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Paradigms or Toolkits? Philosophical and Methodological Positions as Heuristics for Mixed Methods Research

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My main goal in this paper is to challenge the validity and usefulness of the concept of “paradigm,” as this term has been used in the social sciences generally, and specifically in the debates over research methods. This concept was largely drawn from Thomas Kuhn’s influential book *The Structure of Scientific Revolutions*. In his 1969 Postscript to this work, Kuhn described a paradigm as “the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community” (1970, p. 175). Despite this broad definition, however, Kuhn primarily focused on the substantive theories and methods of such communities.¹

In contrast, participants in the methodological “paradigm wars” in the social sciences have focused on the *philosophical* beliefs and assumptions of different methodological communities, and have seen these philosophical positions as foundational for research practices. They generally assumed that quantitative and qualitative research are based on different philosophical paradigms—positivism or postpositivism for quantitative research, and constructivism for qualitative research.

The discussion of paradigms in the mixed method community has largely accepted this emphasis on philosophical beliefs, and has mainly sought to determine *which* philosophical position(s) is, or are, an appropriate basis for mixed methods research. A currently widespread view within the mixed methods community is that pragmatism is the appropriate philosophical paradigm for mixed methods research (Biesta, 2010; Johnson & Gray, 2010; Tashakkori & Teddlie, 2010), and mixed methods research itself has been promoted as a “third paradigm” alongside quantitative and qualitative research (e.g., Johnson & Onwuegbuzie, 2004).

My critique of this view is motivated by, and grounded in, my skepticism about some of the assumptions that have informed the concept of “paradigm” and how this concept has been used in the debates about social research methods. First, I question the assumption that communities are united primarily by their shared beliefs and values, a view that was central to Kuhn’s argument and has been retained in almost all subsequent discussions of paradigms in the social sciences. Teddlie’s and Tashakkori’s (2011) advocacy of “a core set

of conceptual and methodological ideas that could bring the field [of mixed methods] together” (2011) seems to me to reflect this assumption.

This is a premise that is deeply rooted in Western social thought. It influenced, and draws from, the anthropological concept of “culture” as the beliefs, norms, and values shared by members of a society or subgroup. However, the assumptions that culture is intrinsically shared, and that shared cultural beliefs are what unify societies, have repeatedly been challenged in anthropology (Atran & Medin, 2008; Hannerz, 1992; Kronenfeld, 2008; Shore, 1996; Wallace, 1970), on both theoretical and empirical grounds; see Maxwell (1999, in press) for a detailed discussion. These critics have usually taken what is called a distributive view of culture, seeing cultural beliefs and values as differentially distributed within a society, rather than intrinsically shared, and arguing that societies are united to a significant extent by the interaction and complementarity of *diverse* views, rather than solely by sharing or commonalities. Wallace (1970) referred to these two approaches to culture and cultural transmission as, respectively, the “replication of uniformity” and “organization of diversity” positions.

For this reason, I am skeptical of the call (e.g., Teddlie & Tashakkori, 2011) for “convergence on core principles” in mixed methods research. I would argue that what “brings us together” is *not* core principles or supposedly foundational beliefs or practices, but mutually productive dialogue. This issue is one that I have addressed elsewhere (Maxwell, in press; Maxwell & Mittapalli, 2010), and cannot discuss in detail here. However, if the premise that shared beliefs and values are fundamentally what unites a community is questionable, then many of the arguments for the role of paradigms, or “core principles,” in research communities are also questionable.

In addition, I am concerned that any attempt to define “core ideas” will marginalize or exclude people who don’t share these ideas. Teddlie and Tashakkori (2011) acknowledge this concern, but seem to see it as a problem that will disappear as the field becomes more “mature.” I am skeptical both of the perceived need for “core ideas” and of the view that the problem of marginalization is a temporary one. To take a specific example, Teddlie and Tashakkori list as two of their “core characteristics” of mixed methods research an “iterative, cyclical approach to research” and “a set of basic signature designs.” I disagree with both of these characteristics, and in a chapter (Maxwell & Loomis, 2003) in the first edition of the *Handbook of Mixed Methods* I criticized

¹ Kuhn (1970) emphasized that a second meaning of “paradigm,” as a concrete puzzle-solution adopted as a model or exemplar for practice, replacing explicit rules for scientific practice, was more fundamental than the meaning of paradigms as shared beliefs and practices. However, this second meaning has received almost no attention in the debates over paradigms in research methods.

these views, and proposed an alternative, systemic model for mixed method design.

Second, I do not believe that paradigms themselves are typically logically consistent and unified systems of thought, a claim that Kuhn never made. The concept of unified paradigms has frequently been challenged within the mixed methods community (e.g., Bergman, 2008; Biesta, 2010; Hammersley, 1992), and similar challenges have been made to supposedly “paradigmatic” positions within both the qualitative (Pitman & Maxwell, 1992) and quantitative (Gigerenzer, 2004) traditions. There has been ongoing debate within anthropology over the extent to which cultures are “unified” in this sense.

I am not denying that cultures, and “mental models” (Greene, 2007) in general, exhibit *some* form and degree of coherence—for example, the mental models that Mohr (1982) called “variance theory” and “process theory,” which represent the approaches to explanation of many quantitative and qualitative researchers, respectively. I am simply arguing that such coherence is not necessarily a matter of philosophical consistency, but of pragmatic compatibility, and is in principle an empirical question, rather than a strictly logical one (Maxwell, 1990).

A more extensive critique of this view of “paradigms” has been developed by the sociologist Andrew Abbott (2001, 2004). Abbott argued, on the basis of numerous examples from a range of the social sciences, that philosophical and methodological positions, rather than being unified sets of premises that strongly shape the practices of particular communities of scholars, function instead as *heuristics*, conceptual and practical tools that are used to solve specific problems in theory and research. He stated that, “the idea of heuristics is to open up new topics, to find new things. To do that, sometimes we need to invoke constructivism . . . Sometimes we need a little realism” (Abbott, 2004, p. 191). Wimsatt (2007) has provided a detailed philosophical justification for such a heuristic approach, and applied this approach to numerous issues in biology.

Similar views were presented by Hacking (1999), who analyzed how particular phenomena (mental illness, child abuse, nuclear weapons, rocks) can be usefully seen as both “real” and “social constructs,” and by Seale (1999), who argued for seeing different philosophical positions as resources for thinking. Koro-Ljungberg (2004) similarly discussed how qualitative researchers could address validity issues when they employ divergent and potentially contradictory theories, without attempting to reconcile these theories.

My views on this issue are similar to Greene’s (2007; Greene & Hall, 2010) dialectic stance for mixed methods research. The goal of this approach is to create a dialogue between diverse perspectives on the phenomena being studied, so as to *deepen*, rather than simply to broaden or triangulate, the understandings gained. The main difference between my approach and Greene’s is that she deals mainly with dialogue

between discrete paradigms or “mental models,” while I emphasize the decomposability of paradigms into separate conceptual tools that can be used somewhat independently of any larger paradigmatic framework. This perspective relates to another of Teddlie’s and Tashakkori’s “core principles,” methodological eclecticism, but applies this principle to philosophical ideas, rather than simply to methods. If such ideas can function as heuristics, as tools in a toolkit, rather than as “core” or “foundational” ideas and strategies, it is not clear why researchers would need to *agree* on these.²

I want to emphasize that in criticizing what I see as the misuse of the paradigm concept, I am not arguing for dismissing or ignoring philosophical ideas and stances in mixed methods research. This is not only because such ideas can be useful heuristic tools for mixed methods researchers, as argued above. It is also because these ideas and stances are real properties of researchers, and have an important influence on their research practices. These assumptions and influences are often unconscious, and thus it is critical for researchers to become aware of and to understand the philosophical views that they hold, how these are shaping their research decisions, and how they can use these views productively as tools for understanding the phenomena they study (Maxwell, 2005).

The view that I am presenting here draws on a key idea of postmodernism: that diversity itself is fundamental rather than superficial (Bernstein, 1992; Rosenau, 1992). This view is closely linked to a second aspect of postmodernism, a skepticism toward totalizing, foundational theories. While I have serious reservations about much of postmodern thought (Maxwell, 2010), I find these two ideas to be particularly useful conceptual tools for making sense of paradigms.

This paper is itself an example of the approach that I advocate. I am borrowing particular ideas from different authors and perspectives, and I am attempting to put these together into a toolkit that can be useful in thinking about mixed methods research. Thus, I have taken the distributionist view of culture, the critique of paradigms as unified philosophical stances for research, Abbott’s idea of philosophical premises as heuristics, a realist understanding of researchers’ philosophical assumptions and their influence on practice, a constructivist epistemology, and postmodernism’s assumption that diversity is fundamental, and its critique of foundationalism, and tried to show how, together, these ideas can be useful in thinking about mixed methods research. My approach is also pragmatist in that I am more concerned with how these ideas can be productively combined than with their logical implications.

There is a term that captures much of this approach—*bricolage*, which was used by the French anthropologist Claude Lévi-Strauss to distinguish mythological from “scientific” thought (In current French usage, *bricolage* means “do-it-

² Biesta (2010) takes a very similar position to the one I’m defending here, arguing 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” (p. 97).

yourself,” and is used to refer to stores like Home Depot.)³ Levi-Strauss described the *bricoleur* as someone who uses whatever tools and materials are at hand to complete a project. The key idea is that, rather than developing a logically consistent plan in advance and then systematically following this plan, the *bricoleur* spontaneously adapts to the unique circumstances of the situation, creatively employing the available tools and materials to create unique solutions to a problem.⁴ This concept was applied to research methods by Denzin and Lincoln (2000), and developed more extensively by Kincheloe and Berry (2004).

From this stance, I do not think it is generally appropriate or useful to attempt to synthesize different philosophical approaches or assumptions into a single, logically consistent paradigm for mixed methods research. Different situations and research problems may require different sets of assumptions and models, as well as different combinations of methods. In this, I am borrowing the philosopher Nancy Cartwright’s concept of “causal pluralism” and applying this more broadly to research methods and mental models, not just to causation. Cartwright (2007) stated that different approaches to causality “are not alternative, incompatible views about causation; they are rather views that fit different kinds of causal systems” and that “there is no single interesting characterizing feature of causation; hence no off-the-shelf or one-size-fits-all method for finding out about it, no ‘gold standard’ for judging causal relations” (p. 2).

Also, it may be fruitless or counterproductive to attempt to resolve all of the contradictions among different premises. As the philosopher Richard Bernstein (1992) argued with respect to Habermas and Derrida,

I do not think there is a theoretical position from which we can reconcile their differences, their otherness to each other—nor do I think we should smooth out their “aversions and attractions.” The nasty questions that they raise about each other’s “project” need to be relentlessly pursued. One of the primary lessons of “modernity/postmodernity” is a radical skepticism about the possibility of a reconciliation—an *aufhebung*, without gaps, fissures, and ruptures. However, *together*, Habermas/Derrida provide us with a force-field that constitutes the dynamic, transmutational structure of a complex

phenomenon—the phenomenon I have labeled “modernity/postmodernity.” (p. 225)

I am also skeptical of the view that successfully combining diverse methods depends on what they have in common. For me, “coherence” is best understood not as a matter of similarity or shared characteristics, but of pragmatic compatibility. Such a pluralist view of coherence undercuts “incompatibility” arguments against mixed methods research, which are based on the assumption that differences in *premises* lead to incompatibility in practice.

However, I am not endorsing an eclectic, “anything goes” approach to research design or to one’s philosophical premises. As Bernstein states, the different implications of diverse premises for one another, and for the research, need to be “relentlessly pursued.” I advocate seeking the deeper understanding that can be gained from juxtaposing diverse approaches and “mental models,” but also systematically *testing* one’s premises and conclusions against plausible “validity threats” and alternative understandings. This approach requires playing what the writing teacher Peter Elbow (1986) called the “believing game” and the “doubting game,” asking, for each conceptual model or assumption, what believing this model or assumption enables us to see, and also in what ways this model or assumption is misleading, incomplete, or unhelpful (Maxwell, 2010).

The overall point that I want to make here is that philosophical stances and assumptions, like theories, are lenses through which we view the world. These lenses are essential for our understanding, but the views they provide are fallible and incomplete, and we need *multiple* lenses to attain more valid, adequate, in-depth knowledge of the phenomena we study.

This approach recognizes the importance for research of philosophical premises, without assuming that these premises are “foundational.” It also recognizes the connections among the specific premises of an approach, without assuming that these constitute a unitary, coherent “paradigm.” As a flexible toolkit of different methods and “lenses” for understanding the phenomena we study, I believe that this is both a more logical and a more productive stance for mixed method research than locking ourselves into a single paradigm or worldview.

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3 I realize that the term “bricolage” itself is subject to a variety of interpretations, and I agree with the critiques of bricolage as interpreted in many of these ways (e.g., Hammersley, 2008). In this paper, I am emphasizing the aspect of bricolage that challenges the necessity of following a logically consistent paradigm or predetermined plan.

4 A similar point was made by Shadish, Cook, and Campbell (2002), in what is widely regarded as the pre-eminent work on experimental and quasi-experimental research design, of focusing on design elements, rather than a finite series of designs. The former allows the creation of new designs, and the use of specific elements of experimental design in non-experimental research. They state that, “we hope to help readers acquire a set of tools that is flexible enough so that some of them will be relevant . . . in almost any research context” (p. xviii).

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