The purpose was to develop their—and his—reflections around ways of addressing diversity challenges. The sampling process included follow up interviews with certain of the educators toward the same end (of enhancing reflection). In addition, he further actively sampled three adult education training specialists (curriculum specialists) in order to review with them the curriculum in the light of, and in discussion around, the "findings". The sampling thus went hand in hand with an intent to stimulate reflections of key participants and certain stakeholders. The reflections were also geared to considering leverage points for new forms of action on the part of the adult educator participants and the curriculum specialists.

This approach is consistent with White's point that the research facilitator can guide the participants to analyze the "strategic situation" so as to "guide learning" (2003, p. 138). White points out that this, in practice, "is a very different approach from exploring the data for themes or other more in-depth analysis such as grounded theory" (2003, p. 138). The approach is intentionally more action oriented toward participant and stakeholder learning with a view to strategizing/acting. Vannini and Gladue offer a similar (implicit) critique of a grounded theory approach where participants' stories/experiences are treated as "an empirical means to a [purely] theoretical end" (2008, p. 147).

Now I would argue/extrapolate that the sampling approach used in all the examples to which I referred earlier (Chaps. 2–6) can be seen to have carried a more pragmatic/transformative intent than in a grounded theory approach where sampling is geared to supporting what Vannini and Gladue call "theoretical ends". In this sense the sampling can be regarded as having been (more) *active sampling*.

In Chap. 2, I chose the sample to include Black and some White participants so as to engender a discussion around a somewhat "taboo" subject, at least cross-racially. This was so that an opportunity could be created for people together re-reflecting upon the meaning(s) of race and racism. The sampling of these participants had the intent of being "edifying" (to use Rivombo's terminology, 2016, p. 41) in that participants could feel that they were learning through involvement in the process, and from their reflecting together around otherwise undiscussed topics. In further one-to-one and group conversations/discussions, these served to enrich the earlier discussions, in what Lincoln and Guba (1985) call a more prolonged engagement, so as to better delve into feelings, experiences and thoughts. Onwuegbuzie and Leech (2007, p. 107) indicate that sampling in qualitative research means, inter alia, that "a sufficient number of words" of participants are enabled to be expressed, so that the "sample of words" can do justice to participants' understandings (2007, p. 109). They advise that "researchers should pay attention to both the length and number of interviews and focus groups … and should provide a rationale for decisions made regarding the length and number" (2007, p. 108). Kuntz adds the caution, though, that we must be careful of applying an "extractive" logic which considers "voices" of participants as expressed in particular contexts (including contexts of interaction with research facilitators and other participants) as somehow being "there" to "search for and retrieve" (2015, p. 57). He is wary of processes which attempt to "produce voice" and then "interpret it for the consumer" (the audiences of the research). That is, he is wary of "creating an administratively possible voice so that it might become an object for our analysis" (2015, p. 56). Instead, research can be done "in order to make new realities possible—new possibilities for being, knowing, and inquiring" (2015, p. 57). As I indicated in Chap. 2, when discussing focus group research possibilities, Onwuegbuzie and Frels (2015, p. 162) admit that "research facilitators" would do well to treat participants' expressions as part of a relational context, a context which can be geared to exploring what Kuntz would call "new possibilities for being" (see Chap. 2, Sect. 2.7).

I would suggest that one of the rationales which is relevant for the research in Chap. 2, could be that the research was organized—in conjunction with participants' input concerning the emergent research "design"—to enable reflections not normally undertaken. In the case of this research, the participants decided how (and whether) they would contribute in the focus group meeting, and they suggested further options such as the three-personed group encounter (with me continuing to act as facilitator) in one case. The one-to-one and three-personed encounters were also used by participants as an avenue to further discuss and explore manifestations of raced (and classed) relationships.

In Chap. 3, the sampling process in the questionnaire component of the international project followed conventional practices of sampling in survey research so as to be able to confidently make statistically acceptable inferences about connections between variables that had been isolated for attention. However, the samples that we (the South African team) actively chose for the focus group discussions were purposively selected in township areas, so that, insofar as the research could serve communities, these communities could be singled out. Again (as in Chap. 2), it was the participants in the focus groups who found a way of inviting us to "carry the baton" for them (so that the research might have some meaningful impact on their lives) and we took responsibility in turn for arranging the meeting with district officers and head office (in a process that Rivombo would call "active sampling" which is continuous as more engagement with participants/stakeholders takes place). In the national project, we used a similar approach to our sampling of underperforming schools. Furthermore, we organized some intervention visits in selected schools, where focus group sessions had been conducted. In these visits, the participants, who themselves chose also to invite additional stakeholders, could reconsider, with the initiating researchers, options for action. In this way the sampling again became active, with participants and the initiating researchers widening the circle of participants/stakeholders so that the research could have increased "impact" value.

In Chap. 4, Woldegies selected purposively for the study a sample of women whom he considered to represent expressions of "positive deviance" in that they had succeeded in their enterprises, somewhat contrary to conventional societal expectations. This sampling was with the active intent that their positive role model stories could be discussed with them and could later be circulated in the community. He also chose to invite women and supporting family members to focus group discussions so that family and community support could be further theorized and acted upon. From Woldegies's involvement with the Amhara women and supporting family members, it seems that the concept of "positive deviance"—as explicated in Positive Deviance theory—could be re-theorized on the basis of the interviews and focus group discussions (though not necessarily using this jargonized language). The theorizing, where Woldegies admittedly brought initial theories (such as Positive Deviance and Asset Based Community Development theories) into his "thinking with" participants, became inflected as options were explored for supporting family members and community organizations to provide further support to this "deviance". The theorizing amounted to rethinking the social relations between women and men in the region (and how they could function to support positive deviance on the part of women). At the same time, options for poverty alleviation were also explored at family, community and regional level (with the further consequence that some environmentally-friendly solutions were also explored by the now more empowered women).

The sampling process in Ssali and Theobald's study (2016) can be argued to have been organized with the active intent of being able to create credible policy briefs for the policy makers whom they intended to target in the health reconstruction process. The fact that they interviewed 47 participants (men and women)

meant for them (from their understanding of the political context) that the "results" would likely be taken seriously, also by virtue of their links with the ReBUILD Consortium (2016, p. 95). Furthermore, they chose to sample men *and* women so that the gendered character of war-time experiences could be richly explored, but also so that stereotypes could be unseated via the research. In this sense again one could say that their sampling was "active".

In Chap. 5, Stephens' (2012) sampling consisted of using a random number table to select recidivists from a list supplied, and thereafter seeking volunteers to become involved in the experiment on the understanding that they would be exposed to some creativity and/or emotional intelligence training and/or HIV and AIDS training. In all cases of training, Stephens was hoping that participants' involvement would have positive impact on them. His intent was not merely to contribute to theories of learning, but to make a difference to the experienced lives of the participants (as well as to ways of viewing the potentials of these, and other, prisoners).

It is important to highlight here that I do not consider that because Stephens was setting out to "test" hypotheses via his experiment, the research thereby can become classed (as many researchers classing experiments and other "quantitative" forms of research do) as *theory-testing* rather than *theory-generating*. I believe that the distinction of *theory testing* versus *theory generation* to characterize research efforts may be unhelpful—and indeed can have deleterious consequences: If those undertaking more "quantitatively" directed research are classed (and class themselves) as concerned primarily *with theory-testing*, they might then not take responsibility for considering the opening of spaces for contributing to the lives of the participants and others concerned, and for theorizing around this. But instead, we can put the focus on the choices that can be made by experimenters in conducting experiments not only as a *testing of theory* exercise but an exercise *in trying to facilitate the construction of what might be experienced as constructive transformation to the benefit of vulnerable and marginalized participants in the social order, and generatively theorizing around this endeavor.* In Chap. 9, I continue this line of thinking when re-examining Stephens' experiment from the viewpoint of it not only being an exercise in "quantitative" research as traditionally understood.

I would suggest that Oczak and Niedźwieńska's (2007) sampling as I outlined it in Chap. 5 was also done with the active intent of developing an engagement that could be beneficial to those who volunteered to participate in the experimental procedure. They could be said to have developed some form of "prolonged engagement" with participants via the new debriefing procedure, which was directed to both exploring and activating learning on the part of participants. In line with their active intent, they urge researchers undertaking "persuasion" experiments and indeed other experiments, to consider the potential value of the active debriefing procedure. The generalization of results from the sample is partly by way of a plea to those conducting experimental research (especially "suggestibility studies") to continue to explore this potential in different experimental contexts.

The sampling processes in the examples in Chap. 6 all clearly (in my view) were directed by an active intent. McIntyre-Mills was hoping not only to enrich

theoretical categories as the research proceeded, but to work with participants (including the SALGA local government) to reconsider how the scenario of "business as usual" may be turned around in practice, toward enhanced community and planetary wellbeing. Roman-Alcalá's sample of participants comprised those who were actively involved in trying to rethink (and broaden the community's perceptions of) possibilities for re-organizing property relations. And Maathai's "sample" consisted of the participants in her story, who all—together with her—were involved in rethinking the options available in their situations (experienced as impoverishing) with a view to action.

The examples that I used in these chapters can be regarded as being instances of research which is grounded in a dialogue between (professional) researchers and others involved in the inquiries, where all co-inquirers can begin to re-imagine possibilities for thinking and acting. This means that the purpose of the theorizing being developed in the research would not necessarily be to try to arrive at what Charmaz (2012, p. 11) calls "the most plausible explanation" (of issues arising in the process of research). The more pragmatic purpose would be to stimulate a process of *exploring different leverage points where opportunities can be sought for what is taken to be desired transformative action*.

7.4.2 *Abductive Inferencing as Potentially Linked to a Transformative Research Agenda*

In the discussion above I have drawn a link between "grounded theorizing" (in some interpretations of it) and "generative theorizing" insofar as both types of theorizing endorse a logic of making inferences which enable researchers (with research participants) to generate imaginative theoretical leaps. This logic can be called (following Peirce, 1911) abduction or retroduction. I have suggested that Charmaz in her discussion of abduction seems to put the focus on developing, through this logic, the most plausible theoretical explanations of the issues under consideration. But I have indicated that this need not preclude a more transformative-oriented pragmatic approach to defining the value of the theorizing.[14] Charmaz hints at this when asked about it in an interview with Puddephatt (2006). He questions her regarding her views on "pragmatism". She responds that:

> **KC**: I come closer to the pragmatists here because I endorse Marx's statement, "Men make their own history but they do not make it as they please; they do not make it under

[14]Strauss and Corbin (1994, p. 280) suggest that in their consideration of the status of grounded theory, they "follow closely … the American pragmatist position" as expressed by Dewey (1937) and Mead (1917). They remark in this context that "a theory is not a formulation of some discovered aspect of a pre-existing aspect 'out there' …. Theories are embedded 'in history'". (1994, p. 280). Therefore, according to them "historical moments are to be taken into account in the creation, judgment, revision, and reformulation of theories" (1994, p. 280).

> self-selected circumstances, but under circumstances existing already, given and transmitted from the past." (2006, pp. 11–12)

In this interview, prompted by Puddephatt, she reflects further on her position (much in the way that the interviewers in the earlier chapters of this book used the interview process to add input into the discussion and hence stimulate further reflection). She remarks that to some extent she follows a pragmatic view of truth, where theoretical validity is measured partly by what Puddephatt calls "its practical effects through action" (2006, p. 12). She indicates that she would prefer a pragmatic view of truth to a radical constructivist position, where the latter can result in individualist and solipsistic epistemologies.

She seems to be suggesting here that she comes closer to an epistemological position where theoretical statements regarding the way history is made, can be judged as "true" insofar as they inspire transformative action. Actually, she here expresses a connection of pragmatism with Marxist materialism (following Puddephatt's prompting), but because she still adopts a (type of) constructivist view, it would seem that she would hold that material conditions cannot be defined as being an "object" of science, but can be posited (inferred) for the purposes of reconsidering possibilities for action. This would be consistent with Reichertz's (2007) account of the value of abductive logic (in grounded theory) in pursuing possibilities for reconstructing the social order:

> Social science explanations of actions aim at the (re)construction of the order that is relevant to the acting subjects …. For this reason it is highly sensible to examine as closely as possible the life practice that is to be understood, and (on the basis of these data) to (re-) construct the new orders (pp. 217–218).[15]

Reichertz goes on to express how this implies a pragmatic view of the value of grounded theorizing (built on abductive inferencing) as he interprets it:

> New orders, therefore, are also always oriented toward future action. An abductive discovered order, therefore, is not a (pure) reflection of reality, nor does it reduce reality to its most important components …. Abduction is something we all do, when there is a crisis or when we do not know what to do next. For many purposes, particular constructs are of use, and for other purposes, different constructs are helpful. For this reason, the search for order is never definitively compete and is always undertaken provisionally. So long as the new order is helpful in the completion of a task it is allowed to remain in force …. In this sense,

[15]Reichertz here states that in this vision of the value of social science explanations, they would not be able to "be derived from proven grand theories, first because these are, as a rule, not sufficiently local,' and second because they have frequently already been overtaken by constant social change" (2007, p. 217). Hence Reichertz suggests that a more middle range theorizing is appropriate in that such theorizing is more locally attuned. But one can just as well suggest, as do certain Indigenous-oriented author, that theories which purport to explain (and criticize) large-scale processes of globalization (such as that provided in postcolonial theory) can also serve to inspire resistant action, as long as specificities in situ are not thereby outruled. Consider, for example, the suggestions offered by Hall and Fenelon (2009), Akena (2017), and McLaughlin (2001). McLaughlin states her argument thus: "Finally, postcolonial theory, while universally applicable, has to focus on specificities" (2001, p. 8).

> abductively discovered orders are neither (preferred) constructions nor (valid) reconstructions, but usable (re-)constructions. (2007, p. 222)

This argument of Reichertz appears in a chapter in a book edited by Charmaz (2007), which implies that she too would be comfortable with, or at least make provision for, the type of pragmatic approach to abductive (or retroductive) inference as here expressed—where abduction is linked to finding a way of solving experienced "crises".

In her (2009) account of the possible epistemological underpinnings of grounded theory, Charmaz indicates that a pragmatist position (in contrast to an objectivist position) "aims to study people's actions to solve emergent problems" and does not regard "facts" as separable from "values", but as "co-constitutive" (2009, p. 139). She adds that pragmatism "views reality as consisting of emerging processes, addresses how people handle practical problems in their worlds, and sees facts and values as joined" (2009, p. 140). And she suggests that constructivist grounded theory, which she regards as having "congruence" with pragmatism (2009, p. 140), "aims to create theory that has credibility, originality, resonance [with participant understandings as expressed], *and usefulness*" (2009, p. 141, my italics). Part of its usefulness, as I interpret her position, seems to lie in the capacity of constructivist grounded theory to re-examine how people might be drawing on socially constructed discourses with un-examined assumptions, which can become rendered more examinable via the research process (2009, p. 142). She gives the example of how "American discourse on educational achievement focuses on the individual, assumed the legitimacy of certain measures, not others, and takes into account a narrow range of areas" (2009, p. 142). She suggests hereby that constructivist grounded theorizing can be linked to a critical perspective, with implications for stimulating a review of discourses and actions.

I should re-iterate that my purpose in exploring Charmaz's exposition of constructivist grounded theory is not in order to suggest that the processes that Charmaz (and others interpreting her position) advise for developing grounded theories need be followed in toto.[16] As indicated earlier, her suggestion (2009, p. 141) that the aim of grounded theorizing is to strive to understand "historically situated data" could be *further extended* by taking more fully into account the understandings of theorizing as future forming as developed by authors such as Chilisa (2012), Gergen (2015), and Kuntz (2015). My aim in this section has been to indicate how theoretical constructions that are grounded in—but not confined to—"empirical" data can be generated and justified. I have also indicated how this justification can be supported within a (reflective) pragmatically-inclined epistemology. (In Chap. 9, Sect. 9.5, I offer additional angles on the way in which pragmatism as a philosophical position can be conceived as being linked to a social justice agenda.)

[16]Charmaz herself notes that it is not clear whether a researcher must use all the strategies of grounded theory in order to claim to be using "grounded theory" (2012, p. 3).

7.4.3 *Indigenous Epistemologies and Retroductive Logic*

Thus far I have not spelled out how Indigenous or Indigenous-oriented authors might conceptualize the "logic" by which research can proceed, excepting by way of my interpreting the many Indigenous authors and Indigenous-oriented authors whose work I chose to showcase in earlier chapters.

In a search of literature on the links between Indigenous thinking/being and retroductive inferencing I could not find much detail concerning Indigenous authors using the term retroduction to characterize Indigenous research methodologies. I found one reference to this in Hall and Fenelon (2009, p. 38), who explained (in a footnote to their Chap. 2) how they have invoked retroductive logic in considering cases of resistance to forces of globalization and the revitalizing of Indigenous cultural traditions. They explain that the cases that they have chosen to analyze and the way they have proceeded, fits the model suggested by Ragin in "*Constructing social research*" (1994), where he makes provision for retroduction. They state that their way of treating the case studies in their book fits well with Ragin's suggested model.[17] This is because

> the "Ideas" or "Social Theory" [analytic frames] are that of Indigenous resistance and revitalization. The deductive process of developing analytic frames occurs before developing the case studies, then moves from "Retroduction" to be reframed by "Images" constructed through a "mostly inductive" compilation of "Evidence" or "Data" arising from the case studies. (Hall & Fenelon, 2009, p. 38)

Hall and Fenelon remark that this is the inferential process by which they have proceeded to draw reframed analyses from the "images" of the case studies. Based on what they see as a retroductive process they try to explain how Indigenous peoples, who so often have little in terms of "conventional forms of power", have put up resistance to forces of globalization (2009, p. 35). They put forward the suggestion that this can possibly be explained via retroductive thinking around the cases, with reference to an understanding of Indigenous peoples' "community bases, decision-making processes, direct spiritual ties to the land and the sea, their traditional economic distribution networks [generally oriented toward sharing], and their particular group characteristics" (p. 35), which include collective relationship building.

Besides this reference of Hall and Fenelon (2009) drawing explicitly on retroduction to explicate their analytic processes, I found also an implicit reference in McLaughlin (2001), who considers in her article what she calls, following Walker (2001), "the central characteristic of 'Indigenist' research as including interconnectedness, focusing on process and relationships, inclusion of spiritual experience,

[17]Ragin (1994) considers the main feature of abduction/retroduction as lying in the fact that the researcher generates a dialogue between ideas and evidence. He defines retroduction as involving a process of constructing representations from the interaction between analytic frames and images (which are constructed from the evidence) (see also Ragin & Amoroso, 2011, pp. 76–78).

and expanded definitions of empirical data" (McLaughlin, 2001, p. 7). She elucidates what she means by an expanded definition of empirical data:

> While Western research paradigms [tend to] focus on what can be known or observable, Indigenous epistemology may include experiences of visions and dreams as empirical data. This illustrates the interconnectedness and holistic experiences, illustrating an epistemology which is inherently spiritual. (p. 7)

Upon seeing this part of my text, critical reader Francis Akena added his concern that:

> Evidently and sadly however, education systems tend to negate what people cogitate as spiritual, also considered to be "other-worldiness and esotericism" (Wane, Manyimo, & Ritskes, 2011) with little or no axiological values attached to it. (Akena, 9 August 2016)

Akena continued that:

> Clearly in the Indigenous world views, knowledge of the higher order is revealed through intuitions, dreams, rituals, storytelling, myths, and also performance which stand as a bridge between the supernatural and the physical (Wane, Manyimo, & Ritskes, 2011). This worldview cannot be dismissed [in terms of knowledge status] simply because it cannot be subjected to scientific analysis and rationality. Rather, a spiritual relationship must be viewed as an examination of the self through the lens of affiliation and connectivity with society including human to human relations, and human to natural environment (living and nonliving things). When embraced in society, this Indigenous worldview fosters reciprocal relationships of care such as respect, compassion, humility, accountability, hence contrasting competitive and individualistic tendencies that are dominant in colonizing societies. (Akena, 9 August 2016)

Portelli, in his Foreword to the book *Spiritual discourse in the academy: A globalized Indigenous perspective* (2014), expresses a similar concern with styles of knowing that decline to admit into our discourses that which may not be "observable" as such, but which may still be considered meaningful for ways of being in the world. As he comments:

> Reducing evidence simply to empirical data reduces the notion of evidence to the empirical and material. The popular articulations of these central concepts, in fact, marginalizes the spiritual and philosophical dimensions, and even excludes the possibility that these dimensions can be considered as *meaningful forms of evidence*—that is, forms of seeing and justifying, as the root meaning of evidence clearly indicates. (2014, p. xii, my italics)

The notion that *we should give credence to that which cannot be "read off" from observed evidence* or directly logically related to it is, as I explained above, the idea lying behind retroductive inferencing.

I now turn attention to the argument of Price (2005), where I found an elaboration of linkages between Peirce's (1911) view of retroduction and Indigenous epistemology. This I believe can cast further light on the connection between Indigenous epistemologies and the use of alternative logics to deduction and induction, where modes of reasoning are also indissolubly tied to what Price calls "ethical outcomes experienced in our lives" (2005, p. 87).

Price (2005) argues that once we assign credibility to retroductive logic, which she sees as being implicit in much Indigenous thinking and acting,

> it also allows a previously ignored aspect of IK [Indigenous Knowledge], its spiritual/non-empirical beliefs, to be validated through ethical outcomes experienced in our lives, rather than through the previous criteria of empirical validity. (p. 87)

In other words, it becomes clear that the question for IK is whether people's "believing in (whatever) … can provide optimistic, long term, ethical, appropriate ways of living" (2005, p. 87).

She makes the point that because Indigenous styles of reasoning, for example, in relation to environmental management, include references to "spiritual and non-empirical beliefs", it is often treated in Western science condescendingly (2005, p. 89). She suggests that this may be because such "scientific" researchers are wary of believing absolutely in "the spiritual/non-empirical aspects of IK". She explains how this spirituality manifests in IK as related to environment and health concerns:

> Apart from practical knowledge about managing the environment and health, IK also insists on values such as "respect" and "responsibility" for the environment. Much of this respect is imbedded in Indigenous peoples' knowledge about spirit/God, as well as in their non-empirically-grounded cultural norms. (2005, p. 89)

As she interprets it, there are linkages in IK between *spirituality/non-empirically-based beliefs*, *ethics* and *retroduction*. That is, the purpose of constructing "beliefs" (which she argues need not be static in IK as some authors aver) is so that long-term optimistic solutions which can contribute to "better living" socially and ecologically can ensue. And she suggests that this too is the point of Peirce's pragmatic approach:

> Interestingly, and appropriately for this paper, Peirce's most explicit exploration of retroduction was not, as one might expect, within the context of scientific issues, but in the context of demonstrating the existence of God through the measure of ethical behavior …. Peirce's God was perhaps not a typical one [as typified in Western religions]. (2005, p. 89).

She suggests, following Chiasson's (1999) exposition of Peirce's various texts, that for Peirce, God

> does not have to signify a specific being—nor need it have a religion connected up to it. It appears that Peirce's use of the term, God, may have signified an ongoing inquiry into the hypothesis that there is meaning resulting from the way in which an individual conducts his life. This meaning is a consequence of deliberate choices of conduct based upon having abductively developed the hypothesis that what he does matters to both the immediate and ultimate outcome of things that may be beyond his [or her] ken. (Chiasson, 1999, paragraph 26, as cited in Price, 2005, p. 90)

When Peirce refers to "reality" then, according to Price, he is arguing for ways in which (abductively/retroductively derived) perspectives or visions can be directed toward the "living of a good life", even though what this means is not fully graspable, and can be sought only in practices of developing meaning with others (Price, 2005, p. 90).

Bearing all this mind, Price suggests that if researchers in all "scientific" traditions, as well as the general public, could come to understand the implications of what Peirce (1908) calls musement or retroductive reasoning (associated with

people's gearing toward "a good life") this would "remove the stigma attached to non-empirical beliefs". She continues as follows:

> If we assume that scientism was at fault by over-emphasising the empirical, we put IK on a par with scientific knowledge and prevent its reification [possible stultification]. When IK deals with non-empirical issues, this does not mean it is dealing with non-real issues. It will still, therefore, have to ensure that its claims are answerable to "realness". Thus IK can be debated [or entered into engagement with] and critiqued [albeit not in adversarial fashion] along with other forms of knowledge and can develop dynamically to contribute appropriately to current issues. (2005, p. 92)[18]

Her argument is that IK, as with other forms of knowledge construction, can all be judged in terms of their being answerable to "realness", where realness is *defined in relation to living the good life in connection with other humans as well as with all living and non-living things*. Glass and Kaufert (2007) aptly summarize this kind of stance by referring to Castellano's (2004) observations in relation to Aboriginal world views:

> According to Castellano [2004, p. 103], "Aboriginal world views assume that human action, to achieve social good, must be located in an ethical, spiritual context as well as in its physical and social situation". She contrasts this perspective with scientific research which is dominated by "positivist thinking that assumes only observable phenomena matter". (Glass & Kaufert, 2007, p. 31)

This is similar to what the Indigenous authors to whom I have referred in this and earlier chapters have likewise expressed when explicating their concern with the way in which dominant Western styles of knowing try to sift spiritual and moral discourse out of the academy (as also noted by Akena above). (See in addition Chap. 1, Sect. 1.3.5.) In Chaps. 2–6 I have offered examples of how the Indigenous authors to whose work I have referred, can be considered as fitting well (to use Hall and Fenelon's expression, 2009) with a manner of proceeding which tries to re-inject discussion around living the "good life" into the research enterprise, as part of the process of doing what can be called "good research".

7.5 Revisiting Imputations of Causality in Generative Theorizing

In Sect. 7.3, I started off my discussion of generative theorizing by referring to Gergen's point that social theory has the capacity to "shift the attributed locus of causality for a given range of activity and, in doing so, alter common reactions to

[18] I have added in square brackets in Price's above quotation that discussions around "realness" (and the good life) need not take the form of *debate*—which might imply adopting an adversarial stance, as noted for example by Collins (2000), Harris and Wasilewski (2004), and Wilson (2003). Rather the discussions should take the form of a *dialogue in which together people enrich their perspectives and possibilities for action.*

such activity" (1978, p. 1349). Gergen expresses his sympathies with theorists who, for example in the USA, highlight the causal mechanisms which severely limit the life chances of, say, "the ghetto black" as part of their explanation of rioting behavior (p. 349). In this way the theorists, he argues, take into account the consequences of the way of theorizing for the manner in which "blame" might become more generally attributed in society. Now the question arises as to what status any theoretical statements about causal mechanisms should be seen to have. I indicated in Sect. 7.4 that the use of retroductive logic is coupled with the recognition that statements (inferences) about causal mechanisms cannot be tied in any direct logical connection to "evidence". How, then, do those arguing for retroductive reasoning suggest that causality can be understood?

Various authors propounding the value of retroductive logic have offered accounts of what is implied in positing/inferring causal mechanisms which are argued to underlie more immediately observable "effects" (which cannot be revealed via deduction or induction alone). Layder, for instance, taking a critical realist position, contends that the operationalization of variables in order to deductively test specific hypotheses about relationships between the variables is one-sided (1993, p. 56). In addition, he claims that the inductive approach used by certain interpretive-oriented ethnographic researchers, who tend to focus on the "immediate environment of actors" too is one-sided (1993, p. 56). He therefore suggests (1993, p. 31) that a "more complete picture of reality", and of underlying causal mechanisms which exceed actors' perceptions, can be built up through some form of dialogue between different forms of theorizing and different ways of making use of analytic tools, including retroduction (1993, p. 31).

He maintains that through such a dialogue, theorists can take on board "different perspectives which allow them to see empirical reality in slightly different ways" (1993, p. 122). He proposes that by "cutting into the data" from different angles in terms of "different research strategies (methodological or analytic)", a "variety of 'slices' of the research site" can become revealed (1993, p. 123). Through a dialogue, which is not confined to a recourse to inductive and deductive analytic tools, he believes that the research community as a whole is likely to arrive at "better" knowledge of social realities (and operative causes). Davidson and Layder (1994) expand upon this when they suggest that judgments about the quality of research can be made through a rational dialogue within the scientific community—which should function in terms of a "healthy communication" across different ways of theorizing and different ways of anchoring this in the empirical world. Participation by researchers in such a dialogue means that, ideally, the research community as a whole is able to develop more integrated knowledge about reality (see also my account of their argument in Romm, 2001a, pp. 41–43).

However, Redman-MacLaren and Mills (2015), also speaking from a critical realist perspective, are concerned about any elitism that might accompany professional researchers/theorists' deliberations about social realities in the quest to develop what Davidson and Layder (1994) would call "integrated knowledge". Redman-MacLaren and Mills emphasize (citing Potter and Lopez, 2001) that it is crucial to adopt a stance of "epistemological caution" with respect to any scientific

knowing (as organized in the scientific community). In line with what they consider to be a decolonizing perspective (2015, p. 6), they criticize critical theorists who "often theorize in isolation to preserve a distance between themselves and research subjects" 2015, (p. 4).

Redman-MacLaren and Mills argue that we need to bear in mind (as they believe critical realists can and should) that "we [any inquirers] can only partially know the phenomena and this knowing is socially, culturally and historically bound" (2015, p. 4). Therefore, they suggest that "researchers can expand their understandings … by including participants throughout the research project" (2015, p. 5). In this way they state that both the researcher's knowledge and the participants' knowledge (where the engagement between them includes a commitment to "a structural critique for purposes of social change") can increase (2015, pp. 4–5). They suggest that this is the purpose of what they call "transformational grounded theorizing". They propose (p. 5) that one way of practicing this is through a participatory action research approach where there is an "equal interest" in *participation* (of co-researchers) and *action*. They here refer to Teram, Schachter & Stalker's claim (2005, p. 1132) that one need not make a choice between prioritizing participation or action (p. 5). In my next chapter I refer further to ways of conceptualizing participation (of all those involved in the inquiry) and action in what I call *active research*. I suggest that active research need not follow traditional action research cycles, but does need to include what Redman-MacLaren and Mills call "enacting a participatory, action-oriented, power-sharing" approach as part of the research agenda (2015, p. 8).

What is important, then, as Redman-Maclaren and Mills point out, is that "research subjects" are not transformed into "theoretical objects", to be gazed upon and analyzed by professional researchers/theorists (2015, p. 5). They suggest that a Glaserian approach to grounded theorizing has the tendency to turn people (and social life) into an object for theorizing about. As they see it, in a transformative grounded theorizing approach, researchers and co-researchers need to jointly decide on practices of so-called "theoretical sampling", with a view also to reflecting together on social structural forces that may be restricting possibilities for positive social change (2015, p. 5). I elucidated in Sect. 7.4.1 that we might better term this process "active sampling" so that indeed "partnership is centralized throughout the research process, in determining research findings and conducting resultant action" (2015, p. 8). Redman-MacLaren and Mills re-iterate that their intention is to highlight the potential of a grounded theorizing approach linked with decolonizing methodologies for "enabling action and generation of new knowledge in the context of more equal power between the researcher and researched" (2015, p. 5).

So now it may still be asked: how in this approach can those involved in the research inquiry go about the process of locating and imputing causality to social mechanisms that, in action, are regarded as capable of being changed? Redman-MacLaren and Mills make the point that what is important to them is that, as researchers, they themselves are committed to "developing and supporting transformative research methodologies and methods in the small spaces in which we operate" (2015, p. 7). By having this commitment, the question of imputing

"causality" in a way which will be regarded as liberatory for the actors involved in the situation, becomes a practical question of how research can contribute to the living of a more quality existence (using the spaces in which researchers, with co-researchers, operate). The question of the "realness" of the imputed structures, or structuration processes, which are now seen as capable of alteration, is *less important than making "real" a better quality of life*.

Redman-MacLaren and Mills do not quite express their argument this way. Nevertheless, as with other authors, I have extrapolated from their suggestions a pragmatic interpretation of how leverage points for transformative action may be recognized (as offering productive action possibilities) without implying that anyone can claim to have closer access than others to posited causal structures "in reality". In this way I have tried to extend critical realism (which they admit should not be coupled with any claims to "know" on the part of would-be knowers) with a pragmatic twist. Claims to "truth" here—including truth claims about causality as it operates in social life—give way to what Kuntz (from his "new materialist" position) calls "newly relational claims on truth", where people together use their "productive power" (2015, p. 139) to "intervene in the logical production of the future" (2015, p. 135). Knowing and acting are not premised on having to pinpoint "causality" in any clear-cut way, but on "a determination to live other than we have, and a collective hope for a more socially just context of the now" (2015, p. 139).

7.5.1 *Admitting a Plurality of Perspectives in the Imputation of Causality*

Given that no-one (or group of people) can legitimately claim to have found the best (even tentative) way of viewing 'realities", I now address the question of how and whether in theorizing we can admit a plurality of perspectives in relation to the imputation of causality.

Cohen, Manion and Morrison (2011, p. 62) make the point that in all forms of research it is impossible to establish "unequivocal direct causation". Even in the use of "sophisticated statistics such as structural equation modeling, multiple regression and multivariate analysis", causation has to be imputed and cannot be read from the statistical information: that is, "causality" is embodied not in the reading of the data, but in the "theoretical underpinning and assumptions that support the model". Thus questions remain, after the statistical readings, such as: "how is it that X causes Y; what is happening in X to cause Y?", etc. (2011, p. 62). They aver that in order to come to any appreciation of this, we need to regard causation as *dynamic* rather than static, and as a *process* rather than a single event; it is here, they note, that "qualitative data" also comes into its own in offering additional perspectives regarding the "processes of causation", albeit that these too depend on actors' perspectives (p. 62). They refer to the example known as the *Rashomon effect* in social science (Roth & Mehta, 2002) where Kurosowa's film *Rashomon* (filmed

more than 60 years ago) stunned audiences by illustrating how alternative interpretations of the apparently "same" event (the death of a samurai) could be proffered by witnesses to the event. They remark that "at the end there is no clear statement of whose version is correct; truth flounders on the quagmire of epistemology" (2011, p. 62). In relation to this film they note that:

> Anthropologists, lawyers and social scientists seized on the film as an example of the multilayered, contested truth of any situation or its interpretation, coining the term the "Rashomon effect" to describe an event which is reported or explained in contradictory terms, that gives differing and incompatible causal accounts of an effect: a death. (2011, p. 62)

Based on their understanding of the import of the Rashomon effect, they also advise (2011, p. 62) that apart from focusing on "aggregate independent and dependent variables of cause and effect" (as, for example, in survey research) and apart from focusing on individuals' meanings, actions and interactions in social settings (as in more qualitatively-directed research), one can also work at the level of examining "macro-structural features from society" (as I noted in Sect. 7.3). That is, they suggest that researchers (anyone involved in inquiries) should in addition cautiously try to elucidate "how macro-structural features from society … enter into individuals' actions and interactions, and how individuals' actions and interactions determine [or contribute to the structuring of] social structures" (2011, p. 63). They emphasize that theorizing around all of this needs to be "cautious" in view of the epistemological quagmire of the Rashomon effect. Overall, they conclude that as they see it "there is [always] more than one causal explanation at work in a situation" (2011, p. 62). How then, we may ask, should inquirers make sense of the plurality of potential explanations?

Ragin and Amoroso, in their account of the construction of analytic frames, state that it is possible for people to hold multiple frames—that is, ways of interpreting and explaining social situations—rather than holding only onto one frame (2011, p. 78). But they add the proviso that this is mainly when these frames are considered to be *complementary* rather than contradictory. However, the Rashomon effect as described by Roth and Metha (2002) implies that proffered explanations—all seemingly plausible—can indeed be contradictory.

Newbury (2011, p. 335) suggests that a solution to grappling with inconsistency is for people, in processes of reflecting on their theorizing, to admit into their consciousness what appear to be contradictory frames, as the basis for a *conversation that is set up between them* (and between people holding different frames).[19]

[19]Her argument in this regard bears some similarity with what Dressman would call using theoretical frames as a "dialectical scaffold", where tension between theory(ies) used and "the data" is specifically drawn out. Efforts are not made to "align" the data in any obvious connection with a theory or theories, because the purpose is more to "re-read" and "re-think" theoretical frames (2007, pp. 347–349). I would suggest that what is important about "dialectic" logic is that it is forward looking, and is able to incorporate contradictions in practice. Serper (2015) gives an example of how dialectic logic goes beyond propositional logic:

Proposition A: Alon is stupid

She refers favorably to Denzin and Giardina's edited book *Qualitative inquiry and social justice* (2009) and notes that their position is that, as she summarizes, "central to this move for research to contribute to social justice is an emphasis on multiplicity rather than a single, hegemonic truth" (2011, p. 338).

In this regard, Newbury proposes that we can adopt an orientation to "pragmatically drawing connections across theoretical differences" (2011, p. 335). This, I would suggest is a kind of pragmatic position which would also be supported by Collins (2000, p. x) who argues that coalitions across different ways of seeing can be accomplished without having to agree or settle on one (seemingly best) way of viewing "realities". By recognizing that there is no hegemonic truth, people can in practice form coalitions as they engage in generative theorizing around possibilities for furthering social (and ecological) justice.

Newbury expresses her own clearly emotion-full position concerning the failure to admit multiplicity:

> There is nothing that makes me squirm more than a polarized division that has developed around a dinner table during conversations over political matters: right or left, conservative or liberal, sustainability or development. The conversation becomes neither interesting nor productive when people's identities are so intertwined with the camp to which they assign themselves that they can no longer hear anything outside of it. Is this not how wars begin? (2011, p. 340)

Conversely, she expresses her sympathy with, for example,

> John Caputo [who] can speak of religion to a diverse group comprised of both atheists and those who hold various spiritual beliefs, and the issue of his own "religious location" does not have to be addressed? Nobody seems concerned with what he or anyone else in the room "believes in" personally. The gathering is about open exchange of ideas, and about moving toward hopeful possibilities which may draw from multiple spiritual realities in a way that requires none to be more right (or wrong) than any other. (2011, p. 340)

As Newbury indicates, the epistemological question of defining "right or wrong" ways of seeing becomes sidestepped as people venture to "move toward hopeful possibilities" in practice. This is also the argument offered by Mugadza (2015), to which I referred in Chap. 1, Sect. 1.2.1. Newbury suggests that the "postmodern" turn in social research, in which claims to truth (as representation of external realities) become sidestepped, can facilitate a process of initiating change that can be accomplished. As she states:

Proposition B: Alon is smart

According to this (propositional) logic, "If Alon is smart then he cannot be stupid". And "If Alon is stupid then he cannot smart". As Serper notes "evidence is then brought to render one of the propositions truthful and valid which invalidates the other proposition and possible explanation". But according to dialectic logic, Alon can, for instance, *actively transform his intelligence* (by becoming more self-reflective, also by considering others' perspectives) and hence *become smarter* (in practice). In this process, as Serper puts it, "Different relevant propositional enquiries and responses and linguistic assertions that could be contradictory transform each other" (2015).

> Particularly after the postmodern turn, research is understood by many not only as a tool by which things can be proved or disproved, but as a tool by which change can actually be accomplished. (2011, pp. 337–338)

Denzin and Lincoln (2003, p. 15) also suggest that an appreciation of postmodern sensibilities makes room for us, as inquirers, to find other ways of evaluating research work—other than evaluating its claims to getting closer to a singular truth about the world. This is similar to the interpretation of "psychological science in a postmodern context" that is given by Gergen (2001). He considers that what is of value in "postmodern scholarship" is that it "poses significant challenges to pivotal assumptions of individual knowledge, objectivity and truth" (2001, p. 803). He underscores (2001, p. 806) that postmodernists propose that "arguments about what is 'really real' are futile" (because as humans we have no access to realities other than through the use of humanly constructed language and concepts). And he argues (2001, p. 808) that insofar as people (including scholars) in society appreciate this, "we are more fully prepared to engage in the kinds of dialogue from which new and more viable constellations of meaning may emerge". As indicated earlier, Gergen links what he calls "postmodern dialogues" (not directed to finding "truth" as representation of reality) to a pragmatic position, much in the same way as does Newbury (2011). Space in this book does not permit a discussion on the plethora of interpretations of postmodernism that have been proffered since Lyotard's *The postmodern condition* (1984).[20] I refer to the postmodern condition here merely as a pointer to show how one can interpret postmodernism affirmatively to affirm the possibility of using the research space to imagine new possibilities for constructive action (see also Chap. 6, Sect. 6.2.2).

I now refer again to all of the exemplars which I have offered in earlier chapters. I discuss these in relation to a pragmatic conception of causality, in terms of which people can make decisions about possibilities for action, by working across alternative causal mappings of realities and reflecting anew on leverage points for what they consider to be, in the words of Cisneros and Hisijara "hunches" on which we "make decisions that feel right" (Cisneros & Hisijara, 2013, p. 9). By "hunches" they do not mean that those concerned have settled on a way of viewing "realities", but for the purposes of action decisions are made, especially as the aim is also to *form* lived realities (understood as in a process of becoming). Cisneros and Hisijara's use of the word "hunch" resonates with Peirce's language of guessing as one makes inferences for the purposes of action. (Cisneros and Hisijara here make explicit reference to Peirce's views on retroduction.)

In regard to Chap. 2, I would suggest that when we (myself and the research participants) discussed the meaning of racism in the context of the participants'

[20]Gergen likewise remarks that within the social sciences, discussions around the nature of scientific truth "have left a trail of misunderstandings, animosities, and an increasing divide between traditionalists and those variously termed 'postfoundationalist,' 'post-empiricist,' and 'post-modern'". He indicates that it is not his intention to "review the vast body of literature surrounding these issues, nor to resolve the remaining tensions" (2015, p. 288). What he tries to accomplish in his article, is to offer an argument for viewing social science as future forming.

lives, we were not assigning what could be called "theoretical primacy" (and attendant causal efficacy) to either "race" or "class" as determinants of people's life chances. As I elucidated in my book on *New Racism* (2010, pp. 387), Omi and Winant (1986) define what they call a racial formation as constituted in terms of an *organizing principle of social relationships along racialized lines*. As far as the relationship with *class* is concerned, their view is that just as we do not as theorists need to operate with a fixed notion of the meaning of "race" and its way of impacting on lives, nor do we need to operate with "a static and fixed notion of class" (2002, p. 456). Such a way of theorizing and attendant way of assigning causality, they suggest, "provides little room for understanding how race (and gender) could shape class categories, consciousness and organization" (2002, p. 456). The suggestion here is that different axes of discrimination can rather be treated as *mutually constructive* of each other.

Essed makes a similar point when she maintains that her conceptualization of racism carries with it the suggestion that "people are involved differently in the process of everyday racism according to gender, class, status, and other factors determining the content and structure of their everyday lives" (1991, p. 52). For Essed it is thus important that researchers develop mechanisms to identify "how intertwining systems of domination are expressed and experienced in everyday life" (2001). She also considers it important that professional theorists do not theorize above the heads of participants, but aim to develop "a mutually accessible conceptual language for lay persons and professionals to address everyday racism (in and outside of institutions) embedded in larger gender, economic, and historical structures" (Essed, 2001: http://www.un.org/womenwatch/daw/csw/essed45.htm).

Now I would suggest that in the case of the research outlined in Chap. 2, the concerns of the Black participants as expressed were on the level of identifying how "race" for them meant cheap labor (and was thus historically intertwined with class in the development of apartheid structures). But cheap labor was not their only concern, as issues of dignity were also involved in their experiences of not being assigned equal dignity with Whites, with White people historically treating them as "labor" and hence not being interested in learning about them and their cultural expressions. Therefore, they were pleasantly surprised when Koos showed such an interest. Meanwhile, Lynette too intimated that she appreciated the need for affirmative action (on the economic level) at this point in time. All in all, there seemed to be some appreciation of the intertwined determinants of racial and class oppression. For purposes of action in this case, we tried (or I tried prompted by the participants) to make small dents in the arrangements of the apartment complex, via the Trustee meeting, in redefining the value of the labor of the (Black) workers.

For this purpose, it was not necessary to decide with any finality how to analyze the structuration of the institutionalized "racist" and "capitalist" mechanisms in terms of the experienced effects on the participant's lives, but it was possible to try to imagine and make some practical intervention. And I would submit that if more widely practiced in different arenas, these types of practices could contribute to some restructuring of raced and classed relations (insofar as people as collectives imagine and enact more relational social and economic arrangements). Of course,

space in this book does not permit a full discussion of our various ways of theorizing or reflecting together in situ; but I have hereby tried to indicate how in practice inquirers might hold multiple analytic frames of reference (which may be more or less explicit), while moving toward trying to co-develop possibilities for some forms of hopeful action.

In Chap. 3, I tried to illustrate how an analytic frame that treats "self-efficacy" as a possible determinant of teacher attitudes toward inclusive education could be extended with reference to an alternative frame, which focuses on efficiency as being a *relational construct* rather than a matter of "mastery" or exercise of "control". Or rather, these analytic frames could be brought together in conversation (as we tried to do during our focus group sessions and other meetings, albeit not in terms of this jargonized language). Thus here again for the purposes of action the theoretical decision as to which dimension of self-efficacy (treated as an independent variable) is the "best predictor" of attitudes toward inclusive education (Savolainen et al., 2012, p. 55) could be linked to a practical decision regarding how to work with teachers in supporting them to become more "efficacious" in the sense of capitalizing on support within their social network.

In the case of the national 500 schools project (the second example in Chap. 3), Dan Tlale also tried in practice to alert participants to resources within their social system (such as possible business donors, the department of social welfare, the department of social development, the department of basic education, etc.) which could be used to turn around historically perpetuated legacies of apartheid. As Mertens, notes, referring to Rittel and Webber's (1973) definition of a "wicked problem" as constituted by "multiple interacting systems … replete with social and institutional uncertainties", the South African public schooling system can be regarded as such (Mertens, 2016, p. 15). Mertens indicates that what is important is that those involved in inquiries should feel part of "an ongoing search for solutions to the problems associated with education [in South Africa] in ways that create the needed changes for increased social justice" (2016, p. 15). We can say that this was Dan's intention; and through his reflecting with key participants on leverage points for engendering more "justice", some shifts in making a difference to learners' life chances in the Idutywa context could be and were made (as reported in further follow up conversations—see Romm & Tlale, 2016; Tlale & Romm, 2017). This can therefore be seen as one exemplar of a way of *collaboratively theorizing in the form of seeking leverage points for enhancing social justice, to be tried out in action*.

In Chap. 4, I indicated that Woldegies (2014) starts his dissertation by offering some background information on the gender discrimination written into Ethiopian society as also manifest in the Amhara region. He refers to "chauvinistic attitudes that hindered women acquiring legitimate rights of power and socioeconomic status", which he considers as pervasive across Ethiopian communities. But he also points to other social tendencies (forces) that provide leverage for action, and that could be capitalized upon. For example, through the research on which he reports, the women in Amhara were able to locate and strengthen proverbs supportive of women, in order to in turn disseminate these for wider consumption in their

community. Furthermore, supporting structures in the family and community too were seen to offer a leverage point for transformative change toward women's increased empowerment. Meanwhile, neither he nor the various participants needed to adjudicate on what kind of "feminist theory", if any, should be invoked to explain the "real" operative mechanisms of gendered disempowerment. The theorizing in action took the form of seeking opportunities through reflecting anew on hopeful possibilities. Hence some of the women suggested that Woldegies should take pictures of their successes, as a way of inspiring other women. And they also used Woldegies as a conduit (interlocutor) to offer recommendations to other stakeholders in regard to possible extra supportive action, which has the potential to reshape the patterning of gender and economic relations.

As I mentioned also in the case of Ssali and Theobald's research, they themselves do not use the word *theory*, possibly because they did not wish to "settle" on offering some theoretical explanation of the (multiple) causes of the suffering that the research participants expressed. What might be called Ssali and Theobald's generative theorizing was action-directed around the need for breaking stereotypes (a need which they feel arose also from the way the participants recounted their stories). Hence they offer suggestions of how this might be done through the health reconstruction process, along with trying to address other stereotypes (for example, ethnic ones) as a route to social reconciliation. Using Magnat's terminology (2012, p. 172) we can say that they were intent on harnessing the research process to try to contribute toward "alter[ing] the limitations of life as lived" by seeing the research endeavor as a possible route to "strengthen connections".

In Chap. 5, in Stephens' understanding of the experiment—as I extrapolate it—he was not bent on inferring (even in the light of the statistical evidence) whether the creativity training and emotional intelligence training could confidently be said to have made a difference to recidivists' subsequent creativity and emotional intelligence, when faced with the warden's expression of doubt (as he reported to me in personal communication). Nor was he trying to "find out" whether differences in the recidivists' lives could be accomplished given the stigma toward prisoners that he locates as operative in various sectors of the Nigerian society, including on the part of the wardens. He was trying to hold pessimistic and optimistic analytic frames in consciousness, all the while still seeking hopeful possibilities that might work toward transformation. He points in his dissertation (2012) to possible leverage points in reviewing the cultural fabric of this, and indeed all materialist-oriented cultures, where wealth is highly valued alongside a dearth of life chances for the majority in society. He indicates that some of the recidivists during the training had expressed culturally alternative ways of seeing their failed life chances, by recognizing that there may be options for them in, for instance, creating co-operative enterprises. Ultimately, he seems to be using theory as a "dialectic scaffold" (Dressman, 2007, p. 345) to express different ways of connecting his (critical) theoretical analyses with "the data" (as interpreted). For purposes of his own action, he tries to make sure that the prisoners are aware—during and after the experiment—that he believes they should be given a chance, and he

takes initiative in setting up and maintaining opportunities for them within the prison (e.g., for studying while in prison).

In the experiment by Oczak and Niedźwieńska, they also give an indication of different possible interpretations of some of the results of the experiment. For example, they make the point that just by virtue of the researchers showing interest in participants' feelings and perceptions during the experiment, might have made them evaluate themselves higher in self-esteem (following the research) than participants usually do in susceptibility/suggestibility research (2007, p. 56). That is, just because someone (the researcher) had expressed interest in their responses, might have made people feel more valued and hence the self-esteem scores of both the control and experimental groups were higher than one would expect from participants involved in deceptive research. As far as theorizing the effect of the new debriefing procedure as an educative encounter is concerned, they venture to infer from the data that experimental researchers can play a part in skilling people to turn around situations where they might otherwise feel obliged to submit to the persuasion of authority. They concede that the effects carried into everyday life may not be lasting. But they urge that experimental researchers should take some responsibility in generating extended debriefing processes, which if widely taken up, might make a difference to transformation in society along the lines of less authoritative/coercive patterning.

Finally, in Chap. 6, in McIntyre-Mills' discussion set up with key participants who had participated in the research—including some mentors and research assistants—a number of theoretical positions emerged for consideration, which she in turn shares with us in her books (e.g., 2014a, 2014b). What she feels is important about the project, was that at least the scenario of "business as usual" was held up during the research as subject to re-reflection, and indeed some shifts could be inferred on the basis of the participants' responses. Shifts at the level of local government decision-making were also reportedly made. In the case of Roman-Alcalá, he too does not try to close the theoretical discussion around whether and in what contexts the private property relations at the heart of capitalism can be turned around as a way of creating different options for food security. It is the practical process of managing to turn it around in this context that he shares with audiences, for further reflection. And likewise Maathai does not offer her autobiography with a view to trying to direct readers to one view of her understanding of the situations in which she was involved (as remembered), excepting that she is hoping that it will be inspirational for readers who are invited to engage with her reflections around her ways of making decisions, in collaboration with others, in action.

7.6 Conclusion

In this chapter my aim was to offer further deliberations—further to earlier chapters—on what can be said to be involved in what I call responsible generative theorizing. I did this by way of referring to the examples that I used in earlier chapters,

to further spell out a number of "characteristics" of generative theorizing which I have drawn out, based on my reading of some of Gergen's initial arguments (along with other theorists), and based on my further reflections in relation to the proffered examples.

To start off with, I highlighted Gergen's metaphor of research as *shaping* to replace the metaphor of research as *watching* (in the sense of trying to mirror reality). I explained how this *shaping* metaphor *urges researchers to take more responsibility (than in the watching metaphor) for the shaping effects of the research endeavor.* I offered examples taken from my earlier chapters, where I now synthesized how the shaping metaphor can be said (or extrapolated) to have been at play in the way of operating of the researchers whose work I chose to showcase.

I moved on to suggest that involvement in processes of generative theorizing can encourage professional and lay researchers to theorize social structures while bearing in mind, and indeed helping to activate, possibilities for reconstruction of our ways of collective being in society. I indicated how this implies a critique of Merton's (1964) proffered sociological account of middle-range theorizing. I suggested—along with some other chosen authors—that his suggestions are overly focused on converting "larger" theories into slices that are regarded as researchable with reference to "the evidence". As such I argued that researchers can become complicit in reproducing the (observed) patternings of the larger society, as their theories feed into discourses in society. I offered examples, by elaborating upon my earlier chapters, of theorizing—in engagement with research participants—to, as Kuntz puts it, create "new spaces where previously there had been done" (2015, p. 69).

I then offered an explication of how retroductive logic (so-named by Peirce, 1911) can serve as a resource for this kind of generative theorizing. Gergen suggests that generative theorizing is based on what he calls "logics of reflective pragmatism" (2015, p. 287). This logic is directed to inviting us (anyone involved in inquiries) to "further exercise of the imagination, extending and expanding the vision of inquiry". This, I suggested, implies working with what McLaughlin (2001, p. 7) calls an "expanded definition of empirical data" and accordingly an expanded understanding of the "empirical basis" of theorizing. I indicated how Indigenous and Indigenous-oriented authors introduce spirituality into the research process, where they see knowledge generation as tied to notions of living "a good life" in connection with others and with the environment. I pointed to ways in which (forward-looking) generative theorizing can be supported by abductive/retroductive inferences. (I did not try to distinguish retroduction from abduction for purposes of the discussion, as my aim was to locate how abduction/retroduction exceeds inductive and deductive logic.) I referred to some authors (e.g., Charmaz; Reichartz) who see the mode of analysis of "grounded theory" as resting on abduction; and in order to draw out a pragmatic interpretation of grounded theory, I extended the notion of "theoretical sampling" toward a concern with organizing "active sampling" (as understood by Rivombo, 2016). I argued that this is more in keeping with researchers (with research participants/co-researchers) *taking some*

responsibility for the potential effects of research inquiries in shaping emergent social outcomes.

I used examples from earlier chapters to illustrate my suggestions. I also suggested that by juxtaposing views on the development of grounded theorizing (based on abduction) with the generative theorizing proposed by Gergen (as a world-shaping exercise), we can extend our notions of grounded theory to not only building up theories via so-named qualitative inquiry, but to employing abduction in *any* research, to, in Reichartz's terms, create "usable (re-)constructions" (2007, p. 222). This would mean bringing to the fore in any inquiry a conception of abduction as, in Reichartz's terms, a reconfiguring (with others) of knowledge-constructions toward generating (more) "usable constructions". I suggested that linked to generative theorizing as conceived by Gergen, this means that researchers/co-researchers, can practice theorizing with a social conscience as a future-forming endeavor (Gergen, 2015, p. 290).

Further to this, I offered some deliberations around how "causality" might be viewed by those committed to generative theorizing. I pointed out how a pragmatically-oriented generative theorizing might regard the status of any imputations of causality. I suggested that in terms of a pragmatic account, statements about causal mechanisms would be coupled with exploring, in action, leverage points for desired social (and ecological) transformation. Again I gave examples from earlier chapters to concretize the discussion and to extend theoretical visions regarding the epistemological status of causal claims as tied to (participatively) locating potential leverage points for action. The examples explored in Chaps. 2–6 point to the need to critically examine (and shift in practice) the power relations involved in deciding what practical effects linked to the knowing enterprise would be regarded as "desirable" for a future that is yet to be.

References

Akena, F. A. (2017). Customary land tenure and ecological sustainability in Acholi land, Northern Uganda. In J. J. McIntyre-Mills, Y. Cocoran-Nantes, & N. R. A. Romm (Eds.), *Balancing individualism and collectivism: Social and environmental justice* (pp. 221–237). New York: Springer.

Roman-Alcalá, A. (2015). Broadening the Land Question in Food Sovereignty to Northern Settings: A Case Study of Occupy the Farm. *Globalizations*, *12*(4), 545–558.

Bati, T. B. (2015). *Blended learning in large class introductory programming courses: An empirical study in the context of an Ethiopian university*. Doctoral thesis, University of South Africa, Pretoria.

Bendassolli, P. F. (2013). Theory building in qualitative research: Reconsidering the problem of induction. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *14*(1) (Art. 25).

Bhaskar, R. (2008). *A realist theory of science (with a new introduction)*. London: Routledge.

Castellano, M. B. (2004). Ethics of Aboriginal research. *Journal of Aboriginal Health, 1*(1), 98–114.

Charmaz, K. (2000). Grounded theory: Objectivist and constructivist methods. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (2nd ed., pp. 509–535). Thousand Oaks, CA: Sage.

Charmaz, K. (2006). *Constructing grounded theory*. London: Sage.

Charmaz, K. (2008). Constructionism and the grounded theory method. In J. A. Holstein & J. F. Gubrium (Eds.), *Handbook of constructionist research* (pp. 397–412). New York: Guildford press.

Charmaz, K. (2009). Shifting the grounds: Constructivist grounded theory methods. In J. M. Morse, P. N. Stern, J. Corbin, B. Bowers, K. Charmaz, & A. E. Clarke (Eds.), *Developing grounded theory: The second generation* (pp. 127–154). Walnut Creek, CA: Left Coast Press.

Charmaz, K. (2012). The power and potential of grounded theory. *Medical Sociology Online, 6*(3), 2–15.

Chiasson, P. (2001). *Abduction as an aspect of retroduction*. Retrieved July 6, 2008, at: http://www.digitalpeirce.fee.unicamp.br/abachi.htm.

Chilisa, B. (2007). Decolonizing ethics in social research: Toward a framework for research ethics in African contexts. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa: Ethical issues* (pp. 199–208). Addis Ababa: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Cisneros, R. T., & Hisijara, B. A. (2013). *A social systems approach to global problems.* Cincinnati, OH: Institute for 21st Century Agoras.

Clarke, A. E. (2005). *Situational analysis: Grounded theory after the postmodern turn*. Thousand Oaks, CA: Sage.

Clarke, A. E. (2009). From grounded theory to situational analysis: What's new? Why? How? In J. M. Morse, P. N. Stern, J. Corbin, B. Bowers, K. Charmaz, & A. E. Clarke (Eds.), *Developing grounded theory: The second generation* (pp. 194–233). Walnut Creek, CA: Left Coast Press.

Cohen, L., Manion, L., & Morrison, K. (2011). *Research methods in education* (7th ed.). London: Routledge.

Collins, P. H. (2000). *Black feminist thought: Knowledge, consciousness and the politics of empowerment* (2nd ed.). London: Harper Collins.

Davidson, J. O. C., & Layder, D. (1994). *Methods, sex and madness*. London: Routledge.

De Souza, D. E. (2014). Culture, context and society: The underexplored potential of critical realism as a philosophical framework for theory and practice. *Asian Journal of Social Psychology, 17*(2), 141–151.

Deetz, S. (2008). Engagement as co-generative theorizing. *Journal of Applied Communication Research, 36*(3), 289–297.

Denzin, N. K. (2001). The reflexive interview and a performative social science. *Qualitative Research, 1*(1), 23–46.

Denzin, N. K., & Giardina, M. D. (2009). Introduction: Toward a politics of hope. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry and social justice* (pp. 11–51). Walnut Creek CA: Left Coast Press.

Denzin, N. K., & Lincoln, Y. S. (1994). *Handbook of qualitative research*. Thousand Oaks, CA: Sage.

Denzin, N. K., & Lincoln, Y. S. (2003). Introduction: The discipline and practice of qualitative research. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (pp. 1–45). Thousand Oaks, CA: Sage.

Dressman, M. (2007). Theoretically framed: Argument and desire in the production of general knowledge about literacy. *Reading Research Quarterly, 42*(3), 332–363.

Essed, Ph. (1991). *Understanding everyday racism: An interdisciplinary theory*. London: Sage.

Essed, Ph. (2001). *Toward a methodology to identify continuing forms of everyday discrimination*. Retrieved June 5, 2008, at: www.un.org/womenwatch/daw/csw/essed45.htm.

Gergen, K. J. (1978). Toward generative theory. *Journal of Personality and Social Psychology, 36*(11), 1344.
Gergen, K. J. (2001). Psychological science in a postmodern context. *American Psychologist, 56*(10), 803–813.
Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.
Glaser, B. G. (1978). *Theoretical sensitivity: Advances in the methodology of grounded theory*. Mill Valley, CA: Sociology Press.
Glaser, B. G. (1998). *Doing grounded theory: Issues and discussions*. Mill Valley, CA: Sociology Press.
Glaser, B. G. (2002). Constructivist grounded theory? *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *3*(3) (Art. 12).
Glaser, B. G., & Strauss, A. L. (1967). *The discovery of grounded theory: Strategies for qualitative research*. New York: Aldine.
Glass, K. C., & Kaufert, J. (2007). Research ethics review and aboriginal community values: Can the two be reconciled? *Journal of Empirical Research on Human Research Ethics, 2*(2), 25–40.
Grey, S. (2003). Decolonizing feminism: Aboriginal women and the global "Sisterhood". *Enweyin: The Way We Speak*, *8*, 9–2.
Habermas, J. (1972). *Knowledge and human interests*. London: Heinemann.
Habermas, J. (1974). *Theory and practice*. Boston, MA: Beacon Press.
Habermas, J. (1984). *The theory of communicative action (vol. 1): Reason and the rationalization of society*. Boston, MA: Beacon Press.
Habermas, J. (1996). *Between facts and norms: Contributions to a discourse theory of law and democracy*. Cambridge, England: Polity Press.
Hall, T. D., & Fenelon, J. V. (2009). *Indigenous peoples and globalization: Resistance and revitalization*. Boulder, CO: Paradigm.
Harris, L. D., & Wasilewski, J. (2004). Indigeneity, an alternative worldview: Four R's (relationship, responsibility, reciprocity, redistribution) vs. two P's (power and profit). Sharing the journey toward conscious evolution. *Systems Research and Behavioral Science, 21*(5), 489–503.
Holstein, J. A., & Gubrium, J. F. (1995). *The active interview*. London: Sage.
Hoppers, C. A. O. (2015). *Indigenous knowledge systems and the transformation of education in South Africa*. Presentation at the University of South Africa, 29 September.
Joas, H. (1994). Pragmatism. In W. Outhwaite & T. Bottomore (Eds.), *The Blackwell dictionary of twentieth-century social thought* (pp. 505–507). Oxford: Alden Press.
Kahane, A. (2012). *Transformative scenario planning: Working together to change the future*. San Francisco, CA: Berrett-Koehler.
Kazi, M. A. F. (2003). *Realist evaluation in practice: Health and social work*. Thousand Oaks, CA: Sage.
Knudson, S. K. (2015). Integrating the self and the spirit: Strategies for aligning qualitative research teaching with Indigenous methods, methodologies, and epistemology. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *16*(3) (Art. 4).
Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.
Lavia, J., & Mahlomaholo, S. (2012). Introduction: Imagining the postcolonial. In J. Lavia & S. Mahlomaholo (Eds.), *Culture, education and community: Expressions of the postcolonial imagination* (pp. 1–13). New York: Palgrave Macmillan.
Layder, D. (1993). *New strategies in social research*. Cambridge: Polity Press.
Lincoln, Y. S., & Guba, E. G. (1985). *Naturalistic inquiry*. Beverly Hills, CA: Sage.
Lyotard, J.-F. (1984). *The postmodern condition: A report on knowledge* (Bennington, & B. Massumi, Trans.). Minneapolis, MN: Minnesota University Press.
Maathai, W. (2006). *Unbowed: A memoir*. New York: Anchor Books.

Magnat, V. (2012). Performative approaches to interdisciplinary and cross cultural research. In L.-H. Smith & A. Narayan (Eds.), *Research beyond borders* (pp. 157–177). Plymouth, England: Lexington.

Marsden, R. (1998). The unknown masterpiece: Marx's model of capital. *Cambridge Journal of Economics, 22,* 297–324.

McIntyre-Mills, J. J. (2014a). *Systemic ethics and non-anthropocentric stewardship*. New York: Springer.

McIntyre-Mills, J. J. (2014b). *Transformation from wall street to wellbeing*. New York: Springer.

McLaughlin, J. M. (2001). *Accommodating perceptions, searching for authenticity and decolonizing methodology: The case of the Australia/Papua New Guinea Secondary School Students' Project.* Paper presented at the 29th ANZCIES Conference, Curtin University of Technology, Perth, 6–8 December.

Mertens, D. M. (2011). Mixed Methods as Tools for Social Change. *Journal of Mixed Methods Research*, *5*(3), 195–197.

Mertens, D. M. (2016). Advancing social change in South Africa through transformative research. *South African Review of Sociology, 47*(1), 5–17.

Merton, R. K. (1964). *Social theory and social structure*. London: Free Press.

Meyer, S. B., & Lunnay, B. (2013). The application of abductive and retroductive inference for the design and analysis of theory-driven sociological research. *Sociological Research Online*, *18*(1) (Art. 12).

Mingers, J. (2000). The contribution of critical realism as an underpinning philosophy for OR/MS and systems. *Journal of the Operational Research Society, 51*(11), 1256–1270.

Mugadza, G. (2015). Systems thinking and design thinking: Complementary approaches. *Systems Thinking World Journal: Reflection in Action, 4,* 1–7.

Newbury, J. (2011). A place for theoretical *in*consistency. *International Journal of Qualitative Methods, 10*(4), 335–347.

Oczak, M., & Niedźwieńska, A. (2007). Debriefing in deceptive research: A proposed new procedure. *Journal of Empirical Research on Human Research Ethics, 2*(3), 49–59.

Omi, M., & Winant, H. (1986). *Racial formation in the United States*. New York: Routledge.

Omi, M., & Winant, H. (2002). Reflections on "racial formation". In Ph Essed & D. T. Goldberg (Eds.), *Race critical theories* (pp. 455–459). Oxford: Blackwell.

Onwuegbuzie, A. J., & Frels, R. K. (2015). A framework for conducting critical dialectical pluralist focus group discussions using mixed research techniques. *Journal of Educational Issues, 1*(2), 159–177.

Onwuegbuzie, A. J., & Leech, N. L. (2007). A call for qualitative power analyses. *Quality & Quantity, 41*(1), 105–121.

Paavola, S. (2004). Abduction through grammar, critic, and methodeutic. *Transactions of the Charles S. Peirce Society: A Quarterly Journal in American Philosophy, 40*(2), 245–270.

Patel, R. (2015). *The role of reflective practice in educating about race, identity and difference*. Doctoral thesis, Manchester Metropolitan University, Manchester, England.

Pawson, R., & Tilley, N. (1997). *Realistic evaluation*. London: Sage.

Peirce, C. S. (1908). A neglected argument for the reality of God. Retrieved March 5, 2008, at: https://archives.library.illinois.edu/erec/University%20Archives/1515022/OriginalFiles/LITERATURE/PEIRCE/A%20Neglected%20Argument%20for%20the%20Reality%20of%20God%20-%20Wikisource.pdf.

Peirce, C. S. (1911). *The Peirce manuscripts*. Commens Peirce Dictionary (Retroduction). Retrieved March 5, 2008, at: http://www.helsinki.fi/science/commens/terms/retroduction.html.

Popper, K. R. (1959). *The logic of scientific discovery*. London: Hutchinson.

Popper, K. R. (1969). *Conjectures and refutations*. London: Routledge.

Portelli, J. P. (2014). Forward. In N. N. Wane, F. A. Akena, & A. A. Ilmi (Eds.), *Spiritual discourse in the academy: A globalized Indigenous perspective* (pp. xi–xiii). New York: Peter Lang.

Pratto, F., Saguy, T., Stewart, A. L., Morselli, D., Foels, R., Aiello, A., & Eicher, V. (2014). Attitudes toward Arab ascendance: Israeli and global perspectives. *Psychological Science, 25*(1), 85–94.

Price, L. (2005). Playing musement games: Retroduction in social research, with particular reference to Indigenous knowledge in environmental and health education. *Southern African Journal of Environmental Education, 22,* 87–96.

Puddephatt, A. J., & Charmaz, K. C. (2006). An interview with Kathy Charmaz: On constructing grounded theory. *Qualitative Sociology Review*, *2*(3).

Ragin, C. C. (1994). *Constructing social research*. Newbury Park, CA: Pine Forge Press.

Ragin, C. C., & Amoroso, L. M. (2011). *Constructing social research* (2nd ed.). London: Sage.

Redman-MacLaren, M., & Mills, J. (2015). Transformational grounded theory: Theory, voice, and action. *International Journal of Qualitative Methods, 14*(3), 1–12.

Reichertz, J. (2007). Abduction: The logic of discovery of grounded theory. In A. Bryant & C. Charmaz (Eds.), *Handbook of grounded theory* (pp. 214–228). London: Sage.

Rivombo, A. M. (2016). Using interviewing combined with lesson observation to enhance adult educators' reflections around addressing diversity in Soshanguve, South Africa. *South African Review of Sociology, 47*(1), 37–57.

Romm, N. R. A. (2001a). *Accountability in social research: Issues and debates*. New York: Springer.

Romm, N. R. A. (2001b). Critical theoretical concerns in relation to development. In J. K. Coetzee, J. Graaff, F. Hendricks, & G. Wood (Eds.), *Development theory, policy and practice* (pp. 141–153). Cape Town: Oxford University Press.

Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York: Springer.

Romm, N. R. A. (2013). Employing questionnaires in terms of a constructivist epistemological stance: Reconsidering researchers' involvement in the unfolding of social life. *International Journal of Qualitative Methods, 12,* 652–669.

Romm, N. R. A. (2017). Researching Indigenous ways of knowing-and-being: Revitalizing relational quality of living. In P. Ngulube (Ed.), *Handbook of research on theoretical perspectives on indigenous knowledge systems in developing countries* (pp. 22–48). Hershey, PA: IGI Global.

Romm, N. R. A., & Tlale, L. D. N. (2016). Nurturing research relationships: Showing care and catalysing action in a South African school research-and-intervention project. *South African Review of Sociology, 47*(1), 18–36.

Savolainen, H., Engelbrecht, P., Nel, M., & Malinen, O. P. (2012). Understanding teachers' attitudes and self-efficacy in inclusive education: Implications for pre-service and in-service teacher education. *European Journal of Special Needs Education, 27*(1), 51–68.

Sayer, A. (1999). *Realism and social science*. New Delhi: Sage.

Serper, A. (2015). *A future unfolding: The meaning of qualitative transformation in society*. Paper presented at Limmud, 1 August, Idaba Hotel, Fourways, Johannesburg.

Sidanius, J., & Pratto, F. (1999). *Social dominance: An intergroup theory of social hierarchy and oppression*. New York: Cambridge University Press.

Ssali, S. N., & Theobald, S. (2016). Using life histories to explore gendered experiences of conflict in Gulu District, Northern Uganda: Implications for post-conflict health reconstruction. *South African Review of Sociology, 47*(1), 81–98.

Stephens O. A. (2012). *Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, Nigeria.* Doctoral dissertation, Lagos State University, Lagos, Nigeria.

Stephens, O. A. (2015). *Recidivism, emotional intelligence and creativity thinking*. Saarbrücken: Lambert.

Strauss, A., & Corbin, J. (1994). Grounded theory methodology. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (pp. 273–285). Thousand Oaks, CA: Sage.

Tlale, L. D. N., & Romm, N. R. A. (2017). Systemic thinking and practice toward facilitating inclusive education: Reflections on a case of co-generated knowledge and action in South Africa. *Systemic Practice and Action Research.* doi:10.1007/s11213-017-9437-4.

Turner, J. H. (1991). *The structure of sociological theory* (5th ed.). Belmont, CA: Wadsworth.
Turner, J. H. (1998). *The structure of sociological theory* (6th ed.). Belmont, CA: Wadsworth.
Vannini, A., & Gladue, C. (2008). Decolonized methodologies in cross-cultural research. In P. Liamputtong (Ed.), *Doing cross-cultural research: Ethical and methodological perspectives* (pp. 137–159). New York: Springer.
Walker, P. (2001). *Imperial and colonial impact on research.* Paper presented at the Postcolonialism and Education conference, 17–19 August, University of Queensland, Brisbane.
Wane, N. N., Manyimo, E. L., & Ritskes, E. J. (Eds.). (2011). *Spirituality, education and society*. New York: Springer.
Wertz, F. J. (2011). The qualitative revolution and psychology: Science, politics, and ethics. *The Humanistic Psychologist 39*(2), 77–104.
White, L. (2003). The role of systems research and operational research in community involvement: A case study of a health action zone. *Systems Research and Behavioral Science, 20*(2), 133–145.
Wilson, S. (2003). Progressing toward an Indigenous research paradigm in Canada and Australia. *Canadian Journal of Native Education, 27*(2), 161–178.
Willis, P. (1977). *Learning to labor*. Columbia: Columbia University Press.
Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.
Woldegies, B. D. (2016). Research highlighting options for gender empowerment in Amhara Regional State, Ethiopia. *South African Review of Sociology, 47*(1), 58–80.

Chapter 8
Practicing Ethical Responsibility: Reconfiguring the Belmont Model

Abstract In this chapter I develop further my references to responsible social research practice which I have introduced in earlier chapters and I explore in more depth various aspects hereof. I link this to a discussion of the USA-commissioned Belmont report (1979). This report offers guidelines for the practice of ethics in biomedical and behavioral research, which have been highly influential in the way in which Institutional Review Boards (or ethics review committees) worldwide review/regulate the work of researchers proposing to conduct any kind of research involving human participants. The Belmont research delineates three principles and translates these into suggested applications in the realm of research ethics. The principles are "respect for persons", "beneficence", and "justice". In this chapter—with reference to various authors' engagement with this report and with reference to the illustrations which I extrapolate from Chaps. 2 to 6—I suggest ways in which these principles can be reconfigured. The reconfigurations are an attempt to make provision for alternative understandings (other than those encapsulated in the Belmont report) of how social research can be responsibly exercised. I also offer some thoughts on how Institutional Review Boards can be geared to operate with flexibility when reviewing proposed research projects.

8.1 Introduction

I start this chapter by referring to the Belmont report (1979).[1] As noted by Brooks, Te Riele, and Maguire (2014, pp. 31–32), this report, with its suggested applications of various ethical principles, has operated to inform the way in which Institutional Review Boards (IRBs) worldwide—that is, ethics review committees of universities and research institutions—review/regulate the research proposals of researchers proposing to conduct research involving human participants. As Brooks, Te Riele, and Maguire put it, "with some modifications, these principles have informed the development of guidelines for researchers across disciplines and

[1]It was named the Belmont Report, because it was drafted at the Belmont Conference Center, USA.

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_8

countries" (2014, p. 30). Or, as Bubar and Martinez state, "these … principles often form the bases of IRB protocols and human subject research regulations" (2017, p. 141).

The Belmont report was drafted by a national commission in the USA which was set up, inter alia, as a critical response to the way in which biomedical experimentation on human beings had been carried out during the Second World War by the Nazis.[2] The *National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research* was created in terms of a National Research Act passed in the USA in 1974. This Commission was tasked with identifying the basic ethical principles that should underpin the conduct of biomedical and behavioral research involving human subjects. It was out of this process that the Belmont report arose. The authors of the report clarify that:

> It [the report] is the outgrowth of an intensive four-day period of discussions that were held in February 1976 at the Smithsonian Institution's Belmont Conference Center supplemented by the monthly deliberations of the Commission that were held over a period of nearly four years. (1979, p. 1)

The Commission identified three principles, namely: (a) showing respect for persons, which implies respecting their right to decide on the basis of information about the research whether or not to participate in it; (b) minimizing/avoiding harm to research subjects and maximizing the benefits of research as a knowledge-producing enterprise; and (c) adhering to research "justice" in the sense of trying to ensure that some groups in society do not unduly bear the brunt of potential harms of research while others benefit from the increases in knowledge as produced (Belmont report, 1979).

Brooks et al. (2014, p. 31) provide a tabulation of how the Belmont principles can be seen to match up with those embraced by major research institutions regulating research on/with social subjects in the UK, Australia, Canada, Europe, and with the guidelines developed at the 2nd World Conference on Research Integrity held in Singapore in 2010. As part of the Singapore international congress, a "statement on research integrity" was developed.[3] Integrity is defined in the statement as that "researchers should take responsibility for the trustworthiness of their research" (point number 1). One of the suggestions in this statement is that "research institutions should create and sustain environments that encourage integrity through education [of researchers] and clear policies … while fostering

[2]As the authors of the report state: "The exploitation of unwilling prisoners as research subjects in Nazi concentration camps [has been] condemned as a particularly flagrant injustice" (1979, p. 6). They also express concern about how, in general across the globe, "during the 19th and early 20th centuries the burdens of serving as research subjects fell largely upon poor ward patients" (1979, p. 6).

[3]It is specified in the statement that it is not to be taken as "a regulatory document and does not represent the official policies of the countries and organizations that funded and/or participated in the conference". It is advised that "for official policies, guidance, and regulations relating to research integrity, appropriate national bodies and organizations should be consulted" (Singapore statement on research integrity, 2010, www.singaporestatement.org).

work environments that support integrity" (2010, point number 13). The suggestion here is that institutions have a role to play in trying to ensure that "integrity" is adhered to.

Now it is important to note (in terms of my argument in this book) that the Singapore statement also links integrity of researchers to their commitment to basing their research conclusions on "critical analysis of the evidence" and to "reporting findings and interpretations *fully and objectively*" (point number 3, my italics). In this statement, responsibility of researchers has to do with refraining from practices such as fabricating data, plagiarizing others' work, and using "misleading analytic methods" to develop results. Responsibility here implies not engaging in these "irresponsible research practices" and conversely engaging in research which is geared to being objective in the interpretation and reporting of "findings". As in the Belmont report, it is also considered important that "researchers and research institutions should recognize that they have an ethical obligation to weigh societal benefits against risks inherent in their [research] work" (point number 14). This has to do with weighing the potential risks for prospective research participants against the benefits to society of the knowledge that can be developed via the research process. Markedly, contention around whether in our efforts at knowing, "objectivity" (as trying to distill emotions and values out of the knowing process) should be striven for, is not acknowledged in the Singapore statement. What Kuntz (2015, p. 27) calls the "problem of distancing—of removal of the researcher-self from political and material contexts—… that methodologists have [critically] discussed for many years", is not mentioned: it is simply assumed that it is desirable to try to strive for such distancing. The quest for "more and increasingly 'efficient' distancing" (Kuntz, 2015, p. 27) is thus endorsed.

As I shall explain in Sect. 8.3.1, the Belmont report likewise assumes that researchers (or what it calls investigators) must gear their work to improving "knowledge", where the status of the knowledge resides in whether science (defined in terms of a biomedical model) has been properly practiced toward generating findings. Various authors (e.g., Austin, 2015; Bubar & Martinez, 2017; Castro-Reyes et al., 2015; Denzin & Giardina, 2007; Mertens, 2012) have, however, contended that the definition of "knowing" (as undertaken by so-called scientists) in the Belmont report is too narrow and have argued that the three located ethical principles also do not take into account other criteria for judging the ethicality of research undertakings.

In the light of the pervasiveness of ethical regulation by IRBs worldwide, Hammersley and Traianou suggest that it is important to consider the principles underlying such regulation. As they state, "given that 'ethics review' is now a requirement for all social research, it is important to be clear about the principles on which this ought to operate" (2014, p. 1.2). However, they do not side with authors such as Cram (2009), Denzin and Lincoln (2003, 2008), Denzin and Giardina (2007, 2009), Mertens (2007a), Mertens, Harris, and Holmes (2009), and Smith (2005), who have tried to introduce "alternative" goals for social research practice, such as goals of forwarding a more caring and just society. They argue that these authors' pleas to "transform the goal of research" (away from the conventional goal

of production of knowledge) are ill founded (2014, abstract). That is, they do not concur that the ethics of care as introduced by, for example, Gilligan (1982) and Noddings (1984), or the relational justice-oriented ethics propounded, inter alia, by Indigenous authors (e.g., Chilisa, 2009, 2012; Cram, 2009; Smith, 1999, 2005) should be guiding principles for research work.

Hammersley and Traianou argue that one of the reasons for *not* agreeing with advocates who propound what they call an "alternative ethics" as principles for guiding scientific research is that the concepts of "care" and "social justice" are too contested to serve as a basis for judging the worth of social research (2014, para 4.9). For example, in relation to the concept of social justice they argue that "there is considerable scope for disagreement about what would and would not be just" (2014, para 4.9). They assert that in any case "social researchers are not in a privileged position to determine what would and would not count as justice, in other words to interpret what this essentially contested concept implies for particular cases" (para, 4.10).

Regarding the question of including care as part of a research ethic, they suggest that research is not a caring profession as such, and that researchers should not consider their obligations as including attempts to "pursue or realize justice and care" (para 6.1), but should concentrate on the goal of research, which as they see it is to "produce sound knowledge" (para 6.2).[4] They proclaim that, as they express it, "there ought to be a division of occupational tasks and responsibilities" (in society), with researchers being responsible to practice research toward knowledge production (para 6.2).[5] Hence they would not query the Belmont report's understanding of the role of scientists in society as properly concentrating on trying to generate what they call "sound knowledge" (with the ethical provisos as specified in the report).

While not querying this, Hammersley's concern is that the increasing ethical regulation across the globe by IRBs implies an *undue control of social science researchers* based on an institutional distrust of them and of their capacity for

[4]Hammersley would not agree with the Singapore statement regarding scientists reporting "*fully and objectively*", as he recognizes that full objectivity may not be achievable (cf. Hammersley, 1995; Hammersley & Gomm, 1997). But he emphasizes that objectivity should *be striven for* by scientists (and also within the scientific community as a whole, as people engage with their colleagues' work and thereby also help to correct biases): see Romm (2001a, pp. 23–26) for my interpretation of his position. Hammersley's non-foundational position still adheres to the notion that scientific knowledge claims, albeit never fully confirmable, are *propositional* rather than *performative*. This of course implies a critique of authors such as Denzin (2001, 2003), Gergen (2015), Kuntz (2015), and Romm (2001b)—who point to the performative function of all language, including scientific discourse (as I mentioned in Chap. 1, Sect. 1.2.1, and elaborated upon in Chap. 7).

[5]Hammersley (1995) argues that if scientists wish to enter the action domain as part of some *other* role (other than the research role), they may indeed become involved herein (for example, as citizens). But this, he insists, is a separate domain from that of doing science.

ethical decision-making in the field (2010, para. 1.9).[6] He suggests that insofar as ethical review boards are set up, their status could be to *offer advice*, but *not to regulate* the practice of research (2009, p. 220). He proposes that ethical review boards can also serve as a forum in which "ethical principles and their application can be discussed, and they could be used to "initiate discussion about problem cases" (2009, p. 220). But he believes that the ethical discussion should not include considerations relating to felt obligations of researchers to reflect upon, as Ellis expresses it, "the role of connection and feeling in the principles that guide my [research] work" (2017, p. 58). These "alternative" principles, according to Hammersley, are not to be incorporated in ethical deliberations and can indeed detract from scientists recognizing and pursuing the goal of science, namely, the goal of striving to produce knowledge.

While Hammersley and Traianou do not take issue with the model of knowledge production encapsulated in the Belmont report and related ethical guidelines, others have insisted on revising the Belmont principles to make more provision for judging research efforts in terms of *different quality criteria*. Mertens (2012, p. 19) refers by way of example to how the vision of certain constructivist-oriented qualitative inquirers constitutes an expansion upon the Belmont norms. She notes how such researchers promote the need to include the following: balancing (of viewpoints); assisting participants (seen as, indeed, *participants*) to become more self-aware of their constructions; assisting participants to become more aware of others' constructions; trying to stimulate constructive action via the research; and assisting people to better address power relations (2012, p. 20). Here Mertens is pointing to how these normative criteria offer possible ways of justifying constructivist-informed research (which Hammersley & Traianou, 2014, in turn question). Mertens refers favorably to these ways of expanding the ethical principles given in the Belmont report; and she expresses the importance of continuing efforts to reconsider the Belmont ethical guidelines in the light of transformative and Indigenous paradigms (2012, p. 20).

In the USA, certain proposed amendments to the Belmont principles have been suggested by a national collaborative team devoted to revising them in order to cater for community engaged research practices (Castro-Reyes et al., 2015). In addition, Austin (2015, p. 34) notes that the Native American Research Centers for Health (NARCH) are fostering the development of infrastructures for community-based Participatory Research (PR). For instance, she indicates that a NARCH national project undertaken in 2011 resulted in "a resource for researchers, communities and IRBs" (2015, p. 35). (See https://www.ihs.gov/dper/research/narch.) In the context of Australia, The Australian Institute of Aboriginal and Torres Strait Islander Studies (AIATSIS) revised its guidelines for ethical research in Australian Indigenous studies in 2012. (The first version of these guidelines was created in 2002, and a second edition was published in 2010, revised in 2012.)

[6]In similar vein Monaghan, O'Dwyer and Gabe (2013) refer to Haggerty's (2004) term "ethics creep" to name the "growth of bureaucratic control" of researchers (2013, p. 66).

Austin points out that Australia's (2007) national statement on ethical conduct (produced by the National Health and Medical Research Council, and updated in 2013), states that guidelines for research with Aboriginal and Torres Strait Islander peoples are "based on values important to them: reciprocity, respect, equality, responsibility, survival and protection, and spirit and integrity" (2015, p. 35). And she cites developments in Canada where, since 2010, Canada's Tri-Council policy statement created by the Canadian Institute of Health Research (CIHR), now has a "chapter on research involving First Nations (Indian), Inuit and Metis Peoples of Canada, [and] includes recognition of Aboriginal perspectives on ethical research practice" (2015, p. 34).[7] As has also been observed by Putt, these various documents all try to make provision for research which is meant to be a collaborative effort involving communities and more specifically Indigenous communities, so that appropriate ethical protocol can be incorporated in ethical review processes (2013, p. 3).

In this regard it is also worth noting that when the authors of the Belmont report make reference to the three principles contained in their report, they remark that these are "generally accepted in *our* cultural tradition" (1979, p. 4, my italics). They do not lay down what they mean by "our"—but some of these principles have been queried on the grounds that they are Western-centric. For example, Castro-Reyes et al. question the focus on the autonomy of "persons" in the principle of "respect for persons" as if persons (as individuals) can be divorced from communities (2015, p. 5). And Bubar and Martinez argue that all three principles "are set up to protect individual interests". They claim that the principles thus "discriminate against and subjugate Indigenous ethics, knowledge, and practices" (2017, p. 141).

When considering the manner in which institution-based IRBs generally function (guided by the Belmont principles), Castro-Reyes et al. note that they do not make sufficient provision for a research commitment toward *community benefit*; nor do most institution-based IRB application forms seek *community perspectives* on what this might amount to (2015, p. 1, *my italics*). Castro-Reyes et al. state (2015, p. 7) that one of their concerns is that "injustices continue into the present day with the under-representation in research of whole communities such as Indigenous people, ethnic groups, and protected classes" (that is, those groups named in federal anti-discrimination law). When speaking about the definition of "community", they qualify that the notion of community can, for instance, refer to any group that "self-identifies by age, ethnicity, gender, sexual orientation, disability, illness or

[7]A statement released by the Canadian government regarding the second (2010) edition of the Tri-Policy statement (TCPS2) declares that "TCPS 2 is the culmination of over two years of dialogue with the research community on draft revisions issued in 2008 and 2009. The Panel conducted cross-country consultation tours and reviewed nearly 2000 pages of thoughtful comments from the research community" (cf. http://www.pre.ethics.gc.ca/eng/resources-ressources/news-nouvelles/nr-cp/2010-12-07/). Nonetheless, Stiegman and Castleden observe that some authors have criticized the processes involved in developing this chapter, on the grounds that "insufficient Indigenous community engagement or researcher consultation took place". But they point out that at least the intention of "this new chapter has been to provide a distinct framework for university-based Research Ethics Boards (REBs) to evaluate research protocols (2015, p. 2).

health condition". It can also refer to a "common interest or cause, a sense of identification or shared emotional connection, shared values or norms, mutual influence, common interest, or commitment to meeting a shared need" (p. 10). They furthermore point out that the phrase "the community involved in research" embraces the community that is participating in the research and/or the communities that are most likely to be affected directly by the research, its conduct and/or its findings" (2015, p. 10).

In the sections which follow I draw on some of these considerations as presented by Castro-Reyes et al., and I make the point that concerns which apply to community engagement with designated communities/community organizations *can be applicable to all research which is intended to be participatory and to cater for involved communities to benefit from the research*. To build up the discussion, I refer in Sects. 8.2–8.4 to the three Belmont principles in turn: I offer an exposition of how each one is conceived in the Belmont report and I engage critically with these conceptions with a view to reconfiguring them in the light of extended conceptions of the responsibilities of those involved in the research enterprise.

8.2 Reviewing and Reconfiguring the Belmont Report's Principle of Respect for Persons

8.2.1 The Belmont Report's Exposition of the Principle of Respect for Persons and Its Application

The authors of the Belmont report state that this principle

> incorporates at least two ethical convictions: *first*, that individuals should be treated as autonomous agents, and *second*, that persons with diminished autonomy are entitled to protection. The principle of respect for persons thus divides into two separate moral requirements: the requirement to acknowledge autonomy and the requirement to protect those with diminished autonomy [such as children, or those with mental disabilities that might impair their judgement]. (1979, p. 4, my italics)

Autonomy is defined in the Belmont report as the capacity of an individual to "deliberate about personal goals" and to act "under the direction of such deliberation" (1979, p. 4). Therefore "to respect autonomy is to give weight to autonomous persons' considered opinions and choices" (p. 4). To show *lack of respect* for an autonomous agent is to

> repudiate that person's considered judgments, to deny an individual the freedom to act on those considered judgments, or to withhold information necessary to make a considered judgment, when there are no compelling reasons to do so. (1979, p. 4)

In outlining the requirement to respect autonomy, the Belmont report states that

> not every human being is capable of self-determination. The capacity for self-determination matures during an individual's life, and some individuals lose this capacity wholly or in

> part because of illness, mental disability, or circumstances that severely restrict liberty. Respect for the immature and the incapacitated may require protecting them as they mature or while they are incapacitated. (1979, p. 4)

According to the Belmont report, such individuals therefore may need to be specifically protected, with the extent of protection depending on "the risk of harm and the likelihood of benefit" that might be accrued to them when participating in any proposed research (1979, p. 4).

Now the primary way in which the principle of respecting autonomy is applied in a research context according to the Belmont report is by suggesting that in cases where individuals are regarded as indeed able to develop considered opinions based on information at their disposal, potential subjects of research (as the report calls them) *need to be informed, wherever possible, about what the research is about, so that they can make an informed decision regarding whether or not to participate*. As stated in the Belmont report, "in most cases of research involving human subjects, respect for persons demands that subjects enter into the research voluntarily and with adequate information" (p. 4). It is recognized that there may be some cases where telling potential subjects beforehand what the research is about may jeopardize the ability of the research to find out what is being sought, because subjects' knowledge of the purpose may affect their responses. This is what is meant by stating that they need to be informed beforehand "wherever possible". The Belmont report states that in cases "where informing subjects of some pertinent aspect of the research is likely to impair the validity of the research … it may be sufficient to indicate to subjects that they are being invited to participate in research" (p. 7). But it is argued that in cases where people are not informed in depth beforehand, people should be debriefed as soon as possible thereafter (as I noted in Chap. 5 with reference to the deceptive experiment undertaken by Oczak and Niedźwieńska (2007).

In short, the principle of respect for persons in the Belmont report is operationalized into the requirement that potential research participants must be given the opportunity to consent to participation with *sufficient information regarding the research (and the potential risks and benefits to themselves and others) to be able to make an informed decision*. The exception is in research which arguably needs to contain an element of deception, in which case researchers can waive this principle temporarily, as long as it is judged that it will not be unduly harmful to the subjects, especially if they are later debriefed. And, for (potential) participants who are deemed not fully autonomous in terms of their capacity for self-determination, others will have to make a judgement on their behalf in order to protect them, in line with ethical principles of not causing them harm (1979, p. 4).

In discussing the application of the principle of respect for persons, the Belmont report summarizes as follows how this relates to informed consent:

> Respect for persons requires that subjects, to the degree that they are capable, be given the opportunity to choose what shall or shall not happen to them. This opportunity is provided when adequate standards for informed consent are satisfied. While the importance of

> informed consent is unquestioned, controversy prevails over the nature and possibility of an informed consent. Nonetheless, there is widespread agreement that the consent process can be analyzed as containing three elements: information, comprehension and voluntariness. (p. 6)

The Belmont report specifies that for subjects to concede voluntarily to participate in the research they must agree to participate without threat or undue inducement. This implies that undue incentives should not be given to subjects, who may then (especially if they are lacking in resources) decide to participate notwithstanding that they have their doubts about this. As the report puts it: "Also, inducements that would ordinarily be acceptable may become undue influences if the subject is especially vulnerable" (1979, p. 7).

In requiring that consent procedures are properly handled by researchers, Czymoniewicz-Klippel, Brijnath and Crockett note, somewhat critically, that standard procedures in IRBs recommend that researchers ask participants to sign an official form, which is often accompanied by a written explanatory statement outlining details of the research (2010, p. 333). They indicate that the focus of most IRBs on obtaining written documentation proving that the participant has been adequately informed and has consented on this basis, has to do with protecting the institution from liability. Mazonde and Msimanga-Ramatebele likewise state that "the ultimate aim of the [ethics] committee is to protect the rights of participants and the good name of the researcher *and the institution*" (2007, p. 192, my italics).

There is disagreement (in the literature and in practice) over the purpose of gaining informed consent, and accordingly on the process of securing it. For purposes of institutional liability, a signed letter of consent seems the most "secure" way of protecting the institution from later comebacks in terms of whether consent has been obtained. But if one is concerned more with developing good relations with participants when engaged in research in the social field, this may not be the most appropriate way to proceed in certain instances. Chilisa (2009, p. 421) expresses concerns regarding a signed "contract", which she argues, citing also Ellis and Earley (2006, p. 7), may be ill-fitted to more orally-oriented cultural contexts. Chilisa also notes that even when consent is given in written form, this does not mean that the participants are aware of what they have conceded to. She argues, for instance, that it may be unclear to participants whether they are giving consent to researchers to "describe the other [the participants] and then to … generalize the written story to the rest of the community unspoken to" (i.e., those not sampled in the research) (2009, p. 421). What is problematic for Chilisa is that even if participants "agree" that their stories (or information shared) can be told and represented by researchers, they may not be aware that researchers are then supposedly given the go-ahead to define the import of what they have heard/gleaned and can indeed sometimes do so in ways which might be harmful at the level of community, by, for example creating deficit discourses that are negatively impactful in the community. (See Chap. 1, Sects. 1.3.1, and 8.3.2.)

Crow, Wiles, Heath, and Charles (2006) contribute to debates around informed consent in the social sciences with reference to a research project that they set up in the United Kingdom which entailed interviewing various researchers involved in research with what are termed "vulnerable groups" about this issue.[8] From a qualitative analysis of the interview material, Crow et al. located an optimistic view of the way in which informed consent contributes to "data quality" as well as a pessimistic view. Crow et al. synopsize that within the "optimistic' scenario (as expressed by their interviewees, that is, the researchers in the field), it is considered that

> the trend toward researchers paying more careful attention to the issue of informed consent is contributing to better research than that undertaken in the less closely monitored and regulated past. (2006, p. 85)

Various arguments were expressed in this regard, and included claims that the process for ensuring informed consent

- helps to prepare researchers for the data collection process (in that it calls upon researchers to think about ethics and also to clarify what their fieldwork is intended to achieve);
- helps to prepare research participants for the data collection process (in that it gives participants more time to think about their responses/views);
- establishes a relationship between researchers and research participants in which research participants will (presumably) be more open and frank about the aspects of their lives that are being researched (in that those that volunteer to participate will be "appropriately interested and engaged" and also will feel "safer" in terms of the arrangements that have been set up with them); and
- is likely to improve participation rates (because people ought to feel more confident in research processes where they receive "convincing assurances"). (Crow et al., 2006, pp. 85–86)

But many of the interviewees' experiences in Crow et al.'s study pointed to a more pessimistic scenario around "the ways in which consent is handled, if not about the underlying principle of consent" (2006, p. 88). For example, according to the experiences of some of the researchers, organizing consent via formalized procedures may be understood differently by social groups—with people from what are defined as "ethnic groups" less likely to wish to contribute (because they were less likely to want to sign paperwork). In regard to participation rates, Crow et al. quote a health researcher who highlighted the dimension of ethnicity:

> the whole notion of informed consent is based upon this middle-class western sort of stereotypical concept of autonomy … And while we've gone to enormous lengths to get information translated into Urdu, Punjabi and things like that we're still finding that response rates for this type of mechanism are extremely poor. (2006, p. 88)

[8]They interviewed researchers who had been involved in research with "vulnerable groups", which included research with children, young people, older people, people receiving palliative care, people with learning disabilities and people with mental health problems (2006, p. 84).

In pointing to the "off-putting character of formal consent procedures" this researcher remarked that participation "was more likely to come through informal personal contact, … where natural conversations might lead in due course to the question of participation in research being raised" (2006, p, 88). Besides ethnicity which Crow et al. raise as a factor bearing on consent, they refer also to a researcher in their study who pointed to the "greater difficulties of gaining consent from people in poorer social groups". This tallies with Tyldum's (2012) reference to "systematic biases": Tyldum expresses concern that "groups with low human and social capital are less likely to volunteer in research"—and hence their voices remain unheard (2012, p. 199). She states that this poses challenges for researchers in the field in terms of "our search for representative or relevant samples" (2012, p. 208). In addition, Crow et al. point to another issue raised by one of the researchers in their study, namely, the possibility, as one researcher notes, that "powerful" people could utilize their power to "put up more restrictions in terms of what they might consent to" (p. 88). This excerpt from their article links up with Chilisa's (2009) point that when formal consent is gained from vulnerable people (who are not used to putting forward their own conditions/restrictions), this does not always mean the participants are clear on what they have consented to, or that they fully agree with the research terms.

In Crow et al.'s study, the process of obtaining informed consent by handing out information sheets and requesting people to fill in consent forms based on the information provided, was also seen by some researchers as "present[ing] obstacles to rapport between researchers and participants". For instance, Crow et al. cite the experiences of a researcher in their study who noted that as far as seeking consent in writing is concerned, this can be regarded as intimidating especially in the case of "researching … very excluded groups, … it's very threatening to ask someone to sign a form" (2006, p. 90). And they cite the observations of another researcher who "felt that signed consent introduced a quasi-legal element to the relationship" and risked putting participants "in a bit more oppositional kind of frame", getting in the way of establishing "the good relationships you want"' (2006, p. 90).

8.2.2 Further Deliberations

In 2011, a four-day collaborative workshop was held in Kilifi, Kenya in order to deliberate around issues of participant consent in the light of controversies around practices of obtaining it. This was reported upon mainly by Molyneux and Bull (2013).[9] Molyneux and Bull state as background information that:

[9]The authors of the report indicate that "given the length of the author list, we have only included biographical sketches for the two main authors: Dr. Sassy Molyneux and Dr. Susan Bull. Dr. Molyneux prepared the first draft of the paper and Drs. Bull and Molyneux amended it extensively. The remainder of the authors contributed to the ideas expressed in the paper" (2013, p. 13).

> Consent has long been considered as a core requirement for the ethical conduct of biomedical research. The concept is relatively clearly defined, with valid consent to research with competent adults entailing: (1) researchers adequately explaining the proposed study; (2) prospective participants understanding what is being proposed; and (3) prospective participants being able to make a free choice about joining the study (Nuffield Council on Bioethics, 2002). (2013, p. 2)

They argue (similarly to Crow et al., 2006) that despite the seeming clarity around "valid consent with competent adults", many research practitioners in the social field have reported on challenges experienced when trying to operate according to the practices of (1)–(3) above. They note:

> Challenges to achieving these components have been observed all over the world (Edwards et al., 1998; Flory & Emanuel, 2004; Mandava et al., 2012), and are potentially exacerbated in low-income settings by greater inequities in resources, power, and information among stakeholders in research (Nuffield Council on Bioethics, 2002; United States National Bioethics Advisory Commission, 2001). (2013, p. 2)

They state furthermore that "there are also differences in cultural practices around communitarian and individual decision making that can impact on the conduct of consent processes for specific participants" (2013, p. 2). A recognition of this may require seeking consent with trusted leaders in the community and/or members of community organizations/associations, and not only with involved participants at individual level.[10]

In response to the various issues around practices of seeking consent, the report on the Kenyan conference states that community engagement (CE) has begun to be promoted as a way of generating

> protection of, respect for, and empowerment of participant communities, and to improve the relevance and quality of research (Doumbo, 2005; Emanuel et al., 2004; Marsh et al., 2008; Marshall & Rotimi, 2001; Tindana et al., 2007). (2013, p. 2)

While referring to research that is community engaged, the delegates at the conference also recognized, in keeping with Tindana et al. (2007, paras 12–15) that:

> There is no universally accepted definition of CE. Descriptions range from efforts to simply improve information sharing and transparency in communities, through more active consultation, to initiatives aimed at ensuring greater control or partnership by community members (Tindana et al., 2007). (Molyneux & Bull, 2013, p. 2)

Broadly conceived, the report suggests that some form of community engaged research—defined as involving participants and setting out to empower participant communities—is a way of addressing/circumventing challenges arising from "ethical conduct of biomedical research" (2013, p. 1).

[10]As with contention around the formality of informed consent procedures at individual level, the formality of a "memorandum of agreement" also may not be trusted or wished for by the relevant community leaders/organizations, as pointed out to me by Francis Akena, having participated in the debate around this at a community engagement conference held in June 2016 at the University of the North-West, South Africa.

A similar point is made by Mertens (2012, p. 23) when she discusses under the heading of "respect" the way in which those committed to the transformative paradigm try to develop partnerships (as some kind of collaborative arrangement with participants/community members) or relationships (as longer term ongoing involvement with a community). What is emphasized by Mertens is that respect for people implies that *relationships of collaboration* are developed with those involved, such that the research becomes *participatory* at root.

Nevertheless, certain authors have cautioned that what this might mean in practice is also (as with the Belmont guidelines of obtaining informed consent) by no means clear-cut (cf. Austin, 2015, p. 26; Arieli, Friedman, & Agbaria, 2009, p. 277; De Koning & Martin, 1996, p. 3; Minkler, 2004, p. 687). De Koning and Martin indicate that there are various interpretations of Participatory Research (PR); but what is important is that participation—in whatever way it is handled in practice—"should genuinely be empowering and not just a situation where local people work with a researcher for the latter's convenience" (1996, p. 3). Higginbottom and Liamputtong add that PR in its variety of types "provides opportunities for individuals, groups and communities to actively participate and engage in the research process" (2015, p. 2). This, I suggest, is how one can conceptualize the research work that I reported upon in Chaps. 2–6, as I elaborate further upon in Sects. 8.2.3.1–8.2.3.5.

8.2.3 *Respect as Enabling Opportunities for Novel Modes of Research Participation*

In considering respect in terms of a commitment to develop participatory relationships, ideally (as noted in Chap. 1, Sect. 1.3.4) relationships need to be established so that participants can be involved in formulating the issues to be researched, ways of doing the research, ways of analyzing "findings", ways of involving stakeholders, and ways of defining the import of the results in terms of options for action. However, in this section I make the point that the various parties' contributions can emerge over time, which means that the *research relationships are in process of development.* Ross reminds us that "research relations are social relations that, like any others, change and shift" (2007, p. 30). As part of this dynamic, researchers/co-researchers can nevertheless try, as Denzin and Lincoln put it, to "participate in a shared agenda [and] come together in a spirit of hope, love and shared community" (2008, p. 2). I have suggested by way of the examples that I discussed in Chaps. 2–6 that the research space could indeed become used as an opening for those concerned with the issues under consideration to co-consider—in a spirit of hope—options for enhanced social justice (including at times ecological justice).

Hammersley and Traianou argue, for their part, that they do not find that authors such as Denzin and Giardina (2007) and Denzin and Lincoln (2008) have given any

indication of how the quest for research work to be done in a spirit of love, can be reconciled with, for instance, "an ethic of resistance" to intransigent power and structural oppression as felt by certain social groupings (2014, para 3.6). They also aver that there are likely to be conflicts that arise between some researchers and some (members of) Indigenous communities, arising because "the social relations traditional within at least some Indigenous communities are open to challenge on grounds of sex and age discrimination" (2014, para 3.6). They suggest in addition that besides these conflicts (for which they cannot see that those espousing and ethic of care and justice are offering any resolution), there are questions about "the practical feasibility" of what is proposed in the "alternative ethics" as they term it (para 3.7).

Hammersley and Traianou thus question the coherence and the practical feasibility of operating in terms of an ethic of care and justice. However, in response one could suggest that the conflicts which Hammersley and Traianou detect between the various ethical principles supported by those espousing the "alternative ethics", can be reframed; and accordingly options for proceeding in situ can be worked out in practice. This is especially when we recognize, as emphasized by Mertens (2007, p. 86) in her discussion of the transformative paradigm, that the key participants and target beneficiaries that research is geared to serve are the *most vulnerable and the historically marginalized*. This would be consistent with a definition of ethics (including research ethics), supplied by Bongmba when he states that "ethics has come to mean a way of life that has sought justice in a world where many have been oppressed and humanity has ignored, or abused, the environment" (2014, p. 33).

This would then allow, for instance, that Roman-Alcala's (2015) reporting on the OTF resistance to what was seen as the structural violence of (capitalist) market systems generating food insecurity, is in keeping with a care for those considered most exposed to its consequences, as well as for the natural environment. And Maathai's (2006) indication of how she struggled against women's discrimination (while supporting efforts to mitigate poverty as well as environmental degradation), also offers an account of how one can operate to serve those most vulnerable, through research and learning in action. This did not stop her looking for leverage points for the project from a range of people and organizations with (more) social power who could also be seen as "caring" (as Mayan and Daum also note is a feasible strategy, 2014, p. 87). Woldegies's (2014) indication of how he encouraged discourses which support partnerships in gender relations offers a further indication of how professional researchers together with others (including those historically marginalized as well as those with positions of more power who are willing to engage) can find ways of researching which encapsulate a spirit of care and justice.

Hammersley and Traianou declare that they cannot see that there is a commitment to dialogue (in the academic community) by authors espousing the "alternative ethics" (2014, para 3.5). In response to this, it could be said that those forwarding this alternative may find it difficult to dialogue with authors such as Hammersley and Traianou who promote so strongly their own position, namely the ethic that "*what ought to be the central concern* of all social researchers [is to] produce sound knowledge" (para 6.2, my italics). This is a strong statement, which

seems to me to occlude discussion around "what ought to be the central concern of researchers". Furthermore, it does not invite dialogue around the question of how "sound" processes of knowing are to become defined. According to authors who espouse an "alternative ethics" (e.g., Cram, 2009; Cram & Mertens, 2015; Denzin & Giardina, 2007; Gergen, 2009, 2015; Kuntz, 2015), the performative effect of our discourses can *operate behind our backs* if we disregard the way in which framings of research questions and issues already enter (and impact upon) the social and ecological life being "researched". In the light of arguments such as those presented in Chap. 7 concerning the *making/shaping* of the world in the process of its being researched, it could be said that the quest to use the *research endeavor to strengthen conversations and actions geared to justice* "ought to be the central concern of social researchers" (or at least should be seriously considered).

Bearing this in mind, in the following sections I point to/extrapolate some of the ways in which a research space for people to contribute to options for knowing and acting became instantiated in the work discussed in the previous chapters of this book. Before I begin, it is worth re-iterating that I am following the structure of the Belmont report in separating out analytically the discussion of the principle of respect from the other two principles—beneficence and justice. In practice there are connections between them, especially when they become expanded. Castro-Reyes et al., for example, affirm that the principles (as extended) "are often all required in overlapping ways during the course of research" (2015, p. 3). Below I explore various nuances of an expanded principle of respect, with reference to the examples showcased in Chaps. 2–6.

8.2.3.1 Some Ethical Reflections on Chapter 2: Respect as Implying Ongoing Consent and Ongoing Development of the Research Endeavor

In the work reported upon in Chap. 2, I suggest that consent was ongoingly accomplished as part of a developing relationship between myself and the various participants in which the form of our research interaction and its purpose(s) became ongoingly (re)defined. I focus below on a few pointers, for readers to further engage with, also by comparing my discussion here with the text in Chap. 2.

As I indicated in Chap. 2, Sect. 2.3.2, consent of participants to participate in the cross-racial focus group (FG) meeting/talking circle in the apartment complex in Margate (South Africa), was not based on my explaining from the start exactly what the process would involve, as I myself was not sure how it might pan out. It was a matter of participants agreeing to participate based on my having established some relationship of trust with them (as they knew me as a friendly resident) and based on my suggestion that we could develop some insights about racism via the group meeting. Further to this, they participated in defining how our research interaction (between participants and myself) would continue. The participants' consent to participate in the FG was followed through with their consent to further participate in additional conversations where they could share views and concerns.

The setting up of informal interviews/conversations with participants to ask them about their experiences of the FG meeting arose from my having indicated to them (directly after the FG meeting) that I was interested in how they had experienced it. As it happened, in all cases, the participants used this as an opportunity to speak further about issues of concern to them, and the interviews/conversations (with one group encounter) proceeded as in a focused conversation.

I of course appreciated the understandings that the participants brought to bear during our conversations and I also took seriously the workers' (implicitly expressed) hope that perhaps I could carry forward in a small way the concerns that they had expressed regarding low wages. This compares with Cram and Mertens' indication that "compassion" is a force which "compels researchers to engage in activist research" (2015, p. 99). As I have explained in earlier chapters, for me "active research" likewise means that the research space is used to try to make a difference to the experienced quality of life. And, as indicated by Mertens (2007, p. 86), one should consider one's responsibilities as linked to obligations to serve those most marginalized in the social order, in this case the Black workers.

With the White participants, their consent emerged as they agreed not only to participate in the FG meeting but also (as they later agreed) in interviews/conversations after the FG meeting. In the case of Koos and Lynette we spoke mainly about their childhood experiences with racism.[11] And they were willing to further continue to engage with my deliberations (sometimes critical) of how I understood the interviews in the light of some additional literature on which I drew. I used the literature as an entry into raising some critical questions, which they "answered" in their own ways. Nevertheless, I took some responsibility—for the sake of wider discourses and generative theorizing—to engage critically with their responses. (I also, with the help of critical reader's Francis Akena's input, set up additional storying at the point of the write-up in Chap. 2, around the question of how "race" and "class" might be theorized.

While Chilisa might argue that the participants had not consented to the way in which I chose to handle the write-up in this book, I would suggest that professional researchers, for the sake of widening discourses in the "academic" domain, and as part of their attempt to share their understandings with readers, can legitimately offer extrapolated theoretical inferences. This is provided that, as in this case, readers are alerted to how the extrapolations were accomplished and also to the fact that these are *inferences* rather than statements presented with any "authority". (See Chap. 7, Sects. 7.3 and 7.5.1 for my discussion on this.) In addition, I have tried to be careful that the theorizing as proffered—based on drawing out implications of participants' statements—has a chance of contributing to further hopefulness in the social fabric, in the spirit of what Chilisa calls the Indigenous Research Paradigm (see Chap. 1, Sect. 1.3.5).

[11]It will be recalled that Koos and Lynette were not residents in the building. I also continued to have further conversations with my neighbors in the building, some of which material I incorporated in my personal narrative in Chap. 2, (Sect. 2.2.1).

The kind of consent that I secured during the research was orally sought as I had no institution asking me to collect signed forms (or to seek proof of having obtained consent orally).[12] I had taken off some years in order to write material, inter alia, on racism. Later when I went to work in the College of Education at Unisa (2012), I spoke to one of the Indigenous professors about being attuned to oral tradition and encapsulating this in IRB discussions around consent, and he said that in oral cultures there are always ways by which one can "prove" that consent has been given, albeit not in terms of signed documents. (I refer further to my involvement with the Unisa ethics review process since 2012 in Sect. 8.5.1.) In any case, in this research which I undertook while resident in Margate, the consent that was given by participants was an ongoing process, which also included their participating in defining the contours of the research process and ways of speaking about the operation of race and racism in the contexts with which they were familiar. The generative theorizing which I proffer in Chap. 7 can be said to have been grounded in, while extrapolating upon, their various contributions. (See Chap. 7, Sect. 7.3.)

8.2.3.2 Some Ethical Reflections on Chapter 3: Respect as Expanding upon Initial Consent Obtained According to IRB Requirements

In the international project reported upon in Chap. 3 (comparing teachers' attitudes to inclusive education in six countries), the consent procedure adhered to the guidelines as given by the Unisa IRB, as well as by the various other participating universities, and was in accordance with the application of the principle of respect for persons as laid out in the Belmont report. I myself was not party to the beginning phase of the project (the questionnaire administration); but when we handed out the consent forms for participants of the FG sessions to fill in (after they had previously on a separate occasion been told about the project by one of the team members), I felt somewhat embarrassed when the primary facilitator (Norma Nel) —after repeating the purpose of the FG session that day—asked them to please read the form and place their signature on it if they agreed. She too was somewhat embarrassed and she apologized for asking them to do so, but mentioned that our university required their signature and that it was just to be kept in an office. (We also re-iterated that names would be kept anonymous in any reporting and that information divulged would be kept confidential.) The participants took time to read the forms and then signed them. It took some time thereafter to develop the necessary informality required by the FG conversation. What was interesting is that although we followed our university IRB requirements for securing consent, just as Chilisa notes (2009, p. 491), nowhere in the consent form did it state that we as

[12]Actually, I am not sure at the time if universities in South Africa were as regulated in terms of research ethics as I found that, for instance, Unisa had become when I started again to work full time there in 2012.

researchers would have the right to interpret the questionnaire or FG data according to our presumed expertise; nor did it state what participants might expect in terms of a relationship of reciprocity.

During the FG sessions, we explained (in non-jargonized language) that we did not regard any results from the questionnaire analysis as sacrosanct; and we wished to use the FG sessions partly to check/revisit with the teachers how it felt on the ground to be involved in inclusive education. In other words, we signaled to them that the FG sessions might cast different light on the "results". We also explained that we wished to treat the FG sessions as an encounter in which we all could learn from one another. What the informed consent form called a FG interview, was thus shifted/expanded when we pointed out that we did not regard the process as an *interview*, but that we were hoping that it could become a *learning encounter* for us all.

What occurred when we sought feedback on how they had experienced the FG session, was that the participants expanded the remit of the FG session yet further, to include suggesting to us that we needed to "carry the baton" forward. They felt it important that while they had consented to participate in the FG sessions, *their doing so was tied to their hope that something additionally meaningful would accrue*. As it happened, we not only carried the baton for them, but set up a meeting with some district officials and the head office, so that they could themselves (with us acting as mediators at times) explain their concerns. The point I wish to make here is that the participants had consented to the standard requirements of the IRB, *but they would have been disappointed if we had simply stuck to these ethical requirements*. The so-called respect shown by asking them to fill in a consent form was *not what they found important as far as their involvement with the research was concerned*. It was as part of what could be said the building up of a trusting environment during the FG session, that a reciprocal relationship begun to form, and we extended this reciprocity by appreciating their request that the baton be carried forward in some way.

In the national (500 schools) project, we followed the IRB requirement to obtain necessary consent from participants (as well as permission from the relevant provincial departments in South Africa and from principals of sampled schools). African language speaking research assistants went out on visits to further explain to the principals and teachers what the research entailed. They were then shown the questionnaire and asked if they would agree to fill it in. They were informed that the idea of the research was to develop aggregated information and suggestions for "making schools better". And they were informed that some of the schools would be further sampled for FG discussions and additional visits. (Our consent letters for FG participation were handed to participants from sampled schools only later.) Again, as with the international project, despite our efforts to explain that the purpose of the project was to examine the causes of school underperformance with a view to making schools better, exactly what the participants were consenting to in terms of how we might use the information (possibly to draw out their school's weaknesses) was not evident. It required further ethical deliberation on our part to

try to ensure that deficit thinking around school underperformances would not be reproduced via the research. (See Chap. 3, Sects. 3.4.1 and 3.4.4.)

In setting up the FG sessions, we were required in terms of IRB requirements to obtain signed consent forms. However, when creating the ethical clearance application for the IRB we had not taken into account that many of the prospective participants would not be literate. This was the case especially with parents/guardians, many of whom in rural areas had not been to school during the apartheid era. We therefore had to think innovatively. I suggested that it could be recorded on tape that they had agreed to participate (including that they had agreed that the FG session as a whole could be recorded). This would mean that we would be "covered" in the very unlikely event that the university ever needed proof that they had consented. Treating people with respect and dignity in this case meant waiving the formalities of documentation. I later heard from other teams that they too discovered on the spot that when they showed people the consent forms, some of them were not interested in signing (even though they may have been literate). Hence the researchers had to "put aside" the forms so that they could proceed with the research. (In such cases again, there would have been ways of proving that the participants had agreed, not least because they agreed that the whole conversation could be taped, and this was on oral record.)

To return to the issue of consent as linked to respect, as in the international project many of the participants used the opportunity of our asking for feedback (both in the questionnaire and as part of the FG sessions) to explain to us that *the time they had spent would be justified if some follow up was made*, by, for example, liaising with the government, visiting their schools to further discuss matters, setting up further meetings including district officers and school governing body (SGB) members, etc. A reading of their feedback from the (transcripts) indicates that they would have felt that their consent given to spend their time participating needed to be matched by a commitment from our side toward activating some action implications as mediators/catalysts. As indicated by many Indigenous authors and as summarized by Castro-Reyes et al. (2015) the point of social research, for it to be considered ethical, is not merely that participants have the opportunity to offer information or narrate experiences/views. The giving of consent to participate is rightfully accompanied by an expectation that the communities of which participants consider themselves a part should be left in some ways better off via the research. I return to this issue in Sect. 8.3.3.2.

8.2.3.3 Some Ethical Reflections on Chapter 4: Respect as Supporting Dignity and Empowerment at Individual and Community Levels

Woldegies's Way of Supporting Dignity and Empowerment

Woldegies refers in this discussion on ethics (2014, 2016) to his obtaining informed consent by generating interest in the research in the community which was to be

involved (in a region of Ethiopia). His way of seeking consent was in line with various authors who have called for community-geared consent procedures, rather than following the standard procedures for obtaining consent as encapsulated in the Belmont report. Woldegies's account of how he proceeded is summarized in his discussion on ethics in his article prepared for the special issue of SARS on research directed toward social development (2016). The process of his seeking consent was conducted in such a way that the purpose of it would be known in the community and that a "buy-in" could be obtained. He elucidates that:

> Besides obtaining written permission from the Wadla Woreda Administration, I conducted a consultation meeting with the stakeholders, which included government, civil society organizations (CSOs) and faith-based organizations (FBOs), so that they would be aware of the research and so that I could later approach them again to share and discuss the results. Every interview and discussion was preceded by explaining to the potential participants what the study was for. (2016, p. 65)

As regards his obtaining consent from the specific participants who were sampled, he indicates that:

> In all localities/villages, therefore, informed consent from the prospective informants [participants] and group discussants was obtained only after in-depth discussions with the prospective participants. Permission was also obtained from individuals and groups in order to take notes during the conversations and take short introductory audio recordings of conversations to comply with the local cultural beliefs. (2016, p. 65)

Woldegies indicates that in the cultural context of the research, taking recordings of the whole conversation would be considered invasive by the participants; hence he and his (female) assistant recorded only the introductory sections of the conversations and relied on note-taking for the rest.[13]

As regards the "ownership" of the stories told, he refers to Chilisa's (2012) suggestion that participants should not feel that their stories are being stolen by researchers in anonymous write-up:

> Chilisa (2012) explains that this is important when doing research work with research participants in terms of Indigenous research methodologies, so that people do not feel that their stories are being used without the researcher's acknowledgement. Indeed, they were very happy having their pictures taken with their work, so that the practice of "'anonymity" (whereby they become anonymous in the research reporting) did not apply. (Woldegies, 2016, p. 65)

He explains that

> all of the women preferred to participate using their real names. Generally, the information they provided was known in the community as it related to their economic activities and

[13]Kovach notes, though, that in many contexts of research with Indigenous communities, tape recorders are seen (by both researchers and participants) as the best way of ensuring that the participants' expressions "come through as truly as possible" (2009, p. 128). Transcription is then seen as "protecting the words of the research participants" (2009, p. 128). However, other authors have noted that this is not always fitting in certain cultural conrtexts. See McKay and Romm (2015, p. 443) for further discussion on the issue of taping.

> sense of community participation. Assurance was given to the research participants that any private issues and information gathered would not be disclosed to any individuals (including their spouses) or organizations without their consent. (2016, p. 65)

He therefore clarified to participants as part of the consent procedure that what they considered to be "private issues" would not be divulged in any reporting.

As I pointed out in Chap. 4, Woldegies states that he parted company with the standard Western-oriented position of not offering participants what might be called undue "incentives" or inducements to participate. (The definition of what might be considered reasonable in terms of offering any incentives is left somewhat open in the Belmont report—see Sect. 8.2.1.) Woldegies highlights that he recognized that it *would not be appropriate to desist from offering some form of financial incentive in this context*:

> The participants were provided with an incentive in order to conform to the existing culture. This is done usually to help build up the relationship and say "thank you" to them. This is seen as a sign of respect and part of a tradition, which recognizes their contributions to the research. (2016, p. 65)

He goes on to further explain his reasons for this:

> A financial incentive was offered to the research participants as compensation since the women spent part of their working time in the research. I paid a per diem of about $3 per head to the women and their support groups who participated in the study. In some cases, the researcher had to buy drinks (homemade brews, tea, coffee, etc.) for participants and had to provide financial assistance when an urgent need was noticed. (2016, pp. 65–66)

His reference to providing food echoes my reference to this in Chap. 2, (Sect. 2.3.2) and Chap. 3, (Sect. 3.4.4). Liamputtong too refers to experiences of the need to offer food when conducting focus group sessions in certain settings. She gives examples of research with Latina women, Emirati women, immigrant participants in Canada, Aboriginal communities, Mexican migrant farmworkers, and Pacific Northwest Indian communities (2011, p. 139).

As far as Woldegies's reference to offering further financial assistance when an "urgent need was noticed" is concerned, other authors have also highlighted the importance of displays of generosity (for example, in the form of gifts). Hence Smith indicates that for many Indigenous peoples "to have something worth sharing gives dignity to the giver. To accept a gift and to reciprocate gives dignity to the receiver" (1999, p. 105). Liamputtong adds in this regard that gift packages to express gratitude and respect, which may or may not include financial compensation, are culturally significant in many cultural contexts. Researchers need to take this into account, while also seeking other ways of giving something back to research participants (for example, by sharing information which may be meaningful to them)—so that participants can "see that the researchers have genuine concerns about their lives and wellbeing" (Liamputtong, 2010, pp. 80–81).

As I detailed also in Chap. 4, Woldegies organized the research with participants in such a way that the research could contribute to women's empowerment (as a marginalized social group in the society). He states that the interview process itself could be empowering in that while he used a semi-structured format for the

individual interviews in order to be able to compare responses, the questions could at the same time *inspire the participants' rethinking about their agency and also their agency as part of their communities*. Furthermore, provision was made within this format for the participants to tell their stories; and as it turned out, it was suggested by the participants in conversation with Woldegies that these stories could serve to inspire further empowerment for other women (as spread through women's organizations). In this way the women gained "respect" via the research process, as they contributed their stories and their suggestions for how these could be used. The process of their participating was tied to an understanding, shared by them and Woldegies, that the research should make a contribution to their and others' quality of life.

The FG sessions were also handled in what could be called a respectful way, as participants could add their various suggestions into the discussion, including suggestions for how the research results could be rendered more practical, with Woldegies playing a mediating role with other stakeholders not included in the FG sessions. (See Chap. 4, Sect. 4.2.5.) According to participant expectations, Woldegies constructed reports that encapsulated a synthesis of the participants' suggestions, recognizing that their "consent" to participate in the project had been tied to his stating to them that the information that they had co-produced via the research would be made available where it was likely to count. He also wrote his doctoral thesis detailing the entire process and adding further theoretical deliberations, as part of what could be called a generative theorizing, in ways which might serve to generate empowerment in other contexts too. (See Chap. 4, Sect. 4.2.6.) That is, he created draft reports that could be discussed with participants and with community stakeholders, and he also shared certain insights with "the world at large", as Magnat (2012, p. 168) puts it, for consideration.

Ssali and Theobald's Way of Supporting Dignity and Empowerment

In Ssali and Theobald's account of their initiative to organize life history interviewing as a way of researching options for post-conflict reconstruction in Uganda, they explain their decision to use life histories with empowering intent. They refer to Goodson and Gill (2011) as suggesting that:

> Life histories … are not only about life as lived and told, but also life as imagined, which can then be leveraged by respondents and their leaders to improve their situations. The ability to imagine … enables people to exert agency and think of alternative futures which are different from those they experienced and/or are experiencing now. … The ongoing challenge is in rapport and empathy, in trying to ensure life histories are an empowering process rather than an extractive one, an opportunity for people to share their stories with dignity. (Ssali & Theobald, 2016, p. 95)

Ssali and Theobald indicate that they chose the life history approach because they felt that it would enable participants to "share their stories with dignity", with the participants controlling what they chose to disclose to the researchers in their narratives. Showing respect for participants meant that *the research process should*

be a dignifying process for these participants. Hence, instead of controlling the interview structure, they simply asked the participants "to narrate the story of their life as far back as they could remember, from whichever year, as long as it was before the war" (2016, p. 86). Kovach likewise argues in her discussion of Indigenous methodologies that "an open-structured conversational method shows respect for the participant's story" (2009, p. 124).

Besides trying to accord dignity to participants via the life history interview setting, they also tried to enable the stories to be heard in wider circles, by acting as mediators in this regard. They therefore worked closely with The ReBUILD Consortium's networks with policy makers (local and national). From the life histories (as synthesized by them) "a number of policy briefs, blogs and videos were produced and shared with local, national and international policy makers (2016, p. 95).

As with Chaps. 2 and 3 where I noted that many participants expressed implicitly or explicitly that their consent to participate in research should be matched by an active effort on the part of the initiating researchers to stimulate/catalyze "action", we see here that Ssali and Theobald were aware of such an obligation (as was Woldegies). That is, they understood that participants' consent to participate came with expectations that the research material would be likely to be used to good effect.

Ssali and Theobald touch on the issue raised by Chilisa (albeit not mentioning her) that in seeking participants' consent to participate, it is often not clear to participants quite what they are consenting to in terms of how the write-up/synthesis of their various stories will be accomplished. Ssali and Theobald indicate that this posed a dilemma to them as they were aware of the need for researchers to "think critically and carefully about our [their] role as researchers in the research endeavor" (2016, p. 95). In their case they used ATLAS.ti software (a tool for qualitative data analysis) to help them to organize their analyses (2016, p. 86). However, they note, referring to Goodson and Gill (2011), that they consider that life history storying involves "a collaborative interpretation between the teller (participant) and the listener (researcher)" (p. 95). They therefore felt it important to try to do justice to the stories as told/shared during the research encounter. This idea is consistent with Kovach's statement that the write-up of stories (which she calls contextualized analysis) should be directed to "take the reader to a particular behavior or particular events", while also demonstrating cultural knowledge (p. 131).

Ssali and Theobald state that they had to make choices concerning

> how much do we highlight suffering (victimhood) and how much do we highlight resilience (survival) in the face of extreme hardship? How can we ensure we are presenting others' stories how they would like to be told rather than "othering" arguably vulnerable groups. (2016, p. 95)

Another question which posed dilemmas to them was how they might "highlight gendered realities in a given context without exacerbating gender stereotypes". As they state:

> Both women and men experienced gendered violence, suffering and extreme violence; and we have highlighted this in our analysis but in particular showed the experiences of male vulnerability as these are largely absent in discourse, analysis and the post-conflict reconstruction effort and addressing this is arguably key to both women and men's health. (2016, p. 96)

As the initiating researchers they therefore felt that they had to make (responsible) decisions, bearing in mind the impact on discourses and on attendant possibilities for developing post-war reconstruction. They did so to cater (in theory and practice) for those who could be regarded as "largely absent" in everyday discourses concerning post-war reconstruction, and hence who can all-too-easily become neglected in the practice of social reconstruction.

They also indicate that they had to be careful not to breach confidentiality:

> The strength of life histories lies in the depth, detail and nuance of the participants' stories: their own words detailing their own experiences. We ensured confidentiality in the consent process prior to the life histories and needed to ensure this is adhered to, raising dilemmas of the richness, depth and detail that can be provided in research dissemination and uptake activities and written outputs such as this paper. We went through each output carefully to ensure confidentiality was not breached, and this is particularly critical with vulnerable groups discussing their experiences of conflict, including for example male rape, within a homophobic context. (2016, p. 96)

Again, as with Woldegies (2014, 2016), they had assured participants confidentiality as part of the consent process; and they indicate (2016) that they took this obligation seriously. (Woldegies mentions that there were some points at which the participants clearly wished some disclosed information to remain confidential, but there were parts of what they said which they did not wish to remain anonymous in the reporting. This was so that they could act as role-models within the community; hence they themselves suggested that pictures of them and their projects should be included in the reporting—see Chap. 4, Sect. 4.2.4.)

8.2.3.4 Some Ethical Reflections on Chapter 5: Respect as Recognizing (and Activating) Potential in Persons and Communities

Stephens' Treatment of Recidivists as Potential Learners

Stephens offers some detail on how, before seeking consent from participants, he needed to get permission from the controller of the prisons in "Lagos State Command … in order to obtain approval to use the inmates of the [targeted] … prisons for the study" (2012, p. 125). To this end, he asked the Department of Educational Foundations and Counselling Psychology at Lagos State University to create a letter of permission. Approval was granted as a result of this letter and the officers in charge of these prisons were copied. This was followed by meetings with the officers in which he explained further "the modalities and the likely benefits of the study" (2012, p. 125). Thereafter, the researcher was handed over to the officer in charge of the welfare department and officer in charge of prison records" (2012,

p. 125). And further to this he invited a randomly chosen sample of recidivists (from the list of recidivists given to him by the officer) to a hall to explain the purpose of the project to them. People were thereafter given the opportunity to decide whether to stay in the hall or not, depending on whether or not they wished to participate, so that it was clear that they had the right not to participate (and that participation was voluntary, as specified in the Belmont report). Stephens also explained to them their right to withdraw at a later date if they wished. Nonetheless, Stephens stated to me in personal conversation (9 February 2015) how his succeeding to secure the initial and ongoing consent of many of the recidivists was through the fact that they trusted him as a result of previous involvement as a religious minister, a professional counsellor, and the CEO of an NGO called Cottage of Hope—which had assisted them in various ways.

Stephens indicates (2012, p. 128) that during the training sessions he re-iterated to participants that the option to "opt out" was open to them at any time. This was part of his continued effort to *ongoingly obtain their consent, based on their experiencing that the research was meaningful to them*. In itself, I would suggest that *seeing the recidivists as having the potential for learning* would already have been seen in this context as a respectful encounter, which it seems to me (from my interpretation of his accounts) that they appreciated. In other words, I believe that he extended the Belmont principle of respect for persons, to include a respectful recognition of people's potential, in this case via displaying to them that he felt they might well be able to learn about more creative and emotionally self-aware ways of being, and by trying to render the research encounter meaningful to them in this way. Respect thus included relating to people in terms of their (posited) capacities for *becoming* (similarly to Canella and Manuelito's metaphor of "changing woman" that I introduced in Chap. 1, Sect. 1.1.2).

Oczak and Niedźwieńska's Treatment of People as Potentially Able to Resist Authoritative Suggestibility

Oczak and Niedźwieńska do not use the word "consent' in their article. But they indicate that all the participants who were in the study volunteered to participate even though they were not given course credits or money for their participation. In this regard they implicitly refer to the idea of the Belmont report that voluntary participation means that "inducements" that might induce people otherwise unwilling to participate, should not be given. They state their ways of recruiting participants and assigning them to control and experimental conditions in this way:

> The total sample consisted of 60 students … from various disciplines at the Jagiellonian University; 53 were women and 6 were men. They were volunteers receiving no course credit or money for their participation. Students were randomly assigned to conditions with the restriction of an equal number (N = 30) in each group: experimental (the new debriefing procedure) and control (the standard debriefing procedure). (2007, p. 52)

Perhaps one of the reasons why Oczak and Niedźwieńska do not refer to notions of "informed consent" is because in this case participants could not be given full information about the experiment, as this would mean that the experiment would not "work". Participants had to be told, as Oczak and Niedźwieńska mention, that "that they would take part in a memory test and they would be given their results" (2007, p. 53). It was on this basis that the suggestibility test (GSS) could be performed, to see to what extent people were submissive to experimenter suggestibility. Directly after the first part of the experiment, participants were—as required in terms of the principle of the Belmont report in cases where deception is involved—debriefed about the GSS as being a test of *suggestibility* (rather than memory) test. They were informed about the mechanisms of suggestibility that had been used, and they were told how they had individually performed on the GSS. But in the case of one of the groups (the experimental one) the debriefing was more extensive. The new debriefing procedure offered the participants the opportunity to participate in a truncated version of the GSS to try to practice the skills of avoiding suggestibility. Further to this, all participants were again partly deceived in the next stage of the experiment, which was designed to compare their scores on their moods, attitudes to psychological research and self-esteem. At this point the information that they were given was that "a new experimenter was interested in how people felt while participating in psychological research" (2007, p. 53). The new experimenter then stated to participants that:

> We would like to know how people feel during psychological experiments that might be disagreeable and how they perceive various experimental procedures. … Thanks to your honest answers we will be able to modify experimental procedures so that they will be more enjoyable for participants. (2015, p. 53)

This information was partly deceptive in that participants were not aware that they had undergone different debriefing procedures and that the point of the experiment was to assess the experimental group's capacity to resist suggestibility (in the new GSS test) and re-assess all of the participants' (post-test) mood, attitude to research, and self-esteem. Oczak and Niedźwieńska justify this partially deceptive information in terms of the benefits that could accrue to future research participants if this experiment showed that the new debriefing procedure can have beneficial effects on people's learning potential and on their felt experiences of the research process as a whole. They suggest that researchers reading their article could then take this into account; and they express hope that ethics committees too might be interested in "integrating more extensive debriefing sessions into research protocol" (2007, p. 57). Furthermore, they justify that their experiment could have a beneficial effect also on citizens (of any country) insofar as it can be shown that people need not be puppets of authority regimes, but can, if educated, find ways of resisting the suggestibility of those in authority. In the next section I discuss this case in terms of the principle of beneficence, which deals with the issue of potentially causing harm to participants and of weighing the risk of harm against the potential benefits to participants and to the wider society.

What is important to note at this point is that Oczak and Niedźwieńska extended somewhat the Belmont principle which states that researchers can abide by the principle of respect by (simply) organizing a debriefing as soon as possible after the experiment. Oczak and Niedźwieńska chose to create a *new kind of debriefing that would be more educative for participants*; and furthermore they *asked for participant feedback from all participants as another way of showing respect*. That is, just by asking the participants how they felt about their participation and by suggesting to them that their answers might also help further participants to find research more "enjoyable", Oczak and Niedźwieńska can be said to have accorded respect in ways not provided for in the Belmont report.

8.2.3.5 Some Ethical Reflections on Chapter 6: Respect as Nurturing a Respectful Orientation to All that Exists

None of the authors whose work I discussed in Chap. 6 use the term informed consent. But from their discussions of their work, it is transparent (to me) that the idea in all cases was to set up research relationships with research partners, while bearing in mind the *whole web of relations in which one is enmeshed, including human and non-human relations*. This is in keeping with Harris and Wasilewski's suggestion that

> we human beings are related, not only to each other, but to all things, animals, plants, rocks —in fact, to the very stuff the stars are made of. This relationship is a kinship relationship … . We thus live in a family that includes all creation. (2004, p. 492)

The Belmont report, where the first principle is held to be "respect for persons" (based on the biomedical model) does not cater for research that tries to also evidence respect or care for "all that exists". Therefore, it is impossible to discuss the modes of setting up the research that I discussed in Chap. 6 in terms of the framework of this principle. All that can be said is that respect in this case meant trying to engage people participatively, in relationships that extended to considerations of links with the natural environment.

In the case of the work initiated by McIntyre-Mills in conjunction with the South Australian Local Government Association (SALGA), she involved the participants (residents in the community) in defining the value of the research for them, hoping that this would also stimulate them into considering the environment as deserving of respect and care, rather than seeing it as a resource to be exploited for profit (2014a, 2014b). The software package was designed for people to feel "respected" for their contribution that they could make by engaging with others' views and adding additionals that they thought might be important. Various people from the locality participated (and sometimes did so repeatedly as they returned to revisit their responses and to alter them). Participation in opening the software package via the website, or in responding to research assistants in cases of face-to-face assistance, was clearly voluntary and involved their informed consent, as the package itself contained information about the purpose of the research. The purpose was also

publicized by SALGA in the community. Thus people who participated could be said to have done so on the basis of informed consent. *But it seems that just as important for McIntyre-Mills was that the wellbeing of nature, which otherwise might well be marginalized in research processes, was given the opportunity to be included.* Hence she designed the package in such a way that people could be prompted to consider operating not in terms of "business as usual", but in terms of *developing connections through a non-anthropocentric ethic*. As mentioned above, such an ethic is not incorporated in the principles of the Belmont report.

In the case of the research write-up by Roman-Alcalá of the Occupy The Farm (OTF) initiative at the University of California on Earth Day in 2012, he covers events in which he was a participant in the action, while he also interviewed and conversed informally (in 2012 and 2013) with the central organizers of the action and other participants (2015, p. 557). The participants would have been aware of the purpose of the write-up, namely, to make public in various forums (including in article format) the events in which the various protagonists had been involved. Roman-Alcalá synthesizes, while extending, the various participants (interviewees') interpretations of the OTF initiative, in his own quest ultimately to encourage people reading the text to reflect on their options toward creating more social and environmental justice. In this way he tries to evince respect, inter alia, for our connectivity with nature.

Maathai uses her autobiography (2006) to suggest that she was involved in challenging gender discrimination as she and others experienced it, as well as in challenging a non-respectful attitude to nature (as sacred). She considers the beginnings of the Green Belt Movement as involving a confluence of factors which included her reacting to "a set of problems by focusing on what could be done" to alleviate rural poverty and also show reverence to the trees and the rest of the environment, as she recounts she had been taught when she was growing up (2006, p. 119). She thus, through her story, evokes a sense of possibility in addressing both social and environmental justice. She explains that it was her listening carefully to the voices of the rural women and to that of nature (as she interpreted these in this context) that she considered that a solution was at hand: "as I sat listening to the women talk about water, energy, and nutrition, I could see that everything that they lacked depended on the environment". Meanwhile, returning from a United Nations conference on women (1975) the National Council of Women of Kenya delegates "carried the same message: we needed to do something about water and energy". They "had also concluded that the world needed to address the realities of rural women, their poverty, the overall lack of development, and the state of the environment that sustained them" (2006, p. 124).

She recounts that "it suddenly became clear" to her that something needed to be done at the level of finding a solution to the environmental degradation. Her story is built up by her illustrating how she listened carefully to the participants in her story (giving special attention to the plight of rural women) while simultaneously listening to, and organizing her story around, nature/the environment as a participant to be held sacred.

I now turn to a discussion of the second principle of the Belmont report. I offer a brief outline of it, followed by an account of how it can become extended/shifted to

make provision for different research positions. As with the principle of respect, I then offer some detail on how it was arguably reconfigured in various ways by the authors whose work I detailed in Chaps. 2–6.

8.3 Reviewing/Reconfiguring the Belmont Report's Principle of Beneficence

8.3.1 The Belmont Report's Exposition of the Principle of Beneficence and Its Application

The authors of the Belmont report state in relation to this principle that:

> Persons are treated in an ethical manner not only by respecting their decisions and protecting them from harm, but also by making efforts to secure their wellbeing. Such treatment falls under the principle of beneficence. (1979, p. 4)

They indicate that there are two general rules which can be formulated as "complementary expressions of beneficent actions in this sense: (1) do not harm and (2) maximize possible benefits and minimize possible harms" (1979, p. 5). The authors deliberate on this in the light of the Hippocratic maxim "do no harm". But they also point out that "avoiding harm requires learning what is harmful" and, "in the process of obtaining this information, persons may be exposed to risk of harm". Furthermore "learning what will in fact benefit may require exposing persons to risk". The problem therefore is to "decide when it is justifiable to seek when the benefits should be foregone because of the risks" (1979, p. 5). Meanwhile, another issue that needs to be considered is whether "one should not injure one person regardless of the benefits that might come to others" (1979, p. 5).

In further discussing the obligation of beneficence they suggest that

> investigators and members of their institutions are obliged to give forethought to the maximization of benefits and the reduction of risk that might occur from the research investigation. In the case of scientific research in general, members of the larger society are obliged to recognize the longer term benefits and risks that may result from the improvement of knowledge and from the development of novel medical, psychotherapeutic, and social procedures. (1979, p. 5)

When laying out of their deliberations on the principle of beneficence, they thus refer to the benefits of an "improvement of knowledge" or the development of "novel medical, psychotherapeutic, and social procedures" as created through the scientific research. And they suggest that if not necessarily of immediate benefit to participants in the research, the increase in knowledge may well have long term benefits for the public good.

In their account of the application of the principle of beneficence they suggest that in practice it involves different types of decision making for investigators, review committees, and prospective subjects/participants, as follows:

> For the investigator, it is a means to examine whether the proposed research is properly designed. For a review committee, it is a method for determining whether the risks that will be presented to subjects are justified. For prospective subjects, the assessment will assist the determination whether or not to participate. (1979, p. 8)

For investigators, their role is to ensure that the study is well designed so that it can indeed contribute to "the development of knowledge". That is, it behooves them to properly design the research so that it is best likely to result in an increase in knowledge, with the least harm incurred in the process as far as they can determine. Risk-benefit analysis is also to be undertaken by the relevant review committees, whose main concern is that an adequate account of risks and benefits is presented to prospective participants. Risk-benefit considerations as far as the participants are concerned, are related to the application of the first principle (respect for persons), namely, the prospective participant's decision on the basis of the information at their disposal as to whether or not to give consent to participate.

8.3.2 Some Concerns Pertaining to the Belmont Discussion Around the Principle of Beneficence

As noted by Austin, when using the biomedical framework as the basis for ethical discussion, "the idea that knowledge production is the sole domain of academic researchers is perpetuated and the community-based movement for 'democratized' research [is hereby] ignored" (2015, p. 34). The requirement of "investigators" to properly design the research so that it can result in an improvement of knowledge, as stated in the Belmont report, is steeped in a model of knowledge production as undertaken by "investigators" doing "science". Put differently, the authors of the Belmont report hold the assumption that researchers can operate in terms of a clear definition of what doing science involves (drawn from the biomedical model of the practice of science). This assumption remains uninterrogated. Castro-Reyes et al. suggest that one way of correcting this would be to "use the … term 'researcher' rather than 'investigator'. Researchers are defined as individuals who conduct research, whether they hold an advanced degree or not" (2015, p. 10). Once the definitions of "research" and "researcher" are broadened, alternative ways of knowing then also can become admitted into considerations around how to approach research projects.

Furthermore, while the Belmont reports points to the importance of weighing up potential harms (risks) against potential benefits (to participants or to the wider society in terms of knowledge gained), it leaves in abeyance *how the participants/community can participate during the research in defining what is harmful to them or to their communities and what may be deemed beneficial.* Austin points out that although researchers—especially those with a conception of research as striving to generate "objective' knowledge—might feel committed to publishing the research results (apparently for the benefit of the public good),

> community partners may have a different perspective. Data that is unflattering to the community may raise fears that they will be negatively affected (e.g., further stigmatized), and community partners may want potential repercussions to influence how the data is released (Flicker, Travers, Guta, McDonald, & Meagher, 2007; Minkler et al., 2002) or whether it is released (2015, p. 30).

Of course, if one has a view of research as a more or less neutral endeavor in the first place (as, for example, in the various expressions of the Belmont report), then it may seem improper that members of communities/community partners should decide whether, say, harmfulness to the community via potential stigmatization warrants the holding back of the research results. But authors such as Austin (2015), Glass and Kaufert (2007), Chilisa (2012), and Cram (2015) have cautioned that certain ways of seeking so-called "knowledge", arising from narrow research frameworks, can issue in ways of seeing reality which are *too limited in their vision*, with consequent harmful effects. (See also Chap. 7, Sect. 7.2.) As Cram elucidates:

> Failure to recognize the determinants of how people are positioned within societies (i.e. that the soil determines how the flower seeds fare) is an act of omission that is often supported by … research practices that fail to look beyond surface explanations for people's circumstances. (2015, p. 679)

She goes on to illustrate that such "acts of omission" can have negative consequences at both individual and community levels for affected communities (2015, p. 679). She gives the example of how, with reference to the Māori community, this narrowness of perspective can lead to "unfulfilled responsibilities of government and the inaccessibility of health care for Māori, rather than accepting that Māori are somehow sicker and dying earlier than non-Māori through some fault of their own" (2015, p. 679). Glass and Kaufert (2007) likewise note that research may embody risks that "impact on the entire community in ways that interact with, but are not identical to, the impact on individuals who identify with that group" (2007, p. 31). They refer to an example of the publication of research (on Native Americans) which offered deficit visions of individuals, without placing these in social context, resulting in both self-stigmatization (feeling of shame for the participants) and "social stigmatization of the entire community" (2007, pp. 31–32). This is similar to the point made by Chilisa (2012, p. 174) about the potential reproduction of deficit discourses via the research process.[14]

To sum up, those who subscribe to participatory approaches insist that the participants themselves (in the context of communities), and especially those

[14]In the light of considerations such as these, the University of Victoria in Canada has developed *Protocols & principles for conducting research in an Indigenous context* (2003) which specify that:

> The people participating have control over the results of the research process and as such have an absolute right to exercise control over the information they have volunteered. This includes the right to control it, to restrict access to it, or to withdraw part or all of the information from the actual research project findings.
>
> This means that it is specified before the project begins, that this will be the condition of participation.

historically marginalized, should play a central role in defining what can be deemed as enhancing the quality of social (and ecological) life. What this means is that the very way in which the research is set up should give consideration to *how a research space for raising concerns about benefits/wellbeing can be opened*. This is what Mertens also stresses when she notes that it is important to "include reciprocity in the design, that is, [to co-consider] ways to leave the community better off than when they began the research study" (2011, p. 196).

How, then, can we conceptualize the research of the authors detailed in Chaps. 2–6 as contributing to our conception of how the Belmont principle of "beneficence" can become reconfigured? I turn to this below.

8.3.3 *Beneficence as Co-considering the Consequences of the Research Approach*

I have qualified the principle of beneficence in the above heading so that it encapsulates a concern of those initiating research projects to consider, with others, the *potential consequences of the proposed way of going about the research*. In the Belmont report, the issue of *how the research is to be done*, does not arise as an issue, excepting that it is understood that the quest must be to *improve upon our knowledge*. It is not required of researchers to consider how the framing of the issues under consideration, the way of asking questions, and the way of interpreting "results", for example, may all influence what becomes "known". Mayan and Daum (2014) make the point that:

> Just as politics and policy are about values, so, too, is research. Research is always directed. Simply by the questions we choose to pursue and how we choose to work, we are stating our values, the stories that need to be told, and what we consider to be social injustice. (p. 74)

As I have also argued in the rest of the book, ways of asking questions, etc., are not neutral in the consequent discourses that become "current" in society. Hence it is incumbent upon (academic) researchers, with those involved and affected by the research endeavor, to ongoingly reflect upon the impacts—in terms of "harms" and "benefits"—that might accrue from the research. Mayan and Daum suggest in this regard that "academics" who are affiliated to academic institutions can work *alongside* communities, and "integrate" with them, especially those "facing social injustices" (2014, p. 76). This to them does not mean excluding the inputs of "those in power" in the social system, as this exclusion would amount to stereotyping the differences between those occupying powerful positions such as power holders in the public policy system, and others—see 2014, p. 87. [15]

[15]They suggest that from their experience, it is not the case that individuals who "hold power" are necessarily unable to "appreciate the history behind an issue and articulate what it means to live a better life" (2014, p. 87). In practice, they suggest that people who are committed to social change, need to look for leverage points to see/decide where to "push when we can" and "pull back when we need to" in finding leverage points for constructive action (p. 87).

In the following sections I offer some thoughts on how one can define beneficence in an expanded fashion (that is, expanding on the Belmont account) to include considerations regarding the impacts of ways of doing research in/on the social fabric. As with the various extended aspects of the principle of respect which I outlined in Sect. 8.2.3, I highlight specific aspects of the extended beneficence principle which I draw out from the research examples discussed in Chap. 2 through to Chap. 6, recognizing that these various aspects are of course not mutually exclusive in practice.

8.3.3.1 Chapter 2: Co-considering the Consequences of the Research Enterprise in Terms of Potential Healing

With reference to Chap. 2, it be said that I tried to take into account (as recommended in my earlier articles and books) the importance of considering the potential consequences of the way of doing research on/in the social fabric. Looking back now it could, however, be argued that I did not take sufficiently into account that the Black participants in the research might not feel confident to share their views in the focus group forum (with White participants) where English was to be the language of discussion. I was not at that point aware of the potential harm that might accrue to them in terms of their sense of dignity by being asked to participate in an English-speaking encounter with White participants (some of whose first language was English, while the others were very proficient in English from their schooling). I become more aware of this when Boitumela and Thandi requested to Asanda that she could act as translator in a further small group meeting when they could not find the English words (as she had done from time to time in the FG meeting). I realized then (and it is clearer to me now) that I may have put the participants "at risk" in asking them to join in the focus group meeting, without making provision for translation work. (It was also only later that I read about the importance of catering for Indigenous language use, as, for instance, discussed in Romm, 2015, 2017.)

Nevertheless, I had tried to set up the research in an informal forum which was culturally appropriate especially for the Black participants (being a kind of talking circle). As it panned out, just as the Belmont report states (1979, p. 5), one cannot always foresee whether the research will be experienced as harmful or beneficial. However, in this case, the participants themselves also took responsibility in deciding whether any possible harm was outweighed by the chance to talk about these somewhat taboo subjects (race and racism) in a cross-racial context. They chose to do so in their various ways.

Many Indigenous-oriented authors have suggested that research can become a space for "healing". For example, Kovach states that Indigenous research frameworks have a "decolonizing agenda that involves healing and transformation" (2009, p. 125). Chilisa spells this out as follows:

> This book [of Chilisa's] draws your [the reader's] attention to the emphasis on the role of the researcher as a provocateur (Mertens, 2010) and a *transformative healer* (Chilisa, 2009; Chilisa & Ntseane, 2010; Dillard, 2008; Ramsey, 2006) guided by the four Rs: accountable responsibility, respect, reciprocity, and rights and regulations of the researched (Ellis & Earley, 2006; Louis, 2007; Weber-Pillwax, 2001; Wilson, 2008). (Chilisa, 2012, p. 7, my italics)

Chilisa explains that:

> A transformative healer will seek to go beyond Euro-Western research issues of power that mainly focus on the "I" (the researcher) and the "you" (the researched) to more involving I/we relationships that see reality differently. (2009, p. 420)

I suggested in Chap. 2 that I was aware of a sense of responsibility to use the research space as a way of "healing" some of the effects of racism in terms of the problem of so little cross-racial talk around this. (See Chap. 2, Sect. 2.3.2.) Of course, this potential benefit of the research also rested on how the focus group meeting (and further engagements) ensued. In this sense, we jointly took responsibility for the consequences of the research in terms of its healing potential. For example, Lindiwe indicated to me in a one-to-one conversation that she wished to share her story with me, as she wanted me to know more about her and her conditions. Dumisane for his part indicated to me in our one-to-one encounter that he had very much enjoyed the focus group meeting (despite that he had chosen not to speak) as it is very important for Whites to engage in such discussions with Black people. And in the group interview with Asanda, Boitumela and Thandi, Thandi expressed that she was pleased to hear that Koos had chosen to inquire more deeply into what is acceptable behavior in terms of her cultural practices when greeting people. Meanwhile, the Black participants found that they were able to share insights—albeit that they had not been speaking in their mother tongue and had not been educated in English—and that a quality discussion around racism was ensuing. My role as facilitator of the FG meeting was to try to encourage a discussion that could be experienced as fruitful and as healing. Naturally, some of my deliberations here are post facto ones.[16] But I would like to share these with the audiences of this book, as what Gergen (2015, p. 301) calls a "resource" that may be found useful for others.

Apart from what can be taken as the healing effect of the discussion (admittedly in a small way), some participants also used the research to express some of their concerns linked to their very low wages. Some of them implicitly requested that I try to follow this up, which I subsequently did (in the context of the body corporate meeting of Trustees, when I became a Trustee). The research hence became beneficial for them (thanks to the Trustees agreeing that the owners should pay a decent wage). But besides this immediate benefit, one could argue that the research can at the same time serve to illustrate for wider audiences that social spaces can be

[16]As indicated in footnote 5 of Chap. 2, in June 2017 I showed some of the draft of Chap. 2 to Tracey (who had been one of the participants). She indicated that she had the impression (in her memory now) that "we all shared and talked together" and that it was a "good union".

created for reviewing forms of exploitation (as identified by various researchers in different contexts). The benefits of this exposition for what Magnat would call "the world at large" (2012, p. 168) consist in my sharing my understandings, but also with the qualification that readers should further write into my storying as presented in Chap. 2.

8.3.3.2 Chapter 3: Rendering the Research Enterprise Meaningful for Participants and Communities

In the international project reported on in Chap. 3, the initial proposal of the research was designed more or less according to the guidelines of the Belmont report, so that the research was geared to improve upon the body of knowledge regarding teachers' attitudes to inclusive education in various geographical settings. It was deemed that no harm was going to be caused to teachers either by their filling in the questionnaire items or by contributing to focus group discussions; and furthermore the knowledge gained, as written up in reports and in articles, could later be used by those interested in making use of it. However, at the point at which we conducted the focus groups in South Africa (which was the same point that I entered the project) we tried to be *more responsive to participant feedback around what the research might mean to them.*

Actually, when we (Nel, Tlale, and myself) designed our "feedback sheet", as the basis of our asking participants for feedback on their experience of the FG sessions (see Chap. 3, Sect. 3.3.1), we had not thought that they might use the opportunity to ask us to "carry the baton" to places where their concerns could be heard. Having heard this feedback, as noted in Sect. 8.2.3.2, we arranged a session with district officers and also an officer from the head office so that participants could be brought into communication with them, mediated by us, in terms of a co-operative encounter (Nel, Romm, & Tlale, 2015). In this way the research could become beneficial for them and also for the learners who would benefit from more attention being paid to such issues.

In the national project, we (the project team) designed the project so that it could have intervention effects at all stages of the research. We took it as part of our responsibility to consider how our way of going about the research might already be impactful at all stages. Hence already the format of the questionnaire was meant to offer scope for increased reflection on various issues. Feedback received from some of the principals and teachers (those who commented on the questionnaire in our final "question") indeed indicated that the questionnaire could be seen as having the potential to already serve an interventive function in this way. (See Chap. 3, Sect. 3.4.3.)

After the administration of the questionnaire—through the FG sessions and intervention visits to schools—staff from Unisa were encouraged by our research design to contribute "outside" (etic) perspectives in the spirit of mutual learning with participants, while listening carefully to (emic) understandings of participants and engaging with these. From feedback received from some of the participants (in

cases of which I have direct knowledge), it would seem that the process of trying to generate an *emtic* relationship (Onwuegbuzie, 2012) was successful in terms of benefits that accrued in quality of "knowing" and "acting" of various parties. What was also important is that participants felt capacitated to find additional ways of locating and using community resources to enhance the implementation of, for example, inclusive education. (Chap. 3, Sect. 3.5.1.) Consistent with our intent to maximize benefits "for the community", with special reference to learners historically disadvantaged by the school system, the intervention visits to schools were directed at capacitating teachers and others concerned to strengthen prospects for collaboration and to widen their networks in the community—ultimately for the benefit of learners. In addition, the intervention handbook was meant to be a resource for those who had participated in the research as well as for others who could benefit from what was shared in the handbook. (See Chap. 3, Sect. 3.5.)

8.3.3.3 Chapter 4: Directing Research Toward Nurturing Relationality

In regard to the research documented by Woldegies (2014, 2016), Woldegies highlights (2014, p. 28) that it was important ethically to have taken into consideration "the protection of the research participants and their households from possible social, psychological, and economic harm". (This is an implicit reference to the Belmont principle of beneficence as including protection from harm.)

> He indicates that:
>
> To avoid potential marital and other social tension associated with individual interviews and group discussions, no adult other than the informants/group discussants were allowed to attend interviews or discussions. (2014, p. 29)

He also indicates (as I noted in Sect. 8.2.3.3) that part of his responsibility to protect participants from harm included his being careful not to divulge any confidential information that they shared with him in confidence. In these ways he made provision for trying to ensure that the research process would not lead to marital tension or social tension in the community. But what is also interesting, as I see it, is that he tried conversely to generate a research process that would as far as possible *improve gender relationships* in the community. He recognizes (as part of his initial theorizing, based also on literature on the subject) that gender, as defined by Jackson and Scott (2002), is a

> social structural phenomenon but is also produced, negotiated and sustained at the level of everyday interaction" (p. 1). Gender therefore is reproduced through practice whilst intersecting with other social divisions such as class, ethnicity, race, etc. (2014, p. 18)

Appreciating that gender is socially constructed (while also intersecting with other social divisions), he used the research space to *encourage women to consider proverbs in the society that valorize women*, rather than treating them as subordinate. He also *encouraged men—for example,* via *the focus group discussions—to*

recognize (and strengthen in practice) the value of partnership-styled, rather than hierarchical, relationships with their partners. Messages related to the value of supporting women were also relayed to other stakeholders, as a way of undercutting the reproduction of gender inequality in practice, *toward more "balanced" relationships* (including, as it happens, more balanced relationships with nature—see Chap. 4, Sect. 4.2.5). This overall way of practicing the principle of beneficence, was linked to Woldegies's regarding the research process as an opportunity to, in the words of Magnat, revitalize people's sense of connection. Magnat offers her interpretation of the importance of such vitalization by referring to Wilson's (2008) account of research as ceremony:

> Participating in ceremony entails carrying out a serious of actions, which, if performed competently and in accordance with traditional knowledge, can activate, sustain and revitalize relationships to others, to the entire community and to the world at large. (2012, p. 168)

I would suggest that this expression of the purpose of "research as ceremony" aptly characterizes the research initiated by Woldegies. It also offers a succint expression of how we might reconfigure the principle of beneficence, so that it *encapsulates considerations concerning the consequences of the research in terms of potentially revitalizing relationships*. Chilisa (2012, p. 3) also refers favorably to Wilson's (2008) understanding of the "importance of connectedness and relationships" and of using research as ceremony to strengthen connectivity.

Let us turn now to the research initiated by Ssali and Theobald. As with Woldegies, they can be said to have been aware of the need not to harm the participants through the conduct of the research. Hence they used the life history approach with the intention that participants could recount their stories as they saw fit and could share what they wished to divulge when speaking about the traumas of the war. They also state, with reference to various authors' arguments/experiences, that "life histories can be therapeutic, as people are given an opportunity to tell their stories and experiences" (2016, p. 94).

As I remarked in Chap. 4, Sect. 4.3.1, it might have been advisable for Ssali and Theobald to ask the participants for feedback on how they were experiencing or had experienced the life history interview process, as this might be helpful for them to take into account during the interview or in future research, or for others to take into account. Soliciting this feedback from participants might itself have been considered "dignifying" for the participants; and they also would have had the opportunity to express how they had felt about the interview encounter. Besides, this could help other researchers to consider their styles of interaction with participants recounting stories of trauma.[17]

[17]Reddy organized a study in which she asked postgraduate students who had been involved in trauma research with participants to indicate how they felt about their own experiences of the research process. Many reported that they found it difficult to separate the role of "researcher" from that of "caring person" and hence at times used their experiences as counsellors as part of the research encounter. As Reddy notes, their various accounts "demonstrate that researchers struggle most to negotiate and separate the role of researcher from that of counselor/therapist/helper. Ethical

Nevertheless, giving credit to Ssali and Theobald, it could be said that because the participants were asked to share/recount only that which they wished to recount, they were afforded the responsibility for safeguarding that the interview process did not cause them undue emotional trauma. That is, the participants could weigh the emotion of telling of their stories against the emotional benefit of "testifying" to their trauma; and they could also weigh the emotions involved against the possible benefit to themselves and others in terms of health reconstructive efforts that could feasibly be generated further to the research. What is important is that they were not given unjustified expectations about the benefits that might accrue on the level of health and social reconstruction. (Ssali and Theobald did not promise this, as they could not be sure of how the research might be received by leaders in the health sector, policy makers, etc.) Ssali and Theobald thus enabled participants to assess "risks" and "benefits" along the way (in deciding whether as well as how to participate in the research).

What Ssali and Theobald tried to develop, on their account (2016), was a process that could lead participants to a sense of empowerment via their control over their recounting of their stories, which could potentially also enable participants to imagine new possibilities for living. (See Sect. 8.2.3.3.) But Ssali and Theobald do also caution against researchers' overestimating or "hyping the empowering potential of life histories" (2016, p. 94). For their part, they organized research write-ups in various forums with the intention to foster less stereotyping as far as gender and inter-ethnic identifications were concerned—so that more connectivity could be stimulated in the social fabric. (See Chap. 4, Sect. 4.3.3.)

In short, they tried to take into account how research participants might experience the research process (and hence they chose life history storying); and they also took into consideration the possible effects of their write-ups—including setting up of blogs for discussion, and creating forums to discuss reports with personnel in the health sector and with others in the community. In addition, through their article (2016), which involves some generative theorizing, they offer wider audiences options for relational styles of thinking and being, which they hope might be positively impactful.

8.3.3.4 Chapter 5: Gearing Research to Activate Learning Potential

In the case of the experiment organized by Stephens, the experiment was set up in the hope to activate learning possibilities on the part of the recidivists participating in the various training interventions. In order to ensure that the "control group" who were not exposed to either the Six Thinking Hats or the emotional intelligence

dilemmas of how familiar to be with participants challenged the researchers, some of whom had difficulties operating within ethical boundaries while still remaining human in their interactions" (2010, p. 40). It is not reported in Ssali and Theobald's discussion how they handled these "boundaries" and whether they took cues from interviewees as to how to handle the relationship. (See also Chap. 4, Sect. 4.3.1)

(EQ) training did not feel left out—which could possibly cause them psychological upset/harm—Stephens offered them what was also pertinent to them, namely, HIV and AIDS training. In the initial discussion with the prospective participants when explaining the experiment to them and inviting them to participate, as well as ongoingly as the training ensued, he also enabled them to decide on whether and how to participate in the training, so as to maximize its benefits for them. The prisoners could thus become co-responsible for considering benefits and for *making the training beneficial* through the way they participated. For Stephens' part, he tried to encourage, but did not pressurize, active participation during the classes and between classes via exercises to partake in after the sessions.

As regards the Six Thinking Hats and emotional intelligence training (compared with the HIV and AIDS training), although Stephens wanted to assess whether the post-test scores of those who had undergone the Six Thinking Hats and EQ intervention had improved significantly on their scores relative to the control group, he was also concerned during the training to arrange the sessions so that they could be experienced as beneficial for the participants. Although he does not highlight this in his thesis, this is my understanding gleaned from my personal communications with him. (See Chap. 5, Sect. 5.2.6.)

As far as the reporting was concerned, he was careful of how he displayed the outcomes of the experiment and how he chose to create recommendations for action for others to consider, such as counsellors in various prison contexts. In the spirit of Cram's suggestion to which I referred in Sect. 8.3.2, he did not wish to reproduce research practices "that fail to look beyond surface explanations for people's circumstances" (Cram, 2015, p. 679). This was also in the spirit of Gergen's (2015) understanding of generative theorizing (which I would call responsible theorizing): he recognized that ways of theorizing are not neutral in their social consequences. Hence he offers insights/reflections directed at the level of social structure (see Chap. 5, Sect. 5.2.7 and Chap. 7, Sect. 7.3). This was to improve the likelihood that others reading his text might show more compassion for the recidivists' plights and also, in the light of the research, to be more appreciative of their potential, instead of further stigmatizing them (as individuals and as part of a group). He thus takes care not to foster deficit thinking, which arguably would have consequential harmful effects for the broader prison community.

Turning now to Oczak and Niedźwieńska experimental design, they set up their experiment with the intention that their new educative debriefing procedure could be experienced as helpful to participants and also potentially to others subjected to deceptive research in future. They state that they were intent on devising this experiment following ethical principles of avoiding harm and seeking to promote wellbeing in relation to those who participate in deceptive psychological research (2007, pp. 51–52). Although they do not refer to the Belmont principle of beneficence, their talk of minimizing harm and trying to increase benefits for participants (and for others) is an implicit reference to this principle. Considering this principle, and creatively *reconsidering its implications for the practice of deceptive researc*h, their idea was to offer participants the chance of benefitting from the practice of trying out the ability to resist suggestibility (as in the new debriefing procedure).

I would suggest that the way in which Oczak and Niedźwieńska set up the experiment so as to try to render research (in this case deceptive research) an experience which might enable participants to feel empowered, can be considered as reworking the Belmont model. The Belmont report allows for certain cases of deception of participants by researchers for the sake of doing "science". Oczak and Niedźwieńska wished to work scientifically, *but in a way which allowed participants to feel that they were not merely "experimental subjects*". The results regarding participants' experiences of this mode of doing "deceptive research", if taken into consideration by other experimental researchers involved in deceptive research, could be of benefit for future participants too, as they could partake of the new-styled debriefing procedures.

Also, given that the GSS scores of the participants who participated in the truncated GSS showed that most of them had the ability to resist the suggestibility of those in authority (namely, the researchers), this would be a finding that could potentially benefit any readers confronted with "authority", that is, anyone feeling vulnerable in the face of authority. Nevertheless, we can also consider whether Oczak and Niedźwieńska's implicit appeal to the "authority of science" via their experiment, is itself too authoritative in that the data and interpretations thereof are presented as flowing from a rigorous scientific research procedure. From my discussion in Sect. 8.3.2 I would suggest that as long as we, as audiences, *are aware of the way in which the various data and interpretations were constructed*, we too can make decisions as to how to regard the status and import of the experiment.

8.3.3.5 Chapter 6: Taking Responsibility for Enabling the Wellbeing of Nature to Be Included

McIntyre-Mills (2014a,b) associates "harm" to subjects/participants with her understanding that *nature is to be seen as a participant in research processes*. Ways of conducting research, she avers, should thus be concerned with avoiding/minimizing harm and increasing wellbeing of this participant too. She set up the research (as the initiating researcher) with the primary intent to encourage people in the locality in which the research was conducted to be more reflective about their choices. At the same time, she was trying to encourage these participants to recognize their connectedness to all of creation, where human wellbeing and the wellbeing of nature are interlinked. Through her discussions with human participants in various forums (for example, in the workshop preceding the reporting to SALGA and later with SALGA representatives) she considered it as part of her responsibility to enable the wellbeing of nature to be represented. Furthermore, in her write-up in her books, she refers to narratives drawn from spiritual traditions which valorize nature, again recognizing that her way of writing itself may be consequential for discourses that become "current".

Like McIntyre-Mills, Roman-Alcalá (2013, 2015) tries in his research (which includes his writing up of the action of OTF) to offer a needed counterpoint to discourses which buttress economist and single bottom line profit-oriented

agribusiness, with all its damaging effects on people and on nature—where both become seen as "resources" for exploitation. While his write-up focuses on the OTF initiative, at the same time he points to narratives of social and ecological justice which he suggests can become strengthened through action and through reflections on such actions.

Maathai is also acutely aware of the responsibility to bring to the fore the wellbeing of nature in the face of styles of thinking and acting which in effect cause natural devastation, because, as she sees it, the sacredness of nature has become occluded. She pleads, through her evocative writing, for the importance of our holding nature in awe. For example, she recounts that:

> I also learned that someone had acquired the piece of land where the fig tree I was in awe of as a child had stood. The new owner perceived the tree to be a nuisance because it took up too much space and he felled it to make room to grow tea. By then I understood the connection between the tree and water [with the tree root system serving to hold the water], so it did not surprise me that when the fig tree was cut down, the stream where I had played with the tadpoles dried up. … . Ironically, the area where the fig tree of my childhood once stood always remained a patch of bare ground where nothing grew. It was as if the land rejected anything but the fig tree itself. (2006, pp. 121–122).

She here (as in other parts of her text) tries to be responsive to nature, by surmising that nature is trying to talk back to us when, for instance, "the land rejected anything but the fig tree"—thus resisting those who regarded the tree as "a nuisance" in the way of human profit-making.

As I pointed out in Sect. 8.2.3, while we can appreciate overlap between the three principles of the Belmont report, it still can be regarded as meaningful to separate these out analytically, bearing in mind that this is an analytic separation. I now turn to a discussion of the manner in which the principle of justice can be reconfigured along with the other principles.

8.4 Reviewing/Reconfiguring the Belmont Report's Principle of Justice

8.4.1 The Belmont Report's Exposition of the Principle of Justice as Pertaining to Research

The authors of the Belmont report outline the principle of justice as applied to research ethics by posing the following question:

> Who ought to receive the benefits of research and bear its burdens? This is a question of justice, in the sense of "fairness in distribution" or "what is deserved." An injustice occurs when some benefit to which a person is entitled is denied without good reason or when some burden is imposed unduly. (1979, p. 5)

Having outlined the principle in this way, they go on to note that an important implication of this principle is that

> the selection of research subjects needs to be scrutinized in order to determine whether some classes (e.g., welfare patients, particular racial and ethnic minorities, or persons confined to institutions) are being systematically selected simply because of their easy availability, their compromised position, or their manipulability, rather than for reasons directly related to the problem being studied. (p. 6)

These considerations lead the authors to suggest that in applying the principle of justice, care should be taken to *select participants in a way which can be justified* in justice terms. As they explain:

> Just as the principle of respect for persons finds expression in the requirements for *consent*, and the principle of beneficence in *risk/benefit assessment*, the principle of justice gives rise to moral requirements that there be *fair procedures and outcomes in the selection of research subjects*. (p. 9, my italics)

They make the point that, as they conceive it, "justice is relevant to the selection of subjects of research at two levels: the social and the individual" (p. 9). As regards "individual justice" in the selection of subjects, this requires researchers to "exhibit fairness: thus, they should not offer potentially beneficial research only to some patients [persons] who are in their favor or select only 'undesirable' persons for risky research" (p. 9). Considering "social justice" in the selection of subjects, this requires that

> distinction be drawn between classes of subjects that ought, and ought not, to participate in any particular kind of research, based on the ability of members of that class to bear burdens and on the appropriateness of placing further burdens on already burdened persons. Thus, it can be considered a matter of social justice that there is an order of preference in the selection of classes of subjects (e.g., adults before children) and that some classes of potential subjects (e.g., the institutionalized mentally infirm or prisoners) may be involved as research subjects, if at all, only on certain conditions. (p. 9)

For the authors of the Belmont report, then, the principle of justice translates into sampling decisions, where those trying to implement this principle are urged to take into consideration "individual" and "social" justice in the selection of those subjects who will be invited to participate in the research. Researchers and IRBs need to take care to consider "the overall distribution of the burdens and benefits of research" when (re)viewing the selection of research participants (p. 9). People should not be repeatedly asked by researchers to bear the brunt of research that will most likely not benefit them (and that will more likely end up benefitting others); and those who are already suffering burdens in society should not be asked to carry the further burden of having to repeatedly become subjects of research.

8.4.2 Reconfiguring the Principle of Justice: Toward Active Research

The accounts that I have provided throughout this book with detailed reference to examples, suggest that actively seeking social and ecological justice cannot be

focused on sampling decisions alone. As Denzin and Giardina argue, "justice extends beyond fair selection procedures or the fair distribution of the benefits of research across a population. Justice involves principles of care, love, kindness, fairness, and commitment to shared responsibility, to honesty, truth, balance, and harmony" (2007, p. 24). As I have elucidated in earlier chapters, pursuing justice as part of the research agenda means that researcher/co-researchers *consider how the research can contribute to catalyzing transformative learning and attendant action*. This means that those supposedly bearing "burdens" of participating in the research, should ideally not experience this as a burden, because *ways should be sought to enable them to participate in learning, and widen their networks in communities, so that the research is experienced as empowering*. Along the way, they can participate in interpreting information and engaging with views different from their starting positions (which are further co-explored and developed as part of the research) and consider how to regard these in terms of their import on the level of action. (See, for example, the illustrations in Chap. 3, Sect. 3.5; Chap. 4, Sects. 4.2.5 and 4.3.4; Chap. 6, Sect. 6.2.4.)

In my discussion in the various chapters, I have suggested that selection, and continued selection, of people who are to become participants in the research process, means considering carefully the relations of power within and between (members of) communities, while at the same time seeking potential points of connection between people, including those occupying socially powerful positions.[18] All the while, marginalized participants' perspectives/constructions (and co-constructions) become regarded as "key" (as advised by Mertens, 1999, p. 5). Hence, rather than regarding such participants as already overburdened by researchers seeking to use them for extraction of data—which might be the case when adopting a medical model as in the Belmont report—they can become empowered via the research process to better express and develop their understandings. This is not to say that their views as might have been expressed prior to the research effort are to be taken as given/static and as unchanged via the inquiries. In this regard Cram advises that "capacity building" in communities may include developing "with research teams and communities … a structural analysis of the determinants of inequality" (2015, p. 685). This can issue in different understandings of a "problem" than those that were held prior to the research effort. And

[18]At a workshop facilitated by Mertens entitled "mixed methods design as a tool to enhance social justice" (6 August 2016, Durham, United Kingdom), I asked her about trying to locate people occupying "powerful" positions who care sufficiently about issues of justice, and she replied that in all circumstances the idea in transformative-oriented research is to seek "points of leverage". This includes speaking to people "who are decent" and who "do have power" (issuing from their positions) and who "want things to be better" (citations from my notes that I took at the session). (I am citing my rendition of her words here with her permission, which I obtained on 5 October 2016 by email. When asking her for permission to cite this passage, I sent her the whole draft Chap. 8 and suggested that she could look at it as a whole and also at how I have cited her at different points; and she replied "thanks for asking about these citations" and indicated that she was happy that I am continuing to write and think about these issues.)

this in turn means that options for "intervening to resolve it" (that is, an experienced problem), become widened.

Through Chaps. 2–6 I have argued that research as tied explicitly to intervention intent can be used to widen options for thinking and acting on the part of participants (now seen as enmeshed in social relations), stakeholders (including "nature" as a stakeholder in the research enterprise, whose wellbeing needs to be given full consideration), and wider audiences. It is difficult for me to condense here what a reconfiguration of the principle of justice as outlined in the Belmont report might involve, as this whole book is meant to be a testament to *using research to forward justice, defined in expanded terms, as intervention toward justice-oriented transformation*. As Cram notes, "there are still conversations to be heard about what this [more just and equitable world] looks like" (2015, p. 685). Research as consciously directed by a transformative agenda can contribute to strengthening the quality of such conversations and ensuing actions.

Table 8.1 offers a summary of the ethical deliberations as presented thus far. As can be seen from the content of the columns, there is some overlap between the principles as applied in practice, especially when the Belmont model becomes reconfigured. Hence in the dotted lines between the reconfigured columns I have used thinner dots to denote more connection between the principles as practiced. I have also constructed a final row in the table to be further imaginatively filled in by readers.

8.5 Ethical Practices and IRBs

I have alluded above that IRBs guided by the principles of the Belmont report as defined in the first row of Table 8.1 can all-too-easily function to protect institutional interests at the expense of alternative considerations. As Truman summarizes:

> Underpinning the re-constitution of ethical guidelines and research governance [by IRBs], are a range of measures which protect institutional interests, without necessarily providing an effective means to address the moral obligations and responsibilities of researchers in relation to the production of social research. (2003, Abstract to the article)

Truman contends that IRBs are set up largely to "safeguard research organizations if adverse events occur within research" (2003, para 3.13)[19]. But she suggests that with increasing regulation by IRBs in order primarily to protect institutions, "there remain fundamental questions about *how*, by *whom*, and in what *context*, ethical concerns within research might be addressed" (2003, para 2.2). She poses these

[19]Mertens and Ginsberg (2008) likewise note that "regulatory aspects of ethics, … are intertwined with the role of review boards which have, in turn, been criticized as being less concerned about protecting research participants and more concerned with protecting the research institution" (p. 492).

Table 8.1 Reconfiguring the Belmont model

	Respect for persons	Beneficence	Justice
Belmont Principle (& Understanding of Application in Research Ethics)	Treat people as autonomous agents able to make decisions, such as considered decisions about whether to consent to participate in a research project based on details supplied by researchers. (Also, when need be, protect those whose decision-making capacities as agents may be diminished)	When planning & proceeding with a research project, give careful thought to the maximization of benefits & to the avoidance/reduction of the risk of harm that might be incurred through the research investigation	Exhibit fairness in the distribution of research risks & benefits with reference to the selection of research subjects, so that the burdens of participating in research do not fall systematically on specific (disadvantaged) classes of people
Reconfiguring & expanding the Belmont model	Respect: some variations	Beneficence: some variations	Justice: some variations
	Seek ongoing consent & ongoing development of the research endeavor	Co-consider (in dialogue with key participants) likely consequences of the research endeavor, with attention to potential healing of sedimented imbalances	Use the research space to encourage conversations & actions toward attending to social & ecological justice
	Expand upon, & develop during the research process, any initial consent gained in terms of IRB ethical requirements	Render the research enterprise meaningful for participants & communities	Consciously tie processes of inquiry to justice-oriented action intent
	Support dignity & empowerment at individual & community levels	Direct research toward nurturing relationality	Actively use the research enterprise to contribute to strengthening the quality of life as lived
	Show respect by recognizing/activating potential in persons & communities (regarded as in processes of becoming)	Organize research toward exposing learning potential	Gear the research enterprise to catalyze transformative learning & attendant action
	Develop a respectful orientation to "all that exists"	Take some responsibility for the wellbeing of nature also to be represented in considerations of beneficence	Develop the research remit so that it includes recognition of research as immersed in the web of relationality
Additional variations to be imaginatively filled in by readers	Imagine & try to activate practices of respect tied to care for (especially marginalized) people & the planet as part of the (co)inquiry	Imagine & try to activate practices of beneficence which express an assumption of co-responsibility for research framings	Imagine & try to activate research practices likely to further possibilities for generating an inclusive wellbeing

questions in relation to a research project in the UK which focused on user participation within a community mental health service. She notes that "consent" of participants to participate was made on the terms set by participants, as they chose the conditions under which they would participate. For example, they wished to be interviewed individually rather than in a focus group, and they wished the setting of the interview to be the gym, which they experienced as a positive environment (para 3.16). At the same time, the notion of "risk" to participants became less important as the research started to become more participatory as it proceeded:

> Within our research, notions of risk and wellbeing varied with the construction of social relations within the research process. As our research became more participatory in nature, users identified a range of positive reasons for being involved in the research. (para 3.17)

Here Truman indicates how (Belmont-styled) principles, which specify that consent should be contingent on participants being informed about potential risks of participating, was incongruent with participants taking part in defining how the research could become organized to advance their own and others' wellbeing. The research re-organization could occur only because of the way in which the research relationships were set up toward trust building. In the case of this research, she notes, "participants in our research talked about how they often had a general mistrust of forms [questionnaires] because the information gets used by professionals who make judgements about their mental health status" (2003, para 3.18). In any event, trust had to be earned by the researchers, and along with this, a "participatory research approach" emerged (2003, para 3.17).

Kovach likewise remarks how, when working in Indigenous contexts where trust building is key, "ethical standards which are associated with "liability concerns" that relate to the protection of institutions are inappropriate. She remarks that Indigenous epistemic research "holds a unique ethical complexity that is less about liability and is more relational" (2009, p. 147). Otherwise put, the relational ethic can become threatened when institutional protection becomes paramount. In pointing to some recommendations for how to address the apparently conflicting concerns with "liability" and with "relationality" with reference to Aboriginal contexts of research (in work with Aboriginal communities), Glass and Kaufert (2007) suggest that:

> For an REC [Research Ethics Committee] that rarely views protocols for community-based participatory research, the assistance of not only community representatives and Aboriginal researchers, but also outside experts familiar with research ethics issues relevant to the community and to the proposed research, may be required. Without the relevant cultural expertise and local knowledge, an REC will not recognize issues of importance to a community. (2007, p. 34)

That is, they contend that RECs which are in unfamiliar terrain when it comes to reviewing research work to be done with Aboriginal communities should become assisted by community representation on their boards or at least should consult with those "competent in cultural expertise and local knowledge". This, they suggest, can form a basis for reconciling the apparently conflicting demands/interests of IRBs and communities in which the research is to be practiced.

Many authors have urged that in any case IRB members should be more flexible in the application of apparently universal ethical standards (flowing from the Belmont report), by recognizing that there is diversity of standards that can be applied. But of course, the work involved in updating/reconfiguration of standards that will be applied in the review process requires "social work" on the part of researchers and others wishing to question what is experienced as the narrow operation of IRBs (guided in turn by institutional policies). Despite the work that is involved in efforts to challenge narrowly-directed IRBs—as have been detailed by many exasperated authors[20]—this does not mean that the effort is always in vain. Examples are displayed in the advances made in initiatives such as NARCH, AIATSIS, CIHR, and international bodies such as the International Collaborative Initiative in Community Engagement in Global Health, Ethical, Social, and Cultural (ESC) program, some of whose members participated in the Kenyan conference (2011) that I mentioned in Sect. 8.2.2.

In the context of South Africa, or rather of the University of South Africa (Unisa) whose ethical policies I am most familiar with, the policy pertaining to research ethics makes some provision for the kinds of concerns raised also in Sect. 8.2.2. For example, it is stated in the Unisa policy on research ethics that the various Ethics Review Committee (ERCs) should take into account when reviewing all applications for ethical clearance, a "respect for cultural differences". This implies that different visions of ethics among academics and in relation to the communities in which we work are recognized to be at play (Unisa policy on research ethics, 2014, p. 11). It is also suggested under the same heading (respect for cultural differences) that it is good practice to undertake research "*with*, the members of an identified community or communities rather than merely *about* such community(ies)" (2014, p. 11, my italics).

As I elucidate below, the Unisa policy is (or can be interpreted as being) sufficiently open to allow in the various College ERCs that they can be somewhat flexible when it comes to assessing the ethical quality of proposed research projects.[21] This is not to say that this has always been the case or even that most staff

[20]Many researchers have pointed to their exasperation with ethical committee reviews and procedures in various parts of the globe (Brown et al., 2010, p. 3; Guta, Nixon, & Wilson, 2013, p. 301; Honan, Hamid, Alhamdan, Phommalangsy, & Lingard, 2012, p. 386; Hammersley, 2010, para 1.4; Israel & Hay, 2006, p. 1; Monaghan et al., 2013, p. 66; Sikes & Piper, 2010, p. 205; Truman, 2003, para 1.1; and the authors contributing to the book exploring alternatives to formal research ethics review, edited by Van den Hoonaard & Hamilton, 2016).

[21]At a workshop held in October 2016 at Unisa, the manager of research integrity in the research support office at the University stated that as far as she saw it, "mature" ERCs will operate with flexibility, as they will recognize that there is no "one size fits all" approach. She also indicated that although Unisa's research support office encourages researchers to adhere to the Singapore statement (a document that researchers at Unisa are encouraged to consult), this can be read differently by different people—and, for instance, the requirement for researchers to try to report findings "fully and objectively" may not be applicable when doing, say, more qualitatively-oriented research. This was in response to my mentioning to her my disagreement with the tenor of the Singapore statement, where she indicated to me that she reads it differently (and that others too can read it differently).

across the Colleges would regard this as now being the case. In the following section I provide a short vignette of my attempt to bring about a "public" discussion in certain forums relating to concerns that had been expressed to me by staff (when I was facilitating research workshops starting from 2012). Concerns were expressed by staff at the university that they and their students were being unduly regulated by the application of various sections of the university policy within the ERCs. The story that I present below should be read as an autoethnographic account (using this genre of writing that I laid out in Chap. 6, Sect. 6.4.1).

8.5.1 Reshaping a Milieu of Institutional (Dis)trust: Strengthening Trust Relationships

Since my full time employment in the College of Education at Unisa in 2012 (see my academic trajectory in Chap. 1, Sect. 1.1.1), I noticed how many staff in the department in which I am working, as well as in other departments in our College, were expressing exasperation—in unofficial forums—with the functioning of the ERC system; they felt that it displayed an institutional distrust of their sense of responsibility when going about the research process. However, they did not express this too openly, as I explain below.

8.5.1.1 Using Research and Innovation Week (2013) to Raise Contentious Issues for Attention

I became aware (via various notices) that Unisa had been organizing what is called a Research and Innovation Week each year since 2012. I noticed at one point that it was stated in the University advertising material that this week

> provides a platform for cross-disciplinary engagements … and showcases the various … research programs and flagships. The week offers individuals an opportunity to have in-depth discussions on the challenges facing research in the higher education domain (see http://www.unisa.ac.za/research/news/index.php/unisa-has-to-be-an-incubator-of-research-and-innovation/)

In 2013, I approached a person in one of the other Colleges (whom someone else had told me was also interested in exploring ethical questions) and we agreed to organize a panel discussion with various researchers from different Colleges where people from the audience could also join in discussion about ethical issues of concern to them. We entitled the session (for advertising purposes) "Ethical review and innovation" and we stated that the session is "intended as a session in which researchers and supervisors in the [various Colleges] can share their experiences regarding ethical review processes … as these pertain to obstacles, opportunities and implications for research and innovation".

We also invited to be part of the audience a person from the university's Ethics Office, an office which is concerned with all ethical matters. On the day, as arranged, the panel members began the discussion by locating some statements in the university's research ethics policy which were of concern to them, and by offering views of "grey areas" in the policy. This was meant to create an atmosphere where people from the audience (from various departments in the university) would feel free to express their experiences—as we were cognisant that many researchers were afraid to express their frustrations with the ethics procedures too openly, in case they became dealt with as "suspicious"/untrustworthy by the committee chairs.

During the session, persons from the audience spoke about their experiences with the ERCs in their Colleges. For example, they shared experiences around the preference of the ERCs to secure consent from research participants via their signing of consent letters, and they referred to the unviability of this—particularly in research contexts in rural villages, where most of the participants were far more used to an oral culture (but also in other contexts). People stated that what normally happened in practice was that after explaining to participants what the research was about and showing the participants the consent letter, this letter had to be "put aside" (as it was considered unnecessary by the participants). In many cases the research then proceeded with consent being recorded on tape. (It would be recorded on tape that the participants agreed to take part in the research, as well as to be taped, and that the researchers had iterated to them that they could withdraw at any point.) Some of the audience indicated that through oral practices of obtaining consent in the field, they had in effect gone against the rigid committee expectations as required in most of the Colleges. The issue of gaining permission from "gatekeepers" to enter research sites was also given a lot of attention in the discussion, especially in terms of the power that they might exercise over participants.[22]

Space in this book does not permit detail on the discussion. Suffice it to say that within the university some people had had a forum to express concerns; and because this was a public discussion (with views not later attributable to individuals) it was clear that this was not a matter of certain individuals offering "idiosyncratic gripes" (as Monaghan et al., 2013, p. 70, put it, in relation to an example discussed in their article), but of people offering experiences from the field, which pointed to the need for some revisitation of institutional expectations.

8.5.1.2 Attending a "Roadshow"

In between this and the next Research and Innovation Week (2014) I had the opportunity (early in 2014) to attend a "road show" organized by one of the directors of the university's research office, who wanted to ensure that the various research ethics committees were functioning optimally (in terms of risks that might

[22]See also Mertens and Ginsberg's concerns in regard to the roles of powerful gatekeepers (2008a, 2008b, p. 491).

otherwise be incurred for the university, as he saw it). I was asked to attend this meeting by the then Deputy Dean, who felt that I could vocalize some of my concerns of which she was aware and to which she was sympathetic, at this meeting. At the meeting, besides the research director, the following people were present: someone else from the research directorate of the university, the College Dean (the only person who was not White), a research officer from the College, and the new ethics committee chair (new since 2013). Although the meeting was set up to discuss how we could mitigate risks to the university, I managed to introduce my concerns into the discussion.

Subsequently, I sent my comments on the draft ethics policy (which I heard was being redrafted) offering a five-page statement in which I drew attention to debates in the literature around research ethics. I also located places in the policy where I suggested that there was undue cultural bias toward non-Indigenous ethical stances. I gave an example of inbuilt cultural bias in the wording of the policy document regarding the preference for written informed consent, by referring to the experiences of certain researchers trying to practice a relational stance informed by Indigenous research ethics (such as Chilisa, 2012). This is how I stated it:

> If one has done a lot of research in the field one will appreciate that asking people to sign letters (or even just to give written consent) and telling them that if they refuse we can look for an alternative, already puts the balance in favour of asking for written consent and indeed signed consent (implying this is the best form). This is often embarrassing for researchers when addressing people with whom one is trying to establish a different form of (relational) interaction.

And I summarized Truman's (2003) argument in terms of implications for our ethics approach in the university:

> She notes that ethical regulation actually serves to protect what are seen as institutional interests, rather than that of the participants (as ethics normally should be). I would like to say here that in the case of [our university], there are ways in which we can protect "the institution" as well as participants—but so far the policy (and/or its application) is more in favour of the former—and ethics committees become afraid to be "flexible, thus affecting the way in which researchers can interact with people in the field based on ethical sensibility.

I qualified my statement by concluding that: "This is not to say that we do not need an ethics policy and ethics committees—but we do need to take these issues into account when formulating policies and when asking people to 'apply' them in an ethically sensitive way".

While it would be impossible to say what kind of effect these comments had (for those seeing them), I believe that my contribution to the discussion at the "roadshow" meeting that preceded them, might have made a difference to the orientation of the new committee chair in our College. This is because, when I later sought clearance for a particular project by appealing to a more flexible approach, this was granted. Furthermore, subsequent to this (July 2014), I was asked via the Deanship office to play a role of "research advisor" to the research ethics committee. Further

to this, I became a member of the ERC, offering input in all meetings and also offering input concerning possible other staff who could be invited as members. This was in a context where most of the committee members were still White. I suggested that heads of department should be requested to put forward names of Black staff who could be sympathetic to emphasizing the Unisa policy regarding "respect for cultural differences". While not essentializing cultural differences I suggested that this would help the ERC to incorporate forms of cultural expression that might spring from different cultural heritages. Subsequently, our committee has grown, and now includes a mix of people with different stances (with stances of course not always being aligned with cultural backgrounds, but at least with more perspectives from different cultural backgrounds). This to my mind has served to enliven discussion in the meetings.

As far as the ethics policy itself goes, this became revised in 2014. I would not be able to suggest that the ideas circulated during the previous Research and Innovation meetings that we set up, or that my document that I had sent to those drafting the new policy, had any bearing on the way in which some sections of the policy became re-worded. But upon perusing it I detected that there is now a specific section on community engaged research to cater for working with communities in a sensitive fashion, and there is also a qualification to the requirement of preferably obtaining informed consent in writing from participants. The initial (2012) wording in the policy that "if participants refuse to provide their consent in writing, consent may be recorded verbally" (2014, p. 13) is still present, implying that verbal consent can be sought only in cases where participants clearly *refuse* written format. But in a new section on community engaged research there is a statement which reads as follows:

> Alternative ways to record consent if individuals do not want to sign a consent form but are willing to participate in the proposed research, should be sought. These can include using digital recordings of oral consent or signing a register. (2014, p. 31)

Here in my view the tone is softer, suggesting that university researchers can use sensitivity to decide, in conjunction with participants, if it seems that participants "are willing to participate", but would not wish to provide a signature on a consent letter. If this holds for "community engaged" research it can presumably hold for any research endeavor. Another facet of the policy in the section on community engagement that I regard as significant and as possibly spilling over to all forms of research is the notion that "results" cannot be released in an insensitive fashion and that potential repercussions to the community of ways of laying out results need to be borne in mind. As stated: "Researchers must consider the potential repercussions to the community if data (sensitive or not) are released prematurely or in an insensitive or any other manner" (2014, p. 31). This would be consistent with the arguments of authors whom I introduced in Sect. 8.3.2.

8.5.1.3 Interpreting My Account

My brief account of how I tried to become involved in a network of relations in relation to the ethical review process in the university and as pertaining specifically to our College, can be read as an illustration of what various authors concerned with the overregulation of research have pleaded for. (As I noted in Chap. 1, Sect. 1.3.2, regulatory "structures" can be regarded as capable of restructuring insofar as actors proceed to see them as *slow processes* rather than as fixed.) Connolly and Reid state the case regarding working with ethical regulatory structures as follows:

> They [those concerned with the functioning of ERCs] must engage and become involved with the regulatory bodies to which they are subject … . Only then will researchers be able collectively to identify measures and strategies that continue to support and facilitate the essential work [of research] while respecting the rights, privileges, and dignity of human participants. (Connolly & Reid, 2007, pp. 1046–1047)

This view as expressed by Connolly and Reid is supported by Mertens and Ginsberg when they suggest that it can be considered part of researcher responsibilities to [try to] "educate relevant institutional review boards regarding specific methodological and/or cultural issues prior to obtaining institutional review board approval" (2008, p. 492).

It should be mentioned at this point that the Unisa policy (2014) still encapsulates the principles of the Belmont report (which is referred to in the acknowledgments section of the policy as having been consulted, p. 16); but it also seeks to expand them in a way to account for "cultural differences" and also for community engagement practices. Given the content of the policy, I would not consider that those constructing/revising the policy—ultimately people working in the research support office—would necessarily agree with my reconfigurations of the Belmont principles as I have set them out in Table 8.1. Nor would I suggest that the majority of the researchers on the College ERC of which I am a member would agree with these kinds of reconfigurations. For instance, it seems to me that many still subscribe to a view of knowledge improvement as being secured to the extent that researchers strive toward "objectivity". This is irrespective of any cultural backgrounds that might be brought to bear, although I have not spoken to members about this. (I would need ethical clearance from our University to do this!) But judging from discussions that have occurred in the meetings, I would maintain that even if members believe in the principle of objectivity, they (or most of them who support this for themselves) appreciate that not all researchers subscribe to this principle for (ethical) research practice.

In addition, also judging from the discussions, it seems to me that most people on the committee would not see it as the "normal" task of researchers to become involved in working with participants and stakeholders to review options for forwarding social and ecological justice—unless the research has been labelled as participatory action research or as community engaged research. Nonetheless, I would suggest that because in the university as a whole, community engaged research is encouraged this means that when researchers on their application form

indicate that they wish to work with communities to create beneficial outcomes, there is a climate for appreciating that this is a worthy goal of the research; and this goal can also "spill over" into other (more "conventional") research endeavors. (In February 2017, a sub-committee on community engaged research was set up in our ERC in order to give more attention to the meaning and practice of such research.)

Readers may read my story as a somewhat optimistic account of possibilities via ERCs for facilitating dialogically-based reflections on ethical issues. I have not dealt with the issue of whether review boards should, as Hammersley recommends (2009, p. 220), *offer advice* rather than being regulatory bodies. As regulatory bodies, they do have power to demand that a project becomes changed if it appears to the committee (after dialogue in the committee) that it is not consistent with "ethical practice". I have suggested that just because they do have this "regulatory" power, it is all the more incumbent on members of IRBs (and on people who can volunteer to become members) to be aware of different ways of viewing ethical principles and practices, and not to reduce "ethics" and ethical discussions to the kinds of guidelines as laid out in the Belmont report.

For the Research and Innovation week held in 2017 (March 2017), a number of researchers from various departments raised the idea in one of our College research meetings that we should have a panel discussion at the 2017 Research and Innovation week to discuss the mode of operation of the College ERC, which they felt was still too regulatory. This panel discussion was accordingly arranged. Panel members included the chair of the College ERC, the manager of research integrity from Unisa's research support directorate, various members of departments (some of whom had expressed the need for the ERC to be less regulatory), and I too was invited to sit on the panel. The panel members—a mix of Black and White panellists—offered different visions of how the College ERC might function better so that it does not turn into a committee to "monitor research proposals". This was a function which it was argued should rest with higher degrees committees in various departments, while the ERC can better serve as a committee concerned with fostering ethical sensitivity. Meanwhile it was agreed that panel discussions such as the one organized on 1 March 2017, with invitations for audience participation, can become a forum to share experiences.

Some (Black) participants in the audience expressed concern that the current Unisa ethics policy (and its application by ERCs) was at loggerheads in some places with what would be considered ethical in an Africanized research ethics approach. For example, sampling practices may in some cases rely on (trusted) chiefs in communities being given the go-ahead to choose participants, which do not fit into any of the categories of sampling methods provided in our ERC application form; also, before asking people to offer "consent" to participate in the research, some type of gift (a goat, for example) may need to be offered as an entry point into the community. This would seem to clash with the notion of not offering "inducements" to participate (as stipulated in our form). In response, the (White) person from the research support office stated that Unisa is in the process of trying to "Africanize" the Unisa ethics policy, which she admitted still relies on a Westernized model of ethics. But she indicated that she felt that there is some

convergence—such as in the ethical concept that dignity must be afforded to research participants. She suggested that we need to keep open the conversation concerning the possible differences between ethical positions, and in the meantime ERCs need to operate with some flexibility. She suggested that it is important that ERCs have people serving on the committees such that there is a multiplicity of views on ethics being represented. It was underscored by the (Black) panel chair that what he was hearing from all the panellists was that people need to act in terms of a sense of collegiality, where they are also willing to learn from one another. A few days later, after the head of research in our College wrote a letter thanking the panellists for all of our participation, the manager of research integrity in the Unisa research support office emailed the following to us all:

> It was an honour to participate in this event. I was inspired by the deep sense of ethical commitment that I could depict from the conversations, yet the ever present concern for over regulation was evident. The call for shifts toward a more African approach was noted and appreciated. I believe more engagement about this topic is needed—governance without dialogue … is not true governance. (Rheta Visagie, 6 March 2017, quoted with her permission.)

This to me suggests that there is indeed some scope for dialogue, which is also appreciated by the research support office (or at least by this officer!)

Mertens and Ginsberg make the point that in certain scenarios "strict adherence to the codes and/or regulations as defined by governments and ethic boards simply may not be possible" (2008, p. 499). This, however, does not preclude trying to negotiate power issues, as part of a process of trying to build trust not only with those doing "regulating" work in these institutions but also with colleagues, community members and participants", which categories, they suggest, sometimes can overlap—2008, p. 492.

8.6 Conclusion

In this chapter I examined the principles of the Belmont report and their suggested application as the backdrop to my discussing how these can become reconfigured when the concern is with using the research space for people to constructively co-explore ways of living/being in society. The principle of *respect for persons* when reconfigured becomes translated into the requirement to *develop relationships with participants, communities and wider audiences* which recognize their contribution to, and rendering significant of, the research in terms of (re)considering the quality of social and ecological living.

Considering the principle of *beneficence*, one could argue that the Belmont report's discussion of this principle recognizes that harms (for participants) and benefits (for participants and for the wider society) need to be taken into account when setting up a research project. But for the authors of the report this is a matter or assessing potential harm/injury to participants and weighing this against the fruits

of *gaining an improvement of knowledge*, which in turn can benefit people (those who can benefit from the knowledge obtained). I have suggested in this chapter that if we appreciate, as I have argued in the rest of the book, that the very process of knowledge production is a process which is already imbued with performative effect, then giving thought to the *likely consequences of the research framing* takes on other dimensions, not accounted for in the Belmont report. I have also tried to show with reference to my detailed discussion of "beneficence" how this can be reconfigured so that consequences of the research enterprise (such as social and ecological healing, strengthened relationality, and empowerment to activate a better quality of social and ecological life) *can be consciously fostered as part of the research remit*. I have furthermore exhorted that *justice* requires that those involved in the research enterprise actively seek opportunities for using the research space to generate, as Cram puts it, a "more just and equitable world" (2015, p. 685). As Cram affirms, this does not mean to say that the conversation around what this means is finalized; but I would suggest that we have a responsibility to *use the research space to strengthen the quality of this conversation.*

Having focused on these various ways of surpassing the guidelines for ethical research conduct as laid out in the Belmont report, I went onto make some suggestions for researchers dealing with IRBs (including for researchers who are, or who volunteer to become, members of IRB committees). I suggested that they can consider their responsibilities as trying to activate discussion around rules and codes which seem unnecessarily restrictive of ethical conduct in research. I pointed to some possibilities for this (as I interpreted them) with reference to a committee at Unisa in which I became, and continue to be, involved.

When discussing the deliberations of those proposing forms of community engaged research (see Sect. 8.2.2) I illustrated how the contributions of various parties (in this, as well as in research not necessarily labelled as "community engaged"), might change over time: this means that research relationships can be regarded as in process of development. I made the point that in any research that is intended to be participatory-oriented, we need not adhere to a monovocal account of the meaning of "participation". I suggested with reference to illustrative examples from Chaps. 2 to 6 that an "*emtic*" relationship, defined as the development of meeting points between so-called "outsiders" and "insiders"—where this dichotomy becomes shifted in practice—can take many forms. As "outsiders" engage actively with research participants, they become part of the "community" by indicating their commitment to ensuring that *ways of defining issues of concern* as well as *options for action* are *sought as part of the process of involvement with participants and stakeholders*. Likewise, participants and stakeholders can *participate in the research effort in a variety of ways*, contributing (in active fashion) to the manner in which it is handled so that it is rendered meaningful and fruitful. In Chaps. 2–6, I pointed to some examples of what Dickson-Swift, James, Kippen, and Liamputtong (2006, p. 853) call the "blurring" of roles between professional researchers and participants/co-researchers as each contribute to the research endeavor.

References

Arieli, D., Friedman, V. J., & Agbaria, K. (2009). The paradox of participation in action research. *Action Research, 7*(3), 263–290.

Austin, W. (2015). Addressing ethical issues in participatory research: The primacy of relationship. In G. Higginbottom & P. Liamputtong (Eds.), *Participatory qualitative research methodologies in health* (pp. 22–39). London: Sage.

Belmont Report: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research (1979). *Ethical principles and guidelines for research involving human subjects*, Washington DC, Office of the Secretary, US Department of Health and Human Services.

Bongmba, E. K. (2014). Spirituality: An intersubjective practice. In N. N. Wane, F. A. Akena, & A. A. Ilmi (Eds.), *Spiritual discourse in the academy* (pp. 33–50). New York: Peter Lang.

Brooks, R., Te Riele, K., & Maguire, M. (2014). *Ethics and education research*. London: Sage.

Brown, P., Morello-Frosch, R., Brody, J. G., Gasior Altman, R., Rudel, R. A., Senier, L. … Simpson, R. (2010). Institutional review board challenges related to community-based participatory research on human exposure to environmental toxins: A case study. *Environmental Health, 9*(39), 1–12.

Bubar, R., & Martinez, D. E. (2017). Trickster as resistance: Impacts of neoliberalism on indigenous research and indigenous methodologies. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry in neoliberal times* (pp. 136–150). New York: Routledge.

Castro-Reyes, P., Cooks, J., Drew, E., Edwards, K., Freeman, E., & Isaacs, M-L. … Wat, E. (2015). *Research ethics reconsidered in the context of community-engaged research.* American Public Health Association (APHA) Annual Meeting and Expo, October 31–4 November, APHA.

Chilisa, B. (2009). Indigenous African-centered ethics: Contesting and complementing dominant models. In D. M. Mertens & P. E. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 407–426). London: Sage.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., & Ntseane, G. (2010). Resisting dominant discourses: Implications of indigenous, African feminist theory and methods for gender and education research. *Gender and Education, 22*(6), 617–632.

Connolly, K., & Reid, A. (2007). Ethics review for qualitative inquiry: Adopting a values-based, facilitative approach. *Qualitative Inquiry, 13*(7), 1031–1047.

Cram, F. (2009). Maintaining indigenous voices. In D. M. Mertens & P. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 308–322). Thousand Oaks, CA: Sage.

Cram, F. (2015). Harnessing global social justice and social change with multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 677–687). Oxford: Oxford University Press.

Cram, F., & Mertens, D. M. (2015). Transformative and indigenous frameworks for multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 91–109). Oxford: Oxford University Press.

Crow, G., Wiles, R., Heath, S., & Charles, V. (2006). Research ethics and data quality: The implications of informed consent. *International Journal of Social Research Methodology, 9*(2), 83–95.

Czymoniewicz-Klippel, M. T., Brijnath, B., & Crockett, B. (2010). Ethics and the promotion of inclusiveness within qualitative research: Case examples from Asia and the Pacific. *Qualitative Inquiry, 16*(5), 332–341.

De Koning, K., & Martin, M. (1996). Participatory research in health: Setting the context. In K. De Koning & M. Martin (Eds.), *Participatory research in health* (pp. 1–18). London: Zed Books.

Denzin, N. K. (2001). The reflexive interview and a performative social science. *Qualitative Research, 1*(1), 23–46.

Denzin, N. K. (2003). *Performance ethnography: Critical pedagogy and the politics of culture*. Thousand Oaks, CA: Sage.

Denzin, N. K., & Giardina, M. D. (2007). Introduction: Ethical futures in qualitative research. In N. K. Denzin & M. D. Giardina (Eds.), *Ethical futures in qualitative research: Decolonizing the politics of knowledge* (pp. 9–44). Walnut Creek, CA: Left Coast Press.

Denzin, N. K., & Giardina, M. D. (2009). Introduction: Toward a politics of hope. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry and social justice* (pp. 11–51). Walnut Creek, CA: Left Coast Press.

Denzin, N. K., & Lincoln, Y. S. (2003). Introduction: The discipline and practice of qualitative research. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (pp. 1–45). Thousand Oaks, CA: Sage.

Denzin, N. K., & Lincoln, Y. S. (2008). Introduction: Critical methodologies and indigenous inquiry. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and indigenous methodologies* (pp. 1–20). Thousand Oaks, CA: Sage.

Dickson-Swift, V., James, E. L., Kippen, S., & Liamputtong, P. (2006). Blurring boundaries in qualitative health research on sensitive topics. *Qualitative Health Research, 16*(6), 853–871.

Dillard, C. B. (2008). When the ground is black, the ground is fertile: Exploring endarkened feminist epistemology and healing methodologies in the spirit. In N. K. Denzin, Y. S. Lincoln, & L. T. Smith (Eds.), *Handbook of critical and indigenous methodologies* (pp. 277–292). Thousand Oaks, CA: Sage.

Ellis, C. S. (2017). Manifesting compassionate autoethnographic research. *International Review of Qualitative Research, 10*(1), 54–61.

Ellis, J. B., & Earley, M. A. (2006). Reciprocity and constructions of informed consent: Researching with indigenous populations. *International Journal of Qualitative Methods, 5*(4), 1–9.

Flicker, S., Travers, R., Guta, A., McDonald, S., & Meagher, A. (2007). Ethical dilemmas in community-based participatory research: Recommendations for institutional review boards. *Journal of Urban Health, 84*(4), 478–493.

Gergen, K. J. (2009). *Relational being: Beyond self and community*. New York: Oxford University Press.

Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.

Gilligan, C. (1982). *In a different voice: Psychological theory and women's development*. Cambridge, MA: Harvard University Press.

Glass, K. C., & Kaufert, J. (2007). Research ethics review and aboriginal community values: Can the two be reconciled? *Journal of Empirical Research on Human Research Ethics, 2*(2), 25–40.

Goodson, I. F., & Gill, R. S. (2011). Understanding the life history research process. *Counterpoints, 386,* 35–54.

Guta, A., Nixon, S. A., & Wilson, M. G. (2013). Resisting the seduction of "ethics creep": Using Foucault to surface complexity and contradiction in research ethics review. *Social Science and Medicine, 98,* 301–310.

Haggerty, K. D. (2004). Ethics creep: Governing social science research in the name of ethics. *Qualitative Sociology, 27*(4), 391–414.

Hammersley, M. (1995). *The politics of social research*. London: Sage.

Hammersley, M. (2009). Against the ethicists: On the evils of ethical regulation. *International Journal of Social Research Methodology, 12*(3), 211–225.

Hammersley, M. (2010). Creeping ethical regulation and the strangling of research. *Sociological Research Online*, *15*(4), Art. 16.

Hammersley, M., & Gomm, R. (1997). Bias in social research. *Sociological Research Online*, *2*(1), Art. 2.

Hammersley, M., & Traianou, A. (2014). An alternative ethics? Justice and care as guiding principles for qualitative research. *Sociological Research Online*, *19*(3), Art. 24.

Harris, L. D., & Wasilewski, J. (2004). Indigeneity, an alternative worldview: Four R's (relationship, responsibility, reciprocity, redistribution) vs. two P's (power and profit). Sharing the journey toward conscious evolution. *Systems Research and Behavioral Science, 21*(5), 489–503.

Higginbottom, G., & Liamputtong, P. (2015). What is participatory research? Why do it? In G. Higginbottom & P. Liamputtong (Eds.), *Participatory qualitative research methodologies in health* (pp. 1–21). London: Sage.

Honan, E., Hamid, M. O., Alhamdan, B., Phommalangsy, P., & Lingard, B. (2012). Ethical issues in cross-cultural research. *International Journal of Research and Method in Education, 36*(4), 386–399.

Israel, M., & Hay, I. (2006). *Research ethics for social scientists: Between ethical conduct and regulatory compliance*. London and Thousand Oaks, CA: Sage.

Jackson, S., & Scott, C. (2002). Introduction: The gendering of sociology. In S. Jackson & C. Scott (Eds.), *Gender: A sociological reader* (pp. 1-26). London: Routledge.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.

Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.

Liamputtong, P. (2010). *Performing qualitative cross-cultural research*. Cambridge, England: Cambridge University Press.

Liamputtong, P. (2011). *Focus group methodology: Principles and practice*. London: Sage.

Maathai, W. (2006). *Unbowed: A memoir*. New York: Anchor Books.

Magnat, V. (2012). Performative approaches to interdisciplinary and cross cultural research. In L.-H. Smith & A. Narayan (Eds.), *Research beyond borders* (pp. 157–177). Plymouth, England: Lexington.

Mayan, M., & Daum, C. (2014). Politics and public policy, social justice, and qualitative research. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry outside the academy* (pp. 73–91). Walnut Creek, CA: Left Coast Press.

Mazonde, I. N., & Msimanga-Ramatebele, S. H. (2007). Research coordination and ethical considerations in Botswana. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa: Ethical issues* (pp. 191–198). Addis Ababa: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

McKay, V. I., & Romm, N. R. A. (2015). Narratives of agency: the experiences of Braille literacy practitioners in the Kha Ri Gude South African mass literacy campaign. *International Journal of Inclusive Education, 19*(4), 435–456.

McIntyre-Mills, J. J. (2014a). *Systemic ethics and non-anthropocentric stewardship*. New York, NY: Springer.

McIntyre-Mills, J. J. (2014b). *Transformation from Wall Street to wellbeing*. New York, NY: Springer.

Mertens, D. M. (1999). Inclusive evaluation: Implications of transformative theory for evaluation. *American Journal of Evaluation, 20*(1), 1–14.

Mertens, D. M. (2007a). Transformative paradigm: Mixed methods and social justice. *Journal of Mixed Methods Research, 1*(3), 212–225.

Mertens, D. M. (2007b). Transformative considerations: Inclusion and social justice. *American Journal of Evaluation, 28*(1), 86–90.

Mertens, D. M. (2010). Transformative mixed methods research. *Qualitative Inquiry, 16*(6), 469–474.

Mertens, D. M. (2012). Ethics in qualitative research in education and the social sciences. In S. D. Lapan, M. T. Quartaroli, & F. J. Riemer (Eds.), *Qualitative research: An introduction to methods and designs* (pp. 19–39). San Francisco, CA: Jossey-Bass.

Mertens, D. M., & Ginsberg, P. E. (2008). Deep in ethical waters: Transformative perspectives for qualitative social work research. *Qualitative Social Work, 7*(4), 484–503.

Mertens, D. M., Harris, R., & Holmes, H. (2009). Transformative research ethics. In D. M. Mertens & P. Ginsberg (Eds.), *Handbook of social research ethics* (pp. 85–102). Thousand Oaks, CA: Sage.

Minkler, M. (2004). Ethical challenges for the "outside" researcher in community-based participatory research. *Health Education and Behavior, 31*(6), 684–697.

Monaghan, L. F., O'Dwyer, M., & Gabe, J. (2013). Seeking university Research Ethics Committee approval: The emotional vicissitudes of a "rationalized" process. *International Journal of Social Research Methodology, 16*(1), 65–80.

Molyneux, S., & Bull, S. (2013). Consent and community engagement in diverse research contexts: Reviewing and developing research and practice. *Journal of Empirical Research on Human Research Ethics*, *8*(4), 1–18.

Nel, N. M., Romm, N. R. A., & Tlale, L. D. (2015). Reflections on focus group sessions regarding inclusive education: Reconsidering focus group research possibilities. *Australian Educational Researcher*, *42*(1), 35–53.

Noddings, N. (1984). *Caring: A feminine approach to ethics and moral education*. Berkeley and Los Angeles: University of California Press.

Oczak, M., & Niedźwieńska, A. (2007). Debriefing in deceptive research: A proposed new procedure. *Journal of Empirical Research on Human Research Ethics, 2*(3), 49–59.

Onwuegbuzie, A. J. (2012). Introduction: Putting the *mixed* back into quantitative and qualitative research in educational research and beyond: Moving toward the radical middle. *International Journal of Multiple Research Approaches, 6,* 192–219.

Putt, J. (2013). *Conducting research with indigenous people and communities*. Sydney: Indigenous Justice Clearinghouse.

Roman-Alcalá, A. (2013). *Occupy the farm: A study of civil society tactics to cultivate commons and construct food sovereignty in the United States*. Paper presented for discussion at an international conference held at Yale University entitled Food Sovereignty: A Critical Dialogue, 14–15 September (Paper 75).

Roman-Alcalá, A. (2015). Broadening the land question in food sovereignty to northern settings: A case study of occupy the farm. *Globalizations, 12*(4), 545–558.

Romm, N. R. A. (2001a). *Accountability in social research: Issues and debates*. New York: Springer.

Romm, N. R. A. (2001b). Critical theoretical concerns in relation to development. In J. K. Coetzee, J. Graaff, F. Hendricks, & G. Wood (Eds.), *Development theory, policy and practice* (pp. 141–153). Cape Town: Oxford University Press.

Romm, N. R. A. (2015). Conducting focus groups in terms of an appreciation of indigenous ways of knowing: Some examples from South Africa. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *16*(1), Art. 2.

Romm, N. R. A. (2017). Conducting focus groups in terms of an appreciation of indigenous ways of knowing: Invoking an Indigenous-oriented paradigm. In P. Liamputtong (Ed.), *Doing cross-cultural research in health social sciences.* New York: Springer (in press).

Ross, F. C. (2007). Codes and dignity: Ethics and research on violence. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa* (pp. 25–35). Addis Ababa: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

Sikes, P., & Piper, H. (2010). Ethical research, academic freedom and the role of ethics committees and review procedures in educational research. *International Journal of Research and Method in Education, 33*(3), 205–213.

Singapore statement on research integrity. (2010). Retrieved April 6, 2016, at: www.singaporestatement.org.

Smith, L. T. (1999). *Decolonizing methodologies: Research and indigenous peoples*. New York: Zed Books.

Smith, L. T. (2005). On tricky ground: Researching the native in the age of uncertainty. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (3rd ed., pp. 85–107). Thousand Oaks, CA: Sage.

Ssali, S. N., & Theobald, S. (2016). Using life histories to explore gendered experiences of conflict in Gulu District, Northern Uganda: Implications for post-conflict health reconstruction. *South African Review of Sociology, 47*(1), 81–98.

Stephens, O. A. (2012). *Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, Nigeria.* Doctoral dissertation, Lagos State University, Lagos, Nigeria.

Stiegman, M. L., & Castleden, H. (2015). Leashes and lies: Navigating the colonial tensions of institutional ethics of research involving indigenous peoples in Canada. *International Indigenous Policy Journal*, *6*(3), Art. 2.

Tindana, P. O., Singh, J. A., Tracy, C. S., Upshur, R. E., Daar, A. S., Singer, P. A., & Lavery, J. V. (2007). Grand challenges in global health: Community engagement in research in developing countries. *PLoS Medicine*, *4*(9), e273.

Truman, C. (2003). Ethics and the ruling relations of research production. *Sociological Research Online*, *8*(1).

Tyldum, G. (2012). Ethics or access? Balancing informed consent against the application of institutional, economic or emotional pressures in recruiting respondents for research. *International Journal of Social Research Methodology, 15*(3), 199–210.

Van den Hoonaard, W. C., & Hamilton, A. (Eds.). (2016). *The ethics rupture: Exploring alternatives to formal research-ethics review*. Toronto: University of Toronto Press.

Wilson, S. (2008). *Research is ceremony: Indigenous research methods*. Halifax: Fernwood.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.

Woldegies, B. D. (2016). Research highlighting options for gender empowerment in Amhara Regional State, Ethiopia. *South African Review of Sociology, 47*(1), 58–80.

Chapter 9
Practicing Multiple and Mixed Methods Research Responsibly: Some Paradigmatic Considerations

Abstract In this chapter I engage with some of the discourses in the multiple and mixed methods research (MMMR) literature. I consider five paradigmatic positions (postpositivist, constructivist, transformative, pragmatic, Indigenous) in relation to the practice of MMMR, while appreciating that paradigmatic positions are not unitary and also can evolve. In keeping with, and stretching somewhat, the transformative research paradigm—while also looking at options for stretching "other" paradigms—I propose a focus in all social research on how those involved in the research endeavor (the initiating researchers and others involved) can take some responsibility and co-responsibility for the potentially impactful character of the research. This means that their "cognition" becomes recognized as being tied to considerations of the shaping effects of the research in/on the quality of our social and ecological existence. It is this recognition which can inspire an intent to advance more "just" social outcomes—and hopefully ecological outcomes too—as discussed in the research context.

9.1 Introduction

In this chapter I engage with some of the discourses in the multiple and mixed methods research (MMMR) literature, with a focus on foregrounding and extending possibilities for envisaging responsible research practice. As Creswell and Garrett indicate, there is no one definition of what constitutes multiple or mixed methods research: this is currently "being discussed among scholars" (2008, p. 321). My overview that I present in this chapter leads me to suggest that in the discussion on MMMR there is too much focus on doing research with *cognitive intent*, rather than giving attention to how all those involved in the research process might take into account, and assume some responsibility for, the potential impact of the research in/on the social and ecological worlds of which it is a part.

I organize my overview by locating five paradigmatic positions on MMMR and exploring them in some depth, with a view to highlighting options for taking into consideration the consequential character of social inquiry in terms of its potential

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_9

social (and ecological) impacts. I point out that this taking-into-account is a stance advised specifically by proponents of transformative and Indigenous paradigms—which advice I extrapolate further in the discussion. But I argue that other positions too can in principle cater for a recognition that those involved in the research endeavor—including the initiating researchers, participants/co-researchers, stakeholders—need to take some responsibility (and co-responsibility) for the world-forming potential of social research. This is whether or not MMMR is being called upon. The issue as explored in this chapter is how research can be practiced with such a sense of responsibility in mind (and in heart) so that at the moment of doing research, "cognition" is recognized as tied to action implications. As with the layout of Chaps. 7 and 8 (on generative theorizing and ethics), I elucidate my considerations in this chapter with inferences regarding multiple and/or mixed methods research that I draw out from the texts of (and some personal communications with) the authors whose work I discussed in Chaps. 2–6.

But before this, I first point to some of the ways in which researchers writing about MMMR justify the use of multiple or mixed methods. The manner in which I am setting this out here is by extrapolating how authors working from different standpoints in relation to the role of research and researchers in society, conceive MMMR. I start with the position as expressed by Hunter and Brewer (2015a, 2015b), which I see as implying a postpositivist conception of science, albeit that their understanding could also be seen as accommodating some "stretching" of usual conceptions of this—depending on how we interpret, and ourselves try to stretch, the meaning(s) of their various statements.

I then turn to other paradigmatic positions and I perform the same stretching exercise, focusing on the issue of responsible research practice. I thus use my discussion on MMMR research in this chapter to deliberate around the practice of MMMR and at the same time to propose some stretching of the paradigms discussed in Chap. 1. As with other concepts, the meaning of paradigms can and does evolve as people (self-named proponents and others) look at "them" with new lenses. Just as mixed methods research is not a "thing" which exists independently of how various researchers construct its meaning and consider its value (as Hesse-Biber, 2015, p. 775, reminds us) so the same goes for paradigms, which should not be reified as "things" existing outside of our way of deploying them (for instance, to guide our research work, to clarify our position(s) for ourselves and for others, and to "place" others' implicit assumptions as we see/locate them). Furthermore, self-named proponents can also learn from their engagement with "other" paradigms (as they define them in conversation with others) as, for instance, is proposed as a possibility by authors such as: Chilisa (2012), Cram and Mertens (2016), Gregory (1996), Johnson and Schoonenboom (2016), Mertens et al. (2016), Midgley Nicholson and Brennan (2017), and Stephens, Jacobson, and King (2010). That is, we can learn from further self-reflecting on the implications of holding any particular position, and by considering its likely consequences for our practices and for our involvement in society.

9.2 Some Postpositivist-Oriented Deliberations on MMMR

I start this section by offering a perspective of MMMR from the standpoint of Hunter and Brewer, whom I regard as endorsing a postpositivist view/paradigm of science, albeit with some space for stretching the usual conception of postpositivism (as for example, expressed in typologies of paradigms, including the ones offered in Chap. 1). Hunter and Brewer (2015a, p. 621) draw a distinction between multiple and mixed methods research with reference to considerations of types of measurement that can be invoked when measuring "data". They explain their position by noting that measurement does not apply only to quantitative measurement, but includes any way of expressing data or "phenomena of observation" (2015a, p. 619). *Multimethods* research implies using methods that measure one type of data; while *mixed methods* research implies using methods that measure different types of data:

> Firstly, multimethods takes a broad view of the meaning of measurement itself. Measurement may be either quantitative or qualitative in nature. Multimethods can include *multiple qualitative methods* (e.g., interviews and ethnographic observations). Second, it can include *multiple quantitative methods* (e.g., survey Likert-type questions and archival data such as grades in school). Third, it can include a *mix of quantitative and qualitative methods*. That is generally what is referred to as "mixed method" research' (e.g., quantitative Likert survey questions and qualitative data such as life history). (2015a, p. 621, my italics)

Here they consider that what is generally called mixed methods research (MMR) involves use of "different types of data and modes of analysis" (2015a, p. 621). They suggest that common ground that is common to the uses of these different types of data and modes of analysis is that they share the "central concerns of all sciences" (p. 621). The concern of all research as they identify it (p. 621) is to "discern patters" and to do so in ways which allow statements about these patterns to be rendered falsifiable (see also footer a in Table 1.2 of Chap. 1). Interestingly, they admit that in their understanding of measurement and analysis of data they do not wish to make the claim that either quantitative-styled or qualitative-styled methods can lay claim to measuring "reality" as it is. They state:

> The only way we may know what reality might be, is through our observations and systematic research. We cannot compare the results of our research to reality, because we do not know what that reality might be. (2015a, p. 622)

They go on to note, though, that:

> We may place an intuitive higher probability of having captured some aspect of reality from a given piece of research if we have followed procedures that attempt to address various threats to the validity of our assertions. But we recognize that all research methods have systematic biases and errors—known and unknown. All methods and measurement are flawed and have errors, and we [as scientists] strive to reduce these to the best of our ability. (2015a, p. 622)

While suggesting that researchers need to strive to the best of their ability to "reduce flaws and errors" they point out that:

> We can only assess the validity of our research when we compare our research results to other observations and other research results, not directly to reality itself, but it is a reality always filtered through observation and measurement. (2015a, p. 622)

As they see it then, using multiple and or mixed methods research is one way of comparing observations and research results against other observations and results. They add the qualification that this does not mean that researchers need to be alarmed if results yielded by the different methods fail to converge.

> Divergent results from a multimethod piece of research [either multiple or mixed methods research] may speak to different aspects of reality that the different methods are tapping into. … Triangulation [in the sense of locating convergence to confirm results against other methods] may occur or it may not; triangulation is not the "goal" per se; if it occurs, well and good, but absence of "same results" is also knowledge. (2015a, p. 621)

It is noteworthy here that when mentioning that results may not necessarily converge, they use the language of "tapping into" different aspects of reality; thus implying that somehow the methods are tapping into/grasping "reality", although they are recognized as doing so in different ways. As they further explain, "different methods may be seen … to create different facts that may or may not converge", but together they can produce "different facets of a more general and perhaps nuanced conclusion, a different level and type of facticity" (2015a, p. 619). They suggest that the idea when using MMMR is ultimately to "test" any theories that become generated, by using the scientific approach to what Popper (1959, p. 10) calls *falsification*, to which Hunter and Brewer too refer (2015a, p. 621, my italics).

As in Popper's account of the *Logic of Scientific Discovery* (1959), Hunter and Brewer do not believe that an assessment of validity (or confirmation that we have understood any aspects of reality) can be finally achieved. As they aver:

> It is a misconception of science that it purports to state the universal truth about reality. It is much more modest in its claims and in its conclusions. In this day and age, when there are many critics of science and the social sciences specifically, it is important that we do not oversell what science can do and acknowledge its limitations. But at the same time we should not sell it short. (2015a, p. 623)

They explain that although "science is an imperfect system of arriving at a conception of reality … it is far better and more powerful than any others yet devised" (2015a, p. 623). This conception of science is very similar to the one advanced by Popper who states that on the one hand, it is true to say that "our knowledge is vast and impressive" (1994, p. 100). On the other hand, it is also true to say that "our ignorance is boundless and overwhelming" (1994, p. 100). Popper suggests that "both of these theses are true, and their clash characterizes our knowledge situation" (1994, p. 100). For Popper, it is this clash that "inspires the advance of knowledge, and … determines its ever-moving frontiers" (1994, p. 100). Hunter and Brewer (2015b) express this argument in terms of the metaphor of a quest:

> It is a quest that begins with a question leading to a search using research to explore the unknown. It is a purposeful journey to find a treasured goal, to go out into that real world to acquire knowledge. One hopes that the knowledge that one acquires has some validity that is "true" and that what one believes to be true about the phenomena bears some close approximation to reality. (2015b, p. 201)

I have named their position as postpositivist in orientation due to the similarities that I see between their position and the one endorsed by Popper (1959, 1969), which many authors associate with a postpositivist stance because it represents a more "sophisticated" approach to science than a foundationalist position. (See Chap. 1, Sect. 1.2.) The argument here is that science does not have a firm empirical foundation on which it can be seen to rest (cf. Adam, 2014; Phillips & Burbules, 2000), but is nevertheless able to advance our knowledge. Phillips and Burbules, who self-identify with a postpositivist position, suggest that Popper can be regarded as "a key figure in the development of nonfoundationalist postpositivism" (2000, p. 26).[1] But they also point out that postpositivism itself is "not a unitary system of thought", which means that their own rendition of it may not be the same as that provided by others.

Morgan meanwhile notes that the labels of positivism or postpositivism as used to categorize others' work, has often been applied in the social science literature by those who identify with constructivist and/or critical positions, who treat positivism/postpositivism as the "conventional wisdom", in order to pose an alternative (2007, p. 56). As he notes, "the larger debate was about a challenge to the 'conventional wisdom' in social science research and not merely about methods" (2007, p. 56). In other words, considerations around whether use of quantitative methods (normally associated with positivism) or qualitative methods (normally associated with interpretivism/constructivism) are better, was not what the debate was about; it was rather about what people believed they were doing in terms of the *status of the* "*knowledge*" *supposedly attained* via *the use of any method, which was at stake*.

I would suggest in this regard that it is no accident that when speaking about the potential value of using different types of data and modes of analysis, Hunter and Brewer use as an example a combination of "Weber's (1949) *verstehen* with

[1]Popper criticizes certain aspects of positivism (as featured in, for example, August Comte's views and in the views of the so-called Vienna Circle of positivist thought), but draws on other aspects to develop what he calls a critical rationalist position (cf. Romm, 2001, p. 20). Popper considers that his critical rationalist position (as he names it) differs fundamentally from the foundationalist premises of the positivist view of science (1959, pp. 15–19). This is because even basic observation statements (as he calls them) about particular occurrences, are recognized by him as requiring a decision as to whether they should be accepted (1959, p. 105). Furthermore, other statements (at higher levels of generality) also can never be verified with certainty, but only tentatively corroborated—1978, p. 86. Adam points out that what are called postpositivist perspectives draw, inter alia, on "the ideas of falsificationism" as advanced by Popper (Adam, 2014, p. 5).

Durkheim's 'social facts' in the same piece of work" (2015a, p. 622). Both Durkheim and Weber endorsed the idea that social scientists must develop empirical sociological investigation in their work, so that the understandings that they produced could be argued to more or less accurately reflect "realities" rather than being a reflection of the value-laden beliefs of the researchers organizing the investigation. (See Mazman, 2008[2]; Münch, 1988.) Hunter and Brewer's choice of a Weberian stance combined with a Durkheimean one to express the possibility of mixing qualitatively directed research with more quantitatively directed research (as in MMR) suggests that they consider that science must be properly directed toward striving as far as possible to generate "objective" knowledge by focusing on empirical investigation.

As part of their deliberations on the use of multiple or mixed methods research, they do not state that multimethod research is always preferable to mono-method research. Indeed, they argue that

> Monomethod research is at times and under certain conditions and contexts the way to go. We definitely prefer well-executed monomethod research to less competent multimethod research. (2015b, p. 187)

Here they refer to the prime importance of scientists being competent to develop research findings that rest on "building an argument about the link between observations and data about the world and one's ideas and theories" (2015b, p. 200). The focus is on linking observations to theories so that no matter what method(s) are used, scientists set out to base their theories in evidence as observed.

Now Hunter and Brewer suggest one of the goals of science is to convince wider audiences in the scientific community and in the wider public that one's results—and ways of linking theories to evidence—seem plausible (2015b, p. 200). They maintain that one of the goals of social science is to "convince others of the tentative truth of one's assertions about questions posed" (2015b, p. 199). They do not find it problematic that scientists try to thus persuade people of what they call the "truth value" of assertions being made (2015b, p. 200). But they also explain that:

> We operate from the assumption that the real world does exist (physicalism) and yet recognize that our knowledge of that real world is a varied and imperfect product of the observations we make of it (relativism).

Despite their admission that we cannot ever lay claim to grasp "reality", they indicate that "one hopes that the knowledge that one acquires has some validity" (2015b, p. 201). And on the basis of this hope, they suggest that scientists can try to use "the art of persuasion" to convince others of its validity, or at least of its

[2]Mazman argues that "although both sociologists try to apply the positivistic method in their sociological studies, they differ in terms of how they understand positivism to be a scientific method" (2008, p. 81). Munch (1988, p. 3) notes how Weber tried to combine "idealistic and positivist elements" in his scientific approach to forwarding interpretations of meaning while proffering causal explanations.

"tentative truth" (p. 199). They point out that this is whether the goal of science is "understanding alone" or is concerned with offering "prescriptive policy implications" (2015b, p. 200).

It seems then that they see the responsibility of scientists as that of trying—using quantitatively directed and/or qualitatively directed measurements and modes of analysis—to make some (albeit recognized as tentative) assertions about realities. These assertions can be focused on the purpose of understanding (as in basic research) or can be used for purposes of creating policy recommendations on the basis of the information/knowledge to date (2015b, p. 200).

With this conception of science, can we stretch their position to the point of suggesting that just because we can never know to what extent we are creating a match with some posited external reality, we should be cautious of using science with rhetorical force to persuade others of the validity (albeit imperfect) of the knowledge, defined as an attempt to mirror reality? At one point in their deliberations they suggest that "from a pragmatic perspective it [the presented knowledge] is valid if the knowledge works, if it allows one to accomplish what one wishes to get done" (2015b, p. 201). But they do not delve further into whom the "one" is to whom they are referring. Nor do they delve further into how one might define what is "working". Could this pragmatic position, which recognizes that we cannot ever step out of our discourses to "find" reality as it is, be an opening to take some responsibility for our very way of framing the research agenda? Could this become an opening to realizing that any understandings and apparently "informed" policy recommendations are the product of specific ways of measuring and analyzing that might well have been different if we chose, with others concerned, to frame the initial research question(s) differently?

As I elucidate in Sect. 9.7, there may be room for recognizing within such a position—with some stretching—that the responsibilities of scientists/researchers are (or can become) wider than thus far expressed by Hunter and Brewer. At this point, though, Hunter and Brewer do not deliberate around how the way of organizing the study and relating to participants on the basis of the setting of the research question, may already make a difference to how participants themselves come to "see" the issues (which are being identified for attention). It would seem to me that Hunter and Brewer consider the sole commitment of scientists (as in the Singapore statement, 2010, that I mentioned in Chap. 8) as being to try to capture realities which, as they understand it, are in the "physical" world (physicalism). Yet they do admit that "the only way we may know what reality might be, is through our observations and systematic research" (2015a, p. 622). In this research process, "we cannot compare the results of our research to reality, because we do not know what that reality might be" (2015a, p. 622). This implies that if, in the process of "measuring" we come to affect what is supposedly being measured *we would not know to what extent this is the case*, because all that we have access to is what is being "seen". And a further implication of this to me is that we do not need to hold

onto the assumption that the goal of science is to provide knowledge of how the world operates "as it is". Could we not explore it as a process of "*becoming*" (as researchers interact with research participants)?

Hammersley argues that his own non-foundationalist position goes hand in hand with an admission that that there may be "methodological problems" in using the research process to try to "tell us about 'the real world'" (2003, p. 343).[3] That is, he suggests that there may be methodological problems in seeking to access social reality as it exists independently of researchers' involvement in studying the social world. But by making this statement, Hammersley indicates his allegiance to the notion (which I outlined in Chap. 8, Sect. 8.1) that researchers must strive to "produce knowledge", albeit that there is no firm foundation for establishing it. When Hammersley asserts that "virtually all social research" is aimed at establishing how the world operates independently of our knowing efforts, he is of course making certain assumptions. This is why authors who find value in the concept of "paradigm" feel that paradigmatic considerations allow us to clarify these assumptions so that they can be subject to rethinking.

I have tried to show that one way of rethinking the assumptions, is by taking into account that science presented as offering tools (albeit not perfect ones) to try to circumvent the influence that researchers' may have on "realities" when setting out to study them, may be unnecessarily constraining of alternative modes of thinking about the responsibilities of scientists/researchers. I have also suggested that the understanding that scientists must present their work in persuasive mode to try to convince others in academia and in the wider social arena of its (albeit tentative) validity, is a specific understanding that can—and has—been questioned. It can be questioned on the basis that professional researchers can take more responsibility (with others) for their interaction with what is "observed" and for ways of presenting (or rather *discussing*) the status of "results". In Sects. 9.7.2 and 9.7.4, I offer further thoughts on this in the context of discussing some of the work to which I referred in earlier chapters (especially Chaps. 3 and 5), which I see as involving a stretching of what might be seen as a postpositivist position. I recognize that the stretching exercise that I perform, means that the borders between paradigms need to be regarded as permeable, so that, as Chilisa (2012, p. 25) proposes, paradigmatic thinking does not constrain thought along exclusivist lines of thought concerning the meaning of "research" and research endeavors in society.

[3]Hammersley argues that the problem of what is called "reactivity", defined as the problem that what is being researched may be affected by the very process of the research, does not constitute an irresolvable dilemma—as there are various ways of trying to address the reactivity effect. He suggests that "researchers have long been aware of reactivity as a problem, and have sought to deal with it in practical ways: by varying their role in the field over time, and/or by using multiple methods of data collection (for example, combining observation with data from informants)" (2003, p. 344).

9.3 Some Constructivist-Oriented Perspectives on MMMR

Before I begin to point to some deliberations on MMMR from what can be considered as a more constructivist-oriented position, I offer a brief outline of what Lincoln and Guba (2003) call the *anti-foundational* premises of constructivism, in order to suggest its distinction from either foundational or non-foundational approaches (as discussed above). Lincoln and Guba state that:

> Constructivists … tend toward the anti-foundational. … As one of us has argued, truth—and any agreement on what is valid knowledge—arises from the relationship between members of some stakeholding community (Lincoln, 1995). (2003, p. 273)

Lincoln and Guba suggest that the "postmodern turn" (as described by Best and Kellner, 1997) with the

> emphasis on the social construction of social reality … and the partiality of all truths, will simply overtake modernist assumptions of an objective reality, as indeed, to some extent, it has already done in the physical sciences. (2003, p. 274)[4]

They believe that in time, the dualist idea that there is an "objective reality" to be known via the (humanly subjective) research enterprise, will become seen as a "quaint" idea—similar in status to "flat earth theories" (2003, p. 274). As I have noted above and in Chap. 8, Sect. 8.1, clearly some researchers such as Phillips and Burbules (2000), Hammersley (2003), and Hammersley and Traianou (2014) do not regard this as a quaint idea, as they argue that the quest to try to find out about realities as they exist independently of the research effort is indeed the goal of science. Hunter and Brewer for their part refer to the postmodernist view that "facts" as seen through the research process depend upon some degree of collective consensus (of the scientific community) and are not observable without some presuppositions (2015a, p. 619).[5] Nonetheless, Hunter and Brewer's position cannot be classed as constructivist in the sense of their subscribing to an anti-foundationalist stance as elucidated by Lincoln and Guba (2003), where the goal of striving to get closer to a posited (external) reality is relinquished.

What I wish to discuss in this section now, is what an anti-foundational position, broadly understood, might argue vis ā vis the use of MMMR. I do this by looking at the outline as given by Hesse-Biber (2010) in her account of qualitatively-driven mixed method approaches (guided by forms of constructivism and various critical

[4]See Chap. 1, Sect. 1.1.2, where I pointed to the Heisenberg uncertainty principle as applied to the physical sciences.

[5]It could be argued that because Hunter and Brewer take more seriously the postmodern turn, which they interpret as implying that "facts" can be considered as constructed through a degree of "collective consensus" in the scientific community, this would then provide more leeway than in a Hammersley's position for (professional) researchers to take some responsibility for the way in which "truths" become constructed and presented.

theoretical positions) and the argument of Hesse-Biber and Griffin (2015), where they assess certain cases illustrating feminist approaches to multimethod and mixed methods research.

Hesse-Biber points out that according to a constructivist orientation to research, "there is no 'objective' social reality 'out there'" to be discovered through the research process (2010, p. 455). In other words, there are multiple stories to be told regarding "lived experience" in the social world (2010, p. 455). This position can also align with, and be incorporated within, "a core assumption" of feminist perspectives, namely that "knowledge does not exist outside of the social world. There is no view from 'nowhere'; instead, all knowledge contains a perspective" (2010, pp. 455–456). Hesse-Biber furthermore sees links between this feminist assumption and certain postmodernist orientations to research, which suggest that "the search for 'truth' is not a goal of this perspective; rather reality is assumed to always be 'representational' [re-presenting visions of reality] rather than 'real' or 'truthful'" (2010, p. 455). This, as Hesse-Biber notes, implies that as far as the nature of knowledge-building is concerned, it is recognized that what becomes "known" springs from the encounter between researchers and research participants and is a function of the way in which the research is directed (2010, p. 456).

I now concentrate on some of the arguments on MMMR as expressed by Hesse-Biber and Griffin from a feminist perspective, as outlined in Hesse-Biber and Johnson's edited book (2015) on multimethod and mixed methods research inquiry. In laying out their discussion, Hesse-Biber and Griffin elucidate that:

> While qualitative method and the use of *multiple qualitative methods* are an important part of feminist research praxis, mixed methods (qualitative *and quantitative*) … [can] lend themselves [to] … feminist research goals. (2015, p. 75, my italics)

Like Hunter and Brewer (2015a), Hesse-Biber and Griffin refer to multiple methods (in this case multiple *qualitative* ones); and also, like them, suggest that mixed methods implies the use of *quantitative-type* and *qualitative-type* methods as part of the study. But Hesse-Biber qualifies this statement by insisting that in order to avoid a *methods-centric* understanding of the use of mixed methods research (MMR), we need to recognize that *methodology*—which she defines as including theoretical/philosophical stances in relation to our conception of the nature of human existence and our conception of the nature of knowledge construction—*will inform the way in which methods become used* in MMR (2010, p. 456). She spells out, for instance, that if the search is to unearth multiple visions of reality rather than searching for "the truth", this will affect the way in which MMR becomes handled. With this understanding of MMR as not merely a matter of mixing methods, but also of *reflecting on what one is doing* in the process, Hesse-Biber and Griffin propose that mixed methods (rather than monomethod or multimethod research) might better be suited to assist researchers in pursuing the following feminist research goals:

- "Exploring women's subjugated knowledge by giving voice to women's experiences", and by ensuring that "traditional" research is gender sensitive and is not androcentric.

- "Exploring multiple understandings of the nature of social reality" (by using both quantitative and qualitative lenses to generate "understanding").
- "Studying across differences in terms of race, class, gender, and so on" (by foregrounding how these constructions are generated in social life and people's felt experiences of their consequences).
- "Fostering social justice and social change on behalf of women and other oppressed groups" (2015, p. 75).

They add the proviso that neither multiple nor mixed methods research should be conducted just for the sake of organizing such designs. In other words, "two methods may not be better than one" (2015, p. 87). And they add the caution that no matter what method(s) are being used, there is always the potential problem of which researchers may need to become more aware, namely, that they may "inadvertently introduce androcentric biases to their projects" (2015, p. 88). They suggest that MMMR, when used, has the advantage of "increasing the layers of meaning that often remain hidden" (p. 88).

In laying out their considerations on all of this (with reference to various cases of research that they choose to explore), they consider that it is important to "be cognisant of one's research standpoint", acknowledging how this might

> play a role in how we conduct [construct] our research in terms of: a) what questions are asked or not asked in our research; b) what type of data is or is not collected; and c) what type of methods, measurement, analysis and interpretation shape our understanding of the research process. (2015, p. 79)

They also state that we need to be wary of so-called mixed approaches which are infused with "dominant [positivist-inspired?] perspectives on knowledge building" (2015, p. 80). They give one example of such a mixed methods research project, as organized by Buck et al. (2009). This project used a quantitative scale called the "Attitudes Toward Science Inventory (ATSI) scale … designed to measure fifth grade African American girls' and boys' attitudes toward science", combined with eight focus group discussions with three-four girls each (Hesse-Biber & Griffin, 2015, p. 77). They indicate that from a feminist lens one can question whether the "mixed methods study design adequately gave voice to young African American girls' attitudes, problems and hopes" as they expressed them during the focus group discussions (2015, p. 78). They convey their concern that the girls' stories were not

> placed in conversation with the quantitative component but instead were used to enhance and validate findings from the quantitative study. The goal of seeking these voices became used to buttress a pre-existing measure [as given in the scale]. (2015, p. 78)

They propose that the researchers should have reflected more deeply on the meaning of the qualitative results, to indeed *reconsider the value of the scale* (and its specific items) as a measurement device to measure what may be important to such girls. They state in their overall assessment that

> Buck et al. (2009) utilized the ATSI scale to understand young girl's attitudes toward science but as we saw relying too heavily on this as a measure alone limited their ability to unearth new knowledge. (2015, p. 88)

They consider this as having been a missed opportunity (2015, p. 82).

They offer as another example which they feel was better designed, a study (Johnson, 2007) in which multimethods were employed, via interviews and ethnographic participant observation, in order to explore "Black and minority students' science classroom experiences" (Hesse-Biber & Griffin, 2015, p. 79). They indicate that as part of increasing the validity of the study, Johnson employed member checks at different points of the research process, which consisted of "asking her female participants for feedback on their interview transcript as well as the analysis of the data she had collected" (2015, p. 79). Also, because during the interviews many of the students had expressed doubts about continuing with science in further studies (despite their interest in it) this generated new questions for Johnson to pursue by using the ethnographic observation to consider the wider structural barriers these women face (2015, p. 80). They thus point out how the multimethod approach allowed it to be rendered visible in what way "science practiced in the classroom setting often conflicts with younger women's own values and attitudes as expressed in interviews" (2015, p. 80). They summarize that Johnson's research explains the importance of "supporting minority women's strong interest in science" (2015, p. 80).

Overall, Hesse-Biber and Griffin plead that ultimately feminist-inspired research should have the consequence of being supportive of women in their aspirations. They believe that this can be achieved in various ways via the research process. Hesse-Biber and Griffin do not concentrate on detailing a constructivist position when speaking about feminist approaches to MMMR studies, as they note that feminist approaches to research (which can include use of MMMR) "are applicable across a spectrum of views on reality, from positivism to postmodernism" (2015, p. 88). What they wish to draw out, it seems, is that even if one is postpositivist in orientation, one must be careful of injecting androcentric biases into the research process, and one should not miss opportunities to explore what may be important and meaningful for women.

Pointedly, when speaking about the study by Johnson (2007) they iterate that as a validation tool Johnson "allowed the participants to review the interview transcript" and she "changed the transcript in response to their feedback" (2015, p. 80). What I would suggest here is that as the interviewees saw the transcript and were given the opportunity to look at their statements and to "analyze the data", *they were at the same time changing both the data and themselves,* as they were re-reflecting on what they had said. Furthermore, what they indicated to be their aspirations need not be regarded as static, because I would say that they (arguably) *deepened their own understandings in interaction with the researcher's initial questions and in interaction with the transcript* that she later supplied to them to review. The "data" generated through the initial encounters as well as through the later member checking and analysis encounters could thus be argued to be as a *product of the interactions*.

Hesse-Biber and Griffin do not highlight this aspect of the study, which to me would imply that the participants were becoming *more aware of their constructs* (and their aspirations) *just by speaking about this as an issue now being explored*

(via the research). In other words, it could be said that the research process involved an interaction between "researcher" and "researched" in which the participants did not remain unchanged, as they *developed insights/meanings that could serve to activate their sense of aspiration*. Furthermore, Johnson herself was committed to developing further insights as part of her ongoing activist work, which included being "actively engaged in increasing the access of women of color to the social sciences" (Johnson, 2007, p. 809, as cited in Hesse-Biber & Griffin, 2015, p. 79). Hesse-Biber and Griffin consider that Johnson practiced "strong reflexivity" so that her observations/interpretations which she developed during the research encounters would not just be a function of her prior thinking/experiences (2015, p. 80) According to Hesse-Biber and Griffin, Johnson was conscious not to allow her prior understandings to overshadow her observations; so that she could indeed learn from the research experience. This would thus presumably be helpful in her continued activism.

In Hesse-Biber and Griffin's account of the various case studies, how researchers might become involved in trying actively to gear their work toward "social justice" goals is nevertheless left somewhat ambiguous. Hesse-Biber probably would not be concerned with such ambiguity as she suggests that the type of research work that she, as a feminist, supports, "requires a knack for tolerating ambiguity" (2007, p. 348). She indicates that research undertaken from a feminist lens is "not for the 'fainthearted' and it often requires attention to detail, perseverance in the face of chaos, and a knack for tolerating ambiguity" (2007, p. 348). She sees this as a journey that is nevertheless "well worth taking". She adds that:

> For feminists, this journey is always bound to and guided by a set of principles, commitments, and concerns that extend beyond their own particular research projects. For feminist researchers, the knowledge-building process is necessarily linked to a commitment to unearthing the knowledge of women and others who have historically been marginalized. For us, the journey is a site where the personal and political merge and multiple truths are discovered and voiced where there had once been silence. (2007, p. 348)

How the personal and political merge for the researchers involved can of course vary. Lincoln and Guba, in setting out their understanding of constructivist-oriented research (e.g., 2003, 2013), are more intent to offer criteria of validity that more clearly introduce an action-oriented commitment into the very process of knowledge construction. Hence they propose that constructivism introduces into the debate about validity "*extended considerations of validity (goodness criteria)*" which include "relational and ethics-centred criteria" (2003, p. 263, my italics). As I indicated in Chap. 8, Sect. 8.1, this means that they see that knowledge is closely tied to action at the moment of knowledge building/construction. The knowledge construction process harbors the potential for professional/academic researchers to play an active role in alerting themselves, as well as research participants and stakeholders (co-researchers) to implicitly-held constructions while locating—as part of a mutually rewarding encounter—possible alternative constructions which might be applied/activated in practice (2003, p. 262). This would also be in line with the systemic (eco)feminist position espoused by Stephens, Jacobson, and

King, who cite Romm (1996) in their understanding that the realm of "action" cannot be separated from the realm of cognition. As they aptly express it:

> Romm (1996) states that individuals are responsible for the way various modes of cognition, or world views, may influence outcomes. Ethical practice requires relinquishing the neutralist claim that one's impact upon the world is decided in a realm separate from one's mode of cognition. (2010, p. 562)

This ethical stance of mine as they delineate it, implies that those involved in research endeavors should use the research space to stimulate further social "activity" in the social (and ecological) realm as part of researcher responsibilities, as I explored when discussing some of the work of authors showcased in earlier chapters of this book.

As far as the issue of mixing methods is concerned, this would mean that any mixing has to be undertaken such that what Lincoln and Guba (2003, p. 263) call "goodness criteria" become applied when doing the research. Lincoln & Guba (2003) and Denzin and Lincoln (2003) make the point that they do not consider that the use primarily of qualitative *methods* means in itself that the research can be placed under the banner of *constructivist-oriented qualitative research*; furthermore, they make some provision for what may be called qualitatively-oriented research to include some quantitative components (e.g., Lincoln & Guba, 1985, pp. 198–199; Denzin & Lincoln, 2003, p. 15). However, Denzin and Lincoln expressly question whether "positivist methods" as they call them (which are heavily quantitatively-directed to getting to grips with the social world in numerical form) can be considered as "no better or worse than other methods" and as just another way of "telling stories", to be equally respected in the research endeavor (2003, p. 15). Their position seems to be that insofar as mixed methods are used, these should still be what Hesse-Biber would term "qualitatively driven" (2010, p. 455). In addition, Denzin and Lincoln (2003) and Lincoln and Guba (2003) emphasize that the judgement of the quality of the research should also rest on criteria that are *not entrenched in positivist/postpositivist assumptions that action considerations are outside of the domain of the responsibilities of researchers* (2003, p. 262). Lincoln and Guba's worry is that in positivism/postpositivism as they see it, a commitment to become involved in, or stimulate, "action" as part of the research effort is viewed as "advocacy" and as a "threat to validity" (2003, p. 262). This critique levelled by Lincoln and Guba against both positivism and postpositivism is linked to their view that the truth-seeking criteria of positivism/postpositivism are too ethically restrictive.

In short, what I wish to underscore is that those self-defining themselves as constructivist in orientation might well consider that it is part of the remit of those involved in research endeavors to take some responsibility for the realm of "action", which is not seen as a separate realm from "cognition" (Stephens, Jacobson, & King, 2010, p. 562). Whatever method(s) are used in a research study (or across studies) can thus be interrogated with a view to considering *their potential social effects* in use and with a view to seeking novel use of them accordingly. The requirement to innovatively employ methods with this in mind may involve some

"stretching" of meanings associated with "constructivism" in the research literature. That is, sometimes constructivism is identified with researchers setting out (merely) to *explore and interpret* the social construction of multiple realities. Hence Creswell, for instance, states that the constructivist researcher's "intent … is to make sense of (or interpret) the meanings others have about the world" (2003, p. 9).

But if we realise that—like all stances—constructivism is not a unitary position, and also can be regarded as evolving as a construct, then we can *develop the construct of constructivism to more explicitly foreground the issue of researcher responsibilities in taking into account the way in which social realities become formed* via *the very process of research interaction.* I explain this further in my discussion in Sect. 9.7 of some of the examples taken from Chaps. 2–6, which I suggest imply a use of MMMR to include an active component in recognizing such responsibilities.

Mertens (2014, p. 44) for her part suggests that when constructivists operate with a conception that it is part of researcher responsibilities to take on board (with others) how cognition is—and can be more consciously—tied to the realm of action, they can be considered as entering "transformative" paradigmatic territory (as in her discussion of the transformative paradigm). That is, if we assume that there are borders (albeit fuzzy) that researchers cross to reach "transformative" research territory, we can conceptualize such constructivists as wandering into this territory. She refers, for example, to the stances of authors such as Denzin, Guba, and Lincoln, as expressed in various writings of theirs, which connect constructivist qualitative inquiry to "social justice and progressive political action" (2014, p. 44). As I noted in Chap. 1, Sect. 1.3.6, this is one reason why Mertens speaks of the permeability of paradigmatic "borders".

However, it is also important to re-iterate (see Chap. 1, Sect. 1.3.4) that when discussing links between action-oriented research and constructivist research, Lincoln draws attention to what she sees as a time problem for those academics who may wish to "commit themselves to a [community] group over some prolonged period of time, time enough to see change through" (2001, p. 130). She states that:

> For many academics, disciplinary affiliation and institutional loyalty are strong pressures in professional life. These pressures often combine, in the form of professional meetings, "keeping current in one's field", teaching, advising students, overseeing dissertation research, and intra-institutional governance responsibilities. (2001, p. 130)

Lincoln therefore avers that there may be differences in emphasis between constructivist-directed and traditional-styled action research. In the latter, cycles of participatory planning and implementation of forms of action with dialogical observations and reflections on outcomes are all seen as part of the research endeavor. (See Chap. 1, Sect. 1.3.4.) Lincoln's considerations suggest that research practiced along these lines depends on the "particular relationships between [professional] researcher and researched [co-researchers] in the field" (2001, p. 130).

In the next section I pursue this point further, and suggest that perhaps the transformative paradigm itself might need to be "stretched" to take into account that the cycles of involvement with participants and stakeholders that Mertens envisages

as characterizing transformative research (including participatory action research), can be reviewed in situ, depending on the particular commitments that the various parties offer at points in time. This would imply again that the definitions of the paradigms can alter in meaning—as researchers who wish to identify as "transformative" offer new visions of what an "action" commitment may involve. That is to say, we can *widen the space of the transformative paradigm to allow it to cover those researchers who wish to be active in some way but not in the way currently conceived by Mertens,* which seems to imply long-term cycles of involvement with participants and stakeholders in communities, as I explain in the next section.

Lincoln suggests that some university-based researchers do manage the "long term commitments and heavy responsibilities"; and some of them do this by, say, involving their (postgraduate) students (2001, p. 130). There is also the possibility of organizing teams of researchers, as for example, seems to me to be the case in many of the examples cited by Mertens as falling under the umbrella of the transformative paradigm. What connects the constructivist position espoused by Lincoln with the transformative one espoused by Mertens is that "*advocacy*" *(widely defined) is appreciated as falling well within the responsibilities of social inquirers*. In the next section I turn to Mertens' understanding of the methodological stance associated with the transformative paradigm, and her vision of how MMMR can be practiced guided by such a paradigm.

9.4 Some Transformative Perspectives on MMMR: Transformative Research Terrain

In Chap. 1, I offered a detailed account of Mertens' layout of the transformative paradigm as expressed in a number of her writings. What I wish to detail here is how she sees that MMMR can become infused with transformative-directed intent. To start with, it is worth noting that when explaining philosophical positions that can inform the use of mixed methods research (MMR), Creswell and Garrett state that while many authors assume a pragmatic position (which I discuss in the next section) "others argue for a philosophy based on advocacy of under-represented groups (Mertens, 2003)" (Creswell & Garrett, 2008, p. 327). They thus summarize that Mertens considers that MMR (and presumably MMMR) should be used with advocacy intent, that is, to actively advocate for those who are marginalized in the social fabric. But what does this mean in practice?

Mertens offers a myriad of examples of the use of MMR from a transformative orientation/commitment in her various writings. Below I focus on one of these as illustration. In the majority of her writings she does not address the distinction between MMMR and MMR. But in the chapter that Cram and her co-authored for the handbook of MMMR (2015) in which they consider both transformative and Indigenous frameworks for MMMR they refer to the notion that one can mix methods beyond "what is traditionally thought of as quantitative and qualitative

methods" (2015, p. 97). This would mean that multiple method research as supposedly combining the same "type" of methods (either quantitative or qualitative) and mixed methods research (as combining quantitative and qualitative) would give way to simply speaking of *multimethods,* without having to label the methods as quantitative and/or qualitative. Cram and Mertens refer to various methods to give credence to Indigenous knowledge systems as incorporated in stories, poems, folklore, dances, visiting traditional healers' surgeries, etc., which cannot easily be classed along the quantitative-qualitative divide. What they feel is important is that research needs to be able to respond to "different types of knowledge" (2015, p. 99).

They add the qualification that as far as "transformative methodological assumptions" are concerned:

> The transformative methodological assumption does not mandate the use of mixed methods; however, multimethod and mixed methods of research appear to be well suited to conducting an inquiry process that has the potential to lead to the necessary understandings and the use of the research as a mechanism for social change. (2015, p. 100)

What is important then is not so much the method or methods that become(s) used, but *how they are used to advocate for/advance social change toward increased social justice* as defined within the context of the research.[6] As Mertens (2014, p. 21) recaps, the transformative paradigm "places central importance on the lives and experiences of the [social] groups, that, traditionally, have been marginalized (i.e. women, minorities, and persons with disabilities"). In Sect. 9.6 I clarify how Cram and Mertens see points of connection between transformative and Indigenous paradigmatic positions. But for the moment I concentrate on their explication, citing Mertens (2009, p. 60) that:

> Typically, researchers associated with the transformative paradigm begin their studies with qualitative data collection in the form of document review to ascertain the historical context and interactions with relevant community members to gain understandings of the range of perspectives about the phenomena under study.[7] This is an opportunity to build relationships and to get a better idea of culturally responsive (Hood, Hopson, & Frierson, 2015) methodological options. …. This beginning phase of research might be followed with either a quantitative or mixed methods phase. (Cram & Mertens, 2015, p. 100)

[6]The issue of whether in any instance the mixed method research works with so-called dominantly QUAN (quantitative) methods or dominantly QUAL (qualitative) or equal status ones (where QUAN and QUAL moments are given equal weight) as referred to in much of the literature on mixed methods designs and as summarized by Johnson, Onwuegbuzie, and Turner (2007, p. 124), *would then give way to considering whether the inquiry is being undertaken in the service of those most marginalized in society*. (See also Romm & Ngulube, 2015, p. 171.)

[7]Mertens and McLaughlin make the point that when they speak of qualitative data collection, their focus is not on their possible use within a positivist framework, but on "the use of qualitative methods within the interpretive/constructivist and transformative/emancipatory traditions because of the unique criteria for judging quality associated with such research" (2004, p. 94). Like Hesse-Biber (2010, p. 456), they emphasize that *methodological* considerations exceed considerations around *methods* to be used.

Mertens also makes the point (2007) that no matter what methods are used, this should be done with a "conscious awareness of the benefits of involving community members in the data collection decisions with a depth of understanding of the cultural issues involved" (2007, pp. 219–220). And she continues that this should all be done to with a conscious awareness of "the need to tie the data collected to social action" (2007, pp. 219–220). This implies that community participation needs to be included "in the inquiry process at all levels" (2007, p. 220). If the research is to be used to advance social justice, as defined in the context of the research, community participation is needed at all stages, including at the stage of discussing the implications of results (derived from the data collection process) so that mechanisms can be created to enable "results"—as discussed—to be linked to action (2014, p. 33). The way in which this occurs can be determined as the research proceeds. As she affirms:

> The transformative methodological belief system supports the use of a cyclical model in which community members are brought into the research process from the beginning and throughout the process in a variety of roles. (2010a, p. 472)

The initial relationship building phase and the continuing of the research processes via mixed methods which is referred to here are all seen in the context of using research as an avenue for social change.

Mertens gives an example of her involvement (with a team) to explore the effectiveness of a teacher preparation program at Gallaudet University, which was

> designed to prepare teachers of color and/or who are deaf to teach students who are deaf and who have a disability (such as emotional disturbance, severe physical disabilities). (2010a, p. 470)

She indicates that the person (faculty member) who approached her to gather evidence regarding the program, wished the research to consist of a "minimalist effort to satisfy the [funding] agency" (2010a, p. 470). But she informed the person that she would accept the request to gather evidence only if she could do so in a way "that would contribute to social transformation" (2010a, p. 470). She sketched a plan to collect data that was consistent with her axiological beliefs. This entailed that she established

> a research team that was reflective of the cultural complexity of the deaf community. On the basis of my understanding of the historical legacy of power differentials in the world of deafness and deaf people's experiences of oppression at the hands of some hearing people who saw them as less than hearing (called audism), I assembled a team of researchers who reflected important dimensions of diversity in that community. (p. 470)

She states that due to the nature of the team, it was more likely that they would be able to consider "whose reality was being privileged" in the context under consideration (2010a, pp. 470–471). She indicates (p. 472) that the "basic data collection plan" included the following:

- Initial participant observation at a reflective seminar that the teacher preparation program hosted for its graduates (observation and document review).

- Interviews with the graduates who attended the seminar. The questions for the interviews were derived from the data gathered during the observations and document reviews.
- After the research team met and discussed the data collected from the observations, document reviews, and interviews, an online survey was developed that allowed the collection of both quantitative and qualitative data from the program graduates who were not able to attend the seminar.
- The team conducted both qualitative and quantitative analysis of the data and used this as a basis for conducting interviews with the university faculty and the cooperating school staff members. The words of the graduates and quantitative data from the survey were used as triggers for discussion in the faculty and school staff interviews (2010a, p. 472).

We can see from the building up of the research processes that the idea was to ensure that data collected at any stage were in turn subjected to discussion in which the participants (program graduates and university faculty) were involved via seminars or online modes of communication. Mertens states that "this sequence of methodological decisions illustrates the cyclical nature of a transformative mixed methods approach to research" (2010a, p. 472). What was important was that the data collected at all stages (and the discussion around the data) were in turn used as "triggers" for further discussion—with faculty and staff in interviews with them. This means that they became party to engaging with the issues as expressed and discussed by graduates, so that they could learn from this "data" while holding discussions with the research team. Mertens notes that other data were also shared with faculty and with school staff (for instance, data to the effect that when graduates started teaching, only a few reported having a mentor from Gallaudet University). This sharing, she suggests, was "part of the cycle of transformative methodology" (2010a, p. 473). And she reports that one faculty member (upon recognizing this as problematic)

> took it upon herself to approach the department of education to propose the establishment of an online seminar in the fall of 2007. The seminar was designed to provide a forum for program participants in their internships to communicate with faculty and each other as a means to accessing mentor support (2010a, p. 473).

Mertens indicates that the story does not, however, have a fairy tale conclusion. The funding for the multiple disabilities program ended; and faculty who had been involved went elsewhere. In addition, changes occurred at the "upper-level administration at the university". But the research team informed current faculty and staff of their findings and also spoke to the dean, who "made a commitment to integrating changes into the program that are responsive to the issues raised by this research" (2010a, p. 473). Clearly, the research team did not feel that their responsibilities ended after the data had been collected and a report created for the funding agency. The team looked for avenues for seeking "commitments" from those who may be able to take on board the issues raised, with a view to making further meaningful changes. In addition, the research team

> made a presentation at the annual meeting of the Association of College Educators for Deaf and Hard of Hearing (ACE-DHH) at which representative of universities and colleges with teacher preparation programs for teachers of deaf and hard of hearing students heard (and/or saw) the results of this research (Mertens, Holmes, & Harris, 2008). This generated discussion among those participants (including the new personnel who were responsible for teacher preparation at the home university) about the relevance of the issues uncovered in this study and strategies to address them. (2010a, p. 473)

Again we can see that the research team sought out avenues which would mean that the research would be likely to have catalytic effect, not only at the "home university" but elsewhere too. What I would also like to highlight is that, on Mertens' account, she made provision for

- the initial creation of a research team that would be *sensitive to issues of diversity in the community*;
- ways of organizing the "data collection" to *include moments of discussion* around issues that were being drawn out through the data collection process; and
- developing a *wider triggering of discussion* among faculty and more broadly beyond the university.

As far as discovering/capturing realities is concerned, although Mertens states that researchers subscribing to the transformative paradigm differ from constructivists in that the transformative paradigm holds that "there is one reality about which there are multiple opinions" (2010a, p. 470), it seems to me that this research itself was *not* working with the notion of there being "one reality" to try to "capture" (see also Chap. 1, Sect. 1.2.1). Rather, it was trying to *explore and indeed activate ways of seeing/constructing realities connected to options for action that could be experienced by those concerned and affected as more "just"*. Hence the idea was to create discussions at all points of the research process, so that people could consider the import of these discussions for themselves on the action level. In her description of the phases of the transformative research cycle, Mertens hones in on *how realities could become co-constructed in discussion*, with a view to various people (graduates, faculty members, the dean, the members of ACE-DHH) listening to "data" generated during the research process and to viewpoints on these, and *on this basis finding ways of creating new actions*—in line with what Mertens calls "consideration of social justice issues" (2010a, p. 470).

As I suggested in Chap. 1, Sect. 1.2.1, I do not believe that there is a need to posit a supposedly existing "one reality" in the social world in order for meaningful transformative research to ensue. Indeed, the positing of one reality could lead to people directing efforts at trying to uncover this "one reality" rather than *pursuing efforts to generate discussion around issues that are brought forward* via *the research and to locate options for hope-directed actions*.

If one concentrates on this aspect of the research effort (including the example that I have detailed above) then one can appreciate how the transformative paradigm has permeable borders with the "territory" of constructivism (as I mentioned in Sect. 9.3) and with pragmatism (which, at least in some formulations, conceptualizes knowing as tied to action, as I explain below).

9.5 Some Pragmatic Perspectives on MMMR

In their exploration of various definitions of mixed methods research, Johnson, Onwuegbuzie, and Turner (2007) indicate that they "asked many of the current leaders in mixed methods research how they define mixed methods research" (2007, p. 112). From their analysis of themes arising out of the various answers which they received (from 19 people), and from their engagement with additional literature on MMR, they suggest that:

> Many (or most) mixed methods writers have argued for some version of pragmatism as the most useful philosophy to support mixed methods research. We agree that pragmatism is a well-developed and attractive philosophy for integrating perspectives and approaches. (p. 125)

They go on to note that:

> A pragmatist [broadly defined] would reject an incompatibility thesis [where paradigmatic positions regarding the proper pursuit of social inquiry are seen as incommensurable] and would claim that research paradigms can remain separate, but they also can be mixed into another research paradigm [namely, a pragmatic one]. (2007, p. 125)

They take the view that "mixing" in MMR can and should occur at the level of philosophical understanding about the nature of social inquiry. They state that:

> We believe that one or more of the pragmatisms [admitting that pragmatism as a stance is not univocal] can provide a philosophy that supports paradigm integration and helps mixed research to peacefully coexist with the philosophies of [or normally associated with] quantitative and qualitative research. (2007, p. 125)

Johnson, Onwuegbuzie, and Turner arrive at the following definition of MMR which they feel encapsulates most of the literature on MMR, and which they feel is compatible with a pragmatic position:

> Mixed methods research is the type of research in which a researcher or team of researchers combines elements of qualitative and quantitative research approaches (e.g., use of qualitative and quantitative viewpoints, data collection, analysis, inference techniques) for the broad purposes of breadth and depth of understanding and corroboration. (2007, p. 123)

They elucidate that this definition encompasses the idea of using mixed methods in a single study or a program of study:

> A mixed methods study would involve mixing within a single study; a mixed method program would involve mixing within a program of research and the mixing might occur across a closely related set of studies. (2007, p. 123)

They indicate that although most of the definitions of MMR which they elicited from people whom they considered to be leaders in the field considered that the aim of MMR was to develop corroboration and/or breadth of findings via the study (or sets of studies), "a small number of researchers, suggested other purposes" (2007, p. 122). For example, they note that one such stated purpose was to "achieve social justice and avoid oppression" (p. 122). The last-named purpose was the one that

they found in Mertens' answer when they asked her about the definition of MMR (p. 120). Considering some of Mertens' writings on MMR, they remark that her overall approach to this

> is not driven by the research question; rather, it is driven by the researcher's quest to conduct research that is emancipatory, antidiscriminatory, participatory, and the like, which focuses squarely on the lives and experiences of marginalized persons or groups such as women; ethnic/racial/ cultural minorities; religious minorities; individuals with disabilities/ exceptionalities; and members of gay, lesbian, bisexual, and transsexual communities (i.e., transformative emancipatory research, Mertens, 2003). (2007, pp. 122–123, my italics)

While referring to Mertens' argument, Johnson, Onwuegbuzie, and Turner remark that from their analysis overall of the definitions of MMR as given by the various "leaders in the field" whom they sampled, a quest to use MMR to add depth and breadth to the way in which a research question could be approached and answered, was the far more common understanding of MMMR.

Creswell and Garrett refer to this pragmatic understanding of MMR, where MMR is used with a pragmatic underpinning:

> Pragmatism, as discussed by the mixed methods writers means that *the focus of research is on the research question* and *different methods can be employed to answer this question.* (2008, p. 327, my italics)

Nonethelesss, Hesse-Biber expresses concern that although pragmatism is seen by many mixed methods researchers as a way to get past what they view as the "paradigm wars",

> it appears as if the current more popular version of pragmatism has turned into a "practical *pragmatism*" that boils down to a "what works" approach that is able to sidestep the hard "epistemological issues" that "philosophical pragmatic" approach Dewey and colleagues were engaged with early on (see Mutch, 2009). (Hesse-Biber, 2015, p. 782)

She argues that a kind of pragmatism supported by Dewey and colleagues as a "philosophical pragmatic approach", has now commonly reverted to a stance where a research question is posed and whatever method(s) are seen as aiding to answer this question are employed to do so. But she entreats that we need to partake in more reflected-upon considerations as to *why particular research questions are posed in the first place*. She asks:

> Who gets to carve out and determine what knowledge becomes legitimated? To what extent does this process serve specific ends? What is lost? Who gets to challenge and reframe or rename a given concept? … . Just how the research question enters into the MMR project remains woefully unarticulated. (2015, p. 784)

Hesse-Biber suggests that a pragmatism in which researchers appeal to the supposed fact that their mixing of methods is "working" (or has "worked") to explore their research questions, leaves unexamined the problem of *who defines the research question* in the first place and *what criteria are used to establish* "*what is working*" as a research approach. She therefore feels that it is important that we

should strive to generate a more vibrant "mixed methods community", where people do not sidestep continued communication around such considerations (2015, p. 785).

What is worth highlighting at this point, as Johnson, Onwuegbuzie, and Turner (2007) report, is that many of the MMR definitions as put forward by proponents in their study suggested that the "key purpose' of MMR was to develop 'breadth (7) and/or corroboration (5)" (2007, p. 122). That is, 12 out of the 19 "leaders in the field" whom they consulted suggested this. What is further important to note is that Johnson, Onwuegbuzie, and Turner themselves in their own suggested definition, consider that MMR is a type of research which proceeds "for the broad purposes of breadth and depth of understanding and corroboration" (2007, p. 123). This is in keeping with Onwuebguzie and Leech's (2006) account of how one can ask a "mixed" question to aid breadth and depth of understanding. Onwuegbuzie and Leech (2006) explain how one can generate a "mixed" research question that calls for a mixed methods research approach. For instance, they outline that "quantitative" questions can be asked with a view to *identifying and investigating relationships* between what are considered to be dependent or independent variables (Onwuegbuzie & Leech, 2006, p. 480), while qualitative research questions "tend to *seek meanings, to explore a process, or describe experiences* (Onwuegbuzie & Leech, 2006, p. 482, my italics). Their suggestion is that mixed methods-type of research can ask "mixed questions" that *lend themselves to answering them through quantitatively- and qualitatively-directed data collection and interpretation processes*.[8] And they suggest that there are various ways of handling the integration of the data at the point of analysis and interpretation (2006, p. 492).[9]

But in considering that the purpose is to achieve greater *understanding* via this process, do Onwuegbuzie and Leech (as with the majority of "leaders in the field") afford sufficient justice to a philosophical pragmatism where truth-content is considered to be inextricably linked with seeking "the good life"—as for instance, one can interpret Peirce (1931–1958) as suggesting (with Dewey his student re-interpreting)? Morgan believes that pragmatism infused with Dewey's concept of inquiry "includes particular attention to issues of social justice as a broad agenda for social research" (2014, p. 1045). This is how Morgan understands the import of Dewey's pragmatic position (and other pragmatic approaches which focus on giving attention to the outcomes of our choices). He maintains, with reference to Dewey, that:

[8]This would imply mixing quantitatively-oriented research questions which gear the research to investigating hypothesized directional relationships between variables, with qualitatively-oriented questions which gear the research to exploring and understanding people's meaning-making and social processes. (See also Creswell, 2014, p. 50.)

[9]They qualify that when they speak of "integration" they refer to a research stage whereby both quantitative and qualitative data are integrated into either a coherent whole or two separate sets (i.e., qualitative and quantitative) of coherent wholes (2006, p. 491). That is, quantitative analyses (based on comparing groups and relating variables) can become integrated with theme development and theory generation based on analyzing texts (words) and images.

> When we do research, we make our choices according to what we believe is good or bad, right or wrong, and these choices clearly involve preferences between likely outcomes as we ask what difference it would make to do our research one way rather than another. (2014, p. 1048)

In this formulation of pragmatism, the task of inquirers is to reflect (with others) on choices of research approach, *recognizing that choices are value- and emotion-laden.* Morgan argues that pragmatism as he appreciates it asks questions such as:

> How do researchers make choices about the way they do research? Why do they make the choices they do? And, what is the impact of making one set of choices rather than another? Although these questions are not new, making them the center of our program for studying social research reorients us to a new set of issues and goals. (2014, p. 1051)

Morgan suggests that pragmatism as a philosophical position alerts us that:

> Research never occurs in a vacuum, so how is it influenced by the historical, cultural, and political contexts in which it is done? And how do our research communities come together to emphasize one way of doing things rather than another? (2014, p. 1051)

That is, he proposes that pragmatism as a philosophical basis for research (whether MMR-designed studies or not) alerts us as a research community to the need to rethink our research pathways, so that we can fundamentally reconsider our "way of doing things"—in this case, our ways of practicing "research". His argument is that seen in this light "pragmatism can serve as a philosophical program for [any] social research, regardless of whether that research uses qualitative, quantitative, or mixed methods" (2014, p. 1045). What is emphasized by a pragmatic approach is that we need to recognize that inquiries always incorporate a "political and moral dimension" (p. 1049), which we cannot sidestep.

Morgan makes the point that although interest in pragmatism in the social science literature history seems to have been connected to issues relating to the so-called paradigm wars, and efforts to find a resolution to these conflicts when practicing MMR, in its "deep philosophical content" (2014, p. 1049) it is more concerned with considering how our "knowing" is tied to politics and ethics (2014, p. 1051). Morgan contends that just as we should not caricature constructivism or postpositivism by offering "crude summaries" of the positions as we see them, we also should not offer crude summaries of pragmatism as a philosophy for social research which is confined to "an emphasis on practicality" (2014, p. 1046).[10] Morgan argues on the basis of his interpretation of philosophical pragmatism that there are clearly links at a philosophical level between such pragmatism and pursuance of "social justice" (2014, p. 1050).

In this kind of (philosophical) pragmatism—to which I pointed in Chap. 1, and extended and elaborated upon in Chap. 7—*knowing is seen as inextricably tied to*

[10]Creswell concurs with this point by stating that as a philosophical position "pragmatists agree that research always occurs in social, historical, political, and other contexts. In this way, mixed methods studies may include … a theoretical lens that is reflective of social justice and political aims" (2014, p. 11).

action at the moment of cognition (and not as a separate added on dimension). This pragmatism can then serve as a justification for a transformative research intent where the intent is to *advance justice at the moment of inquiry.* As far as the nature of "reality" is concerned, Morgan states that Dewey rephrases (and rejects) the metaphysical concern with reality (ontology). He states that "within Dewey's pragmatism and its emphasis on experience, ontological arguments about either the nature of the *outside world* or the *world of our conceptions* are just discussions about two sides of the same coin" (2014, p. 1048, my italics). Morgan believes that because Dewey concentrates on the *human experience* of reality, there is no need to pose the question as to the existence of reality in some posited outside world or whether it can be appreciated only through our conceptions. Nevertheless, other pragmatic theorists (such as Zhu, 2011; and Gergen, 2015) do offer what they consider to be a pragmatic understanding of "reality". As Zhu expresses it (2011, p. 792), the understanding is that "realities are never ready but continuously enacted, contingently made and relentlessly (re)produced"; and research efforts are part of the process of realities becoming *enacted.*

Hence, for instance, even patterns that appear to be found "in reality", can become recognized as, in Habermas's terms (1976, pp. 208–209), *quasi-causal*—and contingent on their continually being (re)produced by networks of actors. Insofar as research re-inforces their causal and stable character in people's consciousness, it contributes to their appearing as "given" (and intractable). Therefore, research is evidently not innocent in its consequences. Following this line of thought, *the implication is that researchers need to take some responsibility for the potential impact* of ways of asking and answering research questions. Or what Morgan alludes to, "our research communities" can reconsider how we are thus far concentrating on "doing things" when conducting research (2014, p. 1051). It seems to me that a philosophical pragmatic position which recognizes that cognition is already action-linked, would require of people involved in research endeavors to consider the way of doing the research so that possible action implications are rendered more conscious and deliberate. That is, the pragmatism endorsed by Morgan can be seen as providing for, and indeed calling for, such a recognition of responsibility. At the same time, Morgan's understanding of how the "research community" can reconsider pathways to responsible research practice can be "stretched" by drawing on transformative paradigmatic suggestions in regard to the need to

- *include key participants* in considering the way in which research issues are framed so that justice-oriented intentions are provided for in the research endeavor;
- *design the research approach innovatively* so that it can become *increasingly* "*emic*"[11];

[11]I use the word "emic" here in the sense used by Darling (2016), where she advocates "methodological adaptations that encourage emic perspectives throughout all phases of a study: design, data collection, and analysis" (abstract to the article). This would also mean developing in

- enable opportunities for *co-interpretation of* "*findings*" with (key) participants and stakeholders; and
- consider that *co-exploring options for action* is part of the research remit.

In other words, a focus on justice (as pleaded for by Mertens in her exposition of the transformative paradigm) would imply making efforts within a "pragmatic position" to *include these participative dimensions in the research process*. Of course, as indicated earlier, the borders between paradigms need not be seen as impermeable, as Morgan too reminds us that the separation of paradigms into airtight categories "is highly unlikely in a world where paradigms are created through competition and cooperation among human researchers" (2007, p. 62). I return to this issue in Sect. 9.7, when I use the examples taken from Chaps. 2–6 to deliberate further on ways of self-positioning ourselves and positioning others' research work (as part of our conversations). For now, in the next section, I turn to outlining the contours of an Indigenous research paradigm in relation to MMMR practice.

9.6 Some Indigenous Perspectives on MMMR

Before I begin my considerations regarding Indigenous paradigmatic conceptions of the practice of MMMR, I would like to re-iterate as I mentioned in Chap. 1 (Sect. 1.3.5) that not all Indigenous researchers would describe the way in which they conduct their research as belonging to a *paradigm*. As noted also by Cram and Mertens "some describe it as a theoretical lens, an approach or a perspective" (2015, p. 92). It is this lens which allows a focus on what is specifically relevant to Indigenous people, such as decolonization and protecting ways of life that are meaningful to Indigenous people (2015, p. 92). Chilisa (2012, p. 40) and Chilisa, Major, and Khudu-Petersen (2017, p. 1) have offered their views on what an Indigenous *paradigm* tends to concentrate; and Cram and Mertens (2016) too are content to use the terminology of *paradigms* when they refer to both transformative and Indigenous paradigmatic commitments (2016, p. 161). However, as with the other paradigms, clearly there is no need to posit a unitary "Indigenous" paradigm as if all researchers who self-position themselves as endorsing Indigenous paradigmatic research would see the focus in the same way. For the purposes of this discussion I concentrate on Cram's indication from an Indigenous perspective of how researchers can try to "harness global social justice and social change with MMMR" (2015, p. 677).

As part of her deliberations, Cram states that she has found that "knowledge is not power; rather, being able to *define what is acceptable knowledge* is power" (2015, p. 678, my italics). She here refers to the importance of researchers paying

---

the process what Onwuegbuzie (2012, p. 205) calls an *emtic* research relationship, where "etic" and "emic" perspectives are maximally interactive.

attention to "whose knowledge is being privileged within their research context" (2015, p. 678). This is linked to her contention that often the worldviews that are "current" in a society and in global discourses are not recognized as being *a* worldview but are thought of as being "*the* worldview" (2015, p. 681, my italics). Questioning how worldviews come to attain the status of being "the world view" is thus part of the task of Indigenous researchers. With this in mind, she suggests that MMMR studies (informed by an Indigenous lens) can be considered as "offering nuanced knowledge and wisdom about the complexities of how societies exclude certain groups of people" (2015, p. 679). She sees that MMMR can be helpful in that:

> MMMR can support the building of quantitative and qualitative understandings and interventions that help systems to change so that people can *remain themselv*es and experience social inclusion. (2015, p. 681, my italics)

Cram's concern here is that worldviews and ways of life as offered in Indigenous cultures are often not given credit when efforts at social inclusion are attempted in order to try to eliminate disparities in life chances.[12] She suggests that all too often Indigenous people's aspirations to practice an Indigenous worldview are disregarded (see also Cram, 2009, p. 309). Cram and Mertens point out that from an Indigenous research paradigm, inclusion implies

> more than a focus on overcoming disadvantage or eliminating disparities. It is about the needs and priorities that Indigenous people identify themselves, and about their own aspirations and dreams (Malezer, 2013). (Cram & Mertens, 2016, p. 179)

Cram's account of how MMMR can be used in the service of redefining dominant definitions of social inclusion is thus a call to recognize the way in which Indigenous people may define *quality of life*, which is not merely a matter of equity but a matter of experiencing wellbeing in keeping with Indigenous priorities. It is noteworthy here that in Cram's account of the value of MMMR above she immediately ties "understanding" to "interventions" rather than seeing these as separate components of a research project. She asks readers to further reflect upon the question of "what is the relationship between understanding a 'problem' and knowing how to solve it?" (2015, p. 685). By posing this question, she makes the point that ways of seeing "problems" in the first place already come with intervention implications.

Cram presents an example of a research question that was set in order to study a Romani neighborhood in Spain (Garcia et al., 2013). The study operated with the question: "in what ways does community involvement in education contribute to strengthen connections between education and diverse areas of society" (2015, p. 681). Cram notes that the communicative methods used in this study "evolved

[12]In my book on *New racism* I offered examples of research exploring this oft-unnoticed form of racism, which many authors argue is still experienced by people across the globe (Romm, 2010, pp. 282–290).

from dialogue with the communities and included interviews, focus groups, life stories, observations and questionnaires" (2015, pp. 681–682). She highlights that the research team (which consisted of Roma and non-Roma researchers) were intent on developing collaborative research with the community. She summarizes this as follows:

> The research team … demonstrated the transformative power of their mixed methods communicative methodology. They worked with the Romani neighborhood to identify culturally responsive intervention strategies and used the research findings to inform a more inclusive educational solution. (2015, p. 682)

The "more inclusive solution" that was conceived differed from the "remedial" one that was in place and that was considered by Romani families as making Romani children fit into the educational system, but did not address the issues as far as they were concerned—which required attending to issues of "social and cultural justice" (2015, p. 681).

Patently, the way in which the research question was devised—in conjunction with members of the community—meant that the research would already be geared to considering how education can contribute to strengthening connections between education and other areas of society: the question itself implied a *quest to challenge remedial social inclusion solutions*, in line with concerns of the Romani families (2015, p. 681). The research question was not presumed to be a "neutral" one, but was infused with an intent that understanding rooted in experiences of exclusion could be formulated with a view to setting up more inclusive practices. With reference to the work of Smith (1999), Cram points out that:

> Colonization and other exercises in exclusion and displacement rely on the ability of those in positions of power to dehumanize others (Smith, 2012) … . In order to support these people to become more fully human again, we need quantitative methods that identify the inequities that they experience and the determinant of these inequities based on population data. We also need quantitative and qualitative methods that allow us to represent the lived realities of these people within our research and evaluations. A key factor in this is the inclusion of members of these groups on our research teams, as leaders and advocates, so that our research and evaluation does not reinforce the status quo. (2015, p. 685)

I have cited this passage at length to show how and why Cram encourages use of MMMR, but also to show how she sees that research is likely to support and reinforce the "status quo" *unless it becomes consciously redirected.* (This is similar to Gergen's position that research is necessarily world-shaping: see Chap. 7, Sect. 7.2.)

Cram believes that it is important that MMMR is "community owned" (2015, p. 685). She also believes that MMMR can be put to use to "name, critique and challenge privilege and what counts as knowledge" (2015, p. 685). Along with this, she expresses hope that MMMR can speak to the hearts and minds of "those in positions of power" so as to develop connection between "those with privilege and those who are vulnerable" (p. 685). That is, she considers that it is possible that

some "sharing of knowledge and understanding" can occur across those with more privilege and those who are vulnerable. As she puts it:

> As an endeavor undertaken collaboratively with those whose circumstances researchers seek to improve, MMMR can speak to hearts and to minds, instilling motivation and harnessing resources needed for social change. (2015, p. 677)

Her continued concern is when certain privileged definitions of what counts as knowing and as knowledge come to rule the discussion around ways of seeing "realities" and (hence) of options for action.

As far as the link between "transformative" and "Indigenous" research paradigms is concerned, Cram and Mertens propose that:

> The alliance between the transformative and Indigenous agenda is … not an entirely comfortable fit; however, we take the position, at this point in time, that the Indigenous voice can be brought into the transformative paradigm as a way of stretching and enriching understandings of the meaning of conducting research for the purpose of social transformation. This is possible because the transformative paradigm has space within it for many worlds. (2015, p. 92)

Cram and Mertens suggest that the transformative paradigmatic space is capable of encompassing the "worlds" that might be considered as expressive of Indigenous thinking and being (see also my account of this kind of worldview in Chap. 1, Sect. 1.3.5). As I mentioned in Chap. 1 (Sect. 1.1.2) in considering links between transformative and Indigenous research agendas, Cram and Mertens note, citing Cram, Chilisa, and Mertens (2013), that

> the "bad name" that research has within Indigenous communities is not about the notion of research itself; rather it is about how that research has been practiced, by whom, and for what purpose that has created ill-feeling. (Cram & Mertens, 2015, p. 94, citing Cram, Chilisa, & Mertens, 2013, p. 11)

In the light of the way in which Indigenous communities have experienced more conventional research styles (where researchers define research questions and proceed accordingly to answer them supposedly with reference to "the data") the transformative and Indigenous paradigms both are critical of "paradigms of research that yield results that are not seen as useful, valuable, and/or accurate by members of marginalized communities and their advocates" (Cram & Mertens, 2015, p. 94).

Cram and Mertens now suggest that one of the advantages of a mixed methods research approach as practiced with transformative intent is that:

> Using mixed methods increases the likelihood that portions of the research findings will speak to a broad range of audiences. … the combined analytical strength of an integrated mixed methods analysis is a compelling combination of numbers and voices. For change-inducing research findings to enter into the channels stakeholders (including communities) can open, researchers need to build relationships with stakeholders. They then need to strengthen the capability of stakeholders to understand and advocate for research findings, especially why these findings should facilitate something transformative. (p. 94)

In other words, because the mixing of methods can offer findings using quantification-type language *and* expressive/narrative-type language, it may be able to influence audiences who are more comfortable with any of these languages. This can be considered as one of the strengths of MMR. But more than this, the strength of such an approach will not automatically flow from the fact that findings have supposedly been generated: what is important is to liaise with stakeholders so that findings can be discussed with a view to "facilitating something transformative" (2015, p. 95). And as noted earlier, "findings" themselves are not regarded as innocent in their content—because it is recognized that the way in which "knowing" takes place is a contested terrain. Cram and Mertens (2016) suggest that their main concern is when positivist-styled research serves in the "invalidation of Indigenous worldviews" and ways of knowing (2016, p. 181). Hence when evidence in support of arguments is confined to what positivism/postpositivism would regard as "empirical", this is problematic for Indigenous researchers (2016, p. 183). (See also Chap. 7, Sect. 7.4.3.) And insofar as stakeholders insist on the value of only positivist-styled reference to "evidence", a critique of this insistence needs to be instituted.

Considering the nature of the Indigenous paradigm I wish to close this section by remarking again that the Indigenous research paradigm itself need not be regarded as a unitary perspective. Some authors may emphasize, more so than others, that we need to take into account that undertaking culturally responsive research in engagement with communities does *not* mean that any particular culture that might be identified as associated with a community should be treated as a homogenous "entity". Many of the authors to whom I referred in Chap. 8, point to the need to recognize tensionful relations that might be experienced as operative in and across cultural groupings (as identified).[13] The term *culturally responsive research* as characterizing an Indigenous research commitment can therefore be "stretched" to highlight the intention to use the research space to *strengthen relationships and revitalize connectivity*, as I suggested when discussing the work of, for instance, Woldegies, and Ssali and Theobald in Chap. 4. This would mean that research is directed toward seeing culture as evolving and to *strengthening its evolving character in the direction of increased relationality*.

9.7 A Proposal for Paradigm Stretching: Highlighting Options for Researcher Responsibilities

In this section I begin by offering a tabular summary of the various paradigmatic positions as might pertain to the practice, and justification, of MMMR. In the first (extensive) row of the table I pinpoint certain paradigmatic positions that can be adopted in relation to MMMR, thus extending Table 1.2 as offered in Chap. 1.

[13]See also Romm (2017a, pp. 32–39).

Table 9.1 Paradigms and paradigm stretching: revisiting use of MMMR

	Postpositivist	Constructivist	Transformative	Pragmatic	Indigenous
Definitions of MMMR	MMMR involves mixing quantitative and qualitative types of measurement and analyses, both of which types can help discern patterns in the social world (while inquirers should be open to falsify any claimed assertions about these). When scientists compare results from different modes of observation and analyses this can aid their knowledge-seeking and lead them closer to the truth about that which is being striven to be understood. All statements made about reality must, however, remain tentative as they can only be tentatively confirmed or corroborated (not verified)	MMMR involves using different research methods while recognizing that both quantitatively-directed and qualitatively-directed inquiries issue in *constructions of realities* (which have the status of stories). MMMR requires being cognisant that researcher/research participant relations *generate* (rather than "find") the data that become constructed. In addition, emic ("insider") perspectives on the interpretation of data are encouraged. MMMR works with the understanding that the stance taken toward data generation and interpretation is more important than the method used	MMMR involves mixing methods and modes of analysis/interpretation while promoting participant and stakeholder involvement through progressive research cycles. MMMR needs to explicitly incorporate in practice an empowerment, transformative-oriented (intervention) intent in service of those most marginalized in society. MMMR, when practiced (which is often advisable in order to address complex social problems) should be tied to an axiological framework that seeks to enhance social (& ecological) justice	MMMR combines quantitatively directed and qualitatively directed research practices, on the premise that their supposedly different philosophical underpinnings do not exclude mixing in practice. Mixing can be done to explore research questions that have been identified. Paradigm wars between supposedly contending paradigms should not prevent researchers from designing an inquiry approach (and continuing to re-design it if necessary) so that it "works" to help the researchers to answer a specific research question	MMMR is suited to explore the differentiated social pathways leading to unequal life chances for privileged and for marginalized social groups, while incorporating in inquiries the value of (qualitative) appreciation of experiences and views. MMMR can be used in the service of generating culturally responsive inquiry while encouraging reflection and conversation around social as well as ecological justice. Different styles of knowing, including that which takes into account spiritual experiences, can be harnessed via MMMR

Table 9.1 (continued)

	Postpositivist	Constructivist	Transformative	Pragmatic	Indigenous
Ways of (Re)-defining MMMR, foregrounding responsibility and drawing out different meanings that could become associated with "Paradigmatic' Positions (which are understood as not being unitary)	Given that quantitative and qualitative-type measurements and analyses both create observation filters, and given that there is no firm empirical basis on which any scientific assertions rest, we (in the social scientific community) would do well to recognize how such filtering may have social consequences, for which we *need to bear some responsibility.* We can also take some responsibility for encouraging participants and stakeholders to engage with any generated findings, on the understanding that interpretations can never be made with certainty. Scientists need not present results as (more or less) neutral statements. They can recognize that when information is presented as such, it may unnecessarily constrain thinking along these lines	Insofar as we take into consideration that the constructions generated during all research processes —whether quantitatively and/or qualitatively directed —are arguably a product of an interaction of researchers and research participants (where the distinction between these parties can indeed become blurred), those initiating research can *responsibly try to gear the research process toward enhancing the quality of constructions.* Quality of constructions can be assessed in dialogical interaction with those concerned in terms of their experienced liberative potential in reviewing options for conduct. Professional researchers, in interaction with participants (participant researchers) and stakeholders, can operate with a responsible awareness of the need to pay attention to the implications of advancing any social constructions	Any research initiatives (including MMMR ones) should be practiced as undertakings in which researchers, with co-researchers *recognize their responsibility to highlight versions of reality that are hope-forming* and *that point to possibilities for positive social transformation.* Those initiating research must pay attention to the power relations involved in the planning, implementation and reporting of research. This means that all method (s) employed should be used innovatively, as people (all involved in the research endeavor) seek creative ways of planning, implementing and reporting which *recognize that ways of knowing are tied to possibilities for action.* People (all those concerned) can consider together the catalytic possibilities of the research, with special attention to empowerment of marginalized groups	Because all research can become socially influential by making "accessible" certain ways of seeing issues, we need to *responsibly conduct research with a "conscience",* so that it can be forward looking to activate morally-infused potential in social existence. Pragmatism does not mean relying on doing research in unreflected- upon fashion on the grounds that it "works" to answer a research question. Pragmatically-directed research (whether MMMR or not) can interrogate ideas about the "workability" of the research, *by considering wider social impacts* of ways of conducting research and writing it up. Those employing MMMR need likewise to take into account the likely impacts of the research framing	Based on the conception that we (all that exists, living and nonliving) are part of a dynamic web of relations, researchers—that is, anyone involved in the research endeavor—should *take co-responsibility for reflecting upon how the research space is part of this dynamic.* MMMR, needs to concentrate on locating culturally responsive versions of reality that are likely to lead to actions which enhance the relational quality of social and ecological existence, as understood through a range of ways of knowing. "Culturally responsive' does not imply that cultural repertoires are treated as "given"; diverse perceptions of the potential of cultural expressions for living in balance with"all that exists" are open to continued conversation

In the second row of the table, I carry forward the options for stretching of positions as identified in Sects. 9.2–9.6 above, indicating how each one can be stretched in the direction of foregrounding researcher responsibilities (where the term *researcher* can include co-researchers/research participants). Further to this, in Sects. 9.7.1–9.7.5 I explore all the examples given in Chaps. 2–6 in the light of this table.

9.7.1 MMMR Discussed in Relation to Chap. 2 (Reviewing Raced Relations): Some Paradigmatic Considerations

The research set up in the apartment complex in Margate, South Africa (2007) to review raced relations involved a focus group discussion followed by various continued interviews/conversations (mostly one-to-one but also one group session). In the terminology of MMMR we could say that the research used *multiple methods* in that different qualitatively-oriented research methods were employed in the research process. But as many authors have suggested, mentioning that multiple methods were used does not in itself give an indication of how the research proceeded—that is, of *how* the methods were used. For this, one needs to consider the paradigmatic orientation(s) that can be argued to have underpinned the research.

In this case, as the initiating researcher, I could self-position myself by suggesting that the trusting constructivist position which I had spelled out in my book on *Accountability in Social Research* (2001) functioned as a starting framework to guide my belief that what was going to be "found" via the research would have to be admitted to be a *product of the specific interactions* that were set up during the research process. Furthermore, in terms of my thinking at that point, I would have to regard myself as accountable to participants as well as to wider audiences (in this case, readers of the book that I was planning). I would have to account for the way of proceeding with reference to a *dialogue with the different participants* and with reference to a *dialogical engagement with* "*the research community*" (of scholars). In my book on *Accountability in Social Research* I concentrated on how researchers need to earn trust by thus engaging in dialogue, and I called this a "trusting constructivist" position.

At the stage of organizing the research at the site in Margate I was not conversant with the work of Mertens or with the methodological writings of Indigenous scholars such as Bishop (1994), Chilisa (2007), and Smith (1999). But although I was not conversant with this literature, my ethical stance did gear me in the direction of recognizing that—as Stephens, Jacobson, and King put it (2010, p. 562) when describing my position—cognition is always tied to some action implication. As I have explained in previous chapters, I would be able retrospectively to use the criteria of validation as offered within transformative and Indigenous paradigms to "justify" how I continued to interact with the various participants and tried to reach what Cram calls the hearts of the privileged (e.g., the Trustees of the body

corporate). These paradigmatic positions would also allow me to justify why I am sharing the research experiences now with the audiences of this book in the hope to generate some transformative moments for readers with whom the generative theorizing that I have proffered (see Chap. 2) resonates in some way.

As far as the paradigmatic position of postpositivism is concerned, I would have been averse at the time (2007) to associate myself with such a position, which I considered as too insistent on the separation of cognition from the realm of action (as also suggested by Lincoln and Guba, 2003, p. 262, as being associated with postpositivism). However, this does not mean that I was averse to any form of quantification of "data" which may be supplied in the research process. While I would not have regarded myself as sufficiently skilled in either descriptive or inferential statistics (albeit that I had learned about these at University and am conversant with their logic), I would not disregard as irrelevant all quantitatively-expressed information. Rather it was my skills base—and the small group of participants who became involved in the research—which inclined me to draw in this case on qualitative methods to review with participants raced and classed relations. But readers may have noticed that the research as I described it still leaves it open to other researchers more skilled than I to investigate quantitatively (and interpret, with others concerned) the patterning of raced and classed relations. In other words, MMR is not outruled by this research.

What I would suggest therefore, is that instead of us (in the research community) deciding that MMR means that quantitative and qualitative data collection and modes of interpretation are combined in a *single study* or even *a related set of studies*, we can *make openings for other researchers' research work in other studies*. The proviso that I would add is that insofar as quantitatively-directed work is undertaken, this should include reflection, with others, on ways of framing issues, and also should include intentions to generate discussions with key participants and stakeholders around the meaning and import of "findings". That is, the skills base of researchers who may be more "quantitatively" inclined ideally should also include some efforts at relationship building so that these discussions can ensue.

What does this mean in terms of the ways in which those doing/initiating any research can reflect both prior to their work and thereafter on the philosophical underpinnings that can be attributed to the research? I would suggest that it is of value to reflect on these underpinnings prior to, during, and after the project (assuming that one can speak of the end of a project, which may not always be the case if enduring relationships continue). This reflection gives more clarity to oneself and others about how one is seeing the "research" enterprise and one's responsibilities in this enterprise. But no matter what paradigmatic conception(s) of research one begins with, this does not preclude learning, as one goes along and reflects on one's position, from "other" positions that may be proffered (or that one can reflect upon).

Do we need to "place" research work within a particular paradigm when considering the underpinnings of the research? Again my deliberations above suggest that what is important is that considerations around how to "place" research work become *an opportunity for people to (re-)reflect upon the underpinning(s) of the*

research and the criteria of validation that are being, or could be, invoked to judge its worth.

I have not tried above to answer unequivocally the question of how the paradigmatic positions as have been named by scholars are invoked to guide and/or reflect on research projects; but I have tried to suggest that it *is* worthwhile to engage in a process (even retrospectively) of discussing research projects with reference to the paradigmatic debates. And I have tried to broaden the conception of MMMR as supposedly only applicable to a single study or series of related studies by suggesting that this is not the issue at stake. Any study or program of study can leave openings for further research work, which can benefit from philosophical reflection on the purpose of the research endeavour.

In short, the research work reported upon in Chap. 2 could be called a project that involved multiple qualitative methods, but the fact that I did not invoke quantitative modes of measurement or interpretation does not mean that I would be averse to myself drawing on others' work in this mode (which has been or can be undertaken) or that I would be averse to recognizing its value. This to me is not the issue at stake. The issue as I see it is more how inquirers can work innovatively with conceptions of researcher responsibilities which exceed viewing research as a "cognitive" exercise and which include action-oriented considerations, negotiated in conjunction with various participants/participant researchers and stakeholders.

9.7.2 MMMR Discussed in Relation to Chap. 3 (Dealing with Inclusive Education): Some Paradigmatic Considerations

9.7.2.1 The International Project: Dealing with Teachers' Attitudes to Inclusive Education

In the international research project reported upon in Chap. 2, which was dealing with teachers' attitudes to inclusive education across six countries, a team of researchers was involved in this endeavour. On the team were researchers who were more skilled in quantitative research and those who were more skilled in qualitative research. Initially, as part of gaining ethical clearance from our university (Unisa), the project leader stated that the rationale of the mixed method design was that the questionnaires would offer statistically analyzable data, while the focus groups could delve more fully into participants' experiences of teaching in inclusive classrooms in the various country contexts. But when the South African team handling the focus group discussions (Nel, Romm and Tlale) infused transformative intent into the handling of this, the project leader expressed that she was pleased that we had proceeded in the way that we did.

Now the question arises as to whether the various researchers in the project could be said to have learned from our being together on a project involving various

kinds of data collection and analysis. Because we did not speak about this issue to the various researchers across the country or across the set of countries, it would be difficult for me to make any statements on their behalf about this. But it is possible that the researchers (such as the project leader) who used the quantitative scales to seek data regarding teachers' attitudes to inclusive education recognized that these scales need not be treated as offering a non-perspectival picture of teachers' way of approaching questions connected with inclusive education in the different country contexts. It seems to me that they might have been alert to the notion that the way in which the scales were devised would "pick up" particular data and that if devised differently other "data" would appear. Hence one could argue, with some extrapolating, that they felt it important to include a qualitative component into the research to make provision for the particular framings built into the scales that were used. And certainly the project leader accepted the way in which we used the focus groups in the qualitative stage of the research in South Africa partly to "check" with participants whether the data derived from the questionnaires made sense to them, and to enable them to review some of the findings as they spoke about their experiences. The project leader also accepted that our way of handling the focus groups was to generate learning encounters within the group, rather than presuming that we would be "picking up" static (and clear-cut) understandings as held by teachers about the domain of inclusive education. And she accepted that we chose to include a further active component into the research by setting up meetings with participants and the district office to try to institute a more a fruitful relationship between them so that issues that the participants highlighted as problematic could be looked into.

Does this mean that through the project leader and others seeing the way in which we approached the South African research (with what we later named a pragmatic orientation with transformative intent, as in Romm, Nel, & Tlale, 2013, p. 9) their initial view of the status of the questionnaire (and questionnaires more generally) might have shifted? Could they take on board our view that it was important to take some responsibility for the framing of questions in questionnaires (which might imply a reworking of scales in the light of the qualitative research)? And/or could they take on board that the focus group discussions were not only lending depth to the quantitative data, but were also an opportunity for participants (and later for stakeholders) learning—besides the learning of the professional researchers? Would they be able to accept what I would call this stretching of what the research could intentionally be accomplishing, other than merely answering a *cognitively posed mixed methods research question* (about attitudes of teachers and experiences of teachers in regard to inclusive education)?

Meanwhile, we can additionally ask the question whether those of us who tend to be more qualitatively inclined (and keener on considering the research space as a space for expressing and activating transformative intent) learned from the way the colleagues on the team handled the quantitative component of the research? What I would say, speaking for myself, is that I came to appreciate that the quantitative research could be considered meaningful, as long as it was not defined in itself as offering more or less accurate findings, but became used to *trigger discussion* in

focus group settings and in other settings (such as the one we set up in the South African case). By saying this, I am implying that as long as those using quantitative research use it in the context of "combining" it with discussions (as was done in the South African case) and see it as an opportunity for triggering discussions among participants and ideally stakeholders too, it is *made relevant.*

Does this mean that I am taking on board postpositivist views of the worthiness of using the research process to investigate the existence (or not) of correlations and/or causal connections via the use, inter alia, of quantitative modes of measurement and analysis? Or does it mean that I am looking at such views from my (different) paradigmatic perspective, but at least trying to accommodate that this search for the patterning of social life can be regarded as important as a research task? As part of their deliberations on the future of MMR, Mertens et al. (2016) suggest that

> future challenges for mixed methods researchers include how to bring multidisciplinary teams together to share their expertise in respectful ways. While this is not a new problem, it is one that continues to challenge researchers and so warrants continued attention by the mixed methods research community. (2016, p. 225)

Could the international research project as I have described it be called an instance of the team of researchers "sharing their expertise in respectful ways"? In this case, I could not say that the researchers were actively sharing their expertise; nonetheless, perhaps it could be said that we were learning from seeing how the different researchers were choosing to address their involvements in the research project and their ways of writing these up.

9.7.2.2 The National Project: Addressing School Underperformance: Making Schools Better

Regarding the national project, this mixed methods research design again involved questionnaires and focus group sessions. I would say that most of us from the university who were involved in the design of the questionnaire (primarily McKay who also worked with the national department of education on some of the questions) were intent to ensure that the questionnaire could become a *tool for aiding reflection* on the part of those "filling it in". (See Chap. 3, Sect. 3.4.3; and Romm, 2017c.) Furthermore, we were conscious of how we were framing questions in the questionnaire so as not, for example, to contribute to reifying race categorizations. (See Chap. 3, Sect. 3.5.) And we were conscious of not reproducing deficit thinking via the way the questions were framed. (See Chap. 3, Sect. 3.4.1.)

The research design team were also aware in the design of the project that any results from the questionnaire would have to be "communicatively" tested in, for example, the intervention visits at selected schools, the discussion with provincial officers, and the discussions with researchers from the government. This is in keeping with Cram's point (2015, p. 681) about invoking a communicative

methodology, in line with Indigenous research requirements not to secure "control" as professional researchers. I would suggest in this regard that as we proceeded with the various cycles of the research (questionnaire administration, focus group discussions, intervention visits, discussion with provincial and national officers) we could be said to have been invoking the protocols and principles as forwarded in what Chilisa (2012) names as an Indigenous research paradigm. However, as we spoke amongst ourselves (sometimes in smaller teams and sometimes in meetings with all the staff involved in the focus group component of the research) we did not name what we were doing as "Indigenous-directed" research. It was when I authored and co-authored papers springing from the research that I draw on this terminology (e.g., Romm, 2015, 2017b; Romm & Tlale, 2016; McKay, Mohapi, & Romm, 2017).

This of course does not mean that all of the researchers involved in the design of the project conceived the paradigmatic positioning of the project in the same way. It is quite possible (judging from how I interpret some of the team discussions around the setting of questionnaire items) that some of the researchers in the team (where we went through many iterations of the items in various meetings) were regarding the questionnaire items as being aimed at *finding out* (as a world mirroring exercise) how teachers and principals were functioning and whether this was related to identified variables.[14] For instance, the questions could be regarded as serving to, say: ascertain how many schools had certain policies in place and whether this was related to variables such as urban/rural divisions or resource bases; to ascertain the extent to which teachers felt confident to address issues connected to inclusive education and again whether this was related to other variables, and so on. But discussion in our team meetings were also lively, as we *discussed the impact of our way of phrasing questions* also in terms of possibly encouraging principals and teachers to think about matters such as the need for policies (including inclusive education ones), the need for inclusive education training for teachers, etc.

Again, it would be difficult for me to make any claims about whether and to what extent these discussions—as well as further discussions in our meetings around the purpose of the focus group sessions that we were planning—meant that we were using the meetings as an opportunity for *paradigmatic reflections* on the nature of the project. As regards our discussions around the focus group sessions and what their purposes were, all the (46) researchers who were going to be involved in this stage of the research attended workshops in which we agreed that the focus group sessions should be presented to participants as opportunities for their learning from one another as part of the session. But of course this still leaves opening for many paradigmatic conceptions of the status of the focus group findings, their links to the questionnaire data, and their links to further understanding-and-intervention. Should we have had more explicit discussions around paradigmatic commitments in order to activate what Mertens et al. call "space for dialogues across philosophical

[14]This could well have been the case for the researchers involved in (co)constructing the mathematics, science, and educational management directed items in the questionnaire.

assumptions" (2016, p. 222) and also in order to *talk about the issue of researcher responsibilities in relation to all those involved and affected by the research*? As I have suggested via Table 9.1, my view is that it is important to engender discussions across the board in the research community which are focused on the understanding of researcher responsibilities (and development of co-responsibility with research participants and stakeholders).

9.7.3 MMMR Discussed in Relation to Chap. 4 (Highlighting Options for Gender Empowerment): Some Paradigmatic Considerations

9.7.3.1 Locating Empowerment Opportunities, for Rural, Married Amhara Women (Ethiopia)

The first example of research reported upon in Chap. 4, is the research which Woldegies (2016) indicates was aimed at highlighting options for gender empowerment in Amhara Regional State, Ethiopia. In summarizing the methodological approach that was followed in this research, Woldegies states that he

> used a qualitative research methodology which, as Winter (2000) notes, can provide in-depth information regarding people's perceptions and experiences. I considered this most appropriate for the study because it provided space for the women (and the family members who support them), to articulate their experiences on their own terms; it could give room for participants' interpretations and explanations in the foreground rather than erasing their individuality through quantitative numerical clumps. (2016, p. 63)

Woldegies further clarifies that:

> Of the qualitative methods available … I chose to engage in interviews with 10 women entrepreneurs regarding their success stories and their experienced challenges in overcoming problems in Wadla Woreda, with narratives/biographies being the source of the study, followed by FGDs [focus group discussions]. (2016, p. 63)

The idea behind the use of multiple (qualitative) methods was that he believed that the data/stories generated in the FGDs would constitute an expansion of what had been said in the interview forums (2016, p. 63). As I mentioned in Chap. 4, I believe that he took into account when using both methods the constructivist understanding that, as, Mertens and McLaughlin explain the constructivist position, "the inquirer and inquired into are interlocked, each affecting the other through the process of their mutual interaction" (2004, p. 100). Hence he took advantage of the interactive character of the data collection, or rather data *generation*, process. Indeed, we might further say that he "stretched" this constructivist understanding to the point of admitting that he, with the participants, needed to take some responsibility for the way in which the interview and focus group encounters themselves already were *generating effects on meaning-making and on conceived possibilities for action*.

Notably, although Woldegies chose to concentrate in this study on the use of qualitative methods in his interaction with participants, he is not averse to recognizing the value of more quantitatively-framed data. Hence, for instance, as part of his background information that he supplies to readers, he notes that:

> In this [Wadla] woreda, access to basic social and economic services, such as health, education and employment for rural communities, is limited due to poor development of the rural infrastructure. Wadla is one of the food insecure woredas in the region. (2016, p. 60)

He states that this affects women in particular in that "due to male domination, the women are victims of social discrimination, gender-based violence, and power balance barriers" (2016, pp. 60–61). He provides some evidence of their disadvantaged position by adding quantitative explanation:

> The women often have limited access to resources. Their employment rate and representation in local government are low (Wadla Woreda WCYA Office 2012a, 2012b). Their economic status is marginal. At times, their income generation is negative, meaning their returns are less than what they invested, leading them into absolute poverty (Amhara Regional State Bureau of Finance and Economic Development 2007, 2009). (2016, p. 61)

Woldegies here indicates that he appreciates certain quantitatively-created data (in this case the data created by the WCYA office and by the State Bureau of Finance and Economic Development) as a way of setting some contextual background to the qualitatively-directed research, and as a way of justifying his commitment to women's empowerment which he admits to have infused the research reported upon.

Apart from quantitatively-framed data to which he refers, he states that he finds it crucial to engage with research participants via qualitative research, so as not to the erase identities of people in what he calls "numerical clumps" (2016, p. 63). Here he seems to be suggesting that the disaggregated data that is expressed in numerical form (taken from the WCYA office and the State Bureau) complements the data generated through, in this case, the multiple methods qualitative exploration. The considerations that I offered in Sect. 9.7.1 above, when discussing the multiple qualitative methods used in Chap. 2 regarding the possible value of quantitative data and interpretation, thus hold here too. In this case, Woldegies interprets the quantitative data to make the point that women experience "barriers" more so than men, which renders them disadvantaged as a group.

The main beneficiary of the research, he admits, are therefore women, and he makes it clear that the research was set up in order to advocate for them (not necessarily at the expense of men, though, as far as this was possible). If we look at the way of phrasing what he calls "the basic research question" which guided this study (2016, p. 63), I would suggest that it incorporates a transformative intent. The research question was:

> What opportunities, success stories, and challenges exist for rural, married Amhara women engaged in income-generating activities to improve their livelihood status in the context of food insecurity and poverty in Wadla Woreda? (Woldegies, 2016, p. 63)

I find that this way of setting the research question can serve as an example of the posing of a question in *forward-looking fashion* (in line also with Gergen's view of research as future-forming, as I elucidated in Chap. 7). By asking what opportunities exist, and what success stories can be told (while also appreciating challenges), Woldegies implies that the idea behind the research is to *further activate these women's and others' sense of opportunities*, while also considering ways of addressing experienced challenges. This would be consistent with Mertens' position (as also described in the article by Johnson, Onwuegbuzie, & Turner, 2007, p. 120) that the manner of setting up the research question is crucial, if we are to class the research under the umbrella of transformative research.

Woldegies's transformative intent is stated more explicitly in the following extract:

> Clearly, the positive proverbs [which he asked women to call up as part of the interviewing process] can show vocation, equality, heroism, miracles and success. The negative proverbs usually show disempowerment, power imbalance, humiliation, brutalization, undermining, resilience, and so on. The positive proverbs are more powerful to transform the status of women and by participating in collecting and disseminating these, I hoped to create some intervention toward transformation. (2016, p. 75)

Just as Cram and Mertens (2015, p. 99) suggest that compassion is a force that can guide advocacy-directed research, Woldegies states (though not referring to the transformative or Indigenous paradigms) that

> The research was carefully designed and executed with rigor as well as compassion in order to highlight possibilities for gender empowerment. The portraits of the 10 women, their 25 family members, and their communities offer a rich perspective on the multi-level nature of empowerment (2016, pp. 76–77, my italics).

Considering this research in terms of Table 9.1, it can be considered as well placed in transformative territory due to its intended (and compassionate) focus on exploring options for enhancing gender empowerment. It can also be considered as well placed in an Indigenous paradigm, due to Woldegies's commitment to being "culturally responsive" in his way of relating to the participants. For instance, I noted in Chap. 8 how he chose to not follow strictly the Belmont report (1979) guidelines in his offering of what could be called incentives to participants; and also how he tried to proceed in terms of an allegiance to Indigenous conceptions of strengthening harmony in the communities in which the research was situated. (See Chap. 8, Sect. 8.4.2.) Woldegies recognized, though, that the communities could not be considered as having homogenous views regarding the status of women, as expressed in the different kinds of proverbs that all could be drawn upon (and were drawn upon in everyday life). *But what was sought* via *the research was a strengthening of the* "*currency*" *of positive proverbs* and of attendant ways of supporting women's so-called "positive deviance" (2014, p. 82). Woldegies himself took some responsibility for this, in conjunction, of course, with participants. I would suggest that the deliberations that I offered using Table 9.1 help us to conceptualize how Woldegies's work can be positioned as "belonging" in transformative/Indigenous territory while he also found a way of innovatively

exercising what he saw as his responsibilities and at the same time appreciating the need for co-responsibility.

In short, I believe that Woldegies' posing of the research question in the way that he did (which to me implies that it was not just "cognitively" posed as a world-mirroring exercise) *enabled a focus to be put on researcher and co-researcher responsibilities* in generating transformative outcomes. In this case, the intention was to strengthen women's empowerment along with strengthening relationality in the community and indeed also connectedness with the natural environment, as environmental stakeholders were also brought in at a certain stage —see Chap. 4, Sect. 4.2.5.)

9.7.3.2 Exploring the Northern Ugandan Conflict in Terms of Implications for Post-conflict Health Reconstruction

In the second example reported upon in Chap. 4, I showed how Ssali and Theobald make a case for using life histories to explore experiences of the Northern Ugandan conflict in terms of implications for post-conflict health reconstruction. In making their case they refer to the way in which

> the transformative paradigm emphasizes both ethics and partnership in research: being sensitive to people's cultures, power relations and building relationships of trust between the researchers and the researched is critical (Mertens, 2012a, 2012b). … . This paradigm calls for the researcher(s) to investigate meanings from the view point of the researched, exploring how the research participants construct knowledge, and what the significance of the constructed meanings is in the given context. It calls for the investigation of how this meaning varies across groups and how it is layered depending on the position of a given individual or group. (2016, p. 82)

Ssali and Theobald thus make reference to how they regard their transformative paradigmatic commitment in their explication of why they chose the life history interviewing method in this research. They feel that Mertens' position stresses that one needs to gear the research process, first and foremost, to "investigate meanings from the view point of the researched, exploring how the research participants construct knowledge" (2016, p. 82). While making their case for use of the life history method, they refer to certain existing statistical representations of health indicators and their connection to other variables. In looking at the value of this statistically-based research, they do not disregard its stature; but they feel that it is ill-equipped to delve into what they call "psycho-social aspects of health" (2016, p. 85). They state their position as follows:

> While generating district specific information to support responsive planning, the Gulu District Local Government Statistical Abstract 2012/2013 (GoU 2012/2013), as with most statistical abstracts, captures a summary of socio-economic data, focusing on measurable aspects of the sectors considered fundamental for development. With regard to health, only statistically measurable indicators, such as maternal mortality, HIV and malaria prevalence, and fertility rate, are measured. Other critical aspects which cannot be easily captured statistically, such as psycho-social aspects of health, are left out. (2016, p. 85)

It seems then, that although we might say that their research study reported upon in the article (2016) was solely *qualitatively* directed, they believe that it can be usefully combined with data that has been generated through quantitative measures of health (in relation to socio-economic variables that have been located as linked to health data). Hence they offer reference to the statistically-based research, but they suggest that we need to recognize that quantitative measures are *less equipped to explore "psycho-social aspects of health" in terms of the constructs that are meaningful to participants*. Because they wished in their research to ensure that the participants had the power to construct knowledge in this regard from their perspectives, they chose a life history approach to this end, to offer another angle (and fill a gap) in what has thus far been researched "statistically" (2016, p. 85). They believe that this approach of theirs is consistent with a transformative paradigmatic focus on creating empowerment possibilities at the moment of interacting with participants and at the moment of seeking "solutions" to issues raised as problematic (so that these are grounded in participants' construction of knowledge). They affirm that the research as they see it permitted an exploration of "what needs to be done to ensure post-conflict development prioritizes the multiple health care needs of those most impoverished by the war" (2016, p. 82).

In terms of my deliberations above, although readers examining their write-up of the project may decide to consider this as being a monomethod project, one can also decide that because they include in their account an indication of how they see the relevance of other researchers' quantitative studies (as well as their weaknesses) they are suggesting that *across projects there is value in MMR*. That is, as I mentioned in Sect. 9.7.1, if we do not strictly consider that it needs to be a *single* study or *set of closely related studies* that renders research an MMR endeavor, then we can suggest that researchers can draw on secondary research and refer to studies whether or not apparently "related", and that *mixed methods are then still at play*. Again the focus then shifts away from methods (and concerns with naming of studies mixed or not) to the question of what is intended to be achieved via the research endeavor.

What Ssali and Theobald take as important from the transformative paradigm is that it "emphasizes both ethics and partnership" in the research process. They believe that the research which they undertook was indeed geared to partnership in the construction of knowledge through the life history method. They feel that this helpfully extends existing quantitative data relating to health outcomes, so that "social justice" endeavors can be forwarded via the research (2016, p. 82).

Hence, whether we read their research as monomethod (in that their primary research mode is the life history method) or as expressing an appreciation of how this needs to be combined with the quantitative material on health outcomes to which they also refer (thus being appreciative of multimethods), what is foregrounded in their write-up is the *transformative intent* (and the way in which this intention was followed up).

9.7.4 MMMR Discussed in Relation to Chap. 5 (Locating Possible Options for Agency): Some Paradigmatic Considerations

9.7.4.1 Investigating the Effects of Six Thinking Hats and Emotional Intelligence Training on Recidivists' Creativity and Emotional Intelligence (Nigeria)

Stephens' experiment, set in Nigeria, was designed to investigate whether "treatments" in the form of Six Thinking Hats training and Emotional Intelligence (EQ) training would have an effect on the dependent variables of creative thinking and emotional intelligence of the recidivists in the study (as measured in terms of their responses as given in pre- and post-training questionnaires).

When offering his thoughts on the use of experiments, which by their nature are "predominately quantitative", Mark identifies various ways in which qualitative considerations can be brought to bear (2015, p. 21). For instance, he notes that right from the start qualitative research processes can be used to inform the "creation and possibly revision of the research question to be addressed" (2015, p. 31). One could argue that due to Stephens' prior involvement with the recidivists in the various prisons (as a counsellor, a pastor, and also as involved in the Cottage of Hope NGO), he would have had extensive discussions with the prisoners, which might well have informed the way in which the experiment became conceived to "test" possibilities for recidivist learning via the different training packages that became offered. Mark mentions a number of other ways in which qualitative-type explorations can inform the various steps of an experiment, not least in the "interpretation of findings and subsequent reporting" (2015, p. 31). Again, we might suggest here that the anecdotal evidence that Stephens attained in the form of feedback from participants along the way of conducting the training and also thereafter (when he still met them from time to time after the experiment was completed) can help to make sense of the quantitative data as observed via the measurement scores. Actually, Stephens did not refer to any of the informally-obtained qualitative data in his dissertation, but one can now suggest that this data (which he realized on speaking to me that he had been collecting albeit not seeing it as part of the study) is crucial in helping us to consider the meaningfulness of the training as far as the recidivists (or those to whom he spoke) were concerned.

The point that Mark makes is that "qualitative data have long informed and contributed to predominately quantitative studies [such as experiments]" (2015, p. 21). Indeed, he suggests that qualitative data can play a crucial role in so-named quantitative studies, and in this sense they become "mixed" (2015, p. 21). He argues that much "mixing" indeed predates the "mixed methods" literature. He clarifies that "skilled researchers have long been mixing qualitative and quantitative methods" (2015, p. 22).

In terms of paradigmatic considerations, Mark avers that "a good number of quantitatively-oriented researchers" setting up what he calls predominately

quantitative studies "would probably qualify as *paradigmatic agnostics*, being deeply concerned with their particular research area and minimally if at all concerned with the abstractions of paradigms" (2015, p. 23, my italics). I would suggest in this regard that Stephens might be called a "paradigmatic agnostic" in that he did not try to position himself in any paradigm when writing his dissertation. This could also be because he wrote it up as simply "doing science" by the virtue of the fact that he used treatment and control groups to examine hypothesized links between independent and dependent variables.

Mark makes the point that quantitatively-oriented researchers often may be directed toward "obtaining the best available answer regarding the hypothesis [hypothesized relationship between variables] or research question being addressed in a study" (2015, p. 27). He argues that this may be "particularly true for more applied research such as program and policy evaluation, where relevant stakeholders' concerns may have generated the research question" (2015, p. 27). In other words, in such scenarios, quantitatively-oriented researchers may want to pose their findings as "the best possible answer" (to whether or not the hypothesis can be claimed to hold) rather than concentrating on the uncertainties that always beset the research process. Marks argues that if there is too much focus on "uncertainty" of results, this may "defer the use of findings" (2015, p. 27). But as I have suggested in Chap. 5, Sect. 5.2.7, the tentative character of "results" can rather become seen as an *opportunity for rendering open to discussion interpretations regarding the import of findings and options for their possible utility*.

Stephens (2012) affirms that when he presents his findings he does not wish to display them as final. (See Chap. 5, Sect. 5.2.7.) In this way he draws implicitly on the Popperian view that findings can never be finally verified. I would argue that he emphasizes this in an effort to encourage others to engage with the findings and to *consider the consequences of ways of interpreting the results* of the experiment. One could also suggest, as I did in Chap. 7 (Sect. 7.3), that he draws in addition on the logic of generative theorizing to make certain *hope-directed inferences*. I have suggested via Table 9.1 that a "scientifically-focused" stance—when stretched—enables researchers to consider carefully how research questions are framed and how answers are interpreted, in view of the necessary uncertainty of all knowledge-production. But I would suggest, with Mertens (2010b, p. 9), that it is important to examine "philosophical assumptions" that are being (or can be said to have been brought) to bear in any research project so that the researchers involved and others considering the work, are encouraged to reflect upon the status and purpose of the research.

9.7.4.2 Investigating the Effects of a Proposed New Debriefing Procedure on Participants' Experiences of Deceptive Research (Set in Poland)

In the case of this research undertaken by Oczak and Niedźwieńska (2007) it would be difficult to suggest that MMMR was being applied, as the research experiment was

set up quantitatively to test the effect of the new debriefing procedure devised by them. Nevertheless, although one can say that the research was quantitative in focus, one could also say that the original setting up of the research question was based on their own qualitative involvement with (other) subjects/participants who have in the past participated in various experimental procedures that included deception. Oczak and Niedźwieńska indicate that although the results of studies undertaken by various researchers investigating the negative consequences of deception are "far from conclusive" (p. 49), they themselves have the impression from their work with participants, that deceptive research can be experienced negatively by participants. As they state:

> Our interest in ethical problems emerged *from our experience as researchers* conducting studies on suggestibility and related issues. We have *had the impression* that in deceptive studies investigating undesirable behavior the standard debriefing practice is unsatisfactory as it leaves participants with the feeling of being cheated and used, as well as provides them with negative feedback on their abilities. (p. 56, my italics)

Further to this, they proceeded to set up the experiment in a scientific way (using, but also innovatively adapting, the experimental procedures of experimental psychological research). The experiment was designed to assess—via pre-and post-test scales to measure self-esteem etc.—the effect of their new debriefing procedure on participants' experience of the initially deceptive research. As I have indicated earlier (Sects. 9.4 and 9.5), the issue at stake when designing research need not be so much whether or not it is monomethod, but who sets the research question and to what end, as well as the *intent underpinning the research*. In this case one could argue that they conducted the experiment bearing in mind (and indeed in heart) from their previous "qualitative" contact with other research subjects, the need to consider how they might be able to render the experiment more meaningful for participants.

Their hope ultimately was to consider how experiments more generally can be used with *educative function*, and can aid participants to master the skills (as they call it) of circumventing authoritative demands in various contexts. And they hope that others reading about the experimental work can perhaps better appreciate that people are not to be treated as puppets of others' suggestibility. Furthermore, as experimental psychologists they plead for the research community (and ethics committees) to consider for themselves the import of the findings for thinking about how research can be rendered beneficial by performing an immediate learning experience for participants.

In regard to any paradigmatic position that might be seen as guiding their work, they themselves did not introduce paradigmatic considerations into their write-up of heir article. They take it that their way of doing science in this experiment should be able to tell us something about the effects of their new debriefing procedure in relation to the standard debriefing procedure. Yet they also admit that there are various ways of interpreting the findings as observed (via the various scales). In admitting the uncertainty of the knowledge production process they could be said (like Stephens) to be following a kind of Popperian logic, where science is seen as a never-ending process. (See Sect. 9.2.)

But having said this, I would also add that they are (or can be seen as) pleading for researchers to at least take seriously that research *can perform an educative function* and need not be geared to "*finding out*" *about people's propensities* (e.g., their propensities to suggestibility), as if these are to be taken as *fixed*. In all these aspects, the research stretches the usual understanding of postpositivist/nonfoundational-directed science, which according to Hammersley (2003, p. 245) implies that researchers are *oriented to finding out about the world as it operates independently of its being researche*d. Oczak and Niedźwieńska are content to consider that the research arena is one way of *consciously affecting the research participants so that they become rendered more empowered in the face of authority*.

Niedźwieńska thus point to the liberative potential of a science practiced with the recognition that research need not be a matter of merely "finding out", but can be a participant- and indeed world-changing exercise, while still being done "scientifically". In summary, I would highlight that *they take responsibility for the potential shaping effects of their way of designing the experiment* and of their way of setting the initial research question, which seems to have arisen also in relation to what they conceive to be the concern of participants who have in the past been subjected to deceptive research.

I have tried to show via my extrapolations above that it is worthwhile to examine their work in view of a consideration of debates around paradigmatic commitments. Even if we assign a non-foundationalist Popperian-type stance in consideration of their apparently classic way of doing experimental science, on further examination their position lends itself to stretching of this stance. That is, the stance becomes stretched in the direction of researchers taking more responsibility for the world-shaping effects of "science".

9.7.5 MMMR Discussed in Relation to Chap. 6 (Researching Contexts for Vitalizing Social and Environmental Justice): Some Paradigmatic Considerations

9.7.5.1 Development of Participatory Decision-Making Software (South Australia) to Support Reconsideration of Social and Environmental Justice

The research initiated by McIntyre-Mills in conjunction with local government in South Australia (SALGA) involved used of a software package to encourage residents in the community (who could be characterized as mainly middle class) to review their life choices in regard to ecological concerns.[15] The attempt was to try

[15]McIntyre-Mills reminded me (pers. comm. via email, March 2017) that this research came about after the launch of co-designed software to enhance wellbeing for Aboriginal service uses and

to encourage those engaging with the software to recognize that non-materialist-oriented choices can be made in terms of considerations of human and ecological wellbeing. As part of the design of the package, focus group discussions with local residents who volunteered were arranged in the local library, so that various scenarios to be identified in the package for people to think about, could be spelled out. In this sense the research already had a qualitative beginning, which was used to guide the development of the package with which it was hoped that research participants would then engage. Options for engagement with the package/questionnaire were via an online processes and also via some face to face "filling in", with the help of research assistants for those who preferred to handle the software this way.

When I asked McIntyre-Mills (via email) to consider my draft account of this research which I wrote up in Chap. 6, her response was that:

> The software will be ineffective if it is the sole means to gather data or engage. When trying to understand what matters to people it is important to give them respectful attention during human interactions face to face. This is the only way that we can even begin the conversations about spirituality. (pers. comm. via email, August 2016)

Here McIntyre-Mills points out that although she believes that the research via the software package might well have helped people to think through various scenarios (which had been identified for reconsideration) this does not mean that primary importance must be given to this package. As she notes, ways of setting up discussions around spirituality also need to be catered for in research enterprises. She feels that this is important as part of the research project, which she does not see as having "ended" at any point. Her project of trying to facilitate thinking about human and ecological wellbeing, in a manner that takes into account spirituality as a way of engaging with one another and with the planet, did not end once the report for SALGA as part of the funding obligation was written and discussed with them.

From McIntyre-Mills' response to me (2016) I take it that McIntyre-Mills does not see why we should put boundaries around a "research study" or even a "program of study" when setting out to organize/facilitate research across a lifetime. Across all the research endeavours in which she is involved (and which she helps to set up, with participants), she hopes that the research space can be used to

---

health providers. That project was funded by the Australian Research Council, Neporendi Council and then then South Australian Department of Health and it drew on Indigenous philosophy that many interrelated factors shape wellbeing (including how we relate to others and to the environment). The software designed to map different pathways to wellbeing was launched with co-researchers Douglas Morgan, Bevin Wilson, and (informatics designer) Denise de Vries as well as several other mentors (such as Kim O'Donnel). Douglas Morgan was at the time the chair of Neporendi, an organization advocating for Aboriginal people living in the Southern region and Bevin Wilson (from Flinders University) had been his senior mentor. The group wished now to extend the research to local government and Janet went on to work with other researchers too, but with Bevin remaining a mentor. The scenarios presented as a basis for the local government research flowed, inter alia, from what she had learned from these various mentors. (See Chap. 6, Sect. 6.1.)

contribute to people re-examining life choices in terms of human and ecological wellbeing. (See footnote 15 for detail on how she sees the interconnection between projects.)

Hence one can say that McIntyre-Mills' research, which she does not characterize as "ending" at any moment, clearly involves some form of MMMR—as a variety of ways of "doing research" are tried in different contexts. What is more important than putting any boundaries around a project is that research is initiated in the hope to "make a difference" not only to human but to ecological wellbeing (which she sees as linked, and also as linked to our sense of spiritual connection with each other and the planet). McIntyre-Mills also does not consider that it is helpful to think about *frameworks/paradigms* as bounded categories that researchers need to work within. Hence as I noted in Chap. 1, footnote 19, she speaks of working *with*, rather than *within,* frameworks. I would suggest that she innovatively tries, as in my Table 9.1, to stretch various available frameworks/paradigms, all the while being cognisant that an important target of the research enterprise must be planetary wellbeing.

Does this mean that as readers we should not try to "place" McIntyre-Mills' work with reference to paradigmatic considerations? Or perhaps due to her acknowledged transformative intent to strengthen a non-anthropocentric ethic as her guide to her way of proceeding and engaging with others when facilitating research, her work can fairly be placed in transformative territory as well as in Indigenous-oriented territory.

Markedly, McIntyre-Mills names much of her research involvements (including the research reported upon in Chap. 6) as participatory action research (PAR). In this way she signals to readers that she does not consider that the research can be divorced from action considerations. Research is meant to "make a difference" at the moment of doing it. Research makes a difference by co-generating "inspiring narratives" and by setting up space for co-exploring options for action which express this inspiration. (See Chap. 6, Sect. 6.2.) This is not quite the traditional cycle of PAR that I mentioned in Chap. 3 with reference to authors such as Bless, Higson-Smith, & Sithole (2013), Dick et al. (2015) and Niculescu (2014). It is more what I would call active research, where researchers/co-researchers seek out opportunities—using innovatively certain available or created "research methods"—to proceed with action-oriented inquiry. I offer further deliberations in Chap. 10 on ways of conceiving the links between "action" and "active" research.

9.7.5.2 Reflecting Evocatively on the Case of Occupy the Farm (Berkeley, California)

In Roman-Alcalá's article (2015) concerning the Occupy the Farm (OTF) initiative, he names the research in this case as insider-based PAR. The methods that were employed in this research consisted firstly of his reflections on his involvement (as fully involved *participant observer*) in the actions constituting the OTF initiative, and secondly of *interviews and conversations* with other actors, which he in turn

reflects upon. He thus can be said to be using *multiple methods of data collection and interpretation in this research*. As I indicated in Chap. 6 he presents his retrospective storying as rooted in an engagement with quotations from organizers and participants, and as enriched by his way of experiencing the case, which he considers as having been geared to activate a more collaborative use of land as part of a Food Security (FS) initiative.

As I put forward in Chap. 6, it is through his evocative write-up of the case in these terms that he *strives to evoke further possibilities* for creative thought (in the form of development of narratives which run counter to capitalist private property discourses) and action in the FS arena. And he clarifies that this has ecological implications for environmental justice. As with the research reported upon by McIntyre-Mills (discussed above), I would suggest that Roman-Alcalá's self-naming the OTF case study as PAR, coupled with the way in which he writes up the research in evocative style advocating for social and environmental justice, offers a signal that we can justifiably place the work as expressing a commitment to transformative research. Notably, he presents a version of the import of the case as pointing to new discourses and to new possibilities for action, as would be advocated in "transformative" paradigmatic territory. Hence he points to the potential of land occupations such as the one in OTF to

> delegitimize hegemonic food systems and provide a legitimizing vision of alternatives, while contesting the material basis for the production of food and providing non-elites (potential participants in commons and social movements) with means of subsistence and reproduction. (2015, p. 556)

Clearly, his (transformative) allegiance which infuses the research and its write-up (as advised by Mertens) is to "non-elites" as he calls them, while encouraging "environmentally sound" production processes (2015, p. 546).

9.7.5.3 Storying the Journey of Setting up the Green Belt Movement in Kenya

Maathai does not class her memoir (2006) as being a research endeavor, but I have suggested in Chap. 6 that it can be classed as autoethnograhic research and more specifically as what Sykes (2014, p. 4) calls transformative autoethnography. Considering the specific way of organizing her storying, Maathai does not limit herself to recounting experiences which relate only to apparently "observable" aspects of reality. In keeping with an Indigenous experience of the world, she does not shy from pointing out that she mourned the loss of the fig tree in her childhood garden, which she regarded as sacred; she considers that the tree itself rejected the irreverent way in which commercial agriculture has treated the environment as, indeed, resources to be commercialized (2006, p. 122). Her experience of the sacredness of nature is for her a spiritual experience worth sharing, which she shares with us (implicitly urging us to likewise consider viewing it in terms of its sacred character). In this sense she would be averse to any postpositivist insistence

that legitimate ways of knowing need ultimately to be justified with reference to what is supposedly empirically observable. As many Indigenous and Indigenous-oriented authors have stressed, the realm of what is experienced as *empirical* is broadened within Indigenous ways of knowing and engaging with experienced "reality". (See Chap. 7, Sect. 7.4.3.) Her storying might also be well placed in pragmatic paradigmatic territory in that she illustrates how it is in action that her and others' "knowing" about what was possible became developed while they were seeking to generate better quality of life outcomes (including quality of ecological life). Her storying is an expression of what Adams calls "compassionate and consequential research" (2017, p. 65)

As far as using MMMR is concerned, her inquiries can be regarded as questioning the distinction between "quantitative" and "qualitative" research much in the way that Cram and Mertens suggest that in drawing on various "sources of data" included in Indigenous Knowledge Systems, we might need to move beyond "what is traditionally thought of as quantitative and qualitative methods" (2015, p. 97). Cram and Mertens refer to various ways of giving credence to Indigenous Knowledge Systems as incorporated in stories, poems, folklore, etc. Here we might include the stories leading Maathai to regard the trees as sacred, and the folklore around this too, which do not fit the conventional quantitative and qualitative divide where so-called observable and measurable (quantifiable) data are compared against experienced (qualitative) data. In Maathai's account, this divide becomes fuzzy as she extends the realm of the "empirical" when recounting her experiences. Kovach points out in this regard that "through autoethnographies and autobiographical narrative inquiries, researchers reveal how the intuitive and experiential work constructs knowledge" (2009, p. 110). Kovach qualifies this by noting that when presenting such knowledge, those espousing Indigenous epistemologies "often preface statements by stating 'I believe it to be true'" (2009, p. 111). She points out that these kinds of words "qualify knowledge as personal reflection from one's own life experience, and recognize other truths" (2009, p. 111). Hence one can say that space is also provided for oneself or others to employ a variety of sources of "evidence" and ways of developing discursive frames to make sense of it—in the spirit of appreciating the value of MMMR.

9.8 Conclusion

In this chapter I set out to review some paradigmatic issues in relation to data generation and interpretation in research, concentrating also on debates surrounding multi- and mixed-methods research. I expanded the table of paradigms which I presented in Table 1.2 of Chap. 1, to now concentrate on clarifying different paradigmatic visions of multiple and mixed methods research (MMMR). I suggested (with reference to some chosen authors in the literature) that all paradigms can make provision for both multiple and mixed methods research, but they do so in different ways. Meanwhile, I pointed out that paradigms as we

conceptualize them do not need to be conceived as offering unitary positions, because different "proponents" can offer different slants when outlining the paradigmatic position. Furthermore, in conversation between "proponents" (insofar as people express allegiance to a particular paradigmatic commitment at any point in time) paradigmatic positions can evolve. What I also highlighted is my own view of ways of "stretching" all the paradigmatic positions to foreground possibilities for researchers/co-researchers recognizing (which I argued all paradigms can in principle accommodate) that *research is a socially impactful enterprise*, *for which some responsibility needs to be taken*. This means that the issue at stake is not so much the methods used, but *how they are used in a responsible way*—where *responsibility is tied to a recognition of the consequential character of social research*.

I pointed out that the transformative paradigmatic territory explicitly calls upon researchers to set research questions in such a way that *cognition* (of posited patterns and/or of felt experiences and processes) is not necessarily regarded as the goal of the research enterprise. That is, I focused on Mertens' considerations as cited by Johnson, Onwuegbuzie, and Turner (2007, p. 120) that research questions when set, already should explicitly contain an *action-oriented remit, directed toward increasing social justice*. I suggested that when active intent is built into the research endeavour, a pragmatic epistemological position becomes invoked, especially when pragmatism is defined not in narrow practical terms but in terms of attempts to further "the good life" (as created via social conversations and actions). That is, such a pragmatism can serve to support ways of thinking about transformative research agendas. Furthermore, insofar as "the good life" is seen through Indigenous worldviews on strengthening relationality and connectivity, Indigenous paradigmatic positions can further build up our visions of the potential of transformative research (as acknowledged by Cram and Mertens, 2015, 2016). And certain constructivist stances, such as embedded in Lincoln and Guba's "goodness" criteria for judging the quality of research (2003, p. 263) can also serve to further a transformative agenda by enabling us to focus on the need for researchers/co-researchers to take into account their co-involvements in the crystallization or revision of social constructions.

As far as postpositivist-styled research is concerned, I argued that a position such as the one advanced by Hammersley (2003) and Hammersley and Traianou (2014), where research is defined as *ideally not influencing the social (or ecological) world at the point of cognition* cannot easily find meeting points with positions which see research as *part of the unfolding world which is being "researched"*. But I suggested that there may still be options for developing a broader conception of postpositivism (which I outlined via Table 9.1). Such a conception would allow us to consider potential meeting points between this and paradigms which emphasize the mutual interlocking of researchers with research participants/co-researchers (and wider social networks), who are seen as influencing one another during the research process.

I used the examples of the work showcased in Chaps. 2–6 to lend substance to the extended positions that I portrayed via Table 9.1; and I offered some deliberations around how the authors "place" themselves and how readers looking at their work can in turn place the work. These placements are of course to be seen as part

of a conversation which may help all those concerned and involved in the conversation to enrich their conceptualizations of the research (in terms of different ways of judging its worth). I suggested that work need not necessarily be placed under any one paradigmatic category, and that any tentative categorization can still become a trigger for further conversations. As far as researchers who might wish to self-categorize their work as involving a diversity of paradigms (so that it is not "boxed" in any territory), I left in abeyance whether researchers can proceed with multi-paradigmatic commitments. Johnson reflects on this question as follows:

> Sometimes I wonder how incompatibilist or purist or a priori thinking overlooks the millions of people across the world who have multiple cultural allegiances and people who move back and forth between two or even three or four languages. Moving from language to language is not especially dissimilar to moving across worldviews/paradigms, at least not according to those holding a strong or weak version of the linguistic relativity hypothesis. (2015a, p. 689)

Using the metaphor of paradigms as being languages which offer different worldviews, in this case about the enterprise of "research", he considers that there is no reason why researchers cannot make "Gestalt" switches between languages as they proceed with their research, thus applying a variety of ways of doing research (2015a, p. 690).[16] Other authors argue that one or other paradigmatic position is likely to be predominant in one's consciousness/feeling, even if one is trying to incorporate a variety of perspectives/paradigms on what is properly involved in doing research (e.g., Midgley, Nicholson, & Brennan, 2017, p. 155; St. Pierre, 2017, p. 40). This, however, does not preclude researchers (based in a "territory") from learning from what "other" paradigms offer, in order to enrich their position and find some meeting points with others. As Midgley, Nicholson, and Brennan put it, referring to Tadajewski (2008),

> a person from one paradigm can appreciate another speaking in a different "paradigmatic dialect". This way of thinking advances the debate because it allows us to see paradigms in relation to the perspectives of individual researchers. (Midgley, Nicholson, & Brennan 2017, p. 155)

With reference to Chaps. 2–6, I did not try to finalize a view on whether paradigms can be *worked across by researchers moving between positions* (as Johnson, 2015a, suggests) or whether it is more a matter of people learning to *enrich initial paradigmatic positions through conversation with others* (and possibly turn at some point to a different existing or created position). But what can be said is that the diversity of paradigms for guiding research and thinking retrospectively about one's own and others' work, at least leads to what Mertens (2010b, p. 9) calls a more "examined" approach to research as an enterprise in society. And my suggestion is that as part of this "examination", we would do well to *foreground the question of the meaning of responsible research practice*.

[16]Johnson (2015b), Johnson and Schoonenboom (2016), and Johnson and Stefurak (2014) refer to this a dialectical pluralist position, as a meta-paradigmatic position explicating how researchers can operate with a plurality of paradigms.

If I have to "place" myself in the discussion in this book, the terrain that I now feel best placed in would be transformative paradigmatic terrain, especially when the space is seen as being as wide as Mertens considers it, so as to embrace Indigenous worldviews. I have also tried to suggest throughout the book ways in which the transformative outlook can be stretched/enriched with reference to a consideration of research work which is not easily placed in terms of transformative/Indigenous notions of the long-term relationship-building and involvement in communities that ideally mark the research cycle according to the transformative paradigm (narrowly defined). The various examples that I have proffered in Chaps. 2–6 and the deliberations that I offer in Chaps. 7, 8 and this chapter all hopefully contribute to stretching/expanding our vision and enactment of what can be called "transformative" research, by focusing ways in which *researchers/co-researchers can innovatively tie research to considerations of impact*, thus exercising what I would call responsible research practice.

Before I close this chapter, I wish to point out that I have specifically named the research undertaken in Chaps. 2–6 using verbs that express an action intent. In Chap. 2 , I have named the research as an endeavour to review raced relations, that is, to reconsider raced relations, and at the same time enable some healing/reconciliation processes to already become active in this "reviewing". For Chap. 3, when referring to the international project on inclusive education, I used the verb "dealing with" to suggest that the research was not treated simply as an assumed-to-be world-mirroring exercise but became an active one of dealing in practice with concerns in relation to inclusive education. Regarding the national project dealing with school underperformance, I pointed to the attendant aim of "making schools better". In referring to the work done in Chap. 4, for the first project I spoke about locating opportunities for empowerment, again to highlight the active nature of the research endeavor (toward strengthening empowerment opportunities); and in the second project discussed in Chap. 4, I referred to the intention to locate (via the research) implications for post-conflict health reconstruction. In Chap. 5, although I have used the term "investigate" to characterize the experimental aims that were pursued in the two projects I suggested that what was being inquired into was the educative potential of the research enterprise—thus again suggesting the research's "active" intention. In Chap. 6, I referred to the first research reported upon as the development of participatory decision-making software to support reconsideration of implications of personal choices for social and environmental justice (especially by heavy consumers with heavy carbon footprints[17]), thus again highlighting its action-oriented intentionality. Regarding the research exploring the OTF case, I pointed out how the case

[17]Upon reading this chapter (March 2017) McIntyre-Mills indicated that she wished me to emphasize this point and the need to hold "the rich" to account by giving credits to "those who consume less" and providing offsets to subsidize those with low footprints. In her book entitled *Planetary passport: Representation, accountability and re-generation* (2017) she discusses the importance of holding people to account and expands upon her ideas developed in her book S*ystemic ethics* (2014a) that "selfish living affects others and our shared habitat" (pers. comm. via email).

write-up was written evocatively with the expressed intention to stir audiences reading the text. And likewise I pointed to the autoethnographic recounting of the setting up of the Green Belt Movement (GBM), where the focus was on "remembering" with a view to inspiring. In this way I tried through all the examples to extrapolate how the research showcased in Chaps. 2–6 can be said to have been guided in some way by research questions expressing transformative intent.

References

Adam, F. (2014). *Measuring national innovation performance*. Berlin: Springer.

Adams, T. E. (2017). Autoethnographic responsibilities. *International Review of Qualitative Research, 10*(1), 62–66.

Belmont Report: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. (1979). *Ethical principles and guidelines for research involving human subjects*. Washington DC: Office of the Secretary, US Department of Health and Human Services.

Bishop, R. (1994). Initiating empowering research? *New Zealand Journal of Educational Studies, 29*(1), 175–188.

Bless, C., Higson-Smith, C., & Sithole, S. L. (2013). *Fundamentals of social research methods: An African perspective*. Cape Town: Juta.

Chilisa, B. (2007). Decolonizing ethics in social research: Toward a framework for research ethics in African contexts. In A. Rwomire & F. B. Nyamnjoh (Eds.), *Challenges and responsibilities of social research in Africa: Ethical issues* (pp. 199–208). Addis Ababa: Organization for Social Science Research in Eastern and Southern Africa (OSSREA).

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., Major, T. E., & Khudu-Petersen, K. (2017). Community engagement with a postcolonial, African-based relational paradigm. *Qualitative Research*, 1–14. https://doi.org/10.1177/1468794117696176.

Cram, F. (2009). Maintaining indigenous voices. In D. M. Mertens & P. Ginsberg (Eds.), *The handbook of social research ethics* (pp. 308–322). Thousand Oaks, CA: Sage.

Cram, F. (2015). Harnessing global social justice and social change with multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 677–687). Oxford: Oxford University Press.

Cram, F., Chilisa, B., & Mertens, D. M. (2013). The journey begins. In D. M. Mertens, F. Cram, & B. Chilisa (Eds.), *Indigenous pathways into social research* (pp. 11–40). Walnut Creek, CA: Left Coast Press.

Cram, F., & Mertens, D. M. (2015). Transformative and indigenous frameworks for multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 91–109). Oxford: Oxford University Press.

Cram, F., & Mertens, D. M. (2016). Negotiating solidarity between indigenous and transformative paradigms in evaluation. *Evaluation Matters—He Take Tõ Te Aromatawai, 2,* 161–189.

Creswell, J. W. (2003). *Research design: Qualitative, quantitative, and mixed methods approaches* (2nd ed.). Thousand Oaks: Sage.

Creswell, J. W. (2014). *Research design: Qualitative, quantitative, and mixed methods approaches* (4th ed.). Thousand Oaks, CA: Sage.

Creswell, J. W., & Garrett, A. L. (2008). The "movement" of mixed methods research and the role of educators. *South African Journal of Education, 28*(3), 321–333.

Darling, F. (2016). Outsider Indigenous research: Dancing the tightrope between etic and emic perspectives. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *17*(3), Art. 6.

Denzin, N. K., & Lincoln, Y. S. (2003). Introduction: The discipline and practice of qualitative research. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (pp. 1–45). Thousand Oaks, CA: Sage.

Dick, B., Sankaran, S., Shaw, K., Kelly, J., Soar, J., Davies, A., et al. (2015). Value co-creation with stakeholders: Using action research as a metamethodology in a funded research project. *Project Management Journal, 46*(2), 36–46.

Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.

Gregory, W. J. (1996). Discordant pluralism: A new strategy for critical systems thinking. *Systems Practice, 9*(6), 605–625.

Habermas, J. (1976). A positivistically bisected rationalism. In T. W. Adorno, H. Albert, R. Dahrendorf, J. Habermas, H. Pilot & K. R. Popper (Eds.), *The positivist dispute in German Sociology* (G. Adey & D. Frisby Trans., pp. 199–225). London: Heinemann.

Hammersley, M. (2003). "Analytics" are no substitute for methodology: A response to Speer and Hutchby. *Sociology, 37*(2), 339–351.

Hammersley, M., & Traianou, A. (2014). An alternative ethics? Justice and care as guiding principles for qualitative research. *Sociological Research Online*, *19*(3), Art. 24.

Hesse-Biber, S. N. (2007). Putting it together. In S. N. Hesse-Biber & P. L. Leavy (Eds.), *Feminist research practice: A primer* (pp. 329–348). Thousand Oaks, CA: Sage.

Hesse-Biber, S. N. (2010). Qualitative approaches to mixed methods practice. *Qualitative Inquiry, 16*(6), 455–468.

Hesse-Biber, S. N. (2015). Mixed methods research: The "thing-ness" problem. *Qualitative Health Research, 25*(6), 775–788.

Hesse-Biber, S. N., & Griffin, A. J. (2015). Feminist approaches to multimethod and mixed method research: Theory and praxis. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 72–90). Oxford: Oxford University Press.

Hunter, A., & Brewer, J. (2015a). Designing multimethod research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 616–623). Oxford: Oxford University Press.

Hunter, A., & Brewer, J. (2015b). Conundrums of multimethod research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 185–205). Oxford: Oxford University Press.

Johnson, R. B. (2015a). Toward an inclusive and defensible multimethod and mixed method science. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 688–706). Oxford: Oxford University Press.

Johnson, R. B. (2015b). Dialectical pluralism: A metaparadigm whose time has come. *Journal of Mixed Methods Research, 11*(2), 156–173.

Johnson, R. B., Onwuegbuzie, A. J., & Turner, L. (2007). Toward a definition of mixed methods research. *Journal of Mixed Methods Research, 1*(2), 112–133.

Johnson, R. B., & Schoonenboom, J. (2016). Adding qualitative and mixed methods research to health intervention studies: Interacting with differences. *Qualitative Health Research, 26*(5), 587–602.

Johnson, R. B., & Stefurak, J. (2014). Dialectical pluralism: A metaparadigm and process philosophy for "dynamically combining" important differences. *QMiP Bulletin, 17,* 63–69.

Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.

Lincoln, Y. S. (1995). Emerging criteria for quality in qualitative and interpretive research. *Qualitative Inquiry, 1*(3), 275–289.

Lincoln, Y. S. (2001). Engaging sympathies: Relationships between action research and social constructivism. In P. Reason & H. Bradbury (Eds.), *Handbook of action research: Participative inquiry and practice* (pp. 124–132). London: Sage.

Lincoln, Y. S., & Guba, E. G. (1985). *Naturalistic inquiry*. Beverly Hills, CA: Sage.

Lincoln, Y. S., & Guba, E. G. (2003). Paradigmatic controversies, contradictions, and emerging confluences. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 253–291). Thousand Oaks, CA: Sage.

Lincoln, Y. S., & Guba, E. G. (2013). *The constructivist credo*. Walnut Creek, CA: Left Coast Press.

Maathai, W. (2006). *Unbowed: A memoir*. New York, NY: Anchor Books.

Mark, M. M. (2015). Mixed and multimethods in predominantly quantitative studies, especially experiments and quasi-experiments. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 21–41). Oxford: Oxford University Press.

Mazman, I. (2008). *Max Weber and Emile Durkheim: A comparative analysis on the theory of social order and the methodological approach to understanding society*. Retrieved August 31, 2016, at: http://www.acarindex.com/dosyalar/makale/acarindex-1423867208.pdf.

McIntyre-Mills, J. J. (2017). *Planetary passport: Representation, accountability and re-generation*. New York, NY: Springer.

McKay, V. I., Mohapi, S. J., & Romm, N. R. A. (2017). Rethinking school discipline. In M. Magano, S. Mohapi, & D. Robertson (Eds.), *Realigning teacher training in the 21st Century* (pp. 250–270). Hampshire: Cengage.

Mertens, D. M. (2007). Transformative paradigm: Mixed methods and social justice. *Journal of Mixed Methods Research, 1*(3), 212–225.

Mertens, D. M. (2009). *Transformative research and evaluation*. New York, NY: Guilford Press.

Mertens, D. M. (2010a). Transformative mixed methods research. *Qualitative Inquiry, 16*(6), 469–474.

Mertens, D. M. (2010b). Philosophy in mixed methods teaching: The transformative paradigm as illustration. *International Journal of Multiple Research Approaches, 4*(1), 9–18.

Mertens, D. M. (2012a). Transformative mixed methods: Addressing inequities. *American Behavioral Scientist, 56*(6), 802–813.

Mertens, D. M. (2012b). Ethics in qualitative research in education and the social sciences. In S. D. Lapan, M. T. Quartaroli, & F. J. Riemer (Eds.), *Qualitative research: An introduction to methods and designs* (pp. 19–39). San Francisco, CA: Jossey-Bass.

Mertens, D. M. (2014). *Research and evaluation in education and psychology* (4th ed.). Thousand Oaks, CA: Sage.

Mertens, D. M., Bazeley, P., Bowleg, L., Fielding, N., Maxwell, J., Molina-Azorin, J. F., et al. (2016). Thinking through a kaleidoscopic look into the future. *Journal of Mixed Methods Research, 10*(3), 221–227.

Mertens, D. M., & McLaughlin, J. A. (2004). *Research and evaluation methods in special education*. Thousand Oaks, CA: Sage.

Midgley, G., Nicholson, J. D., & Brennan, R. (2017). Dealing with challenges to methodological pluralism: The paradigm problem, psychological resistance and cultural barriers. *Industrial Marketing Management, 62,* 150–159.

Morgan, D. L. (2007). Paradigms lost and pragmatism regained: Methodological implications of combining qualitative and quantitative methods. *Journal of Mixed Methods Research, 1*(1), 48–76.

Morgan, D. L. (2014). Pragmatism as a paradigm for social research. *Qualitative Inquiry, 20*(8), 1045–1053002E.

Münch, R. (1988). *Understanding modernity: Toward a new perspective going beyond Durkheim and Weber*. New York, NY: Routledge.

Niculescu, G. (2014). New considerations about action research. *Annals of the Constantin Brâncuși, University of Târgu Jiu, Letter and Social Science Series*, (4), 28–33.

Oczak, M., & Niedźwieńska, A. (2007). Debriefing in deceptive research: A proposed new procedure. *Journal of Empirical Research on Human Research Ethics*, *2*(3), 49–59.

Onwuegbuzie, A. J. (2012). Introduction: Putting the *mixed* back into quantitative and qualitative research in educational research and beyond: Moving toward the radical middle. *International Journal of Multiple Research Approaches, 6,* 192–219.

Onwuegbuzie, A. J., & Leech, N. L. (2006). Linking research questions to mixed methods data analysis procedures. *The Qualitative Report, 11*(3), 474–498.

Peirce, C. S. (1931–1958). *Collected papers* (8 vols., Eds., C. Hartshorne. P. Weiss, & A. Burks). Cambridge, MA: Harvard University Press.

Phillips, D. C., & Burbules, N. C. (2000). *Postpositivism and educational research*. Lanham, MD: Rowman & Littlefield.

Popper, K. R. (1959). *The logic of scientific discovery*. London: Hutchinson.

Popper, K. R. (1969). *Conjectures and refutations*. London: Routledge.

Popper, K. R. (1994). *The myth of the framework: In defence of science and rationality*. London: Routledge.

Roman-Alcalá, A. (2015). Broadening the land question in food sovereignty to northern settings: A case study of occupy the farm. *Globalizations, 12*(4), 545–558.

Romm, N. R. A. (1996). Systems methodologies and intervention: The issue of researcher responsibility. In R. L. Flood & N. R. A. Romm (Eds.), *Critical systems thinking: Current research and practice* (pp. 179–193). New York, NY: Plenu006D.

Romm, N. R. A. (2001). *Accountability in social research: Issues and debates*. New York, NY: Springer.

Romm, N. R. A. (2010). *New racism: Revisiting researcher accountabilities*. New York, NY: Springer.

Romm, N. R. A. (2015). Conducting focus groups in terms of an appreciation of Indigenous ways of knowing: Some examples from South Africa. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *16*(1), Art. 2.

Romm, N. R. A. (2017a). Researching indigenous ways of knowing-and-being: Revitalizing relational quality of living. In P. Ngulube (Ed.), *Handbook of research on theoretical perspectives on indigenous knowledge systems in developing countries* (pp. 22–48). Hershey, PA: IGI Global.

Romm, N. R. A. (2017b). Conducting focus groups in terms of an appreciation of Indigenous ways of knowing: Invoking an Indigenous-oriented paradigm. In P. Liamputtong (Ed.), *Doing cross-cultural research in health social sciences*. New York, NY: Springer (in press).

Romm, N. R. A. (2017c). Reflections on a multi-layered intervention in the South African public education system: Some ethical implications for community operational research. *European Journal of Operational Research*, 1–13 http://dx.doi.org/10.1016/j.ejor.2017.02.022.

Romm, N. R. A., Nel, N. M., & Tlale, L. D. N. (2013). Active facilitation of focus groups: Exploring the implementation of inclusive education with research participants. *South African Journal of Education*, *33*(4), 1–14, Art. 811.

Romm, N. R. A., & Ngulube, P. (2015). Mixed methods research. In M. Gumbo & E. Mathipa (Eds.), *Addressing research challenges* (pp. 158–176). Johannesburg: Mosala-Masedi.

Romm, N. R. A., & Tlale, L. D. N. (2016). Nurturing research relationships: Showing care and catalysing action in a South African school research-and-intervention project. *South African Review of Sociology, 47*(1), 18–36.

Smith, L. T. (1999). *Decolonizing methodologies: Research and indigenous peoples*. New York, NY: Zed Books.

Smith, L. T. (2012). *Decolonizing methodologies: Research and indigenous peoples* (2nd ed.). New York, NY: Zed Books.

Ssali, S. N., & Theobald, S. (2016). Using life histories to explore gendered experiences of conflict in Gulu District, Northern Uganda: Implications for post-conflict health reconstruction. *South African Review of Sociology, 47*(1), 81–98.

St. Pierre, E. A. (2017). Post qualitative inquiry: The next generation. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry in neoliberal times* (pp. 37–47). New York, NY: Routledge.

Stephens, A., Jacobson, C., & King, C. (2010). Describing a feminist systems theory. *Systems Research and Behavioral Science, 27*(5), 553–566.

Stephens O. A. (2012). Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, Nigeria. *Doctoral dissertation*, Lagos State University, Lagos, Nigeria.

Sykes, B. E. (2014). Transformative autoethnography: An examination of cultural identity and its implications for learners. *Adult Learning, 25*(1), 3–10.

Tadajewski, M. (2008). Incommensurable paradigms, cognitive bias and the politics of marketing theory. *Marketing Theory, 8*(3), 273–297.

Weber, M. (1949). *The methodology of the social sciences* (E. A. Shils & H. A. Finch, Ed. and Trans.). New York: The Free Press.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Antioch University, Yellow Springs, Ohio.

Woldegies, B. D. (2016). Research highlighting options for gender empowerment in Amhara Regional State, Ethiopia. *South African Review of Sociology, 47*(1), 58–80.

Zhu, Z. (2011). After paradigm: Why mixing-methodology theorizing fails and how to make it work again. *Journal of the Operational Research Society, 62*(4), 784–798.

Chapter 10
Conclusion to the Book: Storying Our Co-responsibilities as Part of Methodological Write-up

Abstract Throughout this book I have forwarded suggestions for those involved in research endeavors to responsibly take into consideration the immersion of the research in the "becoming" of social and ecological existence (of which it is a part). I now suggest that when researchers post facto generate methodological stories around how the design of the research became ongoingly accomplished, these responsibilities should be foregrounded, with ethical considerations concerning the exercise of responsibility and co-responsibility included in the storying. Instead of focusing on trying to label research work with respect to designs as given in the (much cited) literature on strategies of inquiry, I propose that the doing, and writing up, of research should become differently geared. That is, we should pay attention to practices of *active involvement with others (as selves-in-relation) in trying to exercise (co)-responsibility for the future-forming potential of research.* I suggest that to highlight the quality of such activity, the word "active" can preface the labels commonly used to characterise research designs.

10.1 Introduction

In this book I have suggested that researchers—no matter what paradigmatic position might function as their starting position—should admit that ways of asking research questions and ways of designing research to address them, are not neutral in their social and ecological consequences. In Chap. 9, I indicated that although postpositivism as a research paradigm is associated with researchers trying to display "disinterest" so that findings in relation to research questions can be more or less objective, there is space in the paradigm, with some stretching, to accommodate a recognition that ways of seeing realities are indeed *ways of framing what is supposedly seen*. I used the arguments of Hunter and Brewer (2015a, b)—who refer favourably, as do many other postpositivists, to the Popperian position (Popper 1959, 1969)—to draw out this point. Once it is recognized that scientists cannot purport to be matching scientifically-produced statements with "reality", postpositivist-oriented scientists cannot absolve themselves of taking some

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8_10

responsibility for the effects of statements made and for how they are presented. (See Chap. 9, Sect. 9.2 and Table 9.1.) The same applies to scientists who express a paradigmatic agnosticism, where they presume simply to be undertaking science in an effort to advance our comprehension of realities (see Mark, 2015, p. 23).

I put forward in Chap. 9 that a constructivist position can more easily accommodate a sense of responsibility on the part of researchers and co-researchers for the potential impact of the research process, insofar as it is understood within a constructivist perspective that constructions generated via the research can *reproduce*, or alternatively, *alter/reconfigure*, "available" discourses in the social fabric. This is what Lincoln and Guba (2013, p. 65) term the *social Heisenberg effect* of research. This means that the task of constructivist-oriented researchers need not be limited, as Creswell implies, to "make sense of (or interpret) the meanings others have about the world" (2003, p. 9). The task can additionally be to make *new sense*, with participants and stakeholders, in terms of what Lincoln and Guba call "goodness criteria" (2003, p. 263).

I have suggested that transformative and Indigenous-oriented research paradigms specifically encapsulate in their assumptions a concern with the felt consequences of the research endeavor. In these paradigmatic positions, ways of "knowing" and versions of reality that are put forward through the research process, are assumed to be socially (and ecologically) consequential at root (cf. Chilisa, Major, & Khudu-Peterson, 2017; Cram & Mertens, 2015; Mertens et al., 2016). I have argued that because these paradigmatic positions recognize the impactful quality of all knowing endeavors, it is considered part of the research task to undercut styles of knowing and of being in society which (further) disadvantage the historically marginalized. (This extends to the wellbeing of nature, which too has become marginalized.)

Some authors espousing pragmatic epistemological positions also see the link between epistemology and ethics, and do not define pragmatism in the narrow sense of doing research that supposedly "works" to better understand realities. That is, the focus of such pragmatism (which Gergen, 2015, p. 290) summarizes as "pragmatism with a conscience" is how "understanding" is already future forming. This then implies that responsibility needs to be taken for modes of understanding and their possible links to stimulating constructive action (considered as action which generates increased justice, as defined in conversations set up during the research process). In his book *The responsible methodologist* (2015) Kuntz likewise contends that:

> This productive intersection between being and knowing—that ways of knowing can come to alter ways of being—produces new ethical considerations. Thinking and writing thus becomes endlessly productive, making possible alternative formations of being in the world. Following this logic, inquiry more easily takes on an activist stance [with the intention of productive intervention] … in the name of social justice. (2015, pp. 29–30)

Kuntz argues that in the light of the potential of inquiry to make possible "alternative formations of being", he himself takes responsibility for "refusing methodological work that results in closure" (i.e., in the likely foreclosure of productive

possibilities) (2015, p. 31). He indicates that his book, where he offers visions of responsibility as linked to supporting engaged relational inquiry with social-justice intent, can be treated as "his own essay in refusal" (2015, p. 130).

I have argued in Chaps. 2–6, supplemented with additional extrapolations in Chaps. 7–9, that it is possible to draw out from all the authors whose work I showcased in the various chapters that they could be said to be operating in terms of a sense of researcher responsibilities similarly conceived. However, to draw out more fully their relationally- and transformatively-oriented sense of responsibility (which "refuses" traditions of research as striving for distanced and disinterested inquiry) I had to engage in some inferencing from their texts, and in some further liaison (via personal communication) with them, to be able to showcase this aspect of their work. This means that they did not consider in their write-ups that they needed to provide detail on their conception of their responsibilities.

This lacuna can be a function of conventional ways of documenting "methodology", where researchers and co-researchers' *active involvement in contributing to the development of new discourses and actions are not regarded as needing to be highlighted*—unless perhaps if the research is specifically labelled as "action research". Meanwhile, discussions on "ethics" in write-ups are often confined to justifying the work in terms of the Belmont-type principles, which I suggested in Chap. 8 are in need of extension to accommodate extended conceptions of responsible research practice. The Belmont report (1979) defines researchers' professional responsibility, as does the Singapore statement (2010), as primarily a matter of their striving to advance "understanding", albeit that they need to show respect for subjects/participants as part of the process of interacting with them and they need to consider who might be bearing the risks and benefits of the research striving for understanding. In Chaps. 8 and 9, I argued that this conception of responsibility mitigates against researchers considering how the research work can be *more consciously future-forming in the "becoming" of the worlds of which research is necessarily a part*. In this chapter I wish to carry forward the point with which I ended in Chap. 9, namely, that an active intent can be foregrounded in the setting of "research questions" and in the (emergent) development of designs which reflect and enact such an intent. And methodological discussion at the point of "write-up" needs to be extended accordingly to focus on such activity.

Kovach, in her book on *Indigenous methodologies* (2009), has a heading called "Ethics as Methodology" (2009, p. 147). She underscores that "in thinking about Indigenous research ethics, the overarching theme is to conduct oneself in a way that reflects *miȳo"* (p. 147). Miȳo, she notes, is a Cree word defined/translated as "goodness" (p. 147). She indicates that this offers a broad interpretation of ethical responsibility. And she advises that conducting oneself in a way that reflects a concern with "goodness", *should be seen as part of one's methodology* (philosophy underlying one's way of proceeding). I would submit, following Kovach, that not only those propounding the value of Indigenous methodologies, but all researchers can consider that ethical conduct (in a broad sense and not merely in terms of Belmont principles) is part of "methodology", and should be written up as such. As Kuntz indicates, an engaged orientation on the part of "the responsible

methodologist" implies *working with* participants and "re-imagines responsibility as contextually situated and relationally aligned" (2015, p. 18). It is this manner of *working with*, in a "relationally aligned" fashion, recognizing that research is an "intervention into the world" (Kuntz, 2015, p. 30), that I propose should be foregrounded when detailing (to audiences) the "methodology" underpinning the research.

In the research literature more generally, as Creswell notes, there is not a univocal understanding of what "methodology" implies (2003, pp. 4–5). Some see it as referring to the strategy (or design) of inquiry that is adopted. Others, such as himself, see it as embracing the philosophical framework that underlies the research design employed, and further to this, the methods of observation, and the types of analysis and interpretation that support the design (2003, p. 5). The underlying frameworks, as I noted in Chap. 9, can be argued to involve paradigmatic assumptions regarding what constitutes valid or worthy research. Creswell considers that *methodology* includes such assumptions, and it also includes ways of linking assumptions to research designs. In similar vein, Taylor suggests that "methodologies are particular sets of theoretical [philosophical] assumptions, which underlie the choice of data collection and analysis methods and processes" (2013, p. 3).[1]

In discussing various research designs, Creswell (2003, pp. 13–16) points to designs that are commonly used in, respectively, quantitatively-oriented approaches, qualitatively-oriented approaches, and mixed approaches to research. He refers (p. 14) to frequently used *quantitative* designs as experimental designs and nonexperimental designs (such as surveys). He refers (pp. 14–15) to frequently used *qualitative* designs as: ethnographies; grounded theory; case studies; phenomenological research; and narrative research.[2] Considering *mixed method* designs, he explains (2003, pp. 15–16) that mixed designs (MMR) can be defined as involving various permutations of setting out to combine quantitative and qualitative philosophical assumptions and methods—which can include using different methods of data collection concurrently with one another or sequentially, and analyzing and interpreting results in parallel or sequentially. He notes that if the mixed methods study is meant to "serve a larger, transformative purpose to change and advocate for marginalized groups" (as advised by, for example, Mertens) then "transformative procedures, in which the researcher uses a theoretical lens … as an *overarching perspective* within a design that contains both quantitative and qualitative data can be used" (p. 16, my italics). This transformative framework, as he defines it,

[1]Taylor (2013) outlines her view of the differentiation between methodologies, methods and processes in *qualitative* research, but the same considerations (definitions) could apply in quantitative research.

[2]Merriam too refers to these common designs (2002, p. 7). But she includes in her account of designs what she calls "critical qualitative research" (2002, p. 9) and "postmodern research" (2002, p. 10), which offer challenges to "traditional" qualitative research. She suggests that when the intention is, as in critical qualitative research, to "bring about change" there are no "guidelines about how to do this type of study" (2002, p. 10). That is, there are no recipes for conducting research with this purpose.

> provides a framework for topics of interest [such as topics related to inequality], methods for collecting data [which should involve collaboration between researchers and participants/co-researchers], and outcomes or changes anticipated by the study. (2003, p. 16)

Creswell states that in transformative research, researchers will often suggest a "call for action" on the basis of the research results (2003, p. 238). But it seems to me that in his account of transformative-oriented research, the focus is still on researchers, together with co-researchers, using the research process to try to "find out" about the investigated topics and to provide a perspective that supports transformation—a perspective which then can *later* function as a "call to action" to try to address, for example, the inequalities etc. that have been portrayed via the research.[3]

Creswell does not concentrate on how researchers/co-researchers can take co-responsibility at the moment of the research, for how the ways of "investigating" are *already an intervention into the course of social (and ecological) life.* As I see it, Creswell does not highlight sufficiently what I wish to draw out in the transformative paradigm—namely, the appreciation that at all stages in the project, the research is necessarily an intervention in a world-shaping process, because *research is part of "the "world" being explored*. This is what I consider as implied by focusing (as in transformative and Indigenous paradigms) on the *development of knowing styles which undercut dominant ways of knowing*, which presume that research can be a more or less neutral enterprise at the moment of "knowledge production". This is also the point stressed by Kuntz (2015, p. 144) when he expresses caution with Creswell's way of defining research designs in a manner which lends itself to "extractive logics", as if knowledge can be separated from the immersion of researchers in the worlds being explored (and being formed partly through the research involvement).

Although Creswell proposes that researchers should be aware of the paradigm(s) informing their work (of which he names the Big Four—see Chap. 1, Table 1.1), he still seems to place his focus on how types of designs—qualitative, quantitative or mixed—are aimed at *explanations or explorations* of the social world, albeit that in transformative-directed research such explorations go hand in hand with an account of the "anticipation of changes" that might ensue from the research. What I want to suggest in this chapter is that a display of paradigmatic positioning together with an

[3]As an example of a transformative mixed methods design Creswell (2014, p. 237) cites Hodgkin (2008, p. 297) who stated her purpose of the research as being to demonstrate the existence of different social capital profiles for men and women (via quantitative data) and to provide a picture of gender inequality and expectation (via qualitative stories). Creswell remarks that unlike many feminist researchers, she does not create in her conclusion a "call for action to change the inequality [that is portrayed as a result of the research]" (2014, p. 238). He points out that for many researchers using a transformative perspective a "call for action" arising from the research would be included (2014, p. 64).

account of the "research design"—including an open-ended and emergent one[4]—is not sufficient to indicate to oneself or to others how one is *considering and exercising one's responsibilities (with others) in making a difference to the unfolding of "the world" at the moment of enacting the "designs"*. I refer at length below to Creswell's account of research designs, because his work is cited by a myriad of research students and other researchers who display the design of their research with reference to his work. (His book *Research design: Qualitative, quantitative and mixed methods approaches* reached its fourth edition in 2014.)

10.2 Reconsidering Designs Construed as "Finding Out" Exercises

Creswell hints that in the kind of pragmatism which he would support (see Chap. 9, Sect. 9.5), it is understood that researchers should shift the conventional focus on "asking questions about reality"—as if "reality" can be laid bare via the research process (2003, p. 12). Nevertheless, in his account/summary of common research designs available for use in the research process, he does not draw out how researchers—professional and others—can forefront considerations as to how, through the research activity, they are already *contributing to the shaping of realities* (viewed as in the process of becoming). This, as I noted in Chap. 9, would be Zhu's (2011) and Gergen's (2015) interpretation of the "reality question" in a pragmatic position. In other words, as I see it, Creswell's account of research designs (which echoes that of many authors before him) is ill-equipped to the task of enabling researchers—when organizing and writing up the "design" of the research—to concentrate on the exercise of responsibility springing from recognition of involvement in world shaping.

In Sects. 10.3–10.7 below, with reference to discussions in relation to the research work that I showcased in Chaps. 2–6, I offer a detailed justification of why I make this statement. But before this, I wish to point briefly to Creswell's summary expression of what he calls "frequently used" designs or strategies of inquiry (2003, p. 13). These designs are: for *quantitative inquiry*, experimental and survey designs; for *qualitative inquiry*, ethnographies, grounded theory, case studies, phenomenological research, and narrative research designs; and for *mixed inquiry*, various

[4]When discussing emergence in qualitative designs Creswell states that:

> For example, the questions may change, the forms of data collection may shift, and the individuals studied and the sites visited may be modified. The key idea behind qualitative research is to learn about the problem or issue from participants and to address the research to *obtain that information*. (2014, p. 186, my italics)

As will become clearer in my discussion in this chapter, his focus is still on *obtaining information* via the research.

permutations of mixing of designs (along with methods of observation, analysis, and interpretation used as part of the design).

In his outline of "quantitative designs" (experiments and surveys), Creswell indicates that these were developed as strategies of inquiry for the social sciences—mainly starting with psychological research experiments—"during the late 19th and throughout the 20th century" and that these strategies "invoked the postpositivist worldview" (2003, p. 13). (See also Chap. 1, Tables 1.1 and 1.2.) Creswell indicates that *experimental designs* are focused on examining relationships between variables with the intent to locate causality as operative in the social world. As he summarizes, "experimental research seeks to determine if a specific treatment influences an outcome" (2014, p. 13). *Surveys*, he explains, are used with the intention to "provide a quantitative or numeric description of trends, attitudes, or opinions of a population by studying a sample of that population" (2014, p. 13). By collecting survey data from representative samples, the data can in addition be used to establish the probability of the existence of correlations between variables in the population. As he notes, in nonexperimental research designs, researchers can "use the correlational statistic to describe and measure the degree of association (or relationship) between two or more variables or sets of scores" (2014, p. 12).

In these quantitative designs as Creswell explains them, the intent—in line with postpositivist thinking— is to *advance our knowledge about relationships of causality and/or correlation between variables.* Nevertheless, Creswell does appreciate that designs can be used with an intent different from their historical origin. Thus although, say, experiments and surveys can be said to have originated in a positivist worldview, this does not mean that they are always used in accordance with their origins. (This is consistent with his understanding that *methodology*, and not *method*s, defines how "methods" become used, and with what philosophical underpinning.) For instance, citing LeCompte & Schensul (1999), he indicates that surveys can be used with intentions normally associated with qualitative research (Creswell, 2003, p. 7).[5] (See also Sects. 10.4 and 10.6.)

As far as "qualitative designs" are concerned, Creswell points out that these have become "more clearly visible during the 1990s and into the 21st century" and that "their historic origin … comes from anthropology, sociology, the humanities, and evaluation" (2003, p. 14). He remarks in this regard that the constructivist philosophical position, "often combined with interpretivism" is typically seen as an approach to qualitative research" (2003, p. 8). As I cited in Sect. 10.1, Creswell defines constructivist research as seeking to "make sense of (or interpret) the meanings others have about the world" (2003, p. 9). Hence his account of the types of research designs associated with qualitative research also implies a concern with "finding out"—this time finding out about human meaning-making and social processes (rather than variables as in quantitative research). Accordingly, in his

[5]Likewise, as an example of qualitative methods being used with a positivistically-oriented underpinning, he cites how "narrative stories, associated with qualitative research, are being linked to quantitative event history modeling (Elliot, 2005)" (Creswell, 2003, p. 7).

advice on how to create a purpose statement for qualitative research he advises the following template:

> The purpose of this _____ (strategy of inquiry, such as ethnography, case study, or other type) study is (was? will be?) to _____ (*understand? explore? develop? discover?*) the _____ (central phenomenon being studied) for _____ (the participants, such as the individual, groups, organization) at _____ (research site). At this stage in the research, the _____ (central phenomenon being studied) will be generally defined as _____ (provide a general definition). (2014, p. 126, my italics)

Below I offer an exposition of his summaries of these designs (ethnographic etc.), which in various ways are aimed at *understanding* and/or *exploring* and/or *developing* [ideas around] and/or *discovering*. I point out briefly how these purposes can be reconfigured in terms of what can be called a more active position (which I depict in Table 10.1).

Creswell defines *ethnographical research* as that in which a "researcher studies the shared patterns of behaviors, language, and actions of an intact cultural group in a natural setting over a prolonged period of time" (2014, p. 14). The purpose, as he puts it, is for the researcher to "learn about broad culture-sharing behavior of individuals or groups (ethnography)" (2014, p. 187). This description of Creswell's does not point to ways in which ethnographic researchers might actively interact with, and *make a contribution, alongside participants*, to the cultural expressions which are being *developed and enacted in the setting under consideration*—as for example, one can say that Woldegies was attempting (see Chap. 4, and my discussion in Sect. 10.5.1).

When describing *grounded theory* Creswell refers to it as a type of research in which the researcher attempts to "derive a general, abstract theory of a process, action, or interaction grounded in the views of participants in a study" (2014, p. 14). In this account of grounded theory seen as a type of research, Creswell does not point to ways in which grounded (and generative) theorizing *can actively contribute to the alteration of discourses and actions* (as, for example, I suggested in Chap. 7 could be the function of *generative* "grounded theorizing" as an approach to theorizing that can cut across all types of research).

Creswell describes *case studies* as studies in which "the researcher develops an in-depth analysis of a case, often a program, event, activity, process, or one or more individuals" (2014, p. 14). That is, a bounded "unit of analysis" (bounded by time and activity) becomes explored in depth—or in the case of multiple case studies, many such bounded units. In this description of Creswell's, clearly the focus is on the researcher exploring the case(s) under consideration, rather than on a process of *re-exploration and re-enacting activities and processes, through active involvement with others*, as for instance, could be argued to have been enacted in the case study of OTF written up by Roman-Alcalá. (See Chap. 6, Sect. 6.3, and Sect. 10.7.2.)

In outlining *phenomenological research*, Creswell suggests that in this design "the researcher describes the lived experiences of individuals about a phenomenon as described by participants" (2014, p. 14). The purpose is to identify "the essence of the experiences for several individuals who have all experienced the phenomenon" (2014, p. 14). Creswell points out that in one sense phenomenology can

be regarded as a *strategy of inquiry* which sets out to explore the essence of an identified phenomenon as experienced. In another sense, Creswell avers that besides offering a type of design for the identification of essences, phenomenology is also a *philosophy* that can inform various types of qualitative research (2014, p. 15).[6] As he puts it, "understanding the 'lived experiences' marks phenomenology as a philosophy as well as a method" (2003, p. 15). This is because phenomenology is geared to focusing on the experiences of participants (life as lived) rather than on quantified variables (as in quantitatively-oriented research). This focus on meaningful experience is considered as the core defining feature of qualitative research. Nevertheless, as a *design* he points to the intention of phenomenological research to describe and/or interpret "essences". From my (active) point of view, what is left out of this account is a possible intention to use the research process to *actively co-consider how what might appear to be essences can be reviewed and shifted.* For example, in Chap. 2, I alluded to how the research inquiry exploring the "essence" of race and racism, *involved a process of our reviewing together ways of engaging cross-racially*.

Ngulube and Ngulube open the door to a more active interpretation of phenomenology when they note that it "may also qualify as a decolonizing research" insofar as it would "approach an inquiry without any stereotypes and prejudgments, which is the naive 'natural attitude'" [in Husserl's terminology, 1913]. (2017, p. 129).[7] Ngulube and Ngulube argue that in this conception of phenomenology, it would *try to shift stereotypes* that may be operative in the "natural attitude" of everyday living. They suggest that social constructions are seen as *open to reformulation* via a phenomenological research process. They argue, though, that as far as phenomenology as a strategy is concerned, "established recipes and frameworks of research procedures are not suitable in a phenomenological approach" (2017, p. 129). Or stated more strongly,

> phenomenology does not approach social reality with preconceived notions and procedures. The context of the research, including the experiences of the participants, and the subjective experiences of the researcher dictate how the phenomenon of interest may be investigated. (2017, p. 129)

On this interpretation of phenomenology as a way of engaging with research participants, Ngulube and Ngulube indicate that the subjective experiences of the (initiating) researchers, taking co-responsibility with participants for the way in which the research proceeds, will determine its course, including what transpires in the interaction between the researchers and co-researchers. This would also be consistent with Serper's indication (2010) that "Husserlian phenomenological evaluation" can involve people in reconsidering experience as it "comes to the experiencing cognition" (p. 131). Here phenomenological analysis implies that a

[6]This dual function of phenomenology also has been noted by Merriam (2002, p. 7).

[7]In offering key terms for their chapter, Ngulube and Ngulube summarize the "natural attitude" as: "Habitual habits of thinking of things including taking things for granted and having preconceived ideas about phenomena" (2017, p. 154).

reconsideration of the "natural attitude" can enable people to better "know what they are doing when they seek to influence the creation of themselves in company with one another" (Serper, 2010, p. 131, citing McNiff, 2007, p. 318). Serper points to the potential of a Husserlian argument (linked with phenomenological perspectives of other authors[8]) to be extended so that it can enable people (as inquirers in relationship with others) to

> auto-phenomenologically, creatively and self-dialectically identify, re-evaluate, process and reverse and … authentically transmute, shift and redirect and qualitatively transform experiences and situations in a person's life [in relationship with others]. (2010, p. 270)

Phenomenology thus need not be equated with seeking to identify given essences of lived experience, but with *facilitating a process of people reflecting together on ways of conceiving (and possibly transmuting) their modes of being together.*[9] Ngulube and Ngulube for their part suggest that considered as a "flexible research approach", phenomenology

> may resonate with ways of knowing of those who are on the margins if used appropriately. Phenomenology … may exhibit some of the traits of critical frameworks that advocate [the] privileging [of]:… practice, politics, action, consequences, performances, discourses, methodologies of the heart, pedagogies of hope, love, care, forgiveness and healing (Pelias, 2004). (Ngulube & Ngulube, 2017, p. 129)

I refer to these points here to underline that if we wish to consider (with authors such as Merriam, 2002, Creswell, 2003) that phenomenology is an underlying philosophy informing "qualitative" inquiries, it *can incorporate an active component*—as I elucidate further in Sect. 10.3.

Finally, as his last-discussed frequently used type of qualitative design, Creswell refers to *narrative research*, which he describes as a "design of inquiry … in which the researcher studies the lives of individuals and asks one or more individuals to provide stories about their lives" (2014, p. 14). He adds that "information is then often retold or restoried by the researcher into a narrative chronology" (2014, p. 14). That is, the synthesized restorying "combines views from the participant's life with

---

[8]He cites authors such as Heidegger (1962), Moustakas (1994), Landsman (2002). In keeping with the notion that phenomenology need not purport to offer an interpretation-free account of preconceptions held by people in the natural attitude, or what Landsman calls their "implicit ways of structuring experience", Landsman notes (2002) that phenomenological work "can never be completed, because for every experience there are myriad horizons".

[9]Beyer (2016) underscores that one of the main themes of transcendental phenomenology is intersubjectivity, which includes feelings of empathy: "From a first-person point of view, "intersubjective experience is empathic experience; it occurs in the course of our conscious attribution of intentional acts to other subjects, in the course of which we put ourselves into the other one's shoes". Also seen from a first-person point of view, "a given subject's lifeworld consists of the beliefs against which his everyday [natural] attitude toward himself, the objective world [as perceived] and others receive their ultimate justification". Beyer confirms that Husserl must not be regarded as an epistemological foundationalist". He notes that for Husserl, "even the objective spatio-temporal world, which represents a significant part of our everyday lifeworld, is constituted intersubjectively" (http://plato.stanford.edu/entries/husserl/).

those of the researcher's life in a collaborative narrative". (He cites Clandinin & Connelly's, 2000's account hereof.)

However, in Creswell's interpretation of narrative inquiry, the endeavor of asking participants to "provide stories" does not seem to involve what Kovach (2009, p. 125) calls "sharing stories" (among participants and/or between the initiating researchers and participants) where the mutuality of the encounter may *shift understandings, discourses and options for conceived action*. Kovach's approach to the sharing of stories also resonates with Simmonds, Roux, and Ter Avest's interpretation of narrative inquiry (2015, p. 36). They focus on Clandinin and Rosiek's conception of stories as being "one of the ways that we fill our world with meaning and *enlist one another's assistance in building lives and communities*" (Clandinin & Rosiek, 2007, p. 35, my italics)—thus stories become a *relational encounter* at the point of their "telling". (See also Chap. 2, Sect. 2.7.) It could be argued, for instance, that the sharing of stories in the context of the FGs sessions discussed in Chaps. 2–4, as well as in the one-to-one encounters described by Ssali and Theobald (Chap. 4), had a more active intent of (re)constructing (with participants) elicited "memories" that would be meaningful toward potential empowerment. The way in which researchers/co-researchers involved in the inquiry might take co-responsibility for such a possibility is not foregrounded in Creswell's account of narrative inquiry.

Overall, all of the above accounts by Creswell of the designs that he describes do not in my view make room for researchers to consider how they might introduce what Kovach (2009) calls *miyo* as part of methodological practice. The description of the designs, in effect, can have the consequence that would-be researchers reading these summary accounts, too will focus only on the ways in which designs are/should be geared to "finding out". That is, when considering how to proceed in their inquiries armed with these definitions of the designs, and when writing up for audiences how indeed they proceeded, the focus will most likely be on considering the research as properly directed to, for instance:

- tell us more (than prior to the study) about the operation of relationships of causality and/or correlation between variables;
- generate findings about expressions of culture;
- generate theories of social processes;
- provide an account of programs, events and activities;
- portray essences;
- provide us with stories of participants (as interpreted);
- develop results in terms of various mixes of the above intentions, as in multiple and mixed methods research (MMMR). (See also Creswell, 2014, p. 141.)

This means that accounts of how researchers/co-researchers might exercise their (co)responsibilities *for the potential impactful character of the research* become left out or de-emphasized in the doing and write-up of research. However, I would suggest that this is precisely what is worthy of attention. Below I offer further

deliberations on this with reference to the examples showcased in Chaps. 2–6. In the course of my deliberations I also offer suggestions for the labelling of designs to make more provision for naming research as *active*. Table 10.1 offers a presentation of what might be implied in actively developing designs with transformative intent and justifying these (in write-up) in terms of co-responsibilities assumed.

Table 10.1 Active development, and justification, of research designs

Design	Research orientation
Active experimentation	Set up and activate possibilities for encouraging participant development via experimental research, and account for design in these terms (with reference to solicited participant feedback on experience of the research)
Active survey research	Take into consideration (ideally with key participants concerned with the issues at stake) how the phrasing of questions/issues in questionnaires might impact on participant and audience constructions/discourses; make provision for (re) interpreting survey-generated data in community settings and for co-exploring their import on the level of action. Account for the development of the design in these terms
Active ethnography	Use the research space to shift sedimented cultural symbols insofar as these are experienced as disempowering of (marginalized) participants, and facilitate a process of co-exploring options for empowering action. Account for research in terms of co-responsibilities assumed in this process
Active grounded (and generative) theorizing	Use all research endeavors as an opportunity toward developing grounded as well as generative theorizing around key issues identified as problematic in terms of social and ecological justice concerns. Account for research in terms of co-responsibilities assumed for developing inspiring, future-oriented discourses
Active case study	Treat the setting of the case(s) as an opportunity to stimulate exploration and enactment of new activities in the settings, which express reflections (with others) around social and ecological wellbeing (as a matter of collective responsibility)
Active phenomenological research	Use the research space to undercut with participants stereotypical sedimented constructions and visions of dominance so that these are recognized as potentially in processes of *becoming* (in view of the potential for more "relationality" in our social and ecological being)
Active narrative research	Treat narratives developed in research spaces (where people generate stories in conversation) as an opportunity to create visions which inspire movements toward enriching lives and building sustainable communities
Active MMMR	Focus on how MMMR can be employed creatively to undercut deficit discourses and nurture empowerment possibilities, by formulating "research questions" with active intent. Account for the (emergent) MMMR design in these terms

With this tabulation as background, I now turn to an exposition of how I believe the research process and justification thereof can be (re)storied to focus on an "active" research orientation. In such restorying (relative to more traditional write-ups) the research becomes justified less in terms of its being a well thought out "finding out" exercise and more by *accounting for its world-shaping potential (and how co-responsibility can be assumed for this*). I elucidate my suggestions with reference to examples taken from Chaps. 2–6.

10.3 Storying the Methodology of Research Discussed in Chap. 2

When I "wrote up" in Chap. 2 my story of the way the research proceeded to review raced relations, I offered a detailed account of why I initially considered that setting up a cross racial focus group with participants might be a *way of starting to review raced relations while at the same time breaking taboos around cross-cultural talk concerning "race*". I then proceeded to show how the research "de-sign" emerged as people chose ways of taking part in the FG meeting and also chose ways of discussing with me their experiences of the FG session while sharing additional concerns that they wished to raise in further one-to-one and group exchanges. I headed Chap. 2 (upon write-up) as "active FG research and follow-up interviews/conversations" as a short hand to offer an impression to readers of how the research had proceeded. But more importantly, along the way of the write-up I tried to offer an account of how the different "characters" in the story, including myself, chose to exercise some responsibility for rendering the research meaningful and constructive in relation to concerns that were raised in the course of the research.

If I have to class the research under one of the "designs" which are available in the literature as labels for research, I might suggest that the term "phenomenological research" can signal to readers that the research was about reviewing lived experience, and *reviewing—as a mutual encounter—the "natural attitude" (in Husserl's terms, 1913), where people do not normally in everyday life specifically reflect on social constructions* (such as "race"). The research space set up through the FG/talking circle provided a space for us all to rethink and at the same time enact in a different way our understandings of race and racism. Retrospectively, one can suggest therefore that we had been drawing on a "phenomenological" research design, but not in Creswell's description of this. Instead, Nguluble and Ngulube's conception of phenomenological designs where procedures are a matter of working out, with participants, in practice *how to proceed to render the research meaningful* was more at play. Or rather, this is a design label that might aid me in my storying

around the "methodology" employed in the research, in that it can signal to readers that lived experiences of race and racism were being (re)-examined.

Before the research began I was familiar with the writings of Husserl and various sociological interpretations of his writings. And in this sense perhaps one can say that the research was informed by my knowledge of this. But the application of phenomenology in practice, and the way it was used could never have been read off from a "framework of research procedure" as to how to do phenomenological research, especially given the variety of interpretations of phenomenology (Ngulube & Ngulube, 2017, p. 129).

What I would say influenced me far more than any "frequently used" available design, was my intent to set up a cross-racial research sharing circle in the first instance, and to enable the research thereafter to take its course, in conjunction with participants. So what can we learn from these deliberations of mine that I am now offering retrospectively? I would suggest that methodological storying should take the form of *presenting a story about the intent behind initiating a research endeavor, which need not be merely a "finding out" intent* (as implied in all the "designs" outlined by Creswell). Besides the intent of those initiating the research (in this case myself), methodological write-ups can express how co-responsibility (invoking *miyo* in Kovach's terms) has been taken by all those involved for *trying to render the research "constructive" in terms of conversations held around concerns raised during the course of the research*. This is what I tried to underscore in Chap. 2.

My write-up in Chap. 2 was not an account of how an available "research design" was implemented. The account was a storying around the actors involved in the research, the various choices made (in relation to others, as acts of "being-in-relation), and people's co-involvement in making the research "work" in terms of its making a contribution that was experienced as such. For this, feedback concerning the research experience from the various actors needed to be encouraged, so that this too could be written up as part of a process of sharing research experiences (as I am doing via this book). Although the "effects" of the research can never be known with any certainty (as with any statements made), this does not mean that statements around such effects are irrelevant to the research enterprise and the storying about it. It just means that readers need to be aware of the status of claims made about the "constructiveness" of the research in making a difference to the quality of life as lived. And as with all storying, readers are invited to further input into the story from their perspectives and interpretations.

I have not tried to offer a clear statement here about the utility of appealing to given "designs" as given in research literature to guide research. Because the language of these designs seems to be oriented to *designing finding out exercises*, we may do well to rather not use these labels, even when stretched to take into account a more active intent. Nevertheless, as long as the labels become qualified/reconfigured by considering as part of methodological practice researcher and co-researcher co-responsibilities in terms of what Kovach might identify as *miyo*, the labels may serve some purpose. As it happens, my write-up in Chap. 2 does not refer to the use of any given design label; but readers may feel that the label I am

invoking in this chapter now to describe the design—which could be called *active phenomenology*—is helpful as a way of conceptualizing the emergent research design.

10.4 Storying the Methodology of Research Discussed in Chap. 3

Readers might have noticed when reading my examples in Chap. 3 of the international and national education research projects how I focused on how we (the university-based research teams) proceeded, and on how as "professional" researchers we tried to render the research participatory in spirit, even when starting with traditionally "distanced" approaches—as in the questionnaire administration outlined in Sects. 3.3.1 and 3.4.1. When explicating the projects, I did not appeal to usual definitions of what "survey" research amounts to when explaining the administration of the questionnaire and the analysis of ensuing data, but I pointed more (with some extrapolation) to how the researchers did, or could, take responsibility for ways of constructing questions (ideally with key participants, as was done to some extent in the national project) and for ways of treating the "data" generated.

I tried to foreground how the questionnaire phase of the research could be handled such that the analysis of data generated therein would not be mystifying for audiences (participants, stakeholders and readers of the chapter). I concentrated on how survey research can invite participation as a relational endeavor, where it is recognized in the first place that "data" are a *function of an interactive relationship* between, in this case the questionnaire items and responses to them, and in the second place that any data generated can be *reviewed* (in terms of retroductive logic—see Chap. 7, Sect. 7.4).

If I had to supply a label for this design I could say that in both cases a survey was first used, and then sequentially combined (via MMR) with FG discussions—as in a "mixed design". But this might still imply to readers (given the postpositivist origin of survey research) that this process was clear-cut—namely, questionnaire items are constructed to measure what they are setting out to measure, questionnaires are then administered, and analysis proceeds (normally so that "hypotheses" can be tested). Hence in Chap. 3 when writing about how the project team proceeded I did not use this terminology. I tried to show that even in the quantitative phase of the research detailed in Chap. 3 (as part of broader projects) the questionnaires could be used to stir creative imagination, to undercut deficit discourses, and to engage participants and others (stakeholders and wider audiences) in redefining what has been "found" and reconsidering its import on the level of action implications. The active intent was to undertake the research in a way which might, as Kuntz puts it, serve to "open up the future to unknown—perhaps even unrecognizable—possibilities" (2015, p. 132). With this in mind we "twisted" the traditional understanding of questionnaires as

being data collection devices, toward a recognition of the part they could play in, as Kuntz advocates, "not foreclosing on a future as … defined by the present" (2015, p. 133).

In the research detailed in Chap. 3, in both projects the intention to involve participants in the proceeding research discussions was partially fulfilled via FG sessions, but could of course have been fulfilled by any form of community discussion. What is important to highlight here is that our way of continuing toward the FG phase of the research was not aimed, as in Creswell's descriptions, at *understanding/exploring/discovering/examining the meaning of* phenomena of interest (2014, p. 126). Kuntz critically considers these purposes as lending themselves to an "extractive logic, implying that meanings can be "found" via qualitative inquiry outside of the relational encounters being formed (2015, p. 144). The FG sessions in both the research contexts discussed were aimed at reviewing with participants some of the "results" of the questionnaires, and continuing further to mutually develop ways of seeing and addressing concerns raised in the course of the research. Following also Kovach's advice (2009, p. 125), as facilitators we "shared as necessary" our own understandings; we did not shy from adding input into the FG discussions, which was appreciated (as can be seen from the feedback).

Furthermore, in the international project (see Sect. 3.3.1 of Chap. 3), we responded to participants' expectations that we would take matters forward into continuing forums for further conversation and action—so that instead of "theorizing" the notion of collaboration with reference to quantitative and qualitative data that had been "collected", *the scope for collaboration could be tried in action, as part of this emergent design*. This does not mean that I would class the research as "action research" (which Creswell, 2014, p. 187, notes, is another design that many authors have written about, as an alternative to the five popular qualitative ones that he displays). I have argued in this book that we did not need to engage in the traditional cycles of action research in order to generate a more active intent in the use of the designs (quantitative and/or qualitative) displayed by Creswell as "frequently used" (2003, p. 13, 2014, p. xx). What I have rather been trying to do in this book is illustrate—by way of discussion of examples chosen—how these frequently used designs and common methods of what are called data collection, analysis and interpretation, *can be re-tuned with more active intent* than currently discussed by authors outlining the nature of such research designs and methods.

As it happened, in the international project the transformative intent of the research appeared (emerged) in the South African context and was not described as such in the initial mixed methods research proposal of the project leader (of the whole project) for purposes of ethical clearance. Upon write-up for journal audiences, we classed our contribution in the South African context as being "pragmatic with transformative twist" (Romm, Nel & Tlale, 2013)—showing how we twisted the MMR design in this direction.

In the national project the research design was consciously organized with a transformative interventionist intent, in which space was opened for different actors (researchers, co-researchers, stakeholders) to take some responsibility for the contribution that the research could make. The term *active mixed transformative*

research could be applied here as a descriptive label. I add the word "*active*" to the word "transformative" to indicate that the inquirers (at least the lead inquirers if not all of us at the university involved in the project), with others involved, could appreciate that our research responsibilities were not limited to seeking "understanding" (as if in any case understanding is free from action consequences in the social world).

10.5 Storying the Methodology of Research Discussed in Chap. 4

10.5.1 Storying the Methodology of Research Reported upon by Woldegies

There are various ways in which we might classify the design used by Woldegies and participants in their reviewing options for gender empowerment in the chosen woreda of Ethiopia. Woldegies chose to invite a set of women (who had manifested "positive deviance" in the context under consideration) to participate in interviews, which would be followed by FG sessions with them and their supporting family members. When considering the classification of research projects, Merriam (2002, p. 9) makes the point that for a qualitative study to qualify as *ethnographic*, it must present a *sociocultural interpretation of the data*" (2002, p. 9, my italics). Merriam here seems to be suggesting that as long as a sociocultural interpretation is provided as a "lens through which the data are interpreted", we can aptly apply the label of ethnography. Seen in this light the study reported upon by Woldegies can be classed as such, as he was exploring with participants the (shifting) cultural context in which these women entrepreneurs were working (albeit that he did not use this label).

As part of her discussion on what renders a study ethnographic, Merriam notes that "most people are familiar with ethnographies of foreign and exotic cultures such as Margaret Mead's *Coming of age in Samoa* (1973)" (Merriam 2002, p. 9). She remarks that besides these familiar examples of ethnography, "there are also many ethnographies of various social groups within a larger culture". She points by way of example to "Cordeiro and Carspecken's (1993) ethnographic account of twenty successful Hispanic high school achievers" (2002, p. 9). In this description again (and via her example) we could place Woldegies's research with co-researchers in the domain of ethnography if we wish to supply such a label, as he was working with "successful" women entrepreneurs in what could be called a subcultural group of "positive deviants".

But as I noted when discussing Creswell's account of ethnography, the idea that ethnography is a matter of *locating* shared beliefs and values "in" a culture (or subculture), does not do justice to Woldegies and co-researchers' inquiries and actions, which can be said to have been *enhancing these women's and wider*

community empowerment through the way in which the research proceeded. Furthermore, it seems to me that as they were interacting they were actively involved in knowledge construction, together forming ideas about how women's empowerment could be forwarded. The intent was not to strive to explore "the culture" via what Naess (2016, para 7) critically terms "an impassive or neutral 'discovery' of facts". In keeping with Naess's understanding of ethnographic research as involving more than acts of "discovery", and taking this further, I would suggest that stories were deliberatively being *generated* via the research; and ways of jointly constructing knowledge were actively geared to being, in Gergen's (2015) terms, world-forming. Or, as Kuntz advocates, citing Denzin and Giardina (2010, p. 15), "we are no longer called to just *interpret* the world, which was the mandate of traditional qualitative inquiry. Today, we are called to *change* the world and to do it in ways that resist injustice" (2015, p. 134). Kuntz notes (2015, p. 133) that Denzin and Giardina here plead for "social-justice-oriented intervention" (as part of methodological practice).

Therefore, if I were to assign a designer label to the research reported upon by Woldegies (2014, 2016) I would call it *active ethnography*; that is ethnography with active intervention intent toward "resisting injustice". I would not call it what is sometimes expressed in the literature as "critical ethnography", which concentrates, as Merriam puts it, on

> critical questions about the influence of race, class, and gender (and their intersections), how power relations advance the interest of one group while suppressing those of other groups, and the nature of truth and the construction of knowledge. (2002, p. 10)

To me this research was more than just a matter of asking these questions—due to its *active component at the point of knowledge construction.*

What I see as important from Woldegies's account of the research is that he details how the *inquiries were used in order to strengthen empowering discourses and actions*. While Woldegies did not use the label of *ethnography* or *critical ethnography* to characterize the research strategy, I would suggest that if Woldegies could have had at his disposal the concept of "*active ethnography*", this might have helped him to signal to readers how he as initiating researcher, with participants as co-researchers, was involved in an inquiry process that could enhance options for gender empowerment in the sociocultural setting (as experienced). Furthermore, he recognizes that readers (in other settings) may be moved/stirred by the generative theorizing which he proffers, for which he also takes some responsibility. Meanwhile, as noted in Chap. 4 and additional chapters in this book, I had to do some extrapolations to extrapolate the active role taken by Woldegies in conjunction with participants.[10]

[10]When encouraging him to write up the work for the special issue *of South African Review of Sociology* entitled "research processes directed toward social development" (2016), I suggested to him to draw out some of these aspects more strongly.

10.5.2 Storying the Methodology of Research Reported upon by Ssali and Theobald

Ssali and Theobald (2016) indicate that they chose to use a life history interviewing approach to enable participants to narrate their stories in their own terms regarding their memories (as now told) of the North-Ugandan war, with special attention to health implications. Merriam indicates that narrative inquiry by definition implies "the use of stories as data, and more specifically, first-person account of experience told in story form" (2002, p. 9). She notes that other terms for this type of research are "biography, autobiography, life history, oral history, autoethnography, and life narratives" (2002, p. 9). Because Ssali and Theobald were "using stories", we can class the research as involving narrative inquiry.

But more important than this label, Ssali and Theobald offer deliberations around the specific relationships that they tried to set up with participants (and later with stakeholders) *so that the research could be an empowering process*. In other words, the label of narrative inquiry in itself would not signal how such inquiry might be organized/designed with (active) empowering intentions and hopefully outcomes.

Merriam alludes to the fact that stories told in research contexts need not necessarily refer to a revelation of experience independently of the context of interaction between the "researcher" and "story teller". As she puts it, citing Manning and Cullum-Swan (1994, p. 464):

> Context is important, … for [in Manning and Cullum-Swan's exposition] "narratives take many forms, are told in many settings, before many audiences, and with various degrees of connection to actual events of persons". (2002, p. 9)

Ssali and Theobald were aware of this—and did not write up their methodological exposition as if the research was an act of "finding out" about people's experiences via their storytelling. They point to the research as being more an act of *generating stories in a way that would enable the participants to feel in control of their storying*. In this way, what Kuntz (2015, p. 31) calls an "extractive logic" was arguably circumvented by Ssali and Theobald. Furthermore, whatever the participants highlighted as worth recounting was taken as important; and was highlighted in turn by Ssali and Theobald in their liaison with leaders in the community, with policy makers, and in their blog development, etc. That is, the issue of whether the experiences as remembered/told refer to "connections to actual events" or not, was not an issue for Ssali and Theobald. Rather, as I am interpreting their write-up, the researchers wished to enable participants to *take responsibility for sharing what was important to them* with the researchers, so that on the basis of the stories as told, combined with interpretations of available statistical data on health care outcomes, health reconstruction efforts could be developed. Here the (professional) researchers acted as mediators, and *in this sense too took responsibility for some of the potential effects* of the research. All in all, I would say that the label of *active narrative inquiry* would be a suitable "design" label for this research.

Again, as with the research reported upon by Woldegies, I had to do some extrapolations to draw out the elements of what I am calling active narrative inquiry. I would suggest that if this label is rendered available as a design label, it might help those doing and writing up research to consider accounting for their responsibilities in view of the world-shaping effects of (any) research.

10.6 Storying the Methodology of Research Discussed in Chap. 5

10.6.1 Storying the Methodologies of Research Reported upon by Stephens and by Oczak and Niedźwieńska

Readers reading Stephens' self-reporting (2012) on the experiment that he set up in the Lagos prisons in Nigeria, could get the impression that all that he was doing was using an experimental design in terms of clearly laid out rules as to what this involves. Creswell argues that "because quantitative studies are the traditional mode of research, carefully worked out procedures and rules exist for the research" (2003, p. 22). Creswell adds that "this means that researchers may be more comfortable with the highly systematic procedures of quantitative research" (p. 23). My way of seeing Stephens' experiment, is that when writing it up, he tried to show that "highly systematic procedures" had been used in line with traditional understandings of experimental research. Hence he detailed how he divided the sample into treatment and control groups and administered pre-and post-test questionnaires with reputable scales to measure creative and emotional intelligence before and after the "treatments" as administered (as in the classical experimental design).

Although he concentrated on all of this in his write-up in order to justify for readers (dissertation examiners in the first instance) that he was "doing science", I would argue—based on my in-depth conversations with him—that he was doing far more than this and was aware of this. Nonetheless, in order to merit the label of science for the work, he gives no attention to the relationships that he had built with the recidivists and to the possibility that the manner of administering the "treatment" so that the recidivists would feel that he cared for them, may well have affected the results (as observed by comparing the pre-and post-test scores across the groups). As Mertens and McLauglin (2004, p. 63) note, "experimenter effects" are regarded within research experimentation as threatening the validity of the research in terms of being able to determine the effects of an administered treatment. However, in Chap. 5 I mooted the idea that possible "experimenter effects" can be regarded as an opportunity to capitalize upon in active fashion. If experimenters through their caring, influence "results" obtained, but indicate to readers how indeed this might have operated (as Stephens did to me in personal conversation), then I would say that *this can be regarded as an example of active experimental research.* In other words, I suggest that Stephens' methodology could

be written up as a story in which he displays to readers his concerns that he brought to the study, his relationship building with participants, his way of administering the "treatment", and various options for interpreting the results.

The value of the research lies, inter alia, in readers being able to consider the context of the treatment (including the qualities of the experimenter and how participants might be responding to these), and to consider how they too may be able to take some responsibility for using experiments to try to generate, in active fashion, valued "results". Furthermore, responsible generative theorizing around the wider significance (so-called external validation) of this experiment in pointing to possibilities for giving recidivists a chance and in reconsidering the cultural milieu of recidivism, still can be accomplished.

Similar deliberations can be made in relation to the experiment undertaken by Oczak and Niedźwieńska (2007). In this case, although the experimenters were not personally known to the participants, the experimenters interacted with them during the new debriefing session and indicated that they cared sufficiently to assist them to learn how they could act in the face of (persuasive) suggestibility; and they also showed that they cared by asking all the participants for feedback on the experience of the experiment. This in itself might well, as Oczak and Niedźwieńska imply, have influenced participants' feelings regarding the experiment. But this does not mean that the experiment thereby is less valuable. On the contrary, I would say that it is valuable precisely because Oczak and Niedźwieńska take some responsibility for trying to positively impact on the participants (and on future ones involved in psychological experimentation). They try to illustrate by way of this experiment how experimentation can become a learning experience for participants and a more enjoyable/interesting encounter for them. I would thus (as with Stephen's experiment) consider their creative use of the experiment to try *to perform an extended educative function*—while additionally requesting participant feedback on their experiences of the experiment (for the benefit of other audiences)—as an example of *active experimental research.*

10.7 Storying the Methodology of Research Discussed in Chap. 6

10.7.1 Storying the Methodology of Research Reported upon McIntyre-Mills

McIntyre-Mills explains that the online software package employed in the project in South Australia with residents in the chosen local community in some ways functioned as a "questionnaire", but in another way was far more than this. Although the software was a means to "gather data" it was also a means for people to engage with the questionnaire items, to add options as they engaged with them, and to enable people through the online package to be aware of the kinds of inputs

that had been made by others. Furthermore, the re-examination of pathways to wellbeing encouraged by the software (which had been developed/co-designed in previous projects in which McIntyre had been involved—see footnote 15 of Chap. 9) included some FG sessions beforehand with residents of the community to flesh out "scenarios". McIntyre's intention was that the software would function as a means to enable people to review their choices in regard to social and ecological wellbeing. (Chap. 6, Sect. 6.2.)

In naming the research I would suggest that we can consider the research design as an *active online survey*, combined with qualitative data generated to support the survey—such as the data from the initial FG sessions with residents, from meetings with key participants in relation to the "results" of the software, and meetings with SALGA (as a stakeholder). McIntyre-Mills herself (McIntyre-Mills 2014a, b) labels the methodology as participatory action research (PAR). But this might imply that she had been involved with participants (for example, those from SALGA and residents in the community) in planning actions, so that these actions could be tried out in practice and dialogically reflected upon, thus initiating further cycles of planning, implementation and reflections on outcomes of the action. I do not see from her storying around the research (McIntyre-Mills 2014a, b, 2017) that these kinds of cycles took place. Although the domain of action research can be regarded as sufficiently broad to incorporate the creative use of online surveys to encourage different people to review their choices, I would argue that it was for want of the term "active" that McIntyre-Mills chose the term PAR to signal to readers the action-oriented intent of the research.

Reason and Bradbury, in their preface to the *Handbook of action research* do define PAR broadly as "the whole family of approaches to inquiry which are participative, grounded in experience, and action oriented" (2001, p. xxiv). But they note at the same time what they call "the difficulty with the words 'action research'" (2001, p. xxiv). They also point out that "action researchers will draw from a range of methodologies, both those described here [in the handbook] and, where appropriate, from recent innovations in qualitative and sometimes quantitative research" (2001, p. xxv). Broadly conceived as a term which includes "action oriented" participatory research, the term PAR may apply to the research work reported upon by McIntyre-Mills. Nonetheless, perhaps a more appropriate label would be "participatory *active* research".

Whether or not we reserve the term *action research* for research involving spiraling cycles of planning, action, observing, and reflecting (cf. Dick et al., 2015; Mapotse, 2012; Sung-Chan, Yuen-Tsang, Yadama, & Sze, 2008), I would suggest that we need a label that points to McIntyre-Mills' managing to initiate an *innovative and active approach to the design of an online survey*—which involved active employment of a questionnaire to generate data while *encouraging people's capabilities to review their choices.* The label could also indicate that this was coupled with *active use of qualitative data* generation (understood as generated in certain interactive encounters, e.g., via the focus group sessions and other methods), *active interpretation* (as an active process involving key participants in reconsidering the import of the quantitative "results") and dialogical reporting (in this case,

inter alia, through discussion with SALGA). The term *active* signals that the purpose of the research (as identified by McIntyre-Mills in the first instance) was that it *could be consciously future-forming*; and it points to how the various actors (including McIntyre-Mills and others appearing in the methodological storying) might have *tried to take some responsibility for this*.

McIntyre-Mills adapted the "traditional" action research cycle while setting up a research space to try to contribute to the furtherance of ecological and social wellbeing. Hence, even if researchers are not deemed as engaged in traditional action cycles, the research work can still be called *active,* or in Reason and Bradbury's terms "action-oriented" (2001, p. xxiv).[11]

10.7.2 Storying the Methodology of Research Reported upon by Roman-Alcalá

Roman-Alcalá (2013, 2015) considers his work—in which he was involved in acting and in reflecting with others in the form of interviews and conversations concerning the meaning of the Occupy The Farm (OTF) at the University of California (2012)—as being a case of insider PAR. Because his concentration is on the OTF case (what Creswell, 2014, p. 14 would call the case of an "event") it can be classed as a type of case study. Many action research projects involve "case" work as people together explore in practice what is possibly achievable in a case identified: see also Romm 2001a, p. 241. In terms of the deliberations that I offered in Sect. 10.7.1, I suggest that, as in the research work detailed by McIntyre-Mills, if the label of PAR is to be applied, this would imply some modification of the usual definition of PAR. In Chap. 6, I suggested that Roman-Alcalá's reflections, with others, around the import of the case with a view also to outlining possibilities for revisiting food security strategizing, would render it an *action-oriented case study research*. By focusing on Roman-Alcalá's intent in reflection with others to display their sense of co-responsibility in forwarding such goals, the research could also be labelled as *active case study research*.[12] And/or it could be described as *insider based participatory active research*. This also leaves leeway for readers to consider how case study research can be rendered *active in various ways* (such as the ways I pointed to in Chapter 6) without thereby committing to traditional designs of "action research".

[11]Kuntz suggests that the term "methodological activism" can also appropriately be applied to signal the intent of inquiry to "make newly possible, to expose cracks and interstices that otherwise escape processes of meaning-making so that we might live differently" (2015, pp. 26–27).

[12]Romm and Hsu (2002) also applied the label active case study research to the research undertaken in a Taiwanese harbor to explore shifting experiences of "power distance".

10.7.3 Storying the Methodology of Research Reported upon by Maathai

Maathai does not label her book *Unbowed* (2006) as "research", but from the description of narrative inquiry that Merriam supplies (2002, p. 9) it can be placed as autoethnographical. In Chap. 6 (Sect. 6.4.1), I suggested that it could be classed as *transformative autoethnographical*. Because Maathai was not necessarily trying to write up her approach for a research audience, she does not assign any label to it for such audiences (in relation to methodological literature). But it is still noteworthy how she details her inquiry-in-action journey so that readers can gain a sense of what became important to her in her learning journey (as now remembered). It is to me a story which points to an emphasis on professional and non-professional researchers (such as the "foresters without diplomas") taking some responsibility for generating inquiry-in-action which can input positively in addressing social and ecological injustices. She offers a story of her own journey as an inspiration to others to undertake journeys of inquiry in action to address felt injustice (as defined in conversation between people, especially those most marginalized, and those in tune with "nature"). Her autobiographical write-up is another exercise in trying to impact positively on a world in motion, through her trying to inspire others to constructive world-shaping. For these reasons I would class it as *active autoethnography*.

10.8 Rendering Frequently Used Designs and Methods More Active

What I have been trying to accomplish in this book is illustrate (and justify) how "popular" or "frequently used" designs and methods can *become more actively used than traditionally catered for in well-cited literature on these designs*, with the methodological literature tending to focus on research as being a *finding out exercise*. I have instead tried to focus on how researchers (professional and others) can exercise their co-responsibilities in taking into account the way in which the research exercise itself may be *world shaping* (as Gergen, 2015, puts it). When reading the authors whose texts I showcased, I glimpsed a recognition—more or less explicitly expressed—of the need to take into account the consequential character of the research endeavor. Nevertheless, I still had to do extrapolations from the texts in order to detail a view of responsible practice, and some of the time this was helped by my communications with the researchers, where I probed/sought more clarity on this.

My intention in doing this extrapolating, was not so much to be able to say that the authors "did" work with a vision of active and responsible research (which also provided for co-responsibility with others), *but to highlight the potential to use "frequently used" designs and methods with active intent*. I chose to concentrate on the two popular quantitative designs specified by nearly all writers writing about

quantitative research (namely, experiments and surveys), and on the five qualitative designs specified by Creswell as popular, which have also been named as popular or as commonly used by authors such as Merriam (2002, p. 6) and Mertens and McLaughlin (2004, p. 97)[13]—along with "popular" uses of MMMR.

And I chose examples of popular data collection methods that support the use of these designs—such as questionnaires (including online ones), secondary statistical data obtained from records, other uses of records/documents[14], interviews/conversations (of various types), focus groups, and observations (including insider participant observation), and use of life experience (autobiographical). *Each time I tried to illustrate how future-forming intervention (with justice intent) can be built into the designs and attendant methods, at the very point of use* and not just as a way of creating "findings" that supposedly might later function as a call to action that may or may not be taken up. I took readers through the detail of the research processes employed (as I interpreted them) and I focused on various ways of exercising responsibility which go beyond the notion of responsibility that I suggested in Chap. 8 infuse the Belmont report on ethics (1979) and the Singapore (2010) statement. (See Chap. 8, Sect. 8.1.)

Mertens and McLaughlin (2004, p. 101) note that because of the plethora of ways in which qualitative research can be undertaken (even "within" the so-called popular designs) some researchers have suggested that when explicating their work for others, it is good practice for authors/researchers to *describe the details of how the research was handled.* They indicate, for instance, that:

> Because there is no one correct method for conducting qualitative research, Bogdan and Biklen (1998) recommend that researchers describe their methodology in detail. (Mertens & McLaughlin, 2004, p. 101)

I have suggested in this chapter that I too would consider this to be good write-up practice, also with a view to underscoring how "*miya*" (Kovach, 2009, p. 147) may have been present in the conduct of the research. I have furthermore suggested that the idea of spelling out the details of the methodology, with an account of the roles and assumptions of responsibility of all those involved need not be confined to the write-up of what is called *qualitative* inquiry, but can be extended to *all* methodological write-ups. Other researchers (professional and lay) reading such write-ups, may well learn from the way in which authors indicate how in the research contexts in question these responsibilities were handled. This then can function as a resource for others to bear in mind—as Gergen (2015, p. 301) also explains.

Readers may have discerned that in the examples that I showcased, it seems that in all cases it was "professional researchers" who could be said to have initiated the

---

[13]As noted in footnote 2 above, Merriam adds some additionals, but does not name them as being as popular. Mertens and McLaughlin, with reference to Merriam (1998) concentrate on four forms of qualitative research: ethnography, phenomenology, grounded theory, and case study (2004, p. 97).

[14]Mertens and McLaughlin state that "typically, qualitative researchers use three main methods for collecting data: observation, interviews, and document and records review" (2004, p. 101).

research enterprises. This set of examples that I chose was intentional: I was not trying to detail approaches to PAR where the source of initiation of research may not be with professional researchers (with their links to academia); nor was I trying to detail community based participatory research approaches (CBPR) where the research partnership is clearly initiated collaboratively. As Creswell reminds us (2014, p. 13), there are very many books/articles on these designs. What I have been trying to do in this book is offer examples of how certain "commonly used" research designs can take on an increasingly emtic character (as Onwuegbuzie, 2012, names it)—as an interactive relationship develops with participants/co-researchers.

Minkler makes the point (citing Reason, 1994, p. 36) that many participative projects, paradoxically, "would not occur without the initiative of someone [or a team of people] with time, skill and commitment, someone who will inevitably be a member of a privileged and educated group" (2004, p. 687). What Minkler proposes, when faced with this scenario, is that it is incumbent on the initiating researchers to seek ways of trying to "shift control" (2004, p. 687). This perhaps implies some revision/stretching of Cram and Mertens' considerations regarding participants being in the driver's seat from the very start in the phase of "relationship building" (2015, p. 98). I would suggest that the way in which professional researcher and research participant relationships become built up depends on the commitments of the involved parties at different points during the research. What is important, as I see it, is that (self-identified) professional researchers need not define their professional identity as geared to locating "findings" (via their methodology)—which then may become shared *as findings* with participants and perhaps stakeholders. Rather, they can define their identities as being oriented to facilitating a co-inquiry encounter—understood as an interactional encounter in which responsibilities and co-responsibilities for the world-shaping influence of research endeavors are recognized and taken into account in some way.

I underscored throughout this book that even if researchers do not feel that, as researchers, they need to take any responsibility (with others) for the potential effects of research work in the social domain, this does not mean that the research being undertaken is free of intervention effects. As I summarized in Romm (1995, 2001b), knowing is already intervention. On these grounds I have suggested that those setting up inquiries would do well to try to facilitate research in such a way that its likely impact is taken into consideration; and of course feedback from participants and others affected (such as communities in which the research is conducted) helps to offer ideas as to how the research is being experienced so that it can be re-tailored if necessary. What I take to be the strength of the transformative paradigm (especially combined with an Indigenous worldview of relational existence), is the focus—which I have tried to further draw out—on the consequential character of the research enterprise. Cram and Mertens articulate this as follows:

> The transformative methodological assumption … aligns with the ethical imperative to support human rights and social [and ecological] justice, the consciousness of the *sources of different versions of reality* and *the consequences associated with accepting different versions of what is real*, and the need to establish trusting relationships that are cognizant of

> cultural and power-related issues (LaFrance & Crazy Bull, 2009; Mertens, 2014; Mertens & Wilson, 2012). (Cram & Mertens, 2015, p. 100, my italics).

Here Cram and Mertens point to the need to *consider the consequences that may be associated with accepting different versions of reality*, given that we do not have unmediated access to "reality". I have suggested that *this understanding of the consequential character of research is what needs to be emphasized as an important contribution of the transformative and Indigenous paradigms*.

I forwarded the idea in Chap. 9 that with some expansion (if necessary), other paradigmatic positions can also make provision for researchers to take on board an increased sense of responsibility for social and ecological consequences of inquiries (see Table 9.1). I have suggested that even postpositivism, which is traditionally understood as expressly not conceiving researcher responsibilities in this manner, can, with "stretching", accommodate such a conception in some way. At the same time, I have suggested that the transformative paradigm too can be understood (and enacted) as evolving and as developing from engagement with alternative paradigmatic positions. I have maintained that what I would call active research—which may not include from the start the highly collaborative relationships as envisaged by, for example, Mertens (2010, p. 472), but which *makes provision for their emergence* (in varying degrees)—can still be termed "transformative research". This would perhaps extend our conceptions of the operation in practice of the transformative paradigm, with the focus being on taking (co-)responsibility for research effects.

What I have attempted to do in this book is make a case for "active research", which can appeal to a wide range of researchers and co-researchers, who can try innovatively to re-tune popular research designs with the intention to make—with others, as "selves in relation"—a conscious contribution to the quality of social and ecological life.

10.9 Some Less Frequently Used Designs

Besides the designs identified in the research literature as common or frequently used, there are of course many designs less commonly used. As indicated in Sect. 10.8, my intention in this book was to concentrate on how research conducted in terms of frequently used (popular) designs might be innovatively shifted toward becoming more active. This is so that the book can reach audiences who might be willing to reconsider the usage of these designs, hoping to stimulate readers to take a more active stance when using such designs (which can be labelled by prefacing them with the word "active", to signal a different approach). But in closing, I will make a few comments on some of the less frequently used designs that I mentioned briefly in the course of my discussion in previous chapters.

10.9.1 The Mmogo-Method™

In Chap. 1, I referred the Mmogo-Method™, and its potential to tie research to strengthening relatedness and togetherness in African cultural contexts. The method itself is based on using what Roos calls "visual narratives" as part of a process of participants "actively collaborating in constructing meanings … so the research event becomes a joint act concerning that which is being researched" (2008, p. 659) To this end, she notes:

> visual objects [are chosen] that represent images that elicit contextually embedded cultural information, facilitating the investigation of concepts and symbols related to social structures such as power, social class, collectivism, relational support and conflict (Mkhize, 2004; Roos, Maine, & Khumalo, 2008; Strauss, 2005). (Roos, 2008, p. 660)

Through the visual presentations that are chosen to set off the discussion, "the cultural undertones that inform social life and people's ideas about it" become highlighted (2008, p. 660). What I would regard as significant about this research approach as potentially an *active* approach, is that it is based on the ontological underpinning that, as Roos, explains (2008, p. 660), "the social reality is continually created and re-created as a result of relationships with others in the social environment (Preskill & Torres, 1999)". The Mmogo-Method™, as I interpret it, is therefore a way of using visual objects to spark off discussion around "cultural undertones", with a view to those involved activating what Roos calls new "relationships with others in the social environment" (2008, p. 660).

Space in this book does not permit a detailed account of the Mmogo-Method™ which Roos sees as involving attempts to **"**produce community-based knowledge that is gathered and disseminated in a coherent way, thereby contributing to the possibility that Indigenous knowledge may form part of national and international discussions" (2008, p. 659). I mention the Mmogo-Method™ here as but one way of designing research that is based on "visual narrative"—that is, narratives sparked by people talking about visual objects. In regard to this way of facilitating research conversations, I would make the point that feedback from researchers and co-researchers as to how they experienced the use of the visual material in practice, and whether (and why) they found it helpful for developing community based knowledge and attendant actions, are important considerations to include in the doing, and writing up, of this kind of research.

10.9.2 Photovoice

In Chap. 4, I referred to the photovoice research initially developed by Wang along with Chinese counterparts (Wang & Burris, 1994; Wang, Burris, & Ping, 1996) in the context of rural China. Photovoice in this context involved providing rural farmers in China with cameras so that they could take pictures, construct narratives around the photos, and ideally "move" policy makers by using this medium to

express their concerns. According to Simmonds et al., photovoice has been used to good effect in China and in other contexts to enable people to reflect their concerns and passions, promote critical dialogues in communities (in interaction with the photos that are presented) and furthermore reach policy makers (2015, p. 35). Other researchers such as Catalani and Minkler (2010) have expressed that photovoice does not always perform these functions as planned. They point to a variety of mechanisms that need to be put in place in order for it to do so (see Chap. 4, footnote 10). Again I would suggest that attention should be given in the doing and writing up of this kind of research to the enactment of co-responsibilities of those involved, and to feedback (which should be sought) received from participants and others affected.

The same considerations apply to the use of "participatory video". Chilisa points out that this approach to research is credited to Snowdon (1967) "who worked with fishing communities on the Fogo islands to pioneer the use of media to enable a people-centred development approach" (2012, p. 240). Since then, she notes, participatory video has been used worldwide with purposes ranging from the following:

> Documenting the research process, monitoring community activities and interventions, sharing research findings from community to community, providing a healing and therapeutic environment to the disempowered or conducting a research process with a decolonizing focus. (2012, p. 240)

Chilisa considers that one of the strengths in the use of participatory video is that during the film making, "a dynamic process of community-led learning, sharing and exchange" is ideally set in motion (aided also by facilitators); in addition, "completed films can be used to promote awareness and exchange between groups"; and video films and messages can be used to "strengthen the communications with decision makers and with other communities" (2012, p. 241). Clearly, for Chilisa, such research has the potential to facilitate inquiries and exchanges in and across communities, with a focus being also on influencing/moving "decision-makers" (p. 241). In this regard I would suggest (as with photovoice) that in any methodological write-up, the mechanisms through which this has been pursued, including how co-responsibilities for influencing community quality of life have been assumed—along with feedback received—should be detailed.

10.9.3 Performance Theatre and Use of Poetry

In Chaps. 4 and 8, I referred briefly to Magnat's conception of performance theatre as one way of activating Indigenous modes of knowing, being, and doing. Magnat argues that performance theatre can be particularly suitable for enabling Indigenous ways of searching for knowledge, considered as a process whereby, in the words of Absolon (Minogiizhigokwe):

> we [those involved] journey, we search, we converse, we gather, we harvest, we make meaning, we do, we create, we transform, and we share what we know. (Minogiizhigokwe, 2011, p. 168, as cited by Magnat, 2014, p. 251)

Magnat questions what she calls the "dominant performance paradigm" in qualitative research, which she argues is not sufficiently attuned to "an ecological understanding of performance", where the focus is on a "material and embodied experience of spirituality" (2014, p. 241). She avers that Indigenous-directed participatory performance theatre can be used to invite people to participate in a "natural ecosystem regulated by energy flow and animated by a self-perpetuating and self-restoring form of life with a capacity for open-ended evolution" (2012, p. 244). The intent here is to *revitalize and enact* this conception of "energy flow and self-restoration" of ecosystems. Performance theatre as research is a way for people together to recognize (as Magnat cites Gergen, 2009, as also emphasizing) that

> whatever value we place upon ourselves and others, and whatever hope we may have for the future, depends upon the welfare of relationship [including relationship to the natural environment] (Gergen, 2009, p. 396, as cited by Magnat, 2014, p. 245).

Performance theatre can enable those participating to experience as equally sacred the relationships made with people and relationships made with the environment (2014, p. 249). Magnat argues that this way of participating in the knowing process (as embodied, and as spiritual) should not be regarded as unscholarly. She cites Absolon (Minogiizhigokwe), 2014, p. 167, who favorably mentions that "the academy is being pressured to create space for Indigenous forms of knowledge production" (2014, p. 251). Finley too (2017) summarizes that performative research methodology as she conceives it "is structured on the notion of possibility, the what might be, of a research tradition that is postcolonial, pluralistic, ethical, and transformative in positive ways" (http://icqi.org/home/plenaries/).

Again, as with the other research designs mentioned in this section, space does not permit a discussion of how various participants in participatory performance theatre might envisage their roles in such performances. But I would suggest that details of the enactment of co-responsibility for revitalizing relationship via acts of performance theatre, and details of how this is experienced by the actors/performers should become foregrounded in methodological write-up.

Readers might also have noticed how in Chap. 9, with reference to the arguments of Cram and Mertens (2015), I referred to the resuscitation of knowledge systems incorporated in, for example, poems, folklore, and dances. Dillard argues in this regard that the failure to appreciate the scholarly importance of such modes of inquiry is problematic, as these are important gifts of wisdom that are needed in "our increasingly troubled world today" (2014, p. 253). In regard to poetry, for instance, and its mode of (re)creating and (re)visioning, she argues, along with Lorde (1984) that we must be careful of drawing an "inherent conflict between theory and poetry" (2014, p. 253). She contends in particular that the power of African women's biographies and experiences expressed in poetic form, "embodies our theory" (2014, p. 254). She sees poetic expression as one way of dismantling,

so as to (re)build "a new house" (a new social and ecological way of being) by "gazing at phenomena in ways different" (2014, p. 255). And she argues that "used in coalition and solidarity with others, such naming and voicing [of new gazes] can serve as a source of power where truth and transformation might emerge for all" (2014, p. 263).

Dillard thus advocates that poetry—amongst other forms of expression of lives with a view to (re)visioning—should be recognized as making a contribution to scholarship and at the same time potentially to the emergence of (experienced) truth and transformation. The purpose of these forms of expression is to revitalize forms of knowing and being that can be experienced as forwarding wisdom in our "troubled world" (as an invitation for people to engage with, and find meaning in, the proffered expressions of wisdom).

10.10 A Concluding Note

Chilisa and Ntseane contend that in order to avoid reproducing dominant discourses in research literature, "when we read the literature and conduct research in formerly colonized countries such as those in the developing world" one of the questions we need to ask ourselves is: "Is the research 'action-oriented' and 'values-oriented'?" (2010, p. 260). I have suggested in this book that we can try to render research "action-oriented" (or what I call active) in *any* social context and can explicitly tie this pursuit to a recognition that researchers/co-researchers are, willy nilly, involved in forwarding values (which of course can be open to discussion in the research space). Questions in relation to the action- and value-orientation of research, foregrounded for consideration by Chilisa and Ntseane, can infuse all research "designs" as initiated and as they emerge innovatively in situ.

In response to the arguments of authors who insist that value-talk and advocacy of social and ecological concerns are not the proper domain of science, this book has tried to offer illustrative examples of how these can legitimately be brought into such a domain—as long as the criteria for good science/inquiry processes include a requirement that *inquirers take some responsibility for the emergence of "goodness" in the web of relations.* The terms "good inquiry processes" and "goodness" are of course open to interpretation and need to be "filled in" in situ as researchers (professional and others) go about the practice of their inquiries. Nonetheless, examples of such practices do serve as a resource for others reading the texts to then take into account.

I am of course hoping that all of the examples in this book and my deliberations in the various chapters around them can become a fruitful resource for those concerned with reflecting upon the meaning(s) of responsible research practice. I would hope that this book can be considered as also contributing to discussions around Cram and Mertens' question, which they ask as follows:

> How would research methodologies change if we started our work with the premise that we are all connected and that we have a responsibility for ethical work that is respectful of all who came before us, who are here with us (living and nonliving), and who will come after us? (Cram & Mertens, 2015, p. 96)

In this book I have suggested, with reference to detailed illustrations and discussions/extrapolations, how indeed such (co-)responsibilities might be enacted in the research process, and foregrounded in methodological write-up.

References

Belmont Report: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. (1979). *Ethical principles and guidelines for research involving human subjects*. Washington DC: Office of the Secretary, US Department of Health and Human Services.

Beyer, C. (2016, Winter). "Edmund Husserl". In E. N. Zalta (Ed.), *The Stanford encyclopedia of philosophy*. Retrieved September 7, 2016, at: https://plato.stanford.edu/archives/win2016/entries/husserl/.

Catalani, C., & Minkler, M. (2010). Photovoice: A review of the literature in health and public health. *Health Education and Behavior*, *37*(3), 424–451.

Chilisa, B. (2012). *Indigenous research methodologies*. London: Sage.

Chilisa, B., & Ntseane, G. (2010). Resisting dominant discourses: Implications of indigenous, African feminist theory and methods for gender and education research. *Gender and Education, 22*(6), 617–632.

Chilisa, B., Major, T. E., & Khudu-Petersen, K. (2017). Community engagement with a postcolonial, African-based relational paradigm. *Qualitative Research*, *13*, 1–14. https://doi.org/10.1177/1468794117696176.

Clandinin, D. J., & Connelly, F. M. (2000). *Narrative inquiry: Experience and story in qualitative research*. San Franscisco, CA: Jossey-Bass.

Clandinin, D. J., & Rosiek, G. (2007). Mapping a landscape of narrative inquiry: Borderland, spaces and tensions. In D. J. Clandinin (Ed.), *Handbook of narrative inquiry: Mapping a methodology* (pp. 35–72). London: Sage.

Cram, F., & Mertens, D. M. (2015). Transformative and Indigenous frameworks for multimethod and mixed methods research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 91–109). Oxford: Oxford University Press.

Creswell, J. W. (2003). *Research design: Qualitative, quantitative, and mixed methods approaches* (2nd ed.). Thousand Oaks: Sage.

Creswell, J. W. (2014). *Research design: Qualitative, quantitative, and mixed methods approaches* (4th ed.). Thousand Oaks, CA: Sage.

Denzin, N., & Giardina, M. (2010). Introduction. In N. Denzin & M. Giardina (Eds.), *Qualitative inquiry and human rights*, (pp. 13–41). Walnut Creek, CA : Left Coast Press.

Dick, B., Sankaran, S., Shaw, K., Kelly, J., Soar, J., Davies, A., et al. (2015). Value co-creation with stakeholders: Using action research as a metamethodology in a funded research project. *Project Management Journal, 46*(2), 36–46.

Dillard, C. B. (2014). (Re)Membering the grandmothers: Theorizing poetry to (re)think the purposes of black education and research. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry outside the academy* (pp. 253–267). Walnut Creek, CA: Left Coast Press.

Finley, S. (2017). *The future of critical arts-based research: Creating political spaces for resistance politics.* Keynote presentation given at the 13th international congress on qualitative

inquiry, 17–20 May, University of Illinois at Urbana-Champaign (http://icqi.org/home/plenaries/).
Gergen, K. J. (2009). *Relational being: Beyond self and community*. New York, NY: Oxford University Press.
Gergen, K. J. (2015). From mirroring to worldmaking: Research as future forming. *Journal for the Theory of Social Behaviour, 45*(3), 287–310.
Heidegger, M. (1962). *Being and time* (trans Macquarrie, J., & Robinson, E.). New York, NY: Harper & Row.
Hunter, A., & Brewer, J. (2015a). Designing multimethod research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 616–623). Oxford: Oxford University Press.
Hunter, A., & Brewer, J. (2015b). Conundrums of multimethod research. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 185–205). Oxford: Oxford University Press.
Husserl, E. (1913). *Ideas pertaining to a pure phenomenology and to a phenomenological philosophy: General introduction to a pure phenomenology* (trans: Kersten, F). The Hague: Nijhoff.
Kovach, M. (2009). *Indigenous methodologies: Characteristics, conversations, and contexts*. Toronto: University of Toronto Press.
Kuntz, A. M. (2015). *The responsible methodologist: Inquiry, truth-telling, and social justice*. Walnut Creek, CA: Left Coast Press.
Landsman, M. S. (2002). Across the lines: The phenomenology of cooperation between an Israeli psychologist and Palestinian mental health professionals. *Radical Psychology*, *3*(1), Art. 2. Retrieved September 10, 2016, at: http://www.radicalpsychology.org/vol3–1/MosheLandsman.html.
Lincoln, Y. S., & Guba, E. G. (2003). Paradigmatic controversies, contradictions, and emerging confluences. In N. K. Denzin & Y. S. Lincoln (Eds.), *The landscape of qualitative research: Theories and issues* (2nd ed., pp. 253–291). Thousand Oaks, CA: Sage.
Lincoln, Y. S., & Guba, E. G. (2013). *The constructivist credo*. Walnut Creek, CA: Left Coast Press.
Maathai, W. (2006). *Unbowed: A memoir*. New York, NY: Anchor Books.
Magnat, V. (2012). Performative approaches to interdisciplinary and cross cultural research. In L.-H. Smith & A. Narayan (Eds.), *Research beyond borders* (pp. 157–177). Plymouth, England: Lexington.
Magnat, V. (2014). Performance ethnography: Decolonizing research and pedagogy. In N. K. Denzin & M. D. Giardina (Eds.), *Qualitative inquiry outside the academy* (pp. 235–252). Walnut Creek, CA: Left Coast Press.
Mapotse, T. A. (2012). *The teaching practice of senior phase technology education teachers in selected schools of Limpopo province: An action research study*. Doctoral thesis, Pretoria: University of South Africa.
Mark, M. M. (2015). Mixed and multimethods in predominantly quantitative studies, especially experiments and quasi-experiments. In S. Hesse-Biber & R. B. Johnson (Eds.), *The Oxford handbook of multimethod and mixed methods research inquiry* (pp. 21–41). Oxford: Oxford University Press.
McIntyre-Mills, J. J. (2014a). *Systemic ethics and non-anthropocentric stewardship*. New York, NY: Springer.
McIntyre-Mills, J. J. (2014b). *Transformation from Wall Street to wellbeing*. New York, NY: Springer.
McIntyre-Mills, J. J. (2017). *Planetary passport: Representation, accountability and re-generation*. New York, NY: Springer.
McNiff, J. (2007). My story is my living educational theory. In D. J. Clandinin (Ed.), *Handbook of narrative inquiry: Mapping a methodology* (pp. 308–329). London: Sage.
Merriam, S. B. (2002). *Qualitative research in practice: Examples for discussion and analysis*. San Francisco, CA: Jossey-Bass.

Mertens, D. M. (2010). Transformative mixed methods research. *Qualitative Inquiry, 16*(6), 469–474.

Mertens, D. M. (2014). *Research and evaluation in education and psychology* (4th ed.). Thousand Oaks, CA: Sage.

Mertens, D. M., & McLaughlin, J. A. (2004). *Research and evaluation methods in special education*. Thousand Oaks, CA: Sage.

Mertens, D. M., Bazeley, P., Bowleg, L., Fielding, N., Maxwell, J., Molina-Azorin, J. F., et al. (2016). Thinking through a kaleidoscopic look into the future. *Journal of Mixed Methods Research, 10*(3), 221–227.

Minkler, M. (2004). Ethical challenges for the "outside" researcher in community-based participatory research. *Health Education and Behavior, 31*(6), 684–697.

Minkler, M. (2010). Linking science and policy through community-based participatory research to study and address health disparities. *American Journal of Public Health, 100*(Supplement 1), 81–87.

Moustakas, C. (1994). *Phenomenological research methods*. Thousand Oaks, CA: Sage.

Næss, H. E. (2016). Creating "the field": Glocality, relationality and transformativity. *Forum Qualitative Sozialforschung/Forum: Qualitative Social Research*, *17*(3), Art. 15.

Ngulube, P., & Ngulube, B. (2017). Application and contribution of hermeneutic and eidetic phenomenology to indigenous knowledge research. In P. Ngulube (Ed.), *Handbook of research on theoretical perspectives on Indigenous knowledge systems in developing countries* (pp. 127–155). Hershey, PA: IGI Global.

Oczak, M., & Niedźwieńska, A. (2007). Debriefing in deceptive research: A proposed new procedure. *Journal of Empirical Research on Human Research Ethics, 2*(3), 49–59.

Onwuegbuzie, A. J. (2012). Introduction: Putting the *mixed* back into quantitative and qualitative research in educational research and beyond: Moving toward the radical middle. *International Journal of Multiple Research Approaches, 6,* 192–219.

Pelias, R. J. (2004). *A methodology of the heart: Evoking academic and daily life*. Walnut Creek, CA: AltaMira.

Popper, K. R. (1959). *The logic of scientific discovery*. London: Hutchinson.

Popper, K. R. (1969). *Conjectures and refutations*. London: Routledge.

Reason, P., & Bradbury, H. (2001). Preface. In P. Reason & H. Bradbury (Eds.), *Handbook of action research: Participative inquiry and practice* (pp. xxiii-xxxi). London: Sage.

Roman-Alcalá, A. (2013). *Occupy the farm: A study of civil society tactics to cultivate commons and construct food sovereignty in the United States*. Paper presented for discussion at an international conference held at Yale University entitled Food Sovereignty: A Critical Dialogue, 14–15 September (Paper 75).

Roman-Alcalá, A. (2015). Broadening the land question in food sovereignty to northern settings: A case study of occupy the farm. *Globalizations, 12*(4), 545–558.

Romm, N. R. A. (1995). Knowing as intervention: Reflections on the application of systems ideas. *Systems Practice, 8*(2), 137–167.

Romm, N. R. A. (2001a). *Accountability in social research: Issues and debates*. New York, NY: Springer.

Romm, N. R. A. (2001b). Critical theoretical concerns in relation to development. In J. K. Coetzee, J. Graaff, F. Hendricks, & G. Wood (Eds.), *Development theory, policy and practice* (pp. 141–153). Cape Town: Oxford University Press.

Romm, N. R. A., & Hsu, C.-Y. (2002). Reconsidering the exploration of power distance: An active case study approach. *Omega, 30*(6), 403–414.

Romm, N. R. A., Nel, N. M., & Tlale, L. D. N. (2013). Active facilitation of focus groups: Exploring the implementation of inclusive education with research participants. *South African Journal of Education*, *33*(4), 1–14, Art. 811.

Roos, V. (2008). The Mmogo-method™: Discovering symbolic community interactions. *Journal of Psychology in Africa, 18*(4), 659–667.

Serper, A. (2010). *An analytical critique, deconstruction, and dialectical transformation and development of the living educational theory approach*. Doctoral thesis, Bath, England: University of Bath.

Simmonds, S., Roux, C., & Ter Avest, I. (2015). Blurring the boundaries between photovoice and narrative inquiry: A narrative-photovoice methodology for gender-based research. *International Journal of Qualitative Methods, 14*(3), 33–49.

Singapore statement on research integrity. (2010). Retrieved April 6, 2016, at: www.singaporestatement.org.

Stephens O. A. (2012). *Investigating effects of Six Thinking Hats and emotional intelligence training on creativity thinking and emotional intelligence of recidivists in Lagos State, Nigeria.* Doctoral dissertation, Lagos, Nigeria: Lagos State University.

Ssali, S. N., & Theobald, S. (2016). Using life histories to explore gendered experiences of conflict in Gulu District, Northern Uganda: Implications for post-conflict health reconstruction. *South African Review of Sociology*, *47*(1), 81–98.

Sung-Chan, P., Yuen-Tsang, A., Yadama, G. N., & Sze, A. (2008). Action research and social development in China: Creating synergy between marginalized populations and government officials. *Action research, 6*(2), 193–212.

Taylor, B. (2013). Introduction. In B. Taylor & K. Francis (Eds.), *Qualitative research in the health sciences: Methodologies, methods and processes*. New York, NY: Routledge.

Wang, C., & Burris, M. A. (1994). Empowerment through photo novella: Portraits of participation. *Health Education Quarterly, 21*(2), 171–186.

Wang, C., Burris, M. A., & Ping, X. Y. (1996). Chinese village women as visual anthropologists: A participatory approach to reaching policymakers. *Social Science and Medicine, 42*(10), 1391–1400.

Woldegies, B. D. (2014). *Economic empowerment through income generating activities and social mobilization: The case of married Amhara women of Wadla Woreda, North Wollo Zone, Ethiopia*. Doctoral thesis, Yellow Springs, Ohio: Antioch University.

Woldegies, B. D. (2016). Research highlighting options for gender empowerment in Amhara Regional State, Ethiopia. *South African Review of Sociology, 47*(1), 58–80.

Zhu, Z. (2011). After paradigm: Why mixing-methodology theorizing fails and how to make it work again. *Journal of the Operational Research Society, 62*(4), 784–798.

Index

A
Action research
participatory, 25, 36, 283, 343, 410, 434, 467, 500
Active research, 3, 25, 26, 37, 101, 343, 374, 467, 500, 501, 505
Autoethnography, 288–290, 298, 300, 468, 497, 502
Axiology, 8, 16–18, 24, 25, 313

B
Becoming, 9, 16, 35, 47, 61, 79, 123, 144, 164, 166, 218, 383, 426, 443, 481, 505
Belmont report, 36, 179, 359–361, 363–367, 378, 383–386, 388, 390, 393, 399, 400, 402, 410, 412, 413, 459, 503
Beneficence, 183, 359, 373, 384, 387, 390, 391, 394, 395, 400, 412, 413
Beneficiary, 458
Binary, 21, 28
Business as usual, 36, 253, 256, 259, 262, 267, 272, 277, 321, 351, 386

C
Capitalism, 55, 72, 275, 280, 351
Care, 9, 14, 53, 66, 67, 74, 85, 108, 115, 137, 139, 142, 145, 167, 187, 191–193, 224, 242, 243, 284, 362, 372, 400, 489
Case study, 184, 250, 277, 301, 486, 501
Causality, 120, 122, 151, 308, 315, 341, 342, 344, 347, 353, 485
Conflict, 160, 161, 186, 190–193, 219, 297, 372, 382, 442, 508
Connectedness, 6, 7, 24, 258, 271, 301, 395, 460
Consent, 104, 178, 243, 366–370, 373–379, 381, 382, 385, 388, 407–409, 411
Consequences
likely consequences, 226, 413, 420
Conversation, 3, 29, 31, 32, 35, 45, 47, 50, 55, 57, 61, 63, 68, 70, 73, 75, 80, 81, 85, 89, 120, 141, 142, 165, 185, 204, 265, 276, 279, 301, 345, 369, 374, 377, 383, 412, 429, 467, 471, 492, 501, 506
Correlation, 112, 145, 453, 485
Culture
culturally accountable, 33

D
Debriefing, 35, 203, 206, 207, 232–241, 351, 384, 464, 499
Decolonizing perspective, 343
Dialogue, 6, 21, 29, 89, 90, 99, 153, 167, 192, 258, 261, 286, 289, 313, 322, 335, 342, 347, 372, 373, 412, 449, 454, 507
Dignity, 17, 54, 66, 79, 88, 348, 377, 379–381, 412
Discourse, 2, 14, 31, 36, 112, 147, 167, 168, 174, 195, 197, 198, 206, 224, 228, 229, 261, 273, 279–281, 284, 313, 318, 325, 337, 339, 352, 367, 373, 374, 382, 389, 398, 419, 425, 468, 481, 489, 493, 509
Discrimination, 14, 17, 146, 159, 181, 196, 291, 319, 349, 372, 386, 458
Dissemination, 36, 105, 168, 175, 177, 181, 183, 311, 382
Dualism, 10, 275

© Springer International Publishing AG, part of Springer Nature 2018

N. R. A. Romm, *Responsible Research Practice*,
https://doi.org/10.1007/978-3-319-74386-8

E
Empirical, 16, 112, 181, 315–317, 323, 326, 328, 329, 339–342, 352, 423, 424, 469
Empowerment, 35, 74, 159–162, 166, 168, 170, 174, 179, 180, 182, 185, 186, 205, 299, 320, 370, 377, 380, 413, 457, 459, 461, 490, 495, 496
Emtic relationship, 90, 152, 394
Epistemology, 3, 8, 14, 17, 21, 22, 28, 308, 337, 339, 480
Ethics
  alternative ethics, 362, 372, 373
  ecofeminist ethic, 9
  ethics review committee, 36, 179, 359, 405
Ethnography, 249, 486, 495, 496
Experiment, 35, 203, 205–209, 216, 220, 221, 225, 227, 232, 234, 236, 237, 239–243, 315, 334, 350, 351, 384, 396–398, 462–464, 485, 498, 499, 503
Exploitation, 55, 79, 318, 393, 399

F
Facilitator, 60, 90, 99, 115, 131, 133, 152, 259, 330–332, 375, 494, 507
Feedback, 22, 92, 100, 101, 111, 115, 126, 128, 130, 134, 139, 140, 147, 149, 177, 189, 198, 207, 220, 227, 233, 238, 241, 257, 377, 385, 393, 395, 462, 492, 499, 504, 507
Feminism
  alternative descriptors (than feminism), 320
Focus group, 4, 20, 35, 45, 97, 103, 124, 131, 161, 170–175, 177, 185, 257, 318, 332, 333, 349, 391–393, 404, 429, 449, 451–454, 466, 500, 503
Food security, 36, 250, 277, 351, 501

G
Generalizability, 89, 184
Green Belt Movement (GBM), 36, 247, 251, 386, 473

H
Hope, 28, 61, 99, 120, 128, 130, 140, 149, 152, 205, 209, 213, 220, 224, 228, 232, 243, 258, 261, 274, 281, 290, 301, 320, 321, 344, 371, 374, 376, 384, 396, 423, 424, 446, 450, 464, 508, 509

I
Impact
  co-responsibility for, 419, 490, 508
  potential, 19, 419, 443, 480, 490
  reflection around, 80, 92, 331
Institutional Review Board (IRB), 36, 160, 359–364, 367, 375–377, 400, 402–405, 410, 411, 413
Interactive software, 301
Intervention, 6, 9, 10, 31, 86, 97, 102, 104, 105, 115, 123, 124, 130, 131, 135–138, 142, 145, 191, 211, 212, 224, 231, 321, 393, 394, 402, 445, 446, 453, 454, 482, 483, 496, 503, 507
Interview
  confrontational, 73
  empathetic, 73, 223
  as event, 65, 167, 277, 386

J
Justice
  conversations around, 88, 318
  ecological, 13, 24, 31, 35, 153, 276, 281, 309, 313, 399, 410, 487
  social, 1, 6, 12, 16, 17, 152, 183, 229, 269, 317, 328, 346, 362, 371, 400, 431, 435, 438, 441, 444, 470, 480

K
Knowing process, 9, 13, 313, 508

L
Life chances, 56, 66, 91, 102, 146, 167, 182, 203, 228, 265, 315, 316, 318–320, 342, 348, 349, 445, 455
Logic
  abductive, 328, 329, 336
  deductive, 325, 352
  dialectical, 148
  inductive, 114, 323, 327, 342, 352
  retroductive, 16, 307, 323–325, 338, 339, 342, 493

M
Marginalized social group, 379
Member checking, 70, 104, 134, 176, 177, 198, 430
Memory, 60, 76, 142, 232, 298, 384
Methodology
  engaged methodologist, 15, 16
  ethics and methodology, 7, 16, 18
  indigenous research methodology, 9, 18, 28, 34, 121, 151, 243
  philosophical underpinning of methodology, 29, 450, 485
  write-up of research methodology, 490
Mixed Methods Research (MMR), 16, 29, 420, 421, 422, 424, 428, 429, 434, 435, 439–442, 447, 450, 452, 453, 459, 461, 482, 493, 494

Miyo, 490, 492
Multiple and Mixed Methods Research (MMMR), 29, 37, 419–422, 427–430, 433, 434, 439, 440, 444–449, 451, 455–457, 462, 46, 465, 467, 469, 487, 490, 503

N
Narrative research, 46, 189, 484, 489

O
Ontology, 8, 11, 16–18, 28, 121, 123, 260, 443
Oppression, 14, 15, 27, 75, 90, 229, 315, 317, 348, 372, 436, 439

P
Paradigm
  constructivist, 14
  indigenous, 7, 27, 28, 30, 91, 229, 272, 363, 420, 435, 444, 447–449, 459, 470, 483, 505
  paradigmatic territory, 419, 433, 468–470
  permeable borders between paradigms, 438
  postpositivist, 6, 14
  pragmatic, 16, 309, 317, 323, 330, 336, 340, 469
  stretching of paradigmatic positions, 16, 28, 33, 34, 37, 419, 420, 439, 448, 449, 451, 470, 471, 480, 505
  transformative, 1, 4, 7–9, 12–14, 16, 17, 25, 27, 32, 34, 73, 112, 183, 196, 317, 371, 372, 433–435, 438, 444, 447, 460, 461, 472, 483, 504, 505
Participant researcher, 14, 34, 35, 90, 330, 451, 456
Participatory research, 25, 363, 371, 404, 500, 504
Performative effect, 150, 373, 413
Phenomenology
  phenomenological research, 482, 484, 486–488, 491, 492
Plurality, 325, 344, 345
Postmodern condition, 261, 347
Postmodernism, 347, 430
Power
  relations, 14, 92, 188, 194, 197, 265, 316, 353, 363, 456, 460, 496

Q
Quality criteria, 363
Questionnaire, 4, 35, 97, 98, 102, 103, 105, 106, 109–112, 114, 119, 122–131, 133, 135, 136, 138, 144–150, 152, 153, 208–211, 222, 223, 237, 238, 241, 271, 319, 333, 375–377, 393, 404, 446, 451–454, 462, 466, 487, 493, 494, 498–500, 503

R
Reciprocity, 17, 24, 45, 47, 58, 81, 149, 364, 376, 390, 392
Relationality, 18, 25, 32, 121, 394, 404, 413, 448, 460, 470, 487
Research facilitator, 90, 152, 249, 276, 300, 330–332
Research participation, 207, 228, 229, 243, 255, 269, 273, 329–331, 335, 347, 350, 352, 361, 366, 368, 371, 378, 379, 384, 394, 396, 400, 407, 412, 413, 426, 428, 431, 449, 457, 458, 460, 465, 466, 470, 488
Respect, 7, 9, 24, 46, 71, 74, 82, 88, 100, 104, 129, 145, 150, 172, 175, 179, 210, 227, 238, 267, 339, 340, 342, 359, 360, 364–366, 370, 371, 373, 375–377, 379, 380, 382, 383, 385–388, 391, 392, 400, 405, 409, 412, 479, 481
Responsibility of researchers
  collective responsibility, 153, 276, 487
  co-responsibility, 37, 419, 420, 457, 459, 479, 483, 488, 491, 492, 501, 502

S
Sampling
  active, 119, 148, 308, 331–333, 343, 352
  convenient, 257
  purposive, 333
  random, 102
  relational approach, 21, 121
  theoretical, 308, 330, 331, 343, 352
Science, 2, 3, 6, 9–11, 13, 15, 20, 37, 102, 113, 133, 134, 151, 152, 188, 204, 223, 224, 261, 269, 293, 309–312, 314, 315, 317, 323–325, 336, 340, 344, 347, 361–363, 388, 398, 420–427, 429–431, 442, 463–465, 480, 485, 498, 509
Singapore statement, 361, 425, 481, 503
Social Heisenberg effect, 10, 153, 480
Social mobilization, 159–162, 171, 176, 181
Spirituality, 27, 30–32, 137, 194, 276, 297, 340, 352, 466, 508
Stakeholder, 173–176, 178, 179, 181, 182, 184, 192, 193, 198, 224, 258, 319, 321, 330, 331, 333, 350, 370, 371, 378, 380, 395, 402, 410, 413, 420, 431, 433, 444, 447, 448, 450–453, 457, 460, 463, 480, 493, 494, 497, 500, 504
Story, 1, 9, 36, 46, 49, 54–56, 58, 65, 66, 72, 78, 83, 87, 88, 91, 152, 164–166, 184, 187, 189, 194, 233, 234, 271, 272, 280,

285, 286, 288–291, 296–298, 322, 335, 367, 381, 386, 392, 406, 411, 437, 468, 479, 491, 492, 497, 499, 502
Storying, 1, 37, 45, 54, 63, 68, 71, 73, 91, 152, 189, 194, 252, 283, 290, 294, 302, 374, 381, 393, 396, 468, 479, 491–493, 495, 497–502
Structural violence, 52, 229, 372
Survey, 121, 127–130, 151, 239, 318, 325, 333, 345, 421, 437, 482, 484, 485, 493, 500, 503
Systemic, 1, 6, 7, 9, 10, 105, 139, 144–147, 151, 193, 258, 274, 275, 319, 321, 431

T
Talking circle, 35, 46, 373, 391, 491
Testimony, 69, 139
Theorizing
  deficit, 167
  forward-looking, 15, 45, 106, 150, 240, 258, 352, 458
  generative, 36, 47, 55, 67, 68, 91, 92, 120, 146, 163, 168, 183, 185, 195, 228, 230, 231, 239–241, 243, 258, 273, 282, 284, 297, 299–302, 307, 308, 310, 314, 315, 320–322, 325, 327, 335, 341, 346, 350–353, 374, 375, 380, 396, 397, 420, 450, 463, 496, 499
  grounded, 36, 308, 327–331, 335–337, 343, 353, 486
  middle range, 315, 316, 327, 352
Theory, 4, 10, 12, 26, 67, 91, 120, 122, 166, 184, 195, 228, 230, 238, 273, 282, 288, 290, 299, 308–312, 314–320, 322–331, 333, 334, 336–338, 341, 350, 352, 353, 382, 482, 484, 486, 508
Transferability, 184, 231
Transformative agenda, 145, 261, 308, 402, 470
Truth, 6, 13, 14, 22, 23, 76, 89–91, 152, 177, 191, 261, 270, 284, 312, 324, 336, 344–347, 401, 422, 424, 425, 427, 428, 431, 455, 496, 509

U
Ubuntu, 4, 7, 29, 69

V
Validation, 19, 26, 90, 163, 183, 192, 193, 225, 270, 271, 282–284, 298, 430, 449, 450, 499
Validity
  catalytic, 23, 91, 181, 185, 193, 225, 299
Values, 1, 11, 21, 24, 28, 29, 54, 72, 88, 89, 101, 153, 164, 217, 258, 289, 316, 337, 339, 340, 361, 364, 365, 390, 430, 495, 509

W
Web of connections, 18, 19, 145
Wellbeing, 17–19, 24, 30, 31, 67, 145, 165, 179, 183, 187, 197, 216, 225, 247, 248, 250, 253, 256, 259–262, 269, 270, 273, 274, 276, 301, 317, 321, 335, 379, 386, 387, 390, 397–399, 402, 404, 445, 466, 467, 480, 500, 501