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15

CONTROVERSIES IN MIXED METHODS RESEARCH

John W. Creswell

Mixed methods has emerged in the last few years as a research approach popular in many disciplines and countries, and supported through diverse funding agencies. With such growth, it is not surprising that critical commentaries have surfaced through papers presented at conferences and in published journal articles. These critics have come from both within (e.g., Greene, 2008; Morse, 2005; Creswell, Plano Clark, & Garrett, 2008) and outside (Denzin & Lincoln, 2005; Howe, 2004) the mixed methods community. Although concerns have mounted, they have been largely ignored by social scientists and the mixed methods community.

This chapter gives voice and focus to these controversies. I discuss 11 far-ranging controversies from basic concerns about defining and describing mixed methods, to philosophical debates, and on into the procedures for conducting a study.

For each controversy, I present critical questions, diverse stances, and lingering questions. At the end of this chapter, I reflect on the implications of these controversies. I hope this discussion will help mixed methods researchers, students, and policy makers appreciate the still-unanswered questions, view the multiple perspectives that have emerged, and reflect on new commitments that the mixed methods field needs to make. For qualitative researchers, I hope that this reflection will encourage the continued discussion of the strong vital role that qualitative research has and continues to play in mixed methods research.

The thoughts to follow will reflect my own writings of the last 20 years and will include, at times, a self-reflective critique. My methodological development consisted of formal training as a postpositivist in the 1970s, self-education as a constructivist through teaching qualitative courses in the 1980s, and advocacy for mixed methods through my writings and teachings from the 1990s to the present. As one spokesperson for mixed methods, many controversies have come to my attention through scholarly papers presented at conferences, articles published in the *Journal of Mixed Methods Research* (JMMR) while I served as founding coeditor for the last five years, and papers sent to me by authors who wanted me to keep abreast of emerging issues. As I look across these diverse materials, I hope to foster the ongoing conversation about the controversies and the many possible answers that scholars have offered to them.

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Some of the controversies that I will present figured prominently in a discussion in March 2009. I was attending and presenting at the University of Aberdeen in Scotland (Creswell, 2009d) at the Economic & Social Research Council (ESRC) Seminar Series sponsored by the Health Services Research Unit at the University of Aberdeen. I had finished my overview of mixed methods research to a gathering of 50 scholars primarily from the health sciences. They had assembled in historic Elphinstone Hall, an ancient venue with a high, vaulted, hammer-beam roof, banners hanging from the rafters, and oak-paneled walls lined with pictures of distinguished scholars dating back centuries. Much to my surprise, the conference organizer suddenly asked small groups to form and record their questions about both the advantages and the challenges of using mixed methods research. Not wanting to miss a key opportunity to capture their challenges and critical thoughts, I hastily began taking notes. They spoke about claims being made about the value of mixed methods research ("Is mixed methods seen as the answer to everything?" "Are there undue expectations raised by mixed methods that cannot be fulfilled?"), about philosophical and theoretical issues ("Is there opposition to mixed methods from those who hold strong worldview positions?" "Does a dominant paradigm prevail in

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mixed methods?" "Is qualitative research working on an even playing field with quantitative in mixed methods?"), and about the procedures and processes of research ("Is there a good fit between the research question and mixed methods?" "Do researchers have expertise and competence in both areas?").

The irony of "new" voices of concern about mixed methods arising in the "old," historic setting of Elphinstone Hall did not escape my attention. But, in retrospect, hearing the issues was not surprising. Concerns have been voiced in recent respected journal articles (Giddings, 2006; Howe, 2004), in the third edition of this handbook (Denzin & Lincoln, 2005), in conference presentations (Holmes, 2006), and in articles published in the *Journal of Mixed Methods Research*. In 2006, I had presented my views about unresolved issues in a journal article on the role of qualitative research in mixed methods (Creswell, Shope, Plano Clark, & Green, 2006), and at a panel presentation made at the 2007 International Qualitative Inquiry Congress (Creswell, 2007). In light of these discussions, it is timely to address these controversies. In this chapter, I address 11 controversies and raise several questions, as outlined in Table 15.1. The controversies, as a group, reflect what Kuhn (1970) said years ago about the transition period in research:

> The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy, and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. (p. 91)

Table 15.1 Eleven Key Controversies and Questions Being Raised in Mixed Methods Research

Controversies	*Questions Being Raised*
1. The changing and expanding definitions of mixed methods research	What is mixed methods research? How should it be defined? What shifts are being seen in its definition?
2. The questionable use of qualitative and quantitative descriptors	Are the terms "qualitative" and "quantitative" useful descriptors? What inferences are made when these terms are used? Is there a binary distinction being made that does not hold in practice?
3. Is mixed methods a "new" approach to research?	When did the conceptualization of mixed methods begin? Does mixed methods predate the period often associated with its beginning? What initiatives began prior to the late 1980s?
4. What drives the interest in mixed methods?	How has interest grown in mixed methods? What is the role of funding agencies in its development?
5. Is the paradigm debate still being discussed?	Can paradigms be mixed? What stances on paradigm use in mixed methods have developed? Should the paradigm for mixed methods be based on scholarly communities?
6. Does mixed methods privilege postpositivism?	In the privileging of postpositivism in mixed methods, does it marginalize qualitative, interpretive approaches and relegate them to secondary status?
7. Is there a fixed discourse in mixed methods?	Who controls the discourse about mixed methods? Is mixed methods nearing a "metanarrative?"
8. Should mixed methods adopt a bilingual language for its terms?	What is the language of mixed methods research? Should the language be bilingual or reflect quantitative and qualitative terms?
9. Are there too many confusing design possibilities for mixed methods procedures?	What designs should mixed methods researchers use? Are the present designs complex enough to reflect practice? Should entirely new ways of thinking about designs be adopted?
10. Is mixed methods research misappropriating designs and procedures from other approaches to research?	Are the claims of mixed methods overstated (because of misappropriation of other approaches to research)? Can mixed methods be seen as an approach lodged within a larger framework (e.g., ethnography)?
11. What value is added by mixed methods beyond the value gained through quantitative or qualitative research?	Does mixed methods provide a better understanding of a research problem than either quantitative or qualitative research alone? How can the value of mixed methods research be substantiated through scholarly inquiry?

Changing and Expanding Definitions

Heading the list of controversies would certainly be the fundamental question: What is mixed methods research? How should it be defined? To answer these questions requires a brief historical review of shifts in the definition of mixed methods over the years. For example, an early definition of mixed methods came from writers in the field of evaluation, Greene, Caracelli, and Graham (1989). They emphasized the mixing of *methods* and the disentanglement of methods and paradigms when they said,

> In this study, we defined mixed-method designs as those that include at least one quantitative method (designed to collect numbers) and one qualitative method (designed to collect words), where neither type of method is inherently linked to any particular inquiry paradigm. (p. 256)

Ten years later, the definition had shifted from mixing two methods to mixing in all phases of the research process, and mixed methods was being seen as a *methodology* (Tashakkori & Teddlie, 1998). Included within this process would be mixing from philosophical (i.e., worldview) positions, to final inferences, and to the interpretations of results. Thus, Tashakkori and Teddlie (1998) defined mixed methods as the combination of "qualitative and quantitative approaches in the methodology of a study" (p. ix). These authors reinforced this methodological orientation in their preface to the *Handbook of Mixed Methods in Social & Behavioral Research* by writing, "mixed methods research has evolved to the point where it is a separate methodological orientation with its own worldview, vocabulary, and techniques" (Tashakkori & Teddlie, 2003, p. x).

A few years later, when Plano Clark and I (Creswell & Plano Clark, 2007) wrote a definition for mixed methods into our introductory book, we blended *both* a methods and a methodological orientation along with a central assumption being made with this type of research. We said,

> Mixed methods research is a research design with philosophical assumptions as well as methods of inquiry. As a methodology, it involves philosophical assumptions that guide the direction of the collection and analysis and the mixture of qualitative and quantitative approaches in many phases of the research process. As a method, it focuses on collecting, analyzing, and mixing both quantitative and qualitative data in a single study or series of studies. Its central premise is that the use of quantitative and qualitative approaches, in combination, provides a better understanding of research problems than either approach alone. (p. 5)

This definition was patterned on describing an approach using multiple meanings, such as found in Stake's (1995) definition of a case study. Our definition of mixed methods had both a philosophy and a method orientation, and it conveyed components of the *core characteristics* of mixed methods that I advance today in workshops and presentations (e.g., see Creswell, 2009a). In mixed methods, the researcher

- collects and analyzes persuasively and rigorously both qualitative and quantitative data (based on research questions);
- mixes (or integrates or links) the two forms of data concurrently by combining them (or merging them), or sequentially by having one build on the other, and in a way that gives priority to one or to both;
- uses these procedures in a single study or in multiple phases of a program of study;
- frames these procedures within philosophical worldviews and a theoretical lens; and
- combines the procedures into specific research designs that direct the plan for conducting the study.

These core characteristics have provided some common features for describing mixed methods research. They evolved from many years of reviewing mixed methods articles and determining how researchers use both qualitative and quantitative methods in their studies.

I am not alone in proposing some common features. In a highly cited JMMR article, Johnson, Onwuegbuzie, and Turner (2007) suggested a composite definition for mixed methods based on 19 definitions provided by 21 highly published mixed methods researchers. After sharing these definitions, they noted the variations in definitions, from what was being mixed (e.g., methods, methodologies, or types of research), the place in the research process in which mixing occurred (e.g., data collection, data analysis), the scope of the mixing (e.g., from data to worldviews), the purpose or rationale for mixing (e.g., breadth, corroboration), and the elements driving the research (e.g., bottom-up, top-down, the core component). Incorporating these diverse perspectives, the authors end with a composite definition:

> Mixed methods research is the type of research in which a researcher or team of researchers combines elements of qualitative and quantitative research approaches (e.g., use of qualitative and quantitative viewpoints, data collection, analysis, inference techniques) for the purposes of breadth and depth of understanding and corroboration. (p. 123)

In this definition, the authors do not view mixed methods simply as methods, but more as a methodology that spans from viewpoints to inferences. They do not view mixed methods as only data collection, but rather as the more general combination of qualitative and quantitative research. They incorporate diverse viewpoints, but do not specifically mention paradigms (as in the Greene et al., 1989, definition) or philosophy (as in the

Creswell & Plano Clark, 2007, definition). Their purposes for mixed methods—breadth and depth of understanding and corroboration—do not speak to how the research question may suggest mixed methods rather than force-fitting a line of inquiry into either a quantitative or qualitative approach. Perhaps most important, they suggest that there is a common definition that should be used.

Another definition has been advanced by Greene (2007), who stated that mixed methods was an orientation toward looking at the social world

> that actively invites us to participate in dialogue about multiple ways of seeing and hearing, multiple ways of making sense of the social world, and multiple standpoints on what is important and to be valued and cherished. (p. 20)

This definition has moved mixed methods into an entirely new realm of conceptualization, and perhaps a useful one. Defining mixed methods as "multiple ways of seeing" opens up broad applications beyond using it as only a research method. It can be used, for example, as an approach to think about designing documentaries (Creswell & McCoy, in press), or a means for "seeing" participatory approaches to HIV-infected populations in the Eastern Cape of South Africa (Olivier, de Lange, Creswell, & Wood, 2009). Lately, I have begun my workshops on mixed methods by indicating that we have many instances of mixed methods in our social world. I start with Al Gore's film-documentary, *An Inconvenient Truth*, about global warming and Gore's combined use of mixed method-like statistical trends and personal stories (David, Bender, Burns, & Guggenheim, 2006). Defining mixed methods as a way of seeing opens up applications for it in many aspects of social life.

However, I still have unresolved concerns after reviewing these diverse definitions. Do we need a common definition or common set of core characteristics? Will such common features limit what we see as mixed methods? Do we need multiple definitions? For those individuals new to mixed methods, do they need a commonly accepted definition to convey the purpose of their research and to convince others of the legitimacy of their approach?

The Questionable Use of Qualitative and Quantitative Descriptors

Researchers talk about mixed methods using descriptors such as "qualitative" and "quantitative." The use of statistics and stories in Gore's film reinforces a binary distinction between qualitative and quantitative research. Are the terms "qualitative" and "quantitative" useful descriptors to use? What are the inferences being made when these terms are used? This controversy has brought forward one group of writers who have found the terms "qualitative" and "quantitative" intermingled with designs and paradigms, rather than referring to methods of data collection and analysis. It also has brought forward another group of writers who feel that the use of these terms fosters an unacceptable binary or dichotomy that minimizes the diversity in methods.

Giddings (2006) felt that the terms "qualitative" and "quantitative" became normative descriptors for research paradigms in the 1970s and 1980s, and that the term "qualitative" gave nonpositivist researchers "a place to stand" (p. 199). When writers have used the term "qualitative paradigm," it has often been in the context of the qualitative-quantitative debates in evaluation and the social sciences (Greene, 2007). Greene pointed out that it was helpful to separate the research methods of "qualitative" and "quantitative" from broader philosophical issues, and to refrain from intermingling methods and philosophy. Another intermingling occurs at the design level. Vogt (2008) took the strong position: "To think in terms of quantitative and qualitative *designs* is a category mistake" (p. 1, emphasis added). He felt that all research designs—such as surveys, document analysis, experiments, and quasi-experiments—could accommodate data coded as numbers and words.

The use of "qualitative" and "quantitative" has been further discouraged because it creates a binary distinction that does not hold in practice. Often writers equate "qualitative" to text data and "quantitative" to numbers data. In a recent *JMMR* article, Sandelowski, Voils, and Knafl (2009) countered this binary thinking by pointing out that counting often involved qualitative judgments, and that numbers often related to context. Further, qualitative data are sometimes transformed in data analysis into categorical data, and a binary configuration overlooked both within-group (e.g., qualitative) and between-group similarities (e.g., qualitative and quantitative). Resonating with this thought, Giddings (2006) stated that binary positioning made methodological diversity invisible.

Adding confusion to the meaning of "qualitative" and "quantitative" have been those who felt that mixed methods should mean collecting mono-methods—*multiple* qualitative sources of data or quantitative sources of data (Shank, 2007; Vogt, 2008) instead of collecting *both* qualitative and quantitative data (mixed methods). Some writers have been clear that multiple sources of one kind of data (i.e., qualitative or quantitative data) should be called "multiple methods" (Morse & Niehaus, 2009, Appendix 1), not mixed methods. Again, regardless as to how mixed methods is viewed, both perspectives rely on a normative, binary distinction between "qualitative" and quantitative" to reinforce their positions.

A strong case can be made that "qualitative" and "quantitative" should refer to methods. A useful diagram is advanced by Crotty (1998), who provided a conceptual framework for sorting out these layers of research into epistemology, theoretical perspectives (e.g., feminist theory), methodology, and methods. But to throw out the terms "quantitative" and "qualitative" seems to disrupt a long-established pattern of communication that has been used in the social, behavior, and health sciences. Until we have replacement terms, a means of discourse across fields is helpful, but we need to be careful how we use the terms. On the issue of the binary distinction, writers in the mixed methods field have tended to dismiss the dichotomy in favor of a continuum for presenting qualitative and quantitative differences (Creswell, 2008; Tashakkori & Teddlie, 2003). Writers in mixed methods are also careful to distinguish "multi-method studies" in which multiple types of *qualitative* or *quantitative* data are collected (see Creswell & Plano Clark, 2007) from "mixed methods studies" that incorporate collecting *both* qualitative and quantitative data. In the health sciences, the term "multimethod" is typically used to convey studies in which both forms of data are gathered (e.g., see Stange, Crabtree, & Miller, 2006), although in a study of National Institutes of Health–funded projects, Plano Clark (2009) found that "multimethod" meant multiple methods of quantitative *or* qualitative data 64% of the time, and "mixed methods" 36% of the time.

In light of these discussions about intermingling and the binary distinction, should we refrain from using the terms "qualitative" and "quantitative?" Why do mixed methods writers not clearly distinguish among methods, designs, and paradigms? Should mixed methods involve multiple qualitative or quantitative methods or some combine of both?

The New Versus the Old

Historically, researchers have used both forms of methods in these studies. This leads to another controversy: Is mixed methods a "new" approach or is it simply pouring new ideas into old packaging? Emphasizing the "new," recent writers have called mixed methods the third methodological "movement" (following quantitative and qualitative) (Tashakkori & Teddlie, 2003, p. 5), the "third research paradigm" (Johnson & Onwuegbuzie, 2004, p. 15), and "a new star in the social science sky" (Mayring, 2007, p. 1). Claims such as these have left some critics to wonder "exactly what the new mixed methods movement is claiming. The major proponents insist that what they developed is a new way of doing research" (Holmes, 2006, p. 2).

I often date the beginnings of mixed methods back to the late 1980s and early 1990s with the coming together of several publications all focused on describing and defining what is now known as mixed methods. These writers worked independently and they came from sociology in the United States (Brewer & Hunter, 1989) and in the United Kingdom (Fielding & Fielding, 1986), from evaluation in the United States (Greene et al., 1989), from management in the United Kingdom (Bryman, 1988), from nursing in Canada (Morse, 1991), and from education in the United States (Creswell, 1994). A critical mass of writings came together within a short space of time, and all of these individuals were writing books, chapters, and articles on an approach to research that moved from simply using quantitative and qualitative approaches as distinct, separate strands in a study to research that actually linked or combined them. At this time, qualitative inquiry had become largely accepted as a legitimate methodology in the social sciences and was moving into the "blurred genres" stage (Denzin & Lincoln, 2005). Philosophical debates between quantitative and qualitative researchers were still underway (Reichardt & Rallis, 1994) but beginning to soften, and new methodologies to address the complex problems of society were being encouraged.

In retrospect, I now wonder if these writers were truly the first individuals to talk about combining quantitative and qualitative data. Individuals in my workshops have for some time been saying that mixed methods is not "new." Holmes (2006) raised this question when he commented,

> The major proponents insist that what they have developed is a new way of doing research—an alternative to qualitative and quantitative research, but what's new about that? . . . ethnographers and other social researchers have been gathering data using mixed methods at least since the 1920s, and case study researchers and anyone using triangulation have also been using mixed methods. (p. 2)

To probe whether or not it is a "new" idea requires returning to historical documents in fields such as sociology, evaluation, and action research. How does the pre-late-1980s discussion fit with what is known about mixed methods today? Three threads of thinking prior to the late 1980s can give us insight: the use of multiple methods, the discussions about using qualitative research within a research world largely dominated by quantitative research, and the informal initiatives to combine methods.

In terms of multiple methods, in 1959 Campbell and Fiske advanced the use of multiple methods in convergent and discriminant validation of psychological traits using a multitrait-multimethod matrix. They felt that more than one trait as well as more than one method must be employed in the validation process. Their discussion, however, was limited to multiple *quantitative* sources of data. During the 1970s, Denzin (1978) identified several types of combinations of methodologies in the study of the same phenomena or programs through his idea of *data triangulation*—the use of various data sources in a study. He said, "I now offer as a final methodological rule the principle that multiple methods should be used in every investigation" (Denzin, 1978, p. 28).

Throughout the 1970s and on into the 1980s, several noted authors were calling for the use of qualitative research on equal footing with more quantitative-experimental methods (Patton, 1980). Campbell (1974) gave a noted presentation at the American Psychological Association meeting on "Qualitative Knowing in Action Research" for the Kurt Lewin Award address. He suggested that a true scientific approach was to eliminate the question of the position of ultimate authority between quantitative and qualitative research and to reestablish the importance

of qualitative research. Cronbach (1975), in his well-known article "Beyond the Two Disciplines of Scientific Psychology," cast doubt on the idea that the social sciences could be modeled only on the natural sciences. Both Campbell and Cronbach started out as quantitative researchers and then embraced qualitative or naturalistic research through their writing.

Other authors began combining methods informally, and these writers were clearly the pioneers of mixed methods thinking today. In sociology, Sieber (1973) discussed the "interplay" of fieldwork and survey methods, and identified procedures for combining the two methods. Lamenting the fact that there were "too few examples to adduce general principles" (p. 1358), Sieber suggested the need for a "new style of research" (p. 1337). He further discussed the sequence of both methods with "concurrent scheduling" and "interweaving" the two methods (p. 1357). Equally important, he cited a number of studies that incorporated both interviews and surveys, and he discussed his own projects that included these forms of data collection (Sieber & Lazersfeld, 1966).

Another example of early mixed methods thinking comes from the field of evaluation in which Patton advanced "methodological mixes" (Patton, 1980, p. 108). He advocated for the use of anthropological naturalistic research in evaluation based on the "holistic-inductive paradigm" to complement the more traditional "hypothetical-deductive" approach. He recommended several models for program evaluation built on this combination. A design could be the pure hypothetical-deductive approach with an experimental design, quantitative data, and a statistical analysis, or a pure qualitative approach with naturalistic inquiry, qualitative measurement, and a content analysis. Then he suggested four "mixed form" models (p. 112) that varied from using experimental or naturalistic designs, qualitative or quantitative measurements, and often the transformation of qualitative data into counts. The diagram he sketched for the four models was remarkably similar to diagrams of mixed methods designs presented by recent authors (e.g., Johnson & Onwuegbuzie, 2004; Tashakkori & Teddlie, 1998).

Taking these readings as a whole, a good case can be made that mixed methods was underway much earlier than the late 1980s. These early writers focused on gathering multiple methods, including both quantitative (e.g., surveys) and qualitative (e.g., interviews) data. They initiated a language for mixed methods through such terms as the more general word "interplay" and more specific terms, such as "concurrent scheduling" (Sieber, 1973, pp. 1353, 1358). They provided examples of studies that employed multiple methods, and they took a process approach of thinking about the "interplay" through design, data collection, and data analysis. They conceptualized different types of mixed methods designs, such as those involving data transformation (Patton, 1980), and those including one form of method building on the other (Sieber, 1973).

On the other hand, although these early writers were interested in the "interplay" of quantitative and qualitative data, they did not specifically discuss how they would integrate the two data sources, or the reasons for integration as mixed methods is described today (e.g., see Bryman, 2006). They did not explicate the vast array of design possibilities in response to different purposes that is seen today (Creswell & Plano Clark, 2007, 2011). Although they started the discussion about names for the designs, they had a limited repertoire for designs (e.g., concurrent scheduling) as compared to the extensive list of design possibilities discussed recently (see Creswell & Plano Clark, 2011). They did not have a notation system (e.g., pluses and arrows) for providing a shorthand description of designs that would begin to emerge in 1991 (see Morse, 1991). Some of the detailed discussions about procedures (e.g., developing an instrument based on qualitative data), the use of mixed methods questions (Creswell & Plano Clark, 2011), or the larger philosophical issues (see Greene, 2007) were not present in their discussions.

The pre-late-1980s writers did, however, lay a foundation for mixed methods. As Tashakkori and Teddlie (2003) commented, these early writers "were mostly unaware that they were doing anything out of the ordinary" (p. 5). They used informal, commonsense ways of conducting research. A colleague recently remarked, "What is most amazing about mixed methods is that all of these (current) writers have taken ideas that have been around for a long time and spun them into a way of research, a methodology!" (Duane Shell, personal communication, August 17, 2009). Today, we have systematic, detailed, and defined ways of thinking about mixed methods research. But is a systematic approach better than the more intuitive early approach? Why do current mixed methods researchers (including myself) not give more credit to the early researchers who had the initial ideas that have now been embraced today as mixed methods?

What Really Drives Mixed Methods?

The ideas of a "new movement" or a "new star" suggest that some trends in methodology are building. What has promoted the escalation of interest in mixed methods? As suggested at the Aberdeen, Scotland, seminar, is it simply a response to funding initiatives?

Interest in mixed methods has grown since the *Handbook of Mixed Methods in Social & Behavioral Research* (Tashakkori & Teddlie, 2003) was published eight years ago (Creswell, 2009b). This handbook, consisting of four sections covering 759 pages, addressed current and future issues, methodological issues, and analytical issues. Using the base year of 2003 as a rough benchmark, it has been documented how interest has developed in the use of the term "mixed methods," as reported in funded projects at the National Institutes of Health (Plano Clark, 2010). Journals exclusively devoted to reporting mixed methods empirical studies and methodological discussions have also been initiated, such as in 2007, the *Journal of Mixed Methods Research* (Sage); in 2008, the *International Journal of Multiple Research Approaches* (eContent Management Pty); and in 2009, the *International Journal of Mixed Methods in Applied Business & Policy Research* (Academic Global). To these journals, I can also add journals started much earlier, such as *Quality and Quantity* (1967, Springer), *Field Methods* (1989, Sage), and the *International Journal of Social Research Methodology* (1998, Routledge). In addition, a number of recent journals have published special issues focusing exclusively or largely on mixed methods, such as *Research in Schools* (2006), *Annals of Family Medicine* (2004), and the *Journal of Counseling Psychology* (2005). At least 16 major books have been written about mixed methods, including recent books by Creswell and Plano Clark (2011), Greene (2007), Plano Clark and Creswell (2008), Teddlie and Tashakkori (2009), and Morse and Niehaus (2009). Mixed methods books are being published that have a distinct discipline focus, such as for nursing and health researchers (Andrew & Halcomb, 2009) and psychologists (Mayring, Huber, Gurtler, & Kiegelmann, 2007; Todd, Nerlich, McKeown, & Clarke, 2004). Chapters can be found in methods books in discipline fields such as social work (Engel & Schutt, 2005) and family research (Greenstein, 2006). An international conference on mixed methods has been offered in the United Kingdom for the last five years, along with international publications on mixed methods around the globe: in psychology from Europe (Mayring et al., 2007), in nursing from Australia (Andrew & Halcomb, 2009), in linguistics from Japan (Heigham & Croker, 2009), in the social sciences from Switzerland (Bergman, 2008), and in education from South Africa (Creswell & Garrett, 2008).

In light of these developments, I must ask what has given impetus to this interest? It may well be that funding sources have encouraged mixed methods research with the global economic imperative—starting in the 1990s—to do more with less (Giddings, 2006). In a mixed methods study of family adoption practices, Miall and March (2005) wrote about how their funders forced them to change their questions and design from their initial plan of starting with quantitative questions that would be intentionally followed by qualitative questions. Holmes (2006) alleged that mixed methods reduced researchers to "depersonalized technicians," which tacitly supported funding agencies to seek projects with convergence on a single answer rather than differences in opinions and beliefs.

On the other side, certainly the legitimacy of qualitative research has encouraged researchers to think in a pluralistic way. Interdisciplinary research problems now call for addressing complex issues using skilled methodologists from both quantitative and qualitative research who bring diverse approaches to studies (Mayring et al., 2007). Still, questions linger about whether mixed methods is simply a response to funding interests and whether the research questions addressed by mixed methods researchers truly merit a "mixed" methodology. Those coming from a philosophical, postmodern perspective have suggested that researchers are "accepting uncritically and undigested" mixed methods (Freshwater, 2007, p. 145).

The Paradigm Debate Continues

Philosophically oriented writers for years have debated whether mixed methods research is possible because it mixes worldviews or paradigms. They ask: Can paradigms (ontologies or realities) be mixed? Some writers adhere to the idea that paradigms or worldviews have rigid boundaries and cannot be mixed. Holmes (2006) asked: "Can we really have one part of the research which takes a certain view about reality nested alongside another which takes a contradictory view? How would we reconcile, or even work with, competing discourses within a single project?" (p. 5). The logic being used here was that mixed methods was untenable because methods were linked to paradigms, and therefore the researcher, in using mixed methods research, was mixing paradigms. This stance has been described as the purist stance (see Rossman & Wilson, 1985), and it has been called the "incompatibility thesis" (Howe, 2004) and discussed in the mixed methods literature as mixing viewpoints (Johnson et al., 2007, p. 123). Individuals that hold this position view paradigms as having discrete and impermeable boundaries, an idea reinforced by the clear-cut boxes and lines around the alternative inquiry paradigms in the literature (e.g., see Guba & Lincoln's tables, 2005; or Creswell's table of worldviews, 2009c). Granted, by 2005, Guba and Lincoln had taken down these artificial boundaries by declaring cautiously that elements of paradigms might be blended together in a study. Contributing to this perspective was certainly a "delinking" of paradigms and methods, such as conveying that many different research methods would be linked to certain paradigms, and that a paradigm justification did not dictate specific data collection and analysis methods (Johnson & Onwuegbuzie, 2004).

With the gate now opened to thinking about use of multiple paradigms, mixed methods writers have now taken varied stances on incorporating paradigms into mixed methods. For example, a dialectic stance by Greene and Caracelli (1997) suggested that multiple paradigms might be used in mixed methods studies, but that each paradigm needed to be honored and that their combined use contributed to healthy tensions and new insights. In my writings, I took a similar stance, but suggested that multiple paradigms related to different phases of a research design (Creswell & Plano Clark, 2007, 2011), thus linking paradigms to research designs. For example, a mixed methods study that begins with a quantitative survey phase reflects an initial postpositivist leaning, but, in the next qualitative phase of focus groups, the researcher shifts to a constructivist paradigm. Relinking paradigms and designs makes sense.

Still others advocated for one underlying paradigm that fits mixed methods, and some found their paradigm in pragmatism with historical roots back to Charles Peirce, William James, John Dewey, Richard Rorty, and others (Johnson & Onwuegbuzie, 2004; Tashakkori & Teddlie, 2003). Pragmatism emphasizes the importance of the research questions, the value of experiences, and practical consequences, action, and understanding of real-world phenomena. Advocates said that it is a "philosophical partner for mixed methods research" (Johnson & Onwuegbuzie, 2004, p. 16). A different paradigmatic stance, suggested by Mertens (2003, 2009), is found in the transformative-emancipatory framework that made explicit the goal for research to "serve the ends of creating a more just and democratic society that permeates the entire research process" (Mertens, 2003, p. 159). Mertens thus creatively relates this goal to different phases in designing a mixed methods study.

Whether the paradigm for mixed methods involves a single paradigm, multiple paradigms, or phased-in paradigms, Morgan (2007) recently reminded the mixed methods community of the importance of Kuhn's (1970) original description of a paradigm. Using the definition of a paradigm as "shared belief systems that influence the kinds of knowledge researchers seek and how they interpret the evidence they collect" (Morgan, 2007, p. 50), Morgan found paradigms to be (1) worldviews, an all-encompassing perspective on the world; (2) epistemologies, incorporating ideas from the philosophy of science such as ontology, methodology, and epistemology; (3) "best" or "typical" solutions to problems; and (4) shared beliefs of a "community of scholars" in a research field. It is this last perspective (embraced by Kuhn, 1970) that Morgan strongly endorses, and he discussed how researchers share a consensus in specialty areas about what questions are most meaningful and which procedures are most appropriate for answering their questions.

Another mixed methods writer, Denscombe (2008), agreed with this perspective and took it one step further. Denscombe outlined how "communities" may work using such ideas as sharing identity, researching common problems, forming networks, collaborating in pursuing knowledge, and developing informal groupings. This line of thinking has focused attention on the emerging fragmentation of the mixed methods field in which various disciplines adopt mixed methods in different ways, create unique practices, and cultivate their own specialized literatures. For example, at the Veterans Administration Research Center in Ann Arbor, Michigan, in the health sciences, colleagues have conceptualized mixed methods as formative and summative evaluation procedures (Forman & Damschroder, 2007). This conceptualization adapts mixed methods to the Veterans Administration health services context of intervention research. The rise of discipline-oriented mixed methods books is another instance of adapting mixed methods to scholarly communities. Still, I wonder if discipline fragmentation of mixed methods will lead to further philosophical differences among scholars in mixed methods. Will the scholarly community line of thinking continue or will the conversation return to the difficulty of mixing realities? Is the idea of mixing realities actually all about whether one paradigm takes precedence over another in mixed methods research?

Mixed Methods Privileges Postpositivism

Critics make the allegation that mixed methods favors postpositivist thinking over more interpretive approaches. Does mixed methods privilege postpositivist thinking and marginalize interpretive approaches? Several authors have taken this position. The context for many of these concerns resides in what is seen as a conservative challenge to qualitative inquiry (Denzin & Giardina, 2006). Denzin and Giardina believe that conservative regimes enforce scientifically based models of research (SBR). For example, the 2001 No Child Left Behind Act (NCLB) in education emphasized accountability, high-stakes testing, and performance scores for students. The model for research being advanced was to "apply rigorous, systematic, and objective methodology to obtain reliable and valid knowledge" (Ryan & Hood, 2006, p. 58). Within this context, qualitative research is marginalized, and it minimizes complex and dynamic contexts, subtle social differences produced by gender, race, ethnicity, linguistics status, and class, and multiple kinds of knowledge (Lincoln & Canella, 2004). In 2002, one year after the No Child Left Behind Act was implemented, the National Research Council established guidelines in their report, *Scientific Research in Education,* that called for a quantitative approach to research through guiding principles asking for significant questions that could be empirically studied, relevant theory, methods closely tied to the research questions, explanations of findings using a logical chain of reasoning, replicated studies and generalizations, and disseminated research for critique by the professional scientific community (Ryan & Hood, 2006; Shavelson & Towne, 2002). Howe (2004) called the National Research Council's perspective "mixed-methods experimentalism" (p. 48) and felt that it assigned a prominent role to quantitative experimental research and a lesser role to qualitative, interpretive research. Further, this approach "elevates quantitative-experimental methods to the top of the methodological hierarchy and constrains qualitative methods to a largely auxiliary role in pursuit of the *technocratic* aim of accumulating knowledge of 'what works'" (Howe, 2004, pp. 53–54). He also stated, "It is not that qualitative methods can never be fruitfully and appropriately used in this way, but their natural home is within an interpretivist framework with the democractic aim of seeking to understand and give voice to the insider's perspective" (p. 54). This interpretivist aim values outcomes assessed by various stakeholders, includes all relevant voices in the dialogue, and engages in qualitative data collection procedures to promote dialogue, such as participant observation, interviews, and focus groups. This dialogue also needs to be *critical* with the views of participants subjected to rational scrutiny.

Howe's theme was echoed again in the following years' publication of the third edition of this handbook (Denzin & Lincoln, 2005). Denzin and Lincoln talked directly about the "mixed-methods movement" as taking qualitative methods out of their natural home, which is in the "critical, interpretive framework" (p. 9). Finally, in a provocative article by Giddings (2006), titled, "Mixed-methods Research: Positivism Dressed in Drag?" the issue of the hegemony of positivism and the marginalization of nonpositivist research methodologies in mixed methods was addressed. She conveyed the idea that certain "thinking" went on in research that was reflected in methodologies and "the 'thinking' of positivism continues in the 'thinking' of mixed methods" (p. 200). Giddings felt that this mixed methods "thinking" was expressed through analysis and prescriptive styles, structured approaches to research design and data collection, and the use qualitative aspects "fitted in" (p. 200).

There is little doubt that a good case can be made that, in certain approaches, mixed methods researchers have relegated qualitative inquiry to a secondary role. A good example would be the embedded research design (Creswell & Plano Clark, 2007), in which qualitative methods often provide a supportive role in experimental, intervention studies. Our feeling has long been that the use of qualitative approaches *whatever their role* in traditional quantitative experiments elevates qualitative research to a new status and opens the door for seeing qualitative research as a legitimate form of inquiry. Whether this will materialize can certainly be debated. The structured ways of designing mixed methods projects that we embrace in our text (Creswell & Plano Clark, 2007) also reinforces Giddings' idea of the structured "thinking" in our approach to mixed methods. In mixed methods data analysis, the use of "manifest effect sizes" by Onwuegbuzie and Teddlie (2003, p. 356) reinforces a postpositivist leaning of mixed methods.

On the other hand, many studies in mixed methods can be found that give priority to qualitative methods. Some designs subordinate quantitative methods to qualitative methods (see the exploratory sequential design mentioned by Creswell & Plano Clark, 2007). Also, the writings on applying the transformative-emancipatory framework to mixed methods emphasize qualitative research (Mertens, 2009). A close reading of the National Research Council's report on scientific research in education shows that the types of questions recommended for scholarly educational research were both quantitative (descriptive, experimental) as well as qualitative (exploratory), a point that Howe (2004) concedes. Although more critical, interpretivist articles are needed in the mixed methods field, some evidence exists that the number of articles is growing. A recent paper (Sweetman, Badiee, & Creswell, 2010) has identified mixed methods studies that honor the inclusion and dialogue of communities of action within Mertens's transformative-emancipatory framework. This paper examined several mixed methods studies that addressed disability, ethnic, feminism, and social class as theoretical standpoints and advanced ways that researchers might incorporate these standpoints into their mixed methods projects. Further evidence of standpoint epistemology—typically found in qualitative research—is found in recently published articles in *JMMR* addressing women's social capital (Hodgkin, 2008) and African American women's interest in science (Buck, Cook, Quigley, Eastwood, & Lucas, 2009). Despite these studies, what is the evidence that mixed methods research marginalizes interpretive approaches? Do we need more mixed methods research that incorporates an interpretive perspective? Is the use of qualitative research in a supportive role in intervention studies marginalizing qualitative inquiry, or is it advancing it within fields that traditionally honored experimental methods? Do we need more articles that embrace "mixed methods interpretivism," in which quantitative research is relegated to a secondary role within qualitative research, as Howe (2004) would recommend?

A Fixed Discourse in Mixed Methods

Unquestionably, more interpretive, theoretical studies in mixed methods would broaden the audience and discourse of it. This raises another controversy about the discourse of mixed methods. Some critics are asking: Is there a dominant discourse in mixed methods? Is messiness allowed in? These questions speak to the issue of mixed methods privileging postpositivist thinking—a postmodern concern about the discourse in mixed methods. Who controls this discourse and the language that is being used in mixed methods research? Several authors have weighed in on this issue.

A recent important article takes up these concerns (Freshwater, 2007). Freshwater is an editor and leading researcher in nursing as well as a postmodernist. She was concerned about how mixed methods was being "read" and the discourse that followed. Discourse was defined as a set of rules or assumptions for organizing and interpreting the subject matter of an academic discipline or field of study in mixed methods. The uncritical acceptance of mixed methods as an emerging dominant discourse ("is nearing becoming a metanarrative" [Freshwater, 2007, p. 139]) impacts how it is located, positioned, presented, and perpetuated. She called on mixed methods writers to make explicit the internal power struggle between the mixed methods text as created by the researcher and the text as seen by the reader/audience. Mixed methods, she felt, was too "focused on fixing meaning" (p. 137). Expanding on this, she stated that mixed methods was mainly about doing away with "indeterminancy and moving toward incontestability" (p. 137), citing as key examples the objective third-person style of writing, the

flatness, and the disallowance for competing interpretations to coexist. She requested that mixed methods researchers adopt a "sense of incompleteness" (p. 138) and recommended that reforms required the

> need to explore the possibility of hybridization in which a radical intertextuality of mixing forms, genres, conventions, and media is encouraged, where there are no clear rules of representation and where the researcher, who is in reality working with radical undecidability and circumscribed indeterminacy, is able to make this experience freely available to readers and writers. (p. 144)

These ideas were a positive criticism, and a call for mixed methods writers to insert questions into their discourses, to acknowledge the messiness of mixed methods, and to recognize that it is a field still in "adolescence" (Tashakkori & Teddlie, 2003, p. x).

Still, by providing a visual of the mixed methods research process that follows linear development, Johnson and Onwuegbuzie (2004) erase the "messiness." A certain tidiness is given when specific names are assigned to research designs (e.g., explanatory sequential designs—Creswell & Plano Clark, 2007), when researchers do not attend to the "messiness" in conducting the designs (e.g., see Creswell et al., 2008), and when writers look for a consensus in definitions (Johnson et al., 2007). These examples all point toward "fixing" or the field "being fixed." But these points open up further questions, such as how should mixed methods writers discuss its messiness, its blurred borders, and its problems? Will unstructured mixed methods serve well the beginning researcher as well as the more experienced researcher?

To Be Bilingual or Not

A related issue is whether any one ideological camp dominates the language of mixed methods research. Is there a dominant language or set of terms for mixed methods? Vygotsky and Cole (1978) propose that the sociocultural perspective of language shapes how individuals make sense of the world, and that the learning process consists of a gradual internalization of this language. What is the language of mixed methods? One issue being discussed is whether we need a "bilingual" language for mixed methods research so that it does not favor quantitative or qualitative research. Raising this question is reminiscent of concerns in qualitative research in the early 1980s around the topic of qualitative validity, and how terms such as trustworthiness and authenticity created a "new," distinct language to discuss validity (Lincoln & Guba, 1985).

As the language of mixed methods develops, a confusing picture has emerged about the nomenclature to use. For example, in writing about validity, Onwuegbuzie and Johnson (2006) intentionally called validity "legitimation" and thereby created a new word in the mixed methods lexicon. In our specification of types of research designs, we created new names, such as the "exploratory sequential design," to provide a descriptive label signifying that the design would first fulfill the intent of exploring using qualitative data followed by explanation using quantitative data (Creswell & Plano Clark, 2007). Illustrating an example of a made-up bilingual term, writers in a recent psychology text used the term "qualiquantology" to express their discomforting hybridity of mixing qualitative and quantitative methods (Stenner & Rogers, 2004).

Other writers in the mixed methods field use a less bilingual vocabulary. Leaning toward a more quantitative language, Teddlie and Tashakkori (2009) use the term "inferences," or "meta-inferences," to denote when the results are incorporated into a coherent conceptual framework to provide an answer to the research question. Although "inferences" may relate to either qualitative or quantitative research, it seems to be employed frequently in drawing conclusions from a sample to a population in a quantitative study. Another example is the use of the term "construct validity" by Leech, Dellinger, Brannagan, and Tanaka (2010) as an overarching validity concept for mixed methods research. This term is drawn from quantitative measurement ideas. On the qualitative side, the idea of personal transformation advanced by Mertens (2009) clearly has qualitative roots. Unquestionably, the language that has emerged is both bilingual and oriented toward one form of inquiry (quantitative or qualitative). The use of glossaries in recent mixed methods books suggests the need for a common vocabulary (see Morse & Niehaus, 2009; Teddlie & Tashakkori, 2009). These examples, however, raise difficult questions about who controls the language of mixed methods, how it is conveyed, and what the language should be. It also introduces questions about how the writing up of mixed methods proposals and projects influences what gets approved, funded, and published.

A Baffling (and Complex) Array of Designs

It is not only the language that introduces confusion and controversy into the mixed methods discourse. In research designs—a topic that has filled the pages of mixed methods writings—researchers are confronted by a baffling array of names and types of ways to conduct mixed methods research. How might a mixed methods researcher conduct a mixed methods study? When my colleague, Vicki Plano Clark, and I wrote an introduction to the field for beginning mixed methods researchers (Creswell & Plano Clark, 2007), we presented 12 different classification systems of designs drawn from diverse fields of evaluation, nursing, public health, and education.

Not wanting to add to the confusion, we suggested a parsimonious set of designs. Triangulation (or now called convergent) designs involved one phase of qualitative and quantitative data collection gathered concurrently. Explanatory or exploratory

designs required two phases of data collection, quantitative data collection followed sequentially by qualitative data collection (or vice versa). Embedded designs, in which one form of data was embedded within another, may be either a single- or a double-phase design with concurrent or sequential approaches. In all of these designs, we focused on the weight given to qualitative and quantitative data, the timing of both forms of data, and the mixing of the data in the research process. To present these designs, we used a modified notation system first developed by Morse (1991), and we sketched diagrams of procedures and advanced guidelines for constructing these diagrams found in the literature (Ivankova, Creswell, & Stick, 2006).

We now know that these designs are not complex enough to mirror actual practice, although our thinking at the time was to advance designs for the first-time mixed methods researcher. Also, we are more aware of the complex designs being used and reported in the literature. For example, Nastasi and colleagues wrote about a complex evaluation design with multiple stages and the combination of both sequential and concurrent phases (Nastasi et al., 2007). The designs reported in journals have incorporated "unusual blends" of methods, such as combinations of quantitative and qualitative longitudinal data, discourse analysis with survey data, secondary data sets with qualitative follow-ups, and the combination of qualitative themes with survey data to produce new variables (Creswell, 2011). The representation of designs has also advanced joint matrices for arraying both quantitative and qualitative data in the same table, an approach encouraged by the matrix feature of qualitative software products (see Kuckartz, 2009).

Our designs and the many classifications bring a typology approach to mixed methods design. Arguing that we need an alternative to typologies, Maxwell and Loomis (2003) conceptualized a systems approach of five interactive dimensions of the research process consisting of the purpose, the conceptual framework, the questions, the methods, and the issue of validity. With this approach, they provided a fuller, more expansive view of the way to conceptualize mixed methods designs. Another approach comes from the creative thinking of Hall and Howard (2008). They suggested a synergistic approach in which two or more options interacted so that their combined effect was greater than the sum of the individual parts. Instead of looking at mixed methods as a priority of one approach over the other, or a weighting of one approach, the researcher considered their value and representations equal. The researcher also viewed the two as equal from an ideology of multiple points of view, balancing objectivity with subjectivity. Collaboration consisted of the equal skill expertise about qualitative and quantitative methodologies on a research team.

The synergistic approach, along with other challenges to typological perspectives has contributed to a softening of the differences between qualitative and quantitative research, provided answers to questions about dominance of one method over the other (e.g., Denzin & Lincoln, 2005), and honored the formation of research teams with diverse expertise. In light of these discussions, are typologies of research designs outdated? Are newer, more free-flowing designs an improved way to think about designing a mixed methods study?

Misappropriating Designs

Another procedural question about designs is whether mixed methods is misappropriating designs from other fields. As mixed methods continues to grow in popularity and use, is the field misappropriating traditional designs and calling them "mixed methods" (thereby overstating the value and claims of mixed methods)? Several examples stand out. Scale development (DeVellis, 1991) has been available to the researcher for many years in quantitative research. Early phases of scale development often call for an initial exploration, even though this may consist of reviewing the literature rather than conducting an extensive qualitative data collection procedure, such as the use of focus groups (Vogt, King, & King, 2004). One might argue that scale development should be a distinct procedure from mixed methods research, and yet, mixed methods designs with the purpose of developing an instrument are available in the journal literature (e.g., Myers & Oetzel, 2003).

Another example would be content analysis, a quantitative procedure involving the collection of qualitative data and its transformation and analysis by quantitative counts. In this approach, *both* qualitative and quantitative are not collected, but both qualitative research (in data collection) and quantitative research (in data analysis) are employed. If one views mixed methods as collecting both quantitative and qualitative data, then content analysis does not qualify as mixed methods research. Is content analysis a separate approach or is data transformation also a part of mixed methods designs as suggested by Sandelowski et al. (2009)? What are appropriate boundaries for mixed methods research?

Perhaps mixed methods is actually a subordinated set of procedures used within a large number of designs. I call this approach using a "framework" for conducting mixed methods procedures. It is basically the idea that some larger framework becomes a placeholder within which the researcher gathers quantitative and qualitative data (or conducts mixed methods procedures). This idea first surfaced when a participant at a workshop asked, "Is ethnography mixed methods research?" The sense of this question was that ethnographers have traditionally collected both quantitative and qualitative data and used both in their description and analysis of culture-sharing groups. Morse and Niehaus (2009) discussed this question, and concluded that many ethnographers do see their methodology as a distinct approach, and that ethnography needs to be viewed as independent of mixed methods.

But I wonder if seeing mixed methods as a subordinate procedure within ethnography is the most appropriate stance. Researchers seem to use mixed methods within larger frameworks of many types. Evidence for these frameworks comes from using mixed methods procedures within narrative studies (Elliot, 2005), experiments (Sandelowski, 1996), and case studies (Luck, Jackson, & Usher, 2006). Other frameworks can be seen as well, such as using mixed methods within a social network analysis (Quinlin, 2010), an overarching research question (Yin, 2006), a feminist lens (Hesse-Biber & Leavy, 2007), or in action research (Christ, 2009). If the mixed methods designs can be stretched to include these different frameworks, then the potential for extending use of mixed methods in many ways is possible. But where is the boundary between mixed methods and other designs? Is a boundary needed? If mixed methods researchers are claiming other designs for their own, can their claims be justified?

■ Value Added?

Regardless of the design and whether it is appropriate, the utility of mixed methods research—from a pragmatic approach—is tied to whether it is a valuable approach. In our earlier definition (Creswell & Plano Clark, 2007), we end with the assumption that the combination of methods provides a better understanding than either quantitative method or qualitative method alone. Can this assumption be substantiated? In tracing the recent history of mixed methods, I referred to a question asked by the president of Sage Publications during a luncheon meeting. He asked me, "Does mixed methods provide a better understanding of a research question than either quantitative or qualitative research alone?" (Creswell, 2009b, p. 22). This difficult question is central to justifying mixed methods and giving it legitimacy. Unfortunately, it remains unanswered in the mixed methods community.

I can provide a hypothetical series of studies on how it *might* be addressed. One approach is to turn to research procedures used in early studies that compared participant observation with survey results (Vidich & Shapiro, 1955) or interviews with surveys (Sieber, 1973) and examine if the two databases converge or diverge in understanding a research problem. A second approach is to proceed with an experiment in which groups of readers examine a study divided into a qualitative, a quantitative, and a mixed methods part. In this experiment, outcomes are specified such as the quality of interpretation, the inclusion of more evidence, the rigor of the study, or the persuasiveness of the study, and the three groups could be compared experimentally. A third approach is to examine some outcomes suggested by authors of published mixed methods studies. One such outcome might be "yield," such as that advanced by O'Cathain, Murphy, and Nicholl (2007), in which they assess it by the number of publications and whether the authors of a mixed methods study actually integrate the data. Other outcomes could be analyzed using qualitative document analysis approaches, and themes developed from statements of value posed by authors of mixed methods empirical articles and methodological studies. For example, authors from the field of communication studies suggested that the value of mixed methods lies in addressing limitations in the results learned from one method:

> To address more thoroughly this question, and account for some of the possible limitations of study-one, a broader based assessment of students' involvement in intercultural communication courses was pursued. (Corrigan, Pennington, & McCroskey, 2006, pp. 15–16)

Other options may also exist. The mixed methods community does not have an adequate answer to this controversy, and so I ask: When and how can we begin to answer this question? Does a mixed method better address the core research question being asked in a study than either quantitative or qualitative alone? What criteria should be used in assessing it? Why have mixed methods researchers not pursued this issue more vigorously?

■ Conclusion

Striking at the heart of its existence, critical comments about mixed methods are being made about its meaning and definition (raising concerns about expectations, as I learned at Aberdeen). The form of this conversation has been to debate whether mixed methods is a "method," a "methodology," some combination, or a way of seeing. Related to this larger issue is whether it is a "new" way of researching, reinforces a slanted use of terms, and creates a false binary distinction between quantitative and qualitative data (and research).

Assuming that mixed methods researchers take paradigms (i.e., worldviews, beliefs, values) seriously (an assumption that several writers have questioned; see Holmes, 2006, and Sale, Lohfeld, & Brazil, 2002), I see the paradigm discussion as an important discussion in the mixed methods literature. Diverse stances have emerged from a single paradigm perspective, such as pragmatism or the transformational-emancipatory perspective, to multiple paradigm use in a dialectic approach, and to relating the paradigm to the design. Some discussion has moved away from which one paradigm, or how many to use, to a focus on paradigm use within communities of scholars. Still, critics are concerned about whether the current approaches to mixed methods privilege postpositivist thinking and create discourses that "fix" the otherwise messy content of mixed methods.

No subject has been so widely discussed in the mixed methods literature as its designs and its methods. This emphasis places importance on the methods, sometimes at the expense of minimizing the importance of the research question in directing scholarly inquiry (Gurtler, Huber, & Kiegelmann, 2007). At other times, critics of the mixed methods literature see a baffling list of different types of designs with unusual names, the potential of mixed methods claiming many more designs than it deserves, and having questionable outcomes.

The implications of these controversies are that many of them are interrelated and my sorting them out here is contrived—a heuristic. When authors talk about the controversies, I have found their discussion to cover many topics rather than an in-depth analysis of any one controversy. Also, the range of controversies is quite extensive, stretching from basic issues of the legitimacy and meaning of mixed methods to its philosophical underpinnings, and on to the pragmatics of conducting a mixed methods study. Fundamentally, my position is that the mixed methods community needs to squarely place these controversies on the table for discussion and honor their presence.

Some readers will say that I have overlooked critical controversies such as the relationship of research problems to methods, validity, and evaluation of mixed methods, the writing of a mixed methods study, and the common question of "who cares about methods?" Other readers will undoubtedly see my views as deliberately "transgressive " (Richardson, 1997): a turn to challenging mixed methods rather than advocating for it. Others will see my remarks as an attempt to open up the discourse about mixed methods, much like I have advocated in authored and coauthored editorials for the *Journal of Mixed Methods Research*. Still others might consider my justifications both for and against the issues as evidence of postpositivist leanings (or even worse the creation of new metanarratives). All of these renderings may be both right and wrong. As a pragmatist, I can confidently say that I am interested in the consequences of this discussion of controversies, the seeds of which were sprouted at Aberdeen. Perhaps rather than finding irony in the space of Elphinstone Hall in Scotland, I should have seen instead the long shadows that the walls were casting. In the end, I advise those interested in mixed methods to reassess their commitment to controversies now being raised. As Kuhn (1970) said, "A revolution is for me a special sort of change involving a certain sort of reconstruction of group commitments" (p. 181).

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16

MIXED METHODS RESEARCH

Contemporary Issues in an Emerging Field

Charles Teddlie and Abbas Tashakkori[1]

The field of mixed methods research (MMR), which we have called the "third methodological movement," has evolved as a result of discussions about methods and paradigms in the social and behavioral sciences that have been ongoing for at least three decades. The "paradigm debate" between quantitatively oriented and qualitatively oriented researchers was based on sets of interlocking epistemological, ontological, and methodological assumptions. MMR offers a third alternative based on pragmatism, which argues that the two methodological approaches are compatible and can be fruitfully used in conjunction with one another (e.g., Howe, 1988; Tashakkori & Teddlie, 1998).

This chapter briefly presents several important issues in contemporary MMR, including a definition of MMR, theoretical and conceptual issues, issues in conducting MMR, and criticisms of the third methodological movement. We advise the reader to consider this to be a "sampler" of some of the contemporary issues relevant to MMR and, if you are interested, to continue your exploration of the field by reading some of the numerous cited references.

Definitions and Origins of Mixed Methods Research

Definition of Mixed Methods Research

As writing in the field of MMR has become more sophisticated, several authors have labored to identify and define exactly *what mixed methods research is* (e.g., Creswell, 2010; Greene, 2007, 2008; Johnson, Onwuegbuzie, & Turner, 2007; Tashakkori & Teddlie, 1998, 2003a). There is even continued debate over what the field should be called with variants including, but certainly not limited to, MMR, multimethod research, mixed methods, mixed methodology, mixed research, integrated research, and so forth.

Fortunately, there appears to be some consensus around "mixed methods research" as the de facto term due to common usage (e.g., the names of the leading journal in the field and of a handbook now in its second edition). We suspect that this term will endure, since it now has the trappings of a "brand name" that has been widely disseminated throughout the social and behavioral sciences.

As for the definition of MMR, Johnson et al. (2007) presented 19 alternative meanings from leaders in the field. While these meanings had varying levels of specificity, the authors of this analysis settled upon the following "composite" definition:

> Mixed methods research is the type of research in which a researcher or team of researchers combines elements of qualitative and quantitative research approaches (e.g., use of qualitative and quantitative viewpoints, data collection, analysis, inference techniques) for the broad purposes of breadth and depth of understanding and corroboration. (Johnson et al., 2007, p. 123)

From our perspective, this definition works because it includes what we believe is an essential characteristic of MMR: *methodological eclecticism,* a term that has only occasionally been used (e.g., Hammersley, 1996; Yanchar & Williams, 2006). Hammersley originally described this characteristic as follows:

> What is being implied here is a form of methodological eclecticism; indeed, the *combination* of quantitative and qualitative methods is often proposed, on the ground that this promises to cancel out the respective weaknesses of each method. (Hammersley, 1996, p. 167, italics in original)

Our definition of methodological eclecticism goes beyond simply combining qualitative (QUAL) and quantitative (QUAN) methods to cancel out respective weaknesses. *Eclectic,* the root word of eclecticism, means "choosing what appears to be the best from diverse sources, systems, or styles."[2] For us, *methodological eclecticism* involves *selecting and then synergistically integrating the most appropriate techniques from a myriad of QUAL, QUAN, and mixed methods* in order to more thoroughly investigate a phenomenon of interest. A researcher employing methodological eclecticism is a *connoisseur[3] of methods* who knowledgeably (and often intuitively) selects the best techniques available to answer research questions that frequently evolve during the course of an investigation.[4]

Origins of Mixed Methods Research, With an Emphasis on Qualitative Methods

MMR emerged as a distinct orientation in the late 1970s from applied fields in the social and behavioral sciences, such as evaluation, nursing, and education (e.g., Greene, Caracelli, & Graham, 1989; Miles & Huberman, 1984, 1994; Morse, 1991; Patton, 1980, 1990, 2002; Reichardt & Cook, 1979; Rossman & Wilson, 1985). Its origin in the applied, rather than pure, human sciences was not coincidental, since those disciplines often require a pragmatic, wide-angle lens utilizing all data sources available to answer practical questions.

Numerous studies from this early MMR involved researchers adding a QUAL component to a study that was initially a QUAN-only project in order to make greater sense out of the numerical findings. In evaluation research, this involved adding a formative component (how or why did the program succeed or fail) to the summative component (did the program work). In the human sciences, this distinction relates to causal effects (i.e., *whether* X causes Y) as opposed to causal mechanisms (i.e., *how* did X cause Y) (e.g., Shadish, Cook, & Campbell, 2002).[5]

In our own research, we have found that information gleaned from narratives generated by participants and investigators often proves to be the most valuable source in understanding complex phenomena. For example, qualitatively oriented case studies of differentially effective schools express the complexity of evolving contextual and behavioral patterns in those institutions much more thoroughly than statistical summaries of numeric indicators (Teddlie & Stringfield, 1993). A simple way of saying this is that narratives (stories) are intrinsically more interesting (and often more enlightening) than numbers to many researchers, the participants in their studies, and their audiences. It is no coincidence that several MMR pioneers (e.g., Creswell, Miles and Huberman, Morse, Patton) have also written QUAL methods texts.

We want to unambiguously express our regard for the powerful contributions of QUAL methods in this fourth edition of *The SAGE Handbook of Qualitative Research* due to the concern that some scholars have expressed about MMR subordinating QUAL methods to a secondary role behind QUAN methods (e.g., Denzin & Lincoln, 2005; Howe, 2004). This is not how we interpret the MMR literature we have reviewed from the past 30-plus years. In fact, QUAL + quan studies emphasizing the detailed, impressionistic perceptions of human "data-gathering instruments" and their interpretations of their outcomes are among the most valuable of all the extant MMR literature.

We also believe that MMR can add an important dimension to QUAN research. There has been much discussion (e.g., Mosteller & Boruch, 2002; Shavelson & Towne, 2002) about the importance of randomized controlled trials (RCTs) in the social and behavioral sciences. While RCTs may represent the "gold standard" for the identification of causal effects, the addition of a QUAL component (e.g., case studies) to the design allows researchers to discuss causal mechanisms as well. There are several examples of this in the MMR literature, including (1) the mixed methods intervention program of research in the health sciences (Song, Sandelowski, & Happ, 2010) and (2) the group-case method (also known as experimental ethnography) in several disciplines (e.g., Teddlie, Tashakkori, & Johnson, 2008). MMR enables researchers to examine issues in those fields in ways that traditional QUAN methods cannot alone.

Some Contemporary Characteristics of Mixed Methods Research

We begin this section by acknowledging that there are others writing in the MMR field who will disagree with the inclusion of some or all of the characteristics described below, or with our interpretation of certain of those characteristics. Such is the nature of most emerging fields in academia, as new ideas are put forth and contested by those highly interested in the topic. This is especially the case with regard to MMR, whose development has been enhanced greatly by the juxtaposition of diverse perspectives. (Table 16.1 presents eight contemporary characteristics of MMR.)

We described the first contemporary characteristic of MMR in the previous section of this chapter: *methodological eclecticism.* This characteristic stems from rejection of the incompatibility (of methods) thesis, which stated that it is inappropriate to mix QUAL and QUAN methods in the same study due to epistemological differences between the paradigms that are purportedly related to them. Howe (1988) countered this point of view with his *compatibility thesis*, which contends that "combining quantitative and qualitative methods is a good thing" and "denies that such a wedding of methods is epistemologically incoherent" (p. 10). Howe proposed pragmatism as an alternative paradigm, a suggestion that has been endorsed by many others (e.g., Biesta, 2010; Johnson & Onwuegbuzie, 2004; Maxcy, 2003; Tashakkori & Teddlie, 1998).

Table 16.1 Eight Contemporary Characteristics of Mixed Methods Research

	Description of Characteristic
1.	Methodological eclecticism
2.	Paradigm pluralism
3.	Emphasis on diversity at all levels of the research enterprise
4.	Emphasis on continua rather than a set of dichotomies
5.	Iterative, cyclical approach to research
6.	Focus on the research question (or research problem) in determining the methods employed within any given study
7.	Set of basic "signature" research designs and analytical processes
8.	Tendency toward balance and compromise that is implicit within the "third methodological community"

Methodological eclecticism not only means that we are free to combine methods, but that we do so by choosing what we believe to be the best tools for answering our questions. We have called this choice of "best" methods for answering research questions, "design quality"[6] and have included it as an essential part of our framework for determining the inference quality of MMR (Tashakkori & Teddlie, 2008). Furthermore, we believe that the best method for any given study in the human sciences may be purely QUAL, purely QUAN, or (in many cases) mixed.

Schulenberg (2007) presented a complex example of methodological eclecticism in a mixed methods study of the processes that occur in police decision making. Her data sources included interviews administered to individual officers, documents provided by the interviewees, QUAL data gathered from police department websites, documents obtained from provincial governments, census data, and tabulations of statistical data on the proportion of apprehended youth actually charged with crimes. The interview data gathered from police officers were originally QUAL in nature (from semistructured protocols), but were also converted into numbers (quantitized).

Schulenberg (2007) used these diverse data sources to generate five separate databases that addressed her research questions and hypotheses. She employed eight types of QUAL techniques and six types of statistical QUAN techniques including *t* tests, chi-squares, multiple regression, analysis of variance, manifest and latent content analysis, the constant comparative method, and grounded theory techniques. The *methodological eclecticism (connoisseurship)* of this criminologist/sociologist is apparent.

The second contemporary characteristic of MMR is *paradigm pluralism,* or the belief that a variety of paradigms may serve as the underlying philosophy for the use of mixed methods. This characteristic is, of course, a function of the rejection of the incommensurability (of paradigms) thesis, which is widely accepted within the MMR community.

We believe that contemporary MMR is a kind of "big tent" in that researchers who currently use mixed methods come from a variety of philosophical orientations (e.g., pragmatism, critical theory, the dialectic stance). We believe that it is both unwise, and unnecessary, at this time to exclude individuals from the MMR community because their conceptual frameworks are different. We agree with Denzin's (2008, p. 322) paraphrase of a theme originally stated by Guba (1990): "A change in paradigmatic postures involves a personal odyssey; that is we each have a personal history with our preferred paradigm and this needs to be honored."

While paradigm pluralism is widely endorsed by many mixed methods scholars, theoretical and conceptual dialogues related to MMR have been, and will continue to be, of great importance. Recent developments and controversies in this area are summarized later.

The third characteristic of contemporary MMR is *a celebration of diversity at all levels of the research enterprise* from the broader more conceptual dimensions to the narrower more empirical ones. This is demonstrated in methodological eclecticism and paradigm pluralism, but also extends to other issues. For example, MMR can simultaneously address a diverse range of confirmatory and exploratory questions,[7] while single approach studies often address only one or the other. Additionally, MMR provides the opportunity for an assortment of divergent views in conclusions and inferences due to the complexity of the data sources and analyses.

MMR emerged partially out of the literature on triangulation (e.g., Campbell & Fiske, 1959; Denzin, 1978; Patton, 2002) and has commonly been associated with the *convergence* of results from different sources. Nevertheless, there is a growing awareness (e.g., Erzberger & Kelle, 2003; Greene, 2007; Johnson & Onwuegbuzie, 2004; Tashakkori & Teddlie, 2008) that an equally important result of combining information from different sources is divergence or dissimilarity, which can then provide greater insight into complex aspects of the same phenomenon and/or to the design of a new study or phase for further investigation.

The fourth characteristic of contemporary MMR is *an emphasis on continua rather than a set of dichotomies.* A hallmark of MMR is its replacement of the "either-or" with continua that describe a range of options (e.g., Newman, Ridenour, Newman, & DeMarco, 2003; Patton, 1980, 1990, 2002; Ridenour

& Newman, 2008; Tashakkori & Teddlie, 2003c). For example, we have applied what we called the QUAL-MIXED-QUAN multidimensional continuum to a variety of research issues including statement of purpose, research questions, designs, sampling, data collection and analysis, and validity or inference quality (Teddlie & Tashakkori, 2009). The either-or dichotomies (e.g., explanatory or exploratory questions, statistical or thematic analyses) have been replaced with a range of options (including integrated questions and innovative methods for mixed data analysis).

The fifth characteristic of contemporary MMR *is an iterative, cyclical approach to research.* MMR is characterized by the cycle of research, which includes both deductive and inductive logic[8] in the same study (e.g., Krathwohl, 2004; Tashakkori & Teddlie, 1998). The cycle may be seen as moving from grounded results (facts, observations) through inductive logic to general inferences (abstract generalizations or theory), then from those general inferences (or theory) through deductive logic to tentative hypotheses or predictions of particular events/outcomes. Research may start at any point in the cycle: Some researchers start from theories or abstract generalizations, while others start from observations or other data points. This cycle may be repeated iteratively as researchers seek deeper levels of a phenomenon. We believe that all research projects go through a full cycle at least once, regardless of their starting point (e.g., Teddlie & Tashakkori, 2009).

This cyclical approach to research may also be conceptualized in terms of the distinction between

- the *context or logic of justification*—the process associated with the testing of predictions, theories, and hypotheses, and
- the *context or logic of discovery*—the process associated with understanding a phenomenon in more depth, the generation of theories and hypotheses.

While several authors writing in MMR acknowledge the logic of justification as a key part of their research, they also emphasize the importance of the *context of discovery,* which involves creative insight possibly leading to new knowledge (e.g., Hesse-Biber, 2010; Johnson & Gray, 2010; Teddlie & Johnson, 2009). This discovery component of MMR often, but not always, comes from the emergent themes associated with QUAL data analysis.

We also conceptualize the cyclical nature of research as a kind of "ebb and flow" that characterizes some of the *signature MMR processes,* such as sequential research designs. More details on these signature MMR processes are presented later in this section.

The sixth characteristic endorsed by many writing in MMR is *a focus on the research question (or research problem) in determining the methods/approaches employed within any given study* (e.g., Bryman, 2006; Johnson & Onwuegbuzie, 2004; Tashakkori & Teddlie, 1998). This *centrality of the research question* was initially intended to move researchers (particularly novice researchers) beyond intractable philosophical issues associated with the paradigms debate and toward the selection of methods that were best suited for their investigations.

Much has been written about the starting point for research; that is, do researchers start with a worldview or conceptual problem, a general purpose for conducting research, a research question, or some combination thereof? Newman et al. (2003) have argued convincingly that during the past four decades the research purpose has gained in importance relative to the research question. We maintain, however, that once a researcher has decided what she is interested in studying (e.g., what motivates the study, purpose, personal/political agenda, etc.), the specifics of her research questions will determine the choice of the best tools to use and how to use them. Experienced researchers are well aware of the fact that research questions undergo (often small) modifications and refocusing during the course of a study. Nevertheless, research questions generally direct the path of a research project.

MMR questions are usually broad, calling for both in-depth, emergent QUAL data *and* focused and preplanned QUAN data. These broad "umbrella" questions are often followed by more specific subquestions. In some (sequential) MMR projects, however, mixed questions emerge after the data are collected and analyzed, rather than being stated as initial "umbrella" questions. For example, a broad and emergent question may be asked and answered by collecting and analyzing QUAL data, followed by a question regarding the pervasiveness of the findings in a broader context or with regard to generaliziblity to a population. Despite the emergent (or sometimes preplanned) sequence in these MMR studies, both groups of findings must be incorporated toward broader understandings (i.e., meta-inferences).

The seventh characteristic of contemporary MMR is a *set of basic research designs and analytical processes,* most of which are agreed upon, although they go by different names and diagrammatic illustrations. For example, we refer to *parallel mixed designs* (Teddlie & Tashakkori, 2009, p. 341, italics in original) as,

> a family of MM designs in which mixing occurs in an independent manner either simultaneously or with some time lapse. The QUAL and QUAN *strands* are planned and implemented in order to answer related aspects of the same questions.

These designs have also been referred to as concurrent, simultaneous, and *triangulation designs* (Creswell & Plano Clark, 2007, p. 85), but there is much commonality across their definitions.

Earlier in this section, we referred to *signature MMR design and analysis processes,* such as sequential mixed designs or conversion procedures. We call these design and analysis processes "signature" terms because they help to define MMR in relation to QUAN or QUAL methods; that is, they are unique to MMR and help set this approach apart from the other two. These signature design and analysis processes include the following:

- *Sequential mixed designs* "are a family of MM designs in which mixing occurs across chronological phases (QUAL, QUAN) of the study; questions or procedures of one *strand* emerge from or are dependent on the previous strand; *research questions* are built upon one another and may evolve as the study unfolds" (Teddlie & Tashakkori, 2009, p. 345, italics in original).
- *Quantitizing* refers to the process of converting qualitative data to numerical codes that can be statistically analyzed (e.g., Miles & Huberman, 1994; Sandelowski, Voils, & Knafl, 2009).
- *Qualitizing*[9] refers to the process by which quantitative data are transformed into data that can be analyzed qualitatively (e.g., Tashakkori & Teddlie, 1998).

More signature designs and analytical procedures indigenous to MMR are discussed later. While there is general agreement about the existence of these unique MMR design and analytical processes, there is considerable disagreement about terminology and definitions, and these disagreements widen as more complex typologies are generated. For example, many believe that a complete typology of MMR designs is not possible due to the emergent nature of the QUAL component of the research and the ability of MMR designs to mutate, while others seek agreement on a set number of basic designs for the sake of simplicity and pedagogy.

The eighth contemporary characteristic of MMR is *a tendency toward balance and compromise that is implicit within the "third methodological community."* MMR is based on rejecting the either-or of the incompatibility thesis; therefore, we as a community are inclined toward generating a balance between the excesses of both the QUAL and QUAN orientations, while forging a unique MMR identity. This balance is in keeping with Johnson and Onwuegbuzie's (2004) depiction of pragmatism as seeking a middle ground between philosophical dualisms and finding workable solutions for seemingly insoluble conceptual disputes.

In this context, we refer again to Denzin's (2008) paraphrase of three of Guba's (1990) themes regarding paradigms:

- "There needs to be decline in confrontationalism by alternative paradigm proponents."
- "Paths for fruitful dialog between and across paradigms need to be explored."
- "The three main interpretive communities . . . must learn how to cooperate and work with one another" (Denzin, 2008, p. 322).

We believe that most mixed methods researchers are in agreement with these themes that call for compromise in dialogues among the three methodological communities.

▣ Theoretical and Conceptual Issues in Mixed Methods Research

While there is agreement on some broad characteristics of MMR, there are several ongoing dialogues regarding basic theoretical and conceptual issues within MMR. We concentrate on two: (1) issues related to the paradigms, which are also referred to by several other terms such as stances, approaches, frameworks, perspectives, mental models, and so forth and (2) issues related to the language of MMR.

Issues Related to the Use of Paradigms (or Conceptual Frameworks or Mental Models)

In this section, we first provide more details on the concept of paradigm pluralism. Then we present three alternative paradigmatic positions for MMR, followed by a discussion of some arguments against the continued focus on paradigm issues.

We presented paradigm pluralism as one of the contemporary characteristics of MMR earlier. The belief that multiple paradigms may serve as the underlying conceptual framework for MMR is a practical solution to some thorny philosophical and conceptual issues: Researchers simply use the philosophical framework that best fits their particular "intellectual odyssey."

Most MMR scholars can agree with paradigm pluralism as a starting point, but then they have to (1) consider the alternative paradigmatic positions and (2) *ascertain which of those positions is most closely related to their own perspective.* The following three paradigmatic positions[10] are the most widely accepted in contemporary MMR:

- pragmatism and its interpretations,
- frameworks associated with the axiological assumption (Mertens, 2007), and
- the dialectical stance, which involves using multiple assumptive frameworks within the same study (e.g., Greene, 2007; Greene & Caracelli, 2003).

Before examining these positions further, we need to briefly reconsider the ramifications of paradigm pluralism, which was posited in opposition to the single paradigm-single method thesis (e.g., postpositivism and QUAN methods; constructivism and QUAL methods). Denzin (2008, p. 317) considers the rejection of the single paradigm-single method thesis to be historical:

> When the field went from one to multiple epistemological paradigms, many asserted that there was incompatibility between and across paradigms, not just incompatibility between positivism and its major critic, constructivism. . . . Ironically, as this discourse evolved, the complementary strengths thesis emerged, and is now accepted by many in the mixed-methods community. Here is

where history starts to be rewritten. That is multiple paradigms can be used in the same mixed-methods inquiry. . . . Thus the demise of the single theoretical and /or methodological paradigm was celebrated.

It is important to realize that Denzin's analysis emphasizes not only paradigm pluralism, but also that researchers *may use multiple frameworks in the same study,* which is supported by only one of the contemporary positions noted above (the dialectical stance). Researchers who prescribe to pragmatism or a framework based on the axiological assumption typically use only that perspective in their research.

Pragmatism and Its Interpretations

There is an affinity for pragmatism as the paradigm of choice for many mixed methodologists (e.g., Tashakkori & Teddlie, 1998). This affinity is a historical one going back to Howe's (1988) postulation of the compatibility thesis based on pragmatism. The pragmatic approach to philosophical issues is appealing to many applied scientists who utilize a kind of "everyday pragmatism" (Biesta, 2010) in their solution of research and evaluation problems.

A more *philosophically nuanced pragmatism* has emerged recently (e.g., Biesta, 2010; Greene & Hall, 2010; Johnson & Onwuegbuzie, 2004; Maxcy, 2003; Teddlie & Tashakkori, 2009). This pragmatism asks, "Apart from the rejection of the either-or, what does pragmatism mean for MMR?" We briefly describe three recent interpretations of pragmatism (Johnson and colleagues; Biesta, 2010; Greene, 2007) that have advanced the conversation.

Johnson and colleagues have ventured into a kind of "paradigm or systems building" with regard to *philosophical pragmatism.* Johnson and Onwuegbuzie (2004) presented 21 characteristics of pragmatism in an effort to more completely delineate the tenets of this philosophy and how they relate to MMR.

Johnson et al. (2007) defined three pragmatisms: of the right, of the left, and of the center (*classical pragmatism*). Johnson (2009, p. 456) further defined *dialectical pragmatism* as a "supportive philosophy for mixed methods research" that combines classical pragmatism with Greene's (2007) dialectical approach. The cumulative contribution of Johnson and colleagues' work is that we now have a clearly articulated and detailed account of pragmatism as it relates to MMR.

In contrast, Biesta (2010, p. 97) contends that "pragmatism should not be understood as a philosophical position among others, but rather as a set of philosophical tools that can be used to address problems." Biesta emphasizes that John Dewey warned against philosophical system building. Biesta concludes that Deweyan pragmatism contributes to the dismantling of the epistemological dualism of objectivity/subjectivity:

> The major contribution of Dewey is that he engages with this discussion from a different starting point so that the either/or of objectivism and subjectivism loses its meaning. . . . This is tremendously important for the field of mixed methods research as it does away with alleged hierarchies between different approaches and rather helps to make the case that different approaches generate *different* outcomes, *different* connections between doing and undergoing, between actions and consequences, so that we always need to judge our knowledge claims pragmatically, that is in relation to the processes and procedures through which the knowledge has been generated. (Biesta, 2010, p. 113, italics in original)

Biesta concludes that philosophical pragmatism leads us to understand that no methodological approach is intrinsically better than another in knowledge generation. We have to evaluate the results from our research studies in terms of how good a job we did in selecting, utilizing, and integrating all the available methodological tools. Did we succeed in our efforts at *methodological eclecticism?*

Greene (2007) referred to pragmatism as the *alternative paradigm* (to the dominant traditional ones) that promotes the active mixing of methods and integration of research findings. Greene and Hall (2010) further described how thinking pragmatically affects the manner in which mixed researchers conduct their research. For Greene and Hall and others (e.g., Biesta, 2010; Johnson & Onwuegbuzie, 2004), pragmatism results in a problem-solving, action-oriented inquiry process based on a commitment to democratic values and progress.

Frameworks Associated With the Axiological Assumption

Mertens (2007) identified four basic assumptions associated with paradigms that were previously delineated by Guba and Lincoln (2005): axiological, epistemological, ontological, and methodological. Mertens, Bledsoe, Sullivan, & Wilson (2010, p. 195) further described the *axiological assumption* that "takes precedence and serves as a basis for articulating the other three belief systems because the transformative paradigm emerged from the need to be more explicit about how researchers can address issues of social justice." The axiological assumption is based on "power differences and ethical implications that derive from those differences" between marginalized and other groups (Mertens et al., p. 195).

In discussions of pragmatism, the philosophical issues that are emphasized are epistemological in nature concerning issues such as what is knowledge, how is it acquired, and the relationship between the knower and "known." On the other hand, scholars working within transformative or critical frameworks (e.g., feminism) give precedence to axiological considerations, which center on the nature of value judgments. This *axiological assumption* means that scholars working within transformative/critical frameworks have a different perspective on research

methods. For these scholars, mixed methods are tools that are used in the service of value systems that are always foremost.

The Dialectic Stance or Way of Thinking

The dialectic stance assumes that all paradigms have something to offer, and that employing multiple paradigms contributes to greater understanding of phenomena under study. Pragmatism and axiologically oriented frameworks utilize one perspective exclusively, while the dialectical stance calls for the juxtaposition of multiple assumptive frameworks within the same study. Greene (2007, p. 114) expresses it thus:

> I have adopted the stance that method cannot be divorced from the inquirer's assumptions about the world and about knowledge, the inquirer's theoretical predispositions, professional experience, and so forth. . . . So when one mixes methods, one may also mix paradigmatic and mental model assumptions as well as broad features of inquiry methodology.

Greene's dialectical stance directs attention away from the so-called incommensurable attributes of paradigms and toward different and distinctive (but not inherently incompatible) attributes such as distance-closeness, outside-insider, emic and etic, particularity and generality, and so forth. Greene and Hall's (2010) dialectical stance agrees with Biesta's (2010) pragmatism in that these philosophical systems are *not* "paradigm packages" with interlocking philosophical assumptions or beliefs.

Arguments Against the Continued Focus on "Paradigms"

The term "paradigm" has played a crucial role in the development of the three methodological communities since the initial publication of Kuhn's (1962) *The Structure of Scientific Revolutions.* Recently, authors have expressed increasing doubt about the utility of the continued focus on paradigm issues in MMR. For instance, Bazeley (2009, p. 203) concluded that "Although the epistemological arguments of the 'paradigm wars' sharpened our thinking about issues related to mixed methodology, their lingering legacy has been to slow the progress of integration of methods."

Morgan (2007) deconstructed the term "paradigm" into four possible (and not mutually exclusive) positions:

- paradigms as worldviews (ways of perceiving and experiencing the world),
- paradigms as epistemological stances (which Morgan called the *metaphysical paradigm*),
- paradigms as model examples (i.e., "exemplars" demonstrating how research is conducted), and
- paradigms as shared beliefs about types of questions, methods of study (and so on) among a community of scholars or within a field of study.

Morgan argued that Guba and Lincoln (e.g., Lincoln & Guba, 1985; Guba & Lincoln, 1994, 2005) used the metaphysical paradigm to draw attention to QUAL research as an alternative to QUAN research. This metaphysical version focused on the basic assumptions or beliefs noted above, with a special emphasis on epistemological considerations, drawing essential, incommensurable differences between the QUAL and QUAN perspectives thereby leading to the paradigm wars.

Morgan further argued that now is the time to move from what he considers the outmoded concept of *metaphysical paradigms*[11] to paradigms as *shared beliefs in a research field* due to conceptual problems with the former position (e.g., a *strong* stand on incommensurability) and to the fact that the latter position is a more accurate interpretation of Kuhn's use of the term.[12] Morgan's focus on shared beliefs in a research field has contributed to an increasing emphasis on the *community of scholars* perspective (e.g., Creswell, 2010; Tashakkori & Creswell, 2008), which is a position that has been reinforced by Denscombe's (2008) discussion of the nature that such a community might take.

The Language of Mixed Methods Research

We previously identified the language of mixed methods as one of the major issues in MMR (Teddlie & Tashakkori, 2003). At that time, we distinguished between MMR using a *bilingual language* that combined QUAL and QUAN terms or generating a *new language* with terms unique to the field itself. Since that time, we have seen manifestations of both tendencies.

For instance, we recently (Teddlie & Tashakkori, 2009, p. 282) generated a list of common analytical processes used in both QUAL and QUAN research that are examples of a bilingual language. These processes are cognitively interchangeable, although one uses numbers and the other employs words as data. For example, a bilingual mixed methods researcher knows that cluster analysis employs the same *modus operandi* as the categorizing process of the constant comparative method: that is, maximizing between-group variation and minimizing within-group variation. Other examples include comparing analyses from one part of a sample with analyses from another part of the sample; comparison of actual results with expected results; and contrasting components of research design or elements to find differences.

Recognition of these common processes is a step in the direction of developing a language that crosses methodological lines. On the other hand, Box 16.1 presents a partial list of unique terms related to mixed methods data analysis that have emerged since the 1990s. The emergence of new analytical processes constitutes one of the most creative areas in MMR and often comes from researchers working on practical solutions for answering their research questions using available QUAL and QUAN data. Using mixed data analysis as an example, it appears that the language used in MMR will involve both bilingual terms and unique mixed terms (e.g., Box 16.1).

Box 16.1 Partial List of Data Analysis Terms Indigenous to Mixed Methods Research

A partial list of MMR data analysis terms includes

- crossover track analysis
- data conversion or transformation
- data importation
- fully integrated mixed data analysis
- fused data analysis
- inherently mixed data analysis
- integrated data display
- integrated data reduction
- iterative sequential mixed analysis
- morphed data analysis
- multilevel mixed data analysis
- narrative profile formation
- parallel mixed data analysis
- parallel track analysis
- qualitizing
- quantitizing
- sequential mixed data analysis
- single track analysis
- typology development
- warranted assertion analysis

These terms were generated or employed by several authors, including Bazeley, 2003; Caracelli & Greene, 1993; Greene, 2007; Greene, Caracelli, & Graham, 1989; Li, Marquart, & Zercher, 2000; Onwuegbuzie & Combs, 2010; Onwuegbuzie, Johnson, & Collins, 2007; Onwuegbuzie & Teddlie, 2003; Tashakkori & Teddlie, 1998; Teddlie & Tashakkori, 2003, 2009.

Box 16.2 How Worldviews Affect MMR Praxis

There is general agreement that a researcher's worldview affects the manner in which that person conducts his or her research, yet there have been few explicit discussions of how that occurs in MMR. Several of the chapters in the recently published second edition of the *SAGE Handbook of Mixed Methods in Social & Behavioral Research* presented detailed versions of actual or hypothetical researchers and how they conducted MMR within a particular worldview (or assumptive framework or mental mode). These chapters included

- Greene and Hall (2010) described Michelle (a hypothetical researcher), who is conducting research on the interactions among middle school children as they go through their daily routines. Michelle's perspective is that of the *dialectic inquirer* who is attuned to the values underlying the multiple philosophical frameworks (constructivist epistemology, feminist ideology) that guide her research.
- Greene and Hall (2010) described Juan (another hypothetical researcher), whose perspective is that of a *pragmatic inquirer* who is studying schools that are struggling to simultaneously serve the needs of their diverse study bodies and to meet the accountability mandates of NCLB (No Child Left Behind).
- Hesse-Biber's (2010) description of research conducted within the *feminist tradition,* including studies as diverse as forestland usage in Nepal and sex work in Tijuana.
- Mertens and colleagues' (2010) description of research conducted within the *transformative paradigm tradition,* including studies on inclusive education for disabled people in New Zealand and poverty reduction in Rwanda.

Issues in Conducting Mixed Methods Research

Issues related to how to conduct MMR appear to have gained in importance relative to discussions of theoretical and conceptual issues recently. This trend is probably a reflection of the growing acceptance of MMR as a distinct methodological orientation and increased curiosity regarding the specifics of exactly how such research is conducted, disseminated, and utilized.

Chapters in the second edition of the *SAGE Handbook of Mixed Methods in Social & Behavioral Research* not only describe how to do MMR, but also illustrate how researchers' worldviews affect the manner in which they conduct their research. Box 16.2 presents information on how the different paradigmatic orientations summarized in the previous section of this chapter affect MMR praxis.

While there are several broad issues in conducting MMR (from generating research questions through making inferences from integrated data analyses), we can only discuss a couple here. We have selected research design, because there has been substantial work done in this area, and data analysis, because this is an area where considerable creative energy is currently being expended.

The Design of Mixed Methods Studies: A Diversity of Options

Design typologies have long been an important feature of MMR starting with Greene et al. (1989) writing in the field of evaluation and Morse (1991) in nursing. The reasons for the importance of MMR design typologies include their role in establishing a common language for the field, providing possible blueprints for researchers who want to employ MMR designs, legitimizing MMR by introducing designs that are clearly distinct from those in QUAN or QUAL research, and providing useful tools for pedagogical purposes.

Recently, some authors have contended that there is an overemphasis on research design typologies (e.g., Adamson, 2004; Bazeley, 2009), arguing that other areas (e.g., data analysis) should be stressed more. While such design typologies may not be featured as extensively in future writing in the field, they will continue to be an essential element of MMR. This is partly due to the fact that many of the proposed data analysis procedures in MMR are actually design-bound; that is, they are related to a specific type (or family) of designs (e.g., sequential data analysis in sequential mixed methods designs).

While some authors argue for a set number of prespecified designs, others contend that MMR design typologies can never be exhaustive, due to the iterative nature of MMR research projects (i.e., new components or strands might be added during the course of a project). This is an important point, since many inexperienced researchers want a design "menu" from which to select the "correct" one, similar to those provided in QUAN research (e.g., Shadish et al., 2002). In contrast, researchers using mixed methods are encouraged to continuously reexamine the results from one strand of a study compared to the results from another, and make changes both in the design and data collection procedures accordingly.

Researchers seeking their own *MMR design family* have a variety of viable options in the current "marketplace" (e.g., Creswell & Plano Clark, 2007; Greene, 2007; Leech & Onwuegbuzie, 2009; Maxwell & Loomis, 2003; Morse, 1991, 2003; Teddlie & Tashakkori, 2009). Nastasi, Hitchcock, and Brown (2010) recently examined various design typologies,[13] divided them into basic and complex categories, and determined that they differed with regard to nine distinct criteria or dimensions.

While some find the lack of consensus regarding the specific number and types of designs disconcerting, we believe that this is a healthy sign and that the most useful of the typologies will survive. The ultimate value of these typologies lies in their ability to provide researchers with viable design options to choose from and build upon (i.e., modify, expand, combine) when they are planning or implementing their MMR studies. The diversity in design typologies can be best exemplified by briefly examining two points of view that are distinct and have continued to evolve since first introduced: those of Jennifer Greene and our own. Other perspectives are equally valuable, but we chose these two because they make particularly interesting contrasts.

Greene contends that researchers cannot divorce method from "assumptive frameworks" when designing MMR studies; therefore, she encourages mixing those frameworks in single research studies. Her designs are anchored in mixing methods for five basic purposes that emerged from Greene et al. (1989): triangulation, complementarity, development, initiation, and expansion. Caracelli and Greene (1993) distinguished between *component designs* in which the methods are connected or mixed only at the level of inference and *integrated designs* in which the methods are integrated throughout the course of the study.

Greene (2007) presented two examples of component designs (convergence, extension) and four examples of integrated designs (iteration, blending, nesting or embedding, mixing for reasons of substance or values). These six examples of MMR designs map onto the five basic purposes for mixing with each example aligned with one or two of the original purposes. Greene (2007, p. 129) concludes that designing a MMR study does *not* involve following a formula or set of prescriptions, but rather is "an artful crafting of the kind of mix that will best fulfill the intended purposes for mixing within the practical resources and contexts at hand."

In our approach to MMR, we have always treated design as separable from research purpose. That is not to deny the importance of purpose; obviously, if you did not have a purpose for doing a study, you would not be doing it. We think purpose is a complex, psycho-socio-political concept and we believe each individual has a multiplicity of purposes for doing research ranging from "advancing your career" to "understanding complex phenomena" to "improving society." These purposes are intertwined and often change over time.

Our design typology has evolved as MMR has developed over the past decade (Tashakkori & Teddlie, 1998, 2003c; Teddlie & Tashakkori, 2009). The base of our system is a three-stage model of the research process that evolved from Patton's (2002, p. 252) "pure and mixed strategies" for conducting research. These three stages are conceptualization (formulation of questions specific to the research study), experiential (methodological operations, data generation, analysis, and so forth), and inferential (emerging theories, explanations, inferences, and so forth). Mixed designs are those in which the QUAL and QUAN approaches are integrated across the three stages. There are (currently) four families of mixed methods designs in our typology: parallel, sequential, conversion, and fully integrated. These families are based on what we call "type of implementation process"; that is, how does the integration of the QUAL and QUAN strands actually occur when conducting a study. Increasingly, MMR studies seem to use a combination of the basic configurations, often leading to fully integrated designs with multiple types/sources of data.

Similar to Greene's perspective, we distinguish between whether integration occurs at only one stage of the process (for us, the experiential stage) or throughout the study. Our latest solution to this thorny issue is the distinction between mixed and *quasi-mixed designs;* the former was defined in the previous

paragraph, while we define the latter as designs in which two types of data are collected, but there is little or no integration of findings and *inferences* from the study.

Both of these perspectives regarding MMR designs

- reflect coherent and internally consistent perspectives,
- are currently viable as they continue to evolve in interesting ways related to changes in the field,
- are heuristic in terms of informing MMR dissertations and other projects, and
- have advanced the MMR designs literature over time (and have, themselves, evolved as a result).

In comparing our position regarding MMR design with hers, Greene (2007, p. 117) concluded,

> my own thinking about mixed methods design shares considerable intellectual space with those of Tashakkori and Teddlie, but also contains some differences. . . . There is certainly ample space in the contemporary mixed methods conversation for these complementary yet distinct sets of ideas.

Mixed Methods Data Analysis

Mixed methods data analyses are the processes whereby QUAN and QUAL data analysis strategies are combined, connected, or integrated in research studies (Teddlie & Tashakkori, 2009). Much creative energy is currently being expended on topics related to MMR data analysis, especially that involving integrated computer-generated applications (e.g., Bazeley, 2010). Bazeley (2009, p. 206) recently concluded that an indicator of the maturation of MMR would come when it moves from "a literature dominated by foundations and design typologies" toward a field "in which there are advances in conceptualization and breakthroughs derived from analytical techniques that support integration."

We limit our discussion of analysis issues to two topics foreshadowed in the previous section on the language of mixed methods: (1) the identification of analogous analytical processes in QUAL and QUAN research, and (2) the generation of a unique lexicon of MMR analysis procedures indigenous to the area. The analogous processes represent what Greene (2007, p. 155) called, "using aspects of the analytic framework of one methodological tradition within the analysis of data from another tradition."

In an early demonstration of this process, Miles and Huberman (1984/1994) took matrices from the QUAN tradition (e.g., contingency tables filled with numbers or percentages generated from chi-square analysis) and applied that framework to the QUAL tradition by crossing two dimensions and then completing the cells with narrative information. In one example, Miles and Huberman (1994) illustrated the implementation of a longitudinal school improvement project by using columns that represented years and rows that represented levels of intervention. Cross-case comparisons between schools demonstrated where there were differences in reform implementation between more and less successful schools.

Similarly, Onwuegbuzie (2003) applied the QUAN concept of effect sizes to generate an analogous QUAL typology including three broad categories (manifest, adjusted, and latent QUAL effect sizes). Effect sizes in QUAN research refer to the strength of the relationship between two numeric variables calculated by statistical indices. The generation of effect sizes in QUAL research is an analytical process in which the strength of the relationship between narrative variables is calculated after these variables have been quantitized.

In the future, we believe that MMR researchers will increasingly apply the analytical frameworks used in either the QUAL or QUAN tradition in developing analogous techniques within the other tradition. This requires both appropriate training in the QUAN and QUAL approaches and the ability to creatively see analogous processes from the mixed methods perspective.

Similarly, creative insight on the part of a variety of researchers has resulted in the lengthy list of data analysis terms indigenous to MMR in Box 16.1. These terms refer to general analytical processes (e.g., data conversion); specific techniques within more general analytical processes (e.g., crossover track analysis within parallel mixed data analysis); and complex iterative mixed data analyses utilizing multiple computer programs. Bazeley (2003, p. 385, italics added) has called the latter process *fused data analysis,* which she describes as follows:

> Software programs . . . offer . . . the capacity of qualitative data analysis (QDA) software to incorporate quantitative data into a qualitative analysis, and to transform qualitative coding and matrices developed from qualitative coding into a format which allows statistical analysis. . . . *The "fusing" of analysis then takes the researcher beyond blending of different sources to the place where the same sources are used in different but interdependent ways in order to more fully understand the topic at hand.*

Another noteworthy trend in mixed methods data analysis was discussed in the section on the third characteristic of contemporary MMR: the celebration of diversity at all levels of the research enterprise. This characteristic is exemplified in mixed methods data analysis by the growing awareness that divergence of findings and inferences across the QUAL and QUAN strands is equally as informative as convergence (or even more so), because that divergence leads researchers to more complex understandings and toward further research studies.

▣ Critiques of Mixed Methods Research

Several criticisms of MMR have been voiced, especially as the field has become more visible since the turn of the 21st century. In this section, we briefly review some of the most salient of those criticisms.

From a historical perspective, the most common criticism of MMR is the incompatibility thesis, which stated that it is inappropriate to mix QUAL and QUAN methods in the same study due to epistemological differences between the paradigms that are purportedly related to them (e.g., Howe, 1988). This issue was addressed in the discussion regarding the first contemporary characteristic of MMR, *methodological eclecticism,* which contends that we are free to combine the best methodological tools in answering our research questions. While the philosophical justification for methodological eclecticism is important, the historical argument against the incompatibility thesis is probably more compelling: Researchers have been fruitfully combining QUAL and QUAN methods throughout the history of the social and behavioral sciences resulting in multilayered research that is distinct from either QUAL or QUAN research alone.

Criticisms of MMR from the QUAL research and postmodern communities (e.g., Denzin & Lincoln, 2005; Howe, 2004; Sale, Lohfeld, & Brazil, 2002) have involved several issues, which have in turn been addressed by the MMR community (e.g., Creswell, Shope, Plano-Clark, & Green, 2006; Teddlie et al., 2008). Perhaps the most salient of these issues is the concern that MMR subordinates QUAL methods to a secondary position to QUAN methods. As noted in the first section of this chapter, we unequivocally express our regard for the powerful contributions of QUAL methods and interpret the overwhelming majority of truly mixed research as involving a thorough integration of both methods. Fortunately, recent literature (e.g., Creswell et al., 2006; Denzin, 2008) indicates that the QUAL and MMR communities can be involved in a productive discourse respectful of diverse viewpoints and cognizant of our many points of agreement.

Valuable criticisms of MMR include logistical ones (i.e., its implementation in actual research studies), including concerns about the costs of such research and about who does the research (e.g., teams of researchers, solo investigators). We believe that the employment of QUAL, QUAN, or MMR approaches in any given study depends on the research questions that are being addressed and that many issues are best and most efficiently answered using either the QUAL-only or QUAN-only approach. MMR techniques should be used only when necessary to adequately answer the research questions, because the mixed approach is inherently more expensive than the QUAL or QUAN alone orientations. Mixed studies take longer to conduct, which is a major issue for doctoral students, as well as researchers operating under stringent timelines to complete contracted work. Researchers bidding for contracts using MMR should be especially careful to provide accurate budgets for what it would take to do the work comprehensively, especially the QUAL component, which may involve time-consuming ethnographies. MMR projects that underestimate the time and money required to complete all components of the design will likely result in "QUAL-light" research that does not deliver what was promised.

As for who does the research, there is concern that a "minimal competence model or methodological" bilingualism is "superficial, perhaps even unworkable" (Denzin, 2008, p. 322). Issues of mixed methods pedagogy are beyond the scope of this chapter, but there is an active literature developing in this area (e.g., Christ, 2009, 2010; Creswell, Tashakkori, Jensen, & Shapley, 2003; Tashakkori & Teddlie, 2003b) that includes details on current MMR courses being taught and how they have evolved over time (Christ, 2010). The collaborative approach to MMR has been described by Shulha and Wilson (2003) and successful examples of it are found in the literature (e.g., Day, Sammons, & Qu, 2008).

In our discussion of "methodological connoisseurship," we indicated that mixed methodologists knowledgeably (and often intuitively) select the best techniques available to answer research questions that may evolve during the course of a research project. The question arises: How is such experience and judgment developed across diverse methods, especially in the QUAL area? There is no simple answer to this question, but we believe that a combination of coursework and field experiences is necessary to begin the journey toward "methodological connoisseurship." The field experiences are crucial and we advocate an active mentorship between professors who are mixed methodologists and their graduate students. Preferably, this mentorship would include field experiences in research projects where the professor is the principal investigator and/or dissertations in which the student is required to conduct extensive QUAL and QUAN research to answer different parts of the research questions being investigated. We have served on several dissertation committees where students have completed successful MMR projects and have begun their journey toward becoming "methodological connoisseurs." (See Schulenburg [2007] and Ivankova, Creswell, & Stick [2007], for examples of research articles based on mixed methods dissertations.)

Another criticism of MMR concerns the quality of the writing of many articles and chapters in the field. Leech (2010) conducted interviews with early developers of the field who concluded that authors need to do a better job of (1) expressing where their research fits within the current MMR literature; (2) presenting their own definition of MMR; (3) explaining where and how the mixing of methods occurred in their research; and (4) explicitly describing their philosophical orientation. Creswell (2009) has recently presented a preliminary "map" delineating subareas of MMR that should help authors in "locating" themselves within the field. The multiple definitions of MMR presented by Johnson et al. (2007) should help authors in describing their own perspectives, while the various design typologies offer options with regard to how authors can describe the mixing of methods in their research projects. Furthermore, the explicit delineation of at least three philosophical orientations in the field (pragmatism, frameworks associated

with the axiological assumption, the dialectic stance) with other emerging alternatives (e.g., critical realism) provides authors with alternative philosophical orientations from which to choose and then make explicit in their writings.

Finally, Freshwater (2007), Greene (2007), Greene and Hall (2010), and others have expressed a concern that MMR is prematurely headed toward some "fixed" unity or consensus for social inquiry that will preclude the consideration of and respect for multiple approaches. For example, Freshwater (2007, p. 141) criticizes the "idolatry of integration and coherence," which she sees as "rife throughout nursing and the healthcare literature." This concern is akin to the apprehension that Smith and Heshusius (1986) voiced about "closing down the conversation" with regard to the quantitative-qualitative debate. We can understand this concern intellectually, since one of the characteristics of MMR is a "tendency toward balance and compromise," but we do not see MMR as becoming a static unified approach toward social inquiry that will stifle diverse viewpoints.

Perhaps our confidence that MMR leads toward a "celebration of diversity at all levels of the research enterprise" comes from our experiences in editing two volumes of the *SAGE Handbook of Mixed Methods in Social & Behavioral Research,* which have presented

- a wide variety of philosophical and conceptual models for MMR,
- an increasingly diverse set of methodological tools that can be employed in all aspects of conducting integrated research, especially those related to data analysis and the inferential process, and
- a diversity of applications of MMR across disciplinary boundaries and within specific lines of research.

Closely linked to this perspective regarding the inherent diversity of MMR is our perception of it as an extension of everyday sense making. Everyday problem solvers (naïve researchers) use multiple approaches concurrently or in sequence, examine a variety of evidence in decision making (or even in forming impressions), and question the credibility of their impressions, conclusions, and decisions. Although using a different type of data, more sophisticated methods of analysis, and more stringent standards of evidence and inference, a mixed methods researcher (the *methodological connoisseur* described earlier) follows the same general path that is characterized by a reliance on diverse sources of evidence.

Where Will We Be in 10 Years?

It is always difficult to predict the future, especially for a field that has only formally emerged in the past 15 to 20 years. The following comments are, therefore, our best guesses based on what we see as the trajectory of the field and are presented with the acknowledgment that future historic events could radically change the course of MMR.

1. There will be a gradual acceptance of pragmatism as the primary philosophical orientation associated with MMR, just as constructivism is associated with QUAL research and postpositivism with QUAN methods. Philosophical pragmatism as it relates to MMR will be defined more precisely. Other philosophical points of view will exist along with pragmatism as a basis for MMR, and this will be acceptable due to the belief of most mixed methodologists in paradigm pluralism. There will be relatively less emphasis on discussion of theoretical and conceptual issues.

2. A generic set of MMR designs will emerge over time and will be popularized in textbooks. These designs will include "signature" designs plus others that will emerge. Debates about which typology (among the half dozen or so most well-known ones) will subside as this generic set of prototypical designs is popularized. There will be relatively less emphasis on discussion of design issues in MMR.

3. Analysis issues will become more important, fueled by advances in the computer analysis of mixed methods data (e.g., Bazeley, 2009, 2010). Within MMR, data will be conceptualized "less in terms of words or numbers and more in terms of transferable units of information" (Teddlie & Tashakkori, 2009, p. 283). Mixed methodologists will develop widely accepted principles of mixed methods data analysis that will supersede the typologies that currently exist. The development of these principles of mixed methods data analysis is crucial to the continuation of MMR as a separate methodological movement.

4. MMR will continue to be adopted throughout the social and behavioral sciences. The form that it takes within any particular discipline will depend on the existing conceptual and methodological orientations within those fields. A challenge for mixed methodologists will be to develop and maintain a "core identity" (e.g., a set of commonly understood methodological principles) that cuts across disciplinary lines.

5. An alternative future is for MMR to continue to pave the way for human sciences research to be more inclusive (eclectic) and research question oriented. This will result in fewer projects being identified as purely QUAL or QUAN, and more that are simply called "research projects" (not labeled specifically as MMR). Unless mixed methodologists develop a core identity of commonly understood methodological principles, it may simply be absorbed into this eclectic blend of research methodologies.

Notes

1. We wish to express our gratitude to Norman Denzin, Yvonna Lincoln, and Harry Torrance for their very helpful comments and suggestions on earlier versions of this chapter.

2. This definition was taken from *The American Heritage Dictionary of the English Language* (1969, p. 412).

3. Denzin and Lincoln (2005, p. 4) similarly refer to QUAL researchers as *bricoleurs,* who creatively use a variety of QUAL methodological practices.

4. We do not want readers to confuse our use of the term "connoisseur of methods" with the well-known "educational connoisseurship" of Eisner (1998), which involves the art of appreciation and is a "qualitative, artistically grounded approach to educational evaluation" (Eisner, 1979, p. 11).

5. We are not implying that causal effects are examined exclusively by QUAN research or causal mechanisms solely by QUAL research. There are many examples of QUAN results being used descriptively and of QUAL results employed in examining the causes of phenomena (e.g., Maxwell, 2004; Yin, 2003).

6. *Design quality* is the degree to which the investigator has utilized the most appropriate procedures for answering the research question(s) and implemented them effectively. It consists of *design suitability, fidelity, within-design consistency, and analytic adequacy* (Tashakkori & Teddlie, 2008).

7. We do not believe in the dichotomy of QUAL and QUAN approaches on the basis of type of questions. Both exploratory and confirmatory questions may be found in QUAN and in QUAL research.

8. Abductive logic is a third type of logic that occurs when a researcher observes a surprising event and then tries to determine what might have caused it (e.g., Erzberger & Kelle, 2003; Peirce, 1974). It is the process whereby a hypothesis is generated, so that the surprising event may be explained.

9. Quantitizing and qualitizing refer to techniques that convert a QUAN-only or QUAL-only into a MMR study. Some researchers within the QUAL community (e.g., poststructuralists) are unlikely to utilize these techniques.

10. Critical realism (Maxwell & Mittapalli, 2010) has recently been proposed as another framework for the use of mixed methods, but its inclusion is beyond the scope of this chapter.

11. In his critique of the metaphysical paradigm, Morgan (2007, p. 68) acknowledged the valuable contribution that it had made in shifting discussions from mechanical concerns about methods only to larger philosophical and conceptual issues.

12. These arguments then lead Morgan (2007, p. 68) to an alternative position, which he called the *pragmatic approach* that concentrates on "methodology as an area that connects issues at the abstract level of epistemology and the mechanical level of actual methods." Morgan's approach emphasizes issues such as abduction, intersubjectivity, and transferability that supersede the traditional dichotomies (e.g., induction/deduction).

13. Maxwell and Loomis (2003) presented a systemic perspective on research design in MMR that was *non-typological* in nature: the interactive model of design, which consisted of five components (i.e., purposes, conceptual framework, research questions, methods, validity).

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17

CASE STUDY

Bent Flyvbjerg[1]

[C]onduct has its sphere in particular circumstances. That is why some people who do not possess theoretical knowledge are more effective in action (especially if they are experienced) than others who do possess it. For example, suppose that someone knows that light flesh foods are digestible and wholesome, but does not know what kinds are light; he will be less likely to produce health than one who knows that chicken is wholesome.

—Aristotle

What Is a Case Study?

Definitions of "case study" abound. Some are useful, others not. Merriam-Webster's dictionary (2009) defines a case study straightforwardly as follows:

> *Case Study.* An intensive analysis of an individual unit (as a person or community) stressing developmental factors in relation to environment.

According to this definition, case studies focus on an "individual unit," what Robert Stake (2008, pp. 119–120) calls a "functioning specific" or "bounded system." The decisive factor in defining a study as a case study is the choice of the individual unit of study and the setting of its boundaries, its "casing" to use Charles Ragin's (1992, p. 217) felicitous term. If you choose to do a case study, you are therefore not so much making a methodological choice as a choice of what is to be studied. The individual unit may be studied in a number of ways, for instance qualitatively or quantitatively, analytically or hermeneutically, or by mixed methods. This is not decisive for whether it is a case study or not; the demarcation of the unit's boundaries is. Second, the definition stipulates that case studies are "intensive." Thus, case studies comprise more detail, richness, completeness, and variance—that is, depth—for the unit of study than does cross-unit analysis. Third, case studies stress "developmental factors," meaning that a case typically evolves in time, often as a string of concrete and interrelated events that occur "at such a time, in such a place" and that constitute the case when seen as a whole. Finally, case studies focus on "relation to environment," that is, context. The drawing of boundaries for the individual unit of study decides what gets to count as case and what becomes context to the case.

Against Webster's commonsensical definition of case study, the Penguin *Dictionary of Sociology* (Abercrombie, Hill, & Turner, 1984, p. 34; and verbatim in the 1994 and 2006 editions) has for decades contained the following highly problematic, but unfortunately quite common, definition of case study:

> *Case Study.* The detailed examination of a single example of a class of phenomena, a case study cannot provide reliable information about the broader class, but it may be useful in the preliminary stages of an investigation since it provides hypotheses, which may be tested systematically with a larger number of cases.

This definition is indicative of much conventional wisdom about case study research, which, if not directly wrong, is so oversimplified as to be grossly misleading. The definition promotes the mistaken view that the case study is hardly a methodology in its own right, but is best seen as subordinate to investigations of larger samples. Whereas it is correct that the case study is a "detailed examination of a single example," it is wrong that a case study "cannot provide reliable information about the broader class." It is also correct that a case study *can* be used "in the preliminary stages of an investigation" to generate hypotheses, but it is wrong to see the case study as a pilot

method to be used only in preparing the real study's larger surveys, systematic hypotheses testing, and theory building. The Penguin definition juxtaposes case studies with large-sample, statistical research in an unfortunate manner that blocks, instead of brings out, the productive complementarity that exists between the two types of methodology, as we will see below.

John Gerring (2004, p. 342) has correctly pointed out that the many academic attempts to clarify what "case study" means has resulted in a definitional morass, and each time someone attempts to clear up the mess of definitions it just gets worse. If we need a definition of what a case study is, we are therefore better off staying with commonsensical definitions like that from Webster's above than with more loaded academic definitions like that from the Penguin *Dictionary of Sociology.*

▣ The Case Study Paradox

Case studies have been around as long as recorded history and today they account for a large proportion of books and articles in psychology, anthropology, sociology, history, political science, education, economics, management, biology, and medical science. For instance, in recent years roughly half of all articles in the top political science journals have used case studies, according to Alexander George and Andrew Bennett (2005, pp. 4–5). Much of what we know about the empirical world has been produced by case study research, and many of the most treasured classics in each discipline are case studies.

But there is a paradox here. At the same time that case studies are widely used and have produced canonical texts, it may be observed that the case study as a methodology is generally held in low regard, or is simply ignored, within the academy. For example, only 2 of the 30 top-ranked U.S. graduate programs in political science require a dedicated graduate course in case study or qualitative methods, and a full third of these programs do not even offer such a course. In contrast, all of the top 30 programs offer courses in quantitative methods and almost all of them require training in such methods, often several courses (George & Bennett, 2005, p. 10). In identifying this paradox of the case study's wide use and low regard, Gerring (2004, p. 341) rightly remarks that the case study survives in a "curious methodological limbo," and that the reason is that the method is poorly understood.

In what follows, we will try to resolve Gerring's paradox and help case study research gain wider use and acceptance by identifying five misunderstandings about the case study that systematically undermine the credibility and use of the method. The five misunderstandings can be summarized as follows:

Misunderstanding No. 1	General, theoretical knowledge is more valuable than concrete case knowledge.
Misunderstanding No. 2	One cannot generalize on the basis of an individual case; therefore, the case study cannot contribute to scientific development.
Misunderstanding No. 3	The case study is most useful for generating hypotheses; that is, in the first stage of a total research process, while other methods are more suitable for hypotheses testing and theory building.
Misunderstanding No. 4	The case study contains a bias toward verification, that is, a tendency to confirm the researcher's preconceived notions.
Misunderstanding No. 5	It is often difficult to summarize and develop general propositions and theories on the basis of specific case studies.

The five misunderstandings may be said to constitute the conventional view, or orthodoxy, of the case study. We see that theory, reliability, and validity are at issue; in other words, the very status of the case study as a scientific method. In what follows, we will correct the five misunderstandings one by one and thereby clear the ground for a use of case study research in the social sciences that is based on understanding instead of misunderstanding.

▣ Misunderstanding No. 1

> General, theoretical knowledge is more valuable than concrete case knowledge.

In order to understand why the conventional view of case study research is problematic, we need to grasp the role of cases and theory in human learning. Here, two points can be made. First, the case study produces the type of concrete, context-dependent knowledge that research on learning shows to be necessary to allow people to develop from rule-based beginners to virtuoso experts. Second, in the study of human affairs, there appears to exist only context-dependent knowledge, which thus presently rules out the possibility for social science to emulate natural science in developing epistemic theory, that is, theory that is explanatory and predictive. The full argument behind these two points can be found in Flyvbjerg (2001, Chaps. 2–4). For reasons of space, I can only give an outline of the argument here. At the outset, however, we can assert that if the two points are correct, it will have radical consequences for the conventional view of the case study in research and teaching. This view would then be problematic.

Phenomenological studies of human learning indicate that for adults there exists a qualitative leap in their learning process from the rule-governed use of analytical rationality in beginners to the fluid performance of tacit skills in what Pierre Bourdieu (1977) calls virtuosos and Hubert and Stuart Dreyfus (1986), true human experts. Here we may note that most people are experts in a number of everyday social, technical, and intellectual skills like giving a gift, riding a bicycle, or interpreting images on a television screen, while only few reach the level of true expertise for more specialized skills like playing chess, composing a symphony, or flying an airplane.

Common to all experts, however, is that they operate on the basis of intimate knowledge of several thousand concrete cases in their areas of expertise. Context-dependent knowledge and experience are at the very heart of expert activity. Such knowledge and expertise also lie at the center of the case study as a research and teaching method; or to put it more generally yet—as a method of learning. Phenomenological studies of the learning process therefore emphasize the importance of this and similar methods; it is only because of experience with cases that one can at all move from being a beginner to being an expert. If people were exclusively trained in context-independent knowledge and rules, that is, the kind of knowledge that forms the basis of textbooks, they would remain at the beginner's level in the learning process. This is the limitation of analytical rationality; it is inadequate for the best results in the exercise of a profession, as student, researcher, or practitioner.

In teaching situations, well-chosen case studies can help students achieve competence, while context-independent facts and rules will bring students just to the beginner's level. Only few institutions of higher learning have taken the consequence of this. Harvard University is one of them. Here both teaching and research in the professional schools are modeled to a wide extent on the understanding that case knowledge is central to human learning (Christensen & Hansen, 1987; Cragg, 1940).

It is not that rule-based knowledge should be discounted; such knowledge is important in every area and especially to novices. But to make rule-based knowledge the highest goal of learning is topsy-turvy. There is a need for both approaches. The highest levels in the learning process, that is, virtuosity and true expertise, are reached only via a person's own experiences as practitioner of the relevant skills. Therefore, beyond using the case method and other experiential methods for teaching, the best that teachers can do for students in professional programs is to help them achieve real practical experience, for example, via placement arrangements, internships, summer jobs, and the like.

For researchers, the closeness of the case study to real-life situations and its multiple wealth of details are important in two respects. First, it is important for the development of a nuanced view of reality, including the view that human behavior cannot be meaningfully understood as simply the rule-governed acts found at the lowest levels of the learning process, and in much theory. Second, cases are important for researchers' own learning processes in developing the skills needed to do good research. If researchers wish to develop their own skills to a high level, then concrete, context-dependent experience is just as central for them as to professionals learning any other specific skills. Concrete experiences can be achieved via continued proximity to the studied reality and via feedback from those under study. Great distance from the object of study and lack of feedback easily lead to a stultified learning process, which in research can lead to ritual academic blind alleys, where the effect and usefulness of research becomes unclear and untested. As a research method, the case study can be an effective remedy against this tendency.

The second main point in connection with the learning process is that there does not and probably cannot exist predictive theory in social science. Social science has not succeeded in producing general, context-independent theory and has thus in the final instance nothing else to offer than concrete, context-dependent knowledge. And the case study is especially well suited to produce this knowledge. In his later work, Donald Campbell (1975, p. 179) arrives at a similar conclusion. Earlier, he (Campbell and Stanley, 1966, pp. 6–7) had been a fierce critic of the case study, stating that "such studies have such a total absence of control as to be of almost no scientific value." Now he explained that his work had undergone "an extreme oscillation away from my earlier dogmatic disparagement of case studies." Using logic that in many ways resembles that of the phenomenology of human learning, Campbell explains,

> After all, man is, in his ordinary way, a very competent knower, and qualitative common-sense knowing is not replaced by quantitative knowing.... This is not to say that such common sense naturalistic observation is objective, dependable, or unbiased. But it is all that we have. It is the only route to knowledge—noisy, fallible, and biased though it be. (1975, pp. 179, 191)

Campbell is not the only example of a researcher who has altered his views about the value of the case study. Hans Eysenck (1976, p. 9), who originally saw the case study as nothing more than a method of producing anecdotes, later realized that "sometimes we simply have to keep our eyes open and look carefully at individual cases—not in the hope of proving anything, but rather in the hope of learning something!" Final proof is hard to come by in social science because of the absence of "hard" theory, whereas learning is certainly possible. More recently, similar views have been expressed by Charles Ragin, Howard Becker, and their colleagues in explorations of what the case study is and can be in social inquiry (Ragin & Becker, 1992).

As for predictive theory, universals, and scientism, so far social science has failed to deliver. In essence, we have only specific cases and context-dependent knowledge in social science.

The first of the five misunderstandings about the case study—that general theoretical (context-independent) knowledge is more valuable than concrete (context-dependent) case knowledge—can therefore be revised as follows:

> *Predictive theories and universals cannot be found in the study of human affairs. Concrete case knowledge is therefore more valuable than the vain search for predictive theories and universals.*

▪ Misunderstanding No. 2

> One cannot generalize on the basis of an individual case; therefore, the case study cannot contribute to scientific development.

The view that one cannot generalize on the basis of a single case is usually considered to be devastating to the case study as a scientific method. This second misunderstanding about the case study is typical among proponents of the natural science ideal within the social sciences. Yet even researchers who are not normally associated with this ideal may be found to have this viewpoint. According to Anthony Giddens, for example,

> Research which is geared primarily to hermeneutic problems may be of generalized importance in so far as it serves to elucidate the nature of agents' knowledgeability, and thereby their reasons for action, across a wide range of action-contexts. Pieces of ethnographic research like . . . say, the traditional small-scale community research of fieldwork anthropology—are not in themselves generalizing studies. But they can easily become so if carried out in some numbers, so that judgements of their typicality can justifiably be made. (1984, p. 328)

It is correct that one can generalize in the ways Giddens describes, and that often this is both appropriate and valuable. But it would be incorrect to assert that this is the only way to work, just as it is incorrect to conclude that one cannot generalize from a single case. It depends upon the case one is speaking of, and how it is chosen. This applies to the natural sciences as well as to the study of human affairs (Platt, 1992; Ragin & Becker, 1992).

For example, Galileo's rejection of Aristotle's law of gravity was not based upon observations "across a wide range," and the observations were not "carried out in some numbers." The rejection consisted primarily of a conceptual experiment and later of a practical one. These experiments, with the benefit of hindsight, are self-evident. Nevertheless, Aristotle's view of gravity dominated scientific inquiry for nearly 2,000 years before it was falsified. In his experimental thinking, Galileo reasoned as follows: If two objects with the same weight are released from the same height at the same time, they will hit the ground simultaneously, having fallen at the same speed. If the two objects are then stuck together into one, this object will have double the weight and will according to the Aristotelian view therefore fall faster than the two individual objects. This conclusion ran counter to common sense, Galileo found. The only way to avoid the contradiction was to eliminate weight as a determinant factor for acceleration in free fall. And that was what Galileo did. Historians of science continue to discuss whether Galileo actually conducted the famous experiment from the leaning tower of Pisa, or whether this experiment is a myth. In any event, Galileo's experimentalism did not involve a large random sample of trials of objects falling from a wide range of randomly selected heights under varying wind conditions, and so on, as would be demanded by the thinking of the early Campbell and Giddens. Rather, it was a matter of a single experiment, that is, a case study, if any experiment was conducted at all. (On the relation between case studies, experiments, and generalization, see Bailey, 1992; Griffin, Botsko, Wahl, & Isaac, 1991; Lee, 1989; Wilson, 1987.) Galileo's view continued to be subjected to doubt, however, and the Aristotelian view was not finally rejected until half a century later, with the invention of the air pump. The air pump made it possible to conduct the ultimate experiment, known by every pupil, whereby a coin or a piece of lead inside a vacuum tube falls with the same speed as a feather. After this experiment, Aristotle's view could be maintained no longer. What is especially worth noting in our discussion, however, is that the matter was settled by an individual case due to the clever choice of the extremes of metal and feather. One might call it a critical case: For if Galileo's thesis held for these materials, it could be expected to be valid for all or a large range of materials. Random and large samples were at no time part of the picture. Most creative scientists simply do not work this way with this type of problem.

Carefully chosen experiments, cases, and experience were also critical to the development of the physics of Isaac Newton, Albert Einstein, and Niels Bohr, just as the case study occupied a central place in the works of Charles Darwin. In social science, too, the strategic choice of case may greatly add to the generalizability of a case study. In their classical study of the "affluent worker," John Goldthorpe, David Lockwood, Frank Beckhofer, and Jennifer Platt (1968–1969) deliberately looked for a case that was as favorable as possible to the thesis that the working class, having reached middle-class status, was dissolving into a society without class identity and related conflict (see also Wieviorka, 1992). If the thesis could be proved false in the favorable case, then it would most likely be false for intermediate cases. Luton, then a prosperous industrial center outside of London with companies known for high wages and social stability—fertile ground for middle-class identity—was selected as a case, and through intensive fieldwork the researchers discovered that even here an autonomous working-class culture prevailed, lending general credence to the thesis

of the persistence of class identity. Below we will discuss more systematically this type of strategic sampling.

As regards the relationship between case studies, large samples, and discoveries, William Beveridge (1951; here quoted from Kuper & Kuper, 1985) observed immediately prior to the breakthrough of the quantitative revolution in the social sciences, "[M]ore discoveries have arisen from intense observation [of individual cases] than from statistics applied to large groups." This does not mean that the case study is always appropriate or relevant as a research method, or that large random samples are without value. The choice of method should clearly depend on the problem under study and its circumstances.

Finally, it should be mentioned that formal generalization, be it on the basis of large samples or single cases, is considerably overrated as the main source of scientific progress. Economist Mark Blaug (1980)—a self-declared adherent to the hypothetico-deductive model of science—has demonstrated that while economists typically pay lip service to the hypothetico-deductive model and to generalization, they rarely practice what they preach in actual research. More generally, Thomas Kuhn has shown that the most important precondition for science is that researchers possess a wide range of practical skills for carrying out scientific work. Generalization is just one of these. In Germanic languages, the term "science" (*Wissenschaft*) means literally "to gain knowledge." And formal generalization is only one of many ways by which people gain and accumulate knowledge. That knowledge cannot be formally generalized does not mean that it cannot enter into the collective process of knowledge accumulation in a given field or in a society. Knowledge may be transferable even where it is not formally generalizable. A purely descriptive, phenomenological case study without any attempt to generalize can certainly be of value in this process and has often helped cut a path toward scientific innovation. This is not to criticize attempts at formal generalization, for such attempts are essential and effective means of scientific development. It is only to emphasize the limitations, which follows when formal generalization becomes the only legitimate method of scientific inquiry.

The balanced view of the role of the case study in attempting to generalize by testing hypotheses has been formulated by Harry Eckstein:

> *[C]omparative and case studies are alternative means to the end of testing theories, choices between which must be largely governed by arbitrary or practical, rather than logical, considerations* . . . [I]t is impossible to take seriously the position that case study is suspect because problem-prone and comparative study deserving of benefit of doubt because problem-free. (1975, pp. 116, 131, emphasis in original; see also Barzelay, 1993)

Eckstein here uses the term "theory" in its "hard" sense, that is, comprising explanation and prediction. This makes Eckstein's dismissal of the view that case studies cannot be used for testing theories or for generalization stronger than my own view, which is here restricted to the testing of "theory" in the "soft" sense, that is, testing propositions or hypotheses. Eckstein shows that if predictive theories would exist in social science, then the case study could be used to test these theories just as well as other methods.

More recently, George and Bennett (2005) have demonstrated the strong links between case studies and theory development, especially through the study of deviant cases, and John Walton (1992, p. 129) has similarly observed that "case studies are likely to produce the best theory." Already, Eckstein noted, however, the striking lack of genuine theories within his own field, political science, but apparently failed to see why this is so:

> Aiming at the disciplined application of theories to cases forces one to state theories more rigorously than might otherwise be done—provided that the application is truly "disciplined," i.e., designed to show that valid theory compels a particular case interpretation and rules out others. As already stated, this, unfortunately, is rare (if it occurs at all) in political study. One reason is the lack of compelling theories. (1975, pp. 103–104)

The case study is ideal for generalizing using the type of test that Karl Popper called "falsification," which in social science forms part of critical reflexivity. Falsification is one of the most rigorous tests to which a scientific proposition can be subjected: If just one observation does not fit with the proposition, it is considered not valid generally and must therefore be either revised or rejected. Popper himself used the now famous example of "All swans are white," and proposed that just one observation of a single black swan, that is, one deviant case, would falsify this proposition and in this way have general significance and stimulate further investigations and theory building. The case study is well suited for identifying "black swans" because of its in-depth approach: What appears to be "white" often turns out on closer examination to be "black." Deviant cases and the falsifications they entail are main sources of theory development, because they point to the development of new concepts, variables, and causal mechanisms, necessary in order to account for the deviant case and other cases like it.

We will return to falsification in discussing the fourth misunderstanding of the case study below. For the present, however, we can correct the second misunderstanding—that one cannot generalize on the basis of a single case and that the case study cannot contribute to scientific development—so that it now reads:

> *One can often generalize on the basis of a single case, and the case study may be central to scientific development via generalization as supplement or alternative to other methods. But formal generalization is overvalued as a source of scientific development, whereas "the force of example" and transferability are underestimated.*

Misunderstanding No. 3

> The case study is most useful for generating hypotheses, while other methods are more suitable for hypotheses testing and theory building.

The third misunderstanding about the case study is that the case method is claimed to be most useful for generating hypotheses in the first steps of a total research process, while hypothesis-testing and theory-building is best carried out by other methods later in the process, as stipulated by the Penguin definition of case study at the beginning of this chapter. This misunderstanding derives from the previous misunderstanding that one cannot generalize on the basis of individual cases. And since this misunderstanding has been revised as above, we can now correct the third misunderstanding as follows:

> *The case study is useful for both generating and testing of hypotheses but is not limited to these research activities alone.*

Eckstein—contravening the conventional wisdom in this area—goes so far as to argue that case studies are better for testing hypotheses than for producing them. Case studies, Eckstein (1975, p. 80) asserts, "are valuable at all stages of the theory-building process, but most valuable at that stage of theory-building where least value is generally attached to them: the stage at which candidate theories are tested." George and Bennett (2005, pp. 6–9) later confirmed and expanded Eckstein's position, when they found that case studies are especially well suited for theory development because they tackle the following tasks in the research process better than other methods:

- Process tracing that links causes and outcomes (see Box 17.1)
- Detailed exploration of hypothesized causal mechanisms
- Development and testing of historical explanations
- Understanding the sensitivity of concepts to context
- Formation of new hypotheses and new questions to study, sparked by deviant cases

Even rational choice theorists have begun to use case study methods to test their theories and hypotheses, which, if anything, should help deflate the decades-old antagonism between quants and qualts over case study research (Bates, Greif, Levi, Rosenthal, & Weingast, 1998; Flyvbjerg, 2006).

Box 17.1 Falsifying Nobel Prize Theories Through Process Tracing

Some years ago, the editor of *Harvard Business Review* contacted me and asked for a comment on an article he was printing by Princeton psychologist Daniel Kahneman. The editor was puzzled by the fact that Kahneman's Nobel Prize–winning theories on decision making under uncertainty explained failure in executive decisions in terms of inherent optimism (Lovallo & Kahneman, 2003), whereas my group and I explained similar phenomena in terms of strategic misrepresentation, that is, lying as part of principal-agent behavior (Flyvbjerg, Holm, & Buhl, 2002). Who was right, the editor asked? Optimism is unintentional self-deception, whereas lying is intentional deception of others. The question therefore boiled down to whether deception, which caused failure—that much we agreed upon—was intentional or not. The statistical methods that both Kahneman and I had relied upon in our studies of deception could not answer this question. It was now necessary to process trace all the way into people's heads in order to understand whether intention was present or not. Through a number of case studies and interviews, my group and I established that deception is in fact often intentional, especially for very large and expensive decisions taken under political and organizational pressure. We thus falsified optimism as a global explanation of executive failure and developed a new and more nuanced theory that combines optimism and strategic misrepresentation in accounting for failure (Flyvbjerg, 2007).

Testing of hypotheses relates directly to the question of "generalizability," and this in turn relates to the question of case selection. Here, generalizability of case studies can be increased by the strategic selection of cases (for more on the selection of cases, see Ragin, 1992; Rosch, 1978). When the objective is to achieve the greatest possible amount of information on a given problem or phenomenon, a representative case or a random sample may not be the most appropriate strategy. This is because the typical or average case is often not the richest in information. Atypical or extreme cases often reveal more information because they activate more actors and more basic mechanisms in the situation studied. In addition, from both an understanding-oriented and an action-oriented perspective, it is often more important to clarify the deeper causes behind a given problem and its consequences than to describe the symptoms of the problem and how frequently they occur. Random samples emphasizing representativeness will seldom be able to produce this kind of insight; it is more appropriate to select some few cases chosen for their validity.

Table 17.1 summarizes various forms of sampling. The *extreme,* or *deviant, case* can be well suited for getting a point across in an especially dramatic way, which often occurs for well-known case studies such as Sigmund Freud's "Wolf-Man" and Michel Foucault's "Panopticon." The deviant case is also

Table 17.1 Strategies for the Selection of Samples and Cases

Type of Selection	*Purpose*
A. Random selection	To avoid systematic biases in the sample. The sample's size is decisive for generalization.
1. Random sample	To achieve a representative sample that allows for generalization for the entire population.
2. Stratified sample	To generalize for specially selected subgroups within the population.
B. Information-oriented selection	To maximize the utility of information from small samples and single cases. Cases are selected on the basis of expectations about their information content.
1. Extreme/deviant cases	To obtain information on unusual cases, which can be especially problematic or especially good in a more closely defined sense. To understand the limits of existing theories and to develop new concepts, variables, and theories that are able to account for deviant cases.
2. Maximum variation cases	To obtain information about the significance of various circumstances for case process and outcome; e.g., three to four cases that are very different on one dimension: size, form of organization, location, budget, etc.
3. Critical cases	To achieve information that permits logical deductions of the type, "If this is (not) valid for this case, then it applies to all (no) cases."
4. Paradigmatic cases	To develop a metaphor or establish a school for the domain that the case concerns.

particularly well suited for theory development, because it helps researchers understand the limits of existing theories and to develop the new concepts, variables, and theories that will be able to account for what were previously considered outliers.

In contrast, a *critical case* can be defined as having strategic importance in relation to the general problem. The above-mentioned strategic selection of lead and feather for the test of whether different objects fall with equal velocity is an example of critical case selection. This particular selection of materials provided the possibility to formulate a type of generalization that is characteristic of critical cases, a generalization of the sort, "If it is valid for this case, it is valid for all (or many) cases." In its negative form, the generalization would be, "If it is not valid for this case, then it is not valid for any (or only few) cases" (see also Box 17.2).

Box 17.2 Critical Case for Brain Damage

An occupational medicine clinic wanted to investigate whether people working with organic solvents suffered brain damage. Instead of choosing a representative sample among all those enterprises in the clinic's area that used organic solvents, the clinic strategically located a single workplace where all safety regulations on cleanliness, air quality, and the like, had been fulfilled. This model enterprise became a critical case: If brain damage related to organic solvents could be found at this particular facility, then it was likely that the same problem would exist at other enterprises that were less careful with safety regulations for organic solvents. Via this type of strategic choice, one can save both time and money in researching a given problem, and one may generalize in the following manner from a critical case: "If it is valid for this case, it is valid for all (or many) cases." In its negative form, the generalization would be, "If it is not valid for this case, then it is not valid for any (or only few) cases." In this instance, the occupational medicine clinic found brain damage related to organic solvents in the model enterprise and concluded that the problem needed to be dealt with in all enterprises in its jurisdiction.

How does one identify critical cases? This question is more difficult to answer than the question of what constitutes a critical case. Locating a critical case requires experience, and no universal methodological principles exist by which one can with certainty identify a critical case. The only general advice that can be given is that when looking for critical cases, it is a good idea to look for either "most likely" or "least likely" cases, that is, cases that are likely to either clearly confirm or irrefutably falsify propositions and hypotheses. A model example of a "least likely" case is Robert Michels's (1962) classic study of oligarchy in organizations. By choosing a horizontally structured grassroots organization with strong democratic ideals—that is, a type of organization with an especially low probability of being oligarchic—Michels could test the universality of the oligarchy thesis, that is, "If this organization is oligarchic, so are most others." A corresponding model example of a "most likely" case is

W. F. Whyte's (1943) study of a Boston slum neighborhood, which according to existing theory should have exhibited social disorganization, but in fact showed quite the opposite (see also the articles on Whyte's study in the April 1992 issue of the *Journal of Contemporary Ethnography*).

Cases of the "most likely" type are especially well suited to falsification of propositions, while "least likely" cases are most appropriate for tests of verification. It should be remarked that a most likely case for one proposition is the least likely for its negation. For example, Whyte's slum neighborhood could be seen as a least likely case for a hypothesis concerning the universality of social organization. Hence, the identification of a case as most or least likely is linked to the design of the study, as well as to the specific properties of the actual case.

A final strategy for the selection of cases is choice of the *paradigmatic case.* Thomas Kuhn has shown that the basic skills, or background practices, of natural scientists are organized in terms of "exemplars," the role of which can be studied by historians of science. Similarly, scholars like Clifford Geertz and Michel Foucault have often organized their research around specific cultural paradigms: A paradigm for Geertz lay for instance in the "deep play" of the Balinese cockfight, while for Foucault, European prisons and the "Panopticon" are examples. Both instances are examples of paradigmatic cases, that is, cases that highlight more general characteristics of the societies in question. Kuhn has shown that scientific paradigms cannot be expressed as rules or theories. There exists no predictive theory for how predictive theory comes about. A scientific activity is acknowledged or rejected as good science by how close it is to one or more exemplars, that is, practical prototypes of good scientific work. A paradigmatic case of how scientists do science is precisely such a prototype. It operates as a reference point and may function as a focus for the founding of schools of thought.

As with the critical case, we may ask, "How does one identify a paradigmatic case?" How does one determine whether a given case has metaphorical and prototypical value? These questions are even more difficult to answer than for the critical case, precisely because the paradigmatic case transcends any sort of rule-based criteria. No standard exists for the paradigmatic case because it sets the standard. Hubert and Stuart Dreyfus see paradigmatic cases and case studies as central to human learning. In an interview with Hubert Dreyfus (author's files), I therefore asked what constitutes a paradigmatic case and how it can be identified. Dreyfus replied,

> Heidegger says, you recognize a paradigm case because it shines, but I'm afraid that is not much help. You just have to be intuitive. We all can tell what is a better or worse case—of a Cézanne painting, for instance. But I can't think there could be any rules for deciding what makes Cézanne a paradigmatic modern painter. . . . [I]t is a big problem in a democratic society where people are supposed to justify what their intuitions are. In fact, nobody really can justify what their intuition is. So you have to make up reasons, but it won't be the real reasons.

One may agree with Dreyfus that intuition is central to identifying paradigmatic cases, but one may disagree it is a problem to have to justify one's intuitions. Ethnomethodological studies of scientific practice have demonstrated that all variety of such practice relies on taken-for-granted procedures that feel largely intuitive. However, those intuitive decisions are accountable, in the sense of being sensible to other practitioners or often explicable if not immediately sensible. That would frequently seem to be the case with the selection of paradigmatic cases. We may select such a case on the basis of taken-for-granted, intuitive procedures but are often called upon to account for that selection. That account must be sensible to other members of the scholarly communities of which we are part. This may even be argued to be a general characteristic of scholarship, scientific or otherwise, and not unique to the selection of paradigmatic social scientific case studies. For instance, it is usually insufficient to justify an application for research funds by stating that one's intuition says that a particular research should be carried out. A research council ideally operates as society's test of whether the researcher can account, in collectively acceptable ways, for his or her intuitive choice, even though intuition may be the real, or most important, reason why the researcher wants to execute the project.

It is not possible consistently, or even frequently, to determine in advance whether or not a given case—Geertz's cockfights in Bali, for instance—is paradigmatic. Besides the strategic choice of case, the execution of the case study will certainly play a role, as will the reactions to the study by the research community, the group studied, and, possibly, a broader public. The value of the case study will depend on the validity claims that researchers can place on their study, and the status these claims obtain in dialogue with other validity claims in the discourse to which the study is a contribution. Like other good craftspeople, all that researchers can do is use their experience and intuition to assess whether they believe a given case is interesting in a paradigmatic context, and whether they can provide collectively acceptable reasons for the choice of case.

Concerning considerations of strategy in the choice of cases, it should also be mentioned that the various strategies of selection are not necessarily mutually exclusive. For example, a case can be simultaneously extreme, critical, and paradigmatic. The interpretation of such a case can provide a unique wealth of information, because one obtains various perspectives on and conclusions about the case according to whether it is viewed and interpreted as one or another type of case. Finally, a case that the researcher initially thought was one type may turn out to be another, upon closer study (see Box 17.3).

Box 17.3 From Critical Case to Extreme Case, Unwittingly

When I was planning a case study of rationality and power in urban policy and planning in Aalborg, Denmark, reported in Flyvbjerg (1998a), I tried to design the study as a "most likely" critical case in the following manner: If rationality in urban policy and planning were weak in the face of power in Aalborg, then, most likely, they would be weak anywhere, at least in Denmark, because in Aalborg the rational paradigm of policy and planning stood stronger than anywhere else. Eventually, I realized that this logic was flawed, because my research of local relations of power showed that one of the most influential "faces of power" in Aalborg, the Chamber of Industry and Commerce, was substantially stronger than its equivalents elsewhere. This had not been clear at the outset because much less research existed on local power relations than research on local planning. Therefore, instead of a critical case, unwittingly I ended up with an extreme case in the sense that both rationality and power were unusually strong in Aalborg. My study thus became one of what happens when strong rationality meets strong power in the arena of urban policy and planning. But this selection of Aalborg as an extreme case happened to me; I did not deliberately choose it. It was a frustrating experience, especially during those several months after I realized I did not have a critical case until it became clear that all was not lost because I had something else. As a case researcher charting new terrain, one must be prepared for such incidents, I believe.

■ Misunderstanding No. 4

> The case study contains a bias toward verification, that is, a tendency to confirm the researcher's preconceived notions.

The fourth of the five misunderstandings about case study research is that the method maintains a bias toward verification, understood as a tendency to confirm the researcher's preconceived notions, so that the study therefore becomes of doubtful scientific value. Jared Diamond (1996, p. 6), for example, holds this view. He observes that the case study suffers from what he calls a "crippling drawback," because it does not apply "scientific methods," which Diamond understands as methods useful for "curbing one's tendencies to stamp one's preexisting interpretations on data as they accumulate."

Francis Bacon (1853, p. xlvi) saw this bias toward verification not simply as a phenomenon related to the case study in particular, but as a fundamental human characteristic. Bacon expressed it like this:

> The human understanding from its peculiar nature, easily supposes a greater degree of order and equality in things than it really finds. When any proposition has been laid down, the human understanding forces everything else to add fresh support and confirmation. It is the peculiar and perpetual error of the human understanding to be more moved and excited by affirmatives than negatives.

Bacon certainly touches upon a fundamental problem here, a problem that all researchers must deal with in some way. Charles Darwin (Barlow, 1958, p. 123), in his autobiography, describes the method he developed in order to avoid the bias toward verification:

> I had . . . during many years followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favorable ones. Owing to this habit, very few objections were raised against my views, which I had not at least noticed and attempted to answer.

The bias toward verification is general, but the alleged deficiency of the case study and other qualitative methods is that they ostensibly allow more room for the researcher's subjective and arbitrary judgment than other methods: They are often seen as less rigorous than are quantitative, hypothetico-deductive methods. Even if such criticism is useful, because it sensitizes us to an important issue, experienced case researchers cannot help but see the critique as demonstrating a lack of knowledge of what is involved in case study research. Donald Campbell and others have shown that the critique is fallacious, because the case study has its own rigor, different to be sure, but no less strict than the rigor of quantitative methods. The advantage of the case study is that it can "close in" on real-life situations and test views directly in relation to phenomena as they unfold in practice.

According to Campbell, Ragin, Geertz, Wieviorka, Flyvbjerg, and others, researchers who have conducted intensive, in-depth case studies, typically report that their preconceived views, assumptions, concepts, and hypotheses were wrong and that the case material has compelled them to revise their hypotheses on essential points. The case study forces upon the researcher the type of falsifications described above. Ragin (1992, p. 225) calls this a "special feature of small-*N* research," and goes on to explain that criticizing single case studies for being inferior to multiple case studies is misguided, since even single case studies

"are multiple in most research efforts because ideas and evidence may be linked in many different ways."

Geertz (1995, p. 119) says about the fieldwork involved in most in-depth case studies that "The Field" itself is a "powerful disciplinary force: assertive, demanding, even coercive." Like any such force, it can be underestimated, but it cannot be evaded. "It is too insistent for that," says Geertz. That he is speaking of a general phenomenon can be seen by simply examining case studies, such as those by Eckstein (1975), Campbell (1975), and Wieviorka (1992). Campbell (1975, pp. 181–182) discusses the causes of this phenomenon in the following passage:

> In a case study done by an alert social scientist who has thorough local acquaintance, the theory he uses to explain the focal difference also generates prediction or expectations on dozens of other aspects of the culture, and he does not retain the theory unless most of these are also confirmed. . . . Experiences of social scientists confirm this. Even in a single qualitative case study, the conscientious social scientist often finds no explanation that seems satisfactory. Such an outcome would be impossible if the caricature of the single case study . . . were correct—there would instead be a surfeit of subjectively compelling explanations.

According to the experiences cited above, it is falsification and not verification that characterizes the case study. Moreover, the question of subjectivism and bias toward verification applies to all methods, not just to the case study and other qualitative methods. For example, the element of arbitrary subjectivism will be significant in the choice of categories and variables for a quantitative or structural investigation, such as a structured questionnaire to be used across a large sample of cases. And the probability is high that (1) this subjectivism survives without being thoroughly corrected during the study, and (2) that it may affect the results, quite simply because the quantitative/structural researcher does not get as close to those under study as does the case study researcher and therefore is less likely to be corrected by the study objects "talking back." George and Bennett (2005, p. 20) describe this all-important feature of case study research like this:

> When a case study researcher asks a participant "were you thinking X when you did Y," and gets the answer, "No, I was thinking Z," then if the researcher had not thought of Z as a causally relevant variable, she may have a new variable demanding to be heard.

Statistical methods may identify deviant cases that can lead to new hypotheses, but in isolation these methods lack any clear means of actually identifying new hypotheses. This is true of all studies that use existing databases or that collect survey data based on questionnaires with predefined standard questions. Unless statistical researchers do their own archival work, interviews, or face-to-face surveys with open-ended questions—like case study researchers—they have no means of identifying left-out variables (George & Bennett, 2005, p. 21). According to Ragin (1992, p. 225; see also Ragin, 1987, pp. 164–171):

> This feature explains why small-*N* qualitative research is most often at the forefront of theoretical development. When *N*'s are large, there are few opportunities for revising a casing [that is, the delimitation of a case]. At the start of the analysis, cases are decomposed into variables, and almost the entire dialogue of ideas and evidence occurs through variables. One implication of this discussion is that to the extent that large-*N* research can be sensitized to the diversity and potential heterogeneity of the cases included in an analysis, large-*N* research may play a more important part in the advancement of social science theory.

Here, too, this difference between large samples and single cases can be understood in terms of the phenomenology for human learning discussed above. If one thus assumes that the goal of the researcher's work is to understand and learn about the phenomena being studied, then research is simply a form of learning. If one assumes that research, like other learning processes, can be described by the phenomenology for human learning, it then becomes clear that the most advanced form of understanding is achieved when researchers place themselves within the context being studied. Only in this way can researchers understand the viewpoints and the behavior that characterizes social actors. Relevant to this point, Giddens states that valid descriptions of social activities presume that researchers possess those skills necessary to participate in the activities described:

> I have accepted that it is right to say that the condition of generating descriptions of social activity is being able in principle to participate in it. It involves "mutual knowledge," shared by observer and participants whose action constitutes and reconstitutes the social world. (1982, p. 15)

From this point of view, the proximity to reality, which the case study entails, and the learning process that it generates for the researcher will often constitute a prerequisite for advanced understanding. In this context, one begins to understand Beveridge's conclusion that there are more discoveries stemming from intense observation of individual cases than from statistics applied to large groups. With the point of departure in the learning process, we understand why the researcher who conducts a case study often ends up by casting off preconceived notions and theories. Such activity is quite simply a central element in learning and in the achievement of new insight. More simple forms of understanding must yield to more complex ones as one moves from beginner to expert.

On this basis, the fourth misunderstanding—that the case study supposedly contains a bias toward verification, understood as a tendency to confirm the researcher's preconceived ideas—is revised as follows:

The case study contains no greater bias toward verification of the researcher's preconceived notions than other methods of inquiry. On the contrary, experience indicates that the case study contains a greater bias toward falsification of preconceived notions than toward verification.

■ Misunderstanding No. 5

It is often difficult to summarize and develop general propositions and theories on the basis of specific case studies.

Case studies often contain a substantial element of narrative and one can get into a terrible quicksand today talking about the matter of narrative in social science (for a good overview of narrative inquiry, see Chapter 25 in this volume by Susan Chase; Todd Landman, in press). After certain strands of discourse theory have defined any text as narrative and everything as text, it seems that narrative is everything. But if something is everything, maybe it is nothing, and we are back to square one. It is difficult to avoid the subject of narrative completely, however, when considering the case study and qualitative research. In my own work, when I think about narrative, I do not think of discourse theory but of Miles Davis, the jazz icon. When asked how he kept writing classics through a four-decades-long career, he answered, "I first write a beginning, then a middle, and finally the ending." Narrative suggests questions about plot, that is, a sequence of events and how they are related, and Davis set out the naked minimum. Obviously, plots and narratives may be hatched in many ways. But if you write the kind of classic narrative that Davis talks about, with a beginning, a middle, and an end, you typically first try to get the attention of the reader, often by means of a hook, that is, a particularly captivating event or problematic that leads into the main story. You then present the issues and who are involved, including their relationships. Gradually, you reel in the reader to a point of no return, from where the main character—who in a case study need not be a person but could be, say, a community, a program, or a company—has no choice but to deal with the issues at hand, and in this sense is tested. At this stage, typically, there is conflict and the conflict escalates. Finally, harmony is restored by the conflict being resolved, or at least explained, as may be the appropriate achievement in a social science narrative.

To Alasdair MacIntyre (1984, pp. 214, 216), the human being is a "story-telling animal," and the notion of a history is as fundamental a human notion as the notion of an action. Other observers have noted that narrative seems to exist in all human societies, modern and ancient, and that it is perhaps our most fundamental form for making sense of experience (Mattingly, 1991, p. 237; Novak, 1975, p. 175; see also Abbott, 1992; Arendt, 1958; Bal, 1997; Carr, 1986; Fehn, Hoestery, & Tatar, 1992; Rasmussen, 1995; Ricoeur, 1984). Narrative thus seems not only to be the creation of the storyteller, but seems also to be an expression of innate relationships in the human mind, which we use to make sense of the world by constructing it as narrative.

The human propensity for narrative involves a danger, however, of what has been called the narrative fallacy. The fallacy consists of a human inclination to simplify data and information through overinterpretation and through a preference for compact stories over complex data sets (Taleb, 2010, p. 63). It is easier to remember and make decisions on the basis of "meaningful" stories than to remember strings of "meaningless" data. Thus, we read meaning into data and make up stories, even where this is unwarranted. As a case in point, consider the inspirational accounts of how the Internet led to a "new economy" where productivity had been disconnected from share prices; or the fairy tale that increasing real estate prices are enough to sustain economic growth in a nation. Such stories are easy to understand and act on—for citizens, policy makers, and scholars—but they are fallacies and as such they are treacherous. In social science, the means to avoid the narrative fallacy is no different from the means to avoid other error: the usual systematic checks for validity and reliability in how data are collected and used.

Dense narratives based on thick description will provide some protection against the narrative fallacy. Such narratives typically approach the complexities and contradictions of real life. Accordingly, they may be difficult or impossible to summarize into neat formulas, general propositions, and theories (Benhabib, 1990; Mitchell & Charmaz, 1996; Roth, 1989; Rouse, 1990; White, 1990). This tends to be seen by critics of the case study as a drawback. To the case study researcher, however, a particularly "thick" and hard-to-summarize narrative is not a problem. Rather, it is often a sign that the study has uncovered a particularly rich problematic. The question, therefore, is whether the summarizing and generalization, which the critics see as an ideal, is always desirable. Friedrich Nietzsche (1974, p. 335, para. 373) is clear in his answer to this question. "Above all," he says about doing science, "one should not wish to divest existence of its *rich ambiguity*" (emphasis in original).

Lisa Peattie (2001, p. 260) explicitly warns against summarizing dense case studies: "It is simply that the very value of the case study, the contextual and interpenetrating nature of forces, is lost when one tries to sum up in large and mutually exclusive concepts." The dense case study, according to Peattie, is more useful for the practitioner and more interesting for social theory than either factual "findings" or the high-level generalizations of theory.

The opposite of summing up and "closing" a case study is to keep it open. Two strategies work particularly well in ensuring openness. First, when writing up their case studies, authors may demur from the role of omniscient narrator and summarizer. Instead, they may choose to tell the story in its diversity, allowing the story to unfold from the many-sided, complex, and sometimes-conflicting stories that the actors in the case have

told researchers. Second, authors of case studies may avoid linking their study with the theories of any one academic specialization. Instead, they may choose to relate the case to broader philosophical positions that cut across specializations. In this way, authors leave scope for readers of different backgrounds to make different interpretations and draw diverse conclusions regarding the question of what the case is a case of. The goal is not to make the case study be all things to all people. The goal is to allow the study to be different things to different people. Here it is useful to describe the case with so many facets—like life itself—that different readers may be attracted, or repelled, by different things in the case. Readers are not pointed down any one theoretical path or given the impression that truth might lie at the end of such a path. Readers will have to discover their own path and truth inside the case. Thus, in addition to the interpretations of case actors and case narrators, readers are invited to decide the meaning of the case and to interrogate actors' and narrators' interpretations in order to answer that categorical question of any case study: "What is this case a case of?"

Case stories written like this can neither be briefly recounted nor summarized in a few main results. The case story is itself the result. It is a "virtual reality," so to speak. For the reader willing to enter this reality and explore it inside and out, the payback is meant to be a sensitivity to the issues at hand that cannot be obtained from theory. Students can safely be let loose in this kind of reality, which provides a useful training ground with insights into real-life practices that academic teaching often does not provide.

If we return again briefly to the phenomenology for human learning, we may understand why summarizing case studies is not always useful and may sometimes be counterproductive. Knowledge at the beginner's level consists precisely in the reduced formulas that characterize theories, while true expertise is based on intimate experience with thousands of individual cases and on the ability to discriminate between situations, with all their nuances of difference, without distilling them into formulas or standard cases. The problem is analogous to the inability of heuristic, computer-based expert systems to approach the level of virtuoso human experts, even when the systems are compared with the experts who have conceived the rules upon which these systems operate. This is because the experts do not use rules but operate on the basis of detailed case experience. This is *real* expertise. The rules for expert systems are formulated only because the systems require it; rules are characteristic of expert *systems*, but not of real human *experts*.

In the same way, one might say that the rule formulation that takes place when researchers summarize their work into theories is characteristic of the culture of research, of researchers, and of theoretical activity, but such rules are not necessarily part of the studied reality constituted by Bourdieu's (1977, pp. 8, 15) "virtuoso social actors." Something essential may be lost by this summarizing—namely the possibility to understand virtuoso social acting, which, as Bourdieu has shown, cannot be distilled into theoretical formulas—and it is precisely their fear of losing this "something" that makes case researchers cautious about summarizing their studies. Case researchers thus tend to be skeptical about erasing phenomenological detail in favor of conceptual closure.

Ludwig Wittgenstein shared this skepticism. According to Gasking and Jackson, Wittgenstein used the following metaphor when he described his use of the case study approach in philosophy:

> In teaching you philosophy I'm like a guide showing you how to find your way round London. I have to take you through the city from north to south, from east to west, from Euston to the embankment and from Piccadilly to the Marble Arch. After I have taken you many journeys through the city, in all sorts of directions, we shall have passed through any given street a number of times—each time traversing the street as part of a different journey. At the end of this you will know London; you will be able to find your way about like a born Londoner. Of course, a good guide will take you through the more important streets more often than he takes you down side streets; a bad guide will do the opposite. In philosophy I'm a rather bad guide. (1967, p. 51)

This approach implies exploring phenomena firsthand instead of reading maps of them. Actual practices are studied before their rules, and one is not satisfied by learning only about those parts of practices that are open to public scrutiny; what Erving Goffman (1963) calls the "backstage" of social phenomena must be investigated, too, like the side streets that Wittgenstein talks about.

With respect to intervention in social and political affairs, Andrew Abbott (1992, p. 79) has rightly observed that a social science expressed in terms of typical case narratives would provide "far better access for policy intervention than the present social science of variables." Alasdair MacIntyre (1984, p. 216) similarly says, "I can only answer the question 'What am I to do?' if I can answer the prior question 'Of what story or stories do I find myself a part?'" In a similar vein, Cheryl Mattingly (1991, p. 237) points out that narratives not only give meaningful form to experiences we have already lived through, they also provide us a forward glance, helping us to anticipate situations even before we encounter them, allowing us to envision alternative futures. Narrative inquiries do not—indeed, cannot—start from explicit theoretical assumptions. Instead, they begin with an interest in a particular phenomenon that is best understood narratively. Narrative inquiries then develop descriptions and interpretations of the phenomenon from the perspective of participants, researchers, and others.

William Labov and Joshua Waletzky (1966, pp. 37–39) write that when a good narrative is over, "it should be unthinkable for a bystander to say, 'So what?'" Every good narrator is continually warding off this question. A narrative that lacks a moral that can be independently and briefly stated, is not necessarily pointless. And a narrative is not successful just because it allows a brief moral. A successful narrative does not allow the question to be raised at all. The narrative has already supplied the answer before the question is asked. The narrative itself is the answer (Nehamas, 1985, pp. 163–164).

A reformulation of the fifth misunderstanding, which states that it is often difficult to summarize specific case studies into general propositions and theories, thus reads as follows:

> *It is correct that summarizing case studies is often difficult, especially as concerns case process. It is less correct as regards case outcomes. The problems in summarizing case studies, however, are due more often to the properties of the reality studied than to the case study as a research method. Often it is not desirable to summarize and generalize case studies. Good studies should be read as narratives in their entirety.*

It must again be emphasized that despite the difficulty or undesirability in summarizing certain case studies, the case study as such can certainly contribute to the cumulative development of knowledge, for example, in using the principles to test propositions described above under the second and third misunderstandings.

▣ Current Trends in Case Study Research

This chapter began by pointing out a paradox in case study research, namely, that even as case studies are widely used in social science and have produced many of the classic texts here, it may be observed that the case study as a methodology is generally held in low regard, or is simply ignored, within large and dominant parts of the academy. This state of affairs has proved remarkably long-lived.

However, as pointed out by George and Bennett (2005, pp. 4–5), recently a certain loosening of positions has taken place. A more collaborative approach is gaining ground, where scholars begin to see that different methodological approaches have different strengths and weaknesses and are essentially complementary. The old and often antagonistic division between quants and qualts is losing ground as a new generation of scholars trained in both quantitative and qualitative methods is emerging. For these scholars, research is problem-driven and not methodology-driven, meaning that those methods are employed that for a given problematic best help answer the research questions at hand. More often than not, a combination of qualitative and quantitative methods will do the task best. Finally, some of the most ambitious claims regarding how the quantitative revolution would make possible a social science on a par with natural science in its ability to explain and predict have been scaled back, making room for the emergence of a more realistic and balanced attitude to what social science can and cannot do. The chapters in this volume on mixed methods, by John Creswell (Chapter 15), and Charles Teddlie and Abbas Tashakkori (Chapter 16), are good examples of this loosening of positions and more balanced attitude.

If the moment of the quantitative revolution in social science is called positivistic, as is often the case, then today we are in a postpositivist and possibly post-paradigmatic moment (Schram, 2006). My own efforts at developing a social science suited for this particular moment have been concentrated on what I call "phronetic social science," named after the ancient Greek term for practical wisdom, or common sense, *phronesis* (Flyvbjerg, 2001; Schram & Caterino, 2006). And this is what the new social science is: commonsensical. It is common sense to give up wars that cannot be won, like the methods war over quantitative versus qualitative methods, or the science wars, which pit social science against natural science. It is also common sense to finally acknowledge that case studies and statistical methods are not conflicting but complementary (see Box 17.4).

Box 17.4 Complementarity in Action: From Case Studies to Statistical Methods, and Back

My current research on megaprojects was originally sparked by events at the Channel tunnel, which links the United Kingdom and France, and the Danish Great Belt tunnel, linking Scandinavia with continental Europe. These are the two longest underwater rail tunnels in Europe, each costing several billion dollars. Soon after construction of the Channel tunnel began, costs started escalating, and at the opening of the tunnel, in 1994, costs had doubled in real terms leaving the project in serious financial trouble. But maybe the British and French had just been unlucky? Perhaps the Danes would do better on the Great Belt tunnel? Not so. Here the cost overrun was larger still, at 120% in real terms, and the tunnel proved financially nonviable even before it opened to traffic in 1997, several years behind schedule. I did a case study of these two hugely expensive projects in order to document and understand the apparent incompetence in their planning and execution (Flyvbjerg, Bruzelius, & Rothengatter, 2003). The study raised the inevitable question of whether the Channel and Great Belt tunnels were outliers regarding cost overrun and viability or whether such extreme lack of ability to build on budget was common for large-scale infrastructure projects. Searching the world's libraries and asking colleagues, I found that no study existed that answered these questions in a statistically valid manner. I therefore decided to do such a study and my group and I now turned from case studies to statistical methods. To our amazement, our studies showed, with a very high level of statistical significance, that the Channel and Great Belt projects were not outliers, they were normal; nine out of ten projects have cost overrun. Even more surprisingly, when we extended our data back in time we

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found that for the 70 years for which we were able to find data there had been no improvement in performance regarding getting cost estimates right and staying on budget. The same apparent error of cost underestimation and overrun was being repeated decade after decade. We now began debating among ourselves whether an error that is being repeated over and over by highly trained professionals is really an error, or whether something else was going on. To answer this question, we went back to case studies and process tracing (see Box 17.1). We found that cost overrun and lack of viability were not best explained by simple error but by something more sinister and Machiavellian, namely strategic misrepresentation of costs and benefits by promoters during appraisal in order to get projects funded and built. From my initial case-based curiosity with the outcomes at the Channel and Great Belt tunnels—and by going from case studies to statistical methods and back—my group and I had uncovered a deep-rooted culture of deception in the planning and management of large-scale infrastructure projects (Flyvbjerg, 2007). As a recent spin-off from this research, my group and I are now investigating whether the success of one in ten projects in staying on budget—documented in our statistical studies—may be replicated or is due to luck. Here, again, we are back to case study research, now studying success as a deviant case.

The complementarity between case studies and statistical methods may be summarized as in Table 17.2. The main strength of the case study is depth—detail, richness, completeness, and within-case variance—whereas for statistical methods it is breadth. If you want to understand a phenomenon in any degree of thoroughness—say, child neglect in the family or cost overrun in urban regeneration—what causes it, how to prevent it, and so on, you need to do case studies. If you want to understand how widespread the phenomenon is, how it correlates with other phenomena and varies across different populations, and at what level of statistical significance, then you have to do statistical studies. If you want to understand both, which is advisable if you would like to speak with weight about the phenomenon at hand, then you need to do both case studies and statistical analyses. The complementarity of the two methods is that simple, and that beautiful.

When you think about it, it is amazing that the separation and antagonism between qualitative and quantitative methods often seen in the literature, and in university departments, have lasted as long as they have. This is what happens when tribalism and power, instead of reason, rules the halls of academia. As such, it is testimony to the fact that academics, too, are all too human, and not testimony to much else. The separation is not a logical consequence of what graduates and scholars need to know in order to do their studies and do them well; quite the opposite. Good social science is opposed to an either/or and stands for a both/and on the question of qualitative versus quantitative methods. The *International Encyclopedia of the Social & Behavioral Sciences* (Smelser & Baltes, 2001, p. 1513) is certainly right when it points out that the case study and statistical methods can "achieve far more scientific progress together than either could alone."

Table 17.2 Complementarity of Case Studies and Statistical Methods

	Case Studies	*Statistical Methods*
Strengths	■ Depth	■ Breadth
	■ High conceptual validity	■ Understanding how widespread a phenomenon is across a population
	■ Understanding of context and process	■ Measures of correlation for populations of cases
	■ Understanding of what causes a phenomenon, linking causes and outcomes	■ Establishment of probabilistic levels of confidence
	■ Fostering new hypotheses and new research questions	
Weaknesses	■ Selection bias may overstate or understate relationships	■ Conceptual stretching, by grouping together dissimilar cases to get larger samples
	■ Weak understanding of occurrence in population of phenomena under study	■ Weak understanding of context, process, and causal mechanisms
	■ Statistical significance often unknown or unclear	■ Correlation does not imply causation
		■ Weak mechanisms for fostering new hypotheses

This being said, it should nevertheless be added that the balance between case studies and statistical methods is still biased in favor of the latter in social science, so much so that it puts case studies at a disadvantage within most disciplines. For the time being, it is therefore necessary to continue to work on clarifying methodologically the case study and its relations to other social science methods in order to dispel the methodological limbo in which the method has existed for too long. This chapter is intended as such clarification.

Note

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18

PERFORMANCE ETHNOGRAPHY

Judith Hamera

My students did not understand "Sandy Sem's"[1] response to her parents' traumatic negotiation of their survivor status, and the relationship between that status and a class assignment she was given by her teacher. "Sandy's" mother and father were victims and survivors of the Khmer Rouge autohomeogenocide in Cambodia between 1975 and 1979, and their experiences of these atrocities haunted them in their new lives as refugees in Long Beach, California. As I observe in my analysis of the family's use of Khmer classical dance (Hamera, 2007, pp. 138–171), her parents would not share details of their ordeal, or even much about their lives, and "Sandy" did not press them. Raised in a cultural moment celebrating memoir and self-disclosure; with understandable pride in their own cultures of origin and family traditions; and deeply, if perhaps unreflectively, inheritors to the idea of testimony as both personally and socially redemptive, my class could not easily assimilate her logic, articulated in a fieldnote of mine that I shared with them:

> "Sandy Sem": We had some project for school, to talk about our culture and our families—like grandmothers and grandfathers and stuff. But you can't ask them [her parents] about that, him [her father] especially because he gets mad and the teacher—right—she's going to believe that. And I'm going to go in, okay, and say: "My family's from Cambodia and everybody's dead from the war or over here somewhere but nobody says," okay? So I just made it up.

How could she just "make it up"? How could she not want to push her parents to "tell the truth"? Don't they, and didn't she, have an obligation to know and share everything about where she came from? How could others learn from what was "just made up," and enabling others to learn was important, right? Why was I, their professor, who proclaimed commitment to rigorous inquiry, not pushing back at the family to "speak the truth to power"? Isn't that what good critical scholars do?

Try as we might, we couldn't come to a collective understanding of why "Sandy's" response might be useful, or necessary, or "right." What if we translate her situation into space, I asked? Where would she sit? Would she look at her audience? Who is her audience? Does she have one? Where are her parents? Who else is around—and where are they? Desks were moved and space, literal and conceptual, opened up. "Sandy's" position was embodied by, not one, but two students. One was sitting facing the audience, looking down at a blank page in an open notebook. "The ethnographer" stood on one side with her own notebook and "the teacher," holding a grade book, stood on other. Another "Sandy" sat with her back to the first, looking in the opposite direction. From that direction, receding in a diagonal as if toward a vanishing point, were "the parents," silent except for occasional sounds (sighs of exhaustion, sharp intakes of breath as if in pain), their backs to one another, and further still were "the others" and "the ancestors." The "others" and "the ancestors" were moving, sometimes in tight circles around one another, sometimes randomly across the space. They murmured, barely audible.

Here, between the murmurs, the paralinguistic articulations of pain and resignation, the inadequacy of notebooks and grade book: Here was the logic of "Sandy's" response. The "others" and "ancestors" were too far away to hear, the "ethnographer" and "teacher" too removed in other ways to understand. "Sandy's" logic was born of a nuanced reading of context—verbal and even more important, extra-verbal: the circulation of affective energy in her home, in her parents' lives, and in their histories. She had negotiated the collapse of time and space ("here and now," "there and then") in her personal, familial, and cultural pasts, and made a decision that my students could only grasp by