

Resistance to Qual/Quant Parity: Why the “Paradigm” Discussion Can’t Be Avoided

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Despite making impressive gains in recent years, qualitative research still faces strong resistance from many, especially within mainstream psychology in the United States. I argue that this resistance stems, in part, from continuing widespread misunderstandings about the nature of qualitative methods, and, more specifically, about the relationship between the paradigms that govern quantitative and qualitative research. Some have argued for a strong separation between these paradigms, whereas others have minimized their separation or questioned the usefulness of the paradigm concept altogether. In this article I argue for the importance of the paradigm concept. I trace the history of this concept and some of the complex philosophical and definitional issues underlying it, and I argue that quantitative and qualitative research methodologies are defined, respectively, by governing paradigms that can be said to be truly separate in certain crucial respects. I conclude that a deeper understanding of the quantitative and qualitative paradigms, and of the distinct and unique kinds of knowledge each generates, supports the parity of these 2 research methodologies and clarifies some of the challenges of combining them in mixed methods designs.

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The growth of interest in qualitative research over the past few decades has been impressive. There are now more than 100 journals that publish qualitative studies, including several with the term “qualitative” in their titles. And although the growth of quantitatively oriented studies has approximately tripled in the past 20 years, the growth of qualitatively oriented ones has increased more than 10-fold.¹ Nor is this explosion of interest limited to professional researchers and authors. A recent Google search found that while quantitative search terms like “control group” and “multivariate” produced many hits (5 to 10 million), a search for the phrase “qualitative research” also produced a respectable number (2,790,000).² Indeed, a search of references across millions of books via Google Ngram Viewer indicates that quantitative terms show a peak just before 1980 and a decline since that time, whereas the phrase “qualitative research” first appears in the early

1980s and increases dramatically over the next 30 years (Google, 2013). Data like these suggest that a genuine cultural shift may be occurring, from an intellectual milieu dominated by a narrow conception of science to one recognizing a much broader and richer range of research methodologies.

At the same time, however, the research landscape reveals a great deal of continuing resistance to qualitative research, particularly within the discipline of psychology in the United States. Some of this resistance comes from the top down, via the official institutional channels of American psychology. For example, the APA publication manual (American Psychological Association, 2010) gives extensive guide-

¹ For example, the number of studies indexed in PsycINFO and retrievable using the search terms “statistical OR measurement” for the five-year periods ending in 1993 and 2013, respectively, increased from 23,104 to 63,914, whereas those retrievable from the same periods using the search terms “qualitative OR naturalistic” increased from 3,084 to 44,877.

² Based on the results of Google searches during the first week of April, 2014. Searches for syllabi of psychology methodology courses produced similar ratios (approximately 252,000 to 104,000).

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lines for reporting quantitative research with virtually no recognition of the significant format differences necessary for reporting qualitative research.³ The APA guidelines for undergraduate programs, which repeatedly emphasize experiments and measurement, only mention qualitative research twice, and then to compare it unfavorably to quantitative research (American Psychological Association, 2013, p. 24).⁴ Other sites of resistance to qualitative research are more broadly rooted. For example, announcements of open tenure-track positions in the U.S. are frequently oriented to quantitative research, with little explicit interest in qualitative research,⁵ and granting agencies often favor quantitative over qualitative research (e.g., see Torrance, 2008, pp. 518–519).

One arena of particularly uniform and persistent resistance is that of undergraduate psychology textbooks. For example, an examination of the 10 most widely used introductory texts (identified by Eagly et al., 2012) reveals the following: All 10 books consistently portray experimentation and quantification as the methods of choice for gaining psychological knowledge. None of the books include the phrase “qualitative research” in the index, nor do any of them discuss qualitative methodology as a coherent research strategy. Although all 10 books do allude to qualitative techniques like naturalistic observation and case studies, these references are brief and frequently portray such techniques as requiring quantitative analysis. Two of the texts (Myers, 2010, and Wade & Tavris, 2011) emphasize what the authors seem to regard as the role of qualitative techniques in disseminating dangerous pseudoscientific beliefs and myths. The other books give more balanced accounts, but all of them contain significant misrepresentations. For example, five of the 10 books imply that case studies focus only on single individuals and/or clinical phenomena, and seven of them illustrate naturalistic observation with studies of nonhuman primates or other animals. Most of the books erroneously characterize qualitative techniques as primarily or entirely descriptive, not useful for providing explanations or general knowledge, and serving primarily (or only) to generate hypotheses for testing by experimental methods.

This resistance is not new, and it has frequently led to counterresistance by qualitative

researchers. During the 1970s, for example, some qualitative theorists began to challenge the assumptions underlying the dominant experimental approach, launching what has now come to be called the “paradigm wars.” Initially, these authors drew from philosophers critiquing traditional positivist science (e.g., Gadamer, 1975; Kuhn, 1970; Taylor, 1971), but as time went on many began to draw from more radical political and postmodernist critiques (e.g., Derrida, 1978; Foucault, 1972; Harding, 1986). By the late-1980s a number of researchers—both quantitative and qualitative—were becoming concerned by what they saw as an irresolvable and increasingly rancorous conflict between qualitative and quantitative researchers. Thus, in 1989, Gage (who coined the term “paradigm wars”) called for the combatants to step back from “an intellectual-no-man’s land” and work together (Gage, 1989, p. 10), and in 1994, Reichardt and Rallis edited a collection of papers aimed at exploring whether “a synthesis of the disparate views might be possible” (Reichardt & Rallis, 1994, p. 1). To some degree, these calls for cooperation succeeded in changing the tone over the next 25 years, as more authors began to focus on mixed methods research and the possibility of combining research options (e.g., Bryman, 1988, 2006, 2008; Hammersley, 1992, 1998).

Significantly, however, the paradigm wars have continued to simmer quietly in the background. The topic of qualitative and quantitative paradigms—how they might be defined, when they should be applied, and whether or not they can be reconciled—continues to be a regular topic of scholarly focus (e.g., Alise & Teddlie, 2010; Feng & Guangwei, 2014; Fernandez-Cano & Fernandez-Guerrero, 2011; Pett & Clark, 2012; Ravasi & Canato, 2013; Wiggins, 2009). Some authors note the longevity of the debate (e.g., Shepherd & Challenger, 2013),

³ All three sample formats given in the manual involve experimental research. Other than minor allusions, no attention is given to qualitative research, and the term does not appear in the index.

⁴ Both of these mentions are framed in neutral language but clearly serve a delegitimizing function in the context of the experimental prescriptions that pervade the guidelines.

⁵ For example, a search of tenure-track positions listed in the *APA Monitor* for November, 2013 identified only 2 positions oriented toward qualitative research, as opposed to 24 positions oriented toward quantitative research.

whereas others still hopefully anticipate its end (e.g., Bryman, 2006, 2008; Griffiths & Norman, 2013). In this article I attempt to clarify why this debate continues. I argue that although the calls for reconciliation are understandable and the concerns that motivate them legitimate, a continuing discussion in terms of paradigms is inevitable and, indeed, unavoidable if parity between qualitative and quantitative research is ever to be attained. That is, resistance to qualitative research continues to thrive, in part, because the qualitative paradigm is still not sufficiently understood, both by quantitative and qualitative researchers. To appreciate the unique features of each kind of research, a closer examination of the paradigm concept is necessary, along with an analysis of the senses in which the quantitative and qualitative research paradigms can be identified as truly distinct and separate and the reasons why a bridging of the two paradigms is likely to be more difficult and problematic than many authors acknowledge. At the same time, however, there are numerous indications that quantitative and qualitative techniques can be legitimately combined and integrated in certain ways, although the issues underlying such mixed methods research are complex and likely to resist straightforward analysis.

The Qualitative Paradigm

Although naturalistic techniques have often been employed in the social sciences, the use of the word “qualitative” as a cover term is relatively recent. The term seems first to have been used by Leon Festinger and his colleagues in 1956 in their classic study *When Prophecy Fails* (Festinger, Reiken, & Schachter, 1956, p. 252). The term was picked up by Glaser and Strauss (1967), after which it began to be used more widely, especially among educational researchers (e.g., Cook & Reichardt, 1979). In the 1980s, as indicated at the beginning of this article, interest in research designated “qualitative” began an exponential rise. Once qualitative researchers had a cover term and a common identity, they began to focus more closely on the nature of their research methodology. Given the influence of Thomas Kuhn’s (1962/1970) *Structure of Scientific Revolutions* during this period, it was natural for these authors to think

in terms of a qualitative “paradigm” (e.g., Rist, 1977).

However, specifying just what this paradigm consisted of was not an easy matter. For one thing, in distinguishing research as “qualitative” or “quantitative” the presence or absence of quantification per se is not always a crucial or deciding factor. Many qualitative researchers draw on demographic data and/or find it helpful to use coding and other types of quantification in analyzing their findings. At the same time, quantification is not always applied in experimental research—for example, some studies of animal and human cognition simply detail the theoretical implications of experiments with one or more subjects. A second issue involves the scope and boundaries of specific paradigms. In both quantitative and qualitative research the set of methods that define the governing paradigm is extremely broad and heterogeneous. In both cases, it includes many different techniques for gathering information and many different methods for analyzing this information once it is gathered. Some of these techniques and methods appear discontinuous from—or even inconsistent with—others within the same paradigm, and some may appear in both paradigms (more about this later).

Qualitative researchers have responded to these problems in a variety of ways, but many have attempted to characterize the differences between the paradigms by distinguishing between the ontological, epistemological, and (sometimes) axiological assumptions underlying them (e.g., see Burrell & Morgan, 1979; Creswell, 2012; Lincoln & Guba, 2011; McGrath & Johnson, 2003; Smith, 1983). Although there are many differences among these characterizations, there is also a great deal of commonality, which can be synthesized into the following composite summary: *Ontologically*, the quantitative paradigm is said to regard reality as a single, knowable realm, composed of material objects that are ultimately understandable as aggregates of particles in space that act on each other over time, according to strictly determinate (nomological) causal laws. *Epistemologically*, it is therefore appropriate to study all things (including human beings) as objects with the above-described properties—that is, there is a deep dualism of subject (the researcher gathering knowledge) and object (the person or thing toward which the research pro-

cess is directed). The *axiological* position of quantitative research is also consistent with the above. Because the research process is fundamentally objective, it must occur in a controlled and value-free manner so that the researcher's wishes and expectations will not interfere with the causal connections being investigated. In contrast to these philosophical assumptions, those underlying the qualitative paradigm may be characterized as follows: *Ontologically*, reality is an ever-changing, dynamic order in which much cannot be exactly known or determined and in which the relations between things are at least as important as the things themselves. *Epistemologically*, because relations are fundamental constituents of the world, reality is, in part, socially constructed, and in this sense, researchers and participants can be said to jointly create multiple meanings and multiple realities. *Axiologically*, this joint construction of reality entails a two-way interaction of values that actually strengthens, rather than weakens, the knowledge-gathering process.

Are the Paradigms Separate?

Given two such different philosophical positions, it is not surprising that the research methods developed within the quantitative and qualitative paradigms would be very different as well. But more than this, the philosophical argument is often interpreted to mean that the paradigms are so different from each other that the results of quantitative and qualitative research are partly or entirely *incommensurable*—that is, that they involve radically different kinds of knowledge that cannot be meaningfully compared (e.g., Marecek, 2003; Guba & Lincoln, 1994; Smith, 1997). This implies a strong separation between the quantitative and qualitative paradigms.

It should be noted, though, that not all qualitative researchers accept the philosophical analyses of the two paradigms and the separatist implications that they seem to entail. In a series of writings spanning more than two decades, Hammersley has made a number of criticisms of the separatist position. For example, he argues that the claim of incommensurability is self-contradictory, for if the quantitative and qualitative paradigms were truly incommensurable, then the analyses of their incommensurability could not be meaningfully formulated since

there could be no neutral or encompassing language in which to do so (Hammersley, 1998, p. 18). Similarly, Hammersley critiques the rejection of objective reality that appears to be inherent in the analysis of the qualitative paradigm. He notes that there must be some mind-independent, objective standards of truth, for if there were not, then conflicting claims about social reality could be nothing more than opinions that could not be adjudicated in any meaningful way (Hammersley, 1992, pp. 60–61, 81). Regarding the role of values in qualitative research, Hammersley cautions that subordinating the investigation to one's values erodes the distinction between research and political advocacy, undermining the authority of the former and robbing it of any potential to contribute to real social change (Hammersley, 1995, pp. 117–118). More generally, Hammersley notes that in the history of philosophy, a much wider range of positions has been articulated than the two attributed above to the quantitative and qualitative paradigms and that there is no reason to assume that research can be adequately classified into just two categories (Hammersley, 1992, p. 60).

Similarly, Bryman argues that conceptualizing quantitative and qualitative approaches in terms of separate paradigms “has produced ideal-type descriptions of each tradition with strong programmatic overtones” and that this has obscured the complexity of the research options that are actually open to investigators, both within and between the two approaches (Bryman, 1988, pp. 172–173). I noted above that each research tradition actually encompasses a wide range of techniques, and Bryman argues that the reach of each approach is so broad that it is difficult to know where one leaves off and the other begins. Some research activities (e.g., coding) may be employed in both traditions, and those that are not may be combined in mixed methods approaches. Indeed, many of the critiques of the paradigm concept and of the persistence of the “paradigm wars” are implicitly or explicitly accompanied by calls to explore mixed methods research (e.g., Alise & Teddlie, 2010; Bryman, 2006; Pett & Clark, 2012). For all these reasons, both Hammersley and Bryman conclude that the paradigm concept is a problematic one: it has directed attention away from a wide range of research possibilities and it may have done

more harm than good in conceptualizing the relationships between quantitative and qualitative research (Bryman, 1988, pp. 172–173; Hammersley, 1992, p. 8).

Taken as a whole, these criticisms would appear to be devastating. In the face of such criticisms, however, we must ask why the paradigm concept continues to be so widely invoked, and why, more than 25 years after the first calls to end the “paradigm wars,” these debates continue. I suggested above that this is partly because the paradigm concept has an important role to play in responding to the resistance to qualitative research. So how might one reply to these critiques?

It will be useful to begin by noting that the arguments against the paradigm concept are directed against this conception in its most extreme form. Hammersley implicitly acknowledges this at times, particularly when he articulates his critiques by focusing on the purest and most sweeping claims of his opponents (e.g., Hammersley, 1992, p. 60; Hammersley, 1998, p. 18). Bryman, too, directs his harshest attacks against “doctrinaire” and “ideal-type” descriptions of the two paradigms (Bryman, 1988, pp. 173, 172). In fairness to these two authors, the theorists they criticize do, in fact, seem to be making radical claims at times, although their claims also seem to be moderated when they give more complete elaborations of their positions (e.g., Smith, 1997; Lincoln & Guba, 2011). The point I wish to make, however, is that it is very difficult for those who write about paradigms, either affirmatively or critically, to do so without relying on somewhat simplified descriptions that lose sight, at times, of the subtleties of the paradigm concept and of the complexity and dynamics that define and explain how paradigms operate. Yet it is precisely these subtleties and these dynamics that we need to understand if we are to appreciate the unique character of qualitative research, and how it stands in relation to quantitative research. Similarly, it will be useful to note that the ontologies, epistemologies, and axiologies that separate the two paradigms may not be entirely contradictory. In characterizing them above, I summarized them in their most extreme and incompatible forms. And I will argue below that these philosophical frameworks do represent genuinely discrepant world views, with commitments to the construction of genuinely

different types of knowledge. But the above characterization of these two frameworks as more or less identical, respectively, with simple positivism and uncompromising constructivism does not do justice to the complex history of the development of these perspectives, nor to contemporary philosophical views, which present a more complicated and contested picture.

As noted above, the word “paradigm” seems to have entered into the qualitative-quantitative debate shortly after Thomas Kuhn used the term in his classic work *The Structure of Scientific Revolutions* (Kuhn, 1962/1970). The paradigm concept, as elaborated by Kuhn, was very influential within both philosophy and the social sciences,⁶ and yet the meaning of the term “paradigm” in Kuhn’s usage is quite difficult to pin down. Kuhn himself notes that the term has several related meanings, and he seems to have spent considerable effort in trying to sort these out. Thus, in the postscript to the second edition of his book Kuhn provides a more systematic and complete account of what a paradigm entails. In this account, Kuhn describes a paradigm as a “disciplinary matrix” of a particular research community (p. 182), containing at least four kinds of components: theoretical terms, conceptual models, methodological rules, and shared exemplars of the application of the paradigm (pp. 182–187). Kuhn indicates that the fourth of these—the shared exemplars—constitute the most important kind of component, as the real-world exemplars of the paradigm shape its other components and continue

⁶ Kuhn originally saw paradigms as applicable only to well-defined areas within the mature sciences (which he equated with the natural sciences; e.g., Kuhn, 1970, pp. 15, 37). Most theorists who have applied the paradigm concept in the social sciences, however, have regarded these assumptions as unnecessarily restrictive. Interestingly, Kuhn himself seems later to have relaxed these restrictions, first to an acceptance of paradigms in social science (including qualitative social science; Kuhn, 1970, pp. 178–179), and later to a recognition of a paradigmatic character of the larger cultural systems supporting different kinds of research communities (Kuhn, 1989, pp. 221–222). In the following analysis I will focus only on the quantitative and qualitative research paradigms. However, consistent with Hammersley’s critique of the two philosophical positions described above, there is no reason to rule out the existence of other kinds of paradigms. For example, some kinds of social research may draw from a third paradigm associated with aesthetic textual analysis (e.g., Adame, Leitner, & Knudson, 2011).

to order them over time (pp. 187–191). Later in the postscript, and elsewhere throughout the book, Kuhn indicates that paradigms also include a fifth category of components: the facts that the paradigm defines and explains. These facts are not the same as those defined and explained by other competing paradigms, which is why paradigms are said to be incommensurable (e.g., see pp. 192–193 and 198–204).

One reason for the great influence of Kuhn's paradigm concept is the particular place it occupied in the history of the philosophy of science. Kuhn's book was published just as the philosophical understanding that had guided science for most of the 20th century was dramatically unraveling. That understanding had been rooted since the 1920s in logical positivism,⁷ and it had formed the intellectual infrastructure, particularly, of experimental research. The positivists had attempted to clarify the nature of science—and to separate it decisively from metaphysical speculation—by grounding it firmly on what was called the “verification principle.” That is, a theoretical claim would be considered meaningful only if it could be reduced to a set of observation statements, which, in turn, could be tested in the laboratory and verified, at least potentially, as true. This conception is based on a correspondence theory of truth because it requires that, to be deemed true, a theoretical statement about reality must correspond to that reality itself (referred to by the observation statements).

But this approach turned out to entail numerous problems. To begin with, it assumed that observation statements accurately represent reality. However, there is no way of stepping outside of our observations to know reality directly so that we can be sure that we are perceiving it accurately (Neurath, 1931). This is a classic problem with correspondence theories of truth. The positivists attempted to get around this problem by showing that observation statements could be framed in terms of the simplest and most fundamental properties of the world like “shape,” “length,” and “color.” These terms could then be operationally defined in the laboratory as specific observations and procedures like “measurement,” “dimension” and “wave-length.” But it turned out that these latter terms also needed to be given operational definitions in terms of other observations and procedures, and so on, in a potentially infinite regress

(Bridgman, 1927). The problem was even worse for dispositional properties like “fragile” and “soluble,” which depend on inferences about tendencies (Carnap, 1936), and worse yet for complex theoretical terms like “electron,” which require extensive contexts to define (Carnap, 1956). A whole other set of problems attended the verification of general or universal statements of laws, which required additional kinds of inference. In attempting to respond to these problems, the positivists had been forced to weaken, and then abandon, the verification principle, replacing it with a notion of falsifiability—that is, of testing and rejecting false theories rather than verifying true ones (Popper, 1959). Yet this was problematic too, as it turned out that no theory could ever be separated from all of its underlying assumptions, entailing that every theory always included an indefinite number of alternate versions and could therefore never be decisively falsified (Lakatos, 1970). What had originally been a straightforward conception of logic and empiricism had now become a complicated system of ad hoc analyses, which amounted to a retreat from the correspondence theory to a more makeshift recognition of the importance of exigency, practicality, and context (see Godfrey-Smith, 2003, pp. 186–189).

Kuhn's book, and its accompanying notion of paradigms, was just one of a series of attempts to reconceptualize this dominant understanding of science, and it was neither the first nor the most radical of these attempts. On one side, the positivists themselves had made numerous repairs and revisions to their epistemological framework, as described above, and could now

⁷ There are a number of good histories of logical positivism and the problems that its adherents encountered in trying to articulate its tenets. In the account that follows I draw primarily from Suppe (1977), but I have also supplied the original sources in which some of the major problems of logical positivism were first identified. A comment is also in order about the term “postpositivism.” Whereas some authors (e.g., Zammito, 2004) use this term to designate the full range of schools that departed from logical positivism in later years, I use the term in the more restricted sense of Robson (2011) and others to designate the empiricist tradition that grew directly out of logical positivism, that attempts to retain as much of the original positivist program as possible, and that tends to regard quantitative research as the legitimate prototype of all knowledge-gathering. Some authors (e.g., Fischer, 1998) use the term “neopositivism” to designate this school.

be more properly characterized as postpositivists. Although they had adjusted their position to recognize a whole array of subtleties and complications in the way claims about reality had to be formulated and adjudicated, they still tended to regard objective experimentation as the prototype of all valid and productive knowledge gathering. Likewise they continued to retain as much as possible of the correspondence theory of truth, but now with a recognition of some of its problems and limitations. On the other side, a completely different kind of revisioning was being formulated. Originating primarily from the European schools of phenomenology and hermeneutics (e.g., Schutz, 1967; Gadamer, 1975), and drawing on critical analyses of language (Lacan, 1968; Wittgenstein, 1953) and the works of postmodernists (e.g., Foucault, 1972; Derrida, 1978), authors writing in a tradition that can loosely be characterized as constructivism⁸ were articulating a very different epistemology oriented around symbolic interaction, the construction of meaning, and larger social and cultural dynamics affecting that process of meaning construction (e.g., see Berger & Luckmann, 1966; Blumer, 1969; Taylor, 1971). Distancing themselves from notions of objectivism and correspondence theories of truth, these writers focused primarily on the social construction of reality and associated coherence and pragmatic theories of truth.

Kuhn's position in the debate between postpositivism and constructivism was an interesting one. Trained as a natural scientist and concerned primarily with experimentation, he saw his work as analyzing the ways in which the more traditionally scientific disciplines operate. Yet his analysis was in many ways more congenial with constructivism. His concept of the paradigm, in particular, was a uniquely flexible and dynamic tool for conceptualizing issues addressed in the debate between postpositivists and constructivists. The paradigm concept retains some recognition of truth-as-correspondence. Contrary to some of the more extreme constructivist critiques (e.g., Feyerabend, 1975; Derrida, 1978), it recognizes the object world as a set of forces to which any paradigm must accommodate itself in one way or another—either in sympathetic adjustment to empirical facts or in the more dire alternative of anomalies, crises, and possible collapse or abandonment.

At the same time, however, the paradigm concept draws from pragmatic and coherence theories of truth as it recognizes the synthetic process of truth construction through an active integration of exemplars, models, theories and methods, and in the shaping of the very facts that the paradigm encompasses. More generally, the paradigm concept illuminates some of the more subtle and elusive aspects of the process by which knowledge is gathered and synthesized. Contrary to some of the philosophical claims discussed earlier, paradigms are not simply derived out of ontologies and epistemologies. The process is much more complex than this, and in some respects almost the reverse: knowledge begins, at least partly, when particular problems are confronted in the real world and addressed in specific research strategies—the so-called exemplars. The refinement of these exemplars and their associated facts not only instantiates but also clarifies and further shapes the methods, theories, and philosophical assumptions upon which the exemplars are based. Likewise, specific paradigms are not “ideal types” with rigid boundaries; rather, they are, among other things, families of related techniques that form organic *social systems*—that is, sets of conventions that reflect, embody, and govern evolving research communities. Subsets of these conventions can overlap and subsume each other, and contribute to other paradigms; but well-developed traditions like those of qualitative and quantitative research can begin with certain kinds of problems and form clearly identifiable systems, defined by their overall coherence and previous successes in attaining knowledge relevant to certain kinds of goals—and they can therefore be reasonably designated “paradigms.”

⁸ The constructivist tradition is broad and deep, and I touch on it here only briefly. I use the term “constructivism” to refer primarily to authors following in the tradition of phenomenology and/or emphasizing the inherently social and interactive nature of knowledge-construction and language use. It should be noted that, like positivism, constructivism has its own problems—particularly a tendency toward extreme relativism (and possibly nihilism) in some of its more radical variants (e.g., Derrida, 1978; Feyerabend, 1975). In this article, I focus primarily on the problems of positivism because they are often overlooked or minimized by quantitatively oriented authors who resist qualitative-quantitative parity.

Paradigms and Exemplars

Given the above, we can now consider how the quantitative and qualitative research approaches can be conceptualized in terms of paradigms. The quantitative paradigm, as noted earlier, encompasses a wide array of investigative methods that includes not only various kinds of experiments but also quasi-experiments, correlational and multivariate studies, demographic analyses, computer modeling, and observations of animal behavior, to name just a few. Each of these methods has its own advantages and disadvantages, but some of them have more explanatory power than others. The “gold standard” in this array of techniques is the *controlled experiment* because of its ability to produce (or at least approximate) knowledge that is exact and reliable and that exhibits causal necessity. *Exactitude* is predetermined, in part, by the use of operational definitions (often, but not always, elaborated into quantitative measurements and/or formulas summarizing probabilities of specific outcomes); *reliability* is required in the expectation of replicable findings; and *causal necessity* can be inferred when experimental controls exclude extraneous factors and isolate lawfully efficacious ones. Other kinds of research in the quantitative paradigm fall short of this ideal. Although most quantitative methods seek at least some degree of exactitude through the use of operational definitions, and although many like correlational studies and quasi-experiments attain significant reliability and/or make some tentative inferences about causality, only the controlled experiment is specifically designed to produce knowledge that has all of these features—exactitude, reliability, and causal necessity—in their most refined states; and this investigative power identifies the controlled experiment as the prototype and exemplar of the quantitative paradigm. To put it another way, all other forms of quantitative research can be regarded as partial or preliminary steps toward the kind of knowledge that is actually attained in the controlled experiment, the paradigmatic exemplar which alone imparts the power to effectively and reliably predict and control.

It will be helpful at this point to consider a specific example. A substantial body of research has focused on the problem of violence against women. Quantitative approaches to this problem have included a number of demographic studies and survey approaches (e.g., Browne & Williams, 1993; Horton, Simonidis, & Simonidis, 1987). Although these demographics and surveys have provided a great deal of useful information, Sullivan and Bybee (1999) note that they have not done much to identify effective strategies for women to extract themselves from violent situations and to remain free from further episodes of victimization. To address this need, Sullivan and Bybee describe an experiment in which residents from a local battered women’s shelter were randomly assigned to either an advocacy group or a control condition. The women in the former group received 10 weeks of carefully designed, intensive advocacy services following their release from the shelter, whereas those in the latter group did not. Over the next two years, both groups were assessed several times on a variety of scales objectively measuring physical and psychological abuse and general psychological functioning. At the end of the two-year period, the women who had received the advocacy services were found to be experiencing less violence and a better quality of life than those who had not. That is, the experimental method had been useful, not only in developing an effective and explicitly defined intervention strategy, but also in supporting the inference that that strategy had been a clear and identifiable causal factor in the women’s improved situations.

In contrast, the kind of knowledge developed in the qualitative paradigm has a very different character. Once again, we see a wide variety of specific methods, including, but not limited to, in-depth interviewing, single and multiple case studies, participant observation, first-person accounts, the study of documents, and visual ethnography. Again, each of these methods has its own unique features, and we may wonder whether any of them can be seen as a prototype or exemplar for the qualitative paradigm. This question has not received much attention from authors in the qualitative

tradition.⁹ But I would argue that such an exemplar exists in what might be called the *collaborative contextualized dialogue* between researcher and participants—that is, the open ended discussion of meanings and of the larger material, social, and cultural context in which these meanings are interactively constructed. This kind of dialogue, which is evident in many qualitative research techniques, can be seen in relatively pure form in the unstructured conversation between ethnographer and participants where the social, cultural, institutional, and economic grounding of meanings is carefully and comprehensively explored; but it is also evident, for example, in exploratory focus groups and in-depth interviewing when these dialogic techniques are used to trace how participants identify and define material, social, life historical, and other contextual factors in the construction of personal and interpersonal meaning. Other, less dialogic, qualitative techniques—such as self-reports, semistructured interviews, and the study of artifacts and documents—all illuminate important aspects of meaning; but their source materials tend, by their very nature, to be more fixed and hence limited, unless opened up with supplementary dialogue. To put it another way, the most uniquely defining features of the qualitative paradigm emerge in their purest form in the open-ended dialogue through which contextually grounded meanings are collaboratively explored. The kind of knowledge that is produced by this process is designed not to predict and control, but rather to discover, facilitate, and support the full range of personal and symbolic forces that naturally impel participants to seek psychological and social well-being—that is, the forces that internally and externally define human subjectivity and its aspirations to individual autonomy and social integration.

Again, an example will be helpful. The problem of violence against women has been explored not only quantitatively in demographics, surveys, and experiments, but also through a variety of qualitative techniques (e.g., Symonds, 1980). Fine (1983–1984) notes that many such studies focus on the need to help abused women gain increased power and control, but she argues that this goal may be unhelpful and inappropriate for some women. In a

case study that has become something of a classic, she describes a 5-hr dialogue with one such woman—an African American mother of three fictitiously named Altamese Thomas. Thomas had been gang raped in an inner city neighborhood while drinking with some women friends. Fine, who was acting at the time as an emergency room rape counselor, began the dialogue by suggesting a number of options that might be available to Thomas for recovery and empowerment, including peer and family support, psychotherapy, social services, and prosecution of the perpetrators. Thomas, however, rejected all of these options, and, over the next several hours, patiently explained to Fine why each option would not be helpful to her. Fine describes how she came to see that the limitations, privations, responsibilities, complexities, and social status location of Thomas's position rendered each of these options difficult, burdensome, unlikely to be helpful, potentially troublesome, and, in some cases, dangerous. She notes that options that counselors offer for empowerment typically presuppose the availability of social and economic resources that middle class individuals take for granted. For those who have such resources, talking to a social worker or prosecuting one's rapist may mean "support" or "empowerment," but for those without these resources such actions may mean dwelling uselessly on bad memories, being disbelieved, being investigated, risking loss of custody of one's children, experiencing retaliation by the rapist, and risking the jailing of male relatives who might try to avenge the crime. Fine concludes that what initially appeared to be resistance or resignation on Thomas's part was actually an effective style of coping by relying on resources she could trust (e.g., friends and religious beliefs), and she recommends that those who wish to help people like Thomas

⁹ A number of theorists have described specific methods as models of specific qualitative approaches, but only a handful of these (e.g., Dervin, Grossberg, O'Keefe, & Wartella, 1989; Mishler, 1990) have analyzed such models as possible paradigmatic exemplars in Kuhn's sense; to the best of my knowledge, none of these authors has examined the details of the core exemplars that define and differentiate both the quantitative and qualitative paradigms. Eisenhart (2006), however, has noted the controlled experiment as the "gold standard" and exemplar of the quantitative paradigm and has argued for the superiority of interpretive studies and practical studies for qualitative research.

support their need to take control of their situations and to develop solutions in their own ways. Rather than contributing to the development of reliable techniques of external intervention, therefore, Fine's willingness to suspend her preconceptions and follow Thomas's dialogue wherever it led produced a radically different kind of knowledge—one that transformed her previous understanding and that equipped her to look at Thomas in a deeper, more comprehensive, and more respectful way, as someone whose own insights and ingenuity might be carefully drawn out and productively supported.

The Relationship Between Quantitative and Qualitative Approaches

Let us return now to the relationship between the quantitative and qualitative paradigms and—specifically—to the question of how separate the two paradigms actually are from each other. Each paradigm is differentiated from the other by the kind of knowledge it produces, and especially by the relatively pure form of such knowledge that is refined in its paradigmatic core exemplars. In the quantitative paradigm, this is exhibited most fully in the ability of controlled experiments to produce (or at least approximate) knowledge that is exact, reliable, and causally necessary and that can therefore provide the basis for prediction and control of important psychological and social phenomena. In the qualitative paradigm, on the other hand, the most refined knowledge is produced in collaborative contextualized dialogues that freely penetrate the many connotations and ramifications of individual and social events, wherever they may lead, and create pathways for identifying, understanding, and supporting the usually unspoken wishes, needs, obligations, and insights that human beings must ultimately confront and reconcile. The discrepancy between these two types of goals—the externally imposed prediction and control sought within the quantitative paradigm and the internally driven aspirations toward psychological autonomy and social reconciliation promoted and facilitated within the qualitative paradigm—defines the separation of the two paradigms.

We must note in passing, however, that this separation is not an absolute one, for there is nothing to prevent the practitioners of either

paradigm from pursuing the goals associated with the other. Quantitative techniques—including controlled experiments—can potentially be used to study socially and culturally rich meanings and support the efforts of research participants and others to appropriate these meanings in shaping their identities and relationships. For example, we can imagine a different version of Sullivan and Bybee's experiment in which the advocates used questionnaires order to measure elements of the battered women's self-images to help them clarify how they felt about themselves, their relationships, and their future options. Likewise, the collaborative dialogue of qualitative studies can theoretically suggest strategies for prediction and control. For example, Fine could appropriate the findings of her interview with Altamese Thomas to anticipate and influence the behavior of future rape victims. It is clearly possible, therefore, and no doubt fruitful in some situations, for the methods of one paradigm to be used to support the goals associated with the other.¹⁰ I would suggest, however, that although such cross-paradigm applications have certain uses, and in some cases may have specific advantages, they also entail a variety of complications, some of which can involve significant problems. At a minimum, cross-paradigm research activity will tend not to exploit the unique strengths of each research approach. I discuss these issues in more detail below, but for the present a consideration of the two paradigms and their particular strengths will bring us back to the issue of resistance to qualitative research and why it is so intransigent.

The quantitative and qualitative paradigms are separated not only by the goals they most effectively support and by the kinds of knowledge they tend to produce, but also by the kinds of logic underlying the two paradigms—particularly by the kinds of logic underlying the paradigmatic exemplars. Research methods within the quantitative paradigm are designed to produce knowledge that progressively moves, as it is refined and accumulated, toward greater exactitude, reli-

¹⁰ Another possibility is that a research method may be used to support a combination of goals (or a general goal) shared by both paradigms, or a goal (or goals) associated with neither paradigm. I will give examples of these below.

ability, and causal specificity, especially in the results of carefully designed programs of controlled experimentation. Epistemologically, this movement can be understood as one that systematically reduces doubt and systematically approaches certainty. This statement should not be confused with the naïve claim that experiments and other forms of quantitative research *actually achieve* knowledge that is certain and final. Experimentalists will be the first to acknowledge that their findings are tentative, and at best probable. The relevant point, however, is that *research methods of the quantitative paradigm are constructed under the constitutive assumption that knowledge produced within this paradigm will move toward certainty, and that it will do so asymptotically as exactitude, reliability, and causal specificity are increased*. Indeed, this is one of the most compelling and attractive features of quantitative research—and more specifically, of controlled experimentation—and it is one of the main sources of resistance to qualitative approaches.

The belief that quantitative research contributes to knowledge that approaches certainty in a sound and reliable way is a powerful motivating and validating factor. This is all the more true when the paradigm governing that research methodology is the same one that has supported the knowledge accumulation and technological success of the hard sciences. From this perspective, qualitative methods, with their lack of adherence to the requirements of operationalism, replicability, and control of extraneous variables, appear deeply—and perhaps fatally—inadequate. At best, they seem to be auxiliary tools, useful only for generating hypotheses to be tested by quantitative methods, and/or for illustrating or clarifying quantitative findings after the fact. This belief—that qualitative research must be accommodated to the goals and criteria of quantitative research—can take a variety of forms and constitutes what Smith and Heshusius refer to as methodological “capture” of qualitative research by quantitative research (Smith & Heshusius, 1986, p. 10). As mentioned earlier, even qualitative researchers sometimes succumb to this perspective—partly, I believe, because they, too, assume that the movement toward certainty is

synonymous with validity; but more, perhaps, because they do not have a clear and firm alternative conception of validity appropriate to qualitative research. That is, consciously or unconsciously, they regard qualitative findings more in terms of what they are not than what they are.

But the knowledge produced by qualitative research has its own unique and important epistemological features. These features are neither promoted by nor supported within the quantitative paradigm. In fact, the more carefully and rigorously quantitative techniques are developed, the more thoroughly they eradicate these very features. The features I am referring to reside in—among other things—the connotative and rhetorical richness of the natural language in which qualitative findings are expected to be expressed. The dynamics of meaning construction and the social and cultural complexities that are opened up in qualitative research, and particularly in collaborative contextualized dialogue, cannot be effectively, or even adequately, contained by the imposition of operational definitions, requirements of replicability, and attempts at reduction to lawlike causation. Rather, qualitative findings—and the insights and potential transformations they encompass—can only be fully captured in the organic flexibility of ordinary language. This is one reason why qualitative findings often include significant amounts of quoted material. Of greater methodological importance, however, is the fact that the qualitative researcher who reports these findings must also make skillful use of natural language—not only in summaries, paraphrases, and narratives, but also in theoretical analyses and inferences which, in qualitative work, may additionally draw productively on rhetoric, connotation, and metaphor. A fact about language that is not sufficiently appreciated but that has been painstakingly demonstrated by Wittgenstein, Quine, and other analytic philosophers is that *the reverence and fluidity of natural language give it greater, not less, precision in penetrating the details of complex social contexts* (e.g., see Wittgenstein, 1953, secs. 65–71 and Quine, 1960, pp. 87–88, 125–129). Thus, Fine crystallizes Thomas’s situation in the following poignant passage:

Unable to trust existing institutions, for Altamese self-disclosure would have exposed wounds unlikely to be healed. Responsible for a network of kin, Altamese could not rely on but had to protect her social supports. Resisting social institutions, withholding information, and preserving emotional invulnerability emerged as her strategies for maintaining control. . . . Expecting God to prosecute, loss of memory to insure coping, and fantasy to anesthetize reality, Altamese is by no means helpless. (Fine, 1983–1984, p. 256)¹¹

It is not necessary for the qualitative researcher to be a creative artist, but he or she does need to understand the power of natural language and to report findings in a way that respects this power and, when appropriate, makes use of it.

Epistemologically, the movement of knowledge in qualitative research is not toward greater exactitude, reliability, and causal specificity, nor toward minimizing doubt and converging on certainty. Rather, what is accumulated here is knowledge that is increasingly rich with heuristic potential, that opens up possibilities for understanding by tracing an ever expanding field of meanings—what Eco calls the “infinite semantic recursivity” of natural language (Eco, 1978, p. 121). This property of heuristic richness defines what Herbert Blumer, in the early years of the qualitative movement, designated “sensitizing” knowledge, to distinguish it from the “definitive” knowledge of quantitative research (Blumer, 1969). What Blumer was referring to was the way in which the findings of collaborative contextualized dialogue and other qualitative methods, when appropriately amplified by the descriptions and analyses of the qualitative researcher, trace the intricate contours of the socially grounded symbolic interaction—and, we may add, provide the insights necessary to facilitate and support the internally driven forces of psychological and social emancipation and reconciliation.¹²

This further clarifies why the goals toward which qualitative findings are most productively applied are crucial in distinguishing qualitative from quantitative research. In the dynamics of collaborative contextual dialogue, in particular, the participant may articulate previously unexpressed or unrecognized wishes or concerns, and these may go in a direction completely unanticipated by either the participant or the researcher. This is clearly illustrated in Thomas’s case, where Fine learned that all of her previous assumptions were wrong and that the

kind of help she thought Thomas needed was worse than useless. But more than this, Thomas articulated, and Fine learned, something about why this was the case. Fine’s study shows why definitive knowledge may be not only inadequate but also seriously problematic in certain situations. A rape counselor in an inner city hospital who works from a base of reliable knowledge and definitive expertise may be *less* able to help a woman like Thomas than one who has accumulated an open-ended *sense* of the *kinds* of complex and unpredictable social dynamics Thomas might be facing—in a word, one who relies on sensitizing rather than definitive knowledge, and who can therefore help Thomas, and women like her, to identify their own needs, problems, and solutions.

Qualitative methodology must not be understood, therefore, simply as a more free-wheeling approach to research which is defined primarily by its lack of adherence to the more traditionally scientific conventions that refine evidence toward progressively greater certainty and progressively more effective prediction and control. Rather, qualitative methodology needs to be recognized as a different kind of knowledge-construction, with its own rules and principles, and, indeed, one that embodies an entirely dif-

¹¹ Minor changes in punctuation have been added for clarity.

¹² This argument assumes that the aspirations of the individual and the needs of society are reconcilable—or at least compatible—and that supporting these needs is desirable. Of course, legitimate questions can be raised about these assumptions. For example, will a qualitative investigation of a white supremacist group support the aspirations of its members? Do we want it to? One possible response is to deny that qualitative findings that support or consolidate racist attitudes represent a true exploration of the issues, but this may be begging the question. A more direct response is that, as noted above, the findings of qualitative research do not have to be applied to the goals ordinarily associated with its governing paradigm. For example, we might study a racist group simply to understand it better, perhaps to inform social policy; but we would also expect that the qualitative findings in this case would give greater insight and depth of understanding than we usually associate with quantitative research. Additionally, however, we should not be too quick to rule out the possibility that some participants in such a study might actually want to reflect on their motivations. Studies of white supremacist groups do, in fact, sometimes produce surprising results of this kind (for example, see Ezekiel, 1995). The larger and more relevant point, though, is that well-conducted qualitative research *can* support the true interests of those who are researched if they are genuinely open to its doing so.

ferent way of thinking about the social world. This other approach to social knowledge asserts, in effect, that for many human problems externally fashioned interventions are not necessarily the most useful approach, that what may be needed, instead, is a more fundamentally democratic type of inquiry that enables those who confront social and psychological issues and problems to define, explore and articulate them in detail, and, when appropriate, to identify plans of action that make sense in the context of their own lives, values, and particular cultural and material situations. The failure to recognize this positive aspect of qualitative research lies at the heart of the resistance it so frequently encounters; and this is the reason why the “paradigm wars” continue to simmer, decades after the first calls for them to end. As long as the differences between quantitative and qualitative methodologies—and between their associated paradigms and exemplars—are not fully understood and appreciated, parity between quantitative and qualitative approaches will not be possible. The dominance of the quantitative paradigm will continue to be mechanically and unreflectively upheld, as vividly illustrated by the introductory psychology textbooks described at the beginning of this article. Those who support qualitative research will have no choice but to critique the misunderstandings that underlie this dominance, and the paradigm discussion will inevitably continue.

Mixed Methods Research

I would like to conclude by making some comments about the combining of qualitative and quantitative research methods. As I noted earlier, one of the main motivations behind the calls to end the paradigm wars has been the interest in promoting mixed methods research. As a matter of fact, publications discussing mixed methods research have shown a notable rise in the past few years, comparable, in some ways, to the rise in publications about qualitative research. As the authors of these publications have pointed out, mixed methods designs seem to be useful for enhancing the findings of quantitative or qualitative studies, strengthening them through triangulation or corroboration, correcting for limitations or blind spots, using qualitative techniques to strengthen quantitative techniques or vice versa, and diversifying per-

spectives (Greene, Caracelli, & Graham, 1989; Teddlie & Tashakkori, 2011). Given the conception of paradigms that has been articulated in this article, therefore, it will be useful to consider how such mixing is possible and what kinds of problems it might entail.

I have argued that quantitative and qualitative research methods are distinguished by the kinds of knowledge they are designed to produce and by the kinds of goals that are most readily supported by these specific types of knowledge. Such knowledge production is most readily observable in the paradigmatic exemplars, respectively, of the controlled experiment and the collaborative contextualized dialogue, and—perhaps most clearly and concretely—in the data collection phase of each of these methods. However, as noted earlier, there is nothing to prevent researchers from using the data gathered through either one of these paradigms or their exemplars to support goals associated with the other,¹³ particularly in the phases of data analysis and application of the findings. The very dissonance of such cross-paradigm applications may produce unique insights; but as I noted above, cross-paradigm applications will also tend not to exploit the unique strengths of the originating paradigm. Furthermore, the application of research findings to paradigm-discrepant problems and goals may introduce inconsistencies as a result of differences in the ways knowledge is constructed in the two paradigms, a point which I will expand shortly.

A second kind of methodological mixing involves using methods in the service of their own paradigmatic goals but combining them with methods from the other paradigm aimed at achieving other goals with regard to the same subject matter. For example, a researcher might interview students about how they experience their education and additionally conduct quantitative assessments of their performance under various conditions. A slight variation on this is when one general goal (such as “helping students”) is divided, in effect, into subsidiary goals (such as “understanding their concerns” and “improving their performance”), which are then supported, respectively, by applying methods from each of the two different paradigms. Such methodological combinations may expand

¹³ Or, we may add, other types of goals (see Note 10).

knowledge by producing complementary findings and/or by achieving mutual corroboration (Greene, Caracelli, & Graham, 1989). A somewhat different application is when specific quantitative findings may lead to a reexamination of specific qualitative findings, or vice versa (e.g., Offer and Schonert-Reichl's [1992] quantitative critique of clinical reports of adolescent turmoil, and Way and Pahl's [1999] qualitative critique of survey data regarding adolescent boys' relationships). Critiques like these may be particularly useful when applied to cases of overreach by adherents of one paradigm or the other in interpreting the findings of their own research type. Again, though, a note of caution is in order. Engaging different methods in the pursuit of different goals can result in a superficial and unfocused analysis, or worse, may usher in subtle and unrecognized contradictions resulting from underlying incommensurate conceptualizations.

A third kind of mixed methods research involves something more like a true integration of techniques from both paradigms. This can involve a simultaneous integration of techniques (as in semiopen survey questions) or it can involve combining methods interactively over time (as when material from an unstructured interview is subsequently coded). The goals of such designs may be associated with one paradigm or the other, or with both paradigms, or they may not be clearly associated with either paradigm (such as constructing a detailed description of the values or attitudes of the members of a particular group). A common variation on this is when a technique from one paradigm is given a subordinate position by exploiting it in the development of a design associated with the other paradigm (e.g., using pilot interviews to determine survey questions or a quantitative measure to select interviewees). In other cases, the researcher may employ techniques that integrate a more truly equal balance of qualitative and quantitative features (e.g., in a semistructured interview). By robustly synthesizing features associated with both paradigms, these investigators aim to actualize the vision of theorists who argue that mixed methods research will open up a vast array of research possibilities that can answer a much wider range of research questions (e.g., Bryman, 1988, 2006; Hammersley, 1992). Once again, however, this raises the possibility of dissipating

efforts and/or combining perspectives that may not be entirely consistent with each other.

This last point is worth further elaboration. When a researcher combines or synthesizes techniques from different paradigms, it is easy to lose sight of the fact that the assumptions underlying these techniques may carve up semantic space in different ways; and when this occurs, the goals supported by these techniques may be inconsistent with each other, or even contradictory. For example, helping a survivor of sexual violence as conceptualized by Sullivan and Bybee is very different from—and in some ways incompatible with—helping that individual as conceptualized by Fine. This is an example of incommensurate conceptualizations, in this case of what it means to “help.” It can be argued that such incommensurabilities are at least partly—and perhaps entirely—bridgeable if researchers can alter their conceptions and combine them into new understandings and new methods for arriving at new kinds of knowledge about helping. Indeed, this is the argument for mixed methods research. The problem, though, is that there are no guidelines for how to bridge such incommensurabilities, and, more importantly, no clear criteria for determining exactly what constitutes success in doing so.

Theorists of mixed methods research have encountered a perfusion of problems in attempting to establish such guidelines and such criteria. One reason for these problems is the formidable variety of research techniques and the vast number of ways in which these techniques can be classified and combined (see Creswell, 2011, pp. 278–279). A more fundamental problem, however, lies in the very notion of trying to formulate such a system. Attempts to specify exact definitions of social research techniques and clear and consistent rules for combining them often resemble the methodological analyses that guide research within the quantitative paradigm: isolating and operationalizing variables and attempting to identify reliable relationships among them. Such attempts are therefore likely to favor quantitative techniques over qualitative ones by legitimizing the former and marginalizing the latter. For example, Bazeley (2003) advocates the use of software programs to integrate qualitative and quantitative data, and Teddlie and Tashakkori (2011) hold that mixed methods research involves a cyclical process of qualitatively generating theories and hy-

potheses, which are subsequently tested by quantitative techniques (p. 288). But arguments like these are—in effect—simply reversions to the traditional perspective that equates certainty with validity, that privileges definitive over sensitizing knowledge, and that sees qualitative research as essentially auxiliary and subordinate to quantitative research. Such arguments are not uncommon in the mixed methods literature and have led critics to warn that the mixed methods movement threatens to enshrine new forms of methodological capture of qualitative research by quantitative research (e.g., Freshwater, 2007; Giddings & Grant, 2007; Howe, 2004).

Thus, the mixing of quantitative and qualitative techniques appears to offer a whole new array of investigative approaches, but the freedom it promises also entails a number of complications, ambiguities, and potential problems. The irreducible complexity of social issues that originally gave rise to qualitative methodology has now been displaced into questions about the proper conduct of mixed methods research. Perhaps predictably, the result is that—based overtly or covertly on a continuing adherence to the assumptions of the quantitative paradigm—many representatives of the mixed methods community now attempt to integrate qualitative and quantitative research by generating typologies, technologies, guidelines, standards, and “absurdly reductionist checklists” (Torrance, 2008, p. 517). But as Torrance goes on to note, “[s]tandards and checklists cannot substitute for informed judgment” (p. 517). If qualitative and quantitative techniques are to be successfully integrated, the researcher must rely not on rules or formulas but rather on a careful examination of the particular problem (or problems) being studied, the goals of the research, the kinds of knowledge appropriate to those goals, the assumptions underlying the techniques to be used, and the ramifications that follow from altering them. A judgment of this kind cannot occur unless the researcher fully understands—among other things—the nature of both the qualitative and quantitative paradigms and how the techniques under consideration stand in relation to each one.

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