The “Silo Problem” in Mixed Methods Research

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ABSTRACT
Mixed methods approaches and methods are being developed and used in a wide range of disciplines and subfields, but there is a striking lack of interchange of ideas between some of these disciplines. This article presents some examples of this lack of interchange, and describes the mixed methods approaches and developments in three disciplinary areas—process tracing in political science, design-based research in education, and sociolinguistics—that are creating and applying important innovations in mixed methods, but are not employing, nor are they recognized in, the ideas and methods of the self-identified “mixed methods community.” The article documents the lack of recognition of some other fields using mixed methods, argues that this siloing is a pervasive problem in the mixed methods community, suggests possible reasons for this lack of communication, and identifies important potential benefits from greater interchange among diverse communities of mixed methods researchers.

KEYWORDS
Cross-disciplinary exchange; design-based research; process tracing; sociolinguistics

The title of this article uses what may be a uniquely American metaphor, so I want to begin by explaining this. Literally, a silo is a large, airtight structure, typically cylindrical, that is used to store grain or feed for animals. Metaphorically, “silied” describes a program, tradition, or group that has little or no communication with people outside of that group. What I’m arguing in this article is that mixed methods researchers are substantially siloed—that there exists multiple groups or traditions that are integrating qualitative and quantitative approaches and methods, but having little interchange with other groups or traditions. I’m defining “mixed methods researchers” broadly as all researchers who are developing and/or using the theory and practice of integrating qualitative and quantitative approaches and methods, regardless of how they refer to themselves or whether they use the term “mixed methods.” My goal in this article is not simply to establish the existence of this siloing, but to argue that mixed methods research could benefit substantially from greater communication among these largely independent communities.

There is widespread support within the mixed methods community for the view that mixed methods research involves not just the joint use of qualitative and quantitative methods or data, but the integration of these (Bryman, 2007; Greene, 2007; Hesse-Biber, 2010; Morgan, 2014); Teddlie and Tashakkori (2006) labeled studies that employ qualitative and quantitative methods, but lack integration, as “quasi-mixed studies” (p. 142). Thus, I’m defining mixed methods researchers broadly, as all researchers who are integrating qualitative and quantitative approaches and methods, regardless of how they refer to themselves or whether they use the term mixed methods. In contrast, I use mixed methods community (in which I include myself) to refer to those self-identified mixed methods researchers who are involved with the Mixed Methods International Research Association, the Journal of Mixed Methods Research, or other labeled mixed methods organizations, or whose work is primarily oriented toward these sources and publications.

The present article draws on a previous article, “Expanding the History and Range of Mixed Methods Research” (Maxwell, 2016), in which I argued that presentations of the history and scope of mixed methods research, in most textbooks and articles by scholars within the mixed methods community, are incomplete and distorted, in three ways:
1) These presentations largely ignore, or simply mention with no discussion, the deliberate and explicit integration of qualitative and quantitative approaches and methods in important works in the social sciences before the 1970s, going back at least to DuBois’s (1899) *The Philadelphia Negro.*

2) They almost completely ignore such integration in the natural sciences; the integration of qualitative and quantitative methods and data is commonplace in sciences such as geology, planetary astronomy, and biology, and can be traced back at least to Galileo.

3) They don’t significantly address those disciplines and subfields in the social sciences that are currently combining qualitative and quantitative methods independently of the work that’s been done within the mixed methods community.

In that article, I argued that there are important lessons for mixed methods researchers in these unrecognized works, and that we need to pay more attention to what has been going on outside the explicitly identified mixed methods community. Some examples of such work, and their implications for integrating qualitative and quantitative approaches, are discussed in more detail in Maxwell, Chmiel, and Rogers (2015).

In the present article, I argue more specifically that multiple fields and disciplines in the social sciences are combining qualitative and quantitative approaches and methods without substantial interchange with other fields that are also doing this. In making this argument, I’m aware that I’m presenting views that differ from other, prominent positions in the mixed methods community, and that much of what I say will be controversial and provocative. My underlying premise in doing this is my belief in diversity and in dialog across differences (Maxwell, 2012, Chapter 4), expanding our understanding by creating a conversation between communities employing different mixed methods perspectives and practices. What I want to do here is to describe the siloing that I see, and to present some potential advantages of overcoming this.

Several recent examples illustrate this siloing:

1) A book by two political scientists, published by Cambridge University Press in 2014, titled *Finding Pathways: Mixed-Method Research For Studying Causal Mechanisms* (Weller & Barnes, 2014), contains eight pages of references, ranging widely across political science, sociology, economics, history, law, and quantitative and case study methods. However, despite using the term “mixed methods,” it contains no citations of work by people who would be recognized as members of the mixed methods community, nor any references to books on mixed methods research or to articles in mixed methods journals.

2) A book titled *Sociolinguistic Fieldwork* (Schilling, 2013) has a section, “Enriching quantitative sociolinguistics with qualitative data/methods” (pp. 8-11), that doesn’t cite any of the explicitly mixed methods literature, nor does it use the term mixed methods. Another book on sociolinguistic methods (Meyerhoff, Schlee, & MacKenzie, 2015) contains a short chapter on “Mixing qualitative and quantitative analysis” that references several works in the explicit mixed methods tradition, but only for the definition of mixed methods. The authors claim that “arguably sociolinguistics ‘got’ the point of mixing methods long before it became trendy in the social sciences” (p. 161), citing Labov’s (e.g., 1966) work in the 1960s, and that “what we are talking about in this chapter is not exactly archetypal mixed methods research—very few sociolinguists do that” (p. 160).

3) A more recent work, *Methods That Matter: Integrating Mixed Methods for More Effective Social Science Research* (Hay, 2016), explicitly addresses the siloing of research methods—specifically, between psychology (seen as dominantly qualitative), anthropology (seen as dominantly qualitative), and education (pp. xi-xii). Mixed methods is presented as a way to overcome this siloing, with 20 chapters presenting examples and discussions of studies that combine qualitative and quantitative methods and disciplinary perspectives. Many of these chapters contain important insights and guidance on integrating methods. However, only one paragraph of one of the chapters cites any of the scholars who are prominent in the mixed methods community, briefly listing several contributions from this literature, but with no discussion of these.

In what follows, I describe three research communities—design-based research in education, process tracing in political science, and sociolinguistics—in which researchers are doing mixed methods research, in the sense that I’ve described, while almost never referencing or, apparently, communicating with researchers in some other disciplines and communities, including the self-defined mixed methods community, that are also doing this. A number of other fields and methodological communities within the social sciences, including anthropology and archeology, have also substantially incorporated the joint use and integration of qualitative and quantitative methods, with little or no reference to work by researchers in the mixed methods community. (For examples in archeology and the natural sciences, see Maxwell [2016] and Maxwell et al. [2015].) I’m not arguing that these communities are siloed in general, but only with respect to interchange with other communities that are also integrating qualitative and quantitative approaches.

My goal is not simply to establish that this siloing exists, but to describe how qualitative and quantitative approaches are being integrated in these three fields. This is because siloing, as a concept, cuts both ways; not only
are these fields significantly siloed with respect to mixed methodology, but the mixed methods community is itself siloed in largely ignoring these developments, which I believe can make important contributions to our understanding of how to combine approaches and methods.

Design-Based Research

Design-based research (DBR) is a widely used approach to improving educational interventions and environments (Anderson & Shattuck, 2012; Bannan-Ritland, 2003; Brown, 1992; Collins, 1992; Kelly, Lesh, & Baek, 2008). This approach was originally conceptualized by Brown (1992) as beginning in an experimental setting and proceeding to more natural environments through multiple iterations of an intervention, continuously testing and refining the intervention, and assessing not just whether the intervention is effective, but how and why it is. However, many DBR studies now begin with more naturalistic observations of, and interviews with, students responding to a new teaching strategy, and then transition to larger-scale trials using randomized or carefully stratified samples, although still incorporating qualitative data collection (Middleton, Gorard, Taylor, & Bannan-Ritland, 2008). Ejersbo et al. (2008) argue that “there is no straightforward way to plan and carry out a design research project. There are many possibilities and many caveats and there are no stable methodological paradigms or theoretical recipes by which to proceed” (p. 159).

DBR thus intrinsically involves an integration of quantitative/experimental and qualitative strategies—specifically, integrating experimental manipulation of the intervention with qualitative data collection and analysis, using observations (including video) and interviews, though often (and increasingly) also involving quantitative techniques, including pre/post testing of students (e.g., Martinez et al., 2008), forced-choice questionnaires (e.g., Wolf & Le Vasan, 2008), and hierarchical linear modeling (Roschelle, Tatar, & Kaput, 2008). As in many of the natural sciences, the approach typically involves concurrent observation, measurement, and the recording of both; the goal is to describe and explain specific, local features of the intervention and its outcomes, as well as the context in which these occur. Both qualitative and quantitative data inform the conclusions, and are closely integrated in order to develop and test the interpretation (theory) of what took place, as well as to generate fresh insights, new perspectives, and original understandings.

DBR has been acknowledged as a mixed methods strategy by some of its practitioners (e.g., Anderson & Shattuck, 2012; Walker et al., 2011); Clements (2008) argued that traditional qualitative methods “are actually stronger if used within the context of a randomized experiment” (p. 417). However, this does not appear to have led to these researchers’ use of ideas and strategies from the self-identified mixed methods literature. I have also found very few references to DBR in the latter literature; the encyclopedic editions of the Handbook of Mixed Methods in Social and Behavioral Research (Tashakkori & Teddlie, 2003, 2010) contain only one brief mention of DBR as a “useful resource for advancing our understanding in the field of education and social sciences” (Niglas, 2010, p. 230), but with no discussion of what, methodologically, this approach might offer.

Process Tracing in Political Science

The term process tracing originated in cognitive psychology, where it referred to attempts to identify the actual processes that led to an individual’s decisions (Bennett & Checkel, 2015, p. 5). It was introduced into political science by Alexander George, who expanded its use from studies of individuals to macro-level phenomena, such as those addressed in political science case studies. It is an approach that has seen substantial, sophisticated development within political science, drawing as well on earlier work by the statistician David Freedman (2008) on integrating qualitative and quantitative methods, and Charles Ragin’s (1987) qualitative comparative analysis (QCA), itself an essentially mixed methods approach based on Boolean algebra. (Ragin’s introduction to the 2014 reissue of this book, although still using the term “qualitative,” emphasizes that QCA was “an effort to combine qualitative and quantitative approaches” [2014, pp. xix-xx]). However, the term mixed methods is used only once, with no citations to work by authors within the mixed methods community. Bennett and Checkel (2015, p. 4) argue that the use of these strategies is almost intrinsic to drawing causal inferences from historical cases, and can be traced back to the Greek historian Thucydides.

Process tracing can be seen as a qualitative strategy, in the sense that most qualitative researchers outside of political science use the term, in that it focuses on particular events within single cases, in order to identify the processes that led to the outcome, rather than aggregating data across cases to determine the average effect of particular variables on the outcomes. It thus embodies two issues that are central to qualitative research: the importance of the local context, and a concern with understanding processes, rather than simply determining that a particular event or variable contributed to an outcome.
However, process tracing does not typically include what, for most qualitative researchers outside of political science, is central to qualitative research: a primary concern with participants’ meanings and perspectives, what is typically called an “interpretive” stance. The definition of qualitative research held by proponents of process tracing is thus quite different from that held by most qualitative researchers outside this field. Brady and Collier (2010, pp. 344-345), in a work that is deliberately intended to defend qualitative research against attempts (such as King, Keohane, & Verba, 1994) to assimilate and subordinate this to quantitative research, heuristically define qualitative research as having (a) a nominal rather than ordinal or higher level of measurement; (b) small rather than large number of observations; (c) verbal analysis, rather than the use of statistical tests, although it may involve examining covariation among variables; and (d) “thick” rather than “thin” analysis, relying on detailed knowledge of specific cases. Similarly, John Gerring’s (2012) wide-ranging textbook Social Science Methodology: A Unified Framework, 2nd edition, focusing on political science, sociology, psychology, history, economics, and anthropology, defines “qualitative” as involving “inferences based primarily on a few dataset observations (insufficient to form the basis for statistical analysis) and/or lots of causal-process observations” (p. 362).

To most qualitative and mixed methods researchers outside of political science, this approach would seem to integrate aspects of both qualitative and quantitative reasoning and methods; it combines qualitative strategies, such as “thick” verbal description of specific cases in order to understand local processes, with quantitative ones, such as thinking in terms of variables and examining the covariation among these. However, process tracing has been largely ignored in the mixed methods literature. A Google search, using the search string — process tracing mixed methods, turned up only two references that connect process tracing to the self-identified mixed methods literature. Song, Sandelowski, and Happ (2010, pp. 740-741), in their chapter in the Handbook of Mixed Methods in Social and Behavioral Research, second edition (Tashakkori & Teddlie, 2010), briefly discuss process tracing as an “emerging trend” in mixed methods research, citing Gerring (2007) and Mahoney (1999); they state that “process tracing directly addresses the challenge of how to meaningfully assemble all of the disparate information about a case into coherence to establish causality” (p. 741). And Thaler (2017), in discussing mixed methods research in the study of political violence and conflict, briefly describes the strengths of process tracing as a qualitative method, with examples of its use.

Despite this recognition of process tracing as a mixed methods approach, the development of process tracing in political science and sociology has almost completely ignored the work done by self-identified mixed methods researchers. Major works dealing with process tracing (Beach & Pedersen, 2013; Bennett & Checkel, 2015; Brady & Collier, 2004, 2010; Weller & Barnes, 2014) contain no references to publications by anyone in the mixed methods community; Andrew Bennett told me that he had looked at the Journal of Mixed Methods Research, but didn’t find anything of value there. Gerring’s (2012) textbook, in a 5-page section on multimethod research (pp. 382-386), has one footnote that lists some works by mixed methods researchers, including Creswell (2009), Greene (2007), and Tashakkori and Teddlie (1998), as well as the Journal of Mixed Methods Research. However, none of these authors are actually discussed in the text. Beach and Pedersen repeatedly mention nesting process tracing within mixed methods designs, but do not explain what they mean by “mixed-methods designs” (the term isn’t even in their glossary), or cite any works by self-identified mixed methods researchers.

Variationist Sociolinguistics

As noted previously, linguists have often combined quantitative and qualitative methods, dating at least from the work of Labov (e.g., 1966). Schilling (2013) noted that variationist sociolinguistics, “though essentially quantitative in nature” (p. 8), “has never really strayed very far from its ethnographic roots and its focus on the local as well as the global” (p. 9). She identified Penelope Eckert’s (2000) Linguistic Variation as Social Practice as “a model study exemplifying the synergistic union of quantitative variationist and qualitative ethnographic methods” (p. 9). Eckert, in discussing her fieldwork, stated that “the pursuit of social meaning in variation calls for a hybrid research practice, for while we can get at local categories and their meanings only through close qualitative work, the study of variation is very essentially quantitative” (2000, p. 69). However, she does not mention “mixed methods,” nor cite any works in the mixed methods tradition.

Both Schilling and Eckert repeatedly argue that qualitative and quantitative methods should be combined in sociolinguistic fieldwork, and note some specific strengths and limitations of each—for example, that quantitative methods can establish broad relationships among linguistic and demographic variables, and discover diversity with respect to these, and that qualitative methods can identify the influence of social meanings and local contexts (Schilling, 2013, pp. 155-173). However, neither author provides much discussion of how they integrate the two approaches.
An earlier, and undeservedly neglected, contribution to the latter issue is an account by Zentella (1990) of her use of qualitative and quantitative approaches in her fieldwork on bilingual code-switching in a Puerto Rican barrio in New York City (Zentella, 1981). This is one of the most sophisticated and original examples of such integration that I know of, and I assign this article in my doctoral course on mixed methods research.

In a discussion of the quantitative-qualitative debate in sociolinguistics, Zentella notes the limitations of each approach for linguistics, and references the work of Sankoff (1972) in integrating these; the anthropological linguist Dell Hymes (1980) characterized Sankoff’s work as “join[ing] quantitative study with the ethnography of speaking” (p. xi) and “the first to treat such integration as ‘normal science’” (p. x). Zentella (1990) then states that

The steps for achieving such an integration arise from an in-depth analysis of each speech community and, initially, of many speech events. More importantly, they demand a flexible reinterpretation of analytical constructs in the light of statistical distributions. Such integration and reintegration was the methodology pursued in our East Harlem study. (p. 79)

Zentella’s study involved two years of ethnographic research, “hanging out” on one block in this community and observing 34 children in their daily activities. “However,” she states, “ethnography alone would not allow me to be as precise as possible about the frequency and variability of particular code switching patterns” (1990, p. 79). To accomplish this, she quantitatively analyzed over 100 hours of natural communication, recorded on tape recorders in backpacks worn by the children. Her goal was “an integrated methodology that was based on real world facts and explicable in real world terms” (p. 79), one that explained code switching in ways understandable not only by scholars but by educators and the community itself.

Zentella organized her results in terms of three categories of phenomena: what can be directly observed (“on the spot”), what is in the knowledge of the speaker (“in the head”), and what can be analyzed in the language itself (“out of the mouth”). “On the spot” referred to features of the person(s) spoken to (ethnicity, gender, age) and the setting of the interaction, identified both from ethnographic observation and quantitative analysis (e.g., the rates of code-switching in different settings). “In the head” included not only community norms, but also the communicative intent and available strategies of the speaker; this also involved both ethnographic data and cross-tabulations of each child’s recorded speech. “Out of the mouth” included factors that were also observable or part of the speaker’s knowledge, but pertained specifically to the structure of the two languages, and which could be explained in these terms. In addition to identifying general patterns, the quantitative analysis enabled Zentella to discover and explain (in combination with ethnographic insights) individual differences in code-switching among the children.

Zentella (1990) argued that “although we submitted each of the variables to computerized tabulation, the true picture of their centrality in each child’s web of discourse patterns and social circle would not have emerged without the reinterpretation of numbers in the light of social facts” (p. 88). She concluded by stating that her “‘on the spot,’ ‘in the head,’ and ‘out of the mouth’ model is offered as an initial step toward a unified qualitative and quantitative approach, one that will allow the particularities as well as the commonalities to emerge” (p. 88).

Explaining the Siloing

I claimed earlier that those of us in the mixed methods research community are also siloed, though in rather different ways. This is not so much siloing by discipline—many disciplines are represented in the mixed methods literature—as it is by a primary orientation to a dialog largely within a particular theoretical and methodological framework. The actual integration of qualitative and quantitative methods in some other disciplines and communities within the social sciences, ones typically having different orientations, is rarely recognized or addressed, nor is earlier work that clearly involved such integration (Maxwell, 2016).

I see several reasons for the existence and perpetuation of this siloing:

1) Design-based research and process tracing use quite different languages and conceptual frameworks to describe their methods from those employed in the mixed methods tradition; in political science, as described earlier, even the terms qualitative and quantitative are defined rather differently from how these terms are understood in the mixed methods community.

2) Most fields outside of the mixed method community don’t see anything intrinsically problematic about combining qualitative and quantitative approaches. The issue of “paradigm conflicts,” which played a central role in the emergence of mixed methods research as a distinct approach, and continues to be a significant concern, simply isn’t salient for most researchers outside of this community. Although there have certainly been
conflicts between qualitative and quantitative approaches in some of these fields (particularly political science), they don’t seem to have led to any sense of “incompatibility” between these, nor to any identification of mixed methods as a distinct paradigm. (This is also true of earlier work integrating qualitative and quantitative approaches and methods in many of the natural and social sciences, as described in Maxwell, 2016.)

3) The mixed methods community’s focus on typologies of mixed method designs (Guest, 2013) is largely absent from mixed methods discussions outside of this community. Other fields, as described earlier and in Maxwell (2016), have developed sophisticated strategies for integrating qualitative and quantitative approaches, methods, and data without creating typologies of ways of doing this, which raises the issue of how necessary, or even useful, such typologies are for conducting mixed methods research.

One possible response to these differences would be to argue that, despite the integration of qualitative and quantitative methods in other fields, and its presence long before the explicit identification of this as “mixed methods,” the self-defined mixed methods community is nevertheless at the forefront of conceptualizing mixed methods research, examining the possible purposes and strategies for combining qualitative and quantitative approaches and methods, and developing new ways of doing this. Fetters (2016), in an editorial in the Journal of Mixed Methods Research responding to articles by Maxwell (2016), Pelto (2015), and Ramlo (2016), seems to take essentially this position. He states that “I believe the current generation of [mixed methods] researchers has witnessed the dawn of a new breakthrough” (p. 8), listing four recent advances—in analysis and integration, in developing “a lexicon and rubric that is understood and used across multiple disciplines,” in the integration of philosophical frameworks, and in the creation of “teams of researchers ... that are heterogeneous, multidisciplinary and unleashing new and powerful approaches that even a staunch critic will acknowledge” (p. 9). He cites only work conducted by scholars within the mixed methods community.

Although I think there is considerable truth to the view that this community has done much to advance mixed methods, Fetters’s claims remind me of a reported headline in a British newspaper: “Storm Blankets Channel—Continent Isolated.” We—the mixed methods community—are the British Isles of mixed methods research; everyone else doing mixed methods research is the continent, and there are arguably (certainly if the natural sciences are included) more of them than there are of us. In addition, our work is not “a new breakthrough” in some important areas:

1) A major development in using mixed methods for causal explanation has been in political science—process tracing.

2) For intensively integrating experimental designs and strategies with qualitative methods, it has been in design-based research and in some of the work described in Hay (2016) and Weisner (2005).

3) For the close, fine-grained integration of qualitative and quantitative description in interpreting local phenomena and processes, it has long been in geology (see Maxwell et al., 2015, pp. 228-230);

4) For the quantification and analysis of qualitative data, the most widespread development and use has been in anthropology, archeology, linguistics, and ethology. In particular, such quantification is important for identifying the diversity of individual actions and perspectives in the settings and populations studied—an issue for which sociolinguistics and anthropology have long been in the forefront (e.g., Heider, 1972; Sankoff, 1971, 1972, 1980; Wallace, 1970), with psychology now making a significant contribution (Atran & Medin, 2008; Rose, 2015). Atran and Medin (2008) integrate theories and methods from both anthropology and cultural psychology, in what is essentially a mixed methods approach, to support a distributed, rather than shared, concept of culture, arguing that “it is just a nonstarter to treat or define cultures or groups in terms of shared properties” (p. 265). For a more detailed treatment of this issue, see Maxwell (2012, Chapter 4).

5) An additional example of the sophisticated development of such integration, largely outside of academic research and scholarship, is the now-widespread use, in professional sports, of advanced analytics in combination with video to evaluate players (Anguera & Hernández-Mendo, 2016). A particularly clear example is Bill James’s (2006) analysis of Derek Jeter’s performance as a defensive shortstop, which relied both on quantitative measures of Jeter’s play and on video comparison of Jeter’s play with that of Adam Everett, a shortstop at the opposite end of the quantitative distribution from Jeter.

Implications

I see several lessons for mixed methods researchers from this examination of mixed methods research outside of the mixed methods community:

1) The first implication that I draw from these fields (and others discussed in Maxwell, 2016) is that paradigm debates, rather than being a fundamental issue for combining qualitative and quantitative approaches, are a very localized phenomenon, limited to the recent history of some of the social sciences. Although there
have been major disagreements between qualitative and quantitative researchers in other fields (e.g., in political science), these have usually been framed in methodological rather than paradigm terms.

I’ve argued elsewhere, in an article titled “Paradigms or toolkits? Philosophical and methodological positions as heuristics for mixed methods research” (Maxwell, 2011), that the concept of “paradigm,” as used in discussions of research, needs rethinking. The term was introduced into general discourse by Thomas Kuhn (1962), with a range of meanings, but he never used it to mean a set of philosophical beliefs that served as a foundation for research. This meaning was basically initiated by Lincoln and Guba (1985; Guba & Lincoln, 1989) in their justification of qualitative research as based in a naturalistic or constructivist paradigm that was incommensurable with the positivist paradigm assumed by quantitative research (Donmoyer, 2008).

In my view, this foundational assumption isn’t an accurate or useful conception of how philosophical views actually function. Drawing on the work of the sociologist Andrew Abbott (2001, 2004) on philosophical ideas as heuristics, I argued that mixed methods research doesn’t need a single philosophical “partner,” such as pragmatism (e.g., Johnson & Onwuegbuzie, 2004); such research can be done productively from a wide range of ontological and epistemological standpoints. This isn’t to argue against the importance of the ontological and epistemological beliefs of a researcher; these are both valuable conceptual tools and potential blinders, and researchers need to be aware of the assumptions they’re making about these issues, and of how these are influencing their research. However, neither the natural sciences, nor many of the social sciences described earlier, exhibited the kinds of purportedly fundamental philosophical differences, or claims of incompatibility of the two approaches, that characterized the paradigm wars.

There are two main implications of this rethinking of paradigm issues for mixed methods research:

a) Researchers can do perfectly good mixed methods research without adopting any explicit philosophical or methodological paradigm. What is needed is an understanding of the different “mental models” (e.g., “variance theory” and “process theory” as typical ways of thinking about explanation held by quantitative and qualitative researchers, respectively; Maxwell & Loomis, 2003, pp. 248-249; Mohr, 1982) that inform research practice, and how to productively integrate these.

b) The field of mixed methods research doesn’t need to agree on a single paradigm (Greene, 2007); mixed methods research needs a single paradigm like a fish needs a bicycle. Diversity, dialogue, and complementarity, and not just shared premises and values, can be a basis for community (Maxwell, 2012, Chapter 4).

2) A great deal of mixed methods research has been conducted without using the typologies of research designs that have been prominent within the mixed methods community. This is not only true of the research described earlier, which has been done without much apparent connection to the mixed methods community. The collection of mixed methods studies edited by Weisner (2005) contains 12 chapters describing mixed methods studies of children and families, many of them program evaluations, a field that has been closely involved in the explicit development of mixed methods since the important volume by Cook and Reichardt (1979). None of these studies made use of these typologies; more broadly, there are few references to the mixed methods literature in general; the only exception is to Jennifer Greene’s work, which is largely non-typological, and is cited multiple times.

This raises the question of to what extent, and how, these typologies are in fact useful for mixed methods researchers. I believe that they are somewhat useful as a way of analyzing a mixed methods study, particularly for the more fundamental distinctions such as component versus integrated, sequential versus parallel, and so forth. However, there are serious limitations on this usefulness. These are actually continuous dimensions rather than distinct categories; studies vary in how separate the two approaches are, how much they overlap in time, and how much integration there is, and any separation into different types is arbitrary.

I’m particularly skeptical of the concept of dominance of the quantitative or qualitative approach, as a dimension for creating typologies. This is mainly because I don’t know how to validly define dominance. For example, a recent excellent article by Vicki Plano Clark and her associates (Plano Clark et al., 2013) labels their study as an embedded design study. The definition of “embedded design” that she gives requires that the embedded component be subordinate to the component in which it’s embedded. However, it’s not clear if she treats the qualitative component as “subordinate” simply because it’s embedded in an experimental design. There are studies (e.g., B. Kaplan & Duchon, 1988; Maxwell, Sandlow, & Bashook, 1986; and many of the studies included in Weisner, 2005) in which qualitative methods were used within an experimental or quasi-experimental design, but in which the qualitative methods and results were clearly not subordinate, and were in fact more important than the quantitative component in developing the final conclusions of the study.

In addition, I don’t believe that typologies of design are useful as a starting point for designing a study, which is how they’re typically presented (e.g., Creswell & Plano Clark, 2011, Chapter 3). The proper starting point (insofar as there is one) is the researcher’s goals and research questions; what approaches and methods are appropriate for addressing these has little to do with typologies, and everything to do with what sorts of data you

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will need to answer these questions. The "type" of study that emerges from this process is an outcome of these decisions, rather than the starting point in making them, although this outcome then has implications for the rest of the design (Maxwell, 2013; Maxwell & Loomis, 2003). Many researchers are doing sophisticated and innovative mixed methods research without any apparent need for such typologies.

3) Finally, I would argue for the value of using studies from outside the mixed methods community in teaching mixed methods research. In addition to providing different perspectives on mixed methods research, many of these books and articles do a better job of describing the actual process of doing the research than most current publications within the mixed methods community. I mentioned earlier the value of Zentella's (1981) article on bilingual code-switching for this purpose; I have also used other, largely unrecognized contributions, including an account by B. Kaplan and Duchon (1988) of integrating divergent qualitative and quantitative findings in a study of the implementation of a hospital computer system, and some of Bill James's (1984, 2006) essays on baseball. One of the publications that students have found to be particularly valuable in understanding how to integrate qualitative and quantitative approaches has been Stanley Milgram's (1976) Obedience to Authority, which provides an exceptionally detailed account of how qualitative observations and interviews were combined with diverse experimental protocols, and in using both qualitative and quantitative findings to support his theory of obedience.

In summary, I have argued that there is much mixed methods research going on outside the boundaries of the mixed methods community, work that is largely independent of, and unrecognized by, that community. I have also argued that there would be considerable benefit to that community in considering how this work could advance our understanding of how to conduct and present mixed methods research. This argument is supported by a classic article by Granovetter (1973), "The strength of weak ties," which argued that individuals with few weak ties [ties to those with whom they have little contact] will be deprived from information from distant parts of the social system and will be confined to the provincial news and views of their close friends. This deprivation will not only insulate them from the latest ideas ... but may put them in a disadvantaged position. (Granovetter, 1983, p. 202)

My argument is also grounded in Jennifer Greene's (2007) dialectic stance for mixed methods research, which advocates using divergent perspectives to yield better understanding, which "takes its most important form as generative insights, which are in turn best attained through a respectful conversation among different ways of seeing and knowing" (p. 79; cf. Johnson, 2017). This stance "seeks not so much convergence as insight ... the generation of important understandings and discernments through the juxtaposition of different lenses, perspectives, and stances" (Greene, 2007, p. 79; see also Johnson, 2017). Greene is making this point mainly for the actual conduct of a mixed methods study; I believe that it is also valid for the relationships among researchers who are integrating qualitative and quantitative approaches.

Notes

1. This approach was originally termed design research (Collins, 1992) or design experiments (Brown, 1992). However, the former term also has a completely different meaning, and "design-based research" is now the most common designation.
2. This is the usual definition of "qualitative" in the natural sciences. For example, qualitative analysis in chemistry simply identifies the presence or absence of particular elements, whereas quantitative analysis measures the amount of these elements.
3. These three categories partially match Abraham Kaplan's (1964) three categories of research goals: description, interpretation, and explanation. For a detailed discussion of these and other such frameworks, see Maxwell (1992).
4. A widespread assumption is that the most advanced quantitative analyses involve inferential statistics. However, inferential statistics have been widely misinterpreted and misused, and do not tell you what many researchers believe they do—the probability that your results are due to chance, or that they will fail to replicate. This misunderstanding has been partly responsible for the recent "crisis of replication" in psychology and medicine, in which many published studies with statistically significant $p$ values fail to replicate (Nuzzo, 2014). For a more detailed discussion of this problem, see Maxwell (2017) and the sources cited there.

References


