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## THE “SILO PROBLEM” IN RECENT MIXED METHODS RESEARCH

In this chapter, I discuss the development of mixed methods research during the “paradigm wars” of the 1970s and later, and the diversity of fields that have developed their own approaches to mixed methods research since that time. (I discuss the “paradigm” issue in more detail in Chapter 3.) A major point of this chapter is that there are multiple fields and disciplines in the social sciences in which researchers have been combining qualitative and quantitative approaches and methods, without substantial interchange with other fields that are also doing this; these fields are substantially “siloed”<sup>1</sup> with respect to other fields that are also combining methods. My goal in this chapter 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.

Two examples illustrate this siloing:

1. A book by two political scientists, 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 we would recognize as self-identified “mixed methods researchers,” 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 also 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

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<sup>1</sup> “Siloed” may be a uniquely American metaphor, so I want to explain this. Literally, a silo is a large, airtight structure, typically cylindrical, that is used to store grain or feed for animals. Metaphorically, “siloed” describes a program, tradition, or group that has little or no communication with people outside of that group.

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 work (e.g., 1966) 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).

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 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 self-identified mixed methods researchers, as discussed in Chapter 1; see also Maxwell (2016) and Maxwell, Chmiel, and Rogers (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 each field. This is because siloing, as a concept, cuts both ways; not only are these fields significantly siloed with respect to mixed methodology, but the self-identified mixed methods community is *itself* siloed in ignoring these developments, which I believe can make important contributions to our understanding of how to combine approaches and methods.

## DESIGN-BASED RESEARCH

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Design-based research<sup>2</sup> is a widely used approach to improving educational interventions and environments (T. Anderson & Shattuck, 2012; Bannan-Ritland, 2003; Brown, 1992; Collins, 1992; Hoadley & Campos, 2022; Kelly, Lesh, & Baek, 2008; Philippakos, Howell, & Pellegrino, 2021). This approach emerged from the “paradigm wars” of the 1980s, which fractured educational research into two antagonistic communities. This split had its origins in the opposing views of educational research held by Dewey and Thorndike, in the early 1900s:

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<sup>2</sup>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.

Research in education, Thorndike argued, should be scientific and modeled after the experimental research and statistical analysis in the physical sciences and in basic psychological research. . . . Thorndike’s views triumphed over Dewey’s for decades. . . .

In the 1980s, the education research community ruptured into factions leading to the so-called paradigm wars. . . . Heated debates about the legitimacy of these two different worldviews and approaches to research continued into the 1990s. However, as blood pressures lowered and the debates cooled, a rapprochement of sorts took hold, opening the way for some researchers to promote the use of mixed methods. (Reinking, 2021, pp. x–xii)

Design-based research (DBR) 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 later DBR studies began 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, p. 159) argued 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). And Hoadley and Campos stated that “these six types of ‘findings’ are dissimilar from those achieved either through positivistic or interpretivist qualitative research” (2022).

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, 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 best describe 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; Reinking, 2021; 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. The recent encyclopedic volume *Design-Based Research in Education* (Philippakos, Howell, & Pellegrino, 2021), aside from the acknowledgment by Reinking, cited above, contains only one brief mention of "mixed methods research."

I have also found almost no references to DBR in the "mixed methods" literature. The encyclopedic editions of the *Handbook of Mixed Methods in Social and Behavioral Research* (Tashakkori & Teddlie, 2003, 2010a) contain only one brief mention of DBR as "especially promising for advancing ideas in the field of e-learning and in more traditional areas of educational and social sciences" (Niglas, 2010, p. 230), but with no discussion of what, methodologically, this approach might offer. However, neither the third edition of *Designing and Conducting Mixed Methods Research* (Creswell & Plano Clark, 2018), the second edition of *Foundations of Mixed Methods Research* (Tashakkori, Johnson, & Teddlie, 2021), nor *The Routledge Handbook for Advancing Integration in Mixed Methods Research* (Hitchcock & Onwuegbuzie, 2022) mention design-based research.

## PROCESS TRACING IN POLITICAL SCIENCE

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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), using techniques such as think-aloud protocols and eye tracking. It was introduced into political science by Alexander George (1979), 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 work by the statistician David Freedman (2008) on integrating qualitative and quantitative methods, and Charles Ragin's "qualitative comparative method" (1987), itself an essentially mixed methods approach based on Boolean algebra. Bennett and Checkel (2015, p. 4) argued that the use of such 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 actual processes that led to the outcome, rather than aggregating data across cases to determine the 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 include what for most qualitative researchers is central to this approach: a primary concern with participants' meanings and

perspectives, what is typically called an “interpretive” stance; see, for example, Ragin’s largely dismissive discussion of this meaning of “interpretation” (1987, p. 3) in his presentation of qualitative comparative analysis. The definition of qualitative research in political science 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 to assimilate and subordinate this to quantitative research (such as King, Keohane, & Verba, 1994), heuristically define qualitative research as having (a) a nominal rather than ordinal or higher level of measurement;<sup>3</sup> (b) small rather than large numbers 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 wide-ranging textbook *Social Science Methodology: A Unified Framework*, 2nd edition (2012), 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 on this term 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*, 2nd edition (Tashakkori & Teddlie, 2010), briefly discussed process tracing as an “emerging trend” in mixed methods research, citing Gerring (2007) and Mahoney (1999); they stated 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 (2015), in discussing mixed methods research in the study of political violence and conflict, briefly described the strengths of process tracing as a qualitative method, with examples of its use. However, none of the recent volumes edited by Creswell and Plano Clark (2018); Hitchcock and Onwuegbuzie (2022); or Tashakkori, Johnson, and Teddlie (2021) contain any discussion of process tracing.

Similarly, the development of process tracing in political science and sociology has almost completely ignored the work done by self-identified mixed methods

<sup>3</sup>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, while quantitative analysis measures the amounts of these elements.

researchers. Major works dealing with process tracing (Bennett & Checkel, 2015; Brady & Collier, 2004, 2010; Weller & Barnes, 2014) contain no references to publications by anyone in the “mixed methods” community; Andrew Bennett once told me that he had looked at the *Journal of Mixed Methods Research*, but didn’t find anything of value there. The first edition of Gerring’s textbook (2007), in a five-page section on multimethod research (pp. 382–386), had one footnote that listed some works by mixed methods researchers, including Greene (2007), Creswell (2008), and Tashakkori and Teddlie (1998), as well as the *Journal of Mixed Methods Research*. However, none of these authors were actually discussed in the text, and the second edition (2012) omitted these references. Beach and Pedersen’s *Process Tracing Methods: Foundations and Guidelines*, 2nd edition (2019), repeatedly mentions nesting process tracing within mixed-methods designs, but doesn’t explain what they mean by “mixed-methods designs” (the term isn’t even in their glossary, and the one mention of mixed methods in their index says simply “see nested analysis”); nor do they cite any works by self-identified mixed methods researchers.

## VARIATIONIST SOCIOLINGUISTICS

As noted in Chapter 1, linguists have often combined quantitative and qualitative methods, dating at least from the work of Labov (1966). Schilling (2013) stated 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 *Linguistic Variation as Social Practice* (2000) 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 didn’t mention “mixed methods,” nor did she cite any works in the self-identified mixed methods tradition.

Both Schilling and Eckert repeatedly argued *that* qualitative and quantitative methods should be combined in sociolinguistic fieldwork, and noted 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, while qualitative methods can identify the influence of social meanings and local contexts (Schilling, 2013, pp. 155–173). However, neither author provided much discussion of *how* they integrate the two approaches.

An earlier contribution to mixed methods research in linguistics, and one unfortunately unrecognized by the self-identified “mixed methods” community (it also isn’t mentioned in any of the sociolinguistics works cited above), is by Zentella (1990), describing her integration of qualitative and quantitative approaches in her fieldwork

on bilingual code-switching in a Puerto Rican *barrio* in New York. This is one of the most detailed, sophisticated, and original examples of such integration that I know of. (For a full account of this study, but one that doesn't explicitly address integrating methods, see Zentella, 1997.) In a discussion of the quantitative–qualitative debate in sociolinguistics, Zentella noted the limitations of each approach for linguistics, and referenced 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 then stated 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. (1990, p. 79)

Zentella's study involved 2 years of ethnographic research, “hanging out” on one block in this community, and observing 34 children in their daily activities. “However,” she stated, “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”).<sup>4</sup> “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 (for example, 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 that 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.

<sup>4</sup>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).

Zentella 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” (1980, 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).

I claimed above that the self-identified “mixed methods research community” is *also* siloed, though in rather different ways. This is not so much siloing by discipline—many disciplines are represented in the mixed methods literature—but by a primary orientation to a dialogue largely within a particular conceptual framework. The actual integration of qualitative and quantitative methods in many other disciplines and communities within the social sciences isn’t recognized or addressed. There are several possible reasons for this:

1. Design-based research and process tracing, in describing their methods, use quite different languages and conceptual frameworks from those employed in the mixed methods tradition; in political science, as noted above, even the term *qualitative* is defined rather differently from how it is understood in the mixed methods community.
2. Most fields outside of the self-identified mixed methods 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 for some mixed methods researchers (e.g., Morgan, 2022; Tashakkori, Johnson, & Teddlie, 2021, pp. 17–23), 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 philosophical “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 Chapter 1.)
3. The mixed methods community’s focus on *typologies* of mixed methods designs is largely absent from mixed methods discussions outside of this community. Other fields 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 papers by Maxwell (2016), Pelto (2015), and Ramlo (2016), seemed essentially to take this position. He stated 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 cited *only* work within the self-identified mixed methods community.

Although I think there is some 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 self-identified 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* cutting-edge in important respects:

1. The “cutting edge” for using mixed methods for causal explanation is in political science—process tracing. I discuss this in Chapter 6.
2. For intensively integrating experimental designs and strategies with qualitative methods, it is in design-based research. I discuss this in Chapter 4.
3. For the close, fine-grained integration of qualitative and quantitative description in interpreting local phenomena and processes, it is in geology and biology, as noted in Chapter 1 (for a more detailed discussion, see Maxwell, Chmiel, & Rogers, 2015). A particularly intensive example of such integration is a 3-year study of hunting associations between badgers and coyotes (Minta, Minta, & Lott, 1992; all excerpts reproduced with permission). The authors stated that

In the research, a badger-coyote association was a sampling unit. Sampling was confined to those of 42 badgers with implanted radio-transmitters [for tracking badgers when underground] that interacted with coyotes. With random sampling, observations of experimental (sampling) units would be independent, but our observations were

not. We believe that the mobility of coyotes and telemetered badgers over four annual cycles (26 months of field time) may have approached randomized sampling. Therefore, descriptive statistics will be followed by qualified inferential statistics.<sup>5</sup> . . .

We used two types of indices of the association's costs and benefits to each species: first we compared rates of prey capture and activity budgets of each species hunting alone and hunting in an association. Then we recorded each species' response to the other's presence assuming that behavior that initiates or maintains the association is evidence that the net outcome for the behaving animal is neutral or positive, while behavior that tends to avoid or terminate the association is evidence that the net outcome is negative.

(For a brief video showing such cooperative interaction between coyotes and badgers, see <https://www.youtube.com/watch?v=eo16wFhWy2E>. The interaction is at the end of this video.)

4. For the quantification and analysis of qualitative data, it is 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; Wallace, 1970), with psychology now making a significant contribution (Atran & Medin, 2008; Rose, 2015). I provide a more detailed discussion of this issue in Chapter 3 (see also Maxwell, 1999, 2011, 2012a).
5. An additional example of the sophisticated development of such integration, completely outside the “box” of academic research and scholarship, is the now-widespread use, in professional sports, of advanced analytics in combination with video to evaluate players (e.g., James, 1984). A particularly clear instance (at least for baseball fans) is Bill James's analysis

<sup>5</sup>“Qualified” is appropriate here. A widespread view is that the most advanced quantitative analyses involve inferential statistics, defined as

a broad class of statistical techniques that allow inferences about characteristics of a population to be drawn from a sample of data from that population while controlling (at least partially) the extent to which errors of inference may be made. (*American Psychological Association Dictionary of Psychology*, accessed June 17, 2023)

However, inferential statistics have frequently been misinterpreted and misused, a misunderstanding that was partly responsible for the recent “crisis of replication” in psychology and medicine, in which published studies with highly significant p values failed to replicate (Nuzzo, 2014). For a more detailed discussion of this issue, see Chapter 7, and Maxwell (2017) and the sources cited there.

(2006) 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. Quantitative measures strongly suggested *that* Derek Jeter was not a particularly good defensive shortstop, but video comparison of the play of Jeter and Everett (combined with quantitative tabulation of the video) not only countered the argument that the initial quantitative results were simply in error in some way, but also provided an understanding of *why* Jeter’s defensive play was less effective than Everett’s. (James is clear that he’s not saying Jeter was a poor *player*; he had other strengths that more than compensated for his defensive deficiencies.)

I develop the argument for combining methods in understanding causation in more detail in Chapter 6.

## IMPLICATIONS

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

1. 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. (For a more detailed discussion of mixed methods design, see Chapter 4.) This raises the question of whether 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, etc. However, there are serious limitations on this usefulness. First, 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 somewhat arbitrary.

I’m particularly skeptical of the concept of “dominance” of the quantitative or qualitative approach as a dimension for creating typologies, mainly because I don’t know how to validly define “dominance.” For example, an excellent paper by Plano Clark and her associates (2013) labels their study as an “embedded design” study. The definition of “embedded design” prevalent in the mixed methods literature requires that the “embedded” component be subordinate to the component in which it is

embedded. However, the authors challenge the view that the qualitative component is “subordinate” simply because it is embedded, and describe the important understandings provided by the qualitative component that differ from those of the quantitative component. There are many other studies (e.g., the study by Milgram discussed in Chapter 1; 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 at least as important as the quantitative component in developing the final conclusions of the study.

2. 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, titled “Choosing a Mixed Methods Design”; in the 2018 edition, this chapter is titled “Core Mixed Methods Designs”). The proper starting point (insofar as there is one) is the researcher's goals and research questions; what conceptual approaches and methods are appropriate for addressing these has little to do with typologies, and everything to do with what sorts of data you will need to answer these questions. The “type” of study that emerges from this process is then 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. (See Chapter 4 for a much more detailed discussion of mixed methods design.)
3. Finally, I would argue for the value of using studies outside the self-identified mixed methods “box” in *teaching* mixed methods research. In addition to providing different perspectives on mixed methods research, many of these papers do a better job of describing the actual process of doing the research than most current publications within the self-identified mixed methods community. I mentioned above the value of Zentella's paper on bilingual code-switching for this purpose; I have also used other, largely unrecognized contributions, including Kaplan and Duchon's (1988) integration of divergent qualitative and quantitative findings in a study of the implementation of a hospital computer system, and some of Bill James's essays on baseball (e.g., 1984, 2006). 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 *Obedience to Authority* (1976), described in Chapter 1, 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 Milgram’s theory of obedience.

In summary, I have argued that there is much mixed methods research going on outside the boundaries of the self-identified mixed methods community, work that is largely independent of, and unrecognized by, that community, and 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 paper 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 of 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” (1983, p. 202).

My argument is also grounded in Jennifer Greene’s “dialectic” stance for mixed methods research (2007), 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). 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, 2005, p. 208). 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 those integrating qualitative and quantitative approaches. I discuss Greene’s views on mixed methods design in Chapter 4.

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