Doing Research in Applied Linguistics

Doing Research in Applied Linguistics: Realities, dilemmas, and solutions provides insight and guidance for those undertaking research, and shows the reader how to deal with the challenges of this research involving real people in real settings. Featuring over twenty chapters by experienced and up-and-coming researchers from around the world, this book:

- outlines the steps involved in solving the problem and completing a successful, and publishable, project;
- provides case studies of obstacles faced at each stage of research, from preliminary planning to report writing;
- addresses issues of validity and reliability during data collection and analysis;
- discusses ethical issues in research dealing with vulnerable groups including children, refugees, and students;
- includes examples from longitudinal studies, and both qualitative and quantitative research.

Doing Research in Applied Linguistics is essential reading for students studying research methods, or for those embarking on their first research project in applied linguistics or language education.

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Doing Research in Applied Linguistics

Realities, dilemmas, and solutions

Edited by Jim McKinley and Heath Rose
This book is dedicated to Rachel Hanemann, for her careful work as copy-editor; to Paul McKenzie, for his constant encouragement throughout the project; to our editors at Ashgate and Routledge; and to the committee and members of the BSA Sociology of Religion Study Group, for supporting the conference at which it all began.
Contents

List of figures xi
List of tables xii
List of contributors xiii
Foreword xix

Editorial introduction 1

1 Realities of doing research in applied linguistics 3
HEATH ROSE AND JIM MCKINLEY

PART I
Responding to problems in the research planning stage 15

2 Studying up, down, or across?: Selecting who to research 17
RYUKO KUBOTA

3 Responding to theoretical shifts in research design 27
HEATH ROSE

4 Overcoming problematic positionality and researcher objectivity 37
JIM MCKINLEY

5 Doing collaborative research: Challenges and opportunities 47
DANIEL V. BOMMARITO, PAUL KEI MATSUDA, JIANING LIU, JUVAL V. RACELIS, TAIMIN TAMMY WU, JING XIA, AND YUCHING JILL YANG
PART II
Responding to problems during data collection  

6 Adjusting to contextual constraints: Methodological shifts in local research projects  
JOHN HEDGCOCK AND HEEKYEONG LEE  

7 Dealing with participant attrition in longitudinal studies  
CORINNE A. SEALS  

8 Dealing with low response rates in quantitative studies  
AVERIL COXHEAD  

9 Dealing with multilingualism in quantitative research  
GESSICA DE ANGELIS  

PART III
Researching vulnerable groups  

10 It isn’t child’s play: Conducting research with children as participants  
VICTORIA MURPHY AND ERNESTO MACARO  

11 Conducting longitudinal fieldwork among adult refugees  
LORNA CARSON  

12 Researching people with illnesses and disabilities: Ethical dimensions  
HANAKO OKADA  

13 Conducting research with deaf sign language users  
LORRAINE LEESON, JEMINA NAPIER, ROBERT SKINNER, TERESA LYNCH, LUCIA VENTURI, AND HAARIS SHEIKH  

14 Researching your own students: Negotiating the dual practitioner – researcher role  
NICOLA GALLOWAY
PART IV
Responding to problems during data analysis 157

15 Dealing with longitudinal quantitative designs and data analysis 159
AEK PHAKITI

16 Managing researcher dilemmas in narrative interview
data and analysis 172
MATTHEW T. PRIOR

17 Grappling with originality and grounding in qualitative
data analysis 182
JESSICA G. BRIGGS

18 Negotiating the relationship between theory and practice
in the fields of literacy and language 192
CONSTANT LEUNG AND BRIAN STREET

PART V
Responding to problems in the reporting of research 201

19 Dealing with controversial findings 203
ROSLYN APPLEBY

20 Reporting on politically sensitive issues: The case of
telling the truth about early L2 instruction 214
DAVID SINGLETON AND SIMONE E. PFENNINGER

21 Dealing with criticism when publishing qualitative research 225
XUESONG GAO

22 Representing the self honestly in published research 235
CHRISTINE PEARSON CASANAVE

23 Publishing from a dissertation: A book or articles? 244
BRIAN PALTRIDGE
Contents

Afterword: Strategies for getting the study published 253
ANDREW D. COHEN

Index 258
Figures

3.1 The theoretical framework 32
5.1 The multiple layers of the research project 49
6.1 Original explanatory-sequential research plan 64
6.2 Revised explanatory-sequential research plan 66
13.1 Our research ecosystem 135
13.2 Screenshots from the Justisigns website and the LifeinLincs VLOG 142
14.1 The ongoing cyclical process of classroom-based research 148
15.1 A flow chart of data-gathering procedures (adapted from Phakiti, 2007, p. 69) 164
15.2 A flow chart of SEM used in this study (adapted from Phakiti, 2007, p. 83) 165
15.3 The relationship of state and trait strategy use to EFL reading test performance over time (based on Phakiti, 2007, Figure 5.6, p. 113) 166
17.1 Extended model of influencing factors in strategic behaviour in informal L2 contact (Briggs, 2016) 188
### Tables

<table>
<thead>
<tr>
<th>6.1</th>
<th>Early phases of the action research sequence (based on Burns, 2015, p. 189)</th>
<th>63</th>
</tr>
</thead>
<tbody>
<tr>
<td>23.1</td>
<td>Table of contents for the dissertation (Paltridge, 1993a) and the book (Paltridge, 1997)</td>
<td>248</td>
</tr>
</tbody>
</table>
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Having served for many years as editor of the *TESOL Quarterly*, I am pleased to write this foreword. Published articles in applied linguistics serve a particular function in scholarly work. They report on findings that develop from the implementation of rigorous methods designed to examine real-world problems. In the process of reporting on these findings, it is essential that such accounts include a thorough description of the methods used in a study. However, because of the stringent length requirements of many journal articles, there is little room for a description of all the unforeseen circumstances faced by the researcher in the process of undertaking the study. Yet, as is demonstrated in this book, researchers typically face challenges in implementing their investigations and, in the process, need to make balanced decisions regarding how to handle the obstacles.

In the editorial introduction, Rose and McKinley point out that “in the presentation of published research as the ‘ideal,’ the reader is often made oblivious to the methodological journey of the project and of the compromises made along the way.” The important contribution of this book is that it documents the “methodological journey” made by most researchers as they implement their studies.

There are indeed many parallels between a journey and research. Both have an imagined view of what the final destination will be like. The ideal vacation destination has everything the traveler desires from memorable sights to perfect accommodations and climate. For the researcher, this imagined destination includes definitive results, groundbreaking findings, and original insights. Yet for both the traveler and the researcher, these ideal destinations are rarely attained.

In addition, for both travelers and researchers, there are countless twists and turns as they make their way to their final destinations. For the traveler, it is the airport lines, the delays, the missed connections, the poor accommodations, the strange customs, and so on. For the researcher, the detours entail gaining access to the desired site, getting consent forms from participants, having participants drop out of the study, not being able to collect the desired amount of data, and so on.
What is critical for both travelers and researchers is to make reasonable moment-by-moment decisions that will enable them to reach their final destinations while maintaining their standards. However, in both cases, compromises need to be made. The strength of this book is that it documents such compromises and, most importantly, shows that these compromises are an unavoidable part of the research process. This is an essential insight for novice researchers, who, based on the research books they read, are likely to believe that research is a clearly defined process that includes developing a research design, selecting a site, gaining access, collecting data, analyzing data, and reporting findings. The chapters in this book demonstrate that this is generally not the case. Instead, the book illustrates that in each of these steps compromises need to be made.

Whereas other research method books do make an occasional reference to the possible compromises researchers need to make, as the editors point out, this is often preventative advice rather than curative steps in which the researchers must deal with what they have, not what they had hoped for. These curative steps involve difficult decisions regarding the most effective way to address the problems while undertaking sound research.

Having undertaken my own research, written and taught about research methods, and served as an editor, I see this book as a critical contribution to research and publishing in applied linguistics in which what is portrayed is rarely what actually happens. The editors and authors are to be thanked for providing us with honest accounts of the struggles they faced as they investigated the real and messy problems of the real world.

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Editorial introduction
When novice researchers are taught about research methods, they are usually only ever exposed to ‘ideal’ research designs, where all avenues of validity, reliability, generalizability, and ethicality have been carefully contemplated and accounted for. When beginning their data collection, however, things may not go according to these perfect plans: they cannot get the representative sample they were hoping for, participants drop out from their longitudinal study, an institution decides not to grant them access to their planned research context, working within a vulnerable community proves more difficult than expected, or they struggle to maintain a positivist, objective stance in an area of research in which they have invested considerable personal and emotional energy. There are innumerable ways in which a research design faces obstacles in the research process, no matter how carefully a project was planned. In these situations, a researcher may be left wondering how to salvage the project from failure.

The idealistic nature of research methods books

When referring to research methods literature, a researcher concerned about his or her project’s feasibility finds little comfort in the ideological nature in which many research methods textbooks are written. The wording of many methodology textbooks can be interpreted to suggest that nothing other than the most stringent research design will stand up to the rigor of academic scrutiny. In these books, each research method, data collection instrument, and analytical tool is meticulously presented with a long list of threats to validity and reliability if not carried out to the exact plan. For example, the dangers of a low response rate to questionnaires is a topic often covered in such books, and this is usually presented alongside advice to help improve response rates. Dörnyei (2007, p. 113–114), for example, offers helpful advice for researchers to effectively administer the questionnaire to improve the response rate. However, there is little advice offered for situations when a low response rate occurs in a project, despite best efforts
in administration. A novice researcher in this case could easily be left feeling that his or her project has failed.

Mackey and Gass (2005) discuss the logistical issues of classroom-based research in terms of obtaining quality audio recordings for data analysis. When taking into account the issues of human interference, classroom movement, and learner interest in the equipment, the authors’ solutions include multiple microphones, video-recording equipment, movable recording equipment, and bringing in the equipment weeks before the actual observation. Such suggestions, while useful in preempting obstacles, turn a one-class observation design into a multiple-week-long, audio-visual production. For novice researchers, such intrusion might not be logistically feasible, and compromises have to be made. There is little suggestion for researchers to work around such logistical issues to, for example, deal with poor-quality audio if other unforeseen factors intrude on the observation day.

It is important to note, however, that Dörnyei (2007) does suggest a pragmatic approach to carrying out a research project, albeit in a brief manner. He writes,

> It is my impression that researchers are often ashamed of the compromises that they need to make, not realizing that making compromises is part and parcel of being a researcher.

(p. 309)

We fully concur with Dörnyei’s assessment here, particularly in the context of novice researchers who are easily led to believe only the most perfectly planned and executed research project is acceptable in the field. This book aims to expand on this notion in its presentation of research projects, which had to overcome obstacles and make compromises in order to achieve successful results.

Mackey and Gass (2005) also emphasize the nature of classroom-based research as a “particularly complex and multifaceted endeavor that must be planned carefully” (p. 212). They do offer helpful, practical advice in the creation of contingency plans when things go wrong, but our book expands on this preventative advice in its offer of curative advice for situations when methodological issues do arise and must be dealt with during the research process. We would also argue that Mackey and Gass’s depiction of the classroom as complex and multifaceted can be expanded into other contexts where applied linguistics researchers collect data, such as workplace environments and social spheres – indeed any research site that exists in the messy real world.

**The idealistic nature of published research**

Published research further perpetuates a stereotype that obstacles in research are anomalies, rather than the norm. Journal articles often document their research designs with scientific precision, and a reader may be led to believe
data were collected and analyzed with few problems in the process. Limitations of a project are always discussed, of course, but they are presented in a way that the reader would believe that the researcher was aware of these limitations from the outset of planning and had accounted for them before data were collected and analyzed. If a researcher had planned on collecting 500 samples, but managed only 250, the original target may not ever need to be disclosed, but instead there might be a throwaway statement in the conclusion of the paper, which stated that because of the small sample size, further research might be needed. If a researcher were denied access to his or her prime research site, the paper might not allude to the fact that the data collection site was the researcher’s second choice. More likely, the paper would justify the new site according to slightly altered criteria. Likewise, multiple case studies become single case studies in the write-up process, or the importance of certain research instruments in the research design is emphasized or downplayed according to the perceived value of the data that was yielded. Researchers are not being deceitful, but rather may wash over the minor details of methodological issues in projects for fear that it will detract from the data they were able to collect and report on. In the presentation of published research as the ‘ideal,’ the reader is often made oblivious to the methodological journey of the project and of the compromises made along the way. All of these alternatives are certainly reasonable solutions to immediate problems, but too often they go unreported.

In Marshall and Rossman’s popular *Qualitative Research Methods*, now in its fifth edition (Marshall & Rossman, 2010), journal articles are described as “pristine and logical,” in contrast to real research, described as “confusing, messy, intensely frustrating, and fundamentally nonlinear” (p. 55). They draw our attention to some insightful advice provided for doctoral students 35 years ago by Bargar and Duncan (1982) in which they describe how, “through such highly standardized reporting practices, scientists inadvertently hide from view the real inner drama of their work, with its intuitive base, its halting time-line, and its extensive recycling of concepts and perspectives” (p. 2). This kind of attention to the dramas of research is unsurprisingly something doctoral students are advised to avoid. But certainly, this purposeful imbalance of focus comes at a cost for future researchers.

Furthermore, published research almost always focuses on the implications of the findings of the study, and almost never discusses the methodological implications of the research process itself. It is our conviction that the methodological implications of any study play an equally important role in shaping our understanding of research in the field of applied linguistics as the content-related findings do. They help to shape our understanding of the project and build upon it when carrying out future research in the area.

This book aims to rectify the imbalance in research methodology literature through exposing the research design and implementation obstacles
that applied linguists and educational researchers face in many of our research projects. The projects discussed in this book were all carried out by experienced, respected academics in their fields. The projects outlined in these chapters all resulted in published research papers, despite the obstacles encountered along the way in the research process. In this book, the projects are presented with a shift in focus from their original publication on the content of their findings to the methodological implications of the study. By bringing the methodological obstacles to the forefront, we can better build an understanding of best practices in overcoming similar research problems in the future.

**A focus on applied linguistics and language education research**

The book focuses on applied linguistics and language education research because these fields occupy a shared space in academia and thus encounter similar problems in the research process. Applied linguists and educational researchers often deal with the ‘real world’ rather than sanitized environments. Many of the methodological issues encountered by applied linguistics researchers very much stem from the fact that applied linguistics is, in itself, a problem-based discipline. We concur with Grabe (2010, p. 35) who argues,

> The notion that applied linguistics is driven first by real-world language problems rather than by theoretical explorations of internalized language knowledge and (L1) language development is largely what set the field apart from both formal linguistics and later from sociolinguistics, with its own emphasis on language description of social variation in language use.

While the exploration of real-world research problems has obvious benefits in its practical implications, the real world is messy, and the potential for something to go wrong increases exponentially with each added uncontrollable variable. A sudden school assembly can ruin a planned classroom observation and result in the object of the research being skipped completely in the curriculum. The end-of-term essay that was going to become usable data for text analysis might suddenly be replaced by a presentation. A company that had agreed to distribute your questionnaire to its employees might suddenly undergo restructuring, and the offer to distribute your questionnaire might easily slip through the cracks. Most research in educational contexts in particular is “pretty messy, or at least complicated” (McArthur, 2012, p. 428), thus when things go wrong, it is to be expected.

Applied linguistics and educational researchers also often deal with people, which can be the messiest part of real-world research. A common cold
can decimate student numbers on the day of important classroom-based research. An overly controlling and chatty group member can destroy a speech sample intended for discourse analysis. A gatekeeper to an important research site can simply decide to exercise his or her right to not take part in your study, simply because the person cannot be bothered dealing with the paperwork or organizing to meet with you. There are immeasurable ways in which a carefully planned research project can go awry in the real world, and these are all too often glossed over in published papers. They lie in the periphery of research methods books, despite being situated as real obstacles at the forefront of the research process.

Other researchers in the past have focused on the messiness of social research, and the current volume builds on the foundations of such work. Mellor (2001) details a very frank account of his practitioner-based research project in the field of educational psychology, providing an ‘honesty trail’ of the problems encountered and mistakes made. He argues that the provision of this honest account formed an essential part of his study’s strength, even if by conventional definitions highlighting these issues posed a threat to the project’s validity. A similar perspective can be found in Cook (2009, p. 290), who argues,

If an indicator of our successful work as action researchers is the integration of the development of practice with the construction of research knowledge, then we must provide honest accounts of that process and incorporate mess as an integral part of a rigorous approach.

We would concur with Cook’s assessment, but argue that honesty in incorporating mess as a part of rigorous practice be extended beyond just action research, as it is an integral part of social science research in general, thus also extending to much applied linguistics research. This book aims to expand on such work and organize it in a central volume. The strength of many voices will, hopefully, showcase the realities of social science research that others have touched on in published research papers.

**An overview of the book**

This book aims at bringing problems in applied linguistics research to the forefront in its presentation of research projects by experienced, established researchers, and up-and-coming researchers alike. Accordingly, each chapter focuses on one isolated problem, or area of research, and uses a real case study of published research to illustrate how the problem was circumvented.

The obstacles outlined in this book are presented in five main sections. Each part discusses a shared position in the research process, starting with the planning stage then moving into data collection, analysis, and reporting.
Section one: Research planning

The first section examines obstacles in research planning, which include deciding who to research, dealing with shifts in theory in the field mid-project, positioning oneself as an outside researcher and negotiating complex and collaborative projects.

Ryuko Kubota takes on the challenge of selecting who to study with a particular focus on the ethical issues and politics of researching privileged populations, i.e., “studying up,” populations that include the researcher, i.e., “studying across,” or underprivileged populations, i.e., “studying down.” Drawing on her experience working with disadvantaged migrants, and in consideration of neoliberal ideologies, Kubota found herself faced with “moral discomfort” coming from an elitist position in relation to her potential participants. To overcome this obstacle, Kubota changed her target population and took advantage of establishing rapport with the participants.

Next is Heath Rose’s response to dealing with theoretical shifts in research design – reflecting on the nature of applied linguistics research that can change drastically in a short period of time. As a researcher on a project in which changes in research paradigms meant the conceptual framework was suddenly found to be obsolete, he was faced with the option of ignoring the changes altogether, or integrating them into his research design. Rose shows an analysis of the options as an important part of the process in overcoming such an obstacle and sustaining the research project.

In consideration of researcher positionality, Jim McKinley deals with the problem of approaching a research context as an outsider faced with the challenge of choosing between developing a situated qualitative analysis or establishing researcher objectivity, both of which were quickly dismissed as viable options. Instead, McKinley managed to overcome the problem by moving beyond positivism and adopting constructionist theory to allow his position as an outsider to inform the research rather than invalidate it.

The last chapter for this first section of the book, led by Daniel V. Bommarito, explores the issues related to negotiating multiple objectives in collaborative research projects. The chapter, which is based on a project led by Paul Kei Matsuda, is presented along with a group of prior and current doctoral students including Jianing Liu, Juval V. Racelis, Taimin Wu, Jing Xia, and Yuching Jill Yang. The chapter deals with a multilayered research project, with a student-led collaborative study at the core, an observation-based study conducted by Bommarito as a secondary objective, and, finally, a third layer that was Matsuda’s objective of an academic professional development opportunity for his students. With this complex research structure, obstacles were expected, but these researchers show that careful planning, flexibility, and open communication allow for different objectives in collaborative projects to be achieved.
Section two: Data collection

The second section outlines problems that arise in the data collection phase of a research project. Problems in this section include dealing with participant attrition in longitudinal studies and low response rates in qualitative and quantitative studies.

The first chapter in this section looks at adjusting to contextual constraints when there are methodological shifts in a research project. John Hedgcock and Heekyeong Lee take on the problem of when the research design falls short of what it was intended to achieve. They describe their experience of adopting alternative data collection methods and strategies in order to maintain their original research focus and questions. The decision required reformulation of subsequent phases of the research process, and Hedgcock and Lee show how strategies to respond to the need to make changes during data collection, even reconceptualizing the research problem, can lead to achieving the research aims.

Next, on the significant problem of participant attrition in longitudinal ethnographic studies, Corinne Seals manoeuvres her way through various issues such as location access and participant commitment, arriving at some important, harsh realizations about the realities of such studies. Seals draws on her experience of utter devastation at the loss of key participants in her study, pointing to creative and flexible approaches that allowed her to actually find an advantage in what would have otherwise been project-ending challenges.

A major problem in quantitative data collection is a low response rate. Averil Coxhead shares her own experiences dealing with this difficult challenge, overcoming it, and going on to publish numerous articles from the study. She explains that dealing with such challenges was familiar, as in the development of her academic word list, gaining access to particular texts caused delays. While her solutions to the low response rate involved a longer timeline and additional recruitment, Coxhead shows that such strategies can effectively sustain the research project.

Completing this section is an insightful chapter on dealing with multilingualism in quantitative research. As recognition of the value of new perspectives on multilingualism and crosslinguistic influence increases, this chapter by Gessica De Angelis is timely, as she describes the obstacles faced in such complex research paradigms and describes her experiences in overcoming those obstacles. She provides important strategies for conducting research with multilingual individuals while preventing a subject selection bias in the design and avoiding such pitfalls as the inconsistent treatment of data in such quantitative studies.

Section three: Researching vulnerable groups

The third section explores particular issues that arise in applied linguistics and education, focusing on vulnerable groups with whom we often work. While we acknowledge many members of these groups may not
think of themselves as ‘vulnerable,’ the fact remains that dealing with certain research populations can bring ethical and logistical challenges to the data collection process. Contexts discussed in this section include working with children, conducting ethnographic research with refugees, exploring learners with disabilities or illnesses, researching within deaf communities, and using one’s own students as research participants.

Opening this section is Victoria Murphy and Ernesto Macaro’s chapter on researching children. Based on their experience conducting a recent study, they highlight seven obstacles they faced and offer solutions for overcoming them. The obstacles focus on research assistants, sampling frames, research sites, recruitment and informed consent, data collection spaces, participant attrition, and feedback to parents and teachers. Murphy and Macaro explain that this is not an exhaustive list, but their review of challenges shows that careful consideration and implementation of studies with children can and do result in successful research projects.

On conducting fieldwork among adult refugees, Lorna Carson provides a very honest reflection on what it means to work with participants who often face difficult and uncertain circumstances. The refugee learners in her doctoral research saw their English language education very differently, from essential to their survival to simply a way to pass the time. Such uncertainty and inconsistency in the stakes involved presented a number of challenges – particularly informed consent, confidentiality, and disclosure of personal details – that Carson negotiated with appropriate sensitivity in order to bring the research project to successful completion.

When researching a population with illnesses and disabilities, certain struggles are expected, but meeting the expectations of such participants in applied linguistics research can be especially challenging to negotiate. In the next chapter, Hanako Okada draws from her experiences in conducting a narrative inquiry to examine how those with illnesses and disabilities interpret and deal with their experiences. The participants in her study held very different reasons for participating in the study than those of the researcher. Okada explains how such research can lead to significant ethical concerns. She goes on to show that responding to the problems and pressures throughout the research process by taking an approach of moral responsibility can be a solution.

In deaf studies research, challenges such as gaining ethics approval, securing funding, collecting data, and disseminating results to a diverse group of stakeholders are of significant concern. In addition to these, political concerns are particularly significant and are a central focus in the next chapter by deaf studies researchers Lorraine Leeson, Jemina Napier, Robert Skinner, Teresa Lynch, Lucia Venturi, and Haaris Sheikh. In the chapter, five challenges are highlighted in order to target the main obstacles when researching signing communities and sign language interpreting communities. Through an examination of a recent research project, this dynamic research team
shares how to overcome the obstacles when engaging national and regional police forces as well as judges, social workers, lawyers, and others who play a role in facilitating access to justice for deaf people in what are bilingual, bimodal, and interculturally negotiated interpreted settings.

The final chapter in this section deals with the apprehensions surrounding researching our own students. Nicola Galloway examines a number of concerns for teacher-researchers such as potentially disrupting class atmosphere and influencing the outcome of the study in teacher research, classroom inquiry, or action research. She offers reflections on her own research experience negotiating these challenges, showing that through careful workload management, and an acute awareness of ethical issues as well as threats to reliability and validity, teacher-researchers can succeed in their dual roles.

Section four: Data analysis

The fourth section considers problems that arise in the data analysis phase of a research project. Problems in this section include negotiating the relationship between theory and empirical research, managing researcher dilemmas in narrative interview data and analysis, dealing with designs and data analysis in longitudinal quantitative research, and grappling with originality and grounding in qualitative data analysis.

In longitudinal quantitative research, change is not only inevitable, it is often precisely what we seek to examine. In this section’s opening chapter, Aek Phakiti reflects on his research involving the assessment of changes and levels of stability over time in strategic competence. Because longitudinal quantitative research requires the matching of two or more data sets, correlations, analysis of variance, or a loss of data can present serious consequences. Phakiti offers seven significant suggestions for researchers for overcoming the obstacles, from understanding the stages of longitudinal research, to clarifying a defensible rationale, to resisting the desire to make others accept the significance of the research simply by its longitudinal process.

Next, in exploring the dilemmatic issues of interaction, representation, and emotionality in his own narrative interview data, Matthew Prior draws our attention to the importance of perspective on these potential obstacles in such qualitative research. While analysis of narrative interview data may seem to unavoidably present what may be described as ‘tedious’ layers to a study, Prior found that avoiding them is not the solution. Instead, we understand through his experience that analysis of such data is in fact a creative activity that requires conscientious navigation and practice working through the dilemmas that are not only natural parts of the process but also opportunities for awareness raising and development as a researcher.

The quest for originality in empirical research is spotlighted as a significant obstacle in quantitative data analysis. Jessica Briggs returns to her doctoral
research, which involved analyzing data generated by a self-developed research tool to provide an example of a daunting experience in such a quest. She notes that the limited grounding data from previous studies means guidance is limited when it comes to overcoming the obstacles in achieving originality. Briggs recommends that when results do not fit neatly into existing theoretical or conceptual frameworks, rather than doubting the methodological approach, look instead for those phenomena in the results that are unique to the study and seek a balance that allows the results to challenge and advance existing models.

Constant Leung and Brian Street complete this section by taking on issues related to the unclear relationship between theory and research practice in the academic literacies approach. In their research involving ethnically, linguistically, and socially diverse student participant populations, Leung and Street highlight a number of key arguments as ways to overcome the obstacles, focusing particularly on the need for shared understanding of the relationship between teachers and learners, as well as a clear sense of the role of subject content in that understanding.

Section five: Reporting research

The final section outlines problems that arise in the report stage of a research project, including dealing with missing data, writing the doctoral dissertation, and overcoming rejection when publishing.

Reporting controversial findings from interviews in studies that explore personal feelings or attitudes can present particularly challenging obstacles. Participants’ responses in such studies can be confronting, unpleasant, and downright offensive to the researcher, as well as future readers of related research output. Roslyn Appleby’s research deals with responses from participants that can, at times, be perceived as racist, sexist, emotionally charged, or taboo. Appleby offers three insightful recommendations for overcoming these obstacles, including building rapport with participants, avoiding the adoption of common-sense judgments, and remembering that responses reflect the participants’ greater communities rather than individual thoughts.

Knowledge of the end state of foreign language instruction is of great value for those involved in language education research, since such research has important implications for multilingual education. David Singleton and Simone Pfenninger reflect on their experience researching early L2 instruction in their chapter, which takes on the challenges of honest reporting on politically sensitive issues, such as negative results in early L2 instruction that reject deeply ingrained assumptions such as “younger equals better.”

In the next chapter, Xuesong Gao opens up as he delves into the painful reality of dealing with criticism in the review stages of publishing research in applied linguistics. As both a reviewer and author, Gao points out two
common problems for qualitative researchers. One is the difficulty of framing a study that is absent of a significant knowledge gap in the relevant disciplinary field. The other is the general vagueness of methodological details where qualitative researchers seem to falter. Through a very honest reflection on his own experience receiving harsh criticisms from reviewers, Gao shows how he turned those criticisms into valuable learning opportunities and provides some strategies to persuade reviewers by dealing with the two common problems.

On finding a way to honestly present oneself in published applied linguistics research, Christine Pearson Casanave uses her trialling experience over a three-year period of writing an article about learning a foreign language that was eventually published in *TESOL Quarterly*. Casanave’s exposing and honest reflection on the experience offers valuable insight into what can be a harrowing task as a qualitative researcher. She identifies three key challenges that deal with a writing style that is both academic and personal, she identifies construction that doesn’t risk the respect of peers, and she shows confidence in making claims.

For the final chapter, Brian Paltridge takes on the notoriously challenging task of publishing from a dissertation. While a book may seem the more obvious choice, it may be that articles are more advantageous. Either way, it is important to understand that publishing from the dissertation cannot happen without major revisions of the original manuscript, with a strong focus on the shift in audience. Through an examination of his own experience of overcoming the obstacles in getting publications from his doctoral dissertation, Paltridge shows how the obstacles that seem to hinder us can in fact help us to improve the quality of the research outputs from doctoral research.

The volume is rounded out by an important afterword by Andrew D. Cohen, who draws on 45 years of publishing experience in applied linguistics to offer advice to researchers for getting their studies published. The foreword by Sandra Lee McKay and the afterword by Andrew D. Cohen open and close this edited volume with seasoned perspectives from two of the field’s most accomplished scholars. We are grateful to both of them for sharing their insight.

**Embracing realities and complexities in our research**

Through the honest narratives in this volume, and the focus on the truths of conducting research in applied linguistics, we highlight the realities of such research. The strategic organization of these narratives around key stages of the research process is intended to provide a research paradigm in its own right – where the complexities of research methods can be explored as topics of investigation. While we recognize that applied linguistics research can be and is often messy, we too often hide the messiness in favour of a cleaner,
more sanitized description of our sometimes very complex methods and processes. However, such approaches are, as we see it, depriving researchers of valuable insights from which we can grow and allow novice and experienced researchers alike the chance to learn from our pitfalls, mistakes, and follies. The obstacles we face in applied linguistics research can come at any stage of the research process: in the planning, data collection, analysis, and dissemination of results. The obstacles may be central to the research itself, such as working with vulnerable groups. These obstacles are not the problems that need solving, but rather opportunities to strategize and reconceptualize our approaches to the realities of conducting real-world research.

Unlike the days of the “and here’s one we prepared earlier” cooking shows that never reveal to audiences what it is really like to prepare picture-perfect dishes, it is time for us to move forward and recognize that picture perfect is not the goal. Rather, successful research can be achieved via, in spite of, and because of, a myriad of troubles and toils. Working through problems, rather than around them, allows researchers to bring a project to successful completion. Like the picture-perfect cooking shows, which hide the failures and the challenges from their viewers, published research disguises many of the failures and challenges from its readers. By not showcasing these challenges and failures, we deprive the viewers and the readers of opportunities to learn to work through these challenges. Let us look at our messy research methods as building blocks rather than stumbling blocks, and when the finished project finally succeeds, regardless of its originally intended form, we can say proudly, we “nailed it!”

References


Part I

Responding to problems in the research planning stage
Chapter 2

Studying up, down, or across?
Selecting who to research

Ryuko Kubota

Introduction

For students preparing to conduct empirical research, the first task is to identify the topic to pursue. For some students, this can be a time-consuming endeavour. They are typically advised that they should select a topic significant and exciting enough, since it may be pursued long-term even beyond their course of doctoral study. Although selecting an inquiry topic and a group of people in the case of socially oriented research is one of the most important stages of research, issues and challenges behind this phase are rarely addressed in the literature on research methodology. In this chapter, I will draw on my own experience of conducting research on language choice in the workplace (Kubota, 2013; 2015) and discuss the politics of selecting research topics.

A research dilemma: Which group should I choose to study?

Several years ago, shortly after I moved to my current institution in Canada from a US university, I decided to apply for a research grant. It was exciting to have funding opportunities internally at my institution and externally from a government agency called the Social Sciences and Humanities Research Council of Canada (SSHRC). My first task was selecting a research topic. During my previous research project in Japan in 2007, I used an ethnographic lens to investigate the views and experiences of Japanese adults learning English – an activity often linked to global opportunities – in non-formal settings in a rural city which was experiencing increased ethnic and linguistic diversity (e.g., Kubota, 2011a; Kubota & McKay, 2009). In order to understand the local diversity, I participated in community events involving migrants mainly from South America and volunteered to assist students who were learning Japanese as a second language in public schools. This experience gave me a strong impression of the social, cultural, and economic challenges faced by the marginalized migrant population. After finding out that there was a Korean scholar on campus with a similar research interest, I brainstormed
for quite a while, searching for a viable research topic focusing on migrants in both Japanese and Korean contexts.

However, I decided to abandon that idea altogether mainly because of a moral discomfort. I increasingly developed an uncomfortable feeling about studying about the less powerful in our society (i.e., *studying down*) and thought that it might benefit my scholarly achievement more than those migrants’ lives. Obviously, there are ways to mitigate this problem by conducting participatory action research, being ethically vigilant in data collection, and choosing appropriate knowledge mobilization strategies to inform the findings to a broader audience. However, being unable to overcome ambivalence, I shifted the topic completely to focus on language choice in the workplace among Japanese multinational corporate employees working in non-English-dominant countries such as China, Korea, and Thailand. Through investigating these participants’ experiences and views of workplace communication, I wanted to critically examine the neoliberal language ideology underlying the prevalent assumptions that English is a universal language for international communication and that advanced English proficiency is essential for all transnational work. Thus, rather than *studying down* a socially and economically marginalized population, I decided to *study up* in the power hierarchy. This was to evade my moral conflict.

This topic was also an extension of my previous 2007 study in Japan. One of the themes that emerged was a contradiction between the aforementioned assumptions about English and the competencies necessary for overseas work assignments, which was expressed in my interviews with managers of four mid-size companies in the city (Kubota, 2011b). I wondered whether this observation would apply to larger corporations that have subsidiaries in non-English-dominant countries and wanted to investigate transnational Japanese workers’ linguistic and cultural experiences overseas.

My application to an internal research grant was successful, which enabled me to conduct a study on Japanese transnational workers in China (Kubota, 2013). Subsequently, I applied for a SSHRC research grant in order to expand the project by involving both Japanese and Korean companies and extending the geographical location to Japan, Korea, and Thailand. My application this time turned out to be a challenge – it was rejected three times before the project was finally funded. Besides the fact that I was a newcomer to a Canadian research funding community, I faced many challenges arising from the difficulties of *studying up* in the corporate contexts. One was the need to convince the reviewers that obtaining naturally occurring interaction data would be impossible because of a difficulty of accessing the physical workplace. Participants in my previous China study (Kubota, 2013), for instance, made clear to me that audio recording of the workplace communication would not be permitted. Another challenge was recruiting participants; in the summer of 2012, only two companies, or 14% of the ones I contacted, agreed to participate in interviews.
Another dilemma has been the one between my scholarly impetus to critique neoliberal language ideologies and the research outcomes appearing to favour neoliberal human capital embedded in capitalism that has exacerbated global and local economic gaps. More specifically, I launched this project to critically examine the ideology underlying the emphasis on teaching English language skills measurable by standardized tests – the belief that competence in English is an essential component of neoliberal human capital because English is deemed a universal language for global communication (Kubota, 2011a; 2011b). My research by and large has revealed that this prevalent belief seems inconsistent with the views of corporate employees, who acknowledged the usefulness of English but also emphasized the importance of being able to use the local language of the host country and developing communicative ability and positive entrepreneurial dispositions more than linguistic knowledge and skills per se. While this finding does problematize the neoliberal language ideology, the reported linguistic practices in the workplace are embedded in the neoliberal capitalist pursuit of increasing corporate profits. Consequently, endorsing the entrepreneurial habitus of the employees, who promote multilingual and multicultural consciousness, seems to be complicit in neoliberal capitalism that widens economic gaps worldwide.

The aforementioned dilemmas may be specific to this particular research project and may not be exclusively attributable to studying up. However, they raise some important issues in selecting which group of people to study and what research topic to choose. In what follows, I will discuss issues and challenges underlying three orientations: studying down, studying across, and studying up in socially oriented (rather than purely linguistic) research involving human participants.

**Overcoming the dilemma: Selecting who to research**

**Studying down**

In conducting a study, searching for voices of underrepresented groups or studying down certainly uncovers their experiences of marginalization in the social, economic, gendered, and racial hierarchy of power. For critical scholars, studying down is a meaningful endeavour to shed light on oppression, discrimination, and subjugation.

This altruistic aim of studying down needs to be accompanied by researchers’ ethical and moral considerations and conducts, just as any other research projects do. The primary concern is to follow the general guidelines for research ethics. They include respecting the autonomy of research participants who make decisions to participate or stop participating in research, protecting and promoting the welfare of participants by
considering the research’s impact on their social and economic conditions, protecting their privacy, ensuring that they are not exposed to unnecessary risks and that the research results will be shared with them, and treating them fairly and equitably by considering their potential vulnerability (e.g., feeling forced to participate in the study), as well as not abusing the power imbalance between the researcher and the participants (Canadian Institutes of Health Research et al., 2014). These guidelines inform the institutional ethics reviews. The guidelines are by and large based on the rights-based protectionist orientation (Asselin & Doiron, 2016). However, they do not address my initial moral concern that studying migrants might benefit me more than them.

In *studying down*, one way to ensure that the marginalized group will gain greater benefits from research is to use an action-oriented participatory approach. This approach encourages both researcher and participants to collaborate as partners at all stages of the research study – planning, collecting and analyzing data, and writing a report. In research focusing on children, for example, this approach engages children as “an expert in their own lives” and aims to affirm their agency, which would hopefully lead to empowerment (Asselin & Doiron, 2016). In conducting research on communities in developing nations, this approach is called participatory development (PD) and aims to empower the people in the community through establishing a partnership between researchers from the North and local people in engaging in development projects (Kapoor, 2005). However, despite the gesture of benevolence, this approach may end up reproducing the preexisting power relations between the North and the South, between the researcher and the participants, and between different groups of people in the local community (e.g., male/female, local leaders/villagers). Critiquing PD researchers’ hidden desire and complicity, Kapoor (2005) argues that behind the self-effacement that researchers assume in every step of the project to ensure transparency and neutrality, they are often involved in each step, including deciding on the needs, purpose, participants, and agenda of the meeting, and on the use of the outcomes of meetings. This leads to aggrandizing, rather than effacing, their power. The public nature of development activities (and other types of research that produces archiving data in the public domain – see Asselin & Doiron, 2016) can expose participants to state surveillance. Furthermore, despite the altruistic intention to help the other, “we may make (at least a part of) our career off it” (Kapoor, 2005, p. 1204). Ultimately, PD research activities may end up supporting the neoliberal interest of the World Bank and IMF, perpetuating inequality and imperialism. *Studying down* in applied linguistics poses similar challenges of ethical issues, which lead to the reproduction of power imbalances, whereby the researcher becomes more empowered than the marginalized participants and broader structural inequalities are kept intact.
Studying across

Researchers, especially those with a minority status in a particular society or context, may sometimes choose to study across. Doing so will serve to empower both the researcher and the participants who share a marginalized status in their own contexts. Although power imbalances between them do exist because the researcher is likely more privileged than the participants are and there may be other power relations with regard to gender, language, and so on, their shared experiences and perspectives mitigate a power disparity.

An example is insider research, whereby indigenous scholars study their own communities or their own family members (Smith, 2012). In these cases, some of the restrictions set by the ethical review board, such as obtaining permission for personal access to community members, are exempt. However, the indigenous researcher needs to be vigilant of the potential impact of the research on the community and the conflict of the academic norms and the community norms (Canadian Institutes of Health Research et al., 2014).

From an epistemological point of view, Smith (2012) advocates resisting hegemonic colonial approaches to research and instead decolonizing knowledge toward restoration and transformation of the ways of knowing and living. For instance, essentialism and authenticity (e.g., womanhood), which have been problematized in Western postmodern discourses, can take on an entirely different significance for indigenous peoples whose authenticity has been wiped out by colonialism (Smith, 2012). For indigenous research, localizing the aim, epistemology, and methodology is crucial.

An example of studying across in applied linguistics is an Asian Canadian researcher interviewing an Asian Canadian ESL teacher – both shared experiences of being racialized in the Canadian society (Lee & Simon-Maeda, 2006). Studying across in this case led the teacher to securely voice her painful feelings of racial exclusion in her professional contexts, which may not have been expressed had the researcher not shared the minority background. Although the interviews uncovered otherwise hidden experiences of racism, the researcher’s moral responsibility – whether or not the cases of racism should be acted on in order to transform institutional practices – remains unclear. Canadian Institutes of Health Research et al. (2014, p. 61) suggest that when researchers unexpectedly obtain information that may require disclosure to a third party (e.g., child abuse, domestic violence), “advising a research participant about the disclosure may be important to respect the trust relationship with the participant, and to ensure the validity of the participant’s ongoing consent.” However, issues of institutional injustices, such as subtle racism, sexism, and homophobia, require engagement in anti-oppressive education and activism rather than legal actions. Whether and to what extent researchers should engage in activism in response to the information acquired through their research is not unique to studying
However, emotional distress experienced by minority scholars who *study across* may be greater or more complex, thus creating a significant struggle for them.

Another example of *studying across* is non-native-speaking (NNS) scholars’ research on NNS language teachers. Here too, the impetus is to empower NNS teachers, who are often discriminated against in employment and other professional contexts, and to transform institutional practices. Yet a mismatch between the purported aim and the actual outcome can become a concern. Kumaravadivelu (2014) deprecates this persisting gap, especially in light of the proliferation of research on this topic and the increased involvement of NNS professionals in leadership roles in the past two decades. Kumaravadivelu (2014, p. 17) critiques the NNS researchers’ complicity in the hegemonic framework of academic epistemology that continues to oppress NNS individuals and advocates NNS researchers’ engagement in result-oriented and proactive, rather than reactive, research “with the view to reducing exhaustive and exclusive dependency on center-based knowledge systems” and “paying attention to the local exigencies of learning and teaching.” This resonates with the aforementioned approach to indigenous research.

**Studying up**

In conducting research, we often prefer to *study down*, since siding with the underdog and playing an advocate role positions us as well-meaning supporters of social justice and makes us feel good. *Studying down* is also viewed by our peers as an inquiry more meaningful and legitimate because of ostensive benevolence (Priyadharshini, 2003). However, this tendency was problematized more than 40 years ago in the field of anthropology.

Nader (1972) pointed out shortcomings of the traditional ethnographic inquiry focused only on *studying down* the poor, the disadvantaged, and ethnic groups. She argued that oppressions experienced by the marginalized are also caused by social and economic systems, which are created and enforced by people in power and embodied in the forms of policies, customs, regulations, and laws. Accordingly, Nader (1972) advocated *studying up* the powerful institutions, arguing that unraveling the institutional mechanisms of the powerhouse, such as large corporations, government agencies, and other bureaucratic organizations, would provide researchers and citizens with essential knowledge to build and maintain a democratic society. However, *studying up* often poses many methodological challenges.

Consistent with my experience, access to research sites or research participants is typically problematic (Gusterson, 1997; Nader, 1972). People with powerful status are prone to say no to researchers’ requests for participation. To them, being interviewed by scholars for academic inquiry may not seem so different from answering questions from journalists. Even if
they have agreed to participate, they may not favour conventional methods of data collection for research – they tend to disapprove the gathering of information that appears intrusive, such as participant observation or audio recording naturally occurring interactions. In my experience, even being permitted access to corporate interviewees has been rather restrained, since in typical cases, a public relations manager selects the interviewees. This requires caution in interpreting the interview data.

These challenges, especially the difficulties of access, led Nader (1972) to question the value placed on participant observation as the only legitimate research methodology in anthropology at the time of her writing (Nader, 1972). The inability to conduct participant observation in banks, insurance companies, government agencies, and manufacturing companies could discourage anthropologists from studying these powerful institutions, even though these institutions significantly impact the functioning of our society. Gusterson (1997) further recommended multiple and eclectic means of collecting data (e.g., formal interviews typically done in journalism, analyzing archival documents) and hybrid writing strategies that blur disciplinary boundaries for studying up.

In my second application for a SSHRC grant, I needed to respond to the reviewers’ comment that an analysis of audio-recorded workplace interactions should be included. I explicitly stated that access to naturally occurring audio data would not be permitted and including this method would make the study unfeasible. I also shifted my theoretical focus to language ideologies reflected in interview accounts vis-à-vis those underlying language education policies in Japan and Korea.

With regard to access, I have recruited participants through personal connections (e.g., asking acquaintances to introduce potential interviewees) where possible. However, recruitment has been done mainly through identifying potential companies from a large business directory or on the web and contacting them directly by phone or by email. Despite difficulties, some companies have agreed to participate. Yet the nature of interviews in my current ongoing study has been quite different from my 2007 study; each semi-structured interview is usually conducted in a company guest room and the ambience tends to be quite formal. Unlike my previous study on adult English learners, follow-up interviews or interacting informally over a cup of tea or a meal has not normally happened. This raises the issue of rapport.

In qualitative research, establishing rapport with participants by spending sufficient time is considered important for achieving trustworthiness of data (Canadian Institutes of Health Research et al., 2014). In studying down and studying across, this is likely to be achieved with relative ease. In studying up, by contrast, establishing rapport is often difficult and complicated. Reflecting on her participant observation in a study on a corporate-owned women’s soccer team in Japan and her experience with a company president as a research participant who later engaged in major illegal
activities, Edwards (2007) questions the role of rapport in ethnographic research. It is traditionally understood that rapport facilitates researchers to obtain truthful information. However, those in power typically disclose only partial information to outsiders and they are wary of interacting with outside reporters or researchers. For those in power, having rapport with a researcher is often a threat, thus leading to preclusion.

This implies that interview accounts may mirror the official view of the institution rather than their own (Walford, 2012). In my research, I noticed that similar accounts were heard from multiple interviewees working for the same company, thus hinting at the effect of playing the role of a spokesperson. This was influenced by the fact that a manager selected the interviewees. There were also instances where interviewees appeared quite cautious of answering my questions, to the extent that the responses sounded rather bland. Nevertheless, interview accounts are actual data obtained in a particular context, where institutional and personal interests are expressed. It is necessary to take these issues into consideration when analyzing the data and to contextualize the data in reporting.

Issues of rapport also raise a question of ethics. In reporting her ethnography, Edwards (2007) had to make an ethical decision about whether or not to disclose the company’s financial scandal and the name of the CEO. On the one hand, she owed the president and managers her access to the research site, but on the other hand, not disclosing the wrongdoings may not do justice to other research participants (i.e., women soccer players) who were exploited for the sake of corporate promotion and eventually laid off. Nader (1972) states, “We should not necessarily apply the same ethics . . . to the study of institutions, organizations, bureaucracies that have a broad public impact” (pp. 304–305). This is because “the rights of some groups to remain anonymous might conflict with the rights of others to know the processes involved in the reproduction of inequality” (Gaztambide-Fernández & Howard, 2012, p. 300).

This leads to the issue of power. Studying up evokes a reverse image of a power balance between the researcher and the researched compared to studying down. However, the power vector is not always unidirectional. Drawing on a poststructuralist conceptualization of power, Priyadharshini (2003) argues that positionalities of participants and the relationship between the researcher and the participants are unfixed and incoherent. Furthermore, reflecting on their research on elite schools, Gaztambide-Fernández and Howard (2012) argue that their own privilege as university researchers in fact afforded access to schools, and through studying up and publishing their research, they were actually becoming implicated in the reproduction of the elite status in society. Gaztambide-Fernández and Howard (2012) suggest we consider studying in by examining our own privilege and complicity in power equalities. In fact, Walford (2012) argues that studying up may not be significantly different from studying down in that both share
problems of access and authenticity of interview accounts. Furthermore, as Gaztambide-Fernández and Howard (2012) suggest, a researcher’s privilege is not minimized by *studying up*; rather, it tends to remain complicit in the reproduction of the socioeconomic power hierarchy.

It seems that the dilemma of conceptual complicity I faced in my research, which I mentioned earlier, reflects a complex web of complicities involving neoliberal ideology, the elite discourse of corporate workers, and the elitist nature of the discourse of multi/plurilingualism. What also needs to be added to this web of complicity is my own privilege, which has enabled me to become a bilingual scholar who studies this topic and to obtain access to research. Although escaping from this dilemma seems almost impossible, poststructuralist conceptualization of power and knowledge could enable appropriation of the elite discourse to express different meanings for transformation.

**Implications and suggestions for researchers**

Imagine this situation: A doctoral student from a mainstream background in Canada is planning her dissertation research. She wants to study highly skilled new immigrants to Canada who are obtaining an additional degree in Canadian universities to avoid downward career mobility. The purpose is to investigate their linguistic and professional struggles, as well as their individual and professional identity shifts. This inquiry is regarded as *studying down*. While solely focusing on individual struggle and identity is a legitimate inquiry, expanding the focus by *studying up* can provide a broader perspective. This is because external issues, such as employment structures, hiring practices, and government policies, obviously affect their lives.

*Studying up* is a worthy endeavour in research. Yet researchers should anticipate difficulties in access, data collection, and ethics, although these difficulties can also be experienced in *studying down* or *studying across* in different quality and quantity. Researchers proposing to *study up* and anticipating these challenges should communicate the uniqueness of the research project, its importance, and its challenges to the advisors, funders, and other stakeholders to gain support. While overcoming these challenges is essential, it is also vital to *study in* by critically recognizing our own privilege, which occupies the top of the socioeconomic power hierarchy, and the potential complicity of our work in perpetuating the hierarchy. Exploring ways to overcome this dilemma is imperative in our field.

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Chapter 3

Responding to theoretical shifts in research design

Heath Rose

Introduction

The nature of applied linguistics research is that the field can move in drastically unexpected directions in short spans of time. Sometimes a single study or an influential research monograph or theoretical paper can impact the field so much that state-of-the-art conceptual frameworks become rendered obsolete.

In the past, we have seen a number of examples of sudden theoretical shifts. Noam Chomsky (1959) turned language acquisition theory on its head with a single paper that critiqued Skinner’s theories of behaviourism. As a result, no linguist today would argue that behaviourist notions account for our ability to acquire a language. Hymes’s (1972) work into communicative competence impacted the way that language was viewed and as a result the way it was taught and assessed worldwide. When Larsen-Freeman and Cameron (2008) posited language acquisition as a complex adaptive theory, it opened up many sub-fields of linguistics to view language very differently from the ordered fashions of the past. As a result, their work has permeated the field and changed the fundamental assumptions on which linguistic study is based. Dörnyei (2009) introduced his L2 motivation self-system in a co-edited volume in 2009, which impacted the field of motivation so much that it is difficult to find a motivation paper published since that doesn’t reference this theory. While these are famous examples of huge shifts in linguistic theory, similar movements occur in the various sub-fields of applied linguistics, where authoritative figures have the power to drastically alter the focus of research and the methods that are used.

When theory shifts, a researcher can sometimes be flung into turmoil mid-project, which is especially true of lengthy research contexts typical of doctoral research or long-term funded projects. Novice researchers are often caught in academic crossfire and are uncertain of how to respond to theoretical shifts or criticisms within their field when designing their studies. Should they respond to theoretical challenges in the report stage of the project, ignore them altogether, or integrate them into their research designs?
This chapter aims to investigate the options available to researchers when such issues arise. The chapter outlines a real research project into language learning strategies that encountered an obstacle in 2006 when the whole field of language learning strategies came into question and there were suggestions that the field be replaced with self-regulation.

The study

In 2005, as part of my doctoral research I had designed a study to investigate language learning strategies, which was later disseminated in a number of publications (Rose, 2012; 2013; Rose & Harbon, 2013). Language learning strategies, sometimes called language learner strategies, are defined as the “[t]houghts and actions, consciously chosen and operationalized by language learners, to assist them in carrying out a multiplicity of tasks from the very outset of learning to the most advanced levels of target language performance” (Cohen, 2011, p. 7). My research particularly focused on the strategies deployed by students of Japanese as a foreign language to learn the complicated Japanese writing system. In order to become functionally literate in the Japanese language, students must learn 2,000 kanji (morphographic characters) – an immense obstacle for learners. Research into the cognitive strategies that students employed to reach this goal, and metacognitive strategies that students used to manage the learning of this difficult script, was hoped to aid students in their learning. I developed a framework for categorizing kanji learning strategies based on previous research into kanji learning (e.g., Bourke, 1996; Toyoda, 2000; Toyoda & Kubota, 2001), as well as a prior pilot study in the area (Rose, 2003).

A theoretical framework is essential to conducting any research project. Research methodologists often discuss framework as a lens through which the research is planned, the data analyzed, and the results reported. A theoretical framework often rests upon epistemologies derived from the literature, prior studies, and our own experiences and ideas. Once a conceptual framework has been developed for a project, it is often presented as a static product whose structure brings an element of objectivity and positivism to the research.

The theoretical framework for my study was built on previous research in the field and included a questionnaire on kanji strategy use, a stimulated recall task to have students introspect on strategies used in the learning process, and an intervention to measure the teaching of kanji learning strategies via quasi-experimental design. The concepts underpinning my framework were borrowed heavily from the Strategy Inventory of Learning Kanji (SILK), which was the product of exploratory doctoral research in the field (Bourke, 1996). The SILK was an original questionnaire, but inspirations behind its creation were clearly drawn from the widely used Strategy Inventory of Language Learning (SILL) (Oxford, 1990). Both inventories
aimed to group strategies into categories and applied a frequency scale in order to measure the use of these strategies in a self-report questionnaire that could be given to learners for the purposes of collecting research data. The stimulated recall task was based in psychological practices as a useful way to observe cognitive practices involved with the memorization of kanji. The task involved students taking a kanji test (the stimulus) in front of the researcher, while talking aloud their thinking processes of how they recalled, and tried to memorize, the kanji leading up to the test. These processes could then be coded according to the strategy inventory of kanji learning, to evaluate the validity of observed practices with self-reported strategies. Thus the research design of the study, I believed, was a solid way to examine the language learning strategies in the kanji learning process.

**The research problem**

In 2006, just as data collection was planned to get underway, a series of book chapters and articles by Zoltan Dörnyei threatened the validity of the planned research by criticizing the theoretical underpinnings of strategy research as a whole. In 2005, Dörnyei had built on a previous paper (Dörnyei & Skehan, 2003) to argue that previous constructs of language learning strategies (e.g., O’Malley & Chamot, 1990; Oxford, 1990) were fundamentally flawed. He, for example, argued that ways to categorize strategy use were invalid. For example, in Oxford’s taxonomy of language learning strategies, there exists a separation of mnemonic strategies and cognitive strategies, whereas logic would hold that mnemonic strategies were a sub-category of cognitive strategies. Around the same time, key figures in the field were also becoming vocal about definitional issues in the field, and Macaro (2006) produced an influenced article outlining areas of a lack of consensus in strategy research, including a lack of understanding of the influence of strategic behaviour on language learning achievement, despite 30 years of research.

Dörnyei (2005) also heavily criticized the SILL, which at the time was the most widely used instrument to measure language strategy use (White, Schramm, & Chamot, 2007). Most criticism stemmed from the practice of computing mean scores from the SILL. Dörnyei (2005, p. 182) argued, “The scales in the SILL are not cumulative and computing mean scores is psychometrically not justifiable.” This argument is based on the fact that the SILL frequency scales are not linear in relationship, and that the SILL items connect to specific (and separate) behaviours rather than general trends and shared inclinations that can be pooled together as part of the same scale. Obviously, such statements reverberated throughout the field of language learning strategies, as they rendered the most widely reported instrument in the field as invalid.
In a later article, Tseng, Dörnyei, and Schmitt (2006) built on a recommendation from Dörnyei (2005) to replace the language learning strategy paradigm with the notion of self-regulation, which had been intensively investigated in the field of psychology, but was relatively new to the field of second language acquisition. Dörnyei (2005, p. 184) lobbied for use of new instruments such as the “self-regulatory capacity for learning vocabulary” (SRCVoc) and claimed, “the SRCVoc does not measure strategy use but rather the learner’s underlying self-regulatory capacity that will result in strategy use.” Tseng et al. (2006) applied this instrument in one of the first studies to do so and concluded that self-regulation was a more valid measure of the driving forces behind strategy use and thus could be more consistently and accurately measured.

The implications to my project were immense. My methodology was not only drawn into question but also the entire field of language learning strategy research, including the very notion of measuring strategies and connecting these with language learning success, was thrown into turmoil.

**Overcoming the obstacle**

With the current situation of my research project, I was left with two choices:

1. to ignore the recent criticism of the field in the hope that it would not have the impact that I suspected it might
2. to delay data collection in order to make changes to my research design to integrate new ideas in the field.

The merit of the first option is that I could collect data as planned. I would also not be taking the risk of integrating a new idea in the field, which was not guaranteed to gain any traction. Self-regulation, after all, was an untested concept in the field of second language acquisition, at least in the form it took in Tseng et al.’s (2006) seminal article, which mapped self-regulation onto an earlier taxonomy of motivational control. However, there was also an equal risk that the concept would gain traction, which would make the results of my findings open to similar criticisms, or at least appear to be very dated by the time they would be published. The merit of the second option would be that if the new concept of self-regulation did prove fruitful, the study could find itself situated within a new approach of examining strategic language learning. However, the time to re-design the study was likely to extend the completion of the project by about 6 to 12 months.

At the beginning of data collection, a researcher is in a good position to judge the worth of theoretical shifts in the field, having read up on recent trends and evaluated similar studies in the field. From my position, I could see much logic in the arguments made by Dörnyei and had indeed
already steered clear of problematic issues such as definitional overlaps in my adoption of Bourke’s (1996) categories of kanji learning strategy use, as opposed to the more widely used Oxford (1990) and O’Malley and Chamot’s (1990) taxonomies of language learning strategies. Also, Tseng et al. (2006) had asserted that the stimulated recall task was a highly promising yet underutilized data collection technique in the field, which helped support at least some of my original research approach. Their call for more qualitative research approaches had also supported some of the original decisions made in the construction of my theoretical framework, as even within the field of language learning strategies there was a recent trend to move towards more contextually situated approaches of data collection.

Upon further examination of the two competing paradigms of language learning strategies and self-regulation, it soon became obvious that there were some aspects of language learning strategy research that could not be adequately incorporated into the self-regulation framework. The most striking difference between the two was that self-regulation, in its examination of how learners manage the learning process, did not focus on cognitive processes of memorization, which were important notions in language learning strategy research. In fact, earlier frameworks of language learning strategy research such as O’Malley and Chamot’s were almost entirely focused on cognitive theory and memory strategies, with social and affective strategies an additional ad hoc category. Therefore, the two models suddenly appeared to be less in competition with each other, but more complementary.

Based on an assessment of the theoretical shift, many months were spent on reconceptualizing the theoretical framework for the study. I placed the behavioural aspects of learning out of the language learning strategy framework and reworked them into the proposed framework of investigating self-regulation. The cognitive aspects of learning were maintained in a framework that would allow the examination of strategies used by learners. This would allow the results to be compared with similar studies in the field, which had largely focused on cognitive strategies. To further improve the theory behind cognitive strategies, I also turned to key literature in the cognitive sciences to give these cognitive processes examined in my study a more scientific theoretical structure. This in a way distanced the study from language learning strategy research and situated it more in cognitive science and psychology, with a context-specific focus on the learning of Japanese kanji. The result was a framework that allowed the study to continue to examine both cognitive and behavioural aspects of learning from a second language learning theoretical standpoint (as both language learning strategies and motivation were part of the ‘individual differences in second language acquisition’ paradigm), as well as a cognitive science/psychology standpoint (where notions of memory strategies
and self-regulation are firmly situated). The framework for the study can be found in Figure 3.1.

The year of 2006 marked a watershed in language learning strategy research, but the field did not crumble, as I had feared. A number of articles and books emerged in response to Dörnyei’s (2005) critique. For example, Gao (2007) suggested that models of strategy use and models of self-regulation were not incompatible, as they measured the beginning and end product of the same event. Such responses were immediately reassuring of the direction my study had taken. The same year, Grenfell and Macaro (2007) stated that Dörnyei was “setting up a straw man in order to knock him down” (p. 26). They claimed that Dörnyei had focused on the historical aspects of strategy research in the critique of the Oxford (1990) and O’Malley and Chamot (1990) taxonomies of strategy use, which were 16 years old at the time, and had ignored huge movements in the field that used more situated approaches. Gu (2012, p. 331) later argued, “conceptual fuzziness should not be a problem serious enough to overthrow forty years of research on language learning strategies.”

However, the field of self-regulation did make important inroads into strategy research, as much of Dörnyei’s critique had resounding effects on the theory that had underpinned strategy research for years. Key researchers such as Oxford (2011) integrated self-regulation into her positioning of strategic learning in the ‘strategic self-regulation (S2R) model.’ Self-regulation

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**Figure 3.1** The theoretical framework.
began to appear alongside learning strategies in second language acquisition literature.

Ultimately, the decision to include self-regulation in the theoretical framework of the kanji study proved to be a wise choice. Incorporating the theoretical shift into the study’s research design put me in a good position to comment on the effectiveness of the proposed design beyond vocabulary learning, which had been the only other context to use self-regulation as the lens of investigation (Tseng et al., 2006). As a result, of the articles that were published from the study, I felt that the most important one was not on the findings of the study itself, but a conceptual article on using self-regulation as a framework to look at the strategic behaviour of learners (see Rose, 2012).

**Implications for researchers**

The study outlined in this chapter is an important illustration of the impact of theoretical shifts in research that can affect projects *in situ*. Published research in the form of journal articles, books, and chapters can knock the foundations from under a carefully planned project by questioning theoretical concepts and criticizing research methods and data collection instruments. The unexpected publishing of similar research can also severely reduce the significance of the project. The delay between data collection and dissemination increases the risk of such changes, and if a project is unable to adapt to these changes, there is a real possibility that the study will have fallen out of favour by the time it is ready to print.

Flexibility in design is one key implication for researchers who are faced with such theoretical shifts. Qualitative research is better equipped to handle such change, as quantitative instruments are often carefully constructed and pre-coded according to existing theory. For example, a questionnaire that was constructed via piloted measures and tested scales would be far more problematic to re-design than an interview guide. Likewise, observational charts, which would also be pre-coded in the form of lists and checkboxes, would be far less adaptable than qualitative field notes, which may contain open topics and themes for observation and commentary. The suggestion here is not to abandon quantitative data collection instruments in favour of qualitative ones, but to point out the inherent advantages that qualitative designs have over quantitative ones in terms of adaptability and flexibility. In recent years, research methodologists have been pointing to the importance of mixed-methods designs, which is potentially of even more relevance to the novice researcher. The advantage of adding a qualitative element to a largely quantitative study results not only in obtaining a richer depth in the data collected but also the added degree of flexibility to the project. Thus, if theoretical shifts or critiques occur, the project can at least make subtle changes in one part of its design.
A multidimensional or multistage element to research design is also a key implication for researchers to adapt to sudden theoretical shifts. The study presented in this chapter highlighted the value of a split approach in the research design to examine both self-regulation and learning strategies, and to explore learning from a cognitive and behavioural perspective. Such a design is not only useful to situate the research into multiple fields (as much applied linguistic research does) but also to hedge the risk of the research design not yielding the quality of findings expected. That is, it is often hard for researchers to predict which of the findings will prove most fruitful, and it is often the unexpected results in research that provide the most insight into the topic being explored. The same benefits can be found in multistage studies, where the first stage is usually exploratory in nature to pave the way for descriptive or explanatory research stages to follow. Alternatively, a first stage could be used to examine the phenomenon being ‘studies in a situated context,’ before subsequent stages expand the research parameters or sample in the attempt to test generalizability of the findings from the first stage. Multistage studies, like multidimensional studies, also have the added advantage of being able to incorporate theoretical shifts mid-project, as they are by their very nature flexible and adaptive.

Finally, it is important to note that researchers should be cautious of theoretical shifts mid-project and not be too hasty to integrate new ideas or respond to criticisms without fully assessing their potential impact. There are many examples of critiques that have failed to create the waves in theoretical change that one might first expect. For example, O’Regan (2014) mounted a scathing critique on the field of English as a lingua franca, which was published in *Applied Linguistics*. The fact that this article appeared in a highly reputable journal may have indicated to a novice researcher that the theoretical underpinnings of the field would be on future uncertain ground. Six months later, however, a response came in the form of a forum piece in the same journal (Widdowson, 2015), which focused on the flaws in O’Regan’s critique and relegated many of his points as moot. Critical pieces in journals are almost always followed by a response from the criticized camp, and thus it is worth waiting for such counterarguments before jumping to conclusions that the field is shifting. That being said, a researcher must weigh up the value of a critique and make the changes based on his or her own assessment. In most cases, it would not be advisable to abandon planned research, but merely to acknowledge such criticisms have emerged. Certainly, smaller shifts can be dealt with in the data analysis and write-up stages of a study.

In conclusion, the theoretical underpinnings of a research project can be put in a difficult position mid-project by any number of factors, including theoretical breakthroughs within the field (such as Larson-Freeman and Cameron’s work in complex adaptive systems), criticism mounted at a field of study (such as Chomsky’s critique of Skinner’s behaviourism), or
findings that question research designs (such as the awareness of the Hawthorn effect on observational and experimental research). In such cases, the researcher must assess the impact of these changes on the research project and act accordingly. Depending on the assessment, the researcher can choose to re-design the project, or carry out the project as planned and deal with the changes in the analysis and write-up stages. The risk of the latter is that the perceived value of the project might be diminished if the theoretical shift gains traction in the field between data collection of the research and the publishing of results — a timeline that often takes years. Flexible research designs, such as those that include qualitative elements, have multiple stages, or bridge fields, are less susceptible to such shifts. Thus, if a researcher feels that his or her field or approach might stand on uncertain ground, such a flexible design might be worth pursuing. Certainly, in the case outlined in this chapter, the project emerged from the theoretical shift in a better position for responding to the changes in a calculated manner.

References


Chapter 4

Overcoming problematic positionality and researcher objectivity

Jim McKinley

Introduction

As language education researchers, we may often find ourselves in contexts where we are outsiders to the research population. This presents a significant ethical challenge – particularly conflict of interest – that requires careful treatment. Researchers enter foreign learning contexts with a great deal of cultural influence and bias. While the exploratory nature of the bulk of TESOL studies suggests that Western researchers are safely avoiding culturally biased negative evaluation of foreign learning contexts, of course there is cultural interference in understanding those contexts. There are two options here: one is situated qualitative research – to try to understand the situation from the students’ point of view (Atkinson, 2005). Another idea is to ‘remove’ oneself from prior knowledge and prejudices to establish objectivity. As a research methodology, this ‘innocence’ is otherwise never really possible, since our own life experiences will always affect our understanding of what we observe (Berger & Luckmann, 1966; Matsuda, 2015). Both of these ideas lead to dilemmas of great philosophical proportions – of trying to understand a cultural scene that is not our own.\(^1\)

Objectivity in such studies is never absolute, as there is a certain partisanship that is necessary to maintain an attachment between the researcher and the study (Darlington & Dobson, 2013). Our subjectivity is pivotal in our research since it ultimately defines the boundaries of any observational study. What is important here is to understand that subjectivity does not have to be a limitation, but rather a clear perspective.

This chapter will first describe the situation I faced in dealing with this issue in my own research. This is followed by a discussion of the cultural setting or ‘embeddedness’ of a foreign context in the form of an English composition classroom at a Japanese university (and thus English as a foreign language – EFL). Finally, I explore the implications of a Western researcher’s understanding of students at that university and, in offering recommendations to other researchers in similar situations, explain how that impacts the outsider’s interpretation of the students’ ability and exercise of critical thinking.
Background to the study

My doctoral research investigated Japanese first- and second-year undergraduate students learning English academic writing in their compulsory English composition courses in a Japanese university. The thesis took a social constructivist approach to investigate the aspects of critical argument and writer identity in these students’ classes and their writing (see McKinley, 2015). However, in this chapter, I refer to the use of social constructionism rather than social constructivism. Rather than focusing on actively constructing knowledge, I emphasize a need to be flexible about knowledge construction as there are no objective interpretations.

The data for the study included classroom observations and teacher and student interviews, all conducted monthly throughout the academic year-long course. In total, there were 6 courses, 4 teachers, and 16 student participants. The data were analyzed using all Western frameworks, as there were no established frameworks in such studies in Japanese. For the observation data, I used an adapted version of Ivanič’s (2004) Discourses of Writing framework, which focused on aspects of identity construction in the writing classroom. The linguistic data included a selection of one major piece of writing from each student, which was analyzed using an adapted appraisal framework within Systemic Functional Linguistics (Martin, 1997; 2000). In order to maintain a focus on writer identity in the analysis, Clark and Ivanič’s (1997) selves were identified through this analysis. In addition, the texts were analyzed for use of Casanave’s (2002) writing game strategies in order to further establish the students’ approaches in writing their texts. The objective was not to generalize about how Japanese students learn to write academic English, but rather to provide, from a social constructivist, Western researcher’s perspective, an analysis of what happened in these students’ writing classes and how it affected their writing for those classes.

The obstacle

There was, from the beginning, some obvious concern in such a study conducted by a Western researcher that focuses on the social and cultural positions of Japanese students and Western and Japanese teachers of English in Japan. Culture and language barriers became a large part of the consideration of the study, generating invaluable discussion of the conflicts that arise for both students and teachers in the EFL education setting. This section examines three different conflicting perspectives in the real research problem I faced: first, the ESL versus EFL foreign language context setting; second, objectivist/positivist versus constructionist positionality; and third, the Western “extending knowledge” versus Confucian “conserving knowledge” divide.
Foreign language context – ESL versus EFL

Many published studies on Japanese students’ English writing have been conducted in English-speaking environments (Sasaki, 2005). This ESL (English as a second language) setting creates a very different environment in which Japanese students are required to complete writing tasks. Although their thinking may still be in Japanese, these students have a different sense of cultural expectations of their writing, and therefore researchers in the ESL setting must consider cultural subjectivity in this situation from the students’ perspective rather than their own (see Casanave, 2003).

In publications discussing cultural issues in EFL writing instruction (see e.g., Kobayashi & Rinnert, 2008), we understand that native teachers have adjusted their teaching styles as well as their curricula to be more readily acceptable and understandable for their EFL students (Cumming, 2003). The teachers in my study were native English teachers (as is the trend in Japan – to have native teachers do the composition classes while Japanese teachers do the literature classes), as well as bilingual Japanese teachers. They were observed giving tasks using familiar topics and asking students to exercise skills they covered in the class. The writing practices were based on loosely organized syllabus ideas they received at the beginning of the year. This was, as research suggests, really the best they could have done. There is still not yet a comprehensive theory of writing (Grabe & Kaplan, 2014; Sasaki, 2005), and models of both EFL and ESL writing vary according to educational circumstances.

Positionality: Objectivist/positivist versus constructionist

‘Positionality’ was crucial to the understanding of how meaning was problematized in such a research project. It was how I identified myself in terms of my sense of where and to what I belonged or did not belong and the social relations that were affected by this (see Anthias, 2013). The concepts of ‘positionality’ and ‘structural embeddedness,’ from an objectivist/positivist perspective (that all reality is objective and external to the mind and that knowledge is reliably based on observed objects and events), have the potential of invalidating my research project (see Trottier, 2014). My positionality as a Western researcher and native user of the English language is quite different from the students’ positionality as Japanese learners attempting to cross culture and display critical thinking in their writing of English, a foreign language. Recently, these students have been increasingly pressured to produce such thinking in their Japanese writing as well, as academic writing at university in Japan has become more established globally, following a deductive form (McKinley, 2014). Culture and language barriers need to be taken into deep consideration and recognition of
these barriers as limitations of my research is essential. Also, my positionality as a Western teacher is one of challenging students and questioning what is commonly accepted – key aspects of critical thinking. The positionality of students in Japan is traditionally quite opposite to mine, in that authority is not meant to be questioned, although relatively recent research suggests that traditional understanding of Japanese students is no longer valid (see McKinley, 2013; Stapleton, 2002).

**Western ‘extending knowledge’ versus Confucian ‘conserving knowledge’**

Finally, there is the issue of contrasting varieties of practice in different cultures, in particular that of the impact of Confucianism on English language education in East Asia. Turner (2011) discusses the dangers of Western hegemony in interpreting the impact as detrimental to East Asian students’ ability to learn English and encourages researchers and teachers to take a more “globally neutral” approach (p.109). She refers to Ballard and Clanchy (1991) who described Western academic cultures as “extending knowledge” and Confucian cultures (including Japanese) as “conserving knowledge.” This distinction implies that Western cultures are inherently forward-thinking, leaving Confucian cultures behind. Turner refers to Kubota’s (1999) criticism of this distinction, labelling it “orientalist.” The problem with this criticism, Turner points out, is that it suggests that the perspective is somehow universally accepted. Even in doing so, there is no reason to assume that a “conserving knowledge” approach should be seen as negative. Turner encourages those involved in this discussion to remember the importance of understanding the differences between the Confucian thinking in Eastern educational approaches and the embedded critical thinking in Western critical approaches and not to place higher value of one over the other. This is particularly important with the increase of globalization in higher education.

**Overcoming the obstacle**

In presenting the real research problem in these three particular sections, my intention is to show that the ‘inner circle’ developments in English L2 writing education have not been considered carefully in Japan because of the rhetorical differences between Japanese and English, and that this may be the cause for the inconsistency of approaches currently taken in EFL writing education in Japan. To clarify, Japan is a country located in what Kachru (1992) described as the “expanding circle.” This is the outermost of three circles of English: the inner circle consists of the countries where English is the native language spoken and where ESL is taught (United States, United Kingdom, etc.); the outer circle includes countries where English is not the
native language, yet it holds important historical value and has official status in certain arenas (India, the Philippines, Kenya, etc.); and the expanding circle, which includes most of the rest of the world where English has no official status but is still used widely and where EFL is taught (most of Europe, East Asia, Egypt, etc.).

Another significant distinction made in terms of the variety of practice of English education was made by Holliday (1994) using the acronyms BANA and TESEP to differentiate between the mainstream English language teaching discourse in native English settings in Britain, Australasia, and North America (BANA) and the English language teaching discourse in educational settings at the Tertiary, Secondary, and Primary (TESEP) levels throughout the world. Holliday raised the issue that there were considerable differences in the discourse and the often difficult circumstances found in classrooms. An important discussion regarding this issue is the misapplication of such methodologies first described by West (1960) with his distinction of Teaching in Difficult Circumstances (TiDC). These distinctions made by West and Holliday emphasized the fact that the mainstream English language teaching methodologies were based on ESL taught in multilingual, intensive settings with a small number of students and trained native-speaking teachers. Outside this context, ESL and EFL practices were used in very different circumstances. EFL was often taught in monolingual settings under the pressure of passing examinations, with less frequent class meetings, larger classes, and uncertain teacher training or proficiency. The distinctions were made to encourage skepticism about monolithic Western methods and to urge researchers and teachers to consider alternatives to the belief that BANA products and ideas were the only options in English language teaching regardless of the context (Kuchah & Smith, 2011).

Regarding positionality, my doctoral research problematized meaning in that it questioned concepts and methodological implications in the creation or development of meaning through research (Trottier, 2014). In my research, the ‘problem’ of meaning is treated by the issues brought up in previous studies that suggest Japanese university students are not able to apply critical thinking to their writing (see Stapleton, 2002). The meaning of ‘critical thinking’ is the problem that I believe has particular implications for Japanese students, and it is these implications that I need to familiarize myself with in order to benefit from research in the Japanese university English writing classroom (see Silva, 2005 for discussion).

In a case study research project where a Western researcher is observing Japanese university students in their own classrooms in Japan being taught critical English writing by Western teachers, certainly the issue of cultural sensitivity is crucial. I feel it is my duty as a Western researcher to try to be as objective as possible to be able to have a better understanding of the social phenomena that is learning English writing in a Japanese university. Both options in dealing with this problematic positionality – situated
qualitative research and attempting to ‘remove’ oneself from prior knowledge and prejudices – lead to dilemmas of great philosophical proportions. Professor Ryuko Kubota offered me this advice via email:

I think it’s impossible to pursue an objective representation of social phenomena. Even if you were Japanese, you would have a certain social, cultural, and economic status that functions as a lens through which you interpret the data in a certain way. So rather than worrying about objectivity, I would expose my own subjectivity and explore what influence it might give to my data collection and interpretation.


My subjectivity is pivotal in my own research since it ultimately defines the boundaries of any observational study I do. But is this a limitation? After reading Professor Kubota’s email, I realized it in fact offered me a clear perspective, rather than a limitation.

Finally, in consideration of the Western versus Confucian divide, it may be that the impact of Confucian ideals has been misinterpreted by the West. Turner (2011) points out that as the number of students from East Asian countries, such as China, Japan, Korea, and Taiwan, has increased in the United Kingdom, their behaviour has served as a point of difference between Eastern and Western students. She argues that this difference has become entrenched, but that Western interpretations, maintaining the negative stereotypes of “passive” or “silent” or “uncritical” East Asian students, are no longer appropriate (p.97). Turner explains that Western instructors see the passive nature or silence of East Asian students in classrooms as resistance to speaking in class, but she argues that there are culturally weighted differences regarding both silence and listening, which are seen as proactive. While Western approaches may emphasize “learning through speaking,” this is an activity traditionally frowned upon by East Asian students who would prefer to follow the tutor who should serve as an exemplar or model demonstrating what should be done. It is a different way of learning (opposite to the Western approach of tutor as facilitator), reflecting Confucian tradition in which students follow their tutor’s lead on “their own individual journey of self-perfection” (Turner, 2011, p. 161). Turner emphasizes the importance of recognizing and understanding both the conflict between, and transformative effects of these two approaches as intercultural interaction plays an increasingly significant role in the global cultural construction of international universities.

Turner (2011) goes on to explain how the “contrasting educational ideologies” of East and West place different value on critical thinking. Because “being critical is historically embedded in Western educational culture as a positive value,” it requires an understanding of the “rhetoricity of being critical” (p.185). As East Asian students have not grown up with such
rhetoricity, they may be seen in Western contexts as ‘uncritical.’ Traditionally, it is important for a Japanese student in Japan to display an ability to listen and read without criticizing or evaluating, and rather than reinforcing opposition, to find harmony in a comparison of two opposing views. However, these actions are not in line with the goals of Western educational approaches. For such Japanese students, Turner explains that providing critique is more than “being critical,” which may be understood as simply disagreeing (p. 189). She emphasizes that being critical has “gone global as an educational issue” (p. 192), and an understanding of “rhetorical exchange value” is of crucial importance, considering the strengths of both Eastern and Western interpretations of it, in order to avoid East Asian students seeing the Western understanding of being critical as superior.

Implications for researchers

The structural embeddedness in my study was the relationship of the students’ experiences with learning English writing and the broader social context of the expectations and understanding of the curriculum at their university in Japan. The broader social context controls these expectations and understandings, which is why researchers in this area are looking more towards social and political aspects to be taken into consideration in second language writing pedagogy, particularly in Japan (see Casanave, 2003). To suggest that Japanese university students are limited by this structural embeddedness and therefore cannot produce critical thinking in their writing is an area of great criticism (Mc Kinley, 2013).

In making sense of collected data, particularly for novice researchers, the framing and focus in interpretation of the data are very important. It is essential to be able to ‘frame’ the interpretation of the data in order to discuss it categorically. In utilizing embedded case studies, these frames or categories are needed in order to develop discussions across the different cases. Researchers in this area, as I did, will undoubtedly use knowledge from previous experience as writing teachers as well as ideas about potential frames from previous studies. As Hyland (2005) confessed, the analysis of data is always informed by our own understanding of theories behind writing. These frames could potentially dominate the researcher’s interpretation, possibly distorting existing forms, but the frames are subject to the context in which the data collection takes place and will ultimately be defined by the context that presents itself.

Also for novice researchers to note, cultural frames create an inner and outer sense to a group and can also segregate and marginalize. Specialist frames exclude those without training in the particular field. For analysis in studies such as mine, language, cultural, and specialist frames were vital. Language was central to my study in terms of how and when students and teachers selected what words they used in a writing classroom.
Cultural frames were necessary in that I was looking at cultural transfer, from the Japanese and Western teachers to the Japanese students. Specialist frames were useful to see how the teachers’ qualifications benefitted their approaches to the development of their students’ writing skills.

So both culture and organizational embeddedness need to be taken into consideration in data analysis. Along the lines of cultural frames is the concept of local culture. This is the culture that is made up of shared meanings that give shape to understanding, also referred to as a discourse community in genre studies (Paltridge, 2001). It is not meant to determine the biographies of the participants in a study (normally an impossible task given the variety in a discourse community) but rather to offer recognizable interpretive resources or frames for understanding. Organizational embeddedness is used to reflect relevant individual priorities outside the routine realities outlined by the frames, while at the same time providing those within the organization with the tools for interpretation of, for example, interpersonal relations, specialized goals, and professional perspectives (see Flowerdew & Wang, 2015). This organizational embeddedness was an essential framework in the interpretation of data in my study, as I needed the input of the students’ (Japanese) and teachers’ (Japanese and Western) perspectives on the discourse community in order to allow mine to emerge.

It seems it is neither an advantage nor limitation being a Western researcher in Japan. It is rather a clearly definable perspective that needs to be not only recognized by the researcher but also used as the basis for understanding the culture in which the researcher is studying. This recognition should (Berger & Luckmann, 1966; Flowerdew, 2005), by moving past positivism and adopting a constructionist theory (to discover the ways that individuals and groups create their perceived reality), allow the ‘positionality’ of the Western researcher and the Japanese university student to inform the research.

Note
1 Portions of this chapter have been adapted from McKinley (2005).

References


Introduction

For most doctoral students, the development of research expertise starts with the acquisition of existing knowledge and gradually moves to an emphasis on creating new knowledge. In US graduate schools, the former is usually achieved through coursework, while the latter happens during the dissertation stage. Too often doctoral students are left to their own devices to make the shift from one type of knowledge work to another, although, in some cases, students may make the transition with the guidance of an experienced researcher who can serve as a mentor. One way of facilitating the transition is to develop collaborative projects in which researchers with varying levels of experience and expertise work together (Matsuda, 2016). This chapter explores the issues surrounding a collaborative research project that was implemented to facilitate the development of research expertise. To systematically document the challenges and opportunities that occurred throughout the collaborative project, another layer was added – a meta-research project – in which another researcher studied the collaborative process and collected data through participant observation and interviews.

We begin this chapter by discussing the rationale and design of the collaborative research project as a site of situated learning. We then present challenges that emerged in the project and the ways in which the researchers negotiated them. We conclude by discussing issues involved with designing complex collaborative research-learning projects.

About the research project

Our collaborative research project started in October 2012 as part of Paul’s ongoing efforts to mentor doctoral students through engagement in real research and professional activities (Matsuda, 2016; Ortmeier-Hooper, 2016; Saenkhum, 2016; Simpson & Matsuda, 2008) informed by theories of situated learning (e.g., Lave & Wenger, 1991). Reviewing the records of the mentoring activities with his advisees, Paul noticed that he had not
had a chance to work closely with a few of his students, including Jing and Taimin, and he began considering possible projects that might fit their research interests, methodological orientations, and possible career paths. Since both of them are Mandarin Chinese (henceforth ‘Chinese’) speakers, Paul thought a project that leveraged their linguistic resources might be useful and decided to develop a study that investigated the experiences of Chinese-speaking undergraduate students in US composition classrooms – a student population that had been on the rise in recent years. To gain a better understanding of the experiences of this growing population, Paul wanted to conduct interviews with the students and their composition teachers, and he believed that Jing and Tai-Min could make substantive contributions in that effort.

Given the focus and potentially large scope of the project, Paul invited three more of his doctoral students to participate, Jianing, Jill, and Juval. Jianing and Jill both spoke Chinese, while Juval and Paul had reading proficiency in Chinese. Among the four Chinese speakers, two were from Mainland China (Jing and Jianing) and two were from Taiwan (Jill and Taimin). Paul included both groups in an effort to encourage the students to think beyond the particulars of political boundaries and to focus on common issues at the level of theoretical constructs – a lesson that all researchers need to learn as they attempt to make their research relevant to an international audience. Paul invited Juval, a Filipino American native English speaker, in part because of his research interests in teacher identity and cognition and in qualitative research methods. Juval’s main role was to interview composition teachers and facilitate the qualitative data analysis. Additionally, having collaborated with Juval on other projects, Paul also hoped that Juval would assume a leadership role, which would give him a chance to learn to mentor graduate students. This became the core study, or first layer, and the first objective of the project.

Paul also invited Dan, whose research interests included collaborative learning and writing, to observe the collaborative process itself and to collect data for his research, including his dissertation. Dan’s study, which aimed to explore the dynamics of collaboration in research, became the second layer of the project. Adding still another layer, Paul was also interested in reporting the process of having doctoral students engage in group research under the guidance of a more experienced mentor. He was interested in how the processes of both the collaborative research project and the observation of the collaborative project could be a vehicle for professional development of the researchers involved – an objective which became the third layer of the project.

Thus the study consisted of multiple layers, as depicted in Figure 5.1. First, the study by doctoral students, supervised by Paul, was at the core of the project, providing the necessary foundation for other studies. Publications from this project are still being developed, but a spin-off study
Doing collaborative research 49

utilizing some of the core project’s data was published in Racelis and Matsuda (2015). As the second layer, Dan’s study aimed to observe the process of collaboration, which has been reported in a doctoral dissertation (Bommarito, 2015) and an article (Bommarito, 2016). Finally, in the outer layer, Paul aimed to explore this activity as a means of researcher development – a concept that has been included in a published chapter (Matsuda, 2016).

The remainder of this chapter is based largely on the data Dan collected, as he was in the ideal position to observe the collaborative processes of the core study and, as a collaborator-participant, to achieve Paul’s professional development objective. In the spirit of dialogic research “on, for and with” the participants (Cameron, Frazer, Harvey, Rampton, & Richardson, 1992, p. 22), this chapter goes beyond member check by involving all participants as co-authors. That is, the insights shared in this chapter are co-constructed through the negotiation of multiple layers of knowledge – including the mentor’s philosophy and instructional design work, the results of the meta-research project, and the research team members’ critical reflections, which occurred during the processes of data collection, analysis, and drafting.

To avoid exerting undue influence, Paul refrained from accessing the raw data or from commenting on Dan’s descriptions until he had the chance to form his initial interpretations. For the same reason, Dan worked with the other co-authors to finalize this chapter. Thus researchers working at all levels have had the opportunity to represent their own insights on the overall project.
Challenges and solutions in context

**Challenge 1: Developing a shared purpose for research**

To develop a shared rationale for the core research project, the team discussed the broad disciplinary concerns motivating the study and the specific procedures for investigating those concerns in their local context. From Paul’s perspective, the impetus for this study was the concern among US college writing teachers about the influx of Chinese undergraduate students who demonstrated low proficiency with English despite having high language proficiency scores on tests such as the TOEFL (Test of English as a Foreign Language). At the team’s first meeting, Paul described this issue in detail, indicating key questions that needed to be addressed and suggesting related topics and resources the team might investigate to gain further insight. With that broader context in mind, Paul also suggested possible sources of empirical data, which included interviews with Chinese undergraduate students and their teachers and, for triangulation, students’ writing, teacher feedback, and observations of classroom interactions. As possible venues for publication, Paul mentioned a few journals that would likely be interested in publishing qualitative studies on the topic.

During that first meeting, as the team’s shared vision for the project came into sharper focus, the discussion of plans and procedures became more practical and open ended. What began as a typical teaching scene – with Paul speaking in the front of a classroom to his students sitting in desks and taking notes – shifted into a discussion more akin to an open forum in which the team deliberated over specific design-related questions. At times, Paul posed open questions, such as, “How many teachers should the team plan to recruit?” and “How might the interviewer’s ethnicity or language background affect the way interviewees respond to questions?” These open questions facilitated decisions about practical matters of design – for example, capping the number of teacher recruits at ten and having Juval, the most experienced researcher and a native speaker of English, conduct the most linguistically demanding interviews.

At other times, the doctoral students posed their own open questions. For example, Jianing asked if observing a classroom lecture as part of data collection would yield insight into students’ learning experiences. Juval added that, from a design perspective, scheduling observations during periods of student interaction would likely be a challenge. In response, Jianing suggested visiting a classroom workshop or student-teacher conference where rich interactions were likely to occur – a suggestion Paul found compelling and worth including in the International Review Board (IRB) protocol. During this deliberative process, Paul was positioned as a collaborator with whom team members could work through meaningful issues, rather than as the sole knowledge broker.
Challenge 2: Assigning roles and responsibilities

Once the team had a clear sense of purpose, Paul left it up to the doctoral students to develop research materials. Paul asked Juval, who already had experience preparing an IRB protocol, to be in charge of managing documents for the IRB review, such as the application narrative, recruitment letters, and interview guides for teachers. Juval also created a shared wiki space in which to store and distribute the various materials needed for IRB. The other team members – Jing, Jill, Jianing, and Taimin – took charge of drafting interview guides and a background questionnaire for the students in English and in Chinese. To manage the workload, two subgroups were formed: Jing partnered with Jill while Jianing worked with Taimin. Both pairings included one person from Mainland China and one from Taiwan, which, as Paul had hoped, created opportunities for the students to negotiate linguistic differences.

These roles were somewhat fluid, however, as team members collaborated with one another depending on who was available at any given time. Additionally, Jing assumed the unofficial role of coordinator among the Chinese-speaking doctoral students, owing in part to her seniority and prior experience with research. She became a go-to person for defining and distributing tasks, setting deadlines, and managing materials. The doctoral students carried out the collaborative process remotely, relying on Google Docs and its embedded chat function to communicate in real-time while revising. They also used email to exchange progress summaries. While the students initially relied on Google Docs and email as a way of managing their busy schedule, the process at times seemed less efficient than synchronous communication might have been. The team later incorporated some face-to-face meetings to address important editing issues involved with nuances of translation and the structuring of interview scripts.

When the doctoral students felt they had made satisfactory progress on a portion of the materials, they requested feedback from Paul. Managing the feedback and revision cycle proved difficult for the doctoral students. Paul’s copious feedback, which was delivered over email and through the comments feature in Google Docs, addressed a range of issues and created frequent needs among the doctoral students to confer and revise collaboratively. The struggle to work with the feedback was an important part of achieving the multiple objectives of the collaborative project. For the doctoral students, the struggle reflected a need to learn to write and arrange effective qualitative interview questions, which called for careful consideration of the many ways their questions might be interpreted by different audiences. From Dan’s perspective, the struggle with feedback meant the students were internalizing Paul’s expectations and, through collaborative negotiation, using approximations of those expectations to provide feedback to one another, thus enacting a distributed, or ‘trickle down,’ form of mentoring.
Challenge 3: Working from varied linguistic and cultural perspectives

Another difficulty was the lack of prescribed roles and responsibilities, particularly surrounding the varied linguistic and cultural backgrounds of the research members. Juval, for example, was unsure whether he could provide commentary on the Chinese doctoral students’ work because he did not share the same background with those team members. His reticence stemmed from the perception that, as an outsider of that group, he may not have the appropriate cultural context in which to understand why or how a particular question was asked (see McKinley, this volume, for related discussion). As a result, Juval elected not to comment on the work of his colleagues – a significant choice because Juval had the most familiarity with Paul’s feedback style. Similar uncertainties about roles and responsibilities were also felt among the Chinese-speaking ‘insiders’ of the group. For example, as speakers of a Taiwanese variety of Chinese, Jill and Taimin wondered to what extent they could comment on interview questions that were drafted for an audience that was likely to speak a Mainland variety of Chinese. Feeling somewhat limited in their ability to contribute, Jill and Taimin were sometimes unsure of the best way to support the team’s progress.

Challenge 4: Recruiting participants

The recruitment process was complex and intensive, as it involved multiple stages and ongoing communication with prospective participants. Additionally, recruitment posed a serious challenge to the study as a whole when it became evident that the team would be unable to recruit the population of students they had originally targeted. Ultimately, these obstacles created opportunities to consider ethical aspects of the study and to grapple with constraints within the research environment.

One issue that arose during correspondence with prospective participants was the study’s focus on so-called ‘struggling’ students as defined by the students’ own writing teachers. After a student emailed Jianing requesting additional information about the study, the team deliberated over how to share information with the student honestly, while also avoiding the stigma associated with language proficiency. Jing pointed out that, while the team should be forthright in their correspondence with the student, there did seem to be leeway in the amount of detail the student needed to know and the team was obligated to provide. Juval proposed relying on language from the IRB protocol, which suggested that, in a reply to the student, Jianing should emphasize the team’s interest in the general experiences of Chinese students rather than their struggles. Juval also noted that he had used similar language when communicating with teachers, and he expressed the importance of having a shared script across all correspondence with prospective participants.
Despite the meticulous study design and repeated attempts to recruit via email, only two students volunteered to participate, neither of whom seemed to struggle with their writing. In response to this unexpected problem, which threatened to derail the entire study, the team discussed alternative recruitment strategies. For example, after Paul called a team meeting to debrief, members acknowledged that the Chinese undergraduates they aimed to interview were a vulnerable population and thus more difficult to recruit than initially expected. To help assuage fears among potential recruits, the team decided to visit composition classrooms in person, believing that students might feel more inclined to participate in the study if they heard directly from the researchers. Moreover, team members thought it would also be a good idea to communicate with those students in Chinese to reflect the researchers’ similar linguistic backgrounds and shared stake in the project.

Despite these efforts, however, no students within the desired demographic volunteered. In the end, the team decided to broaden the study significantly by extending an invitation to all Chinese-speaking students, rather than only to those who were facing language issues. While the decision to broaden the study was a success (resulting in 23 student participants), the shift also created an imposing challenge during the data analysis, which is described in the next section.

**Challenge 5: Repurposing data**

Widening the pool of participants had a direct impact on the ways data were used. An immediate issue the team had to address was how to approach the two data sets in light of the study’s shift in purpose. Under the original plan, the data sets were strategically linked to examine the relationship between the teacher’s perceptions of ‘struggling’ Chinese students and those students’ actual experiences. After broadening the participant pool, however, the two data sets did not seem to fit together any longer – the teachers who were interviewed exclusively discussed issues related to students with severe language issues, while the student participants did not consider themselves to be struggling with language issues. After much deliberation, the group decided to use the data sets separately, which opened up the possibility to repurpose the data in unplanned ways. Juval, for example, given his interest in teacher identity, worked with Paul to develop a spin-off project that analyzed the teacher interview data from a different angle – namely, second language writing teacher identity (Racelis & Matsuda, 2015).

Despite original project plans, repurposing the data from student interviews became the primary focus for the remainder of the project, as team members worked to identify new research questions that could guide their analysis. During meetings, Jianing and Jill, who held primary responsibility for analyzing the Chinese transcripts, shared coding schemes they developed,
which were then opened up to critique by the rest of the group. Ensuing discussions typically worked to highlight areas where the coding schemes broke down and offer suggestions for alternatives that would account for the data in more effective and illuminating ways. As Jianing and Jill focused on data analysis, Juval and Taimin focused on locating relevant scholarly literature. During team meetings, Juval and Taimin would listen carefully for key issues that came up in discussions of the data, and, between meetings, they would visit the library to compile and organize a cache of relevant articles and books. Juval and Taimin shared this evolving body of literature during team meetings and suggested how it might inform the data analysis. This dialectic between the coding of data and organizing of literature was characteristic of many of the team meetings during the analysis phase of the collaboration.

Approaching data analysis in this exploratory manner was a challenge for some. Team members indicated feeling adrift without a clearly defined research question with which to orient themselves. Analysis was also difficult because the interview data carried vestiges of the study’s original purpose. Data from student interviews, for example, often referenced issues no longer of concern for the research team. The difficulty, then, was reconceptualizing data that were gathered under different assumptions (see Rose, this volume, for related discussion) – a difficulty that surfaced regularly. Additionally, team members found progress to be slow moving because the concurrent processes of analyzing data and locating scholarly literature were reliant on one another. If progress stalled on one, it stalled on the other. As a result, team members felt a tension between the need to advance their portion of the work and the need to work through difficulties collaboratively.

Despite progressing slowly at times, the large and multifaceted project lent itself to various spin-off projects that were related to the team members’ individual research interests. For example, some members attended conferences to present various findings from the collaborative project. Additionally, two members developed individual dissertation projects using the shared data – projects that Paul suggested because of the amount of time and effort students were devoting to the collaborative project.

Team members were sometimes unsure whether and how data could be used for new purposes. They also wondered whether, when, and how to obtain permissions from other members. When a team member submitted a conference proposal before consulting the entire team, Paul emailed each member separately and, in a later group meeting, discussed the conventions involved with a repurposing of this kind. For example, Paul pointed out that members should always contact collaborators prior to using the data for another purpose. Paul also explained different conventions for acknowledging collaborative work, including listing others as co-authors or ‘with’ authors, or mentioning them in the acknowledgments.
Implications and suggestions for researchers

As collaborative qualitative research, the project we have described was complex and messy. To manage complex research projects with multiple purposes and people, principal investigators or team leaders need to know the characteristics, interests, strengths, and learning trajectories of each of the team members. Additionally, the team leader needs to have the ability to identify or carve out projects that are important enough for the members to take seriously and feel ownership over, but also remain flexible so that the team members are able to change the focus and structure of the project if needed. Establishing projects in which both students and mentors have a clear stake is important to the success of collaborative learning at the graduate level. The team leader also needs to be able to anticipate various twists and turns, while allowing the members to struggle through them before intervening. In the case of working with graduate students, it is also important to engage in the ongoing reflection and reassessment of students’ strengths, limitations, developments, and shifting roles, as well as the mentor’s own developments and limitations. Because of his prior experience, Paul was able to design and manage this complex collaborative project involving multiple research students at different stages of development. Those who are new to collaborative mentoring may need to start small, especially in working with less experienced team members (see Simpson & Matsuda, 2008).

Because of the complexity of the project, team members needed to constantly negotiate their objectives and roles. Much of what was negotiated fell outside the scope of issues easily described in the methods literature. An important, and particularly troublesome, aspect of such negotiation was the practice of inductive data analysis (see Briggs, this volume). Team members engaged in the interpretive and recursive work of building up and breaking down hypotheses to establish claims that withstand critique and account for variations in the data (Agar, 1986). This process – the movement from observations of interesting phenomena to articulations of supportable claims – proved to be one of the most difficult stages of the research project. And while constructing and critiquing data-driven hypotheses was indeed a challenge in itself, it was made even more difficult as the team situated their findings amidst relevant scholarly literature. The twofold objective of establishing claims and situating them in contemporary scholarship was an elusive, moving target that reflected the dynamic nature of the research environment. By engaging in this complex dialectic – between data and claims and between claims and literature – the team members found themselves ensconced in one of the central activities of real research.
Engaging in a real research project created a host of opportunities for the doctoral students to take control of their own development as they devised various strategies to cope with emergent challenges. Realizing that they could not always turn to Paul for help, team members turned instead to each other for support, which created opportunities for them to play different roles as needs arose. Team members also turned to published studies, not just as sources of new findings but also as models of how other researchers responded to methodological issues similar to the ones they themselves were facing. In short, the doctoral students were learning how to learn – as collaborative contributors in the community of practice.

This collaborative project not only helped the graduate students develop as researchers but also as potential mentors. During the data analysis phase, and after Jing, who had been serving as a team leader, had completed her dissertation and moved on, Juval began to play a more central role as a leader among the graduate student group. When Paul came to a meeting late, Juval started leading the discussion on the data and continued to lead the group even when Paul came in and quietly sat in the corner of the meeting room. Coming to the meeting late was Paul’s deliberate attempt to create an opportunity for leadership to emerge, and seeing that it had worked, Paul continued to practice strategic tardiness. The graduate student members were also aware of the emerging leadership and at times expressed their appreciation for Juval’s effective leadership.

As our collective experience made clear to us, unexpected obstacles are all but inevitable, and a mark of a strong research team is an ability to flex together in accordance with shifting circumstances. In many ways, it is the process of responding to the unexpected that makes learning through real collaborative research uniquely effective.

References


Part II

Responding to problems during data collection
Adjusting to contextual constraints
Methodological shifts in local research projects

John Hedgcock and Heekyeong Lee

Introduction

As researchers in any field can attest, a carefully planned research design is essential to the success of any empirical investigation. Identifying and framing a research problem, sharpening a purpose, and formulating meaningful research questions lay the necessary groundwork for determining methods appropriate for conducting productive research in a given context (Brown & Rodgers, 2002; Paltridge & Phakiti, 2015). Ideally, we design research projects with contextual constraints in mind, adopting data collection methods and analytic procedures that reliably target the relevant constructs. Despite care and forethought in laying out a sound design, however, unexpected circumstances (and discoveries) can emerge, necessitating adjustments to research plans as data collection gets under way.

When impediments to completing a planned procedure emerge in the research setting, and when the data collection process itself reveals limitations in the research design, researchers may be faced with starting again from scratch or abandoning the project altogether. On the other hand, researchers may have the option of pursuing their research aims and addressing their original research questions by adopting alternative data collection methods and strategies, a decision that may require reformulation of subsequent phases of the research process. When circumstances allow for mid-project adjustments, researchers need to consider how contextual obstacles may reshape not only the research design but also the epistemological fundamentals of the study, such as the research questions and target variables. This chapter describes similar circumstances that arose in the course of collecting and analyzing preliminary data for a small-scale investigation that we undertook on our campus in 2013 (Hedgcock & Lee, 2014a; 2014b). In presenting our account of the decisions that we faced and how we acted on those decisions, we hope to provide fellow researchers with insight into the challenges of conducting empirical research in a small institutional setting. This chapter similarly
aims to recommend strategies for responding productively to the need to shift gears (and even to reconceptualize research problems) once an investigative process has begun.

**Our study**

Interest in our MA students’ socialization and apprenticeship into applied linguistics – and more specifically into the practices and value systems of language teaching (LT) – initially motivated our effort to explore how they observed, used, and internalized genre knowledge. The project was partly inspired by our ongoing concern for students’ difficulties with interpreting, learning from, and reproducing high-stakes written products (e.g., forms of critical and synthetic writing such as book and literature reviews, research papers, and pedagogical tools) and oral performances (e.g., microteaching events, presentations).

Our study initially endeavoured to examine how students, all pre- and in-service language teachers, perceived and appropriated the prototypical written and oral genres featured in their coursework. We began our project by completing the preliminary steps conventionally promoted in approaches to conducting empirical social science research: (a) topic selection → (b) literature review → (c) identification of research problem(s) → (d) identification of research purpose → (e) formulation of research questions → (f) selection of research methods → (g) framing of research context and method (Brown & Rodgers, 2002; Paltridge & Phakiti, 2015). In the midst of clarifying our purpose and formulating research questions, we realized that the context for our project was very localized and that our findings could produce concrete implications for our practice as teacher educators. As such, the project could be classified as a type of action research (AR), as its aims included undertaking inquiry “to bring about positive change and improvement” in the local setting and “[to] generate theoretical as well as practical knowledge about the situation” (Burns, 2015, p. 187). The activities that we discuss in this chapter chiefly involve early phases of the AR sequence as shown in Table 6.1.

These interrelated phases naturally converged with the *a priori* research sequence guiding our overall design, leading us to recognize that our process would not be linear, but rather (re)iterative and overlapping. Guided by principles of mixed-methods research, we began to re-envision our descriptive, exploratory study as both concurrent and sequential in its design (Creswell, 2014; Hashemi & Babaii, 2013). That is, our study featured overlapping stages of quantitative and qualitative data collection, as well as recursive iterations of data analysis and interpretation. Our procedures reflected a descriptive, data-driven approach
that began with inspection of our data sources, identified patterns of meaning, and then led to refinement of our global research questions (Riazi & Candlin, 2014).

Informed by our experience with our participant population, we formulated the following research questions:

1. How do candidates perceive the benefits and limitations of a reflective orientation to Language Teacher Education (LTE)?
2. How do candidates perceive the contribution of implicit and explicit genre awareness to teacher development?
3. How do teacher candidates evaluate the effectiveness of genre-oriented instruction in developing their professional knowledge and skill?

Well in advance of starting data collection, we requested and secured Institutional Review Board approval for our study, completing the necessary steps to ensure and document the ethical treatment of our participants and the data that they would supply. Through convenience sampling, we recruited 58 participants from four student cohorts over the course of two academic years. Constraints affecting our research design and method involved aspects of our research setting – our MA program at a small, private institution in the United States. Many (if not most) of our potential participants had been, were, or would be our own students, a condition that could compromise the quality and reliability of our participant-generated data (see Galloway, this volume).

The instruments that we initially devised for our study included (a) an online questionnaire eliciting quantitative and qualitative data reflecting participants’ perceptions of their academic socialization and (b) a set

| Table 6.1 Early phases of the action research sequence (based on Burns, 2015, p. 189) |
|----------------------------------|--------------------------------------------------------------------------------------|
| • exploration                   | identification of areas for investigation                                           |
| • identification                | fact-finding to refine ideas                                                        |
| • planning                      | development of an action plan                                                       |
| • data collection               | selection and deployment of data-gathering techniques                               |
| • analysis and reflection       | simultaneous scrutiny of, and reflection on, emerging data                          |
| • hypothesis formulation and speculation | development of initial predictions and explanations based on data               |
of semi-structured interview prompts designed to generate more detailed information about participants’ perceptions. Our original plan was to conduct interviews in one-on-one sessions with self-selected volunteers (a subset of the total participant sample) following preliminary analysis of questionnaire results. In addition to eliciting demographic information, our anonymous survey comprised 18 parallel-structure belief statements to which respondents assigned agreement scores on a six-level, Likert-type scale. The following items offer a representative sampling:

- As an undergraduate, I earned high grades (As and Bs) on most of my academic assignments. (Item 6).
- I learn a lot from the comments and corrections that my instructors write on my assignments. (Item 16).
- After getting instructor feedback on my writing, I learn things that I can apply to future writing tasks. (Item 18).

6 = Strongly agree . . . 1 = Strongly disagree

To capture participants’ self-appraisals of their academic literacy strategies and skills (i.e., reading, composing, navigating the discursive landscape of graduate school), our questionnaire also included controlled-response items requiring participants to rate their familiarity with a menu of written and oral genres using the same six-level scale. The final section of the survey invited participants to describe academic assignments, evaluate their successes and failures, and describe their implicit and explicit knowledge of relevant genres. In line with an “embedded mixing strategy” (Creswell & Plano Clark, 2011, p. 67) and an explanatory-sequential approach (Edmonds & Kennedy, 2013, p. 164), our original vision was to analyze and interpret the first iteration of the survey results and then to integrate recurring themes and salient trends (i.e., high-frequency response patterns) into the process of eliciting richer qualitative data collected through semi-structured interviews, as illustrated in Figure 6.1.

![Figure 6.1 Original explanatory-sequential research plan.](image-url)
The research problem: Shifts in conceptualizing elements of the research process

Collecting and analyzing survey data from our first participant cohort produced insights into our research design and shed helpful light on our constructs. For example, survey responses suggested some mismatches between participants’ self-appraisals of their literacy skills and their actual success in completing high-stakes academic assignments. These mismatches suggested that the relevant questionnaire items were perhaps insufficiently sensitive and may have elicited less-than-critical and candid responses. They further led us to question whether our participants would respond openly and honestly when queried about their academic successes and challenges in semi-structured interviews.

Also telling in our review of survey responses was participants’ tendency to associate particular assignments with particular courses and professors – including us. We discovered that, owing to our dual (and potentially incompatible) roles as researchers and professors, participants might understandably perceive our research endeavour as a program evaluation exercise (see Galloway, this volume; McKinley, this volume). This realization raised ethical and methodological concerns for us. First, although our invitation and informed consent documents strongly emphasized that students’ participation was purely voluntary, we recognized that some might have sensed pressure to take part based on the simple fact that we were their professors and thus in a position of authority. Conducting research with our own students as participants also placed us in a peculiarly awkward position when it came to determining how to compensate volunteers for their willingness to participate, their time, and their effort. Second, despite written affirmation of our commitment to preserving the anonymity and confidentiality of participants’ contributions, qualitative responses from the survey and one-on-one interview data could potentially reveal individual participants’ identities. After all, our research activity took place in a small, intimate institutional setting where student cohorts form strong and sustained social relationships and where faculty and students interact frequently inside and outside the classroom.

Further compromising our research process was the perennial difficulty of recruiting a sufficiently high number of participants to take part in one or both phases of our study (see Seals, this volume; Coxhead, this volume). Facing heavy course loads and a relentless series of deadlines, students understandably found it challenging to find time to complete our survey, let alone to schedule one-on-one interviews. Our survey invitation yielded a relatively encouraging 42% response rate, 58 completed surveys, and a 16,823-word corpus of constructed responses to open-ended questionnaire items. Nonetheless, we became concerned not only about securing a suitably robust number of potential interview participants but also about how fully our students would respond to interviews in which we (their professors)
would ask them to reflect candidly on their graduate school experiences. We thus confronted the unlikelihood of eliciting authentic reflections and honest opinions from our student participants. Finally, constraints beyond our control also undermined our sample size and the viability of our study: An unexpected decline in enrolment and a delay in our start date owing to the Institutional Review Board calendar. At this medial juncture in our interwoven data analysis and collection processes, we faced a moment of decision.

**Overcoming the obstacle: Adjusting to shifts in the research process**

Having invested considerable time and effort in the project, we were determined to pursue our research goals and to make productive use of the survey data that we had collected and analyzed. To forge ahead, however, we would need to modify our research design by considering (and quickly adopting) a more effective data elicitation method. First, because of the practical flaws in our intended plan to conduct semi-structured interviews, Heekyeong proposed focus group (FG) discussions as an alternative to this method. Initiating FG discussions, she reasoned, would allow us to elicit a greater quantity of more detailed information about how students understood and perceived the constructs featured in our study (i.e., reflective practice, genre knowledge and awareness, genre-oriented instruction). As Kamberelis and Demitriadis (2008) observed, FGs “are efficient in the sense that they generate large quantities of material . . . in a relatively short time” (p. 397). A more substantial advantage of the FG method involves the enhanced likelihood of eliciting uninhibited ideas, beliefs, and opinions from participants in a non-threatening environment. Because of “the synergy and dynamism generated within homogeneous collectives,” FG sessions “often produce data that are seldom produced through individual interviewing . . . and that result in especially powerful interpretive insights” (Kamberelis & Demitriadis, 2008, p. 397). We thus revised our research plan to reflect the design illustrated in Figure 6.2.

![Figure 6.2 Revised explanatory-sequential research plan.](image_url)
To stimulate focused but naturalistic conversation among participants, we constructed a discussion guide composed of prompts such as the following:

1. Select and describe two or three written or oral assignments that you have completed (or are currently completing) during your graduate program.
   a. Why do you think your instructors required these assignments?
   b. Discuss how you identified expectations for the assignments and how you completed the tasks.

2. What kinds of academic and/or professional products (written texts, oral presentations, and so on) do you think you are skilled at developing?
   a. How did you learn about the particular features of these forms?
   b. What strategies did you use to produce these forms?
   c. Did you make mistakes along the way? What did you learn from them?

Although we composed each of the two prompts with a specific theme in mind (e.g., genres and their purposes, familiarity with genres), we hoped that the intentional topical overlap would generate discussion about the full range of constructs embedded in our research questions.

As a means of maximizing participation, we scheduled FG sessions in the early evening and invited prospective volunteers for a casual meal, which we were happy to cater as partial compensation for students’ time and effort. The shared meal set an informal tone and generated a smooth flow of natural interaction among peers. Given the institutional and social constraints that we have described, we departed from the convention of facilitating FG discussions ourselves (Hennink, Hutter, & Bailey, 2011). Instead, after the meal, we divided participants into subgroups of three to five members and asked one student per group to take charge of the discussion prompts and digital recording device. We then previewed the discussion guide, reviewed the research goals as outlined in the invitation to participate, and described the steps that we would take to preserve the anonymity of participants’ recorded contributions (e.g., by assigning numbers to each speaker in the transcription process). We further emphasized that our research purpose was neither to evaluate their academic performance nor to elicit their appraisals of our MA program. As we wanted to promote frank, uninhibited interaction, the subgroups then moved to separate locations to record their discussions independently. Each FG operated its own recording device, managed its own time, and concluded its session once everyone agreed that the topics in the discussion guide had been satisfactorily covered.

In reviewing and transcribing our audio recordings, we noted several behavioural patterns that supported our decision to abandon interviews in
favour of FGs. After a preliminary analysis of the two sets of FG data, we recognized that the FG format had likely generated not only *more* data but also that it may have stimulated the production of *more valid* data. Our corpus presently amounts to eight conversations ranging from 30 to 54 minutes in length; these samples total 5.48 hours (330 minutes) of audio data, yielding 44,857 words of transcribed speech. More meaningfully, the content and tenor of the conversations suggested that the FG format had created a low-stress, if not relaxed, environment that encouraged participants to express themselves freely, to “uncover unique perspectives” on the issues at hand, to portray group processes, and to represent “how an issue is discussed” (Hennink et al., 2011, pp. 136–138). Examination of the recordings and transcripts revealed how participants influenced one another as themes emerged and as group members interacted cooperatively. The following FG extract, in which participants discuss their challenges with literature review assignments, exemplifies this kind of joint co-construction:

S3: Going back to the literature review, though, what [was] freaking me out so much is . . . ’cuz I had never, ever, even talked about, *I never even knew what a literature review is like*, the technical term, until . . . and we got the piece of paper. I was like, “Okay, this needs to be included in the paper, which is due next week . . . Whatever . . .”

S2: But you know what? Actually, we have read a ton of literature reviews up to that point, but they never called our attention to it so I never realized that there was a literature review . . .

(Extract 7, FG01)

We observed extensive evidence of such reflexivity throughout the FG conversations: “What speakers say keys people to construe the context in certain ways while, at the same time, people use how they view the context to interpret what is said” (Gee, 2014, p. 224). Further, because participants shared such extensive contextual and experiential knowledge, had already developed rapport, and construed their interlocutors as their peers, they readily self-disclosed and eagerly built on one another’s contributions (unlike what we would have expected in semi-structured interviews).

As we came to appreciate these advantages, our FG data became the centrepiece of our data collection and subsequent analysis. Reviewing the data also led us to revise our research questions in response to the salient themes that recurrently emerged in the FG sessions. For instance, one of the research questions that we posed in our Institutional Review Board application queried the contribution of explicit and implicit genre awareness to teacher development. The reflections that participants shared during FG interactions revealed that the construct elicited did not actually entail *development*, but rather participants’ *perceptions* of their developmental processes. We therefore reformulated that research question to read, “How do candidates perceive the
contribution of implicit and explicit genre awareness to teacher development?” A further insight resulting from the data collection and analysis processes was the need to complete the action research loop by sharing key descriptive findings with our participants. The students who contributed to one or more phases of our project expressed sincere interest in learning about our findings and how they might influence our future practice. We were naturally gratified to learn about students’ investment in our investigation and grateful for the reminder that action-oriented research should serve formative purposes (Burns, 2010; 2015).

**Implications for researchers**

The evolving research process examined in this chapter offered helpful methodological and ethical lessons that we hope will also be instructive for fellow researchers. In planning and executing our investigation, we confronted practical constraints that precipitated changes in our thinking about our constructs and research design. Circumstances also necessitated the pursuit of a methodological alternative that likely yielded richer data than our original plan would have elicited. Not only did we encounter difficulties in recruiting and retaining a robust participant sample, but we also had to take steps to avoid threats to the validity and meaningfulness of our qualitative data. As we confronted these challenges, the shape and substance of our original research plan necessarily underwent shifts that thankfully resulted in productive outcomes (see Figures 6.1 and 6.2). These outcomes included clarification and narrowing of our research questions, the decision to replace interviews with FG discussions as our primary data collection method, and the generation of robust qualitative data.

At the level of concept and design, some of the lessons that we have learned through this research process align with those articulated elsewhere in this volume. Others relate specifically to our mixed-methods investigation:

- It is wise to build flexibility into research designs and timelines to allow for unanticipated circumstances, obstacles, and shifts (see Rose, this volume). Indeed, it can be very useful to anticipate potential course corrections in the planning process so that researchers can respond nimbly to discoveries that occur once the process is in progress.
- As they exercise flexibility in the planning and execution phases, researchers should also maintain a clear focus on their research goals and questions. Although researchers can benefit from adjusting plans and sharpening research questions, they can also become vulnerable to ‘mission drift’ and potentially losing sight of their original purposes and the target niche for their work.
- Researchers can keep a project on track by systematically reviewing how an unanticipated change (e.g., participant attrition, low response
rates, challenges related to working with students as participants) can influence instruments, constructs, and research questions.

- Investigators pursuing mixed-methods designs may find that formulating mixed-methods research questions is particularly challenging, as a single study must be guided by both quantitative and qualitative questions. Moreover, “a mixed methods research question necessitates that both quantitative data and qualitative data be collected and analyzed either concurrently, sequentially, or iteratively before the question is addressed” (Onwuegbuzie & Leech, 2006, p. 483).

In navigating the methodological crossroads encountered in our study, we came to appreciate the rewards and responsibilities of mixed-methods inquiry, as well as the value of selecting the most productive data collection tools available. Our decision to elicit data in FG discussions rather than in semi-structured interviews required adaptations that, in our research setting, enhanced the quality of our participants’ contributions. The most helpful discoveries made in our process included the following:

- Our FG sessions fulfilled their “synergistic potentials” by producing co-constructed data “that are seldom produced through individual interviewing . . . and that result in especially powerful interpretive insights” (Kamberelis & Demitriadis, 2008, p. 397).
- Similarly, as our FGs consisted of participants who had developed rapport as members of their student cohorts, their interactions took the interpretive process “beyond the bounds of individual memory and expression,” encouraging them to mine “historically sedimented collective memories and desires” (Kamberelis & Demitriadis, 2008, p. 397).
- Although FG discussions are typically led by a facilitator (Hennink et al., 2011), we found it preferable to acquaint our participants with our discussion guide and then to retreat from the scene. Because our students were fully accustomed to collaborative classroom tasks, we felt that participant-led FGs would be more ecologically valid in our context. Further, we were confident in their ability to conduct targeted, purposeful discussions on their own.
- An indispensable tool in generating FG data, a systematically designed discussion guide should methodically activate cognition, build rapport among participants, generate broad and specific types of data, and lead to closure, as in the funnel design proposed by Hennink et al. (2011). It is also essential to ensure that participants fully understand the discussion guide, have an opportunity to seek clarification about its content and purpose, and feel comfortable operating recording devices.
Any empirical study, no matter how thoughtfully planned, can present researchers with unexpected stumbling blocks at any stage of the research process, including data collection and analysis. We managed to persevere in our descriptive investigation thanks partly to the built-in flexibility and iterative nature of our mixed-methods, action-oriented framework. This research design enabled us to transform challenges into opportunities by making principled adjustments to our data collection methods and research questions without fundamentally altering the purposes of our inquiry.

References

Chapter 7

Dealing with participant attrition in longitudinal studies

Corinne A. Seals

Introduction

One of the tenets of the social turn in applied linguistics is that language learning cannot be separated from learners’ individual lived experiences. This has become an even more firmly established belief since the development of identity-focused research, which has grown rapidly in popularity over the past two decades (e.g., Block, 2007; Norton, 1997; 2000; Pavlenko & Blackledge, 2004). Due to the strong social and human emphasis of much sociolinguistic research, researchers now tend to draw upon ethnographic methods to study the experiences of language learners (e.g., Duff, 2008; 2013; Menard-Warwick, 2009; Piller & Takahashi, 2006). Ethnographic methods focus on “the behaviors (including the linguistic behaviors) of the members of a particular community by studying them in naturally occurring, ongoing settings, typically while they participate in mundane day-to-day events” (Dufon, 2002: 42). More specifically, ethnographic methods inherently aim to provide a rich description of some aspect of a community and mix both emic and etic perspectives, the former of which is “the culturally specific framework used by the members of a community under study for interpreting and assigning meaning to their experiences” and the latter of which is “based on the academic frameworks, concepts, and categories of the researcher’s discipline” (Dufon, 2002: 42).

As one could imagine, there are several notable challenges in pursuing longitudinal studies, such as an ethnographic approach to data collection. There are often challenges regarding securing access to a location for long-term use with participants, whether it be checking in, testing, or regular observation (see, e.g., Kubota, this volume). Additionally, all longitudinal studies, by their nature, take a long time to complete and thus require ongoing resources to support them. One of the most likely and frustrating challenges to overcome, however, has to do with participants themselves. Longitudinal studies ask a great deal of participants in terms of their time and extended commitment. Therefore, it is not uncommon for participants to electively end their participation in the study early. How can researchers
respond when facing participant attrition during a longitudinal study? In this chapter, I will illustrate this particular challenge by drawing upon my PhD dissertation research (Seals, 2013a), which I conducted in 2010–2013 using ethnographic methods, though the challenges discussed in retaining participants could be applied to any type of longitudinal study in applied linguistics.

**Background to the study**

In 2010, I began the initial pilot research for what was to be the basis of a qualitative applied linguistics study running through the beginning of 2013. The goal of this research was to investigate the connections between language and identity for heritage language–speaking children of Russian and Ukrainian origins, as well as how a multilingual identity is performed within a mainstream heritage language program. To conduct this research, I received permission from a rural public primary school in the Western United States to conduct a longitudinal study with their in-school heritage language program. While the program included Russian, Ukrainian, and Spanish heritage language speakers, I chose to focus on the Slavic languages in order to keep the data set and analysis manageable.

When I began the initial pilot research for this study, I started with informal ethnographic observations twice per week for four weeks over the course of two months. During this time, the students, teachers, and administrators became comfortable with my presence, and I learned the general structure and organizational operations of the school. Following this, my ethnography extended formally to two of the heritage language students’ main classrooms (given the pseudonyms Darya and Tolya), where the ethnography was conducted for one hour per day every day for four weeks. After that time, I continued to informally visit the school to keep in touch with the administrators every few weeks, leading up to the fall of 2012. During my visits, I would also frequently see the teachers and focal students who would greet me and share stories from time to time. It was during the pilot phase that I was able to identify and become familiar with the focal students as well as to get to know the workings of the program. I then collected the extended data via ethnographic methods during the 2012–2013 school year. This is where the challenges truly set in.

**The research problem**

When I officially began my ethnography in 2010, I initially selected five students as key, focal participants. The participant criteria that I laid forth included several factors for comparative purposes. First, the students had to be currently enrolled in the heritage language classes so they were actively adding to their heritage language abilities. Second, they had to be either
generation 1.5 or generation 2 immigrants within émigré families from Eastern Europe, and they had to have at least one parent who grew up in the home country, so the home culture would still be prevalent in the home, and thus the heritage language would be more likely to be used at least by the parent(s) in the home. Furthermore, Ukrainian families were chosen as my primary focus because the presence of Ukrainian heritage language programs in the United States is rare, with only 35 public community Saturday schools in the United States and no known in-school programs other than that at the school in which my study was conducted (Seals, 2012). The final criterion for participant selection was they had to speak Russian and/or Ukrainian at home as a heritage language since these were the languages of study, and I was investigating the multilingual identity negotiation of children who have at least some native abilities in these languages.

Of the students enrolled in the school at that time, five fit the profile that I was looking for: Darya, her twin sister Vika, Elena, Andre, and Tolya (all pseudonyms). After receiving approval from the Institutional Review Board to move forward with this project, I requested permission from the families of these children, their teachers, and the school administrators, all of whom approved. Then my initial informal observations and basic interviews with the students began, as I sought to establish rapport with them and to learn the environment of the school before beginning the recorded ethnographic investigation.

However, it was after this initial phase that my challenge as a longitudinal researcher really arose. The heritage language program underwent a marked change in the 2011–2012 school year, during which time a good many of the heritage language resources were removed at the behest of the administration. This included the heritage language students’ school bus, where they reported spending much time speaking heritage languages with each other, and the Language Enrichment Program, where the heritage language students had the opportunity to become experts in their home language(s) and to teach them to other students.

When I returned to school at the start of the next school year in September 2012, after the students’ summer vacation was over, the number of changes that the school had implemented regarding the heritage language resources stunned me. Due to continued budget cuts at the state, district, and school levels, the school administration decided to scale back multiple social services program offerings. One of the areas identified to receive cuts were the programs housed in the English Language Development Program, including heritage language resources. While the core English Language Development Program designed to increase students’ proficiency in the English language was maintained, other affiliated aspects of the program were altered or removed. First, the Home Language Program, focused on developing students’ heritage languages, which previously occurred twice per week for an hour each session, was reduced to once per week for 45 minutes each time.
Even then, the heritage language instructors felt that they had to petition vigorously to be allowed to keep any aspect of this program (Seals, 2013a). Additionally, the Language Enrichment Program, which allowed heritage language students the opportunity to act as experts in their home languages by teaching their heritage languages to their monolingual English-speaking peers, was cut in its entirety.

Reflections of these program cuts occurred throughout the entire school district. Of the multiple elementary schools, one middle school, and one high school that are included in the school district, the only school that kept even a basic English Language Development Program for the 2012–2013 school year was the school in which I conducted my study. During the 2011–2012 school year, all heritage language resources were eliminated from all other schools in the district, and 2013 completed the elimination of resources from those schools for speakers of languages other than English, despite the presence of over 120 of the more than 4,000 students enrolled in the district (2.8%) being speakers of languages other than English at home. Of those 120 students, 50 attended my focus school where the total student population was only 300, making them 16.7% of the total student population – a significant amount (Seals, 2013a).

The total number of 50 students at the school who were speakers of languages other than English did not change for the 2012–2013 school year despite the increase of Spanish speaking students enrolled because there was a simultaneous reduction of Russian and Ukrainian speaking students. During the 2011–2012 school year, eight students were heritage language learners of Russian and some also were heritage language speakers of Ukrainian. It was projected at that time that there would be six of these students still enrolled for the 2012–2013 school year, as two would advance to the middle school. Additionally, at least two more Ukrainian students (younger siblings of current students) were set to begin their primary schooling, bringing the total number of anticipated participants to at least eight. However, because of the reduction of resources available for heritage language instruction, some of the families, who drove up to an hour to specifically have their children attend this school, felt that the investment of their time was no longer balanced by the language program rewards. Thus the actual number of Russian heritage language students for the 2012–2013 school year had reduced to three (including the new addition). Additionally, all of these students were now picked up and dropped off by their parents; they were no longer taking the shared heritage language student school bus. Thus, in addition to a rapid and unexpected decrease in participants for my research, the culture of the school had noticeably changed with a reduced heritage language presence.

As a result of the changes made to the heritage language program, some key focal participants were lost from the project. For example, Tolya and his family were no longer in attendance at the school, which also meant the
loss of his younger siblings who would have attended for the 2012–2013 school year. However, despite the losses, there was an addition to the participant pool in that existing participant Elena was joined by her younger sister Alla in 2011 and thus was there for the second phase of the study in 2012–2013. Notably, Elena and Alla are nieces of the then heritage language instructor, Vera, which provided them with extra family incentive to remain at the school. As a result, the only students to remain in the Russian heritage language program (the Ukrainian program having been cut) and in the 2012–2013 year of this study were Darya, Elena, and Alla – all generation 2 children, meeting all of the aforementioned criteria.

With the change in the structure of the heritage language program and subsequent participant attrition, I reflected upon many ethnographers’ advice that the nature of a qualitative ethnographic project is to constantly be willing to rework one’s methodology (e.g., Atkinson, Coffey, Delamont, Lofland, & Lofland, 2001; Bagley & Castro-Salazar, 2010; Becker, 1998; Grimshaw, 2001; Madison, 2012; Silverman, 2000). I knew that I could either despair and become overwhelmed at the two enormous and unexpected obstacles in my way, or I could find a way to overcome the problem.

**Overcoming the obstacle**

In deliberating my options, I considered several possible ways to address the participant attrition and structural program changes. One option was to expand the investigation to include the Spanish heritage language speakers as well. While this was an intriguing option, it would have meant an additional pilot study and investigation into this other aspect of the program. Additionally, it would bring my investigation back to a more traditional, broader look at heritage language education instead of a focused look at an understudied population. In order to maintain a focus on the Ukrainian and Russian heritage language speakers, I decided to change the investigation to a more thorough multiple case study investigation into the language education and practices of the three remaining students. So instead of an ethnographic approach focused solely on multiple children’s lived experiences at school, I applied for an IRB modification to instead investigate these three children’s lived experiences at home as well as at school. The result of this change ended up producing a very well-rounded, robust collection of data representing Darya’s, Elena’s, and Alla’s daily experiences with all of their languages, supplemented by relevant supportive findings with the initial participant group from 2010–2011. Additionally, I changed my perspective on the program changes, deciding to examine the events as a research advantage – tracking the results of the program’s existence and subsequent decline.

This change in the study provided some significant opportunities. A multiple case study (Merriam, 1998; Stake, 1995) investigates more than one participant in the same setting and/or types of settings; in the case of the
Dealing with participant attrition

In the present study, I explored the same school and two different homes, incorporating in-depth analyses of many different sources of data (Eisenhardt, 1989: 534–535). In this way, I was able to achieve a broad understanding of the participants’ daily lived experiences and a narrow understanding of their moment-to-moment interactional experiences by observing them “over time through detailed, in-depth data collection involving multiple sources of information rich in context” (Creswell, 1998: 61). The multiple sources of data and analysis methods from these case studies facilitated triangulation of the study, which means that the research findings were validated by checking them against various sources of data and multiple research perspectives, therein reducing the chance of bias (Aziz & Raof, 2008: 21).

In addition to the primary, original ethnographic location of the heritage language classroom, two other spaces at the school were also adopted to serve as ethnographic observation areas. First, the playground was a general area of observation where I could better understand interactions between the focal students and their peers. Additionally, because I was standing off to the side of the playground during recesses, the focal students, teachers, and administrators often chose that time to informally talk to me about various aspects of the school and dynamics between people there. This proved to be a useful area for collecting information about the activities that took place at the school as well as the people present there. Also, the students wore voice-recording watches during these times, which were regular-looking wristwatches that recorded audio files, so general conversations could be captured. The data collected proved to be generally useful in understanding the types and tenor of interactions (Halliday, 1985; Poynton, 1985) that the focal students had with their peers while on the playground.

Additionally, the primary classrooms that the focal students attended were areas of observation. At times, the students would wear their voice-recording watches in the classroom, and this gave some insight into the focal students’ participation patterns in their regular classrooms. One classroom in particular was the focus of my initial ethnographic observations when I began this study in 2010–2011. At the time, Darya and Tolya were in the same third grade classroom, and I observed their daily routines and work in that classroom using the same methodology that I later employed for the heritage language classroom. This initial mainstream classroom ethnography provided the data that enabled me to better understand the heritage students as learners in general before then continuing to understand them as heritage language speakers.

Regular recordings also took place within Darya, Alla, and Elena’s homes. In the interest of minimal interference in the students’ daily lives and interactions with their families, the students brought home the voice-recording wristwatches and used these to collect data, without me actually being present (based on Seals, 2013b). The students learned how to operate the wristwatches and found an area of their home where they could...
collect family interactional data with multiple family members on a regular basis. They were instructed to turn on the voice-recording function of the watches when they were around their family doing regular daily activities. For Darya, this ended up being her living room, where she and her siblings regularly watched television. Additionally, Darya would occasionally record in her kitchen and dining room while she was with her family before dinner. Alla and Elena took turns recording and chose to record while either at the dining table or at the computer, which is by the dining table. This way, they were able to record their interactions with their parents and siblings, as their parents were often either at the dining table or next to it in the kitchen, and their siblings were usually at the computer. In addition to providing natural home data, this method also empowered the focal students, as they were active contributors to the data collection process and reported gaining increased symbolic status at home and at school with their ‘superhero watches’ (their words).

As a result of these methodological changes, I was able to examine the lived experiences of multilingual children who are heritage speakers of Russian and/or Ukrainian, in the two atmospheres where they received the most frequent linguistic input: at school and at home. In both locations, all of the children received multilingual input and produced multilingual output, both in their dominant language of English and in their heritage language(s). By observing and analyzing their lived experiences with language in these two contexts, I was able to understand how they negotiated their developing multilingual identities. I was also able to observe how the ways others discursively positioned them, and how they positioned themselves, played a part in this negotiation and development. Therefore, I was able to overcome the original problems of participant attrition and program change through creative thinking and a willingness to adapt. The resulting study focused on fewer participants than originally intended but was a much richer study, with possibly more important results in the long run (see Seals, 2013a; 2016; Seals & Kreeft Peyton, 2016).

Suggestions for researchers

The challenges presented in this chapter illustrate lessons for longitudinal researchers. First, the nature of longitudinal research and the high demands placed on participants means that it is crucial for researchers to be flexible in terms of research design and scope. Undoubtedly, some aspects of the longitudinal investigation will need to be adjusted, as longer studies are more susceptible to unexpected occurrences, especially in terms of participant attrition. However, a willingness to adjust the research can also lead to new and exciting directions and findings. As with the study illustrated in this chapter, such new approaches can often lead to more meaningful research contributions than may have otherwise been developed.
Additionally, by exploring options when unexpected challenges arise in longitudinal research, researchers have the opportunity to learn more about research areas outside of the previous scope, which can lead to becoming a more developed, well-rounded researcher. Understanding the circumstances surrounding participant attrition can also mean contributing to the current needs of a participant community and/or research field. Attrition happens for a reason. Why has it happened, and what can be learned from it?

The study presented here can also lend insights into what researchers might do if their participants leave the project mid-way through data collection or choose to withdraw from the project after they have already collected all of their data. While unpleasant situations, they are also not uncommon ones. Ethical research requires that we should not use data from participants if they have withdrawn from the project. Likewise, researchers cannot continue to collect data from these participants. However, researchers will have gained crucial data collection experience while working with these participants, which can then be applied to the collection of further data with new participants. Any refinement of approach can be reported on in the final report as well, bringing further validity to the study.

If existing participants withdraw from the study and it is not possible to recruit more participants, this could present the opportunity for conducting a more thorough investigation of the data that remain. For example, the same set of data could be analyzed from multiple theoretical perspectives, with the results potentially yielding a very useful discussion for all researchers in the field regarding the importance of justifying the chosen approaches. Trusting one’s abilities, knowledge, and skills as a researcher will present opportunities for discovery in the face of unexpected challenges.

References


Chapter 8

Dealing with low response rates in quantitative studies

Averil Coxhead

Introduction

I was in the audience at an international cricket match in Wellington, New Zealand, when Sir Peter Jackson recruited the crowd to be the soundtrack for an army of orcs. We had to stamp our feet in time, murmur, and chant. Over 30,000 people participated that day, and I remember looking around thinking, “This is one way to get more people to go and see your movies.” And then I thought, “If you could get this sort of participation in applied linguistics research projects, there would never be a problem with low response rates ever again.”

Low response rates in quantitative research studies can be a real problem for data collection in applied linguistics research, because they can cause bias in the data set (Punch, 2014, p. 243) and affect sample size. These problems can cause statistical interference and affect the generalizability of findings. They therefore also make it difficult to suggest that there is reliability and validity in research studies, and carrying this research through to publication can also be a frustrating process. Survey research tends to be the area where low response rates have been noted (see Wagner, 2010 for a useful discussion of sampling in survey research; see also Punch, 2014). However, low response rates can affect other kinds of research methods in applied linguistics, including experimental and corpus linguistics methodologies. Gass (2010) points out that in experimental research,

Sufficiently large sample sizes are always a goal as a way to increase the likelihood of true differences between groups (e.g., experimental and control). Small sample sizes leave the researcher with the uncertainty of understanding the results.

(Gass, 2010, p. 15)

In this chapter, I will draw on a study I conducted, reported in part in Coxhead, Nation, and Sim (2014), but see also Elgort and Coxhead (2016) and Nation and Coxhead (2014), where I countered low participation rates
by recruiting new groups of participants over a longer period of time than initially planned. This approach is also taken in Boers, Demecheleer, Coxhead, and Webb (2014). Building the picture of that research took patience and persistence, as multiple data sets were gathered over time from a range of participants. Sample size was also an issue in a review of two experimental studies in a series on replication studies in *Language Teaching* (Coxhead, 2015), where I note that larger sample sizes, and therefore, hopefully, higher response rates, would provide firmer ground for findings. For more on replication in applied linguistics, see Porte (2012).

In corpus linguistics, a corpus is designed to be representative, which means that it employs a sample frame. The notion of representation of the texts (for example, ensuring there is a range of authors in the case of written texts and speakers in the case of spoken texts) is a cornerstone of corpus design (Sinclair, 1991). For example, in an earlier corpus study of mine on academic vocabulary (Coxhead, 2000), including as many different academic writers as possible in the corpus (in this case, over 400) was important for avoiding bias towards the writing of one or two writers in a discipline or subject area. However, access to particular texts can turn out to be problematic. It could be that permission to use a text is refused, or a text cannot be scanned or made electronic, has been lost, transcription costs are too high, or a text could be damaged. Problems such as these can affect the sample size.

A research project, such as a PhD dissertation, with several stages that rely on large numbers of participants can be drastically affected by a low response rate. Unfortunately, it is very likely that other researchers have faced, are facing, or will face this problem in a range of quantitative studies in applied linguistics, so researchers need to be sure that there is a Plan B and even a Plan C. In this chapter, I report on problems with low response rates in my recent collaborative study on vocabulary size testing (Coxhead et al., 2014). This study is not a survey, so ‘response rate’ here refers to the number of people who actually stepped forward to participate in the study.

**About the study**

The purpose of the research project reported in this chapter (Coxhead et al., 2014) was initially to test the vocabulary size of first-year students in a university setting in Aotearoa/New Zealand. This project focused on first-year students because vocabulary-load research had investigated the number of word families needed to reach particular coverage figures in academic texts (Nation, 2006), whereby a vocabulary of 8,000–9,000 word families plus proper nouns would be needed for 98% coverage of academic texts. The testing project was focused on how many words first-year students would know in English and therefore whether vocabulary was likely to be a problem for them.
The Vocabulary Size Test (VST) (Nation & Beglar, 2007; see Nation, 2013) is a receptive vocabulary size test, developed by Paul Nation, that draws on frequency data from the British National Corpus (BNC) (see Nation, 2006) and is supplemented by the Contemporary Corpus of American English (COCA) (corpus.byu.edu/coca/). The original monolingual (English) version of the VST used the first 14,000 levels of the BNC and was developed through stratified sampling. The original intention for the VST was testing vocabulary size with second and foreign language learners of English. The test has a multiple-choice format with 140 items (ten per frequency level) and can be found online in several locations, including Tom Cobb’s Compleat Lexical Tutor (www.lextutor.ca/), Myq Larsen’s Vocabulary Size.com (http://my.vocabularysize.com/), and on Paul Nation’s website at Victoria University of Wellington (http://www.victoria.ac.nz/lals/about/staff/paul-nation). For more on the development, application, and use of the test, including bilingual versions, see Elgort and Coxhead (2016) and Nation and Coxhead (2014). Gyllstad, Vilkaite, and Schmitt (2015) discuss problems with guessing and sampling rates in the VST.

In order to test high-proficiency second language learners of English and native speakers at the university level, new VST versions were needed, as well as a computer-based application of the test. The project started with developing new versions of the test up to the level of 20,000 word families and followed by developing and trialling computerized versions. Initially, the new versions were based on the sample size of the original VST, with ten test items per 1000 frequency levels of the BNC. The resulting very long test would therefore include 200 items. During that time, Beglar’s (2010) Rasch analysis on the original VST was published, and his research suggested that five test items per 1000 frequency lists would be adequate sampling. Therefore, a 100 item approach was adopted in this VST project. This meant that six new versions of the VST were created up to the 20,000 word frequency level, with five items per 1000 frequency level. It was important to establish whether the versions of the test behaved in different ways so that teachers could have access to different versions of the test and researchers could also conduct research using versions which were not in the public domain (and therefore subject to future test takers deciding to learn all the items in the test) (Coxhead et al., 2014).

The next stage was attracting participants to take the VST, and therefore planning for sampling the population of first-year students at our university. This meant finding adult first and second language speakers of English in their first year of study at university to take multiple versions of the new tests to allow for comparisons of the results. Punch (2014, p. 243) notes that there are difficulties with accessing large sample sets and,

Very often indeed the researcher must take whatever sample is available, and the incidence of convenience samples (where the researcher takes advantage of an accessible situation that happens to fit the research context and purposes) is increasing.
This trend is almost always the case in applied linguistics research (see Woods, Fletcher, & Hughes, 1986).

The VST study was very much based on the population in our university, but if we could obtain a complete sample from the first-year cohort, this would have been a positive for the study as a non-convenience sample. It was preferable for the participants to sit the test individually to limit opportunities for checking answers to test questions using online tools. Participants all received immediate feedback on the results of their test as well as suggestions on how they might increase their vocabulary size, depending on their result.

At the end of the project, 217 people had taken various versions of the test in total: 103 native speakers and 114 non-native speakers from 22 nationalities. A rough estimate suggests that around 0.75% of undergraduate students took part in the VST project. Forty-six people took all six versions of the test, and based on this data, my team and I (Coxhead et al., 2014) reported that the tests fall into two groups of three tests. Two versions of the VST were subsequently used for testing the vocabulary size of secondary school students in Aotearoa/New Zealand (see Coxhead, Nation, & Sim, 2015). I now turn to obstacles which arose in this study in relation to the response rate.

The research problem

The main obstacles for this study in terms of response rate were time, recruiting enough participants in the context of the study and for each version of the test, and ensuring that the sample obtained was not significantly biased to any particular category of participant. Time was an obstacle for the study in terms of response rate – on average, test takers took between 20 and 45 minutes to finish the test; some participants took up to an hour. It was important that test takers took more than one version of the test so that comparisons between the test versions could be made. Taking all six versions of the test represents a major time commitment on behalf of participants and fatigue could have become a problem. In addition, there could be a practice effect once participants had taken more than one version. It is easy to see that there is a limit on how many responses can actually be gathered for this project, with so many possible constraints around scheduling, even with willing participants.

Motivationally, the test posed a challenge to some test takers, both native and non-native speakers. The VST tends to stretch the limits of the participants’ vocabulary knowledge as the items move from high-frequency lexical items to much lower frequency items. This problem meant that some participants could have chosen to end the test early rather than finish it, or they might rush through, hoping to get it done more quickly. In follow-up interviews to shed light on strategy behaviour and what knowledge the test
takers might have (to triangulate the test data), one of the test takers, a native speaker, drew this analogy:

It feels like things get triggered way down deep in your memory. Deep in your memory gland. It’s like looking for something in the bottom of your handbag.

Because the research was demanding high performance and stamina from the participants, it was possible that other potential volunteers might have been put off by the experiences of others, which could have resulted in low participation rates and created a bias in the sample. This problem is particularly important to longitudinal research (see Seals, this volume). On the other hand, there were some instances of competition between test takers which actually encouraged participation, as new participants came along saying that their friends had taken the test and they wanted to do so too.

The context of the study also provided a range of obstacles for recruiting enough participants for each version of the test. As mentioned earlier, the project was aimed at first-year university students as a population to help identify whether vocabulary size is a problem for first-year students at the beginning of their studies. The expectation, therefore, was that first-year students would be around 18–19 years old and new to the university environment. Contrary to those expectations, the first volunteer to take the test was a 56-year-old, first-year student. It also became clear that the kinds of people who like to take vocabulary size tests tend to be those who believe they have a large vocabulary, which means that we did not appear to be getting responses from a whole range in the target sample, creating the possibility of testing a skewed sample. Interested staff members as well as students at second and third year, and even postgraduate levels, began to show up to take the test. It did not seem sensible to turn them away, particularly since it was proving challenging to have people sitting multiple versions of the test and these people were willing to participate.

Finally, when the sample frame is heterogeneous, response rate is a particularly difficult issue to deal with. The context for this study played a clear role in the study, in that the language backgrounds of participants were likely to be quite different from each other. That is, one participant could be a monolingual or bilingual native speaker of English and another could be a Vietnamese first-year student studying economics, music, or biomedical science. If the testing site had been a country or educational context where a large population of speakers of the same language/s and educational backgrounds could have formed the pool of participants, then this problem would have been mitigated, as in Beglar’s (2010) VST study based in Japan. Balancing the number of test takers, ensuring there was a range of ages, language backgrounds, sex, and proficiency in the case of the second/foreign language learners all added to the complexity of the data gathering. So these
different aspects of the study created a special set of difficult circumstances, a perfect storm, for this vocabulary size testing project.

**Overcoming the obstacle**

When it became clear that there were obstacles in terms of the low response rate, several strategies were developed. The first strategy employed targeted the logistical obstacles. First, I sought funding to support the recruiting and training of research assistants to help with the data collection and management. The research assistants, recruited from the sample population, also proved to be very good at recruiting further participants, as they drew on their own networks within the university, and gave suggestions on strategies for helping recruitment. They also spotted and handled the need for controlling more closely which versions of the test were being used with each new participant or which version needed to be used for a returning participant. Funding also helped improve the experimental conditions through the provision of vouchers to award the participants, better reflecting time commitments, attracting more participants, and potentially raising motivation levels and therefore improving the response rates. Loosening the requirement for one-on-one testing and testing in small groups and computer classrooms meant increased opportunities for testing and monitoring more people at the same time. Increasing flexibility on testing in terms of numbers was a fairly major change in my view, so even in group testing, the numbers were kept fairly small. Other adjustments to address the problems with time included building breaks and monitoring participants for signs of fatigue. Breaks, unfortunately, add to the amount of time for taking the test. Frustratingly, but not surprisingly, the number of single tests increased but it proved difficult to gather complete sets for the research project.

The problem of needing complete sets lead to the development of a second set of strategies which focused on recruiting enough participants for each version of the test. When it became clear that minutes after putting up the posters around campus, my office was not being inundated by people who were desperate to take part in the study, the first reaction was to panic slightly. The first thing to do, then, was to lower expectations on how quickly people would respond to the call to participate. The second strategy was to increase exposure for the project across the university. I had already used a poster campaign around campus in areas where students tend to congregate. To increase exposure, I conferred with colleagues in faculties and campuses (the university has four campuses) about where to display posters to the best effect and arranged visits to first-year lectures with high enrolment numbers to call for participants. On reflection, social media is an obvious answer to recruiting more participants and at the time of this writing, these opportunities are now more readily available through the university’s own Facebook and Twitter accounts, for example.
To attract a wide range of participants in the data set, I recruited new populations of test takers. As with the Boers et al. (2014) study mentioned earlier, this recruitment involved changing the sampling frame. This was achieved through targeting particular population groups within the university, such as the English Proficiency Programme and those enrolled in pre-degree English for Academic or Occupational Purposes students. Widening the pool of test takers meant that the interested staff, other undergraduate and postgraduate students could be included in the testing. Once again, there was a snowball effect of people who had taken the test and recommended it to their friends, which also helped our sample sizes. People who had taken several versions of the test were contacted and asked whether they would be willing to take more tests.

Narrowing the focus of the recruitment at one point to make sure as many participants as possible sat all six versions of the test helped, because it became clear that this was particularly important. Survey research discusses this point in terms of following up on respondents with reminder emails and considerations of how many times you should contact the same participants (see Dörnyei & Taguchi, 2010, p. 66). A second contact (for example, using email) can increase response rates by as much as 30%.

Finally, readers will have already spotted that narrowing the focus of the research from the outset would have been beneficial. An ambitious and time-consuming project like this would have been a major undertaking for a fairly large group of researchers, let alone for a small group. Statistical advice was sought throughout the project, particularly when major changes occurred such as Beglar’s study (2010) where the number of items in the test at each frequency level was reduced. More funding for statistical support also proved very helpful for this project.

**Implications for researchers**

If researchers are facing problems with low response rates, there are a range of possible courses of action to take both before and during data collection. Planning is without a doubt one of the most important ways to deal with low response rates. Thinking things through in terms of the kind of research project which is to be undertaken, troubleshooting ideas and seeking feedback on plans for research are all strategies which can help identify possible problems with low response rates. Reading widely is also important for planning, and if it is possible to find people who have done similar research or used similar methodologies, then discussing ideas with these people would be very helpful too (Barkhuizen, 2014, p. 7). Planning involves scoping the size of the project, which was part of the problem with the VST study. There are plenty of possible variables in the VST in terms of native speakers of English and non-native speakers, and introducing six versions of the test added more complexity. Another important part of planning is
recognizing that research takes time and requires some flexibility, perhaps involving having to identify extra sources of funding in the course of a research project, and developing and extending networks for collaboration.

In a study with relatively low response rates, the sample size finally obtained can be difficult for the researcher. The question of how many participants are enough is potentially problematic (Barkhuizen, 2014). One person’s low response rate can be another person’s reasonably good response rate. Reviewers of Coxhead et al. (2014) suggested that 46 test takers was not a large enough sample for the comparison of the six versions of the VST. Our statistician had a very clear response to that suggestion. In her view, 46 people taking six versions of a test gave 276 data points, which is more than enough to test the new versions of the VST. Different perspectives such as these can cause confusion, which highlights the importance of consultation and reading widely to look for norms in the field or learning more about statistical approaches through reading and discussion with colleagues.

In the course of the VST research, it became clearer that one way to move forward was to break the project into smaller pieces of research and build towards a bigger picture. It also became abundantly clear that many hands make light work. Having the capacity through funding to recruit and pay research assistants certainly helped move the research project along more quickly and more smoothly. It may not be in the scope of everyone doing research to be able to apply and be successful with research grants, but it is well worth identifying possible funding streams. Important questions to ask include the following:

- Could a bigger team be recruited to help with the data gathering?
- Is there a team of PhD students in your institution or in your networks who might be able to help a research project along in return for some help at another stage on their projects, if possible?
- Are there possibilities for working with sister institutions?
- Are there conferences where like-minded researchers gather who might be able to help with planning and data collection in another institution or even another country? Gyllstad et al. (2015) use this technique well in their VST study, drawing on data from three different research sites (Sweden, Lithuania, and the United Kingdom).

It is also important to think about timing and participation in studies. Some ways to help plan for timing include:

- Developing a timeline for the research to help with decision making around the number of participants. A timeline helps set out clear periods for data gathering and sorting, and for evaluating whether and when another push for participants might need to take place.
• Putting in place a time frame on invitations for participation could also be helpful, because posters without dates on them can be easily ignored, taken down, or posted over.
• Having a clear date for the end of the project might also prompt people to take part, if a snowballing approach is being used and other people are pushing your recruitment for participation out to others.

Persistence in gathering data is not to be overlooked. It might include considering whether there is an angle to the research which might catch people’s eye in the media, for example, and encourage participation. Vocabulary size tends to be a regular topic in the media, particularly when people like to lament the possibilities of loss of language and vocabulary in these modern times of computers, tablets, and emojis. After a newspaper article was published here in New Zealand on the VST research, thousands of VST test takers took the test on my.vocabularysize.com, and not just from Aotearoa/New Zealand. If a research project can catch the eye of the public, it could start a snowball sample of epic proportions, which brings me back to Sir Peter Jackson and his recruitment of an army of orcs. While we might not all have sufficient sway to get a sports stadium to take part in our research projects, strategies such as planning, networking, and funding might all help reduce low response rates.

References


Introduction

Our understanding of how multilingual speakers use and process linguistic information in language learning has advanced considerably over the past few decades, and so has our awareness that non-native languages may influence the acquisition of additional languages in some meaningful ways.

Since the late 1980s, a substantial amount of research has been steadily showing that learners rely upon their knowledge of non-native languages when learning additional languages and that crosslinguistic influence (CLI) occurs even when proficiency in the background languages is relatively low (for reviews, see De Angelis, 2007; Falk & Bardel, 2010).

While non-native languages have been regarded as a potential source of CLI for a long time, their treatment in quantitative research has been inconsistent and uneven at best. In this chapter, I will discuss why the presence of non-native languages in learners’ minds has not been examined more consistently in the past and the type of problems associated with non-native language knowledge, particularly in relation to the possible introduction of a subject selection bias in research. I will then discuss how we can conduct research with multilingual individuals without introducing a subject selection bias in the design. By referring to a real example of quantitative research on CLI and multilingualism (De Angelis, 2005a), I will outline how I dealt with proficiency assessment then, how I would deal with it ten years later, and the lesson we can learn from past practices, including the current knowledge we have gained in between.

The subject selection bias

The increased awareness of the potential role of background languages in language learning allows us to raise two main questions of relevance to empirical research:

1. At what proficiency level does a non-native language begin to influence the acquisition of another language?
2 In the absence of a clear threshold level, what criteria do researchers use when deciding who to include in their studies?

The first question relates to using proficiency level as a criterion for subject selection procedures. The question needs to be addressed because researchers working in language acquisition and development have the objective need to classify informants on the basis of prior language exposure. Should a learner who has studied another non-native language for a year or two, for instance, be regarded as a monolingual learner of an L2 or a bilingual learner of an L3? Most researchers would probably assume that one or two years of non-native language instruction are not going to affect the language acquisition process to a significant extent and would consequently classify the learner as a monolingual learner of an L2. But is this assumption empirically justified, or is it a random decision? Are perhaps three to four years of instruction more suitable for us to be able to distinguish an L2 from an L3 learner? The reality of the matter is that we do not know where the threshold level lies or whether it would equally apply to all domains and guessing boundaries does not seem to be a sufficiently rigorous or reliable criterion to safely use in empirical research.

This initial problem leads us to raise the second question (In the absence of a clear threshold level, what criteria do researchers use when deciding who to include in their studies?). We can only assume that each researcher sets his or her own criteria on the basis of personal knowledge and beliefs and that great variability will result from this practice (see Kubota, this volume, for related discussion).

The absence of clear answers to both of these questions leads us to conclude that researchers have not dealt with non-native language knowledge consistently in the past and may have introduced a subject selection bias in research. An example may help clarify how easily this may occur. If we take a study involving French L2 learners enrolled at a Californian university, for instance, it would be more than reasonable to wonder whether these learners have had some prior exposure to Spanish as a non-native language in school. Spanish is widely studied in secondary schools across the United States, above all in California, and there is a strong possibility that a large number of these French L2 learners are in fact English L1 learners of French with some prior knowledge of Spanish as a non-native language. Let us now assume that some of these learners were indeed exposed to Spanish for a few years in high school, but only developed the ability to handle basic conversations in the non-native language. Should the researcher proceed to classify them as L2 learners or as multilingual learners? As we already know, this is usually at the discretion of the researcher, and subjective decisions are likely to lead to a great deal of variation within the discipline.

Most learners who live and study in a non-English-speaking country would choose to study English as their first non-native language (Eurostat,
Dealing with multilingualism (2016). This entails that all other languages will be acquired as third or additional languages. As an Italian native speaker, I often read research on the acquisition of Italian as a non-native language, and I typically see that learners are labelled as 'Italian L2' learners. I know, however, that in most cases they are not L2 learners at all, as English will have been the first non-native language learned in most contexts.

Monolingual learners of an L2 are in fact becoming a rarity for most languages other than English, especially in contexts outside of Anglophone countries. The question of how we can account for prior non-native language knowledge without introducing a subject selection bias in research is then a crucial one, as a great deal of studies on language acquisition and language development take place in universities and secondary schools, where students with knowledge of languages beyond their L1 are the norm rather than the exception.

Some scholars may argue that, in the absence of substantial evidence of non-native language influence, one may continue to assume that a few years of instruction do not significantly affect the language acquisition process. At present, we can say that we have no evidence that non-native languages do not affect the language acquisition process and some evidence that they do. Research conducted over the past two to three decades has clearly identified CLI phenomena from both high-proficiency background languages (Ahukanna et al., 1981; Chandrasekhar, 1978; Clyne, 1997; Clyne & Cassia, 1999; Ringbom, 1986; Schmidt & Frota, 1986; Singleton, 1987; Williams & Hammarberg, 1998) and low-proficiency background languages (Bardel & Lindqvist, 2007; De Angelis, 2005a; 2005b; De Angelis & Selinker, 2001; Rast, 2010; Rivers, 1979; Selinker & Baumgartner-Cohen, 1995; Vildomèc, 1963).

The issue does not seem to be whether non-native language influence occurs, but rather when a non-native language begins to influence the acquisition process and, consequently, when we can safely classify an individual as an L2 learner or a multilingual learner in empirical research.

In quantitative research, the most effective way to avoid dealing with this problem and introducing a subject selection bias is to work with learners who have developed a high level of proficiency in their background languages. Bilinguals or trilinguals, for instance, are easy to classify, but they are not as easy to locate in large numbers.

In the sections that follow, I will explain how the subject selection bias first came to my attention and how I dealt with it in a study with learners who had low proficiency in their background languages.

**About the study – De Angelis (2005a)**

In the late 1990s, research on non-native language influence was still in its infancy and most of what we knew stemmed from qualitative research and occasional observations. Non-native language knowledge did not seem to
have a clear and stable place in CLI research, nor in second language acquisition theories.

As a then multilingual doctoral student, I believed in the crucial role of background languages in language learning, and I was determined to find quantitative evidence of L2 influence in L3 production so that non-native languages would become a more permanent feature of CLI theory – then almost entirely based on L1 influence phenomena. Up to then, L2 influence had been mostly treated as an occasional rather than a systematic phenomenon. In order to obtain reliable quantitative data, I needed to identify groups with similar language backgrounds but different language combinations that were large enough for quantitative analysis to be employed. I identified two such potential groups in Pittsburgh, USA, and Puerto Rico.

A prerequisite was that the native language would be a constant (either English or Spanish), and the target language would also be a constant (Italian as L3 or L4). The first group included English L1 speakers who were learners of Italian as a third language (L3) attending first-year language classes at the University of Pittsburgh. These learners differed only in their knowledge of the non-native language, which was either Spanish or French. The second mirror group included Spanish L1 speakers studying first-year Italian as an L3 or an L4 in Mayagüez, Puerto Rico. These participants also differed in their knowledge of non-native languages: some had knowledge of English and some of English and French. Puerto Ricans usually begin to study English at the age of five in a non-immersion context. Some of them also study French, but the study of French is far less common.

The combination of Romance and Germanic languages these learners were familiar with allowed me to examine the use of non-native function words in written production, with special emphasis on subject insertion and omission in the Italian target language. Subject insertion and omission was an ideal candidate for analysis, as French and English require obligatory overt subjects while Italian and Spanish do not. In Italian and Spanish, person marking can be equally expressed with an overt subject (noun or pronoun) or with verb morphology. For instance, a person marking in the Italian sentence ‘io studio’ (I study), is expressed by the subject pronoun (io) as well the first singular verb ending (-o). The subject pronoun can therefore be dropped with no difference in meaning.

An initial analysis of learners’ texts showed the frequent use of the French subject pronoun *il* (he), which led me to consider whether the overall rate of subject insertion or omission differed depending on language background. English L1 speakers with French or Spanish as an L2 were thus compared to each other and so were Spanish L1 speakers with English or English and French as an L2 or L3. Results showed that both English and Spanish L1 speakers with knowledge of French used significantly more overt subjects than speakers of other non-native languages (Spanish or English). The finding suggested that (a) prior exposure to French as a non-native language
informed learners’ choices of surface structures to a significant extent and (b) learners with the same L1 but different non-native languages developed some significant differences in their knowledge of the target language.

Of relevance to the subject selection bias illustrated earlier is the fact that learners with knowledge of French had limited knowledge of French, amounting to a maximum of two years of instruction. The study then gave the additional indication that some significant differences may indeed arise after one to two years of study of a non-native language, which is a period of time most researchers would dismiss as being insufficient for an influence to arise in production.

The research problem

A core difference between native and non-native language influence rests on what researchers may safely assume to be part of learners’ knowledge and therefore on what can be identified as transferable to a target language.

In the case of native language influence, language knowledge can be safely assumed to be in the mind, as all native speakers will share the same type of knowledge. Individual differences may naturally exist, but one can assume that a substantial amount of information will be shared. In the case of non-native language influence, learners’ knowledge may not be safely assumed, as learners may know the language at different levels of proficiency, and some knowledge will not be shared.

For example, a researcher may safely assume that all English L1 learners incorporate obligatory overt subjects into their sentences and will have shared knowledge of present tense verb morphology. The same type of information cannot be assumed to exist in someone’s mind when English is the second language. The learner may be influenced by a prior language (the L1 or other language) that does not require obligatory overt subjects in the sentence structure, or may quite simply lack the knowledge of a particular structure or element of the English target language.

A way to partially control this problem is to include some constants in the research design. In De Angelis (2005a), the L1 and the target language were treated as constants so that differences in production would be easily traced back to exposure to a non-native language. Once the possible source of the influence is isolated – in this case the non-native language – it is imperative that learners are at comparable proficiency levels, which then must be properly assessed.

The study I am using as an example (De Angelis, 2005a) was conducted in the late 1990s, when some of the tests now commonly used for proficiency assessment in research were not available. Non-native language knowledge was assessed using two methods: a) a sociobiographical questionnaire which learners completed in their native language and b) a lexical knowledge test.
The literature available at the time had given no indication of *when* a non-native language would begin to influence the acquisition of an additional language. I was, however, spared the decision to establish a threshold level, as most learners had only achieved low proficiency in their non-native languages, with the exception of English in Puerto Rico. Even though my primary research questions were concerned with non-native language influence and the role of typological similarity and dissimilarity between source and target languages, proficiency level quickly became a central concern because of the need to form groups that would be comparable with one another. Because of learners’ low level of proficiency in the non-native languages, it was inevitable that any evidence of CLI was going to be linked to low proficiency in the source language.

In sum, the problem I was facing related to the need to assess non-native language proficiency for CLI research and how such assessment was to be conducted.

**Overcoming the obstacle**

According to the criteria outlined in the design, all participants had to be native speakers of English or Spanish, depending on location, and they also had to have the following language combinations: French or Spanish (for English L1 speakers based in Pittsburgh) and English, or English and French (for Spanish L1 speakers based in Puerto Rico). Participants with other language combinations were to be discarded.

The sociobiographical questionnaire included several questions that allowed me to eliminate learners with language combinations that differed from those mentioned earlier. Even a little exposure to another non-native language was regarded as important and was a reason for elimination. The answers to the questionnaire also allowed me to identify and eliminate learners who had studied a foreign language for a long time or had lived abroad and used the non-native language extensively. The typical student profile was then that of a student who had learned a foreign language at school prior to starting university.

The answers given in the questionnaire were then compared with the results of a lexical test all learners were also asked to complete. The lexical test consisted of the translation of 30 basic words such as *apple, to get up, to go out,* and *leg* into Italian (see De Angelis, 2005a).

Learners who provided fewer than ten correct answers in the test were included in the final subject pool. This decision was made because knowledge of ten or fewer words was taken to be an indication of low proficiency. In the case of French, this corresponded to students having studied French for one to two years. In Puerto Rico, knowledge of English was safely assumed to be high, as students reported having studied it for an average of 14 years.
The strict requirements set out in the original design allowed me to isolate comparable groups of learners that were sufficiently large for quantitative analysis to be employed, but a substantial number of subjects were also excluded from the final subject pool. I had started with large numbers (n = 356) but the final subject pool included 54 English L1 speakers (37 with Spanish L2, 17 with French L2) and 54 Spanish L1 speakers (45 with English L2 and 9 with English L2 and French L3). Number reduction then amounted to 60.67% (see Phakiti, this volume, for discussion of data loss in quantitative studies).

Implications for researchers

In quantitative research, the presence of background languages is highly inconvenient because it is likely to lead to a substantial reduction in the number of potential participants in a study whenever the researcher tries to eliminate the confounding variable. This is not, however, a reason for ignoring their presence during the screening process, as doing so will inevitably introduce a subject selection bias in research.

The study discussed in the present chapter was completed more than a decade ago when only a handful of quantitative studies with multilingual individuals were available in the literature and therefore many of the issues related to proficiency testing had not been properly addressed. There are several difficulties associated with proficiency assessment and using a combination of methods was advisable then as it would be advisable today. Assessment practices have however changed.

First and foremost, since publication of the study, an invaluable tool has emerged: the Common European Framework of Reference (CEFR). As a result, testing proficiency in a classroom context has become a much easier task. If I were to conduct the same study today, I would replace the lexical test with a reading or writing test used in a past CEFR exam. The test would be objective and reliable for research purposes and would be simple to administer to a group of students learning a language in a formal context. In addition to a portion of a CEFR exam, I would ask students to fill out a CEFR self-evaluation grid as well as a sociobiographical questionnaire. The information from both could then be triangulated to get a fuller picture of the learner’s past exposure to the non-native language and proficiency level at the time of testing. The use of the self-evaluation grid alone would be controversial, as it is too subjective. When used together with additional information, it may, however, be informative, as it provides a window into learners’ perception of their own proficiency level in the language being tested.

There are additional difficulties associated with proficiency testing for research purposes. First, proficiency tests are difficult to carry out in classes where speakers have knowledge of different languages, as testing is time consuming for the researcher and the institution allowing research to take
place. Second, a group of students cannot be tested in more than one language on the same day, as it would be too tiring for them to do so. Third, all testing has to be completed within a few weeks or perhaps a few months. One should bear in mind that when a non-native language is concerned, source language proficiency can fluctuate quite rapidly within a short period of time, including a few months (Sanchez, 2011).

How we assess language proficiency should not depend on subjective judgments or self-assessment, although the collection of biographical information can be useful, particularly when learners are at lower levels of proficiency. In De Angelis (2005a), for instance, most students reported having studied French for one to two years. When such a short period of time is involved, it is fairly safe to assume that proficiency is low, as it would be unusual for someone to go beyond an A2 level in such a short period of time.

Additional information may also come from teachers, who usually know the subjects well and can offer accurate ratings that can be compared with the results of a test. The CEFR is so widespread that nowadays all teachers in Europe with some experience would know what an A2 or B1 level is, and use of the CEFR continues to spread as it is translated into more languages. The CEFR has helped to create a culture of testing among teachers which has turned into shared knowledge a researcher may choose to rely upon.

Doing quantitative research with multilinguals is not an easy task, as whatever method is used, chances are that number reductions are going to be great. It is however possible, but as a rule of thumb one needs to plan for a reduction of at least 50%. If all students are known to be multilingual, then the task becomes much easier to accomplish.

Expecting a number reduction of 50% implies that researchers must be prepared to collect a larger than average amount of data. This may mean setting aside additional time for data collection as well as additional funds for analysis. My advice to anyone embarking in quantitative research with multilingual subjects would be to locate a large number of individuals with similar language backgrounds in a few locations, possibly close to one another. As a first activity, I would also advise to screen for language patterns that are not shared amongst the majority of subjects. This early screening helps the researcher narrow down the final subject pool and makes sure that data is not analyzed unnecessarily.

The most important lesson learned from the study reported in this chapter is that we must stop assuming that the presence of a non-native language will have no impact on language acquisition and language development. While we are still far from having gained a true understanding of the role of non-native languages in language learning, accounting for non-native languages and defining source language proficiency remains essential for the field for two main reasons. The first is that it allows us to advance our understanding of the CLI process and the
role of low- and high-proficiency background languages during learning. The second is that it allows us to avoid introducing a potential subject selection bias in research.

Quantitative research with multilingual subjects remains less common than qualitative research. The number of potential participants is an issue for many scholars, but it can be easily solved with some careful planning and the identification of suitable populations in bilingual or multilingual contexts. More quantitative research with learners who have achieved low proficiency in their background languages will additionally help us understand where threshold levels lie and how they differ across domains.

References


Part III

Researching vulnerable groups
Chapter 10

It isn’t child’s play
Conducting research with children as participants

Victoria Murphy and Ernesto Macaro

Introduction

In this chapter, we explore some of the main issues and challenges that arise from carrying out research with children. Working with children generally, and researching the nature of language learning in children in particular, is an exceedingly important and fascinating endeavour. Not only does such work help us understand key issues in language development (i.e., it helps us to move closer to answering the question ‘how are languages learned’), but it also is increasingly timely given more children around the world are either growing up bilingually, or being taught foreign languages (FL) from ever younger ages (Murphy, 2014). Indeed, a recent review of early childhood education in English for speakers of other languages has identified that increasingly, children in the pre-primary years are taught a foreign language (usually English), often as young as three years old (Murphy & Evangelou, 2016).

Despite the fact that children have been learning second or foreign languages for a long time, surprisingly, little is actually known about the nature of L2 and FL learning in children at different ages, resulting in a plethora of questions about child L2 learning that have yet to be fully answered. These questions include the following: What is the best age to start learning a FL? How many hours a week should children spend in a FL classroom? What is the most effective way of teaching a FL to young children? What is the most appropriate way to assess FL knowledge in young children?

It is not the focus of our chapter to attempt to delve into these questions in any detail. However, to illustrate some of the general methodological issues we raise in this chapter, we have centred our discussion on a specific project that we carried out. In our study, we aimed to identify what impact learning a foreign language (FL) might have on a developing child’s first language (L1) literacy skills (Murphy, Macaro, Alba, & Cipolla, 2015). The motivation for this study was the observation, noted earlier, that increasingly, governments around the world are lowering the age at which children are required to learn a taught FL as part of the primary curriculum (Murphy,
One of the consequences of such policy decisions is that skills in the child’s L1 may not be fully developed at the time when they begin the task of learning a foreign language.

In the Murphy et al. (2015) paper, we were interested in examining whether L1 (in this case, English) literacy skills might be one of those L1 features that were likely to be influenced by learning a FL. We will first briefly outline what this study was all about and what the main findings were. Then we will go on to identify specific methodological challenges and considerations that were not only relevant to this particular project, but we feel are also worth considering as a whole in carrying out research with children. Finally, we identify some implications and make some general suggestions about conducting research with children. ¹

The study

In 2003, Cook published what we believe to be the first volume focused exclusively on whether and to what extent L2 learning/knowledge impacts on L1 knowledge. Whereas much research in applied linguistics generally has focused on whether and to what extent particular L1s will influence (positively or negatively) L2 development (e.g., Lado’s Contrastive Analysis [Lado, 1957]), comparatively little research has examined crosslinguistic influence (see De Angelis, this volume) from the other direction – whether an L2 (or FL) can influence an L1. We hypothesized that one area where L2/FL learning in children might influence the L1 is in the area of literacy skills, because (as mentioned earlier), literacy skills are still in development in the early to mid-primary school years – the period of time in which many children are introduced to FL learning. We, therefore, designed a UK study in which we recruited children in Year 3 who were aged seven/eight years from schools that were not offering any FL instruction. We randomly assigned some of these children to an L2 Italian group, some to an L2 French group, and the remaining children participated in a waiting control group. ² The L2 Italian and French groups were taught Italian and French, respectively, for 15 weeks, for approximately one hour per week in small groups. Italian and French were chosen as the FLs to be taught in our study for two particular reasons: one pragmatic and one theoretical. The theoretically motivated reason stems from research that has suggested there are particular benefits to learning a FL that has a more transparent set of grapheme-phoneme correspondence rules (where the sounds map more directly on the orthographic patterns of the language) relative to the L1. The pragmatic reason is that members of the research team had knowledge of both Italian and French. Consequently, our research questions were aimed at identifying whether we could find any evidence for L2 learning influencing L1 literacy skills and whether there might be a differential effect depending on which L2 was being learned. It is generally accepted that Italian is a transparent language,
whereas French, although not as ‘opaque’ as English, is nevertheless much less transparent. So the additional theoretical motivation was to explore whether learning a transparent language (Italian) or a similarly opaque language (French) had an impact on an opaque (English) L1.

The participants in our study were administered a range of pre-tests which examined their English (L1) reading and spelling skills, as well as their nonverbal IQ, which we used to ensure the groups were matched on general cognitive processing skills. Those groups that received L2 instruction were taught parallel forms of the same teaching programme which focused primarily on vocabulary. Following the 15 week FL teaching, we then post-tested all participating children on the same tests as at pre-test, and we also tested them on a measure we created ourselves aimed at identifying whether and to what extent the children learned their respective FLs. Our results showed that first, the children were able to learn aspects of both Italian and French (depending on which group they were in) after only 15 hours of instruction in the FL. Most interestingly, however, those groups who participated in the FL learning showed advantages over the control group in some aspects of English reading and, importantly, the L2 Italian group outperformed both the L2 French group and control groups on non-word reading in English and aspects of English phonological processing – a key variable in reading skill. We interpreted these findings as confirming the benefits of learning a FL at primary school and that we should explore further the relative effects of learning FLs/L2s with different grapheme-phoneme correspondence systems.

**Challenges and how to overcome them**

Having briefly outlined the study that serves as the backdrop for our methodological discussion, in this section, we identify some key issues that we encountered in this research project with young children, but that are certainly not unique to this project. We also identify how we tried to overcome these challenges in our own research and how these strategies might be helpful in carrying out research with children more widely.

**Finding appropriate research assistants**

In our study, our research assistants (RAs) taught the children either Italian or French. As the critical issue was whether there would be changes as a consequence of learning a FL, and we wanted to compare the influence of specific FLs on the L1, we needed to ensure that the teaching itself was matched as closely as possible. To that end, we needed RAs who could work closely together in implementing their teaching practice, who had the same levels of expertise in the subject (i.e., both were native speakers of Italian...
and French, respectively) but who also knew the language they were not teaching. Crucially, they also needed to have a teaching qualification and appropriate levels of experience teaching and working with young children. This last point is particularly important in carrying out research with children. Children can often be quite shy in general, and in research projects in particular. If a child does not feel comfortable around the researcher (who is likely to be a ‘stranger’) then the child is not likely to behave as he or she would in typical situations, and hence the data collected will not accurately reflect the child’s abilities. This problem raises a serious threat to the reliability and validity of the data. Hence, it is vital in carrying out research with children that researchers create an environment where children feel comfortable and at ease for both methodological and ethical reasons.

We tackled this problem in a number of different ways. First, to find the right people to work with us as research assistants, we recruited widely in the hopes that we would be able to find a competitive set of applicants with the right set of qualifications. Through our wide recruitment, as well as through our own professional networks, we were able to find research assistants who met the key criteria for our post. To combat against the problem that one RA might deliver the intervention (L2 teaching) differently from the other, we ensured that both RAs were involved at all stages of materials development so they both knew precisely what the rationale was for different aspects of the materials. They worked closely in preparing not just the materials themselves but, importantly, how those teaching materials would be implemented in their respective groups of children. As the study was unfolding, the two RAs also kept in very close contact with each other, having regular (i.e., weekly) debriefing meetings to discuss any issues that arose while the teaching was being offered. This procedure allowed all of us involved in the project to feel more confident that there was parity between the two experimental groups’ experiences with the L2.

Establishing a suitable sampling frame

In order for a sample to be truly representative of the population, every individual member of the population has to have an equal and non-zero probability of being recruited into the sample. Clearly, in our study, when the population constitutes millions of children, and with our limited resources, this was not possible. However, if we can replicate our studies with other (equally non-representative) samples drawn from the same population, then we can feel increasingly confident that our samples are representative. Unfortunately, replications themselves (particularly in social sciences research) are relatively rare and quite expensive. This means that researchers have to be keenly aware of the limitations of their own research and the strength of the conclusions that can be drawn.
Practical considerations play a key part in carrying out any research study. Bearing in mind that we were interested in working with children who were not already learning a FL as part of their primary education, out of 200 available schools in our commutable research site area, we were then left with only 28 schools who met this criterion. Of these 28 schools, only 9 agreed to participate in our study. From these nine schools we recruited a total of 120 children who were randomly assigned into one of our three groups. We can see then that starting from our population of native-speaking English primary school children in English-speaking countries, to native-speaking English primary school children in England, to those in one county of England, to those in a school within a ten mile radius of one city, to those who were in a school which did not offer Modern FL instruction, and to those who agreed to participate, the sampling frame is narrowed significantly, which seriously undermines how well the sample matches the population. However, when this situation occurs, it is important to at least ask what factors could be undermining the match and in relation to the research questions and methodology being employed. For example, would children in different geographical regions of the country react differently to the intervention? Would children in urban areas have been exposed, perhaps outside schools, to writing systems different from English? Asking these questions helps us to assess the magnitude of the limitations of our study.

**Finding appropriate research sites**

In much research with children, as in our study, the site for research is the school. Schools are very busy environments, where teachers and other school staff have huge demands on their time. As researchers, it is important to be aware of this and ideally to be able to work *with* the school instead of simply using the school as a research site. To that end, we believe it is important to establish good *collaborative* relationships with the school. Schools should be able to benefit from participating in the research as much as the researcher benefits from working with the school. There are various ways in which this can be achieved. As many teachers are time pressed, and in primary schools, they typically need and can benefit from volunteers, we often spend some time in the classrooms from which our participants will be drawn as a kind of unpaid teaching assistant volunteer. For example, a major focus of the beginning of a primary school education is to learn to read. To that end, teachers need to spend time both reading to children and listening to children read. One of the ways we try to help with teachers’ practice is to offer to sit and read with individual children (listening to them read as well as reading to them), particularly with any children who might need some extra attention. Additionally, we offered to present our research findings to staff at the school and organize workshops in our department at the end the school day to enable those schools who have kindly agreed to
participate in our research to get together and discuss the main findings and possible future directions of the research project. This more collaborative model therefore values a number of important knowledge exchange activities which are beneficial to both schools and researchers working within and with schools.

**Recruiting children and obtaining informed consent**

Even once a school has agreed to participate in a research study, research ethics often require that the parents or legal guardians of the children provide signed informed consent to have their children participate in the study. The level of consent depends on the nature of the study. Our study involved activities outside of class time (randomization meant that the different teaching groups were ‘re-created’ rather than using ‘intact classes’). It also involved individualized testing – both of these aspects creating some disruption to the normal everyday experience of the children. We therefore needed to obtain ‘opt-in’ consent from parents rather than asking them to only let us know if they wished to ‘opt-out’ from the research. To that end, we prepared a letter to the parents carefully outlining the key details of the study, what we were trying to find out and why, what we were going to do with the children, how we are going to behave with the children, and, importantly, what we were going to do with the data (including issues such as keeping children’s identity anonymous). Furthermore, sometimes children themselves might not want to participate in the study, even if their parents have granted permission. It is not ethical to ‘force’ a child to participate just because his or her parents have agreed. Therefore, it is important to also obtain the children’s assent to participate in the study.

A further challenge for recruitment in school settings is a lack of face-to-face opportunities to communicate with parents directly to answer questions and send reminders about the study. The main route of communication to parents is through their children’s book bags, which they bring with them to school each day and take home. Schools often insert letters alerting the parents to various events and issues in the book bag. The participating schools in our study allowed us to send letters home to the parents of potential participating children in their children’s book bags. A certain level of polite persistence is required to remind and encourage parents to read through the letter carefully, ask any questions they may have, and then sign and return the consent forms. In some of our studies, we have visited the school at drop off and/or pickup times in order to make ourselves available to parents to answer any questions and to, again, politely remind them to return their signed consent forms. Polite persistence and perseverance is sometimes required and is often effective. Once we obtained signed informed consent from a parent or legal guardian, and after having obtained verbal assent from the participating children themselves, we were able to begin!
Many professional bodies within the domains of social science offer very clear guidance on the specific ethical issues relevant to working with young children. In our case, we followed the guidance of the British Educational Research Association, which can be found on the association’s website. Additional professional guidelines which we have found to be helpful are from the British Psychological Society and the American Psychological Association (APA). In carrying out research with any human participant, but young children in particular, as they constitute vulnerable participants, it is critical that researchers familiarize themselves with appropriate ethical research guidelines and follow these procedures carefully (Farrell, 2013).

Finding a space to conduct the research

Once the school, parent, and child have all agreed to participate, the next hurdle is to find a space in which to actually carry out the work. This can sometimes be quite tricky, as primary schools in particular do not often have the luxury of having empty rooms available for research projects. The key is to strike the right balance between ensuring the child can engage with the researcher in an environment where he or she is relatively free from distraction but also recognize that researchers are guests in schools and consequently always have to be respectful of the pressing demands on their resources and to try as much as possible to cause the least amount of disruption as possible. In other projects we have been involved with, we have sometimes carried out research in the cupboard where the school keeps the sports equipment, as it was the only place where there was relative quiet! If there is no space in the school which is suitable on a given day when the research is meant to be conducted – that is relatively quiet and free from distraction – it is best to reschedule the data collection to another time of mutual convenience. We have to be flexible, but at the same time, we cannot compromise the reliability of the data itself. This is sometimes a delicate balancing act.

Participant attrition

Occasionally, in studies such as ours, which required repeated visits to schools and consequently repeated sessions with the same student, we might arrive at the school to find that the pupils we need to work with are not there, perhaps due to illness. If this happens, it really just requires a re-visit. However, in some research, it is common to find that participants might drop out entirely for one reason or another (see Seals, this volume). We were fortunate in our study that this did not happen, and we were able to follow through every child from the beginning of our study (at pre-test) until the end (post-test). However, for studies with a longitudinal component, it is important to factor in the very real possibility that sometimes events
happen which mean that individual children who began the study might not be able to finish it. Two things need to be considered here. The first is whether the participant has left the study because of the study itself. This is a problem because then it means there is a problem with the study design and most likely adjustments need to be made to enable participation to be more accommodating for the families involved. The second issue needing consideration is how many participants are needed in the first place in order to adequately address the research questions. In studies that are largely quantitative such as ours, we can do this by carrying out a power analysis (see Tabachnik & Fidell, 2013), which identifies the required sample size in order to reach appropriate effect sizes (should there be a statistical effect). After identifying the sample size, it is advisable to over-recruit in studies with a prospective component to ensure that even with a few cases of attrition, the sample size is robust enough to detect relevant effects (see Phakiti, this volume).

**Feedback to teachers and parents**

Another variable that we often encounter in carrying out research in schools with children is the notion of feedback. We have already mentioned that we regularly offer to present the results of our studies to participating schools, teachers, and parents, including inviting them to workshops where we can better engage in in-depth discussions. However, it is quite common that parents or teachers might ask for individual feedback relating to the performance of a specific child on a given measure. For example, in our study, we administered a range of English language tasks that assessed literacy skills, mostly through standardized tests. A standardized test often allows the researcher to identify where the child fits in the overall population. Understandably, parents and teachers are often quite interested to know how their child/children are situated within the population. We therefore are regularly asked to provide feedback on how an individual child performed on such measures; however, to provide this feedback would be a serious breach of ethics.

When participants are recruited into a study, their performance has to be anonymous, known only to the researcher(s). Indeed, often researchers themselves are blind to the identity of an individual child/participant if they code participants in the data file. We cannot, therefore, give feedback to a parent or teacher because we would be violating the agreement we made with the child – or the child’s parent in protecting that child’s anonymity. Additionally, within the context of research, we usually carry out tests with children to assess their performance for research purposes only. Even if we are appropriately familiarized with the psychometric properties of given tests we use (as we should be), that does not make us diagnosticians who are qualified to use standardized tests to report on individual performance on
any given child. We can, however, report group performance, which maintains the anonymity of all the individuals concerned.

Summary and implications for researchers

Working with children within the context of research projects is hugely engaging and enjoyable, not to mention extremely and increasingly important. We have highlighted only a few of the issues that need to be taken into consideration in carrying out research with young people. Fortunately, there are many guides available which provide much more detailed discussions of the practical and ethical issues surrounding research with children (e.g., Alderson & Morrow, 2011; Gallagher, Haywood, Jones, & Milne, 2010; Greig, Taylor, & MacKay, 2013; Harcourt, Perry, & Waller, 2011). It is also worth considering children as research partners, rather than sources from which we collect data. Noteworthy in this area is Pinter’s work (e.g., Pinter, 2014; Pinter, Kuchah, & Smith, 2013; Pinter & Zandian, 2014; 2015) in which she and her colleagues present a persuasive argument for thinking carefully about researching with children rather than on or about children. She identifies key ways in which conceiving children as researchers, and not mere sources of data, can offer researchers real and tangible insights into the nature of their language learning process. In her work, she argues that the specific challenges concerning children’s status and roles as research participants has been largely ignored in research generally, and applied linguistics research more specifically. Pinter (e.g., 2013 and elsewhere) reports that historically, in research contexts, children can be seen as either objects, subjects, social actors, or co-researchers, and it is the last category, she argues, that is particularly important for researchers in applied linguistics to begin to take note of. She points out that the questions that children ask, in acting as researchers within second language acquisition research, are inherently different. They reflect different concerns than those of adults and can lead to deconstructing perceived hierarchies and lead to new perspectives. We think this is a really interesting perspective and one that researchers who work with children should be aware of.

In summary, in this chapter we have presented a brief discussion of selective issues that are relevant in carrying out research with children. It is crucial to stress here that this is by no means an exhaustive list, and interested readers should most definitely familiarize themselves with the many excellent resources available which offer sound guidance for carrying out research with children, both generally and within applied linguistics research in particular. We are pleased to see a growing interest in work concerning children in applied linguistics and note that, commensurate with this interest, there must be an increasing knowledge and sensitivity to specific methodological issues of relevance in working with children. Research with children may not be child’s play, but it is certainly worth doing, and worth doing right.
Notes

1 It is perhaps worth noting that for this context ‘children’ refers to primary school-aged students aged between 5 and 11 years old. However, similar issues also are generally relevant in research with pre-primary and secondary students.

2 A waiting control group is one where the intervention is withheld during the study, but given to the control group at the end of the study. This procedure is predicated on the belief that the intervention will be beneficial, and the desire to ensure the control group participants do not lose out on an opportunity to benefit from the intervention.

3 Note that the data collection for this project took place prior to the introduction of Modern Foreign Languages (MFL) teaching in the primary curriculum at Year 3 – implemented in September 2014. This meant there were schools that did not offer any MFL instruction.

4 It should be noted that in England, to carry out work with children, it is often necessary to complete a DBS (Disclosure and Barring Service) check, but this may not be required in all countries. It is important, therefore, to check the requirements of the context in which the research is being carried out.

References


Chapter 11

Conducting longitudinal fieldwork among adult refugees

Lorna Carson

Introduction

This chapter highlights some of the issues I experienced during fieldwork conducted for my doctoral thesis in applied linguistics. My research project focused on the language needs of adult refugees in Ireland. I collected data on the learning journeys of a small group of students registered at Integrate Ireland Language and Training (IILT), the government-designated provider of English as a second language courses for those granted refugee status in Ireland. The project was exhilarating and exhausting in equal measures, and I now know that the specific challenges that I encountered were certainly not unique to my research project.

Researching what always seems to be described as a vulnerable group was an eye-opening experience, not least because many of the refugee learners I met in the classrooms at IILT were empowered and inspiring individuals who were seizing the opportunity on a daily basis to make a fresh start in Dublin. I also met students who had a sense of entitlement to social welfare support, who saw such support as a valid alternative to future employment, or who were only learning English as a way of passing the time. I encountered many grey areas during my fieldwork, which often led me back to my thesis supervisor and to the classroom teachers at the centre for advice on how to proceed. In the next section, I focus on a few aspects of the project (such as informed consent, confidentiality, and disclosure of personal details) that were particularly challenging to me as a junior researcher with little experience of longitudinal research in the field and no prior background in conducting research among adult refugees.

The study

The research project that I describe in this chapter was a qualitative, longitudinal investigation that explored the motivational role of goal setting in adult migrant language learners (Carson, 2006). I conducted fieldwork at IILT in the centre of Dublin. During a 15-month period (2003–2004), I
employed multiple data collection techniques including participant observation, group interviews with learners, teacher interviews, attitudinal questionnaires, and archival research. My project aimed to record and to analyze the motivational impact of a curriculum designed to encourage learner autonomy, as the learners at IILT used the Milestone version of the European Language Portfolio to set goals, record language learning activities, and reflect on their own progress. Case studies drawn from this doctoral research project were later published in Carson (2007; 2008a) as well as in an article which reported on the findings of the project as a whole (2008b).

IILT was, at the time, the government-designated body responsible for coordinating ESOL (English for Speakers of Other Languages) provision for adult refugees in Ireland. Adult refugees (and other ‘Stamp 4’ individuals who had been granted leave to remain in the country) were entitled to attend free English classes for approximately one year. IILT provided classes in General English adapted to living and working in Ireland at five proficiency levels from complete beginners as well as pre-vocational classes and Academic English for students who sought to ratify professional qualifications or enter higher education in Ireland. It also provided support, resources, and training to primary and secondary school language support teachers, and distributed language and integration classroom materials and resources free of charge to teachers and tutors throughout Ireland. The students who participated in my research attended full-time General English classes with 20 contact hours per week over a term of four months. In 2004, there were 529 learners enrolled in General English classes. All staff and the students with whom I was in contact knew that I was conducting fieldwork for my doctoral thesis: my role during the 3 months of familiarization and 12 months of data collection was as a volunteer classroom assistant. I spent at least two full days each week at the centre in this role and also attended special events and social occasions.

As a campus company established by the Centre for Language and Communication Studies at Trinity College Dublin, IILT was not a typical language school. Based on the work of David Little, the founding and managing director, all activities in the centre aimed to support learners in becoming autonomous language learners. Teachers systematically encouraged students to assume responsibility for the content and the modalities of the course through collaboration, reflection, and self-assessment (Little, 2009; Little & Lazenby Simpson, 2009). Students developed individual agendas which corresponded to their needs as well as a plan for their class as a whole. Retention and transition rates to further education, training, and employment were remarkably high, and IILT attracted international attention as a model of good practice.

Although IILT was involved in providing innovative and groundbreaking language support which was making a visible difference in its learners’ lives, its facilities were not quite up to the quality of its pedagogy. When I first
commenced my project, the centre was located in a repurposed industrial building which had been – just about – adapted to meet the needs of a language school. As with so many other associations and centres in this sector, the centre faced substantial funding challenges. Whilst the centre moved to a more appropriate location towards the end of my data collection, it is worth noting that the centre became one of the first casualties of the Irish financial crisis, when funding was diverted by the government in 2008. IILT ceased its activities in August of the same year.

At the time when I was writing my doctoral thesis, Ireland was in a period of transition from a country of emigration to a country of immigration. There were only 39 requests for asylum in Ireland in the early nineties (ORAC, 2006). A dramatic increase occurred in the years before my research project. In 1996, there were 1,179 asylum applications. By 2002, this had increased to 11,634 applications. During the time of my fieldwork, numbers had decreased to some 4,000 annual applications for asylum. The top-eight countries of origin of individuals in 2004 were Somalia, Iraq, Sudan, China, Iran, Zimbabwe, Nigeria, and Afghanistan (Irish Refugee Council, 2006).

Being a refugee is a difficult and complex life situation. A group of Irish scholars from different disciplines were beginning to publish incisive work on the struggles of refugees, the government’s responses, the legal framework, and the host community response (see, for example, Collins, 2002; Fanning & Mac Éinrí, 1999; Mac Éinrí, 2001; Maguire, 2004; Torode, Walsh, & Woods, 2001). I immersed myself in these readings, supplemented by regular reports in the press and in the NGO community on topics such as unaccompanied minors, the system of direct provision, ‘bogus’ asylum seekers, court interpretation, and the growing, very substantial, delays in the asylum process. Media coverage was often negative, and much of the Irish population was largely uninformed about the changing reality of asylum and immigration, and there was confusion about the terms used (refugees, asylum seekers, economic immigrants, illegal immigrants, etc.) (Breen, Devereux, & Haynes, 2008). In brief, asylum seekers are individuals who ask to be recognized as refugees, for instance, according to the definition in the Geneva Convention, and await consideration of their application. Successful applicants are granted refugee status, which offers refugees rights and entitlements similar to those of Irish citizens. Torode et al. described the response of the Irish population at this time of striking growth in the number of requests for asylum as “ambiguous, complex and varied” (2001, p. 59). Once individuals were granted refugee status in Ireland, they had considerable bureaucratic process (most of it just newly in place) to navigate including annual police registration, registering for social welfare support, obtaining a medical card, finding accommodation and arranging rent supplements or registering on waiting lists for Local Authority Housing, obtaining international travel documents, and accessing education, training, and employment.
Immigrants in general, and perhaps particularly asylum seekers and refugees, easily feel excluded and isolated, and may perceive discrimination against themselves, even if this is not the case, which can in turn generate feelings of resentment towards the host community. Without sufficient proficiency in English, some refugees may never access employment or anything more than minimum wage jobs and become victims of the disadvantages which accompany poverty, such as low self-esteem, loss of dignity, and mental and physical health problems. This vicious cycle leads to a culture of dependency and prevents refugees participating in processes of Irish society. Refugees, unlike other minority groups, are individuals who have fled their homes and taken the risk of seeking a new life elsewhere. The psychological and emotional effects of this disruption mean they may not have positive attitudes toward the host community. I witnessed during my time in the classroom at IILT that whilst some individuals were relieved to settle in Ireland and were looking forward to a new life, others were angry that they had to leave their country and desired to return home at the first possible opportunity. And of course, many students expressed in the same conversation feelings of relief and frustration, hope and anger, ambition and resentment. No research site is homogeneous in terms of participants, but the incredible diversity of human experiences, emotions, and aspirations that I encountered at IILT led to an exhilarating, sometimes just plain exhausting, and ultimately rewarding stint of longitudinal fieldwork.

Challenges and dilemmas

“You just assume that everybody brings their rucksack and their apple for break and they go home and do their homework diligently. It’s just not the case; it’s just not the case with our learners” (Class teacher, audio interview recording during fieldwork at IILT).

As my discussion at the end of the previous section suggests, language provision for adult refugees is a challenging educational context because of many and diverse needs of the learners. Conducting research in this context was also difficult. The 15 months of the project were hard to manage at times, and I faced a number of obstacles and dilemmas during my time at IILT.

First, working with a group of students with very clear language needs was something I was accustomed to, having taught EAL (English as an Additional Language) for some years prior to this project; however, the needs of the students at IILT went beyond acquiring a sufficient IELTS score. Equipping refugees with language skills means they are more likely to access training and employment, to become less welfare-dependent, and to participate in their local communities. Individuals move from passive recipients to active producers and take charge of their own lives. Some of the things they needed to learn were fairly straightforward: going to the shop, describing symptoms to a doctor, talking to a child’s teacher. These
types of topics were covered fully in class, and it was enriching to see the progress made by some learners. Other steps, such as putting down roots in a new community, gaining financial independence, participating fully in a child’s education, are more demanding. Learning English had an important psychological impact for many students, redressing exclusion, alienation, and depression. I saw first-hand how speaking English helped refugees take control of their lives, but I also witnessed students who remained helpless or closed off from what was being offered at IILT, who were always silent or afraid to speak, sometimes angry or cold, and who seemed unlikely to be able to cope with the demands of their new life in Ireland. Others gained a new voice and learned to speak out, not just in the classroom. These voices were heard in all sorts of places: the Iraqi student who had never dared ask any shop assistant anything but who started asking for samples of cosmetics in department stores as her first attempt at engaging with native speakers of English outside IILT, or the Bosnian student who had been previously too scared to complain about poor service in Dublin – she returned a faulty product to a shop with her receipt and was thrilled to recount receiving a full refund after rehearsing her complaint in class. Other students described daring to speak with their neighbours for the first time (‘nice day, isn’t it’), or learning how to respond to the greeting ‘How are you’ with the typical response of ‘Fine, how are you’ (a greeting which, in Ireland, doesn’t expect any account of health or well-being!).

One of the biggest obstacles was related to conducting research among a vulnerable population – although many individuals who participated in my research would have denied that they were vulnerable. Responsibility lies with the researcher to ensure that no harm comes to any individual because of his or her participation in the study, and I felt a particular burden to ensure that any involvement in my study would be a benign process. During the process of obtaining their informed consent to participate in my data collection, I could see that it would be easy for me to skim over, for instance, implications arising from their participation in the project – such as quotations from their interviews appearing in published articles and in my teaching slides as well as in my PhD thesis (something that some of them referred to as my ‘big book’). For financial and practical reasons, I was unable to provide translations of my information leaflet and consent forms into all the languages represented in the classrooms I was researching (some very small language varieties were represented, for example).

In any research project, a fundamental step to protecting research participants is a complete guarantee of confidentiality. My project involved following 13 learners during the 12 months of data collection, and as IILT was a relatively small place, it would have been easy for a reader to figure out the identity of the 20-year-old Afghan female in the complete beginner class in the autumn of 2003. Just providing a pseudonym would not have been sufficient to protect her identity. In other words, I needed to disguise the
identities of the group in such a way, yet without confounding important aspects of the study.

I also experienced first-hand that when trust has been established between a researcher and a group, it is often difficult to stop individuals from sharing some sensitive personal details, even during audio recordings, when the cassette recorder was clearly visible on the table in front of us. This kind of sharing as relationships develop is especially likely in a longitudinal study. Some learners eventually forgot that I was working on my doctoral dissertation and may have seen my central role as classroom assistant. During several of the group interviews with students, stories were shared about some very difficult, indeed visceral, experiences: an arrest, a miscarriage resulting from a fall at home, many accounts of racist attacks both verbal and physical, discussions of ways that some refugees had exploited the various social welfare programmes, and criticisms of some aspects of IILT (as well as praise too). When it came to transcribing these interviews, I faced the dilemma of deciding whether or not to include everything that had been said. I also heard and recorded many statements which were not palatable (see Appleby, this volume, for similar discussion), and indeed would have fuelled much of the anti-migrant sentiment in the press – for instance, how the generous social welfare programme in Ireland meant that individuals would never have to work again or whether it was worth working if it meant giving up a medical card.

Finally, this was my first longitudinal study, and I worried constantly about my sample size, attrition, cooperation of both students and staff, whether I would obtain sufficient data from the repeated questionnaire administration and so forth. Some of the other challenges I experienced during my doctoral research project have been tackled by other chapters in this book and included how to manage attrition among my group of research participants (see Seals, this volume) – as the composition of the classroom was constantly in flux – and how to tackle a large dataset with multiple data sources and participants.

**Overcoming these challenges**

Working in this particular research context was a fairly steep learning curve for a young researcher. It was immediately clear that I had a lot to learn from the staff at the school who had many years of experience in teaching and working among vulnerable population groups, and conversations in the staff room were especially enlightening. I learned not to rush away from the centre, but instead to stay around for a while and to ask questions. Without realizing it explicitly, I learned the importance of a regular debriefing procedure. Another way of dealing with tears of a student in the classroom or the challenging stories I had just heard (about racism, poverty, exploitation) was through maintaining regular field notes. I wrote up approximately one
page of notes each day I was present at IILT, and this diary provided a way of making sense of some of the experiences I was witnessing. Sometimes, what seemed like minor incidents became the memorable ones. I recorded one instance of tears in the classroom in my diary, and the class teacher remembered it some months later in an audio interview. My log entry read,

Alina was sitting in her usual corner, and on either side of her were Musa and Sergei. We were going round the class asking how to get to a place and giving the directions. Susan [the class teacher] asked Alina a very simple question and she seemed to get a bit flustered, and then Musa and Sergei repeated (I think) what Susan had asked her in Russian. Alina speaks a bit of Russian, but those two were on either side both talking to her whilst Susan asked her the question again, and then said, we really have to speak English in this classroom. Alina then burst into tears, and Susan felt so awkward. The class could all see what was going on as we were sitting in a semi-circle with all the desks pushed beside each other. Alina went bright red, very very embarrassed. I think Susan didn’t want to stop the class as she would have drawn even more attention to Alina, who is so shy.

(Carson, 2006, p. 147)

The class teacher’s description of the same incident confirmed that she was aware that although Alina knew what had to be done in class, two distracting voices speaking in another foreign language were a pressure she could not cope with: “Naturally she knew she should be speaking English in English class, she didn’t need to be told that” (ibid.).

I also shared my field notes regularly with the two teachers whose classes I was observing, and they talked through their perspective on the same events as well as recording their own account in the margin of my notes. At the end of the project, when I conducted a long and in-depth group interview with both teachers, there was a clear sense of shared ground. This particular interview yielded some very valuable and insightful comments which helped me make sense of the data I had collected from students. In this way, I learned not only how to be transparent about what I was writing up (which would become public down the line anyway), but I also learned about the value to sharing and triangulating my emerging thoughts, ideas, and ways that I could try to understand what I was witnessing.

In my thesis and the ensuing peer-reviewed articles, I provided subjects with pseudonyms and slightly altered some identifying features (e.g., ages and assignation of a neighbouring nationality) to ensure that all identities were comprehensively protected. Although I felt I was very careful in creating a new set of identities for my 13 participants, one reviewer of a peer-reviewed journal called this into question when it came to publishing part of my doctoral thesis. I felt somewhat offended that the reviewer was
challenging me on the care I had taken to mask the participants’ origins and ages, but on reflection, it was good that a reviewer had tackled this aspect of working with a vulnerable population so specifically. I was able to satisfy the journal editor that the identities of my participants were protected fully through the measures I had taken. Seeing this process in action highlighted the importance of explicitly outlining the confidentiality procedures used when reporting on work with vulnerable groups, even more so than the usual assurances offered in published research.

As I mentioned earlier, some of the relationships that developed with learners during my time at IILT became very close, especially with the female students. IILT did not limit language learning to the classroom, and I accompanied the class on regular trips to museums and other destinations in the city, and eventually I began to meet some of the students outside class for coffee or lunch. I became friendly with one particular student who is now a close family friend. Hospitality and reciprocating friendship is often an important part of the process of putting down roots in a new home, and over food we shared stories together which were not just about language learning. By the time it came to conduct interviews, trust had been established but I was not prepared for intimate stories to be shared on tape, as I describe earlier. Following these experiences, I immediately spoke with my PhD supervisor about how to treat the data without divulging the specifics of the conversations. I was advised to ensure that I treated the cassettes themselves with great care when transporting and storing them, and especially when digitizing them, but to transcribe as much as possible without compromising identities.

In terms of the interview transcriptions, I therefore omitted some information that could have directly identified any participant in the future. The aspects omitted were small, but included details such as the number of family members in the household or the specific area of Dublin where the individual was living. However, I also learned to be sparing about omissions when there was nothing said that could identify the individuals who were speaking. I included all the statements which I (and perhaps my readers) found unpalatable, such as when students criticized aspects of life in Dublin, when they described untenable host community attitudes to refugees, exhibited a sense of entitlement to all possible social welfare supports, or shared exactly how to make the most of the system. I learned not only to include what I may have disliked but also to write sparingly and to allow the data to speak.

For example, one interview included a long debate between participants named Rose and Nicolae. Rose argued with Nicolae that simply taking English classes because they were available, or even learning just enough English to cope, was unacceptable to her. Part of her debate with Nicolae was about a hypothetical refugee who did not speak English and was employed as a driver. She pointed out that the low income jobs that do not require much English were problematic if the worker falls sick, “maybe he lose his leg” (Carson, 2006, p. 161). Nicolae’s response was that he would be taken care
of by Social Welfare, “If he lose his leg, it’s for social pay!” Rose was adam-
antly that this was the wrong attitude. Later in the interview, the conversa-
tion again turned to the importance of speaking the language of the host 
country in order to integrate fully. Nicolae continued to contest this point, 
and Rose became incredibly flustered and angry with her fellow student. 
Their words painted a much richer picture of the complexities of their situ-
tion than my description could have ever achieved.

**Suggestions for researchers working in similar contexts**

On reflection, my experience conducting fieldwork at IILT was a research 
apprenticeship in a fairly literal sense. I was able to turn readily to my thesis 
supervisor for practical advice when I encountered a dilemma such as what 
details to include or exclude. Asking for help in these situations is a must – 
especially when issues of consent and confidentiality are concerned. The age-
old advice of ensuring that your dissertation supervisor is someone who you 
can rely on is a crucial part of completing a successful research project in a 
context such as the one I described earlier. One way of determining this before 
you start your project is to talk to other people who have been supervised by 
the academic in question about how their fieldwork went and whether they 
{} felt they had sufficient support from the supervisor in question.

My concerns about confidentiality, revealing identities, and obtaining 
inform consent are all crucial aspects of conducting good research. 
However, each situation brings a different set of ethical issues to bear (see 
Okada, this volume), and no amount of reading can prepare you for the 
challenges that you will encounter in the field. This lived experience of learning 
how to research in an ethical manner (rather than simply obtaining ‘ethi-
cal approval’ in a university, perhaps seeing that as a box-ticking exercise) 
was something that the teachers and learners at IILT helped create for me, 
through fostering a culture of honesty and openness about the aims of my 
project, my data, field notes, interview transcripts, and so forth. I should 
say that I have been the chair of my department’s ethical review committee 
for some years now and that this first experience of researching extensively 
among a vulnerable group was a seminal part of my training for such a role, 
without realizing it at the time!

Then, setting up a way to debrief regularly during fieldwork is something 
that I had not imagined I would need or want to do, but in fact the oppor-
tunities to talk through when I heard something difficult, such as a racist 
attack or personal trauma, afforded me a way of managing my own emo-
tions and helped me through the project. Many universities provide a formal 
system of confidential debriefing for researchers in the field, and if that is the 
{} case in your university, do consider it as an option. Otherwise, even keeping 
a diary during fieldwork is a good way of processing what you see and hear.
On reflection, more than a decade on, I suppose when I started my project I was fairly idealistic about conducting research about adult refugees. I was sympathetic to their situations, and frustrated by the lack of a joined-up response by the Irish government. I had read all the guides on how to conduct fieldwork, but I had no idea about the reality of working with a ‘vulnerable’ group. I learned through my time among this heterogeneous group of people that the dataset I was assembling was more than rich, and indeed, the voices I recorded there continue to speak.

References


Chapter 12

Researching people with illnesses and disabilities

Ethical dimensions

Hanako Okada

Introduction

The value of illness and disability narratives has been foregrounded in the field of the sociology of health and illness. Such narratives not only offer insights to medical practitioners and researchers regarding the patients’ experiences (Hamilton, 2003; Kleinman, 1988), but they also help overcome dualisms in the discourse of health and illness, such as mind/body, biological/social, and impairment/disability. Furthermore, they weave together bodily, emotional, and sociocultural experiences and construct meaning from them, and provide voice to the otherwise silenced patients (Bendelow & Williams, 1995; Charon, 2006; Frank, 1995; Kleinman, 1988), thus functioning as a source of empowerment (Ellis & Bochner, 2000). In the field of applied linguistics, the examination of the roles of language in health-care settings, such as doctor-patient communication (e.g., Hamilton & Chou, 2014; Jones, 2013) and the linguistic representation of the self/body and identity in illness and disability have been a topic of inquiry (e.g., McPherron & Ramanathan, 2011; Ramanathan, 2010). In both health-care communication and “languaging” of the body (Ramanathan & Makoni, 2007), narrative is a salient theme as, to use Gwyn’s (2002) words, “within the discourse of medicine, narrative is not simply an expository or descriptive facility, but the most convincing resource with which to make rhetorical acts” (p. 3), particularly given the elusive and amorphous nature of bodily conditions.

In this chapter, I draw from my experiences working on two of my studies (Okada, 2011; 2014) where I examined how people with illnesses and disabilities negotiate, construe, and communicate their experiences through narrative. The aforementioned power of narrative strengthened my studies, but at the same time, the narratives that the participants told also brought forth a dilemma in the area of research ethics, as the participants had their own agendas in the telling of their stories. I reflect on this dilemma along with the more obvious issues that involved bodily constraints that I have
faced while working with those with illnesses and disabilities, and discuss what researchers could pay special attention to when conducting research with such populations.

The study: Narratives of embodied experiences (Okada, 2011; 2014)

The first study, “Negotiating the Invisible” (Okada, 2011), focused on the narratives of embodied experience of two women with ‘invisible’ chronic illnesses: Shiori with fibromyalgia and chronic fatigue syndrome (CFS), and Mia with rheumatoid arthritis. Both women had no visible symptoms and appeared “healthy” by outward appearance. Due to this invisibility, situating and communicating their bodily experience was challenging and simultaneously crucially important as those around them, including health-care professionals, struggled to understand their situations and alleviate their suffering. This lack of understanding also affected the women’s understanding of their own situations and how to act on that understanding. In this study, I employed Merleau-Ponty’s (1962) embodied phenomenology as a theoretical guide, and analyzed the narratives to investigate how these women narratively perceived, negotiated (or failed to negotiate), and communicated their embodied experience.

The second study, “And Then There was a Battle” (Okada, 2014), focused on the narratives of Emi, a young woman with sensory disabilities. Specifically, Emi could not hear out of her left ear and had a severe case of amblyopia. Given that her vision and hearing were partially – but not completely – impaired, although Emi started her formal education in the school for the blind, she received mainstream education for the most part of her life. Due to the complexities of her disabilities and educational environment, like the participants in the first study, negotiating and communicating her experience was important as her peers and teachers struggled to understand her situation and challenges – they were unsure of how much she could hear or see, and what she was capable of. In this study, I took the approach of identity as a “performative struggle over the meanings of experience” (Langellier, 2001, p. 3) and analyzed not only what was being told but also how Emi used performative features and social positioning in her narratives to negotiate and communicate her experiences of struggles and to construct her identity as a disabled person.

These two studies had several commonalities. First, the approach to inquiry: the narrative data were elicited primarily by multiple open-ended narrative interviews (Elliot, 2005). Additional data sources such as email messages, field notes, and reflective accounts were also used in the narrative construction. Second, and also regarding the approach to inquiry, my positionality vis-à-vis my participants played an important role (see McKinley,
this volume), as I had previously known the three women. Shiori and Mia were my friends, and Emi was a student in my class in the university where I previously taught. I also had the same chronic illnesses that Shiori had—a reality all three participants were aware of to varying extents. Thus, as a researcher, friend, teacher, and a bearer of illnesses, I was situated in the research (Haraway, 1988), and this may have affected the participants’ process of narrative construction. Third, the salient theme in the narratives of all three women was struggles from not being properly understood. Although the conditions of the women varied, their illnesses and disabilities were ambiguous to others, and sometimes even to themselves—for Shiori and Mia, the cause was the invisibility of their illnesses; for Emi, it was the complexity and partialness of her sensory disabilities. For the purpose of this chapter, and because of space constraints, I focus on my experiences with Shiori and Emi, as they were the two participants who had conditions that were more ambiguous and not well known to the public, especially in Japan.

The research dilemmas that I faced

Changes in the data collection period: Cooperative participants but uncooperative health

Despite their bodily constraints, all three women were cooperative and willing to engage in my studies. However, there were issues that got in the way of moving on with my projects smoothly because of the participants’ health. Upon planning interview locations and dates, I made an effort to consider the participants’ conditions and bodily constraints along with their schedules. I made it clear with the participants not to push themselves and to prioritize their health. I asked Shiori and Mia to choose the interview location of their choice, including their homes, my home, and hotel lounges with comfortable chairs and sofas with cushions, as they had pain in their bodies. Shiori’s first interview was held in my apartment. Shiori told me she was feeling fine, and she also appeared well to my eyes, and we started the interview in a relaxed and pleasant manner. However, approximately halfway through the interview, I noticed that Shiori was looking pale and rather drained. I suggested that we stop and told her that she did not need to worry about my schedule, but she insisted on continuing. I would have forced her to stop if she was doing this out of obligation, but I also knew that she wanted her story to be heard (to the larger public through me), because both CFS and fibromyalgia were not commonly known, particularly in Japan, and because of the aforementioned invisibility of these illnesses, she was often misunderstood by her family, colleagues, friends, and even medical professionals and received hurtful comments such as “it’s all in your head,” and “snap out of it.” She continued telling her story, but the more the time
passed, her story seemed to drag, lacking order and detail compared to the first half of the interview. I felt caught between honoring her desire to tell her story and asking her to stop for her health (and I must admit, also for my ego as the researcher wanting ‘quality data’).

Unlike Mia and Shiori, because Emi’s disabilities were sensory, I was initially less worried about her general health. We agreed to meet once a week for an hour after class over a five-week period. However, shortly before the first interview date, Emi began to have problems with her right ear – her hearing ear – and had to be hospitalized for a few days. Even after the hospitalization, she continued to miss classes, as she was not feeling well enough. It was through this sequence of events that I learned that Emi was quite frail. The first interview finally took place a month after the original scheduled date. Although the initial plan was to meet every week, the second interview took place another month later as Emi continued to feel weak. Therefore, the data collection period ended two months past the original schedule. I had anticipated some disruption with the schedule, but I did not expect this much gap and delay (see Seals, this volume, for discussion of participant attrition in fieldwork studies).

The participants’ purposes versus the researchers’ purposes: An ethical obligation

Although I did not expect other health issues from Emi, what happened with Shiori during the data collection period was not surprising given that she was suffering from chronic illnesses. However, the most prominent difficulty that I had with these two studies was somewhat unexpected.

Upon negotiating participation, I had explained the nature of the studies to the three participants. All three understood that I was an applied linguist, and that the studies were about narrative, specifically, how the participants negotiate, construe, and communicate their experiences through narrative, and that I was not simply reporting or retelling their life stories (see Prior, this volume). Even though the women understood my purpose in collecting narrative data, their purpose was to be heard. This was particularly important for them, as their conditions were either unknown or not well understood by the public. In their stories, Shiori expressed her strong desire for fibromyalgia and CFS to be better known to the public, and to be researched further as the cause and cure were unknown, and Emi explicitly mentioned that one of her goals in life was to raise awareness and educate Japanese society about sensory disabilities. Both Shiori and Emi longed to be understood – in fact the most salient and prominent theme in both of their narratives was not bodily struggles due to their illnesses or disabilities, but struggles from not being understood due to the ambiguity of their conditions and disabilities. This may be in part because of my researcher positionality, but I was moved by their stories and their pressing desire to be
understood and began to feel obligated to at least take some part to assist in informing the readers about their illnesses and disabilities. Even though the participants willingly took part in my studies, it was an ethical imperative for me not to deconstruct the participants’ stories into bits of decontextualized categorical chunks or sequences of ordered clauses that do not convey the essence of what the participants wished to be conveyed. The question was how am I going to do this? How am I going to fulfil my purposes as an applied linguist/researcher while informing the readers about illnesses and disabilities?

Overcoming the obstacles

Maximum flexibility and multiple interviews

In my past experience as a qualitative researcher, there were times when I had to be flexible; when we deal with human beings, unexpected happenings occur. Working with those with illnesses and disabilities required maximum flexibility on my part. Even though the participants were willing and responsible, as mentioned earlier, their bodies were not always cooperative. More time was needed, and I had to revise my plan and reschedule interviews; luckily, I had set aside enough time so that I did not need to cut short the data collection.

Obviously, when the participants were not well enough to take part in the interviews, all I could do was to wait for their recovery. However, a case like Shiori’s was more complex, as she was not feeling well, but still wanted to proceed with the interview. It may have been her wish to be heard (to the larger public through me) and/or to benefit from the therapeutic and emancipatory nature in the telling of the narratives (Bell, 1999; Ellis & Bochner, 2000), or it could well have been her sense of obligation as a research participant. While it was an ethical obligation to take care of my participant, it was also a moral responsibility for me not to treat her as someone incapable of making her own decisions. Hence, I honored her desire to continue to tell her story, even though I was not exactly satisfied with the quality of her account and refrained from asking further questions that would initiate a new story.

As a solution, I asked Shiori for an additional follow-up interview at the very end of the data collection period to confirm some points that were either left out or too thin. In the follow-up interview, perhaps because she was feeling better and had more energy, and as she was retelling parts of her story, Shiori provided more detailed and vivid descriptions and anecdotes, adding more depth and layers to the original story. The follow-up interview not only functioned as a supplementary data source, confirmation of details, and member check for some my preliminary analysis but also as an opportunity for us to collaboratively reflect on her story. This allowed me
to hear what Shiori had thought about the whole interview process and to ask her whether I could use some of the narrative segments that included sensitive issues. Learning from this experience, I also conducted a follow-up interview with Emi for the same purposes.

Embedding the participants’ wishes in the study

In her discussion of ethics in qualitative research, Ryen (2011) posed that ethical dilemmas are “emergent and contextual and call for situational responses” (p. 417). My sense of moral obligation to inform readers about the participants’ illnesses and disabilities emerged as I listened to their stories of struggles. While I had no intention to turn my studies into participatory action research or advocacy projects, I did my best to include details in the study that would raise the readers’ awareness of the illnesses and the disabilities.

Obviously, I had originally planned to include the descriptions of Shiori’s illnesses and Emi’s disabilities when introducing the participants in the methods section of the studies. Although space constraints precluded detailed descriptions, I added the important aspects that complexified their situations. Specifically, for Shiori (Okada, 2011), I provided the following details about CFS and fibromyalgia: they are chronic and may last for years, if not lifetimes; they have no known cures, and there are no quantitative diagnostic measures so they are “invisible in terms of their objective signs even to medical practitioners” (p. 149). I did not add further details in this section, as the chapter included Shiori’s story with her own words weaved into it. In ‘Shiori’s story,’ I made sure that the readers got a glimpse of the experience of what it was like to have CFS and fibromyalgia, while making sure that the story would not turn into a mere description of the illnesses. I purposely selected one of the earlier interview excerpts that powerfully captured Shiori’s experiences and presented it towards the beginning of the story as an introduction to her struggles, specifically, how her pain and suffering were not acknowledged or warranted:

So I went to the hospital claiming that I couldn’t walk and I couldn’t sit because of these pain. So they made special arrangements for me to go through MRI and things, but they couldn’t find anything wrong. . . . So they basically told me in the face that I wasn’t telling the truth, that I shouldn’t be feeling that pain. . . . I was overwhelmed, depressed, and if the doctors told me I shouldn’t be feeling the pains, I was doubting myself at the same time. Knowing I had pain, I doubted myself. It’s like “Ok, am I really faking it?” Because I mean they’re supposed to be the professionals in the field, and I don’t know anything about the medical background of illnesses and such.

(p. 151)\textsuperscript{2}
With data presented in story form, it was easier for me to provide further descriptions and quotes regarding her illnesses to enable readers to have a better understanding of the illnesses and the kinds of problems they manifested in Shiori’s life.

On the other hand, in the study that featured Emi (Okada, 2014), because the focus was on how Emi used performative features and social positioning in her narratives to negotiate and communicate her experiences of struggles, data were presented as excerpts, and not a story. Furthermore, as Emi chose to talk more about social struggles that rooted from the disabilities, such as getting bullied at school and her dilemma between choosing the school for the blind or a regular high school, there were fewer excerpts about the disabilities per se. Therefore, in order for the audience to be informed of her disabilities, I decided to provide a more detailed description as I introduced Emi as the participant:

Emi is amblyopic by birth due to deformed retinas. Although her vision was weak, she used to be able to see with both eyes; however, she lost the vision of her right eye due to a detached retina while she was in high school. She does have some vision, but is severely challenged: she uses the white cane to assist her walking, has a hard time identifying faces, and does not have a peripheral vision, meaning she can only see what she is immediately focusing on. For example, she may not notice someone who is standing right beside her. She cannot read small print and can only read one short word at a time; thus, she takes approximately twice or thrice as much time to read compared to a person with a normal, healthy vision. Emi also has a hearing difficulty by birth, and cannot hear with her left ear; she needs to be spoken to clearly and loudly from the right side, if not from the front.

In addition, because the amorphous nature of amblyopia was the culprit of her being misunderstood, I provided the following information about amblyopia in general: “Amblyopia is wrongly known as ‘lazy eye,’ and the types, symptoms, and degrees of severity vary significantly across individuals; therefore, amblyopic individuals cannot, and should not, be generalized” (p. 3).

I must add that upon doing this, I was faced with another dilemma: the descriptions of her unique set of disabilities made Emi quite identifiable. I explained to Emi the possibility that her identity might be revealed, but she did not mind, as she was aware that my publications and presentations were academic and not in popular media. However, I thought it would be best to protect her anonymity as much as possible. Therefore, I made further modifications to her non-bodily profile, such as the description of her university and her relationship with me, and not just the standard use of pseudonyms.
I purposely refrain from providing further details here, as doing so would lead to disclosing what I have attempted to conceal. 3

As mentioned earlier, Emi’s data was presented in the form of excerpts. Rather than presenting them as discrete and decontextualized excerpts, I presented them in chronological order and fronted each excerpt with an explanation to link the excerpts into a more coherent story. This was my attempt to provide a fuller picture of her life so that in the end, the audience could have at least some sense of the challenges sensory disabilities bring forth and at the same time understand something about the disabilities themselves.

**Implications for researchers: Towards an ethical research relationship**

The discourse of health, illness, and disability has shed much light on the studies of language and interaction (Hamilton & Chou, 2014). Such studies may also foster positive change; for example, a doctor-patient discourse analysis may contribute to reforms and refinement in training programs for medical practitioners. While most of these studies are not participatory action research or advocacy projects where the purpose is to contribute to social change, I assert that regardless of the different purposes of the research, researchers still need to consider ways in which they can contribute in getting the participants’ voices to be heard – particularly the vulnerable, otherwise often silenced populations such as those with illnesses and disabilities.

In order to do this, researchers must first and foremost be ethical. Being ethical is not a prescriptive act that only involves going through the International Review Board process and obtaining informed consent. While these procedures are certainly necessary, they tend to turn research ethics into an either-or issue (Edwards & Mauthner, 2002), and consider the participants as “an object upon which research is done” (Blake, 2007, p. 414). To embody an ethical research relationship with the participants, researchers need to be aware of the contextual and situational nature of ethics, and consider ethics not only as a procedure but also as a practice (Guillemin & Gillam, 2004). Performing ethical practice means keeping in mind the researchers’ moral responsibility to the participants at every stage of the research project (cf. Shaw, 2008) and being flexible with and open to the participants regardless of the purposes of the research, bearing in mind “the gift you [they] are being entrusted with” (Atkinson, 1998, p. 37) by the participants. 4

Not only should researchers be reflexive, but they should also regard their participants as reflexive individuals with desires, needs, agendas (Blake, 2007; Guillemin & Gillam, 2004), opinions, and bodily conditions and honor the telling of their narratives as “part of the human, existential
struggle to move life forward” (Ellis & Bochner, 2000, p. 746). Although researchers may not always be able to honor the participants’ wishes or prioritize their agendas, even a minor adjustment or addition, such as providing a fuller description of an unknown or misconceptualized disability to raise the readers’ awareness makes a difference. While absolute ethicality may be impossible, being responsive to the dilemmas, tensions, and contingencies that occur at every stage of the project and being prepared to face them with moral responsibility may be what defines ‘being ethical.’ Such awareness and sensitivity promote ethically grounded research and are imperative to not only those researching vulnerable populations or to other kinds of narrative researchers but also to all researchers dealing with real lives of real people.

Notes
1 All participant names are pseudonyms.
2 These are Shiori’s own words. The interview was conducted in English – the language that we normally communicate in.
3 This kind of decision making is an example of reflexive ethical practice which I discuss later in the chapter.
4 Also see the following for discussions on ethical issues: in qualitative research in applied linguistics, Rallis and Rossman (2009); in narrative research, Clandinin and Connelly (2000); in qualitative research in general, the 2004 special issue of Qualitative Inquiry dedicated to research ethics and regulation.

References


Chapter 13

Conducting research with deaf sign language users

Lorraine Leeson, Jemina Napier, Robert Skinner, Teresa Lynch, Lucia Venturi, and Haaris Sheikh

Introduction

This chapter looks at the complexity of securing funding and subsequently negotiating multidisciplinary research projects with a specific population: deaf communities of sign language users. While texts on research methods in applied linguistics tend to focus on considerations of the range of generic approaches that one might consider for a study, and institutional requirements such as seeking to secure research ethics approval for qualitative data collection (see, for example, Dörnyei, 2007), the complex ecosystem that is the broader research context is often left undiscussed, with some notable exceptions (see Hale & Napier, 2013).

For us, this broader frame, illustrated in Figure 13.1, constitutes the research ecosystem that we operate within. It includes the requirements of a funding agency (which determine *inter alia* if funding is awarded and how funding supports the priorities of the funding programme at the EU level and within each partner country), those of the institutions we work for, and, often juxtaposed against these frames are the communities our research seeks to serve: deaf sign language using communities and stakeholders working with signing communities, which for the purposes of our research is predominantly the community of sign language interpreters. Our work is further complicated by relationships that hold between deaf signing communities (as linguistic and cultural minority communities) and majority communities made up of non-deaf (hearing) people, and by our own positions vis-à-vis deaf communities as deaf/hearing researchers with sign languages as a ‘mother tongue’ or who have acquired a sign language as a second or subsequent language. This positionality impacts on the approaches we take in planning, implementing, and reporting on our work. It also impacts on how we are seen, experienced, and engaged with (or not) by the key stakeholders we set out to recruit as participants in our work.

Essentially, we seek to conduct action research as a practical approach to professional inquiry in any social situation (Carr & Kemmis, 1986) in order to understand, and ultimately improve, access for deaf sign language
users to services and also to understand and improve sign language interpreting practices. In this chapter, we present a case study of one particular action research project funded by the European Commission – the Justi-signs project – and report the key challenges faced with respect to various aspects of the action research process.

A note on deaf communities

It is conventional in the deaf studies, sign linguistics, and sign language interpreting literature to distinguish between deaf people who use a sign language and identify with other sign language users (using a capital ‘D,’ Deaf) and those who have a hearing loss but do not use a signed language
or identify themselves with a community of signed language users (‘deaf’). Napier and Leeson (2016) note that because of the complexities of sign language transmission and the evolving nature of the deaf community due to medical interventions and changes in educational policy, greater numbers of deaf people come to the community as late learners of sign language. As a result, they note that definitions of deaf community membership are changing, and they use ‘deaf’ as a generic term, making no judgment about the hearing and linguistic identity or status of people who use a sign language and suggest that in order to encompass all sign language users (deaf or hearing) in applied linguistics terms, then the term ‘signing communities’ may be more appropriate. Following this principle, the only time we retain the D/deaf convention is when directly quoting other authors.

Situating our work: Intersectionality, privilege, and responsibility

While we see our work as occurring in the space of applied linguistics, it equally ‘belongs’ in the multidisciplinary field of deaf studies. The term deaf studies was coined in 1984 following the establishment of the Centre for deaf studies at the University of Bristol in 1978 (Marschark & Humphries, 2010). Over time, researchers concerned with documenting and exploring the language, culture, and lives of deaf sign language users from a social rather than medical model began to identify as deaf studies scholars. That is, these scholars aligned their work with “the Deaf community’s view of deafness as a cultural identity rather than a disability” – a view that “contradicts the medical community’s perception of deafness as a disease or deficiency in need of correction or elimination” (McKee, Rosen, & McKee, 2014, p. 2174).

The term ‘deaf studies’ has subsequently become synonymous with “sociological, anthropological and ethnographic explorations of deaf lives” (Marschark & Humphries, 2010, p. 2) and our work falls within this scope, including work we report on here, concerned with European deaf people’s access to justice as part of the European Commission funded project, Justisigns (www.justisigns.com).

Who you are as a researcher, and your relationship to your informants, is important (see McKinley, this volume). For example, Ganga and Scott (2006, p. 1), discussing sociological research on migrants, argue that “to a large extent, interviewing within one’s own ‘cultural’ community – as an insider – affords the researcher a degree of social proximity that, paradoxically, increases awareness amongst both researcher and participant of the social divisions that exist between them.”

Cameron, Frazer, Harvey, Rampton, & Richardson (1992) began early discussions of the unequal power relationships that can be evident in applied linguistics research and urge researchers to work with all stakeholders. This message is also clear in applied sign linguistics and deaf studies, with
emphasis on the need for a ‘community participatory approach,’ for research to be with deaf sign language users (Napier & Leeson, 2016; Turner & Harrington, 2000), and for research to be ethical and ‘deaf-led’ (Harris, Holmes, & Mertens, 2009; Hochgesang, Villanueva, Mathur, & Lillo-Martin, 2010; Mertens, Harris, & Holmes, 2009; Singleton, Martin, & Morgan, 2015).

For us, within the field of deaf studies, our audiological status and our position vis-à-vis the deaf community and indeed, interpreting community, can impact on how one is received. In deaf studies, there have been attempts to separate the identity of hearing people who are involved in the deaf community from those ‘other’ non-deaf people who do not use sign language and who are considered as ‘outsiders’ (see Ladd, 2003; Napier, 2002). Given the importance of positionality, and what that means for expectations of how we approach our research, engage in data collection and reporting, we would encourage the readers to take a moment to read our biographies at the beginning of this book in order to understand who we are as writers and what our positions are as sign language users. Given our multilingual, multicultural, individual, and collective backgrounds, our driving goal is positive social change. We believe that applied sign linguistics research on and with signing communities cannot occur in a vacuum. And it most certainly cannot happen without the engagement of deaf experts – academics and community leaders. Deaf studies researchers, especially hearing researchers, are guests in the deaf community (O’Brien & Emery, 2013; Napier & Leeson, 2016). Reciprocity is a key value of deaf signing communities (Mindess, 1999), and as such, a social justice agenda, an action research agenda that seeks to bring about sometimes subtle and sometimes paradigmatic shifts in how deaf communities are seen, understood, valued, and, in terms of public services, provided for, aligns with this cultural value provided that there is dialogue with deaf community leaders and ongoing engagement across and beyond the life of a project.

Yet there are intersections (after Crenshaw, 1991) that exist in the power relations that hold between deaf signing community members and those who typically research deaf communities. There are intersections of power based in ‘hearing privilege’ (www.deafecho.com), even when you come from the deaf community, have deaf parents, and have a sign language as your ‘mother tongue.’ Hearing privilege is described in analogy to ‘white privilege’ (McIntosh, 1989), as “a way of conceptualizing racial inequalities that focuses as much on the advantages that white people accrue from society as on the disadvantages that people of color experience” (Frederickson, 1981, p. 23), hearing privilege is when hearing people view their social, cultural, and economic experiences as a norm that all deaf people should experience. It is a privileged position; hearing people possess an undeniable advantage over deaf persons. . . . hearing people have a physical advantage (being able to hear), but the privilege itself remains.

(Napier & Leeson, 2016, p. 11)
For those who have entered into the community as second language learners, engagement is predicated on trust, a relationship that cannot be fast-tracked, and one where introductions from core community members are de facto vouched references (for example, see Higgins, 1980). More recently, the discourse of privilege has been referenced, with consideration of how hearing people recognize their ‘hearing privilege,’ something that we, as researchers, are particularly cognisant of.

**Privilege: What it means for us in this project**

Privilege also entails negotiating the position of the researchers (deaf and hearing) vis-à-vis the signing community and other communities in which a research project is situated and being explicit about who we are, where we are coming from, and our commitment to signing communities (Napier & Leeson, 2016). For researchers who are deaf, we ask to what extent are we aware of the responsibility we have as deaf academics, and are we bringing our privileged position to bear in appropriately representing deaf community perspectives and values when we engage in the research process? For us collectively, this entails considering where we spend our time and effort – are we spending precious research time on topics that are meaningful for signing communities? What does that even mean? Will the work we do have impact for deaf sign language users? Will it make a tangible difference? For those of us who are not deaf – even if we have grown up in the deaf community with deaf parents and are L1 users of a sign language – are we aware of our ‘hearing privilege’? How do we make our work congruent with the values of signing communities? And, if we strive to do this, are we in some way undermining our ambition to positivist, impartial analyses of the data sets we gather? Or do we acknowledge that the filter through which we view Deaf communities and their engagement with ‘hearing society’ (and vice versa) necessarily shapes our perspective? These are questions that we have explicitly addressed across the project that we are collectively working on at present – *Justisigns*.

**The project**

The *Justisigns* project (www.justisigns.com) focuses on deaf sign language users’ access to justice, with a particular focus on engagement with police forces. Project partners include pan-European NGOs (The European Forum of Sign Language Interpreters, the European Legal Interpreters’ and Translators’ Association) and a number of academic institutions (Trinity College Dublin [Ireland], KU Leuven [Belgium], Heriot-Watt University [Scotland], HfH Zürich [Switzerland], and a project coordinating and management team [Interesource Group Ireland Limited]). Furthermore, the project was carried out through forging further collaborative partnerships with key stakeholder organizations, such as the Irish Gardaí, the Dublin Rape Crisis Centre, Police Scotland, and the respective national deaf and interpreter associations in each partner country.
The project is predicated on the highly limited empirical analysis of the bilingual, bimodal, intercultural engagement that occurs as a matter of course when deaf people engage with law enforcement officials. Justisigns is timely given the passing of a number of European Union (EU) directives in recent years that relate to the position of those who come in contact with legal systems in the EU as accused persons, witnesses, or victims. Key among these is Directive 2010/64/EU, which establishes common minimum rules for EU countries on the right to interpretation and translation in criminal proceedings as well as in proceedings for the execution of the European Arrest Warrant. A second legal instrument, Directive 2012/29/EU, established minimum standards on the rights, support, and protection of victims of crime. It also makes explicit reference to the need to offer support for victims in a language that they understand, which may entail the provision of linguistic assistance.

To effectively document and benchmark provisions and map current practice against the requirements of the Directives, a mixed-methods approach was undertaken. Mixed-methods approaches are increasingly valued in applied linguistics and interpreting studies in order to examine real-world professional practice (see Dörnyei, 2007; Hale & Napier, 2013), and we have strongly recommended this approach for sign language interpreting research (Leeson & Napier, 2016). Our research approach included a pan-European survey, focus group meetings or one-on-one interviews with interpreters (hearing and deaf), police officers, deaf sign language users who had come in contact with the justice system, and their families, as well as lawyers/chaplains/social workers, and discourse analysis of an authentic sign language interpreted investigative police interview (provided by Police Scotland).

The research problem

Since the project began, we have encountered several challenges to be negotiated throughout the project, which we present here. These challenges can also be mapped to the linear, intersectional, hierarchical, and horizontal relationships that hold between researchers, their funders, signing communities, interpreting communities, and other stakeholders key to the research process as illustrated in Figure 13.1.

Challenge 1: Securing funding or “if at first you fail, try, try again.”

The Justisigns project went through a number of incarnations over a decade of seeking funding – often reviewers scored the application highly, but funding was not forthcoming. When the project finally secured funding, the funders initially refused to provide funding for third-party interpreting costs; however, the consortium was able to successfully appeal this initial decision. A surprisingly common struggle when working with signing communities is the challenge of raising awareness within the funding agencies of the need...
to fund interpreting costs. Logically, the only way to ensure participation in the project teams for multidisciplinary, pan-national projects where not everyone knows the same sign language – indeed, not everyone knows how to sign – is by ensuring that high-quality interpreting is provided.

The European Union of the Deaf regularly points out the fact that funds for sign language interpreting have to come from within a project’s funds rather than from a separate fund that might be dedicated to accessibility. This negatively impacts on what a project that includes sign language users might attain, as a significant portion of sub-contracted costs are spent on interpreting, which could be better spent on other aspects of the project. In contrast, consortia made up of all hearing people do not incur this cost; while they may incur costs for translating documentation, projects including deaf sign language users do, too. The issue is the additional economic burden that accessible projects bear – a cost that does not seem to be factored in to ‘value for money’ evaluations and which makes securing funding challenging in a highly competitive arena. This speaks to ‘hearing privilege,’ and it is pervasive, though, for many, invisible.

**Challenge 2: Securing responses from across Europe to benchmark current practice**

Napier and Haug (2015) led work on a pan-European survey of interpreters, interpreter associations, and interpreter education providers to establish the processes that are in place, document issues arising, and provide a sense of the scope of the challenge that lay ahead. The survey response, while acceptable, was significantly lower than expected (see Coxhead, this volume, for related discussion). A challenge of administering a multinational survey in one language (in this case written English) is that it may impact on the number of responses received if potential respondents are not comfortable using English (Napier, 2015). Although the creation of multilingual surveys has its own inherent challenges in ensuring all versions are created or translated accurately to retain the integrity of the questions, and back translations of responses can be time consuming, a potentially more suitable option here might have been the production of a questionnaire with questions asked in sign language. This approach has been previously used in other European projects (Insign: www.eu-insign.eu and ProSign: www.ecml.at/PROSign), which proved an effective way of engaging deaf sign language users directly (Napier & Leeson, 2016; Skinner, 2015).

**Challenge 3: Focus groups versus interviews**

Deaf communities are tightly knit. While certain topics (access to education, access to health care) are openly discussed in focus group settings, others are not, such as encounters with the criminal justice system. The Heriot-Watt team successfully recruited deaf sign language users (who had experience
of being interviewed by the police) via video invitations recorded in BSL, which were sent by the British Deaf Association Scottish Branch for one-on-one interviews. However, in Ireland, the Trinity College Dublin team originally set out to recruit deaf and hearing interpreters for focus groups, but it soon became apparent that interpreters preferred to participate in one-to-one interviews. There were several reasons for this. First, in a small country, the interpreters were very aware of their responsibility to maintain confidentiality and were conscious that even innocuous details could provide clues as to the identity of parties referred to in a legal context (see Carson, this volume, for related discussion). Second, interpreters who routinely work in the legal domain are few in number (approximately ten in Ireland). In this context, interpreters who were beginning to work in legal settings reported that they did not necessarily wish to disclose this to the established legal interpreters for fear of being judged. Worryingly, a third concern raised (and one that had been raised before in focus groups for another study) was that (hearing) interpreters did not wish to risk participating in a forum where specific colleagues might be present, as they said they did not wish to raise certain views or give specific examples in their presence. This also presents a challenge regarding how we reflect back such sensitive findings to the interpreting community: clearly there are highly professional considerations at work regarding confidentiality and regard for clients but also at work are issues of dignity and respect for individual interpreters within their community of practice.

**Challenge 4: Accessible dissemination**

Adam (2015) stresses the importance of disseminating information about sign language research to deaf communities *in* sign language. We are faced with the challenge of how to deliver the information across several countries (in the case of the European survey) and the United Kingdom and Ireland (in the case of the qualitative data) where different signed languages are used. The consortium agreed to develop an executive summary of the survey results, which will then be translated into International Sign – a form of crosslinguistic communication, which deaf people from different communities use to communicate by exploiting their awareness of iconicity and their access to visual-spatial expression (Adam, 2012). Despite its growing usage, the use and existence of International Sign is still hotly debated linguistically and politically, as research reveals that complex meanings are not easily conveyed (Rosenstock & Napier, 2015), and thus may not necessarily be the ideal solution to ensure the information is disseminated to the relevant signing communities in Europe.

While as academics we will ensure that there are journal publications/book chapters arising from our work, we are also committed to ensuring that interpreter practitioners and Deaf community members can easily access content, too. With this in mind, our dissemination strategy
necessarily includes short articles for association newsletters like the European Forum of Sign Language Interpreters and the Association of Sign Language Interpreters (United Kingdom) – but we also will have to ensure that we present on our findings in our respective signing communities in our respective sign languages, for example, by ensuring that information about the project is available on our project website (see Figure 13.2) in a range of sign languages.

Figure 13.2 Screenshots from the Justisigns website and the LifeinLincs VLOG.
Challenge 5: Securing change: ‘Dissemination hacks’ may be required

We also have to reach out to the policing and legal communities, building relationships and, collaboratively, identifying the best ways of disseminating information to their members. Timing is sometimes key, especially when working to funder deadlines; in order to successfully complete components of project work, we sometimes employ ‘dissemination hacks.’ For example, we circulated draft documents to police forces rather than final documents which would require formal sanctioning by a range of personnel. When reporting to our funder, we can note the dissemination has occurred, but for the purposes of securing final approval from police authorities (and permission to add their logos), more time will be required – ‘unfunded’ time we will invest beyond the life of our funding period.

Further, some dissemination will take place in private meetings and are not formally reported as they are ‘under the radar’ with significant individuals who function as key gatekeepers. Such meetings have the potential to bring about significant medium to long-term impact and can open up lines of engagement that will live beyond the life of a single project. Such ‘dissemination hacks’ have to be negotiated against funders’ expectations, which typically seek information regarding those impacted by research findings. Funders often request photos of dissemination events or evidence of approval of disseminated data via the inclusion of stakeholder logos on printed materials. Community organizations (deaf, interpreters) may also want to know who research teams met in order to judge progress. But to be effective within the life of a project, those expectations must be negotiated in light of local contexts and long-term objectives.

Conclusion and suggestions for researchers

Action research that desires to be authentic, engaged with the linguistically diverse communities involved in a project, requires a great deal of reflexivity on the part of the research team. To be considered appropriate, a team collecting data from sign language using deaf people must include deaf researchers who can shape and inform the process in culturally appropriate ways. A key point is that funded research with signing communities and other stakeholders entails linear, intersectional, hierarchical, and horizontal relationships. These have to be negotiated vis-à-vis the immediate goals of the projects, funder expectations, the limitations of our own institutions, and bureaucratic obstacles we encounter, especially in relation to conducting ethical research. Finally, dissemination is key. In today’s world, much of this is immediate and digital, but it must be accessible and meaningful. It must resonate with stakeholders. Moreover, it must contribute to meaningful change. That, we suggest, is what action research is all about.1
Note

1 Our thanks to the deaf signing communities, interpreting communities, police officers, NGOs, and key policing and legal organizations who have supported our Justisigns work. The Justisigns consortium acknowledges the support of the European Commission. Justisigns products and publications reflect the views only of the author(s) and the European Commission is not responsible for any use which may be made from the information contained therein.

References


Chapter 14

Researching your own students
Negotiating the dual practitioner – researcher role

Nicola Galloway

Introduction

Research on the pedagogical implications of the globalization of English has been gathering momentum in the field of applied linguistics. There is also increasing debate of these implications at the theoretical level in the field of Global Englishes and many scholars have put forward suggestions for English language teaching (ELT) practice. However, very few researchers have conducted studies at the classroom level, and the study reported in this chapter aimed to fill this gap. As an ELT practitioner with an interest in Global Englishes, I have been involved in researching this topic with students in my own classroom for over a decade. This chapter focuses on the challenges I faced and overcame in researching my own students in my doctoral research (Galloway, 2011). As students are key stakeholders in ELT, understanding their current use of, and attitude towards, the English language is an important part of needs analysis, which should be central to all curriculum evaluation and design. The study was conducted with my own students at a university in Japan – a country where there is growing interest in Global Englishes. It aimed to investigate students’ attitudes towards English and their English teachers in relation to the role of English as a world language. It was also conducted at a time when increased attention was being paid to the pedagogical implications of Global Englishes research and, therefore, it investigated the direct influence of Global Englishes instruction on such attitudes. Overall, it aimed to build on previous research and on proposals for change in ELT to inform the researcher’s own teaching practice and contribute to theory in the field of Global Englishes.

Teacher research, classroom inquiry, classroom research, and action research are important, interrelated fields of inquiry for both teachers and researchers. They have a long tradition in the field of education, many educational institutions actively promote them, and many teacher education programmes include such research designs in research methods courses.
However, they are not always straightforward and teacher-researchers may find themselves asking questions such as the following:

- How do I conduct research without disrupting the atmosphere of the class, or influencing the outcome of the study?
- Should I conduct the interviews or recruit someone, who doesn’t teach the students, to do them?
- What influence might my own stance on the topic have on the data?

Adopting the dual role of teacher and researcher can present a number of challenges, and practitioners should deal with them when undertaking such work. The study described here illuminates the various issues that can arise in such studies and how these challenges can be overcome. It ends with a call for more research of this kind.

The background to the study

Action research, teacher research, classroom-based research, teacher/practitioner inquiry, and classroom inquiry are amongst some of the terms used to describe the research in this chapter. They may be quantitative, qualitative, or mixed methods; they may involve a number of different data collection instruments including classroom observation, practitioner journals, questionnaires, interviews, and focus groups; they may be short term or longitudinal; and they may be small or large scale. The study discussed here resembles an action research project in many respects. The topic was of direct interest to me, and the research aimed to evaluate and influence my own pedagogical practice. However, the term classroom-based research has been adopted, given that the study also aimed to contribute to bridging the gap between theory and practice in the field of Global Englishes. Much of what is described here about classroom-based research also applies to action research projects.

Classroom-based research is an important field of inquiry that can help teachers gain first-hand insight into what is going on in their classroom and contribute to a practitioner-informed knowledge base. It involves a systematic process of inquiry, which can be viewed as a cyclical approach to research design that aims to inform practice and, in this case, theory (see Figure 14.1). It begins with the identification or observation of a problem: the problem-solving stage (Robson, 2011). Various versions of the cycle have been put forward in the literature. In this study, the observation stage was followed by a period of planning, action, and then reflection. The process is ongoing and once pedagogical practice has been improved, the whole process may start over when another problem is observed. Thus teacher-researchers identify a concern, try out a different way of doing things, reflect on what was happening, check any new understandings with others, and,
in light of their reflections, they may try something new (McNiff & Whitehead, 2011). After this, they may evaluate their changes and the cycle may begin again.

The cycle began for me in 2001 when I took up a position as an assistant language teacher in a Japanese elementary and junior high school as a participant in the Japanese Exchange and Teaching Programme, a scheme that is heavily biased towards native English-speaking countries, particularly the United States (Galloway & Rose, 2015). Interest grew in the dominance of the native English speaker model in Japanese ELT, culminating in an MSc TESOL dissertation study. The results of this study influenced my subsequent pedagogical practice, yet stage 1 of the cycle (observation) began again when I returned to Japan to teach in a university. My interest in the pedagogical implications of Global Englishes research grew, and I embarked on a PhD (Galloway, 2011) and began to ask myself questions such as the following:

- How do my students use English outside of the classroom?
- To what extent is the native English-speaking model, and being taught by a native English speaker, meeting their needs?
- What are their attitudes towards English and their teachers? What is their understanding of the role of English as a global language?

As I researched the topic, it became evident that the lack of an extensive body of research was problematic for the many pedagogical proposals
that were being put forward in the literature. Further questions in my own inquiry that arose included the following:

- How can I approach this topic?
- What do I want to achieve?
- How can I improve on current studies in the field?

I decided to conduct my study with my own students at the university where I was working in Japan. Having the opportunity to design and teach two elective content-based courses for third- and fourth-year students also gave me the opportunity to use a quasi-experimental design and investigate the influence of Global Englishes instruction in the classroom. The fieldwork took place over four university semesters from October 2007 until July 2009. Two different content-based courses were designed and taught for 13 weeks, twice a week (90 minutes each and each contained 30 students). For the control group, a tourism course was designed after a needs analysis survey of students’ future goals showed that many wished to work in the tourism industry. For the experimental group, a Global Englishes class was used (see Galloway, 2011; 2013 for an overview), where issues surrounding the global spread of English were directly taught to the students. This sequential mixed-methods study had three phases of data collection beginning with a questionnaire \( n = 116 \), followed by interviews \( n = 20 \) and focus groups \( n = 24 \).

**The research challenges**

Conducting research within a familiar context can offer numerous advantages. Having taught English in Japan for several years, I was familiar with the students and the Japanese ELT context, which helped to identify gatekeepers and gain access into the classroom. However, taking on the dual role of teacher and researcher proved complex and posed a number of challenges, as outlined in the following sections.

**Managing workload**

Managing the dual workload of teaching and research was a concern in the early stages of the study. As Hopkins (2008) notes,

> The teacher’s primary job is to teach, and any research method should not interfere with or disrupt the teaching commitment . . . It is naïve to assume that the adoption of a research role will make no inroads on a teacher’s private time.

\( (p. 59) \)
I was concerned that the study may negatively influence my teaching practice as an ‘outsider’ researcher to some extent (see McKinley, this volume, for a discussion of researcher positionality). I wanted to be respectful of the hierarchy of consent in the Japanese university and familiarize myself with the appropriate channels to obtain permission – both of which proved time consuming. Before beginning the study, I considered issues related to reliability and validity, ethical issues, and my position as an insider to the research context.

**Reliability/validity**

Practitioner-oriented research raises issues in relation to validity and reliability. “If a change in teaching strategy is to be made, then that decision needs to be based on reliable data” (Hopkins, 2008, p. 60), and being teacher-led does not mean that a research project should lack rigour. In this study, I employed convenience, or ‘availability’ (Schutt, 2014), sampling techniques, which are often criticized for their inability to yield a representative sample making generalizing the findings difficult. High participant numbers were needed to conduct inferential statistics with the quantitative data; however, workload concerns did not permit me to teach additional classes, and it was not possible to collect the data over a longer period of time with more students because of the time limit of my PhD candidature.

**Ethical issues – The blurred line between teacher/researcher**

Ethical guidelines, related to the aforementioned concerns with validity and reliability, have to be followed in every research project, but teacher-researchers, who can be viewed as ‘insider’ researchers, are often presented with specific ethical challenges. Managing the asymmetrical power relationship between the teacher-researcher and the students proved problematic. Voluntary participation was a concern and, although all of the students agreed to participate, I did not want them to feel coerced. I was concerned that student anxiety could influence the data if students felt obliged to participate based on the false understanding that participation would lead to better grades.

The possible influence of the researcher and the research process on the nature of the data gathered was another issue. Research is always an intrusion but in this study, the Hawthorne effect was a particular concern since the courses were credit bearing. I wanted to ensure that the students did not alter their behaviour because research was being conducted. To further complicate this, my position as both the students’ teacher and a native English speaker (topics I wanted to elicit attitudes on) also had the potential to influence the data. Thus I was also concerned with social desirability (or prestige) bias, where students answer with the desirable/acceptable/expected
answer. I wanted to approach the study in an objective manner, yet gain an ‘insider’s’ perspective at the same time. As Cohen, Manion, and Morrison (2011) note, it is important to be aware of the subjective, or rather the intersubjective, when conducting interviews, given that data is influenced by how much the interviewer controls the interview and the subjects discussed (Richards, 2003). Thus my decision to conduct the interviews raised further issues. Researcher stance was another issue. As with other research designs, classroom-based research begins identifying a topic worthy of investigation, generates research questions, considers methods of collecting data, analyses the findings, and makes claims based on such findings. However, as McNiff and Whitehead (2011) note,

Where research traditions differ is how they perceive the values-based positioning of the researcher (ontological commitments), the relationship between the knower and what is known (epistemological commitments), the processes of generating knowledge (methodological commitments) and the goals of research in terms of how the knowledge will be used (social and political commitments).

(pp. 26–27)

The study was based on the principles of constructivism, where the social word is seen as being constructed by those individuals involved. The researcher and the participants were seen as having an important role in shaping the research and, therefore, the important role of the researcher on the data collection process could not be ignored.

**Conducting research with students from a different context**

Since I wanted to conduct research with my own students, I was concerned with my position as a native English-speaking teacher. Thus students were given the opportunity to express their attitudes in Japanese, although the pilot data revealed their reluctance to use Japanese in this study. In fact, they appeared to view the interviews and focus groups as extensions of class discussions and as a valuable opportunity to speak with me, their teacher. This may have been related to the university policy that encouraged them to seek every opportunity to speak with their teachers in English. One focus group participant noted that it was a valuable language practice activity, encouraging the group to “challenge in English.” When the moderator, a Japanese student, informed them that they could use Japanese, one student quickly responded, “No guys, let’s use English,” and another, “Yes, we are motivated students.”

The pilot focus group also revealed issues related to conducting research with students in different contexts. I was concerned that group thinking...
may hinder the disclosure of personal opinions and this was felt to be a particular issue in Japan, where the ‘group’ is valued over the individual. The pilot discussion was often halted and students took a long time to formulate their opinions, thus highlighting further issues related to conducting research with students in second language contexts.

**Overcoming the problem**

Various strategies were utilized to overcome the issues outlined earlier, showcasing how classroom-based research can be done in a systematic and rigorous fashion.

**Managing workload**

In order to ensure that the research study did not interfere with my teaching practice, I made every effort to familiarize myself with the ‘hierarchy of consent’ and identify ‘gatekeepers’ outside of class time. Living and working in Japan, both before and during the study, meant that this familiarization process was relatively straightforward and collecting data in my own classroom gave me relative control over the time scale of the project.

**Reliability/validity**

In this study, an exploratory pilot study was conducted before the main round of data collection. The use of a pre- and a post-course questionnaire also helped me to examine students’ attitudes at different points in time. Instructions and questions were provided in both English and Japanese and in the qualitative data, transcripts included verbatim accounts as opposed to reconstructions based on the researcher’s perspective. Unfortunately, due to time constraints, inter-rater reliability, which involves giving the data to a number of readers and asking them to analyze it according to an agreed set of categories, was not possible. Nevertheless, both the data collection and the data analysis processes were done in a systematic way. Creswell and Plano Clark (2007) discuss two forms of validation: comparing different kinds of data and taking one’s findings back to the subjects. Data triangulation, often used by ethnographers to “use different theories, methods, techniques, etc. in order to avoid a one-sided view” (Richards, Ross, & Seedhouse, 2011, p. 35) was, therefore, another strategy to increase the trustworthiness of the data. It also insured as far as possible that the study’s findings were the result of the experiences and ideas of the informants, rather than the characteristics and preferences of the researcher. The use of focus groups also enabled me to take a less directive and dominating role and, as noted, an external moderator, a neutral third party, was hired. Respondent validation, or pragmatic validation, whereby interview transcripts are sent to participants to
clarify that they are representative of their opinion, was also used. However, only two students responded and confirmed that the transcripts were correct. Further validation was ensured in the qualitative data analysis process, where care was taken not to assume that one explanation could explain all the variance in the data and every piece of data was accounted for. Clear themes emerged in the open-ended questionnaire data, yet each qualitative data set was approached separately.

**Ethical challenges**

Final data collection did not commence until I had ceased teaching and grades had been decided. To address the ‘observer’s paradox’ (Richards et al., 2011, p. 34), every effort was made to distinguish between activities that related to the study and the actual teaching, and efforts were made to integrate data collection as unobtrusively as possible into the classes. Of course, a neutral observer does not exist in constructivist approaches, yet I made every effort to keep my teaching behaviour consistent in both classes, and I did not mention the research during lessons. In the Global Englishes class, I presented a balanced perspective and refrained from giving my opinion on the topic. The problem of reactivity is often addressed by careful negotiation in the field, remaining in the field for a considerable time, and also ensuring a careful presentation of the researcher’s self (Cohen et al., 2011). Conducting data with my own students in Japan put me in the fortunate position of being able to gain a rich understanding of the students’ context, and my familiarity with both the students and their context may have meant that I was more readily accepted than an unknown researcher. Thus I strived to understand their context, but also to remain distant enough to research the topic subjectively. To help elicit valid responses, students were offered the choice of responding in English or Japanese. They were left alone in the classroom to complete the questionnaire. To enable them to talk openly and extensively about their attitudes, the problem-centred interviewing approach was utilized for the first half of the interview (Scheibelhofer, 2008).

**Conducting research with students from a different context**

The option to use Japanese was stated more strongly in the main study than the pilot study, yet the majority of students continued to use English. To encourage more discussion in the focus groups, the main study prompts were shortened to prompt more discussion. Students were each given an overview and a topic guide. The nominal group technique (see Gallagher, Hares, Spencer, Bradshaw, & Webb, 1993, for description) was deemed appropriate for the Japanese context and students were given a short time to read and note down their responses before commencing discussion. This
aimed to give them an opportunity to both formulate and voice their opinion. It is recommended that focus group moderators should be recruited from the same population as the participants. Therefore, working with my own students made it relatively easy to appoint a moderator.

**Implications for researchers**

The range of issues that surrounds conducting research in one’s own classroom should not deter teacher-researchers from embarking on such studies. Undertaking the dual role of teacher-researcher can be time consuming, particularly for those with heavy teaching loads. Clear support-mechanisms and training may be necessary and collaborative projects should be encouraged. As Hong and Lawrence (2011) note, “Teacher education programs need to foster opportunities for candidates to engage in action research” (p. 12). Stage 1, the observation stage (Figure 14.1), may take place over a long period of time, and field notes or teacher journals can be a useful starting point. Many teachers are already engaging in research, conducting a needs analysis with their students, or collecting feedback at the end of semester. Even though the word ‘research’ is often equated with large-scale, longitudinal quantitative research, it is important to stress that teacher research ‘is’ research; it is practical in nature, seeks to understand and improve teaching, can lead to better learning, and also has the potential to influence theory.

This chapter has illustrated that gaining a valuable insider’s perspective can be achieved by those teaching in contexts that are linguistically and culturally distant from their own. Further, being familiar with the students does not mean that data cannot be collected in an objective manner. A number of strategies can be employed to ensure the trustworthiness of the data. Exploratory and pilot studies can help refine data collection tools and flexible designs can be a useful way to continue the research, given that the cyclical process (Figure 14.1) may begin again as teachers evaluate their changes or observe another problem. Fixed quantitative designs are often associated with the idea of a ‘detached researcher’ (Robson, 2011), yet in classroom-based research, the important role of the researcher cannot be ignored.

Classroom-based research, as with other research design frames, can be done in a systematic and rigorous fashion, following transparent ethical guidelines. Levels of anonymity and confidentiality are important, and teacher-researchers should be careful not to force students into participating in the study. Participant recruitment via an email or poster announcement could be useful here to give students the opportunity to avoid having to decline the invitation to participate. If generalizability is a concern, then a variety of sampling techniques are available. Conducting research with your own students does not mean that you have to rely on convenience sampling. Different teacher-researchers will have access to different numbers of students and different timeframes, and projects will have differing
aims and objectives. Those interested in recruiting students who meet particular criteria, or those whose membership is not readily identifiable (e.g., students who seek the help of a university counsellor to get extra help with their studies), may find snowball sampling useful, for example. It is important to think about the study’s aims and objectives at stage 2 in the cycle (Figure 14.1) to help determine the research design. For those who wish to undertake solely qualitative research then the concepts of transferability or ‘resonance’ may be more appropriate than conceptions of generalizability (Richards, 2003). Here, qualitative research aims to connect new contexts by providing enough detail to allow another researcher to “share in the researcher’s understandings and find instantiations of them in their own professional experience” (Richards, 2003, p. 266). Those conducting such studies can allow for teacher-researchers in other contexts to make connections with their project if rich data is provided on the students and the research context.

Overall, despite the challenges that teacher-researchers may encounter when collecting data with their own students, this is a valuable field of inquiry that should be actively encouraged. It can give teachers an enhanced understanding of their classrooms, and their pedagogical practice, thus increasing their confidence as both teachers and researchers. As such, it can help them develop as both practitioners and researchers, thus giving them a sense of professional autonomy. Important pedagogical decisions cannot be made without such research, which has the potential to influence the teacher’s own practice, but also make a contribution at the theoretical level. Within the field of Global Englishes, Dewey (2012) calls for researchers to work collaboratively with teachers, calling for classroom-based action research. We need to change the mindset that teachers do the action, while the researchers do the research. We need more collaboration, and teachers, as key stakeholders in the delivery of ELT, should also be encouraged to recognize their valuable role in contributing to theory. The living theories generated by action researchers (McNiff & Whitehead, 2011) demonstrate accounts of real classroom practice, helping teachers understand what they are doing, why they are doing it, and how they can improve it. This teacher-informed knowledge base is something that is particularly needed in the field of Global Englishes, where much of the debate is centred on the pedagogical implications of research by detached researchers rather than ELT practitioners.

References


Part IV

Responding to problems during data analysis
Introduction

Second language (L2) learning or use is a highly complex phenomenon that involves multiple interactions among individuals’ characteristics or attributes (e.g., age, first language [L1], gender, proficiency level, aptitude, memory, and motivation) and contexts of language learning or use (e.g., the L2 is being learned in preparation for a high-stakes test, learning the L2 in an instructional setting or informally at home; see, e.g., Ellis, 2015; Macaro, 2010; Ortega, 2009). It has been known that time is a contributing dimension which determines the extent to which individual factors interact with contextual factors (e.g., time determines a changing status, progress, loss, or gain of language learning). In L2 research, *intra-individual differences* are related to changes or sequences in language learning or use within individual learners over time, whereas *inter-individual differences* are related to group differences (e.g., age, gender, L1, conditions) in a specific aspect of language learning or use (e.g., fluency, accuracy, and complexity). An understanding of both intra- and inter-individual differences allows researchers to develop robust theories that account for and predict changes, sequences, or developments of language learning.

Longitudinal research is related to research on intra-individual differences in which the same individuals complete a research instrument (or multiple instruments) at two or more distinct points, so that two or more sets of the repeated instruments can be compared to determine any changes or development in language learning or use (see, e.g., Dörnyei, 2007; Ortega & Iberri-Shea, 2005). Longitudinal data allow researchers to examine duration and differences or changes within a variable through the use of repeated measures across different waves of data collection. It should be noted that the term ‘longitudinal’ used in this chapter is not the same as the term ‘prolonged,’ which is often used in ethnographic research in which researchers spend an extensive period of time in a research setting. Unless ethnographic researchers aim to trace changes of something over time, a prolonged data collection is to allow the researchers to fully immerse themselves in a setting and establish trust with participants in the community.
To date, more publications in which researchers share their actual experience of and insight from doing longitudinal quantitative research are needed, so that new researchers or those interested in knowing more about longitudinal research can understand better what is involved in conducting a longitudinal quantitative study. A lack of longitudinal research, as compared to cross-sectional research, may be explained by uncertainty about how it should/can be implemented. Because each longitudinal study is unique in its focus and context, I discuss and reflect on my experience of doing a longitudinal-like study, which led to three key publications (Phakiti, 2007; 2008a; 2008b). I present some crucial research stages, considerations, and challenges of this longitudinal study as published in Phakiti (2007).

This chapter presents key methodological considerations and discusses research strategies and challenges in designing and applying a longitudinal study to examine intra-individual differences in L2 learning or use. In particular, it highlights the needs for researchers to (1) understand the whole longitudinal research processes, (2) obtain an adequate sample size necessary for quantitative analysis, and (3) deal with quantitative analysis suitable for testing research hypotheses (in this chapter, a structural equation modeling [SEM] approach).

**Research strategies and challenges of the study**

It should be noted from the outset that I called this study (Phakiti, 2007) *longitudinal-like* because although the study focused on stability and stationary relationships among different constructs over time, it did not take several months or years to complete the study. Furthermore, as I point out in the *implications for researchers* section, some researchers (e.g., Ployhart & Vandenberg, 2010) would not label this study a longitudinal one because it does not meet their definition of longitudinal research, which these researchers strictly define as having repeated measures at a minimum of three time points.

**Context of the study**

A language test has a social function since a test score can be used to determine whether a student can pass a course, be admitted into a university program, or is allowed to migrate to another country, etc. Given the potential impact of the test on students’ lives, language testing researchers need not only to make sure that a test measures what it is intended to measure but also that a test score is used ethically and fairly. In language testing, several theoretical models of language proficiency have been proposed to help us understand what constitutes language ability (e.g., linguistic
competence, strategic competence, and sociolinguistic competence) in order to properly design a test to measure it (see e.g., McNamara, 1996; Purpura, 1999).

Research aims

My study examined the role of strategic competence, which is defined as the higher-order cognitive ability that regulates language use processes as test takers complete a given test task (e.g., a set of metacognitive strategies). In language testing, strategic competence is believed to be a key non-linguistic factor that distinguishes successful test takers from unsuccessful ones. An influential model of communicative language ability by Bachman and Palmer (1996) drove the concept of strategic competence in this study. At the time, Bachman and Palmer’s (1996) depiction of strategic competence was argued to be preliminary (McNamara, 1996) and was not based on empirical research (Purpura, 1999). Cross-referenced to cognitive and educational psychology, the study aimed to further explore the nature and parameters of strategic competence that were not discussed in Bachman and Palmer (1996).

Research hypotheses

I began the study by conducting a comprehensive review of the literature in both L2 research and cognitive and educational psychology. Prior to the study, previous research had focused on understanding trait strategy use and its influence on language test performance, and no distinction between trait and state strategy use was explicitly made. Trait strategy use, on the one hand, refers to a person’s tendency to use a particular strategy in general, regardless of a specific setting or situation (e.g., when I read an English text, I focus on identifying the main ideas rather than looking up every word). Trait strategy use tends to be relatively stable over some time points and is theorized to be part of strategic knowledge stored in the long-term memory. A trait strategy use questionnaire item is usually written using the present tense. State strategy use, on the other hand, refers to a person’s perception of a particular strategy being used in a specific situation (e.g., when I read this chapter, I focused on identifying the main ideas, rather than looking up every word). State strategy use is theorized to be fluctuating and unstable across different situations and to be part of the ongoing regulation of cognition that takes place in the working memory. A state strategy use questionnaire item, especially when assessed after an event, is normally written using the past tense.

I formulated and aimed to test three related hypotheses: (1) trait strategy use was more stable than state strategy use over time (because it is a general tendency of a person to do something), (2) state strategy use fluctuated over...
time (due to the influence of a specific test situation), and (3) trait strategy use was a higher-order factor influencing state strategy use, which in turn affected test performance. These hypotheses could be appropriately tested through time-series, repeated measures of trait and state strategy use in language tests.

**Types of research**

No single study had examined the reciprocal relationship between the trait and state facets of strategic competence over time in language testing research. Furthermore, previous research was found to be cross-sectional and no longitudinal quantitative research was conducted to address the question of the influence of strategic competence on language test performance over time. In order to carry out a successful longitudinal study, I studied several books on longitudinal research designs and considered options for statistical analysis. I also consulted quantitative experts and experienced researchers about my research ideas and plans for the study. I considered several design options (e.g., whether the study would be an experimental study in which I manipulated the research setting and controlled research variables, or whether it would be conducted within an educational setting). Because the study focused on language tests used to make a decision about students, I decided that it was most suitable to conduct the study in an academic setting, in which students were officially enrolled in a language course. Of various forms of longitudinal research, the study was considered a **cohort** study (a group of students with common characteristics) in which the same participants were studied at successive stages over time. The study utilized a sequential/repeated design in which EFL reading and strategy use variables were measured on two different occasions. I chose this academic context because I believed that changes in trait and state strategy use and EFL reading performance were likely to occur among this group of students.

**Structural equation modeling analysis**

During the planning stage of the study, it was important to think carefully about options for data analysis. I chose a structural equation modeling (SEM) approach to test the hypotheses of the differential characteristics of trait and state strategy use. SEM provides researchers with a comprehensive method for testing data fit and examining alternative hypothesized models. In particular, SEM simultaneously analyses the relationships among latent variables in a single analysis. In the study, SEM was used to compare longitudinal differences in trait and state strategy use as measured by Likert-type scale questionnaires at Time 1 (during the midterm test period) and Time 2.
(during the final test period), and to determine the stationary relationships between trait strategy use and state strategy use over time.

Deciding to use SEM in my study was challenging because SEM is a multivariate analysis, which combines correlational analysis, factor analysis and regression or path analysis in a single analysis. To make sure that I used this statistic correctly, I had to take SEM courses and studied several SEM books and SEM studies. I joined a SEM group, in which a regular meeting was organized and individuals’ data analysis and problems were discussed and shared. Understanding how SEM worked prior to the study helped me build my confidence that I could do the study successfully. Of available SEM programs, I chose EQS 6.1 (multivariate software) to help me perform the SEM analysis. Based on my experience, it is useful to know the types of data analysis before commencing a longitudinal study.

**Sample size**

It is important to note that a large sample size for a quantitative study is required (see Coxhead, this volume). Adequacy of sample size can help researchers avoid the error of rejecting a statistical hypothesis that there is no relationship between two variables when in fact there is (i.e., the so-called Type II error). In some quantitative studies, a number of participants as low as 30 may be sufficient for longitudinal analysis (e.g., correlation, regression analysis, or analysis of variance). However, in my study, a very large sample size was critical for success in SEM. Prior to the study, I considered some recommendations by SEM scholars. For example, some suggested that the sample size should be at least 200 (e.g., Baldwin, 1989), whereas others suggested a minimum sample of 400 in order to prevent violation of the statistical assumptions. A large sample size for SEM is required for researchers to be confident that parameter estimates in SEM models are stable (e.g., Boomsma, 1987). I estimated that I needed about 400 participants for my study.

**Data collection**

Figure 15.1 presents a flow chart of the data collection procedures. In a longitudinal study, such a flow chart is important because it helps researchers control the manner in which data are to be collected and managed. This flow chart indicates two key phases of data collection. In the study, given the large number of the participants (N ≈ 650), a trait strategy use questionnaire was answered by participants one week before the midterm and final tests. After participants had completed the test (midterm or final), they answered a state strategy use questionnaire.
Data preparation

One of the major challenges of the study was dealing with the levels of data preparation and analysis. Data entry in this study was highly demanding and labour intensive because I had to enter 4 Likert-type scale questionnaires (k = 35 each) and 2 reading tests (k = 85 each) of approximately 650 participants into SPSS as well as preparing test data for Rasch Item Response Theory (IRT) analysis.

Figure 15.2 provides a flow chart of the statistical procedures adopted in the study.

After data entry was completed, all data needed to be double-checked for accuracy and completeness. Descriptive statistics and reliability analysis were performed for each data set. All research instruments were found to be highly reliable. Prior to SEM analysis, confirmatory factor analyses for each research instrument was performed, so that I had measurement models for SEM analysis to test my hypotheses about trait and state strategy use over time.

Participant/data loss

Initially, there were 653 students who agreed to participate in this study. At an early stage of data entry, the number of participants was reduced to 586 (i.e., there was a 10% data loss) because of some missing or incomplete sets of data either in the midterm or final test phase (N = 36). Furthermore, Rasch IRT was used to examine whether a particular question was problematic as a measure of students’ reading ability (i.e., item misfit) and whether some test takers’ reading ability was not properly measured by a given test (i.e., test taker misfit). Misfitting test takers were
excluded from the data set (N = 31). It should be noted that if students were misfitting in the midterm test and were excluded from the first data-set, they would also be excluded from the final test dataset and vice versa. Finally, the number of participants was reduced to 561 because the EQS 6.1 program detected 25 multivariate outliers (i.e., an additional 4% loss of data). This number was less than anticipated. These participants were further excluded from the whole data set. It is important to start a longitudinal study with a large sample size and if there is a loss of participants in later stages, there still may be a sufficient number of participants for statistical analysis. See Goodman and Blum (1996) for a discussion of bias due to missing data and strategies for dealing with missing data in longitudinal research.

**SEM analysis**

I started SEM analysis for each test phase (midterm and then final tests). This type of SEM analysis was considered cross-sectional. This stage of SEM analysis was crucial as I needed to make sure that an SEM model for each phase was specified appropriately through an evaluation of fit indices (i.e., it was necessary to verify that the SEM models were valid) and that the parameter estimates were meaningful. Later, I connected the two SEM models (based on the midterm and final test phases) so that I could test the hypotheses discussed earlier.
**Longitudinal results**

Figure 15.3 is a simplified SEM model of the two test occasions in regard to the three hypotheses being tested. It should be noted that the variables in the ovals represent latent variables in SEM, which were inferred from observed variables (e.g., strategy composites from questionnaires, test scores from different test sections), which were excluded from this figure (see Phakiti, 2007, p. 113, for a full model).

In SEM, regression analysis helps researchers understand changes in parameters over time. In Figure 15.3, the one-way arrow from one latent variable to another represents the direction of the regression coefficient. That is, a latent variable was regressed on itself at an earlier time. Such regression coefficients in SEM allow us to understand the strength of a relationship between two or more latent variables. The key findings are summarized as follows. First, it was found that trait strategy use was confirmed to be a higher-order factor influencing state strategy use. The positive relationship between trait and state strategy use was found to be highly stationary across the two occasions (i.e., 0.83 \( R^2 = 0.69 \) and 0.80 \( R^2 = 0.64 \)). In the study, it was inferred that strategic knowledge (trait strategy use in general contexts) is strongly related to strategic regulation (i.e., state strategy use in a specific situation), indicating that individuals who perceive themselves

![Diagram](image)

**Figure 15.3** The relationship of state and trait strategy use to EFL reading test performance over time (adapted from Phakiti, 2007, Figure 5.6, p. 113).
as using a set of strategies in general (context-independent) are likely to 
employ them in a specific context. The findings also indicated that trait and 
state strategy use facets were not identical constructs because trait strategy 
use did not fully account for state strategy use.

Second, trait strategy use was found to be more stable than state strat-


gy use over time (i.e., 0.57 \( p < 0.05; R^2 = 0.32 \) versus 0.12 \( p > 0.05; 
R^2 = 0.01 \)). A non-significant regression coefficient of 0.12 means that state strategy use at Time 1 was not predictive of that at Time 2. The findings 
confirmed the hypothesis that state strategy use fluctuates over time and is 
unstable. The regression coefficient of 0.57 suggested that while trait strategy use was not highly stable, perhaps due to test takers’ ongoing language 
and cognitive development, trait strategy use at Times 1 and 2 was more 
predictive of each other and trait strategy use was more stable than state strategy use. The study was in line with other psychological findings that 
trait and state had a differential stability because traits reside in the long-
term memory, whereas states operate in the working memory. This finding 
from the longitudinal modeling highlighted the influential role of a specific context on how individuals approach a set of strategies in a specific situation 
because “what strategies are to be used also depends on the task demands at 
hand” (Phakiti, 2007, p. 116). The study had the implication for language 
learning strategy research that an understanding of learners’ tendency to use 
set of strategies in general does not warrant that they use such perceived 
strategies fully in a specific context.

Third, the regression coefficients between state strategy use on EFL reading performance were statistically significant at 0.05 (0.58 at Time 1 \( R^2 = 0.34 \) 
and 0.35 at Time 2 \( R^2 = 0.12 \)). The findings suggested that in both test sit-
ations, strategy use positively contributed to test performance (e.g., those 
who performed well reported more frequent use of strategies than those who performed poorly). Apart from the longitudinal modeling, two separate cross-sectional analyses (one for the midterm reading test and one 
for the final reading test) were also performed. In cross-sectional analysis, the regression coefficients between state strategy use on EFL reading perfor-
mance at Time 1 was the same as that in the longitudinal model. However, 
at Time 2, it was found to be 0.62 \( R^2 = 0.38 \), which was much larger than 
the coefficient found in the longitudinal model. Hence my insight from analy-
zing both cross-sectional and longitudinal analyses was that researchers 
may have drawn a different conclusion about a relationship between two 
variables, depending on whether or not they examine the relationship cross-
sectionally or longitudinally. In the report, I pointed out that “one plau-
sible explanation was that when the two datasets were analyzed together, 
information became comprehensive and SEM might have detected that indi-
viduals’ test performance varied significantly between Time 1 and Time 2” 
(Phakiti, 2007, p. 120). My further analysis suggested that the final reading 
test was statistically more difficult than the midterm reading test, implying
that when a test is beyond test takers’ linguistic ability, strategies may not always be employed successfully. The longitudinal modeling provides important information to support Bachman’s (1990, p. 105) postulation that in some contexts “strategic competence may contribute to a language test performance more than in others.”

**Implications for researchers**

Although longitudinal research requires a great deal of patience as data collection is completed slowly, longitudinal data allow L2 researchers to have a more holistic approach to research since data are collected sequentially, thereby clarifying the direction and magnitude of changes (e.g., increase, decrease, or stabilization). The study discussed in this chapter has provided several implications for researchers considering a quantitative longitudinal research project.

First, longitudinal researchers need to have a thorough understanding of the longitudinal research stages. For example, they need to be able to consider a particular research design (e.g., experimental, survey, or case study), to deal with population and/or sample sizes, to decide the number of repeated measures and the interval between measures (or spacing between time points), and how to perform data analysis that is acceptable to other researchers.

Second, competent implementation of longitudinal research requires that researchers proactively try to understand not only the theoretical/conceptual issues of the research construct under investigation but also the methodological/analytical choices for their longitudinal research (e.g., types of designs, data analysis, weaknesses, and potential threats to research validity). Both theoretical and methodological frameworks need to be closely aligned (see, e.g., Campbell, 1988). For example, the theoretical framework of a construct of interest helps researchers envision the way in which the construct of interest will change, and thereby how and when to measure it over time. A theoretical framework is also important to help researchers predict whether change within a construct is linear or non-linear. The predicted nature of linear or non-linear change has implications for the number of repeated measurement points as well as their intervals in longitudinal research.

Third, success in a longitudinal study depends on careful planning and researchers’ ability to execute the stages of the study. Researchers need to understand and prevent several bias threats to research validity (e.g., participant selection, attrition, and instrumentation) and at the same time expect challenges, such as dealing with participant withdrawals or data loss (see Seals, this volume), and perform complex data analysis, which is likely to be more sophisticated than that in cross-sectional research. They need to explore options for statistical analysis (e.g., repeated-measures analysis of
variance, regression analysis, and latent growth modeling) and statistical software for use during the planning stage.

Fourth, it is important to have a clear and defensible rationale for the number of repeated measures required in order for the study to be considered longitudinal. A number of authors mention that two measurement times make a study a longitudinal one (e.g., Dörnyei, 2007; Johnson & Christensen, 2008; Ortega & Iberri-Shea, 2005; Rogosa, 1995). However, Rogosa (1995) points out that researchers cannot plot changes using just two time points because the shape of the growth curve cannot be determined from only two data points. Ployhart and Vandenberg (2010) strongly argue that a minimum of three repeated measures is needed in order to consider a study longitudinal because two repeated measures are insufficient for many reasons. According to Ployhart and Vandenberg (2010), for example, on the basis of two observations, we do not know whether observed change is steady, delayed, or displays plateau-like behaviour (see Singer & Willett, 2003). I agree with Ployhart and Vandenberg’s (2010) proposition. My study could have revealed whether the stationary relationships between trait and state strategy use could actually be steady, had I collected the same measures over three or more occasions, for example. However, it was not possible in my study because there were only two major tests and it was not possible to follow the 541 participants to their next unit of study.

Fifth, researchers should be prepared to accept the possibility that longitudinal research does not guarantee that they will observe any changes in the target variables (see also Ployhart & Vandenberg, 2010). In L2 learning, it is highly possible that the sleeper effects exist (i.e., it may require a considerably long time to be able to observe change; see Cook, Gruder, Hennigan, & Flay, 1979). Furthermore, longitudinal data may not show any changes or development of a variable despite a longitudinal observation since some constructs may be highly dynamic. If some constructs are dynamic, their development over time can be non-linear. Some L2 constructs may be unidirectional at some times, but recursive at other times (see, e.g., Verspoor, de Bot, & Lowie, 2011).

Sixth, the longer the time between phases of data collection, the greater the difficulty researchers have in retaining participants. For example, unlike Phakiti (2007); Woodrow, Hirsh, and Phakiti (2011) conducted a longitudinal study (over 1.5 years) that employed multiple data collections and combined both quantitative and qualitative research (e.g., questionnaires, interviews). This study examined the long-term relationships among English proficiency, academic performance, and other identified factors. The number of participants was 341 in the first phase, but by the third phase in which all the three data sets were combined, the researchers had only 91 participants to examine longitudinal changes. They originally planned to use a structural equation modeling (SEM) approach to examine changes in the relationships. However, the researchers had to alter their analysis plan due to the much smaller sample size.
Finally, researchers should not believe or attempt to convince others to believe that their study has made a significant contribution to the field merely because it is longitudinal. The merit of an empirical study is determined by the research problems it addresses and the manner in which the study is designed and carried out.

References


Managing researcher dilemmas in narrative interview data and analysis

Matthew T. Prior

Introduction

Narrative interviewing (Riessman, 2007), the elicitation of accounts of personal experience, has become a favoured investigative method in applied linguistics and neighbouring disciplines. It has enhanced our understandings of the dynamic trajectories of language learning and use, and it has inspired alternative ways of conceptualizing and critiquing both narrative and identity (e.g., Bamberg, De Fina, & Schiffrin, 2007; Barkhuizen, 2013; De Fina & Georgakopoulou, 2012; Pavlenko, 2007; also Okada, this volume). The introspective attitude of autobiographical inquiry has also encouraged close scrutiny of the “black box” (Kvale & Brinkmann, 2009) of social research and the various problems as well as practices surrounding knowledge production. The present chapter contributes to these goals of researcher reflexivity and methodological transparency. Reflecting on a sample narrative interview study, I trace some of the dilemmas I encountered in the stages of data analysis and discuss steps I took toward resolution. I conclude by considering implications for other qualitative researchers.

The study

During my graduate studies, I pursued long-standing research interests in the sociocultural and psychological aspects of multilingualism and L2 (second language) learning, use, and identity development. I found narrative interviewing attractive for its constructionist and ecological orientation and for establishing immediate, interpersonal connections with individuals and the ways they represented their experiences, motivations, beliefs, and other personal facts and perspectives. Out of that early research program I published a longitudinal case study (Prior, 2011) of a Southeast Asian immigrant man living in Canada. That study centred on two repeated and emotionally charged narrative tellings, where he described recurrent institutional mistreatment by bank representatives. In my analysis, I sought to explicate his agentive self-presentation work as narrative teller and interviewee. I showed
that despite similarities between the versions, the speaker used them for different ends: in one, he represented how anger led to an empowering turning point in his social and professional relationships; in the other, he justified his anger toward ongoing social mistreatment while foregrounding his own moral conduct and self-control.

Overall, I was pleased with the study and its contribution to L2 interview research and narrative re-tellings. I also considered it successful in bringing greater recognition to L2 users as linguistically and emotionally competent communicative participants. But what went left unsaid, as is usually the case in our neatly packaged (and deliberately sanitized) publications, was any discussion of the behind-the-scenes dilemmas that had a hand in shaping the analysis, the final product, and even the researcher. Taking up these issues, in the following sections, I describe three intersecting dilemmas and my responses to them.

Three researcher dilemmas

Interaction

What distinguished many of the narrative sequences in my research corpus, including those eventually selected for my 2011 study, was their extended and highly performative production. Though the surrounding material was conversational or dialogic, once the interviewee began a narrative, my contributions as interviewer were often limited to nonverbal head nods and eye gaze signalling active listenership or minimal verbal responses (e.g., ‘mm,’ ‘uhuh’) encouraging the teller to continue. When I did interject during such extended tellings, it was mainly to confirm particulars of the setting (e.g., ‘When?’ ‘Where?’) or speakership (‘He said that?’). The apparent monologic nature of these tellings therefore encouraged analysis that ignored the interviewer and focused on the interviewee and the narrative material.

Through repeated listening and analysis, I sought to unpack both the “whats” and the “hows” (Holstein & Gubrium, 1995) of the audio-recorded data. I began by focusing on the thematic content of the individual narratives (the ‘whats’) as well as salient aspects of the macro-social context. As I focused on the two tellings of my participant’s mistreatment by bank representatives for my 2011 study – hereafter referred to as the ‘bank narrative’ – the macro-social frame offered a lens through which to view the talk of ethnic discrimination of L2 English-speaking Asian immigrants by the Caucasian, L1 (first language) English-speaking majority. Using sequential CA (conversation analysis) alongside narrative and discursive approaches, my emphasis was on examining the teller’s various narrative resources and rhetorical strategies (the ‘hows’). Together, this resulted in what I believed was a careful analysis of his communicative competences and agentive self-presentation work as he described ‘fighting back’ against institutional mistreatment and incompetence.
My intellectual commitment to a model of interview as a “social and collaborative practice” rather than a neutral “research instrument” (Briggs, 1986; Talmy, 2010) notwithstanding, the interactional frame between interviewer and interviewee was conspicuously absent from my initial examination. I knew that narratives are always recipient designed, that they are influenced by the contexts in which they are told and the questions and topics that occasion them, and that they are connected to what came before as well as what is projected or expected to follow (see Kasper & Prior, 2015). I nevertheless attempted to tread silently around the traces of my influence on the data and the interviewee, despite the fact that as researcher and co-interactant I was already implicated in their production (see McKinley, this volume).

In retrospect, I recognize my resistance to implicate myself in the analysis stemmed not from a lack of awareness of my influence; rather, it arose from self-critical concerns driven by hyper-awareness. I feared that if I explicitly attended to my active role in the data history, then any analytical claims I made could be criticized on the grounds of researcher over-involvement and failed objectivity or neutrality. In sum, at the heart of this dilemma was the question of not just how best to analyze the narrative interview as action produced by the narrative teller/interviewee but as interaction involving also the narrative recipient/interviewer.

**Representation**

Another multilayered challenge confronting me in analysis was that of representation. First, there was the issue of transcription. The “transcription stage . . . [is] an important part of the analysis, not a preliminary step” (Brown, 2004, p. 261; also Ochs, 1979). CA, for example, has developed transcription conventions that enable the analyst to capture a high level of granularity of verbal and nonverbal conduct (e.g., turn design, pauses, gestures, phonetic detail). A reality is that there is always an unavoidable trade-off between the possible level of representational detail and practical readability (Cameron, 2001). Therefore, the analyst is constantly faced with the difficult challenge of striking an optimal balance.

Because the interviews were conducted mainly in English, and I was working with audio data from an individual whose grammar and phonology marked him as an L2 speaker, I also struggled with how best to represent his speech (and him through that representation). For example, he frequently left off word final consonants (e.g., *tol’* for *told*, *an’* for *and, twi’* for *twice*) and repeated single words and sounds (*I-I-I*, *to-to*), phenomena regularly produced by both L1 and L2 speakers in spontaneous speech. Though we rarely experienced communicative difficulties, I desired to represent his speech as faithfully as possible yet without normalizing it through erasure or stigmatizing or pathologizing it through marked transcription.
Representation also became a concern in the ways that the interviewee’s narratives evolved over time. He and other research participants often repeated their narratives across interview and various non-interview settings. Details would sometimes change, and individual narratives would even merge while others became fragmented or simplified. This raised a number of questions and concerns: When participants produced their narratives for my research, how did I know they were being truthful? If they produced multiple versions, which one(s) should I use for analysis? When analyzing a narrative for a study, should I disclose that this was one of multiple versions – and if so, was I obligated to analyze all versions to which I had access?

Related representational tensions that arose throughout and beyond analysis concerned how I saw myself as a researcher and how I wanted others in the field to see me. In my 2011 study, I was largely drawing on CA to carry out analysis of interview interaction; yet, CA typically has eschewed interview data as ‘non-naturalistic.’ Because I was drawing also on narrative inquiry and ethnographic insights, I became uncomfortable framing the study as CA. I was concerned CA scholars would reject my work for not being ‘CA enough’ and that narrative and interview researchers would dismiss it for being overly ‘micro-analytic.’ This led to a bit of a researcher identity crisis where I considered revising the study into one more fitting along canonical CA lines or reframing my approach altogether.

**Emotionality**

The third dilemma I confronted over the course of data analysis concerned emotionality. By this I refer to the active management of emotions and the ways they are manifested, communicated, interpreted, and experienced in and through contact with the data. This was perhaps the greatest challenge, since it was one that I was the most unprepared for, professionally and personally, and one that research methods handbooks and courses have addressed the least, if at all. Though sociology, the health sciences, and related fields have acknowledged the emotional effects of qualitative inquiry on the researcher (e.g., deMarrais & Tisdale, 2002; Gilbert, 2000), applied linguistics has been slow to address such matters (but see Prior, 2016).

In their narrative interviews, participants throughout my data set frequently talked about the role of language in mediating the emotionally charged intersections of race and ethnicity, education, social class, sexuality, and various other contested aspects of identity and social belonging. As listener and co-participant, I became a ‘witness’ to these recounted and often re-enacted events – at the time of data recording and during subsequent analysis. The emotionality of this material therefore became interpersonally relevant, especially when interviewees expected me to show empathy or sympathy. Though I offered supportive responses, when following a
question-answer interview format (even in loosely structured settings), there was little opportunity to directly address or work through the emotionality of the talk. When the interaction got uncomfortable or intense, we sometimes changed the topic or stopped the interview. I was discovering that what happens in the talk surrounding the narratives and in other parts of the fieldwork can later have significant consequences for analysis. It felt irresponsible for me to ignore emotionality when it permeated so much of the research. Nevertheless, I was hesitant about bringing it into the analysis and questioned how to do so without risking a superficial or voyeuristic treatment of the narrative interviews as well as the participants.

Emotionality came out in other unexpected ways. At the time I was transcribing and analyzing the larger data set informing my 2011 study, I reviewed a large number of emotionally charged narrative sequences, particularly those that detailed participants’ complaints and various difficult experiences. I discovered myself becoming agitated and physically exhausted after repeatedly immersing myself in the recorded interactions. Following some timely readings in the psychological literature (e.g., Gilbert, 2000), I eventually recognized that I was experiencing “compassion fatigue” and even “secondary trauma” and distress in response to this material. I was not just exposed to emotionality by vicariously ‘reliving’ participants’ narratives through repeated data analysis; I was also (re)exposed to emotionality by reliving the intensity of the interviews and the fieldwork.

Responding to the dilemmas

I now describe some of the steps I took to resolve the dilemmas described in the preceding section.

Interaction

Less an action than an attitudinal shift, this most pivotal step was nevertheless the hardest since it required me to go beyond lip service by genuinely taking the data on its own terms regardless of how it reflected upon me personally or professionally. Once I recognized that resisting incorporating myself in the analysis was counterproductive – that ignoring my interactional contributions as researcher/interviewer was ultimately compromising the integrity of the data as well as the interviewee’s active representation and interpersonal work – I found analysis much less burdensome. Surrendering to the reality that the researcher is unavoidably a part of the data trajectory allowed me to focus on the data and worry less about matters of ‘data contamination’ or ‘researcher neutrality.’ Yet it also required that I become more sensitive to the evolving interviewer-interviewee relationship and the ways that the interviewee designed his talk for me as well as for potential future readers/listeners: so I had to look more closely to the questions that
prompted the narratives, the responses that we both produced and withheld, the many other ways that we each signaled our expectations for the interview activity, and the types of talk and responses that we were collaboratively generating.

Even in analysis of the extended, largely monologic narrative sequences, it became apparent that the narrative teller was not just recounting events; he was performing an account to engage me as recipient, to convince me of the truthfulness of the story, and to enact identity and relational work. Although CA and discourse analytic training provided me with tools for interactional analysis, I found it essential to acknowledge and examine how the data and the interviewer-interviewee relationship were not just part of a micro (‘here-and-now’) context but were embedded in an extended interactional chain that was built up by our shared knowledge and relational history.

**Representation**

As for transcription, though I was wary of stigmatizing my participant’s L2 English, I aimed to convey as faithfully as possible his manner of speech while maintaining readability. Even subtle prosodic cues can indicate speaker stance, and in the early stages (particularly in cases of ambiguous reported speech; Holt & Clift, 2007), microanalysis helped me to better delineate the boundaries in his narratives between talk attributed to himself and other individuals and groups. I used CA transcription conventions to capture not just the words but also prosody (e.g., word stress, pauses, pitch, intonation, volume) and other sequential features (e.g., turn taking, repair) of the interaction. This also allowed me to represent the emotional intensity of his escalating anger in the climax of the narrative where he ‘talked back’ to the bank workers for their improper and discriminatory conduct toward him. Because transcription entails analysis, I found that attending to prosodic and other cues also enabled me to better notice and analyze those aspects of identity (e.g., ethnicity, immigrant status) that the speaker was seeking to make relevant.

As I mentioned, the narratives in the data corpus often changed over time, which posed a serious problem because of my concern with validity and truth. Instead of obsessing over this apparent problem of consistency, I decided to take this up as a point for analysis. This led to me focusing on repeated narratives for the 2011 paper, and it allowed me to make a contribution by showing that variability is itself a communicative resource. Unfortunately, due to word count limits, the bane of the discourse analyst, the final article did not allow me to show the transcripts in their entirety. However, as is now the case with most professional journals, I was able to make the full data transcripts and the larger contextual sequences available as supplementary material via links in the online publication.
Another dilemma concerned my methodological positioning. Though my study did not fit comfortably within mainstream CA, neither did it fall in line with typical narrative interview studies in applied linguistics. To avoid potential misalignments and misrepresentations, I chose to label the study under the “narrative constructionist” (Sparkes & Smith, 2007) umbrella because I felt that best encompassed my discursive approach while giving me flexibility in my methodological choices.

**Emotionality**

Through carrying out repeated data analysis and acknowledging that I, as researcher, was a part of the interaction, I became more conscious also of my contributions to the emotionality in the interview. For example, in the material leading up to a later telling of the bank narrative, the interviewee was speaking about his mortgage with the bank. My familiarity with his intense negative feelings surrounding his dealings with bank employees prompted me to offer the meta-comment, “Weren’t you mad at them?”

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005 M: weren't you mad at them?  
006 T: I'M MAD AT THEM (. ) I'M VERY MAD. BECAUSE  
007 (. ) WHAT THE HELL IS IT WITH ↑PEOPLE↓.  
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(Prior, 2011, p. 71)

This, in turn, led to him responding with escalating anger (indicated by capital letters and underlined emphasis) and producing his extended bank narrative. This prefatory material is crucial to the analysis. Presenting the interviewee’s narrative to the reader without this material offers up a partial (and less-than-honest) representation that fails to acknowledge how the anger-filled bank narrative came about. Therefore, to contextualize the focal sequence within the larger interview and meta-narrative interaction, I found it necessary to include this material in the published study.

Would the bank story have been told without my comments as interviewer? Probably, since the interviewee was making use of that shared contextual information in the interview setting to produce a series of narratives to complain about ‘rude’ people. Nevertheless, the fact remains that this narrative and the interviewee’s lexical description of his emotional state as ‘mad’ are at least partially attributable to my comments (“Weren’t you mad at them?”). This example also demonstrates how interaction, representation, and emotionality are interconnected at the time of the interview and in subsequent analysis.
In my 2011 study, I noted the need for more attention to narrative tellers’ interactional and emotional work and histories, but I was silent about my own emotional responses. As I have described, repeated exposure to emotionally intense data can take a psychological toll on the researcher. This is one reason I chose at the time to focus on the bank narrative data rather than some of the other, more intense narratives (see Prior, 2016). Nevertheless, the bank narratives were still highly emotional, and as a means to manage the emotionality of these and other narratives, I found it necessary to consciously pursue self-care activities such as taking regular breaks away from the data, keeping a reflective journal, and talking about the data (while protecting confidentiality) and my reactions to it with trusted advisors and peers.

Concluding thoughts for narrative interview researchers

In revisiting some of the ways I negotiated dilemmatic issues in my own narrative interview data, I have emphasized analysis as an iterative and holistic process, though one not free of difficulty. There is perhaps a tacit assumption among (often novice) qualitative researchers that analysis, with its demanding and sometimes ‘tedious’ layers of listening/viewing, transcription, coding, interpreting, synthesizing, and organizing, is mainly a set of mechanical hurdles to overcome (i.e., that it is just a matter of acquiring the proper tools and techniques) – before getting on to the ‘real business’ of writing up and disseminating the research. Beyond the technical, analysis is also a “craft” and an “art” (Miles, Huberman, & Saldana, 2013). As a creative activity, it can only be honed through practice and by conscientiously navigating analytical and other challenges rather than avoiding them. We may find that many of these dilemmas are in fact brought into our awareness, or are magnified by the activity of analysis itself.

The dilemmas I encountered in my 2011 study ultimately proved methodologically “therapeutic” (Rapley, 2012) because they forced me to reflect on the stance toward narrative interviewing that shaped my lens as researcher. Analysis, re-analysis, and reflection require that we examine our taken-for-granted practices – not just the bits and pieces of data that we initially set out to “collect” but which closer inspection shows we actually had a part in “generating” (Talmy, 2010). Our research dilemmas, too, are objects deserving more careful analytical attention. What we urgently need in research are more “unsettling dialogues with humility” (Back, 2007, p. 262). We can never be fully prepared for all the difficulties we will encounter; however, we can cultivate a discipline of humility that fosters reflexivity and transparency and is receptive to critique. Though researchers have tended to shy away from talking about, much less publishing on, their challenges and failures, we are beginning to appreciate that attending to these
aspects of our investigative activity offers a wealth of analytical and methodological insights, greater researcher sensitivity, and practical benefits for improved researcher training and practice (on related discussions for interview research see, e.g., Kasper & Prior, 2015; Potter & Hepburn, 2012; Prior, 2014; Roulston, 2010, 2014; Talmy & Richards, 2011). Narrative interviewing will undoubtedly remain a popular research method with applied linguists interested in the personal experiences and perspectives of ‘real people’ in the ‘real world.’ Regardless of how intrinsically interesting the individuals and contexts we study may be, we must not forget that analysis is one of the most crucial tasks required of the researcher. Even the most introspective and compelling narrative account cannot speak for itself. As we examine our data let us give renewed attention to the interactions, the representations, the emotions, and other features and assumptions that intersect throughout all the stages of data generation and analysis. Let this encourage us to also engage with the curious and the troubling aspects of this work, not just the neatly packaged final products. After all, an applied linguistics that fails to acknowledge and reflexively examine the dilemmas we encounter in our research would be a rather uninteresting and irresponsible discipline indeed.

References


Chapter 17

Grappling with originality and grounding in qualitative data analysis

Jessica G. Briggs

Introduction

Researchers often design their research projects in light of a desire to say something new, or different, about their topic of study. Indeed, the “glamour and . . . exhilaration of innovation” in original research (Valdman, 1993, p. 505) is actively promoted by the persistent inclusion of ‘originality’ as a criterion for the assessment of research funding applications, academic journal submissions, and doctoral theses (Mackey, 2012), as well as, crucially, one of the three key criteria adopted by the UK’s Research Excellence Framework: originality, significance, and rigour.

The desire to make a novel contribution to the collective knowledge has likely been a driver for the “wide constellation of research tools” (Hornberger & Hult, 2006, p. 22) in existence in the field of applied linguistics today: by investigating a phenomenon from a fresh methodological perspective, a researcher might hope to generate data that sheds new light on a language learning phenomenon or context. It is no surprise, therefore, that guidance on instrument design abounds in applied linguistics research methods handbooks (e.g., Dörnyei, 2010; Paltridge & Phakiti, 2015). However, over and above the challenges inherent in design and development, there are implications of the use of a novel research tool in terms of the analysis of the generated data. For example, to what extent are existing analysis strategies or coding schemes appropriate, or even useful, to use? What course of action can be taken if existing theories do not encompass comprehensively the story the data have to tell? Far less has been written about this latter stage of the research process with regard to self-developed research tools and as such there is a paucity of published guidance for the researcher who has broken new methodological ground and finds him or herself faced with a dataset that does not entirely speak to prior research.

This chapter explores the challenges and opportunities inherent in grappling with originality and grounding in data analysis. It uses an example from my doctoral research (Briggs, 2014) that involved analyzing data generated by a self-developed research tool, which was designed to elicit
context-specific vocabulary-related strategic behaviour from study abroad learners of English in the United Kingdom. This study has since been disseminated in a number of research articles (Briggs, 2015a; 2015b; 2016).

**The study**

My doctoral research was a mixed-methods longitudinal study that investigated the relationships between informal (i.e., out of class) second language (L2) contact, vocabulary-related strategic behaviour, and vocabulary gain in a study abroad context. Specifically, the project aimed to find out whether there was a relationship between L2 vocabulary development across the length of a sojourn in the United Kingdom, the amount and type of informal contact with the L2 during the sojourn, and the type of vocabulary strategies employed in that informal contact. The sample comprised 241 adult L2 English language learners from a range of L1 backgrounds and with a minimum proficiency level of CEFR B1 (low intermediate), and the sample was grouped by location of the study abroad program (Oxford or London) and length of stay (short, medium, or long). Data were collected between May 2012 and December 2013.

To measure vocabulary gain and informal contact in this study, I amended existing research tools that were well suited to the theoretical and conceptual frameworks I had adopted. Finding an approach to capturing strategic behaviour in informal L2 contact, however, was less straightforward. Although a wealth of questionnaires and interview protocols exist for measuring and eliciting strategy use, none were sufficiently context-specific to suit the highly contextualized strategic behaviour that I hoped to uncover: as stated by Schmitt and McCarthy (1997), the benefit of individual or combinations of strategies on second language acquisition depends in part on the context in which they are used. Therefore, I developed my own tool to measure strategy use in informal contact: the Opportunities With Language Simulator, or OWLS (Briggs, 2015a; 2016).

The OWLS comprises computer-based simulations of informal language contact situations (e.g., conversing with cohabitants across the dinner table / watching television) that are commonly encountered by L2 leaners who are resident in the target language community (such as study abroad learners). The OWLS simulations were used as a series of stimuli in semi-structured interviews. In each simulation there is one target vocabulary item that was hypothesized (from the piloting phases of the study) to be unknown to the sample of my study and in response to which I predicted the participants would take strategic action. An interview protocol was employed in tandem with each simulation in order to elicit the strategies being used. The OWLS was administered to a subsample of 36 participants (18 matched pairs).

To analyze the OWLS data, I developed, from the work of Gu and Johnson (1996), Nation (1990), Oxford (1990), and Schmitt (1997), a taxonomy of
vocabulary strategies that are available to L2 learners outside of the classroom. The taxonomy divided the strategies into five categories (planning, individual determination of meaning, interactive determination of meaning, memorization, metacognition). Mackey and Gass (2005) argue for the use in qualitative data analysis of existing coding schemes as a means of enabling greater comparison between studies, but this was not an option in my study for the simple fact that existing strategies taxonomies all included a number of classroom-based strategies that are not available to learners in informal language contact (e.g., ‘Ask the teacher for a synonym of the new word/phrase’). I used the taxonomy I had developed to quantify reports of vocabulary strategy use (both individual strategies and combinations of strategies) in the transcriptions of the OWLS interviews and conducted further content analysis to reveal the factors that were influencing what strategies the participants reported using, and why.

The problems

The first problem to arise in the analysis of the data was that a number of strategies reported in response to the OWLS simulations could not adequately be explained by any of the strategies included in the taxonomy (nor by the taxonomies of other researchers). Additionally, some participants reported no strategies in response to some or all of the simulations, or reported avoidance strategies (i.e., cognition/behaviour which exempted them from having to apply vocabulary-related strategies). The implications of these problems for the study were serious given that one intended aim of the project was to provide a theoretically grounded, comprehensive, and comparable account of the strategies used in informal contact situations by the study abroad learners in my sample. As I saw it, I had three choices: (1) to try to fit the data to a grounded coding scheme that I believed it did not match, (2) to use the coding scheme I had originally developed but leave a large amount of interesting data ‘on the cutting room floor,’ or (3) to add ‘new’ strategies derived from the data to the coding scheme and jeopardize the ‘transferability’ (Lincoln & Guba, 1985) of my findings (i.e., the confidence that my findings might be applicable to other settings).

Another challenge arose with regard to the coding of strategic combinations. There is a strong consensus in the field of strategy-based research that strategies are more effective when used in consort (Cohen, 2014; Macaro, 2006) and as such I was keen to discover if, how, and why strategic combinations were being used by my sample in informal contact with the L2. Happily, there were many instances in the dataset of vocabulary strategies being used in cooperation in response to the target words in the OWLS simulations. However, coding these strategic combinations was not a straightforward endeavour.
One of the main reasons why coding combinations of strategies was problematic was that in the literature the definitions of strategic combinations were somewhat opaque. Combinations of strategies have been conceptualized as either clusters or chains. Cohen (2014) defines clusters as multiple strategies deployed simultaneously, and chains as multiple strategies deployed sequentially. These definitions were problematic to work with in coding because they do not specify how simultaneous the deployment of multiple strategies has to be in order to code a reported combination of strategies as a cluster or a chain. In addition, I was not convinced that the difference between clustered and chained strategy combinations was the temporal nature of their deployment, primarily because some strategies by their very nature cannot be used simultaneously with others (e.g., a planning strategy must come before other strategies). Furthermore, even if a participant deploys multiple strategies exactly simultaneously, they are only able to report the strategic combination sequentially, if at all. It was clear, therefore, that existing definitions of clusters and chains were not facilitative of coding and making sense of the strategic combinations reported in response to the OWLS.

Perhaps the most significant problem to derive from the analytic phase of the study arose from the content analysis: the OWLS data were strongly suggesting that the relationship between context and strategic cognition was being facilitated by contextual intention (i.e., the learner’s overarching reason for engaging in a given situation). That is to say, contextual intention was mediating whether and how the participants applied strategies in response to the target vocabulary in every single simulation. For example, one of the OWLS simulations depicts a woman listening to a song with English lyrics. The target word in this simulation occurs three times in the chorus of the song, and the participants hear the chorus once after first listening to an instrumental section. In response to this simulation, a large proportion of the sample reported that they would only employ vocabulary strategies in this situation if they actually liked the song, and if their contextual intention at that moment was to develop their English vocabulary. For the most part, they stated their objective when listening to music is relaxation or enjoyment – an objective that would be undermined by strategic processing of the lyrics of the song.

Before this study, strategic behaviour had been conceptualized from a cognitive and then, more recently, from a sociocultural theoretical perspective. Over the past decade many researchers (e.g., Grenfell & Harris, 2013; Macaro, 2004), regardless of epistemological stance, have argued that strategic behaviour is context dependent and that it therefore cannot fully be understood separate from the context in which it is manifested. In simple terms, strategies have been theoretically bound together with context. As such, the emergent facilitative role of contextual intention in my data was
problematic because (1) it was not grounded in the existing theoretical conceptualization of strategic behaviour and (2) it meant that my findings would not be transferable to the findings of prior research in the field. Furthermore, if, as my data suggested, contextual intention directly influences strategic behaviour, and if every informal contact situation can be approached from a range of contextual intentions, then to make any robust claims about the relationship between a given situation and the strategies used in that situation, one would need first to control for contextual intention. Unfortunately for me, contextual intention was not a variable I had controlled for in the study because it had not emerged as a potential variable from either reading of the literature on strategies or from the piloting of the OWLS.

Overcoming the problems

To deal with the occurrence of strategies in the dataset that were not included in any of the existing taxonomies yet were reported by my sample, I added to the taxonomy a number of ‘new’ strategies. In order to ensure that these original strategies were empirically and theoretically grounded, my description of them adhered strictly to Macaro’s (2006) cognitive operationalization of the term ‘strategy,’ whereby strategies are (1) conscious and goal oriented, (2) located in working memory, (3) discrete (i.e., they do not involve the use of other strategies), and (4) situation- and task-specific yet transferable to other situations and tasks. For example, ‘Repeat word/phrase in head to aid memorization’ was an existing strategy in the ‘Memorization’ category of the taxonomy. The OWLS data clearly demonstrated that silent repetition of a target word/phrase was not only occurring as a means of aiding memorization but also as a means of providing another opportunity to recognize the word/phrase in context and thus to strengthen the possibility of guessing from the immediate textual context the meaning of the word/phrase. Thus, I added ‘Repeat word/phrase in context in head to aid recognition’ to the ‘Individual determination of meaning’ category of the taxonomy. To enhance the trustworthiness of this process, each time I devised an original strategy, I selected excerpts from the dataset illustrating that particular strategy in use and then selected excerpts that illustrated similar existing strategies in use as a means of pinpointing the difference between them.

In terms of the coding of strategic combinations, I felt I had but one option: to further conceptualize the constructs of clusters and chains in order to clearly differentiate the terms and to generate a working operationalization of them that could be practically applied to self-report data. Thankfully, I had an expert on strategies at hand (my academic supervisor) to help me do this. I operationalized clusters as multiple strategies used in no predetermined order, and chains as multiple strategies used in a systematic sequence (Briggs, 2016). This differentiation was an important one to make
because it explained why, instead of when, language learners used strategy combinations: clusters as indicative of the learner applying multiple strategies experimentally to see what works in a given situation or task; chains as indicative of the learner applying multiple strategies based on previous experience or hypotheses about what combination(s) works in a given situation or task.

Because of the mediating role of intention between context and strategic behaviour that emerged from the dataset, I was not able to claim (as I had hoped to) that the OWLS data was indicative of how study abroad learners of English behave strategically when they encounter novel lexis in specific informal L2 situations. In other words, from one perspective, my findings lacked ‘transferability’ (Lincoln & Guba, 1985). There was, however, a flip side to this drawback. Merriam (2009, p. 70) states,

> Most qualitative research inherently shapes or modifies existing theory in that (1) data are analyzed and interpreted in light of the concepts of a particular theoretical orientation; and (2) a study’s findings are almost always discussed in relation to existing knowledge (some of which is theory) with an eye to demonstrating how the present study has contributed to expanding the knowledge base.

In line with this argument, I was able to claim a unique contribution of my study to the theoretical conceptualization of strategic behaviour. I argued that the relationship between vocabulary-related strategic behaviour and context is wholly dependent upon the intention of the learner in that context, and I used the data to show that without the intention to comprehend or acquire a newly encountered lexical item, regardless of the potential of a given scenario to facilitate varied and numerous strategic responses, there is no relationship between context and vocabulary-related strategic behaviour: intention is what makes the difference. Thus the very obstacle to achieving one of my intended research outcomes became post hoc one of the most important findings of my study.

As I developed further a conceptualization of the relationships between intention, strategy use, and context, I reanalyzed the dataset numerous times to look for other mediating factors in the relationship between context and strategy use and was eventually able to model (as shown in Figure 17.1) the variables that were influencing the strategy use of my sample in response to the OWLS.

In summary, therefore, theorizing the facilitative role of intention in the relationship between strategic cognition and context, and building a model of strategic cognition in informal L2 contact which accounted for this role, became a key focus and novel contribution of the study. It has also become an ongoing research focus of mine, leading me to develop a conceptual model of informal L2 contact as a move towards the development of
a theory of language learning beyond the classroom that is comparable to Ellis’s (1994) theory of instructed SLA.

**Implications for researchers**

The study discussed in this chapter is an example of the challenges that researchers can face when attempting to ground their data in prior research during the data analysis process. The study also demonstrates the potential of a novel methodological approach to yield ‘original’ findings, and illustrates that the decisions taken by a researcher during data analysis – as the linking stage between methodology and findings – are key to the full realization of this potential.

It is an ever-present possibility (particularly when researching a complex, unobservable, and/or uncontrollable phenomenon or context) that empirical data may not fit neatly into existing conceptual or theoretical models of the key construct(s) under investigation. When this happens, a natural response is to call into question the methodological approach of the study and/or hypothesize about its failure to result in findings that are firmly grounded in prior research. An alternative (or perhaps complementary) perspective, however, is to consider whether any features of the phenomenon
under investigation in the study are sufficiently specific to your study (and sufficiently different from prior research) to have engendered data that say something original. A fine balance needs to be struck: On the one hand, claiming originality without due reference to existing literature and theory, or due critical awareness, is a threat to the trustworthiness of a study’s findings. On the other hand, failure to see farther than existing literature and theory where the data indicate that there is something more to be said, or indeed over-criticality, is a threat to a credible interpretation of data.

One key implication for researchers grappling with originality and grounding in qualitative data analysis is the adoption of a flexible approach to coding and comprehension of the data. Flexibility here refers not only to being open to originality and an acceptance of that which was unexpected but also refers to being open to multiple viewpoints on the story that the data have to tell. Building multiple hypotheses and then testing them against the data and against existing theories and concepts is a practical means of straddling empirical ‘reality’ and theorization in qualitative data analysis. This is because using deductive and inductive approaches in tandem limits the capacity to force data into existing conceptualizations (the deductive) whilst simultaneously limiting the extent to which data is coded and recoded, and conceptualized and re-conceptualized (the inductive).

Analyzing qualitative data both deductively and inductively can be particularly problem-laden (and especially for novice researchers) because it is so heavily reliant on both the interpretative skill and subject knowledge of the researcher. In terms of deduction, a strong grasp of the existing body of literature on the topic(s) under investigation is key, as is the ability to make comparisons and draw links between the findings and theories derived from prior research. In terms of induction, iteration and reflexivity are the gateway to comprehending a complex dataset: revisiting and reflecting on the data over time, and forming and reforming conceptualizations of the data are hugely facilitative of teasing out emergent concepts and identifying substantive, if small, developments in knowledge and/or understanding.

The quest for ‘originality’ in empirical research need not denote innovation in terms of the development of ‘grand’ theory, nor the metaphorical ‘reinvention of the wheel.’ Even refinements of smaller concepts, as advocated by Casanave and Li (2015) and demonstrated by the refinement of strategy clusters and chains in the study here discussed, are sufficient for a researcher to be able to add to the wall of collective knowledge a weight-bearing brick. Indeed, ‘originality’ can only be identified and appropriately situated in the wall in relation to the bricks upon which and next to which it is laid (i.e., when grounded with reference to what has gone before).

In conclusion, grappling with originality in qualitative data can be daunting, particularly where grounding data in previous research begets only limited guidance in terms of overcoming challenges to the data analysis process. However, findings which do not adhere to previous research or which
indicate the influence of variables not a priori addressed can be viewed as an opportunity rather than an obstacle; indeed, without anomalous data we as a research community are limited in our capacity to advance our field of research and our understanding of how second language learning takes place. Doing research in applied linguistics is rarely problem-free and, as advocated by Alvesson and Kärreman (2011), research problems are an opportunity for construction as opposed to destruction.

References


Negotiating the relationship between theory and practice in the fields of literacy and language

Constant Leung and Brian Street

Introduction

In this chapter, we address a theory-cum-research issue related to the application of the perspectives developed in the academic literacies approach to classroom-based studies. The account can also be seen as a case study of the ‘problems’ showcased in this volume, which we, as applied linguists, face in many of our research projects. In this context, our chapter highlights one particular problem – the sometimes unclear relationship between theory and research practice in the academic literacies approach – and outlines some of the methodological moves involved in solving the problem. By bringing the methodological obstacles to the forefront, the chapter, then, contributes to the aim of the book to build on our understanding of research practices.

Drawing on the data collected from a research project investigating the use of language in London schools and university settings, our discussion will pay attention to the subject content-driven nature of classroom literacy activities – an important issue in so far as literacy activities in academic settings almost always occur in a disciplinary context. We also take into account the high levels of ethnic, linguistic, and social diversity among the student population (reflected in some of the data that follows, and we will pick up on these points in the final discussion).

We start with an outline of the theoretical perspectives of academic literacies to provide a backdrop for the methodological discussion.

Background to the academic literacies approach

In an ethnographic study of academic literacy practices reported in the journal Studies in Higher Education, Lea and Street (1998) reported on research in a number of UK universities. They put forward three ‘approaches’ that participant university staff were seen to hold regarding student writing: study skills, academic socialization, and academic literacies. The approaches are not mutually exclusive, and they should not be viewed in a simple linear
time dimension, whereby one approach supersedes or replaces the insights provided by the other. That said, we do tend to describe the ways in which each approach successively encapsulates the other so that academic socialization takes account of study skills but includes them in the broader context of the acculturation processes, and likewise the academic literacies approach encapsulates academic socialization, building on the insights developed there as well as study skills.

The study skills approach has assumed that literacy is a set of atomized skills which students have to learn and which are then transferable to other contexts. The focus is on attempts to ‘fix’ problems with student learning, which are treated as a kind of pathology. The theory of language on which it is based emphasizes surface features: grammar and spelling. Its sources lie in behavioural psychology and associated training programmes, and it conceptualizes student writing as technical and instrumental. In recent years this monolithic and linear approach has been found wanting in the classroom. The search for greater pedagogic efficacy has led to refinement of the meaning of ‘skills’ involved and attention to broader issues of learning and social context, what Lea and Street (1998) have termed the academic socialization approach.

From the academic socialization perspective, the task of the tutor/advisor is to inculcate students into a new ‘culture’, that of the academy. The focus is on students’ orientation to learning and their interpretation of learning tasks. The sources of this perspective lie in social psychology, in anthropology, and in constructivist education. Although more sensitive to both the student as learner and to the ‘local’ cultural context, the approach has nevertheless been criticized because it appears to assume that the academy is a relatively homogenous culture, whose norms and practices have simply to be learnt to provide access to the academy everywhere. Despite the fact that contextual factors in student writing are recognized as important, this approach tends to treat the language used for reading and writing as a transparent medium of representation and so fails to address the situated and complex language, literacy, and discourse issues involved in the disciplinary and institutional production and representation of meaning.

The third approach, the one most closely allied to Literacy as Social Practice (previously signalled under the heading posed by Jim Gee’s 1990 ‘New Literacy Studies’), and on which we mainly draw in this account of literacy activities in the classroom, is described by Lea and Street (1998, see also 2006) as academic literacies. This approach sees literacies as social practices, and views student writing and learning as issues in terms of epistemology and identities rather than skill or socialization. The academic literacies approach views the institutions in which academic practices take place as constituted in, and as sites of, discourse and power. It sees the literacy demands of the curriculum as involving a variety of communicative practices, comprising different genres, fields, and disciplines. From the
student point of view, a dominant feature of academic literacy practices is the requirement to switch practices between one setting and another, to deploy a repertoire of linguistic practices appropriate to each setting, and to handle the social meanings and identities that each evokes. This emphasis on identities and social meanings draws attention to deep affective and ideological conflicts in such switching and use of the linguistic repertoire. As Lea and Street note, both students and their tutors can learn much from the foregrounding of both meaning making and identity in the writing process. There is a substantial (and still growing) body of literature based upon this approach, which suggests that one explanation for student writing problems might be the gaps between academic staff expectations and student interpretations of what is involved in student writing (Gee, 1990; 2001; Lea, 1994; Lea & Street, 1998; 2006; Leung & Street, 2014a, 2014b; Street, 2005; 2012). We draw upon this approach in our account here.

As mentioned earlier, the relationship between theory and research is sometimes seen to be unclear. A particular criticism of the academic literacies approach is that it has been formulated like a ‘grand theory,’ attempting to explain ‘everything’ at a stroke, and that it does not provide sufficient analytic purchase for empirical investigations that can yield pedagogic insights (e.g., Wingate, 2015, chapter 2; cf. Lillis, Harrington, Lea, & Mitchell, 2016). A particular issue that has surfaced is that the research approach connected to the academic literacies perspective pays relatively little attention to actual classroom activities and interactions related to reading and writing. In the following account of our study of classroom literacy activities, we will attempt to show how research drawing on the academic literacies approach with its ethnographic concerns can attend to classroom activities in a close-up way. We will also show that observing, describing, and analyzing the moment-to-moment teacher-student interactions in context can lead to helpful pedagogic understandings.

Methodological commitments in linking theory and practice through ethnographic investigations

In describing and analyzing classroom interactions, we follow Lea and Street (1998) and called upon an ethnographic methodology, with a view to entering into the meanings and practices of local participants – in this case mostly teachers and students. In particular we draw upon the notion of ‘situated participatory processes.’ Green, Castanheira, and Yeager (2011: 50) define ‘situated participatory processes’ as:

the ways of knowing, being and doing constructed in and through the actions of participants in a particular moment or across times and events in the classroom . . . [this approach] enables us to focus more
closely on how . . . students have opportunities for developing a particular academic or social practice and how these opportunities are constituted in and through local and situated processes of interaction among members.

We follow the teachers’ and students’ actions from this perspective, aiming to identify and explore how spoken and written language, alongside other semiotic resources, were used to generate a particular set of opportunities for participating in classroom events and learning activities.

In this primarily socially oriented view, we focused on what the participants say and do in trying to come to grips with the role of language and literacy varieties in classrooms and their relationship to different subject disciplines and associated modes of communication. Furthermore, we acknowledged that classroom interactions do not monolithically orient toward any simple notion of ‘teaching’ or ‘learning.’ As noted by Bloome et al. (2008) in their analysis of classroom interaction, language and other semiotic means are implicated in, among other things:

- the construction of “relationships between individuals and political identities including students, teachers”
- the construction, exercise and maintenance of power relations
- the definition and creation of “knowledge, including academic knowledge, disciplinary knowledge, pedagogic knowledge and everyday world knowledge.”

(Bloome et al., 2008: 18)

The ethnographic perspective, then, led us to use concrete local examples in order to provide ‘telling cases’ of literacy activities. Drawing on Mitchell (1984), we make the methodological point that such cases are not intended to be taken as ‘representative’ in terms of empirical typicality.

What the anthropologist using a case study to support an argument does is to ask how general principles deriving from some theoretical orientation manifest themselves in some given set of particular circumstances. A good case study, therefore, enables the analyst to establish theoretically valid connections between events and phenomena which previously were ineluctable.

(Mitchell, 1984: 239)

The episodes we are presenting are, then, part of the ordinary everyday flow of classroom activities. We see these episodes as ‘telling cases’ of the issues regarding language and literacy that we were attempting to address. Their concrete and ‘ordinary’ character precisely make the point that these larger themes can be found in local events and practices.
The study: Academic literacy for biology class

We draw on a corpus of video recordings collected in a two-year research project investigating language and literacy practices in ethnolinguistically diverse classrooms located in London, where more than 300 languages are spoken in schools (for other reports of this project see Leung, 2013; 2014a; 2014b; Leung & Street, 2014a; also endnote 1). The particular focus here was on classroom interactions in a first-year literacy ‘skills’ class for biology students at a university in London. Our focus was on the ways in which the teacher interacted with the students and articulated the features of academic writing. A group of eight students were seated around a table, with the teacher providing feedback on the students’ written assignments. For reasons of scope, we focused on the interactions related to two issues: the importance of a clear focus and ‘crisp’ writing style.

At the start of this class, the teacher, Andy (pseudonym), provided feedback on the writing exercise – a 250-word summary based on three readings on human evolution and the FoxP2 gene. The students reported that they found it difficult to summarize, and many just put ‘points down.’ Andy told the students that he found their summaries ‘boring’ and emphasizes the importance of having a ‘plan for writing’ that would provide more ‘structure.’ Referring to his own knowledge of writing on biology, he recommended the strategy of establishing a balance between a ‘global’ and a ‘micro’ perspective – that the writer should provide the reader with an overview of the field being addressed and also go into scientific details. He told the students that their summaries showed no clear ‘story’ and structure, leaving the reader ‘without a clue.’ He said that the summaries ‘drove me to sleep.’

To address the problems Andy advised the students to pay attention to what they should be saying in their writing. This was a content issue related to the readings the students had been given. Andy engaged the students in long stretches of discussion on the issues related to the part played by the FoxP2 gene in human evolution (about 20 minutes in the 45 minutes session). He further suggested that to ensure they keep the focus it would be important to ‘go back’ to their own title, as a way of selecting appropriate content. He made the statement that follows.

Transcription key:

. . . – truncated transcription
(.) – short pause.

The title is very important (.) because (.) it is producing the points of what’s exactly is wanted underneath there so (.) you really want to work at dissecting out the title.
Andy went on to tell the students that they should explain what FoxP2 means in terms of human evolution. The students expressed the view that it was very difficult to explain this gene, as it is very complex. Andy then said that it would be important to reflect the complexity. Marianne, one of the students responded,

I did it in a conclusion I read something (. ) that (. ) they had a research about the FoxP2 I can’t remember their name it was in nature [the journal] and in the conclusion they’re saying that (. ) we cannot say that FoxP2 is a gene of language (. ) or the gene of speech (. ) because it’s a part (. ) of the pathway of evolution (. ) and (. ) it’s more research going on (. ) so we cannot say that for sure that it’s a most significant mutation (. ) just mention in the conclusion I didn’t do it there in the introduction.

Marianne attempted to draw upon the advice that the teacher had indicated and drew on her reading of the background material to relate her ‘struggles’ in writing.

On the need for a ‘crisp’ writing style in academic writing, the issues emerged initially through the points made by students that they found it difficult to work with the restricted length of 250 words. Andy expressed the view that one can say a lot in very few words. He cited the example of the article that reported the very significant discovery of the double helix was only 1000 words. At one point one of the students, Jesse, said that she initially wrote 600 words and then found it difficult to cut it down to 250 words. Andy responded by saying,

Don’t worry too much about the length . . . writing 600 words is no (. ) I’ve got no problems with that and I think probably a bad idea (. ) and then go back to those 600 word (. ) and you say to yourself that is what I want to say (. ) I know (. ) how can I say that without losing any (. ) any sense any important points (. ) and that isn’t cutting whole sentences out.

At this point Andy pulled out one of the student assignments that he has marked and asked the students to suggest a number randomly. One of the students called out loud the number ‘six.’ Andy then turned to the assignment and counted six lines from the top of the page and said,

Go to the sixth line and let’s see if we can chop a word out . . . [reading from text] “this allows the FoxP2 gene to be passed on through generations” (. ) look look . . . we could I bet we could put a comma here and put [writing in words] enabling FoxP2 to pass through generations . . . I’m prepared to bet all we have to is to put a comma there (. ) and we
have lost 1 2 3 4 5 6 words gain 1 (. ) 5 words lost (. ) OK I’ve halved that sentence (. ) now all I’m saying to you and at zero sense loss zero (. ) absolutely no information loss whatsoever and I will tell you something else very likely when you do that the read is likely to be better as well (. ) almost always that’s the case (. ) so I think when you write any sort of essay you really should go through this sort of thing, however long it is (. ) looking to doing this sort of thing to it.

**Insights from examining the classroom interactions**

The classroom episodes reported earlier point to a whole host of complex pedagogic and teacher-student interpersonal relationship management issues. For the purpose of this discussion three issues stand out. First, the reading and writing, or what counts as academic literacy, in biology is fundamentally and inextricably embedded in the subject content. In this particular case, the scientific complexity and the (students’ lack of) detailed knowledge of the impact of the FoxP2 gene on human evolution is at the heart of what counts as content cogency and thematic coherence. The idea that the title should be holding any piece of writing together precisely reflects the importance of disciplinary content in academic reading and writing.

Second, to carry off the kind of public ‘honesty’ displayed by the teacher when evaluating his students’ work (e.g., ‘boring’ and ‘drove me to sleep’) requires a good deal of professional confidence on the part of the teacher and a high level of built-up trust and possibly mutual respect between the teacher and the students. The students’ apparent readiness to disclose their difficulties and struggles with the writing can be seen as part of the shared ethos in this class. In another classroom, where this apparent level of ease of interaction may be absent, it would be more difficult to engage in the kind of detailed public criticizing, editing, and ‘improving’ of a student’s work that we have seen here. So in a way what is possible in the teaching and learning of academic literacy in any particular classroom has to be understood in terms of the classroom practice shared by all the participants.

Third, in this classroom the students are from a variety of ethnic and linguistic backgrounds. The learning of academic reading and writing is, for some students, part of their learning of English as an Additional Language. In this context, Andy’s attempt at editing and ‘improving’ a sentence from one of the students’ writing can be seen as teaching sentence-level grammar, albeit in a highly content-driven way. Likewise, the lengthy discussions on the FoxP2 gene can be seen as content elaboration through the teaching of academic literacy.

It is obvious that we are not making evaluative judgments on the conduct of teaching and learning we have seen in this class. Returning to the theory-cum-research issue we signalled earlier, we can see here how the larger claims about classroom discourse and pedagogy can be established and followed
through by close attention to the classroom actions, interactions, and utterances. Whereas most of these goings-on would be ignored in accounts of formal teaching and learning, an ethnographically sensitive academic literacies perspective enables the researcher to address the ordinary flow of the interaction and thereby to begin to understand what the students are making of teacher requirements – and, as the above interaction indicates, what the teacher is doing in trying to advise and guide students from his point of view, redrafting the student’s sentences and using punctuation as part of the linguistic resources to express content meaning in an academically acceptable style. It is attention to such detail that enables us to see what is going on and what constitutes academic literacy in this particular classroom.

**Implications for researchers**

In using these data to address some of the methodological issues raised in academic literacies research, we have outlined a number of key arguments: 1) what is possible and what actually takes place in the teaching and learning of academic literacy in any particular classroom has to be understood in terms of the classroom practice involving all the participants; 2) the reading and writing, or what counts as academic literacy, in biology is fundamentally and inextricably embedded in the subject content; 3) a key issue in the pedagogy is the relationship between the teacher and the students – in this case, the professional confidence on the part of the teacher and a high level of built-up trust and possibly mutual respect for the status and authority difference between the teacher and the students are a key ingredient of the literacy activities; and 4) the teacher but also the researchers need to take into account that the students are from a variety of ethnic and language backgrounds, and this would be a key issue if we wish to get an even more close-up account of the participants’ views and understandings. In addressing these issues we have also, we would argue, taken some account of the ‘problems’ raised by this book, concerning methodology and the relation between theory and practice, and we have built on our understanding of research practices in a way that we suggest could be helpful in this research field generally.

**Note**

1 ESRC-062–23–1666 Modelling for diversity: Academic language and literacies in school and university.

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Part V

Responding to problems in the reporting of research
Introduction

Qualitative studies in applied linguistics often rely on interviews as key research tools designed to generate data that illuminate participants’ views, opinions, and experiences. As such, interviews are frequently a central feature of ethnographies, case studies, action research, and narrative inquiries (Talmy & Richards, 2011). However, some opinions and behaviours reported by participants in interviews, or recorded in observational field notes may be quite confronting or unpalatable in that they transgress the views of the researcher or may potentially offend a wider audience who may eventually read the research findings. This is especially true of much sociolinguistic research, which traverses topics such as nationality, ethnicity, gender, sexuality, identity, language beliefs, attitudes, and learner behaviours. For example, participants’ views or behaviours may be perceived by the researcher as racist or sexist, they may reveal interviewees’ involvement in questionable or illegal activities, or they may pertain to issues and experiences that are deeply personal, intimate, emotionally charged, or taboo (see Carson, this volume, for similar discussion). Research of this sort is often referred to as ‘sensitive’ and requires careful negotiation by the researcher (Cohen, Manion, & Morrison, 2013).

In this chapter, I discuss my own experiences of dealing with controversial findings in an extended research project that led to multiple publications, and I draw out some general points that are intended to guide other researchers when negotiating similarly sensitive topics.

The background to my study

My study of white Western men teaching English in Japan (Appleby, 2013a; 2013b; 2014a; 2014b) was one such study that raised controversial issues to do with romantic and sexual relationships between male teachers and female adult students.

The initial aim of the research project was to examine the opportunities afforded to, and challenges experienced by, white Western male teachers of
English working abroad in East Asia. My interest in the topic was prompted by an American man I met at a conference in Japan who introduced me to the Charisma Man comics (charismamam.com) which lampoon the ‘zero to hero’ status of ‘average’ white Western men who teach English in Japan and pursue sexual relationships with Japanese female students. I knew that there had been research on the desire expressed by some Japanese women for Western men (Kelsky, 2001; Ma, 1996) and research on the way this desire was exploited by English conversation schools in Japan in order to draw female customers (Piller & Takahashi, 2006), but at that stage, there was no research that had explored this phenomenon from the men’s point of view. I began to wonder: what was it like for these white Western men who had become the object of desire in Japan? What significance did this have for their work as English language teachers?

Over the next four years, I interviewed 34 men and 20 women who had taught or were currently teaching English in Japan. I also made several field trips to Japan where I was able to meet many of these teachers and visit various educational institutions. The interviews lasted between one and two hours, were semi-structured according to a set of guiding questions, but were also collaboratively constructed to maximize opportunities for discussion of issues and topics raised by participants (Mann, 2011).

In the interviews, I asked my participants to focus on topics such as the following:

- their awareness and perceptions of a ‘Charisma Man’ stereotype, and the effects of the stereotype in their professional and personal lives;
- their personal and professional experiences and relationships with Japanese and Western women, and with Japanese and Western men;
- their views on whether being a white Western man in Japan afforded any particular opportunities or challenges in comparison with Western women.

For more than a few of my male participants, the interviews appeared to provide a space for confession, especially about their sexual experiences with Japanese women, including their Japanese female students. While accounts of romantic liaisons were important in terms of my research questions, the information provided by participants was also potentially controversial in that it exposed a degree of sexuality – traditionally a taboo topic – in the pedagogical relationship between teachers and students.

**The research problem**

Researching romantic or sexual relationships between teachers and students poses “many difficult ethical issues, questions and dilemmas, which may help to explain why few have ventured into the field” (Sikes, 2010, p. 143).
Traditionally, even consensual romantic or sexual relationships between teachers and adult students have been regarded as improper, primarily because of the power imbalance believed to be inherent in the pedagogical relationship. This is the case, of course, in the context of school education where—in the vast majority of cases—power is understood to be heavily weighted in favour of teachers (Sikes, 2008; 2010). But in the context of tertiary education, where students are adults, the dynamics of power are not so transparent.

In the case of English education in Japan, research suggests that some Japanese female students actively pursue romantic relationships with white men—some of whom may be working as teachers of English (see, for example, Bailey, 2006; 2007; Kelsky, 2001; Kubota, 2011; Ma, 1996; Piller & Takahashi, 2006; Takahashi, 2013). Despite this attribution of agency or power to Japanese female students, however, the taint of taboo lingers in any hint of sexual relationships between teachers and adult students. In general, where such relationships have occurred, they have tended to be “sexualized as harassment” (Gallop, 1995, p. 81). Male teachers engaging in such relationships tend to be viewed as licentious predators, and, correspondingly, their female students tend to be viewed as passive victims of male sexual desires.

Researchers who focus on teacher-student sexual relationships may also be stigmatized by association with the taboo topic, and doubts may be cast on the motives and ethics of researchers who examine and discuss such relationships “without moralizing or condemning what many people take to be obvious sexual transgressions or improprieties” (Cavanagh, 2007, p. 192). The researcher may even be seen as complicit in supporting teacher-student sexual relationships or may be viewed as endorsing agendas that seek to defend and justify sexual exploitation.

So the problems I encountered were these: How could I incorporate, analyze, and report on these confessional interview data, in which my white male participants discussed their own and other men’s sexual relationships with Japanese students? How could I best present the data as part of a serious research project and demonstrate their significance in educational linguistic research? How could I respect the confidence of my participants and fairly represent their points of view, while resisting a hegemonic normalizing gaze that would cast my participants as morally deviant and their female students as unwitting victims? And how could I convince journal editors and reviewers that my research was worthy of publication?

The iterative process I adopted in my research design highlighted my difficulties. In this research process, I shuttled between collecting data, presenting my emerging analysis at conferences and seminars, and being approached by audience members who wished to participate in the research and to be interviewed. While this process contributed to the expanded generation of rich data and helped me to test and refine my analysis, it also meant that I
needed to present my research openly but in such a way as to avoid needlessly discouraging potential participants and informants currently working in the relevant professional contexts.

The potential of my research to offend audiences and deter potential participants (and publishers) was a very real danger. As a female researcher, I expected male teachers who attended such conferences to view me with some degree of caution. This expectation was confirmed during an interview with one participant who, after seeing one of my conference presentations to a male-dominated audience in Japan, spoke of being ‘nervous’ about the prospect of an interview:

When I was watching you talk – and I’m sure this is what most people think, most people like me, the guys that are watching the talk [with me] – part of you really wants to be interviewed. [. . .] I think most guys, part of them really wants to talk about it, and then the other part of them is like ‘oh, this is a bit – what will she ask?’ [. . .] I’m sure a lot of guys, their first reaction is to think that you’re out there to- you’ve got an agenda and you’re going to say they’re all terrible and that kind of thing. [. . .] They probably have some stereotypical view of ‘uh oh, I’m going to get slaughtered by a feminist’ or something.

The man’s expectation of ‘being slaughtered by a feminist’ was echoed in an email from another participant who had offered to invite his male colleagues to participate in my research. I was disappointed, but not surprised, when those male colleagues declined to be interviewed on the grounds they suspected I was ‘just another comfy shoe wearing gaijin [foreign, white] woman with her nose out of joint.’ My aim, then, was to reassure participants that I wasn’t antagonistic towards their personal circumstances and would endeavour always to represent participants’ views fairly and openly; at the same time, I was attempting to understand, interpret, and report on the views and experiences of individual participants within a broader framework where the personal, political, and professional are intertwined.

**Overcoming the problem**

Resolving the problems associated with my controversial findings required some careful thinking and the adoption of a diverse range of strategies. I will discuss these here in terms of acknowledging methodological constraints, adopting a suitable analytical approach, and searching for interpretive perspectives from outside the disciplines of applied linguistics and TESOL.

Because ethnographic research focuses on lived experience and privileges the opinions and accounts of those involved in the situation under study, my aim was first to listen to participants’ perspectives with an open mind and then to present the interview data as fairly and openly as possible. In the
interview context, I made efforts to ease any doubts or fears expressed by my participants and to share my rationale for investigating a topic that might be considered personal and private. I told my participants that I had worked as a language teacher myself, that I had previously researched the gendered experiences of Western women who had worked as English teachers in international development projects (Appleby, 2010), and that I was interested in gaining another perspective on the experience and impact of gender in English language teaching. On a more personal note, I also told my interviewees that I have two sons of my own, now young adults, and was curious about the issues facing men in the contemporary world of gender politics. Conversations around these topics opened the way for talk about the professional and personal challenges and opportunities experienced by my participants. In this respect, I hoped that the interviews would be more like conversations between peers.

There were times, however, when some participants expressed views that I found somewhat troubling in that they seemed racist or sexist in their reproduction of unpalatable stereotypes about Japanese women and men, and about Western women. These included instances when Western men implicitly presented themselves in a favourable light through a series of negatively stereotyped ‘others.’ Japanese men, for example, were depicted as physically and attitudinally ‘offensive with their chainsmoking, snottin noises [. . .] po-faced regimentation and ridiculous notions of cultural superiority,’ and were said to fetishize Western women as ‘turbocharged, highly sexualized things.’ Several participants also claimed that Western women who lived and worked in Japan were bitter, sexually frustrated, jealous, and obstructive in their attitudes towards Western men. At these points in the conversational interviews, I had to decide whether or not to challenge my participants’ views. As Gailey and Prohaska (2011, p. 378) point out, “remaining silent certainly facilitates the interview process, but . . . silence can serve as a form of reinforcement and affirmation” for participants’ views that I found objectionable. This was a dilemma I never fully resolved in the immediate context of the interview, during which I felt unable or unwilling to challenge my interviewees directly.

The subsequent process of reflection and analysis provided me with an opportunity to consider and interpret my participants’ statements within the broader framework I had adopted, although I never felt comfortable in overtly critiquing the men who had generously participated in my research. In my publications, I decided to report interviewees’ words directly (as discussed further in this chapter), in part with the intention of leaving readers to interpret and respond to those words in their own way. However, in my later publications, I gained confidence in adopting a more critical stance. In particular, I wrote that in expressing certain stereotypical discourses of masculinity and heterosexuality “Western men risk being seen as complicit in, and beneficiaries of, forms of racialisation, sexism and patriarchy that they [might otherwise] explicitly reject” (Appleby, 2014a, p. 790).
When it came to presenting or writing about the interviews, I always included stretches of interview transcripts so that audiences and readers could hear the voices of my participants. In my discussion of the data, I wanted to demonstrate the nuanced ways in which the men presented their experiences and the ways in which their own negative moral judgments about male promiscuity were foregrounded. I showed the careful ways in which men distanced themselves from behaviour that they themselves considered sexually exploitative and emotionally abusive or professionally inappropriate. I also showed the ways in which many of the participants saw themselves being objectified and commodified as a stereotype of white masculinity rather than being seen as a unique individual. I endeavoured to convey their sense that being the object of desire was not necessarily a playboy dream, but was rather a hindrance in constructing a mature masculine identity, forming meaningful interpersonal relationships, and developing a professional career.

Another strategy that helped me resolve the problem of dealing with controversial interview data was to situate my analytical approach within a Foucauldian framework. From this perspective, the data are not a transparent representation of the opinions and actions of individual men but rather a reflection of discourses – the available ways of thinking, seeing, and doing – that circulate within a given context (Cameron, 2001; Miller & Fox, 2004). In reporting on these discourses, and the ways they were taken up or resisted in the data, my intention was not to castigate, ridicule, or champion individual men, or white men in Japan as a whole, but rather to highlight the influence of discourses that circulate in this context (of Western men in Japan) and to invite critical consideration of what is sometimes presented as a straightforward, uncontroversial issue (of teacher-student romantic relationships). Following Sikes (2008, p. 237), my aim – particularly in my earlier analyses – was to interrogate the normative view that sexual intimacy between teachers and students is always illegitimate, abusive, and exploitative on the part of the teacher . . . to put forward a version of gendered sexual agency and the exercise of power that did not cast women as the passive recipients of active male desires and the inevitably weaker and harassed party in any relationship.

In order to do this, I needed to read widely and outside the conventional fields of applied linguistics to develop a broader understanding of the issues discussed with my participants.

The fact that none of the topics I was investigating – masculinity, heterosexuality, and teacher-student romantic relationships – had been previously explored as noteworthy issues in applied linguistics and TESOL research meant that there was little I could draw on as a pointer to critical ways
of interpreting these phenomena. Indeed, this may often be the case if a researcher is dealing with controversial or taboo topics that are seldom studied in mainstream applied linguistics research. And yet when I looked outside my home disciplines, I found a body of research focusing on eros and pedagogy, and demonstrating that the frisson of eroticized attraction can emerge within the pedagogical relationship as a significant – and at times even productive – experience for some teachers and students (see, for example, Bellas & Gossett, 2001; Gallop, 1995; Johnson, 2006; Jones, 1996; Kipnis, 2015; Sikes, 2006; 2008; 2010). These studies, located in both the high school and adult education contexts, provided a range of alternative ways in which teacher-student relationships could be analyzed and understood. In particular, these studies did not immediately represent male teachers as licentious predators, nor did they cast female students as the unwitting victims of predatory behaviours; they resisted the easy imposition of moral judgments that rob adult students of gendered and sexualized agency and thereby reproduce a normative structure of gendered power.

With this alternative body of research as a background guide, I was able to situate the men’s accounts in a broader and more complex web of desire and to show the various intersecting discourses that came into play in their accounts. I endeavoured to avoid a moralizing gaze and to present the data at conferences with gentle humour, explaining the men’s self-positioning and inviting the audiences to gain a sense of the men’s vanities and vulnerabilities, while articulating the broader discourses that circulated in these contexts. Across the various articles I wrote, I attempted to present not just a single version of events, but instead to cover a wide range of issues that emerged from the interview data: from the dilemmas posed by the teachers’ sexualized bodies (Appleby, 2013a), to the damaging effects of normative heterosexual imperatives in professional sites (Appleby, 2013b), and to a stronger critique of the broader gendered inequalities that persisted in institutions and were ultimately meshed with the men’s personal narratives (Appleby, 2014a). By presenting a range of different perspectives on those controversial data I hoped to provide a fuller picture and a more nuanced understanding of why those teacher-student relationships mattered, how they impacted on the personal and professional lives of my participants, and how they shaped the gendered field of English language teaching in particular contexts.

The rocky road to publication

A further set of problems arose when I endeavoured to publish a journal article based on the controversial findings of my research. I selected a journal that I considered a suitable forum for reporting the study but had considerable difficulty in convincing the (male) editor and one of the (male) reviewers to accept my article for publication. Although their objections were
expressed as a concern for ‘research standards’ and methodological rigor, I felt that the underlying problem was with the taboo topics I was exploring. At one point, the reviewer – who had himself worked as a language teacher in Japan – suggested that I was a ‘radical feminist,’ a term that I don’t identify with but one which I felt pointed to the reviewer’s own emotional reaction to my research. The objections and doubts expressed by the editor and reviewer were, I believed, a form of displaced nervousness around the topics of masculinity and sexuality in English language teaching practices. After all, it is not often that the specific experiences of men are spotlighted in applied linguistics research, and I assumed the discussion of these topics would induce the same nervousness amongst male journal reviewers as they had amongst male audiences at my conference presentations.

In contrast, a second reviewer had provided enthusiastic support for the article and “was impressed by its accurate and well-crafted coverage,” noting that publication of “this paper is essential for our field.” A third reviewer, appointed by the editor to assuage his continuing doubts about the manuscript’s suitability, reported the following:

The data and analysis are unique and compelling, making a significant contribution to the field. The phenomenon addressed in the paper is widely recognized among professionals who are involved in this activity and yet has not been paid scholarly attention. In addition, the topic is both private and sensitive to a large extent and thus often difficult to approach through research. I commend the author for her courage and competency to elicit compelling responses from her participants. The topic is relevant to the broader field of TESOL not only in Japan but also in other parts of the world.

Despite the positive response from two of the three reviewers, the editor and one reviewer remained unconvinced and further revisions were demanded. In all, the manuscript went through five rounds of reviews and revisions over a period of 18 months. During that time, both the article itself and the process of review became the focus of a series of tense email exchanges before it was finally, and it seemed to me reluctantly, accepted for publication.

I eventually overcame the journal editor’s objections with the deployment of two strategies. First, and perhaps most importantly, in anticipating objections, I had made a very persuasive case in the opening of my article that the topics I was investigating were significant and worthy of research and publication. I pointed out that studies of normative gender and sexuality were almost entirely absent from research in our field and that my study represented an important contribution to our understanding of language teaching and identity. Second, I was patient and persistent in responding at length to the editor’s and reviewer’s ongoing questions about my analytical approach. As a result, the methods section in my final version of the article...
became excessively detailed and was certainly far longer than any I had read in previous studies published in the same journal. Nevertheless, the editor’s objections had finally been overcome and the article appeared in print more than two years after my original submission.

**Implications for researchers**

Drawing from my research in this project, and reflecting on the dilemmas I encountered, I conclude here by outlining some guiding considerations for researchers who find themselves dealing with similarly controversial findings.

First, when conducting interviews in any social research project it is, of course, important to build rapport with participants and to listen with genuine curiosity and empathy. If controversial or taboo topics are likely to arise, it can be useful to think through in advance one’s own position on those topics and consider the sorts of responses you might wish to make in circumstances where interviewees express opinions that you consider to be sexist, racist, or objectionable on other grounds. Do you want to challenge such opinions in the course of the interview (see, for example, Mickelson, 1994; Pacholok, 2012)? Or do you want to remain silent in order to maintain rapport in the interview, encouraging the interviewee to express and expand on such opinions in the knowledge that publishing your findings will give you “the last word” (Gailey & Prohaska, 2011: 379)?

Second, when endeavouring to understand and interpret controversial data, it is important to avoid the adoption of common-sense judgments that may unfairly damage the reputation of one’s participants or inadvertently reinforce inequitable stereotypes. In this respect, it can be instructive to read outside one’s home discipline for research literature that may offer alternative perspectives that challenge normative judgments on the topic at hand. This is not to say that all non-normative positions are necessarily to be championed but rather that a critical lens should be applied when interpreting one’s data.

Finally, when analyzing and reporting on controversial findings it is crucial to remember that participants speak with the voices that circulate in their communities (Cameron, 2001). The controversial views interviewees might express don’t emerge solely from their individual thoughts and experiences, nor are they windows onto the interviewees’ external ‘reality.’ Rather, those views reflect, on the one hand, the social voices that circulate in our particular worlds and, on the other hand, are also generated as a product of the interaction between interviewer and interviewee. In this respect, it is important to take a reflexive stance in our research data, to understand the interview itself as “an interactional event” (Talmy & Richards, 2011, p. 2) in which various identities and controversial views may be performed and articulated for specific audiences – the interviewer and
the wider research-reading public – and to adopt a discursive perspective in reporting on data generated in qualitative interviews.

My final call is for researchers to be inquisitive and courageous in exploring sensitive and controversial topics that may otherwise remain hidden from view. When faced with criticism and nervousness from audiences, reviewers, and editors, be persistent in responding to opposition and in making the case that such topics are crucial for broadening disciplinary knowledge in applied linguistics and uncovering the ways in which language practices structure our social worlds.

References


Chapter 20

Reporting on politically sensitive issues

The case of telling the truth about early L2 instruction

David Singleton and Simone E. Pfenninger

Introduction

The advocacy of the introduction of foreign languages into the primary school curriculum in the 1950s and 1960s was fuelled by the “younger = better” ideas of Penfield (Stern, 1983). Penfield and Roberts (1959) propounded a brain plasticity hypothesis, based on studies of recovery from brain damage, according to which the optimal period for language acquisition ends when, at the end of childhood, the brain – according to their hypothesis – begins to lose its plasticity – that is, the ability to form new connections between brain cells.

Findings concerning the age factor in naturalistic settings speak in favour of an early start to L2 learning in these settings. Subjects with several years’ naturalistic experience of their L2 whose exposure to the L2 began early in childhood generally tend to outperform those whose exposure began later. Even though there is an initial rate advantage on the part of older starters in naturalistic settings, robust findings point to a general long-term advantage on the part of younger starters (cf. Hyltenstam, 1992; Johnson & Newport, 1989; Krashen, Long, & Scarcella, 1979; Oyama, 1976; Patkowski, 1980; Snow & Hoefnagel-Höhle, 1978). Research in formal instructional L2 learning settings has confirmed the finding relating to the initial faster rate of older starters (e.g., Álvarez, 2006; Cenoz, 2003; Mora, 2006), but has not confirmed the long-term benefits of an early start when younger and older starters have had the same number of hours of instruction (Muñoz, 2006; 2008; Navés, 2009).

Actually, such negative findings regarding the effects of early instruction go back a long way. Thus the idea of introducing L2 instruction into primary/elementary schools in the 1950s and 1960s was dealt a severe blow by the findings of research in the 1970s which cast doubt on the capacity of early instruction to deliver higher proficiency levels as compared with later instruction (e.g., Burstall, Jamieson, Cohen, & Hargreaves, 1975; Carroll, 1975; Oller & Nagato, 1974). The disillusionment occasioned by such findings seems, however, to have been rather short-lived, and more recent and continuing negative results in this connection have also been largely ignored.
Since the 1990s, throughout Europe and indeed across the world there has been a clear and accelerating trend towards the introduction of additional languages into primary-level curricula (e.g., Audin, 2008; González Davies & Taronna, 2012; Murphy, 2014). This trend appears partly to have been underlain by the widespread (mistaken) belief on the part of parents – whose views feed into the decisions of governments – that an early start in L2 instruction is a panacea overriding and neutralizing all other factors. The Swiss situation, with which we are largely concerned here, is in line with this international trend, and Swiss governmental agencies have, as illustrated in the following section, put together a range of dubious arguments in support of their policy of introducing FLs into the early grades. In this regard, Swiss officialdom has behaved like officialdom in many other countries. The researcher is thus faced in all these countries with the task of confronting and undermining the ‘big lie’ of the significant benefits of early L2 instruction – in a very unequal struggle in terms of the resources available to either side of the debate. This is not dissimilar to the challenge faced by investigators in other domains – from educationalists who time and again demonstrate to unhearing ministerial establishments the benefits of smaller class sizes to cancer researchers who have for decades struggled with the powerful tobacco industry.

Clearly, for educators, teachers, and policy makers, as well as for theorists, it is of compelling interest to know more about the end state of foreign language instruction, since such research has important implications for multilingual education in terms of arriving at decisions about (1) early instruction in different languages in elementary school and (2) later instruction in different languages in secondary school. On a theoretical level, the publication of negative results concerning early L2 instruction has been vitally important in age research for many reasons, not least because the null effects emerging from classroom research complement the differing pattern of results typifying research in naturalistic settings. The question then arises not only as to how studies of real-world problems, which have direct relevance to L2 pedagogy, can be brought closer to citizens but also how deeply ingrained assumptions such as ‘younger = better’ can be shifted.

The background to the study

The official Swiss propaganda

Like so many Ministries of Education throughout the world before them, the Swiss Conference of Cantonal Ministers of Education (Schweizerische Konferenz der Kantonalen Erziehungsdirektoren; EDK) decided to revise their teaching policies by moving the teaching of French and English from the secondary to the primary level in 1989 and 2004, respectively (EDK, 2004). Reasons given were the supposed positive impact of early foreign
language (FL) learning on later learned languages, alleged extra-linguistic advantages (e.g., metalinguistic awareness, favourable attitudes to other languages, people, and cultures), the steady growth of English as a lingua franca at an international level, hoped-for economic competitiveness, parental encouragement, and the official multilingualism requirement/goal in Europe (Eurobarometer, 2006; European Commission, 1995). The decision to introduce English language instruction at earlier grade levels was also based on the commonly held assumption that when it comes to learning an L2, earlier is better. The following quotes from Swiss policy documents illustrate this line of argumentation:

1. “On neuro-psychological grounds early learning is especially important and profitable specifically for the learning of languages: early language learning is more efficient, creates favourable preconditions for the learning of further languages and promotes the development of language learning strategies” (EDK, 2004, our translation).

2. “Younger learners are capable of acquiring and storing a language unconsciously, provided they are exposed to regular and rich input. Language skills that are stored in this manner will automatically be available to the learner later in life . . . An early start increases the learning period” (Advisory Council of Education [Bildungsrat] of the Canton Zürich, 2003, our translation).

These quotes perfectly exemplify the “leaps in logic” made in the misapplication of research that was carried out in one context to another context, as described by Hatch (1979, p. 138) 30 years ago. Clearly, the arguments of the policy makers are based on (a rather simplified account of) research that has investigated age and L2 learning in the naturalistic, not the instructional, setting (see also Spada, 2015). As example (1) shows, the main explanations that have been offered for the differences between early and late starters in Swiss policy documents are concerned with neurobiological advantages of younger learners. According to Marinova-Todd, Marshall, and Snow (2000, p. 14), the sad fact is that “[g]iven the glamour of brain science and the seemingly concrete nature of neurophysiological studies, the conclusions have often been readily accepted by the public.” Example (2) illustrates what we call the “linguistic sponge myth”: children soak up language like a sponge; you just have to talk to them and they learn automatically.

Our study
Since 2008, we have been carrying out a longitudinal study in Switzerland to analyze how what happens in the course of mandatory instructional time in a multilingual country can be optimized for long-term benefits to unfold before the end of secondary education (see, e.g., Pfenninger, 2014a, 2014b;
Pfenninger & Singleton, in press). The bulk of the data were collected during a period of transition when there co-existed for some time students who were subject to one or other of the two educational policies that were implemented before and after the Swiss Conference of Cantonal Ministers of Education issued a new set of guidelines for foreign language (FL) instruction throughout Switzerland (see EDK, 2004). In total, we were able to recruit 500 participants. In the longitudinal part of the project, we followed 200 of the same learners, belonging to different age of onset groups, over five years in secondary school.

Findings of the study

In a nutshell, the results provided no clear evidence that there is any special advantage in starting the study of a foreign language very early. After just six months, the late starters had caught up with the early learners and sometimes even surpassed them – e.g., in terms of oral and written morpho-syntactic accuracy and complexity, syntactic fluency, grammaticality judgment, and content-related and structural aspects of written expression. What is more, the early starters were not able to retain their learning advantages in the long run: all age-related differences were levelled out over the course of secondary school. This means that overall, the late starters did better than the early starters, as they were able to acquire more FL knowledge within a shorter period of time and progress faster than younger learners on a variety of oral and written tasks including productive and receptive vocabulary, morpho-syntactic accuracy and complexity, syntactic fluency, grammaticality judgment, and content-related and structural aspects of written expression. We have come to the following fundamental conclusions as regards the factors that do not seem to work in early starters’ favour – i.e., that provide the late starters with a ‘kick start’ in the initial stages of FL learning:

A The late starters began English education with a better foundation in Standard German (their primary language of literacy) than the early starters. Since high statistical correlations were found between L1 literacy skills and target language literacy skills irrespective of starting age, this finding may contribute to the ‘kick start’ of the late starters described earlier. However, in the long run, early English did not have a negative impact on German.

B Type of instruction (e.g., partial immersion instruction) and, above all, motivation were stronger predictors of FL performance than starting age. Strong motivation in learners and the experience of immersion was thus able to make up for a later start.

C At the beginning of secondary school, the early starters’ motivation was less strongly goal and future focused, they were more anxious than the late starters and displayed less positive attitudes to FL learning and
the learning circumstances, partly because of their dissatisfaction with Early English and the transition from EFL at primary level to EFL at secondary level.

D Classroom effects (i.e., class size, classroom organization, the role of the peers, teacher, and teaching approach) impacted on students’ motivated behaviour and, by extension, affected their FL achievements and mediated age-related differences. In more simple terms, it was more important in which class a student was than when they started learning an FL.

In sum, the early starters did not surpass the older starters over time, as has been observed generally in the research on age with naturalistic L2 learners, and we could not detect any age-related differences at the end of mandatory school time. The older learners started out more strongly (e.g., in terms of motivation and L1 literacy skills), and even though the younger learners improved along the way, the older learners maintained their advantage in these regards throughout. For many people (including many applied linguists) such results are surprising, but, as indicated earlier, they are in full accord with results consistently obtained for at least 40 years.

The real research problem

In contrast to such fields as bilingualism, where positive outcomes (i.e., studies with significant results) are favoured over studies with a null result irrespective of quality of design or the way the experiment was carried out (e.g., de Bot, in press; de Bruin, Treccani, & Della Sala, 2015; but cf. Bialystok et al., 2015), there seems to be no such publication bias in the age factor research area. On the contrary, recent years have seen consistent results published in the age literature showing very few linguistic and socio-affective advantages to beginning the study of an FL earlier in a minimal input instructional situation, as pointed out earlier. What is more, research on the age factor has recently begun to adopt the kinds of statistical models (e.g., linear mixed effects models) that allow multiple random factors and thus take account of both participant and item variability, allowing for the simultaneous generalization of the results on new items and new participants as well as the assessment of the impact of context-varying factors on age (see Pfenninger & Singleton, in press), which means that we are nowadays able to achieve adequate estimates of variances and therefore correct standard errors and correct inferences and (likelihood-based) $p$-values. Owing to these statistical advances and the general picture that has emerged in Europe and across the world, we can thus safely assume that the age literature is not skewed due to a publication bias. The struggle for us relates not so much to getting our findings published as to getting them taken account of.

Unfortunately, the findings showing advantages for older instructed beginners over younger instructed beginners have not had much influence when it
came to making decisions about when to introduce FL instruction in Swiss schools (see also Spada, 2015). The overwhelming popular consensus – not just in Switzerland but universally – is that childhood is the best time to start to learn an additional language. First language development is something that happens in childhood and so the assumption is that children are best equipped to acquire languages. Moreover, common experience tells us that starting to learn *anything* early in life – the violin, chess, golf – often appears to yield dramatic advantages. This consensual view appears to be supported by the everyday observation of newly arrived immigrants: “young children . . . pick up a second language with little trouble, whereas adults seem to struggle ineffectively with a new language” (Macnamara, 1973, p. 63). According to Watts (2011, p. 4), doubting the factuality of such a deeply ingrained myth can be “interpreted as an act of heresy if the story, or even only part of it, is firmly and widely believed by the group.”

The ‘younger = better’ view is extremely difficult to budge. McLaughlin (1992, p. 2) suggested that people continue to believe that children learn languages faster than adults in instructional contexts despite all scientific evidence to the contrary, partly because they apply the same criteria of language proficiency to both the child and the adult:

The requirements to communicate as a child are quite different from the requirements to communicate as an adult. The child’s constructions are shorter and simpler, and vocabulary is relatively small when compared with what is necessary for adults to speak at the same level of competence in a second language as they do in their first language. The child does not have to learn as much as an adult to achieve competence in communicating. Hence there is the illusion that the child learns more quickly than the adult.

**Overcoming the obstacle**

It has always been clear to us that, for reasons mentioned earlier, our findings were of considerable theoretical and practical significance, since they are at the heart of the debates revolving around age as one of the most powerful and misunderstood variables in the research on FL learning and teaching, and they are integral to designing effective FL pedagogy. Our main ‘strategy’ has been (1) to endeavour to convince the members of the general public that the time is ripe for closer integration between SLA research and L2 pedagogy and (2) to educate them about recent trends in the age factor tradition in SLA research. We realized that consistent and intensive collaboration between practitioners, politicians, and researchers is needed in order to understand and address mutual interests and concerns through shared discussions, data collection, analysis, and interpretation.
Our essential approach has been to ‘go public’ – as public as possible – with our research findings – with which we are very comfortable and in which we are totally confident. We have of course presented papers at numerous academic conferences, but this was just the beginning. We have had experiences that were poignant reminders of deeply held beliefs about the advantages of early L2/FL instruction when we have been invited to speak with teachers, teacher educators, and representatives of the Cantonal Ministers of Education about achievement targets in Early FL learning, research on the role of age in L2/FL learning, beliefs, and practices. Besides summarizing our findings that Swiss learners who start learning a language early in the school years do not succeed more than learners who start later when exposure to the target language is restricted to the school setting, we also emphasized the importance of factors other than starting age that contribute to success in FL learning (or lack thereof), such as quality, quantity, and intensity of instruction in primary and secondary school and the problems with the transition from primary to secondary school (e.g., mainstreaming). Even though by and large, these encounters, meetings, and workshops were very positive and pleasant, our input did not seem to be welcome. Teachers and practitioners felt we were accusing them of ‘failing’ their students; politicians considered us to be ‘unpatriotic’ for pointing out the problems that come with Early French and to be ‘not keeping up with the times’ for suggesting a later start for English instruction.

Furthermore, it was very clearly communicated to us that Early FL learning has overt political and economic dimensions that trump the arguments of SLA researchers. There are several more reasons why our input and ideas have fallen on deaf ears:

1. The national reforms for foreign languages in Switzerland have posed several challenges, as the complexity of the Swiss educational system includes several models, different types of state schools, and different approaches to the teaching of English; and reflects cantonal differences; in fact, there are 26 different education systems for the 26 cantons and half-cantons (Brohy, 2005). This means that undoing the reforms would also be problematic.

2. Reorganizing and redistributing the instructional input to primary school students does not seem feasible at this stage, as this would entail the cutting of instructional hours for other school subjects. The decision to introduce foreign language instruction in the elementary grades is always weighed against the costs to other components of the school curriculum. Since the Cantonal Ministers of Education introduced two Early FLs, major changes would have to be made to the organization of the entire primary school curricula to increase the amount of FL instruction.

3. Education politicians are not willing to intensify FL instruction over shorter periods of time later in the school curriculum (i.e., in secondary
school), as this would be considered “back-pedalling” and a failure of the new education acts.

Undeterred, we have also exercised an ‘open-door’ policy with regard to the media. Whenever it was possible for us to respond positively to invitations for newspaper interviews and radio and television appearances we have done so. In all of our public pronouncements, we have consistently highlighted the fact that there are numerous problems other than teachers’ incompetence that account for the lack of success of younger learners and that our study highlights the consistent advantages for, and great progress of, older learners in school contexts rather than the failures of younger learners. Furthermore, we have emphasized that it is neither our goal nor our job as age researchers to criticize teachers’ practices or politicians’ decisions but to provide information that can – but does not necessarily have to – help teachers, politicians, and policy makers set realistic expectations for themselves and the students involved.

Despite these qualifications, our willingness to engage in public discussion and debate has quite often entailed quite bruising experiences for us, as we have intimated. No one should be under any illusions that this kind of process is an easy one. Nevertheless, in 2015, age research won a small victory when a decision was made to begin English in Zürich in grade 3 rather than grade 2 (Bildungsdirektion des Kt. Zürich, 2015). The main avowed reason given for this, as explained in the relevant language policy documents, was the goal of uniformity in the Swiss educational system – i.e., ironing out cantonal differences. To (mis)quote the late Mandy Rice-Davies, they would say that, wouldn’t they!

Implications for researchers

This chapter has clearly shown that research sometimes contradicts commonly held beliefs and myths that have influenced instruction, assessment practices, and the organizational structure of educational programs. We have seen that despite the consistent findings of large-scale, longitudinal classroom studies, evidence against the advantage of an earlier start to foreign languages in an instructional setting is far from being established as the basis of accepted wisdom. Yet if one wants to understand the nature of a phenomenon, such as the potential benefits of early foreign language learning, one needs to take account of relevant good studies regardless of their findings and regardless of one’s own prejudices or agenda. As Nikolov and Mihaljevic Djigunovic (2011, p. 112) rightly point out, realistic aims and achievement targets must be continuously re-examined in specific educational contexts to meet local needs and reflect local realities.

We think that politically sensitive issues in SLA should be confronted with honesty and seriousness, and that researchers should not shy away from
investigating them. Sometimes doing good research means being the killjoy. In Watts’s words,

No amount or logical argument or practical demonstration will prevent people from constructing and believing in myths, and any attempt to do this is doomed to failure. As linguists, however, we need to make ourselves aware of the basis of our own beliefs about language, and if we feel that there is something not quite right about those beliefs, perhaps an analysis of the myths may help us to transform them.

(2011, p. 23)

In relation to the mistaken popular beliefs about age effects in the context of instructed foreign language learning, as in relation to other false notions about language, we cannot necessarily wipe the slate clean, but it is our responsibility to be true to our research in resisting the application of such notions in education.

References


Introduction

The community of applied linguistics researchers has become increasingly receptive to qualitative research in the last decade. Previous reviews have identified a trend of rising presence of qualitative studies published in leading applied linguistics and language learning journals (e.g., Benson, Chik, Gao, Huang, & Wang, 2009; Lazaraton, 1995; 2003). While researchers now increasingly appreciate the importance of adopting qualitative methodological approaches to gain in-depth contextualized and often cultural understanding of key issues in language learning and teaching, for many researchers, publishing qualitative studies in major journals continues to be a challenge. Among many challenges that researchers are likely to experience, I believe that the framing of research issues and methodological rigour are the most difficult issues for qualitative researchers to address. In this chapter, I recount and reflect on experiences of dealing with these two challenges in publishing qualitative studies in international journals. Drawing on my encounters with reviewers, I discuss how qualitative researchers can respond to these challenges posed by anonymous reviewers as well as other researchers.

Qualitative researchers’ claims and challenges

Qualitative research is usually believed to have a number of strengths that justify it as a legitimate methodological approach to language learning and teaching research. According to Dörnyei (2007), researchers have argued that qualitative inquiries are often exploratory in nature and help us understand complex phenomena in an in-depth manner as they often address questions as to why particular phenomena, especially those contradictory, occur at the research sites. Though limited in terms of generalizability, qualitative studies usually gather rich data about particular research issues and can examine “dynamic phenomena” in a longitudinal manner to capture the dynamics and “broaden our standing” (Dörnyei, 2007, pp. 39–40).
These strengths claimed to be characteristic of qualitative research, if fulfilled, do strengthen qualitative methodologies as crucial means of knowledge production and verification in applied linguistics research. Unfortunately, the actual realizations in research have proved rather strenuous and complex in my experience. As Holliday (2002) commented, “[whereas] in quantitative research the source of validity is known, qualitative research has to show its workings every single time” (p. 8). I regard this validity issue as a challenge imposed by quantitative researchers and believe that the quality of qualitative research needs to be assessed in other terms, such as transparency and trustworthiness. Nevertheless, it must be noted that qualitative researchers are often challenged by critics for failure to deliver the promised strengths as outlined by Dörnyei (2007). When a qualitative study is under review for publication, critical reviewers also likely identify many half-hearted measures undertaken by researchers striving to “show its workings” before recommending publication (Holliday, 2002, p. 8). For instance, many qualitative writers, including myself, may rely too much on the conceptual framework when analyzing data and reporting results that appear to have stretched the relevant data to fit the existing frameworks rather than expanding alternative interpretations about the relevant research issues. Instead of being exploratory in nature, such qualitative inquiries are apparently constrained by the conceptual frameworks used. Such use of conceptual frameworks in interpreting data for reporting can hardly be considered in-depth examination of data that aim to answer ‘why’ questions. Instead, it shows that qualitative interpretations are highly subjective with bias brought in by the adopted conceptual framework (LeCompte, 2000).

As a manuscript reviewer, I noticed that many qualitative writers present their findings to address particular research issues in descriptive and superficial ways. The so-called insights are often presented in a variety of themes emerging from the analysis. Since some qualitative writers do not attempt to identify conceptual links among the themes that emerge from analysis, the results are often presented as if they were scattered beans on the floor. Moreover, the process of identification of themes emerging from the qualitative data analysis is often a mysterious process. Though research is always a messy process, it is desirable for qualitative researchers to demonstrate and justify how particular themes emerged from the analysis. Therefore, it is necessary to reflect on the proclaimed strengths of qualitative research and take up challenges of delivering them when writing research papers for publication. Reflecting on these critiques, one may contend that qualitative researchers should respond to these challenges by framing qualitative inquiries properly and making the entire research process transparent for close examination. To this end, I recall my experience of publishing two qualitative studies by addressing the relevant critiques from the reviewers while revising the manuscript in the course of the publication process.
Challenges in publishing qualitative studies

In order to illustrate how qualitative researchers can respond to critical comments, I recount the challenges I experienced in publishing two studies in applied linguistics journals. The first study (Gao, 2006) reinterpreted the data that I had collected for my dissertation project on Mainland Chinese students’ language learning experiences in the United Kingdom. The second study (Gao, 2015) examined the ideological framing of regional Chinese varieties or ‘dialects’ in Mainland China’s state print media. Since the two studies were conducted at different times on different research issues, I displayed quite different understandings of the quality and rigour of qualitative research when writing them up for publication. In the process of publishing the first study, I was made aware of the critical importance of adopting an appropriate conceptual framework for some observed phenomena or findings emerging from the analysis to ensure significant contribution to the relevant disciplinary areas. In the case of the second study, I managed to respond to the challenge of presenting qualitative research, especially qualitative data analysis, in a detailed manner so that the study’s rigour could be well captured in writing.

The dissertation project (Gao, 2006) drew on an inquiry into Mainland Chinese students’ experiential accounts of language learning before and after arrival in United Kingdom for academic studies. The inquiry involved a mix of 14 students from foundation and postgraduate programmes at a British university for retrospective life history interviews, in which they were encouraged to share why and how they learnt English in China and Britain. In the manuscript first submitted to the journal, I claimed a grounded theory approach (Strauss & Corbin, 1998) was used and participants’ accounts were compared to identify significant themes, such as difficulties or challenges, strategy use, and motivation. These themes helped construct a meaningful story in which participants recounted having encountered quite a few challenges or difficulties in learning English after they arrived in Britain and had adopted a variety of strategies to overcome these. The research narrative also explained why and how these participants were motivated to learn English in China and Britain. Since this is one of the first pieces of work that I had ever submitted to a journal for consideration, I just presented the findings by listing major themes with data extracts and made limited efforts to theorize the themes identified from the analysis. It was not surprising that the original manuscript received devastating reviews. However, I was luckily given a chance to revise because the editor was aware that I was a junior and new researcher.

In their detailed reports, the reviewers largely challenged me for being too descriptive and simplistic in the presentation of the findings. One reviewer wrote explicitly in the report, “This is a very nice story but so what?” It was the first time for me to receive such a comment but it was
not the last. The comment is indicative of this particular reviewer’s uncertainty as to what significant knowledge contribution the study could have made to the relevant field. Though it might be demoralizing for junior researchers to receive such comments, the remark prompted me to search for what could convince anonymous reviewers – gatekeepers of academic communities – that a particular qualitative study is worthy of publication. For this reason, I reflected on what roles qualitative inquiries can possibly play in the field of language learning strategy research, on which the dissertation project hinged.

My experience of publishing Gao (2015) testifies to the fact that the field of applied linguistics research has progressed and as a result, it has much higher expectations of methodological rigour, in particular, in qualitative studies. Gao (2015) reports on a study that examined the ideological framing of regional Chinese varieties or ‘Chinese dialects’ in the Mainland Chinese state print media coverage of ‘dialect crisis.’ The analysis focused on how the state print media reports justified the preservation of regional Chinese varieties, revealing that governments at various levels are fully aware of the importance of these regional Chinese varieties in sustaining individuals’ regional, cultural, and social identities. Governments and business corporations were also found to have used individuals’ identification with particular regional Chinese varieties to better promote their messages and products. These findings are believed to have important implications for preservation of regional Chinese varieties whose existence is being threatened by the promotion of the standard Chinese spoken variety (‘Putonghua’) as endorsed by the national language policy and massive internal migrations induced by rapid socioeconomic changes.

As a reader of manuscripts for a dozen international journals, I always notice that manuscripts reporting on qualitative inquiries usually have more detailed and longer descriptions of data collection but limited or little information on data analysis. The lack of details concerning data analysis has become problematic in recent years as this shows that authors are usually not clear or transparent about the research procedures when reporting qualitative studies. Consequently, qualitative data analysis often remains a murky unfathomable process in many manuscripts. It is not only difficult for potential readers to evaluate the quality and strength of the related claims put forward by these manuscripts, but it also reduces credibility of the results for readers. Many years ago, it might have been tolerated with some reservation if an author wrote, “We read the interview data again and again till themes start emerging” to describe the qualitative data analysis. As the field is progressing rapidly, it is no longer possible for a manuscript with such descriptions to survive the initial reading of journal editors. I have repeatedly received critical comments on descriptions of qualitative data analysis. For instance, one reviewer requested more details to be provided about analytical
procedures rather than expecting readers to work out how data were analyzed, when commenting on a manuscript I wrote some years ago:

I think that the author needs to provide more information on data analysis. For instance, ‘commonly used processes of qualitative analysis in grounded theory’ is not a convincing presentation of data analysis to this reader. . . . There are also many versions of grounded theory. What was the version of the theory that the author adopted to guide his or her data analysis?

(Author’s own data)

For this reason, I always conscientiously present relevant details about data analysis when drafting manuscripts for submission. Yet, even after providing detailed description of analytical procedures in an early version of Gao (2015), the reviewer still requested that these procedures be illustrated with specific examples to show how these procedures were followed. Considering each data extract could have multiple interpretations, the reviewer further commented,

The identification of ideological frames, as it were, is achieved through multiple readings of the news texts. But no introduction of qualitative content analysis as the methodological approach is provided, nor is a brief explanation of how to systematically handle multiple interpretations of the same news reports.

(Author’s own data)

**Overcoming the challenges**

**The journey of a novice writer: The case of Gao (2006)**

The challenge that I experienced in publishing Gao (2006) invited me to examine the then dominant theoretical assumptions or approaches in language learning strategy research since qualitative inquiries have the potential to enable researchers to identify alternative theoretical explanation of research issues. If a qualitative study on language learning strategies only describes a small number of language learners’ strategies in their own words, such a study has little significance in comparison with a large-scale survey which can capture a large number of students’ strategy usage. If qualitative inquiries could help researchers understand a research issue in a highly contextualized, in-depth manner, I should not focus only on describing what had happened and how participants responded in the inquiry. Instead, I should
identify conceptual links among the themes identified during data analysis and conceptualize the themed findings so that the study could constitute a theoretical contribution to facilitate future studies to examine language learning strategies with the newly acquired theoretical lens. Further readings of relevant literature led me to believe that most language learning strategy research had been dominated by cognitive approaches to language learning. These “cross-sectional, descriptive, co-relational and intervention” studies have concentrated on “listing and classifying language learners’ strategy use, establishing relationships between strategy use and various factors and efforts to develop language learners’ strategy use to enhance their language learning” (Gao, 2006, p. 56). Therefore, it is necessary to examine language learners’ strategy use as a “dynamic,” “temporally and contextually situated phenomenon” (ibid). To this end, I introduced a sociocultural perspective that sees “learners’ strategy use” as “the result of both their individual cognitive choices and the mediation of their particular learning communities” (ibid, p. 57). This sociocultural perspective also postulates that “learning communities mediate learners’ strategy use through ‘semiotic symbols (especially language),’ ‘objects,’ and ‘(powerful) persons’” (ibid). This resulted in the crucial analytic foci when analyzing the data in search for conceptual links.

By reframing language learner strategies for an alternative perspective, I felt obliged to reanalyze the data with the new perspective. The analysis involved a close examination of themes identified from the previous analysis and an attempt to recognize any possible conceptual links among these themes as informed by the sociocultural perspective. It also involved recategorization of various themes and their related extracts under the sociocultural framework, which include “language (discourses about language learning), objects (assessment) and agents” (Gao, 2006, p. 58). In addition, particular attention was paid to contexts where particular participants had learnt English in particular ways.

The substantial revision I undertook in response to the reviewer’s remark exceeded what was expected of me by the reviewer and the editor. The reframing of language learner strategy within a sociocultural perspective also helped to situate the study within a niche in the field. It helped the organization and presentation of relevant findings in a conceptually related manner. The original qualitative data, which could have been presented descriptively as difficulties and strategic responses, could now be used to illustrate how and why these language learners adopted particular strategy sets in response to shifting contextual conditions. They were used to extrapolate how language learners can well consider contextual conditions and adapt their strategic language learning efforts on their own, for new learning demands. For these reasons, the reviewers and the editor made no requests for further revision and the manuscript was accepted for publication shortly after this revision. Having managed to
have this manuscript accepted for publication, I continue to face similar challenges in many other academic publication endeavours. For instance, a prestigious journal in the field rejected one of my manuscripts for the following reason:

XXXX is committed to publishing high-quality studies that help build a sound theoretical understanding of the field. Unfortunately, your manuscript at present does not fulfil this criterion. We would be happy to reconsider a new submission with a substantial theoretical underpinning and contribution to new theory-building, rather than just a description and evaluation of XX.

(Author’s own data)

In addition, the published manuscript could still be considered weak in terms of methodological representation if it were evaluated according to expectations of rigour and quality these days. For instance, I did not critically reflect on how the use of a theoretical framework could be compatible with a grounded theory approach and how qualitative data had actually been analyzed. If the same manuscript were submitted to the same journal today, it would probably be rejected by both reviewers and editors.

**Being explicit about qualitative data analysis:**

**The case of Gao (2015)**

Some of these methodological issues are exactly what I experienced as challenges when I was trying to publish Gao (2015). To address the concerns that the reviewers had about an early version of Gao (2015), I felt obliged to provide explicit and detailed descriptions of different analytical procedures when revising the manuscript. In addition, I included a data extract to demonstrate how all the data were analyzed in line with the outlined procedures when writing about data analysis. For these reasons, a lengthy description of analysis could be found in the revised manuscript in response to the reviewers’ comments. First of all, it presents the particular analytic approach adopted for the analysis and what was involved in the process:

In the recursive analysis, initial codes of the ideological frames were developed, continually revised, related and linked with the help of Nvivo 8.0 and the assistance of a PhD student with training in qualitative analysis, until they were organised into ‘meaningful clusters’ or themes recurring in multiple news texts.

(Gao, 2015, p. 472)
Following the naming and description of the analytic approach, I describe how the analysis was conducted in multiple stages and what the foci of analysis in each stage were as follows:

This was achieved by multiple readings of the news texts as guided by the research questions. The first reading of the news texts was to get a general understanding of issues covered and starting from the second reading, efforts were made to identify different frames used to represent or project images of particular regional Chinese varieties. Since many texts covered different views on the status of specific varieties, the researcher focused on identifying multiple frames a news text might have to develop the initial codes. With these initial codes and NVivo 8.0, the assistant coded all the media texts and further expanded the list of codes in the third reading. In the fourth reading, the researcher refined these emerging categories of frames, during which attention was paid to the ideological linguistic presumptions underlying these frames [. . .] Both the researcher and the assistant were involved in the fifth reading, after which the main arguments as advanced by a text were summarised and the codes were reduced to nine major frames. The researcher conducted the sixth reading to identify how different frames related to and could be linked with each other. In this final reading, data relevant to negative attitudes towards dialects’ were removed and the remaining frames were combined into four major themes to structure the current findings. In cases where a news text had multiple frames, the researcher undertook additional readings and decided the main ideological frames of the data extract after discussing them with the PhD student. The following extract is used to illustrate the analysis of ideological framing of regional Chinese varieties in a news text.

(Gao, 2015, p. 472)

As suggested at the end of the extract, I include a data extract which can lead to multiple interpretations so that readers can better appreciate how multiple interpretations of data extracts were processed in the study. Immediately after the sample data extract, I demonstrate how multiple interpretations of the same data extracts were achieved:

As reflected in the extract above, regional varieties are objectified into valuable art forms closely associated with speakers’ identities [. . .]. In this particular case, the Yan’an dialect of the Shaanxi group is presented as a defining attribute of the people from the area, who have often appeared as supporters of the communist cause in revolutionary dramas[. . .]. In addition, the contrast between Yan’an folks speaking Putonghua and those speaking the Yan’an dialect can be described as a process of recursivity [. . .], which helps strengthen the differentiation
between the Yan’an and the non-Yan’an. For these reasons, this extract is categorised as a frame of some regional varieties as ‘identity markers.’ Nevertheless, it is also possible to relate this extract to my attempt to frame regional varieties as objects having commercial or entertainment value since references are made to ‘art forms’ and ‘drama’ also. Both the researcher and the assistant decided to code this text with ‘identity markers’ because the entire news report was believed to have focused primarily on the important association between the identity and ‘dialects’ in the summarisation process.

(Gao, 2015, p. 473)

The extract demonstrates how I worked with my research assistant in deciding which emerging themes we should assign to particular news extracts. It must be noted that dialogues between the researcher and colleagues are particularly important, as they help minimize the impact of individual biases on data interpretation (LeCompte, 2000). With this data extract, I also show how ‘the researcher and the assistant’ came to agree that ‘identity markers’ should be the code for the particular text.

In contrast to two or three lines of description of data analysis in many qualitative studies 15 years ago, such lengthy writing is indicative of the close attention now paid to methodological procedures. I have been increasingly adopting such detailed methodological descriptions in writing up qualitative studies for publication because of the belief that a crucial step in enhancing qualitative studies’ rigour is to make the whole data collection and analysis transparent, open to critical censure. This does not mean an attempt to present a misleading picture of qualitative research and negate the fact that “day-to-day research comprises short-cuts, hunches, serendipity and opportunism” (Holliday, 2002, p. 7). However, reviewers’ critical comments on qualitative data analysis do oblige qualitative researchers to write increasingly lengthier texts to show how data analysis exactly works in qualitative inquiries.

Conclusion

As I reflect on my own experiences of publishing qualitative studies, I could not help becoming acutely aware of the progress that the field of applied linguistics has made, as reflected in the reviewers’ expectations with regard to the presentation of qualitative findings and methodological information. I started publishing qualitative research at a time when reviewers were still happy with new insights one could gain from analyses of discourses or narrative data. However, researchers are increasingly wary of ‘interesting’ stories and stress the importance of methodological rigour and theoretical contributions when evaluating manuscripts for publication. If qualitative researchers believe that qualitative methodologies allow researchers to gain
in-depth rich knowledge of particular issues situated in particular sociocultural contexts, they are justifiably expected to deliver these promises fully in execution of qualitative data analysis and presentation. This means that qualitative researchers need to aim for conceptual or theoretical insights if the limited number of participants prevents generalization of the relevant findings to larger populations. Researchers also need to explicitly demonstrate the procedures used to analyze and interpret the data so that the whole process is transparent enough for close and critical scrutiny. By doing so, qualitative studies can then generate trustworthy findings to move the field onward.

References


Chapter 22

Representing the self honestly in published research

Christine Pearson Casanave

Introduction

As part of my doctoral education, I was immersed in the typical author-evacuated prose of published social science research, particularly of the quantitative variety. At the time, I did not think much about how odd it was that the researcher-author hardly ever made an appearance in the finished piece. Facts and findings seemed to happen on their own, as they often seem to do in science, but many of these lifeless articles left me yawning and wondering if I was smart enough for doctoral study. It was only later that I became familiar with tomes on writing such as Dillard (1989), Zinsser (1998), and Williams (1997), with some rare models of beautiful academic writing (e.g., Eisner, 1997; 2002), and with powerful autoethnographic texts (Richardson, 1997a). I want to write like that, I told myself.

But that took me to another dangerous place: the potentially dangerous confessional space of honest reporting (see Smagorinsky, 2011; van Maanen, 1988). In this chapter, I explore some dilemmas of self-representation in scholarly writing, focusing on an article I wrote over a three-year period about my experiences learning some Japanese informally (Casanave, 2012). The 2012 journal article was one of the most difficult I have ever written, in part because I was trying to critique some of the well-established motivation research, although I was not an expert in this area. But perhaps more importantly, I did not know how to represent myself as something more than a failed language learner. That was not the image I wanted to convey. I ended up revising this article 20 times before it was published, as I tried to figure out how to represent myself in this tale.

As it turns out, it is nearly impossible for authors to hide their identities in their writing (Ivanič, 1998). Moreover, published authors quite often refer to themselves in first person, although disciplinary differences exist, including fewer self-mentions in the hard sciences (Harwood, 2005a; 2005b; Hyland, 2012) and odd proscriptions continue by some publishers (Starfield, 2015).
Younger writers might lack the confidence to front themselves in first person, but cultural constraints might also inhibit their taking on the role of expert (Hyland, 2012). However, what even expert authors avoid talking about are the difficulties of crafting a publication that looks seamless in finished form, from research that did not proceed linearly or even smoothly. For the published version, authors usually remove the struggles, twists, and turns so as to comply with the rhetorical conventions and stereotypes of academic writing. They do not write about themselves. Readers are thus left with the impression that indeed there are real authors out there, but that they do not struggle with their research or writing. I hope to provide a different, more honest, model, especially for novice scholars.

I need, thus, to distinguish between two kinds of self-representations in published writing. The first concerns tales of the self, as in autoethnographic or autobiographical work (Ellis, 1997; Ellis & Berger, 2001; Ellis & Bochner, 2000; Richardson, 1997a; 1997b), or more limited self-presentational genres such as bios and home pages (Hyland, 2011a; 2011b; Hyland & Tse, 2012). The second type concerns the transparency with which authors write about research or writing processes, even when the study is not about themselves. In both cases, authors confront the dilemma of deciding how much, and what, to reveal about themselves, and how to do this so that they project an identity that does not undermine how they wish to be seen by readers in a public forum.

My problem

Here was my main problem. I tried to write an article on my efforts to learn some Japanese, without formal instruction, during the many years I was working in Japan (Casanave, 2012). I wanted to write about myself, in an honest way, but in a way that did not convey the impression that I was a failed language learner. I also wanted to critique the existing motivation literature. Note that I did not have a problem using first person in my writing, a style I have been using for many years. I have also wrestled publicly numerous times with how to connect with and represent myself in published academic discourse. I have emphasized implicitly and explicitly that academic writing can and should front authors, and that being a native speaker of English does not mean that academic discourse flows magically into one’s prose (Casanave, 1997; 2003; 2008). You don’t need to hide your struggles, I wanted to say to readers; they are normal. Struggling to write does not imply incompetence.

However, the problem of self-representation in the 2012 “Dabbler” article was special. After all, I had been an English language instructor and teacher educator for many years, and therefore had studied how people learn additional languages. I had learned Spanish and some French from high school and became fluent in Spanish with further study over the years.
As part of my philosophy of language education, I believe that language teachers need both to be competent in at least one additional language and also to pursue further language study to the extent they can find time and energy to do this over the life of their careers. When I moved to Japan for work, I had no particular interest in Japanese, but saw my time there as a chance to practice what I was preaching. I kept journals for 13 years, during which I was working full time at a Japanese university, and for nine of those years worked part-time on weekends at a branch of an American university in Tokyo. These journals featured notes and observations on Japanese and my learning activities, and as time went by, I wrote more commentaries on other aspects of my life.

But learning Japanese was not a central focus or goal in my life. After 13 years there, when everyone around me expected me to be fluent, at least in conversation, I was still a beginner. Some years later, settled back in California, I decided to respond to an invitation to write a book chapter on my language learning history. That’s where the Dabbler paper originated. I could easily represent myself in that tale as a successful learner of Spanish, but how would I represent myself as anything but a failed learner of Japanese? The chapter was written and submitted to the editors five days late and was instantly rejected: The manuscript has already been sent in, the main editor said (with no editing, reviewing, or revising). I closed the file on that version and then started over.

The next stage

As I continued to think about my experiences trying to learn some Japanese, and my failed attempt at writing a personal essay about my language learning history, I realized that my main theme was really about motivation, or lack of it. I decided to spend some weeks reading my Japan journals to see what I could find, knowing that over many years in Japan my motivation to learn Japanese indeed fluctuated wildly. In my failed essay for the initial book chapter invitation, I had tried to focus on my interactions with a conversation tutor, a neighbourhood friend. I wanted to show how the tutor was eventually not able to run the conversation tutorials the way I wanted – as real, if simplified communication, without lengthy grammar explanations and corrections in English. I found myself increasingly frustrated and angry, giving up my sessions with him and then giving up my self-study of Japanese altogether for many months.

In my journal books, I did indeed find evidence of much frustration with the conversation tutor. However, there was also much more, hinting at ‘real life’ getting in the way of my efforts. I went back to the motivation literature, and refreshed my understanding, beginning with the early Gardner, Lambert, and MacIntyre work (e.g., Gardner & Lambert, 1959; Gardner & MacIntyre, 1993), and continuing through the more recent work of
Dörnyei (e.g., 2001; 2009). Two things struck me about this work. First, it was all based on survey research that took place in language classrooms. I had decided (for reasons I describe in the 2012 article) not to take classes. Second, it presumed that language students should be motivated and that if they are not, teachers can and should help them become so. From these two perspectives, because I did not take classes and I did not exhibit the kinds of motivation documented in the literature, I was surely a failed language learner. How, as a language teacher committed to helping people learn English, with enthusiasm and motivation, could I possibly write about myself as a failed learner?

Steps to a solution

Reconceptualizing motivation

In the years I lived in Japan, I could not make myself study Japanese regularly, even on my own terms. Among other things, I knew that to become proficient in Japanese would require a great deal of focused time and energy. I had neither and was not willing to sacrifice other time-consuming activities (e.g., writing for publication) to devote to focused Japanese study. Most important for the 2012 article, however, was my realization that many environmental and internal factors were affecting my willingness to devote effort to study, from weather, to health, to sleep, to personal and work-related stresses. I could find only minimal attention in the motivation literature to these factors, which seemed to colour all aspects of my irregular efforts.

Yet I did not wish to give up what I came to call “dabbling” in Japanese, or to consider myself a failed language learner. Much later, upon returning from Japan, I coined the phrase ‘ecology of effort’ to try to move away from traditional views of motivation and toward a characterization of my fluctuating desires and ability to devote effort to language study. In particular, I wanted to characterize my fluctuating effort as normal and acceptable, especially for adults, rather than as failed motivation needing to be addressed as a problem.

Deciding to present my story as ‘research’

In spite of my experiences potentially being seen as a failure of motivation, I decided to proceed with my effort to write something for publication. If my story were presented as ‘research’ rather than a tale of woe and failure, perhaps I could communicate my increasing sense that there were many goals for language study besides that of becoming a fully competent user, and that students who lacked traditional motivation and drive could and should be honoured for their honesty and for other interests that did motivate them. After all, many of my past students of ESL and EFL had little desire to study
English intensely or to work or live in an English-dominant country. Should they give up their half-hearted attention to English if they had no deep or long-lasting interest in it? Should they give up just because life (other interests, health, stresses) got in the way? Did language study have to be all or nothing?

Why, in my own case, did my interest and effort seem to fluctuate so wildly?

Because there was so much evidence in my journals of the ecological factors influencing my ability to invest effort in Japanese study, I decided to turn my original language learning history essay (the rejected one) into a piece of autoethnographic ‘research,’ in the tradition of the diary studies (Bailey, 1983; 1990; Carson & Longhini, 2002; Curtis & Bailey, 2009; Hall, 2008). A few of these studies actually documented a version of the self-study that I was attempting, with a focus on the adult language learner (Jones, 1995; 1998; Schmidt & Frota, 1986). However, I needed real data to do this.

So I read those studies again and then studied eight years of my journal entries, 788 pages, and made extensive notes of themes. This was not always pleasant; life was not always easy in Japan. Stresses came out, anguishes and angers, health and sleep problems, and tensions at work. Still, many pages documented my observations of bits and pieces of Japanese language, and showed the interest and curiosity I had about the language itself. When the journals recorded tensions and stresses, my attention to Japanese plummeted. This happened on page after page.

Rather than count and code topics in the style of some qualitative research, I reviewed my Japanese-learning entries repeatedly, adjusting themes as needed, and extracted juicy quotes that I thought could be presented in the article. Yet once again, I was risking representing myself as, in some sense, a failed language learner. I had not persisted in the face of what I came to call ecological influences that robbed me of energy and focus. Why did I not give up altogether, as many of my English-speaking colleagues had? (I note as well that many others thrived in Japan and became fluent users of the language.)

I revised the original rejected essay, already on draft five, and decided to submit the revision to TESOL Quarterly. The initial drafts of the article for TESOL Quarterly continued to focus on the experiences with my conversation tutor, which were easy to document and vivid in my memory. “Losing and Regaining Motivation” continued to appear in the title. Reviews came back months later, many pages worth, with a request to revise and resubmit. I think I am not unusual in not being able to immediately plunge into the next set of revisions; I read the pages of comments and, overwhelmed, put them aside for many weeks.

As often happens in my writing when I harbour deep feelings about a phenomenon as diffuse as ‘motivation,’ I had wandered in this draft. I was asked to be clearer about what my point was. I thought it was obvious, but
apparently not. I went back to my journals for further evidence of influences on my motivation, beyond the one set of experiences with my conversation tutor. That tale was beginning to look increasingly peevish.

By draft 15, I was shifting away from a title that highlighted the vexed term ‘motivation’ and toward a concept of ‘ecological influences’ on my willingness to invest effort in self-study of Japanese. The paper now displayed several categories of influences, in particular, ones that affected my moods and energy, such as the weather, my health, and problems at work. Approximately two years later, after getting more feedback from some colleagues, I submitted another draft to TESOL Quarterly. More months passed and another set of reviews once again recommended that I revise and resubmit. Nearly ready to give up, I continued shifting away from the motivation literature and beefed up my discussion of ‘ecology of effort’ (as suggested by one of the reviewers). I also emphasized my belief that there was great value in ‘dabbling’ in an additional language. What counted, I felt, was a continued curiosity and interest in the language, and, above all, a sense of fun in what I was doing. My empathy for students who were forced to study English deepened greatly, and I came to respect those who chose not to study in an all-or-nothing manner.

Draft 20 of the Dabbler paper was finally accepted. I swore at the time that I would never write another article for publication, especially one about myself. It was just too dang hard.

Suggestions for crafting a self in published writing

I am sure that some reviewers still saw me as a failed language learner, and maybe some current readers do too: Just do it, they might think, and quit waffling and whining. Yet I made every effort to justify, honestly, the activity of dabbling in another language, not as evidence of failed language learning but of continued curiosity. Language study does not have to be all or nothing. I also continue to feel that I made the right decision to try to represent myself transparently as someone whose efforts to do something difficult were affected by many internal and external factors that do not get talked about in the motivation literature. I also managed to retain my personal writing style and so made a case for this kind of writing.

Novice writers might feel that it is difficult to front themselves openly like this in their writing, particularly if they hope to get published in a competitive and convention-bound academic community. But by not doing so, they risk misleading readers as to what happened, how they interpreted events in their lives and the lives of those they write about, and how their inquiry actually got done. They can begin by following the suggestions in guidebooks on writing (including the infamously conservative APA Manual, 2009), which recommend using active voice and first person to tell what you
did in a research project, rather than hiding behind the passive voice. They can recognize their own roles in their research projects and situate themselves as influential actors in their own writing. And, yes, they can write about themselves if their stories contain messages that relate to others in ways intended to move readers to think and act differently.

Some academics may continue to ask why we need to represent ourselves honestly in our writing, or, indeed, why we have to represent ourselves at all. In the first place, it’s not ‘scientific.’ In the second place, the dangers of narcissism and maudlin confession are too great. However, I think that narcissism and maudlin confession are not inherent to first person writing in scholarly work. As Richardson (1997b, p. 296) reminded us, “We are always present in our texts, no matter how we try to suppress ourselves.” Moreover, “[T]here is a kind of pleasure in writing yourself into the text” (Richardson, 1990, p. 61). Presenting a tale that goes beyond the self, that has a point beyond confession, and that connects with readers is what makes this kind of writing so hard, and so important.

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Chapter 23

Publishing from a dissertation
A book or articles?

Brian Paltridge

Introduction

It is, unfortunately, not easy to get a dissertation published as a book without making major revisions to it. The audiences for a book and for a dissertation are quite different and have to be considered when writing a book based on a dissertation. Converting a dissertation into research articles also requires substantial reworking of dissertation material. This often involves recontextualizing, reframing, reprioritizing, trimming, condensing, and even restructuring (Kwan, 2010). It is usually not as simple as taking a chapter from a dissertation and submitting it as an article to an academic journal. The article needs to stand-alone and be able to be read and of interest to a much wider readership than that of the dissertation. Revising a dissertation for publication, then, “demands careful selecting and rewriting, as well as the difficult task of figuring out the most important points in the dissertation in order of importance” (Nonmore, 2011, pp. 85–86). In this chapter, I describe my experience of overcoming these obstacles in getting publications from my doctoral dissertation, how I went about it, and the feedback I received on my work as I did this. I conclude by making suggestions for beginning academic authors wanting to rework their dissertation for publication.

Overcoming obstacles in publishing PhD research: A sample study

My PhD (Paltridge, 1993a) was an examination of research writing, drawing on the systemic functional view of genre (Hasan, 1989; Martin, 1984), frame semantics (Fillmore, 1985), and discourse and semantic relations (Crombie, 1985a; 1985b). I used Swales’s (1990) definition of genre as the starting point for my research and, on the basis of my analysis, proposed a framework for examining genres that drew on social and psychological aspects of genre production and recognition. The conclusions I reached were very tentative but did make some observations that had not been discussed
in the research literature, so I hoped I would be able to publish some of my findings.

The problem I was faced with was how to publish my dissertation so that it did more for me than just give me the academic qualification I needed to pursue an academic career. I considered both a book and journal articles to do this. Beth Luey’s (1995) *Handbook for Academic Authors, Third Edition* contains an excellent chapter on revising a dissertation for a book, which I read very carefully as I started to think about how I might do this with my dissertation. Luey outlines three possibilities for doing this: cosmetic cover-up, limited remodeling, and complete overhaul. The most successful of these, she argues, is the latter approach: complete overhaul. A publisher can easily see if only superficial changes have been made to a dissertation and it has not been rethought for the newer and broader audience of an academic book.

The main options for getting my work published, as I saw it, were journal articles, a book, and possibly a book chapter if I was able to find a call for proposals for book chapters where material from my dissertation might fit. In the end, to my great delight, I published five articles, a book chapter, and a book from my PhD. However, this was not an instant process, with five years spanning the first and final publication to come out of the PhD research. The dissertation needed considerable reworking, and each of the publications needed to be completely rewritten from their original format in the dissertation.

I published two key analyses from my dissertation separately as research articles in *System* (Paltridge, 1993b; 1995a) then published an article in the *Journal of Pragmatics*, which, in a much broader sense, drew them together (Paltridge, 1995b). I also published an article in *Applied Linguistics* (Paltridge, 1994) on an aspect of my analysis that I was sure my examiners would ask about (which in fact they didn’t). Both my *System* articles went through the review process fairly smoothly. The *Journal of Pragmatics* and *Applied Linguistics* articles both required much more work, however, before they were accepted for publication. The reviewers for the *Journal of Pragmatics* article were in disagreement with each other, with one finding my paper ‘interesting’ and the other saying there was nothing new in my paper. The editor said he found the comments in this latter review somewhat harsh but if that reviewer – who was a very experienced linguist – had not got the point I was making, then perhaps I hadn’t been sufficiently clear about what the contribution of my paper was. He asked me, therefore, to rework my paper taking account of both reviewers’ comments, stressing the ‘newness’ of my approach in the paper. The *Applied Linguistics* reviews, while generally positive, also asked for considerable reworking of my paper. These reviews each made similar points, mostly about the structure and focus of the article. The editor, in this case, left it to me to decide what the overall focus of the revised paper would be.
I also published two practical articles for teachers – in *Prospect* (Paltridge, 1995c) and *ELT Journal* (Paltridge, 1996) – which discussed how a number of observations I had made in my study could be taken up by language teachers. My book chapter (Paltridge, 1998) was the result of my replying to a call for proposals for chapters in a book to be published by TESOL titled *New Ways in English for Specific Purposes* (Master & Brinton, 1998). My chapter was also aimed at teachers and drew on an analysis I did in my dissertation of technical vocabulary, showing how teachers could use this kind of analysis in their classrooms. I purposely chose to submit the articles to the journals that were aimed at teachers as I was a language teacher myself and was wanting to establish myself not only as a researcher but also someone who knew what the implications were of my research for teachers. The book chapter was chosen for a similar reason. It wouldn’t have counted as a research output at most academic institutions as the book it was published in was basically a set of lesson plans for teachers. Nonetheless, it was another way of showing I understood the practical implications of my work. So what I did was take my dissertation and found stand-alone topics within it that I could publish in their own right and which were not dependent on the larger piece of work in order to understand them. It is important to point out, however, that I submitted the journal articles before I started on the book, as most journal publishers will allow the reuse of journal article material in a book. Book publishers will generally not agree to an author doing this the other way round.

Before I started on my book (Paltridge, 1997) and before I approached a publisher, I looked at book catalogues to see if there was a publisher where my book might fit with one of their areas of specialization. I chose a publisher (John Benjamins) who had a series which published books on similar topics to mine. I identified the series editor, found his contact details via the web, and sent a very general inquiry to him. The response was positive but not, of course, making any commitment to publishing the book. I then read the chapter in Luey’s (1995) book on revising a dissertation for a book and decided a complete overhaul was required. I decided the analyses I had presented in my PhD were not all ones that may be of interest to a wider readership so I reframed my text to make it much broader than it was in my dissertation. As a result, my text became more of an argument, putting forward a particular point of view rather than answering a particular research question as it had done earlier. I then sent my manuscript to the series editor. After some months, I got a reviewer’s report which started, “Actually, I would suggest publication of the work,” and then listed two pages of changes that needed to be made to the text for this to be possible. One of these was to remove a chapter in which I had outlined the key theory for my analysis with the reviewer saying, “Is it really necessary to present the systemic functional concept of genre in such detail? It reads much like a student’s summary paper on this concept of genre. The book would gain
a lot if this part were deleted.” The reviewer also said, “If one claims to present an overview of the main approaches to genre analysis, one should not reduce existing approaches to genre analysis to justify these approaches. There are major linguistic and semiotic traditions of genre analysis which are missing in the discussion.” The reviewer then outlined theoretical work on my topic which I wasn’t aware of, as it came from a different theoretical tradition from the one I had been working with at the time. This resulted in a new chapter which showed how my topic (genre) had been discussed in other theoretical traditions such as folklore studies, literary theory, linguistic anthropology, the ethnography of speaking, and the sociology of language – something that I hadn’t done in my dissertation. There were, of course, many other points in the review which I went through one by one as I revised my manuscript. In all, it took me three months to do this. I locked myself in my office and did the further reading and writing that was needed to write the new chapter. I then did the substantial reworking and revising of my manuscript that the reviewer had asked for. This included reframing parts of my study as well as reanalyzing some of my data. It required writing multiple drafts, and rewriting the drafts very many times.

To illustrate the changes I made to my PhD when it became a book, Table 23.1 shows the chapter titles for each of the two texts. As can be seen from this table, the book has fewer chapters and was much shorter than the dissertation. The dissertation had two theory and two analysis chapters, whereas the book had only one theory and one analysis chapter. Chapter 2 in the dissertation is the chapter that the reviewer told me to delete. Chapter 2 in the book is the chapter that was not in the dissertation and which I specially wrote for the book. Chapter 3 of the dissertation became a single paper (Paltridge, 1993b), part of Chapter 6 became a paper (Paltridge, 1995a), and the rest of Chapter 6 became another paper (Paltridge, 1995b). It was not possible, however, to just lift each paper from the chapters. In each case, I needed to write new and different contextualizing material and cite different background literature as each of the papers had a different theoretical and empirical focus. The two teacher-oriented papers (Paltridge, 1995c; 1996) were based on issues that were raised in Chapter 7. In these two cases, I had to cut back on the theoretical discussion that was in the dissertation as the focus of these papers was more practical than theoretical. At the same time, I had to extend what I was saying so that it would be of relevance to teachers. The book chapter (Paltridge, 1998) was based on an analysis I had presented in Chapter 6 of my dissertation. In this case, all of the theoretical discussion was removed, as the analysis was simply presented as an example of data that could be used in classroom teaching. The theoretical paper (Paltridge, 1994) was drawn from an analytical point I had raised in Chapter 2 of the dissertation. The paper required much more reading and theorizing of the point I was wishing to make than I had done in the dissertation. In this case, I was extending an issue I had only just touched on in my dissertation.
Table 23.1 Table of contents for the dissertation (Paltridge, 1993a) and the book (Paltridge, 1997)

<table>
<thead>
<tr>
<th>Dissertation</th>
<th>Book</th>
</tr>
</thead>
<tbody>
<tr>
<td>A Challenge to the Current Concept of Genre: Writing Up Research</td>
<td>Genre, Frames and Writing in Research Settings</td>
</tr>
</tbody>
</table>

Chapter 1: Introduction to the study: Genre and English for specific purposes

Chapter 2: Working with genre: A social semiotic perspective

Chapter 3: Working with genre: A systemic functional analysis

Chapter 4: Working with genre: A socio-psychological perspective

Chapter 5: An interim framework for genre analysis

Chapter 6: Working with genre: A socio-psychological analysis

Chapter 7: Summary, conclusions and implications for teaching and further research

Appendix

Bibliography

and making it the focus of a stand-alone paper. The appendix in the dissertation contained all of the analyses I had carried out, whereas the appendix in the book only contained the sample texts and just one of the analyses – the one I considered to be the most important.

**Implications for beginning academic authors**

**Choosing the right publisher**

There are many factors that need to be considered when submitting a book proposal to a publisher or an article to a peer-reviewed journal. Key amongst these is which publisher or journal you will send your work to. There are a number of ways of going about deciding on this. One way is to look at which publisher or journals well-regarded authors on your topic are publishing with. You can do this by looking at work by these authors that you have cited in your dissertation. State-of-the-art chapters or review
articles on the topic are also helpful for this. A search in Google Scholar to find highly cited articles on your topic and where they are published is another useful strategy. The key authors may, of course, publish across a range of publishers and in different journals, but this can be a good starting point for finding publishers and journals that might be interested in your work. It is also important to look at a range of journals to get an idea of the kinds of articles they publish. The notes to authors on the journal’s website also need to be examined carefully. These are usually very clear about what kind of article the journal publishes. In my experience, these have proved useful strategies and have resulted in my books and papers appearing in journals and with publishers where other people who research in my area are publishing.

Another strategy is to look at editorials in academic journals. These often give clues as to what the journal is looking for, what are considered ‘hot’ topics in the area, and, in general, what kinds of articles the journal publishes. The names of people on the journal’s editorial board are also worth looking at. If they are all (or mostly) people who work in your particular area, this is an indication that the journal might be worth considering. And there is also the matter of the fit between your paper and the journal. A lack of fit between an article and a journal is a very common reason for the rejection of an article without going out for review (a ‘desk’ rejection). It is also important to be aware of how long it takes to get an article accepted for publication. This often takes much longer than beginning authors expect.

**Publishing a book**

In order to publish a book, the first thing you need to do is write a proposal for the book. Publishers do not want to receive a copy of a dissertation with an inquiry asking if they would be interested in publishing it. This is the case with larger publishing houses as well as smaller publishers that specialize in research monographs in particular areas of study. You then need to choose a publisher to submit your book proposal to. One way to start on this is to look to see which publishing houses key authors in your field of research are publishing with. Next, a look at these publishers’ catalogues will give you a sense of whether the book would fit with the kinds of books they publish. It is a good idea to then identify the publishers’ commissioning editor in your particular area. This can usually be found on the publishers’ website. A brief inquiry can then be sent to this person to see if the topic of the proposed book is one they might be interested in. If you get a positive response, you then need to write the proposal and a sample chapter.

The publisher’s website very often has guidelines for writing a book proposal. Each of the issues listed in these guidelines needs to be addressed as thoroughly as possible. Once the proposal has been sent to the publisher, if they want to proceed with it, it will be sent to reviewers who will be asked to
comment on the proposal, addressing matters such as the aims and strengths of the book, the market for the book, the place of the book within the broader field, and suggestions for improving the book, as well as to make a recommendation on whether the publisher should offer a contract on the book. If, after this, the publisher decides the book is not one they wish to publish, this does not mean that another publisher will not be interested in it. You need to take on board the feedback you have been given and revise the proposal and sample chapter in line with this. You can then submit the proposal to another publisher, taking account of the readership the new publisher’s books are aimed at.

Publishing journal articles

Many PhD students, however, look to produce research articles from their dissertation rather than a book as the first step in disseminating their research. The process of getting articles from a dissertation, though, is not just a matter of cutting and pasting. You need to avoid what is sometimes called ‘salami publishing’ – that is, dividing the dissertation “into the thinnest possible slices and submitting each slice as a separate article” (Kitchin & Fuller, 2005, p. 36), or publishing the same work with only minor changes in different journals.

You need to remember, however, that only a small proportion of papers submitted to journals – especially highly ranked ones – are published. If the article does not succeed, you should not take this as a rejection of your work. Instead, you need to consider the reasons for the rejection and revise the paper in light of the feedback you have been given. Then you can submit your article to another journal. New writers are often disconcerted when they find they cannot seem to get it right the first time when writing for publication. What they do not realize is that published books and research articles have nearly always been through a number of drafts and rewritings, often a great many, before they finally appear in print (see Paltridge & Starfield, 2016 for further advice on getting published in academic journals).

Predatory publishers

Amongst all this, you need to be cautious if a publisher writes to you saying they would like to publish your dissertation as a book or an article (usually for a fee, although this might not be disclosed at this point). My students receive requests such as these regularly, and I tell them to ignore them. Often these publishers do not have a peer-review process and are not considered acceptable publishers from most institutions’ points of view. With books, as with journal publishers, the standing of the publisher and how academic institutions view that publisher are things you need to consider. Some ‘predator journals,’ unfortunately, take advantage of writers trying
to get published. On his website (scholarlyoa.com/individual-journals/), Jeffery Beall provides a list of possible predatory journals. He also has a list of publishers (scholarlyoa.com/publishers/) who may publish predatory journals. On another web page (scholarlyoa.com/2012/11/30/criteria-for-determining-predatory-open-access-publishers-2nd-edition/), Beall provides advice on how to identify a predatory journal:

- no academic information is provided about the editor and members of the editorial board
- the publisher’s owner is named as the editor of the journal
- the publisher does not reveal its physical location
- the journal falsely claims to be listed in legitimate indexing services.

**Conclusion**

Getting published, then, is never a straightforward task. This is especially the case for writers who are new to academic publishing. Nicola Johnson (2011), in her book *Publishing from Your PhD*, describes this process as “negotiating a crowded jungle.” As she argues, a jungle is always crowded and navigating the jungle can be difficult, even for people who have experience in doing this. She also adds that navigating a jungle is like entering unmarked territory about which, especially for beginning writers, much is unknown. The path through this jungle, further, is unique to every researcher and each journey through the jungle is different. Throughout all this, she says, writers need to keep their bearings and think about how they fit into the jungle and which paths are the ones they should take.

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Afterword
Strategies for getting the study published

Andrew D. Cohen

Introduction
The aim of this brief afterword is to reflect on the strategies that I have used in writing journal articles and book chapters over many years in applied linguistics. The following strategies may be of benefit to others as they have been of benefit to me. I particularly have in mind the publishing of empirical studies, such as those described in this volume, since this seems to be what many international journals are looking for, rather than think pieces.

Nine strategies for publishing your work

Strategy 1: Find topics that are highly motivating for you
A strategy that I have followed is only to write in areas where I am passionate about the topic. In such cases, I have high motivation to get my findings out into the literature. As a result, there is no possibility that I will wake up the next morning with second thoughts about my topic and the research findings. So the challenge is finding a comfortable niche for work – an area perhaps where others have yet to tread. This not only makes the study more motivating to conduct but also is likely to make the work more attractive to others as well. In addition, if the work has its own special niche, it may be more likely to become seminal in the field. So how do you find a niche? One way is to do a careful review of literature to see where the gaps are. Another way is to write to experts in the field, asking them what has not been done. I recently mobilized this kind of effort with regard to the field of language learner strategies and, with the assistance of a colleague, wrote it up for the TESOL Quarterly (Cohen & Griffiths, 2015). I sent out requests to 23 language learner strategy (LLS) experts from around the world to send me their wish list of possible future research in this field of work. Their feedback spawned 26 research thrusts in six categories. I should think that the resulting paper will be useful to those in search of topics for LLS research.
Strategy 2: Turn to colleagues for support

A tack I have usually taken before attempting to publish something I have written is to check with key colleagues to see what they are doing on the topic. I ask myself, “Who are the experts on this topic?” and I check with them to make sure that I am on the right track and not overlooking some crucial theoretical underpinning, design feature, or seminal study on this topic before I forge ahead with my own work. It flatters them and has served me well over the years. Reinventing the wheel is counterproductive and time consuming. Then once I have written the piece, I may run it by some of these experts to see what they think. While there is the view, “I better not share my ideas because someone else could steal them,” and there are undoubtedly cases of plagiarism, I would like to think that sharing ideas and getting feedback are crucial elements in the academic experience. It may be that the very person who sees an early version of your work will be one of the reviewers of your piece when it is submitted. Consequently, they may look more kindly on it than they would have otherwise.

Strategy 3: When feasible, collaborate

With regard to collaboration, my own personal view is that two or more heads can certainly be better than one. In fact, sometimes those collaborators could be undergraduates. They often bring to research a fresh pair of eyes, and if properly coached as to the aims of the study, their insights may be invaluable at the design stage and in the conducting of the study. Whereas they may not actually write the paper, they may have invaluable input at the stage of data analysis and with respect to any limitations found in the study. One more recent study of mine where two undergraduates contributed greatly was a study of the impact of a Spanish grammar strategies website. The study involved tracking the use of this website by 15 undergraduate learners of Spanish over a six- to eight-week period (see Cohen, Pinilla-Herrera, Thompson, & Witzig, 2011). Thompson and Witzig both made valuable suggestions while piloting the strategy tracking instrument and then having these two undergraduates collect the data from their peers also lent credibility to the study.

Strategy 4: Have authorship reflect merit rather than seniority

A further strategy is being careful to place myself in authorship according to the amount of work that I put into the given study, not on the basis of seniority (see Bommarito et al. in this volume). Ironically, as my career has progressed I have been contacted by more junior scholars who would like nothing better than to have me take first authorship of some study that they
did, largely in the hopes of getting it published. Just as I would not think of taking lead authorship for a study in which my role was limited, I certainly do not wish to take on any form of joint authorship for a study that I had no role in designing or conducting, and simply coming on board at the write-up stage. That does not work for me at all. If I have not planned and executed the study myself or with colleagues, I do not want to be involved in its write-up. Thus early career researchers who are interested in collaborative work with more senior scholars might be advised to enlist their support and participation from the early stages of the project.

**Strategy 5: Publish widely**

When asked to publish for any journal, I have tended to agree. I have never considered any venue too small or insignificant. I realize there are pressures mounted by senior colleagues which reflect the view that only publications in premier journals ‘count.’ I have blatantly ignored that recommendation, and I think my career has benefited from this approach. An author can learn a lot from being published, wherever. Each publication experience provides an opportunity to learn how to write more effectively and how to disseminate your work.

**Strategy 6: Make each new publication in some ways unique**

Another approach I have taken is that while I may recycle similar material in different papers, I have made it a point not to publish the exact same material in two different venues. From time to time, I might use some of the same material in several different publications, but I always make an effort to include new material in any new publication. It forces me to be creative and forward thinking rather than being mired in my previous thoughts and previous material.

**Strategy 7: Circulate the draft of your paper before submitting it**

Yet another strategy that I have used over the years is to circulate my papers to colleagues so they would know what I am doing. I used to keep a circulation list of colleagues to send my papers to. This policy has had a beneficial impact. Not only has it meant that my work has gotten cited more because others have been more aware of what I am doing research on, but it also has also meant that I have been more likely to be invited to give presentations at meetings and to write articles and chapters for their edited publications. In the early days, it meant sending out hard-copy stencil and mimeo copies of my work. Then it meant attaching my papers electronically to email
messages. Nowadays, it means posting my papers on my university website and being sure to abide by any copyright restrictions imposed by certain publishers. Some journals do not permit the posting of articles at all (Modern Language Journal), while others (e.g., Language Testing) ask you to wait a year before posting them.

**Strategy 8: Be open to negative feedback**

Another strategy is that it has made good sense for me to incorporate whatever negative feedback I have gotten on my papers into improving the quality of the submissions. It has usually been enough to get the reviewers to accept the submission. There has been little value for me in taking an adversarial position. As a reviewer of submissions over the years, I have encountered more than one feisty author who is rather pugnacious and defensive in the face of negative feedback. This tack usually does not work to the prospective author’s advantage, especially when the journal sends me a revised version and I notice that the researcher has not addressed one or more of the issues that I had raised in my critique. A better approach is to be open to all feedback, taking the position that responding to each and every criticism is of potential value for improving the quality of the paper. In addition, since it is often a small community of paper reviewers for journals, the very same reviewers may be approached if the paper is submitted to another venue! I would want a reviewer to see a newly revised version if possible so that he or she ideally looks more favourably on the submission.

**Strategy 9: Make your work accessible to others**

I have gotten feedback from many scholars, especially those in the third world, that they very much appreciate this willingness to make my work accessible to others through my own personal website. Additional venues for sharing your published work include websites such as ResearchGate.net and Academia.edu. Especially for those publications appearing in local university bulletins, working papers, and other venues that can be a challenge to access, it can be a real service to make them available in more public ways. Someone may read this publication and contact you with a request that it be submitted for publication in a collection of articles that they are currently planning.

**Conclusions**

It has been my aim in this brief romp through the issues of getting published to share some strategies that have served me well over the more than 45 years that I have been publishing in the field of applied linguistics. Many of my keenest insights have come from reviewing the submissions of my
colleagues to a host of journals. Spending many hours plodding through papers that are weak in the ways spelled out in this chapter has encouraged me to put in writing what I personally consider to be some effective strategies for getting published.

References


Index

academic literacies approach 12, 192–9
academic socialization approach 192–3
action research 20, 62–3, 134, 143, 146–8, 154
adapted appraisal framework 38
adult refugees 114–23
affective strategies 31
age 94, 103–11, 159, 214, 218, 219, 254–5
anonymity 65, 67, 110–11, 130, 154
audio recordings 4, 18, 23, 67–8, 77–8, 119
authenticity 21, 25
authorship 254–5
autoethnographic/autobiographical work 236
‘availability’ sampling techniques 150
behaviourism 27, 34
bias threats 168
case studies 5, 7, 41, 77, 115, 135, 168, 172, 192, 195, 203; embedded 43; multiple 76
children participants 10, 11, 19–20, 103–11, 146–55
Chinese students 48, 50–3, 227–8
claims 13, 55, 151, 174, 186, 198–9, 225–6, 228, 247, 251
classroom-based research 4, 11–12, 73–8, 103–12, 146–55, 192–9, 237–8
coding schemes 184–5
cognitive psychology 161
cognitive science 31
cognitive strategies 28, 29
cognitive theory 31
cohort studies 63, 65, 70, 84, 162
collaborative research 47–56
Common European Framework of Reference (CEFR) 97–8
communicative language ability model 161
conceptual frameworks 226
confidentiality 118–19, 122, 130–1, 141, 154
constructionist theory 8, 38, 44
constructionist research 172, 178
constructivism 151, 153
content analysis 184, 185, 229
contextual constraints 9, 61–71
contextual intention 186–7
control groups 104–5, 149
controversial findings 12, 203–12
cconvenience sampling techniques 150
conversations 67–8, 77
corpus 65, 82–3, 173, 177, 196
corpus linguistics 82
corpus linguistics methodologies 81
critical thinking 37, 40–3, 208
criticism 12–13, 225–34
cross-sectional research 160
cultural frames 43–4, 52
data: coding 54; interpretation 43–4, 55, 62, 226; preparation 164; presentation 130; sources 125; triangulation 152
data analysis: audio recordings 4, 18, 23, 67–8, 77–8, 119; culture and organizational embeddedness considerations 44; focus group discussions 68–9; longitudinal quantitative designs 11, 159–70; narrative interviews 11, 172–80; negotiating relationship between
Index 259

theory and practice in fields of literacy and language 12, 192–9; qualitative data analysis 11–12, 182–90, 226, 231–3; recursive iterations of 62; repurposing data 53–4; structural equation modeling analysis 162–3; survey responses 65
data collection: adjusting to contextual constraints 9, 61–71; classroom-based research 149, 153, 154, 155; collaborative research projects 50, 53–4; dealing with low response rates in quantitative studies 9, 81–9; delay between dissemination and 33; ethnographic approach to 72; focus group discussions 68–9, 70; framing data in context of place for 43; heritage language program research study 77; limitations in research design revealed by 61; longitudinal research 163–4, 169; low response rates 9, 81–9; participant attrition 9, 72–9; people with illnesses and disabilities 126–7; qualitative data collection 62; quantitative data collection 9, 33, 62, 91–9, 150; role of researcher on 151; value of early position in 30–1
deaf sign language users research study 10–11, 134–43
debriefing 122
Discourses of Writing framework, 38 dissertations 244–51
embedded: embedded case studies 43; “embedded mixing strategy” 64; embeddedness 37; organizational embeddedness 44; structural embeddedness 39, 43; emotionality 175–6, 178–9
essentialism 21
ethics: blurred line between teacher/researcher 150–1; classroom-based research 153; confidentiality issues 118–19, 122, 130–1, 141, 154; informed consent issues 65, 69, 108, 118, 122, 131; issues of rapport 23–4; participants’ purposes versus the researchers’ purposes 127–8; protecting participant identity in published research 120–2, 130–1, 141, 154; researching children 108–9; researching romantic/sexual relationships between teachers/students 204–6; of studying down 19–20; towards an ethical research relationship in working with people with illnesses 131–2
ethnography: 10, 17, 22, 24, 72–7, 203; auto-ethnography 235–6, 239; ethnographic methods 72–3, 159, 192, 194–5, 206–7
experimental research 28, 35, 81, 86, 106, 149, 162, 168, 218

feedback 10, 50–1, 64, 84, 110–11, 256
field notes 33, 120, 125, 154, 203
field work 10, 114–17, 122–3, 149, 176
flexibility 33, 69, 86, 128, 189
focus groups 66–8, 70, 140–1, 151–2
frames/frameworks 28, 31, 38, 43–4, 154, 208
funding 86, 88, 139–40
generalizability 154, 225
genre 63–4, 67–9, 193, 236, 244–8; analysis 247; knowledge 62, 66; studies 44
globally neutral approach 40
grounding/grounded (in theory) 182–4, 186, 188–90, 227, 229, 231

Hawthorne effect 35, 150
heritage language 73–8

identity: -focused research 25, 38, 48, 53, 72–4, 124, 125, 136–7, 172, 177, 194, 203, 208–10; markers 233; participant 120–2, 130–1; writer 38
inferential statistics 150
informed consent 65, 69, 118, 122, 131
insider research 21–2, 136, 150–1, 154
interaction 173–4, 176–7
interactional analysis 177
inter-rater reliability 152
intersectionality 136–8

interviews: adult refugees 119, 121; classroom-based research 151; focus groups versus 140–1; follow-up interviews 87, 128–9; interview guides 33; longitudinal research 169; narrative interviewing 11, 172–80; as neutral “research instrument” 174; open-ended narrative
interviews 125; replaced with focus group discussions 69; researching romantic/sexual relationships between teachers/students 208; semi-structured interviews 23, 63–5, 204; as “social and collaborative practice” 174; student interviews 38; teacher interviews 38; on workplace interactions 23–5

Japan 17–18, 23, 42, 85, 126, 127, 203–10, 235–41
Japanese students 28, 31, 38–44, 148–52
journal articles 4, 33, 209–11, 250

Korea 17–18, 23, 42

L1 instruction 92–7, 103–5, 183
L2 instruction 27, 92–7, 103–5, 159, 183, 214–22
L3 instruction 92–7
language education research 5
Language Enrichment Program, 74–5
language frames 43
language learner/learning strategies 28–33, 229–30
Likert-type scales 64
longitudinal research 72–3, 159–70, 216–18
low response rates 9, 65–6, 69, 81–9

memory strategies 31
metacognitive strategies 28
methodological frameworks 168
mixed-methods designs 33, 62, 69, 138, 149
mnemonic strategies 29
multidimensional studies, 34
multilingualism in quantitative research 9, 91–9
multistage studies 34

narrative 124–6
needs analysis 146, 149, 154
negative feedback 256

objectivity 8, 28, 37, 39–40, 92
observations: observational charts 33; observational data 38; observational research 35; participant 23, 47, 115; in research 4, 6, 8, 18, 37, 42, 48, 50, 55, 72–4, 77, 93, 103, 147–8, 154, 169, 203, 219, 237, 239
one-on-one testing 86
online surveys 63, 65
Opportunities With Language Simulator (OWLS) 184–90
originality 182–90

participants: attrition 69, 72–9, 109–10, 164–5, 169; criteria 73–4; identity 120–2, 130–1; observation 23, 47, 115; recruitment 9, 23, 52–3, 65–6, 84–7, 91–2, 108–9, 141, 154; sample 63–4
participatory action research 18, 20, 129, 131
participatory development 20
people with illnesses and disabilities 10, 124–32
pilot discussion 152
piolated measures 33
politically sensitive issues 12, 214–22
positivism 8, 28, 44
practitioner-informed knowledge base 147
pragmatic validation 152
principal investigators 55
privilege 24–5, 136–8
proficiency tests 95, 97–8
published research 4–6, 12, 33, 55–6, 209–11, 235–41, 253–7

qualitative research: collaborative research project 47–56; criticism 225–34; ethical dilemmas in 129; issues of rapport for 23–4; longitudinal study 169; with multilingual subjects 99; of non-native language influence 93; qualitative data analysis 11, 182–90; qualitative data collection 62; research design and 33; research dilemmas in 179; situated qualitative research 8, 37, 41–2; for teacher-researchers 155
quantitative research: low response rates 81–9; multilingualism 91–9; quantitative data collection 33, 62, 150
questionnaires 28–9, 33, 51, 64, 65, 96, 97, 115, 119, 140, 147, 152–3, 161, 163–6, 169, 183
rapport 8, 23–4
Rasch Item Response Theory 164
reflective accounts 125
reliability 150, 152–3
replication studies 82
representation 174–5, 177–8
research: goals 69; questions 63, 69;
topic selection 8, 17–25, 253
research assistants 105–6
research design: action research 62–3,
134, 143, 146–8; adjusting to
textual constraints 9, 61–71;
classroom-based research 4, 11–12,
73–8, 103–12, 146–55, 192–9,
237–8; developing research tools for
182–3; iterative process in 205–6;
methods literature 3–6; mixed-
methods designs 33, 62, 69, 138,
149; multidimensional/multistage
studies 34; necessity of flexibility
in 33, 69; research sequence 62–3;
sequential/repeated design 162;
theoretical shifts in 27–35
research dilemmas: frameworks 28;
managing researcher dilemmas in
narrative interview data and analysis
11, 172–80; researching people
with illnesses and disabilities 126–8; research topic selection 17–25; target
population selection 17–19
researchers: indigenous 21; objectivity
8, 28, 37, 39–40, 92; positionality 8,
37–44, 125–6, 134, 137; stance 151;
teacher 146–55
research planning: collaborative
research projects 8, 47–56;
longitudinal research 168–9; low
response rates and 87–8; overcoming
researcher positionality and
researcher objectivity 37–44; research
topic selection 8, 17–25; responding
to theoretical shifts in research design
27–35
research reporting: controversial
findings 12, 203–12; criticism
12–13, 225–34; politically sensitive
issues 12, 214–22; publishing from
dissertation 13, 244–51; representing
self in published research 13, 235–41
responsible validation 152
responsibility 21, 115, 118, 136–8;
moral 128, 131–2
sample frame 85, 87, 106–7, 154
sample size 82, 88, 150, 163
self-appraisals 64
self-reporting 29, 186
self-representation 12, 235–41
seniority 254–5
sequential/repeated design 162
situated participatory processes 194–5
situated qualitative research 8, 37,
41–2
sleeper effects 169
social constructivist approach 38, 44
social strategies 31
sociocultural perspectives 124, 172,
185, 230
sociolinguistic research 72, 203
specialist frames 43–4
state strategy use 161, 163, 166–7,
168–9
statistical analysis 162, 165, 168–9
statistical models 218
stimulated recall task 28–9
story forms 129–31
strategic behaviour 185–8
strategic combinations 184–7
strategic competence 161–2
strategic self-regulation (S2R) model 32
Strategy Inventory of Language
Learning (SILL) 28, 29
strategy research 29, 32
structural equation modeling analysis
162–3, 165
study skills 192–3
subject selection bias 9, 91–3
surveys 63–6, 81, 87, 139, 140, 141, 149
Systemic Functional Linguistics 38
target population selection 8, 17–19
teacher research 146–55
tested scales 33
theoretical frameworks 27–8, 31–3,
168, 183, 231
theoretical shifts 8, 27–35
timelines 9
timing 84, 88–9, 143
trait strategy use 161, 163, 166–7,
168–9
transcription 67, 82, 121, 174
transferability 155, 187
United Kingdom 40, 42, 88, 141, 142,
183, 227
validity 150, 152–3, 168, 226
workload management 149–50, 152
writer identity 38