The Logic(s) of Inquiry
Reconsidering Multi-Method Approaches

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I. Introduction

Recent scholarship in political science attests to the rapid proliferation of approaches engaged in mixed-method research (MMR), research that employs two or more methods selected from an array of qualitative, quantitative, and formal methods typically used in the social sciences. Of course, the general notion that different types of methods can be employed to advance or test a particular theory is not in and of itself new. What is different about the more recent movement towards MMR is the extent to which the use of multiple methods is undertaken self-consciously by a single scholar in a single work in relation to a single research question, predicated on the assumption that the use of different methods will yield better results in addressing that question. Indeed, for some, MMR has come to represent not a pragmatic response to the complexity of a given problem but an end in itself and even a new universal standard for good scholarship.

Contemporary applications of MMR certainly show a great deal of promise. For one, the rise of MMR has spurred progressively more sophisticated and inclusive dialogues among adherents of alternative methodological traditions. In addition, scholars who have engaged in MMR have sometimes found new ways to formulate questions and to construct new hypotheses on a range of topics, ranging from political economy and electoral politics to democratization and social movements. The rise of MMR has also done much to consolidate and normalize the spirit of methodological pluralism, with the result that the value of several different methodological approaches can be taken for granted to an extent that was not possible two decades ago.
While we embrace the pluralist spirit that originally motivated MMR, in this paper, we wish to sound a cautionary note. First, while we concur that proponents of different methods often share common ground on issues related to theory and evidence (Hopf 2007) or at least complement one another in terms of their comparative strengths and trade-offs (Sil 2000a), we are concerned that insufficient attention has been paid to the epistemological underpinnings of different methods and the implications of this for the theories and inferences being advanced. Specifically, we fear that unreflecting attempts at methodological triangulation can end up producing conceptual “muddiness” or theoretical incoherence as scholars seek to employ the same variables and categories in very different ways to capture processes unfolding at quite different levels of generality over quite different time horizons (Johnson 2002; Lichbach 2003). Related to this, because of the difficulty of harmonizing methods in relation to a single coherent research question, applications of MMR face the danger of devolving into either a juxtaposition of separate intellectual exercises that happen to address a given substantive topic, or a methodological hierarchy where one of the methods is ultimately responsible for revealing the crucial mechanisms, causal logics, and empirical observations. Finally, we are concerned that what initially grew out of a genuine appreciation of the value of mixing methods in the service of problem-driven research is increasingly being transformed into a formalized “new best way,” marked by a continuing tendency to privilege methodological credentials over the practical issues related to formulating and investigating substantive problems (see Shapiro, Smith and Masoud 2004).

The paper proceeds as follows. In the next section we provide a necessarily brief (and partial) history of the evolution of the current phase of MMR, with a focus on the
path that led from King, Keohane and Verba’s (1994) *Designing Social Inquiry* to Brady and Collier’s (2004) volume, *Rethinking Social Inquiry*. In section three, we examine three commonly found combinations that exemplify the basic applications of MMR: formal-quantitative approaches, formal-qualitative combinations, and qualitative-quantitative designs. In the following section, we assess the progress made by approaches explicitly committed to MMR as well as the challenges and pitfalls of such research designs. In the process, we offer some reasons why a specific commitment to methodological triangulation and multi-method research needs to be distinguished from a more diffuse methodological pluralism and from the more fundamental challenge of building problem-specific eclectic analytic frameworks that draw upon concepts, puzzles, and mechanisms developed within separate research traditions (Katzenstein and Sil 2008; see also Sil and Katzenstein, forthcoming). In concluding, we note that methodological pluralism need not be translated into the application of different methods within a single research product, and we especially point to the dangers of new generations of scholars mechanically acquiring and applying different methodological tools because they feel pressured to do so as a result of the reification of MMR as a new standard for “good” scholarship. Instead, we urge scholars to consider the virtues of a division of labor where research is aggregated and assessed not at the level of the individual scholar but rather at the level of the entire community of scholars concerned with a given problem, regardless of whether they employ one, two or many methods.
II. Mixed-Method Research (MMR): A Short History

The use of multiple methods does not represent a fundamentally new turn in the social sciences. Scholars as far back as the late 19th century have employed different types of methods to investigate related aspects of particular theories. Emile Durkheim, for example, employed statistical research in his analysis of suicide rates (1951 [1897]), but then drew upon anthropological and ethnographic studies in his comparative study of religious beliefs and symbols among Aboriginal and Native American groups (1967 [1912]). Both methods supported a broader theoretical project aimed at illustrating the functionality of particular beliefs, social institutions, and forms of collective behavior in maintaining different types of social solidarity (1933 [1897]). The behavioral revolution of the 1950s-60s also brought with it an understanding that hypotheses derived from existing theories can be advanced or tested through both quantitative and case-study methods, even if some preferred to see the latter as a way-station to the former or perhaps a second-best alternative where reliable data and accurate measurement were not possible (e.g. Lijphart 1971). Moreover, during the 1970s the notion of methodological triangulation, later popularized in political science by Tarrow (1995) among others, was already being advocated in other social science disciplines (Denzin 1978; Jick 1979). Yet, until the 1990s, individual social scientists very rarely invested in acquiring training in different methodologies, and individual research products did not commonly use multiple methods in investigating a research question. At best, methodological pluralism implied that a community of scholars identifying with a particular intellectual tradition or
research program could collectively draw upon different methods to advance a theoretical project they were committed to.

Contemporary advocates of MMR, while still subscribing to the basic notion that multiple methods can be used to support or test particular theories or hypotheses, take one more step: They embrace the notion that individual scholars and research products can and should employ multiple methods in addressing a research question; and they implicitly or explicitly place a higher value on research products that incorporate the application of different methods. More importantly, the calls for MMR in political science are not only coming from the margins of the discipline; they have come to be embraced, at least in principle if not in practice, by the mainstream across the various subfields of the discipline. Perhaps the most important manifestation of this shift is the recent renaming of the Organized Section on Qualitative Methods of the American Political Science Association (APSA) as the Organized Section on Qualitative and Multi-Method Research. The section now has the most dues-paying members of any section of the APSA.

The current wave of MMR can be traced back to the 1994 publication of King, Keohane, and Verba’s highly influential *Designing Social Inquiry (DSI)*. The work attempted to bridge the gap between quantitative, case-based and interpretive work, downplaying the epistemological differences underpinning these different methods and highlighting the common ground they shared in relating empirical observations to more general inferences. They maintained that the goal of all social science research was to draw inferences, and they noted that “... the difference between the amount of complexity in the world and that in the thickest of descriptions is still vastly larger than the difference
between the thickest of descriptions and the most abstract quantitative or formal analysis” (1994, 43). As a result, the process of identifying, ordering and representing “significant” facts in an interpretive study or a small-N comparative study would not be fundamentally different from the way that quantitative researchers employ analytic constructs to sort out information in a large-N analysis. In effect, this suggested that the rules of descriptive and causal inference guiding quantitative analysis could be translated for qualitative research. KKV did note that the rules of scientific inference were “sometimes more clearly stated in the style of quantitative research,” (1994, 6), but they steadfastly maintained that their objective was not to privilege any one method but to devise some common-sense rules to prevent inferential and evidentiary errors regardless of whether one is doing quantitative or qualitative research.

In initiating and offering workable parameters for a dialogue among the diverse methodological camp, KKV helped to break down the walls between practitioners of quantitative analysis and those partial to any of a wide range of approaches that could be legitimately called “qualitative.” Additionally, blurring the lines between quantitative and qualitative methods helped to create a space where the methods could co-exist within the same discipline and conceivable within the same body of work. In fact they maintained from the outset that “Most research does not fit neatly into one category or the other” and that, “[t]he best often combined features of each” (1994, 5), citing the works of Lisa Anderson (1992) and Robert Putnam (1993) as noteworthy examples of such cohabitation. Thus, by downplaying the technical aspects of data analysis found in most methodology texts and illustrating how the basic challenges of avoiding bias, selecting appropriate cases, and gathering information inform both quantitative and
Since the publication of *DSI*, numerous prominent scholars have offered strong praise as well as criticism – sometimes in the same breath. The initial responses of the discipline were collected and published in an important symposium published in the American Political Science Review (APSR) in the year following the publication of *DSI*. Laitin (1995) was among those who lauded KKV’s effort to “discipline” the discipline of Political Science by bringing some common standards of rigor into the work done by qualitative scholars. Others, however, raised issues concerning the validity, practicality, and originality of KKV’s prescriptions. Caporaso (1995), for example, questioned the extent to which these prescriptions could truly bridge the epistemological divide, given that they could be meaningfully applied only to those strands of qualitative work that adopted the language of variables and the experimental logic of quantitative work. Tarrow (1995) questioned the extent to which *DSI* represented a serious inclusive dialogue as opposed to a condescending lecture by KKV aimed at qualitative colleagues who also had much to offer quantitative scholars in terms of strategies for managing non-quantifiable data. Collier (1995) and others noted that many of the problems KKV identify with qualitative work are based on poor scholarship, and that the core principles of research design emphasized in *DSI* had been long understood by qualitative researchers who simply addressed these issues in a different language. Rogowski (1995) criticized KKV’s lack of attention to the role of theoretical anomalies, causal process,
and deductive reasoning, noting that the preoccupation with inference in *DSI* would effectively lead us to ignore some of the most brilliant analyses in the field.

In the decade following the appearance of the APSR symposium on *DSI*, other scholars have continued to point out problematic aspects for what was initially presented as a quite reasonable plea for following some basic rules and standards for linking claims to evidence. Qualitative scholars working in the area studies tradition (e.g. Ames 1996) also wondered whether some of the norms brought to bear in quantitative work (such as the norm of replication) could be usefully reproduced given the distinctive challenges and aims taken on by qualitative area specialists doing fieldwork abroad without the benefit of the more reliable and plentiful data available to scholars working with established data sets. Those assessing *DSI* at the level of epistemology (e.g., Sil 2000a) noted that KKV’s attempt to “side-step” controversial issues in the philosophy of science in its quest for unified methodology (KKV 1994, 6) served to conceal a deep-seated commitment to an empiricist perspective which, while informing with some types of case-study research, is not necessarily shared by those proceeding from other variants of positivism (such as realism) or from post-positivist epistemologies (such as pragmatism or phenomenology). Thus, *DSI* is criticized for developing a uniform standard for social scientific research only insofar as that research shares KKV’s own readiness to discount ontology and to emphasize observable regularities in social life.

These rebuttals, far from diminishing from the value of *DSI*, in fact helped to advance at least one of its goals: that of developing a shared standard for research and encouraging scholars to recognize some of the common ground shared by practitioners of different methodologies. The notion of “methodological triangulation,” already in play in
such fields as sociology (Denzin 1978) and public administration (Jick 1979), was encouraged by Tarrow (1995). Triangulation involves not just the application of different methods, but the reformulation of questions and reoperationalization of hypotheses in ways that reflect the parameters and profiles of separate methods. If a substantive problem can be represented as a set of related research questions, each designed in the context of applying a distinct method, then triangulating different methods yields more data, more measures, as well as greater confidence in the findings while also making up for any limitations or obstructions that any one of the methods might face in a given context. Moreover, given that some methods may be better suited than others to explore existing theoretical approaches, methodological triangulation enables researchers to engage alternative theories and to compare their relative utility in relation to a particular research question (Olsen 2004).

In carefully articulating the strengths of non-quantitative methods, such criticism helped to put the diverse methodological perspectives on more equal footing. The disparate lines of criticism were further developed in the following years and culminated in the publication ten years later of the edited volume Rethinking Social Inquiry (Brady and Collier 2004), a collection of essays that dealt systematically with the claims of DSI and sought to more thoroughly assess the strengths and weakness of both qualitative and quantitative work. One of the most important contributions of the volume with regard to the development of MMR was its emphasis on tools rather than overarching methods. Though certain tools were conventionally associated with certain methods, it was argued, they need not be exclusive to them. And as the subtitle “Diverse Tools, Shared Standards” suggested, the diversity of methodological tools need not be an obstacle to
finding common grounds for social inquiry. Emphasis on tools served to disaggregated methods into their component part such that a case study does not need to be wedded to historical narrative any more than a regression need to be restricted to the goal of drawing statistical inference. In this way, the work paved the way for the use of different research tools outside their conventional methodological framework.

Aiding the broad methodological shift was the publication in the following year of Ian Shapiro’s *The Flight from Reality*, a collection of essays advocating a scientific realist approach to social inquiry which privileged questions over methods. An emphasis on problem-driven research which began with Green and Shapiro’s critique of formal theory was extended to the whole of the discipline. According to Shapiro, political science has appropriated only the most technical aspects of scientific inquiry without embracing the underlying probative nature of the enterprise. “Scientists” he maintained “begin neither with method nor theory but with an aspect of reality they are trying to understand.” (40) The nature of problem-driven research he argued necessitated methodological opportunism. “The ways in which scientists show how outcomes are produced by causal mechanisms depend on the nature of the question being asked. Sometimes, quantitative, cross-sectional analysis will provide strong material for good abductive inferences. In other cases, qualitative or historical analysis will be more appropriate.” (40-1). Scientific realism according to Shapiro is agnostic with regards to particular methods, driven as it is by concrete problems and questions that do not pre-supposed established methodological practices.

Other manifestations of the shift toward MMR could be found within the discipline, as evidenced by a symposium on the state of qualitative research in the different
subfields, which emphasized the increasing reliance on mixed method approached (Bennett and Elman 2007). Later that same year scholars confirmed their commitment to this shift with the reformulation of the Qualitative Methods section of the APSA as the Qualitative and Mixed Method Research section. The ascendance of MMR was also accompanied by several efforts to formalize specific MMR methodologies. This can be seen in Laitin’s “tripartite” approach which seeks to combine formal, qualitative, and quantitative methods, as well as Lieberman’s “nested analysis” which integrates quantitative and qualitative techniques.

These developments have set the stage for an intellectual environment where, not only are different methodological approaches valued, but the mixing of methods is often seen as a virtue. What started as an attempt to discipline the discipline has yielded an entirely new field of inquiry, with many welcome changes but also significant dangers for social science research. In the following section, we discussed some of the most common combination of methods found in political science literature today, highlighting problems particular to each as well as common pitfalls of MMR.

III. MMR in Practice: Mixing Formal, Quantitative and Qualitative Methods

This section provides a necessarily brief and unrepresentative sampling of works that might be classified generally as attempts at applying mixed-method research strategies. The works considered are not evaluated in terms of their general intellectual merits. They are simply brought in for the purpose of revealing the different ways in
which “formal,” “quantitative,” and “qualitative” methods have been triangulated in a variety of studies. For the sake of simplicity, we break down the discussion below into three categories corresponding to three dyadic combinations: formal-quantitative, formal-qualitative, and quantitative-qualitative. We are certainly aware that all three of these labels are broad and encompass diverse approaches; and we are especially aware that the category of “qualitative” research is particularly problematic since, more than the other two categories, it is used to capture a very wide range of methods proceeding from quite disparate epistemological foundations. Nevertheless, for the purpose of setting the stage for the cautionary notes we are gravitating towards, the discussion below should suffice.

*Formal-Qualitative Approaches*

Attempts at marrying formal and qualitative research have received a great deal of attention in recent years. Perhaps the most prominent example is the work on *Analytic Narratives*, a collection of essays which aimed to develop a synthesis between game theoretical modeling and case-based narratives. (Bates, Greif, Levi, Rosenthal, and Weingast 1998) The approach was problem-driven in that what animated the research was a common substantive focus on the development and transformation of institutions. The work was also self-conscious in its attempt to combine methods in such a way that one would compensate for the deficiencies of the other. Each essay in the volume addressed issues that the authors argued could not be adequately addressed exclusively
with one method or the other, but required the creative juxtaposition of detailed narration with parsimonious game-theoretical analysis.

The analytical narratives project is an ambitious one and occupies an important place in the ongoing dialogue among the disciplines; however as a work of MMR it displays several problems. The first is the dichotomy drawn from the beginning between the formal modeling which is to provide the analytical insights, and the qualitative work which is to serve as the basis of narration (p. 10). Because theory in this context is often conceived narrowly as formal theory, there is little appreciation of the theoretical insights that the narrative itself has to offer. While the authors in the volume show a genuine commitment to take context seriously, they are ultimately unable to integrate it fully as method, rather than simply as a tool of data collection. For example, in the essay by Greif, which seeks to explain the self-reinforcing character of medieval Genoese political systems, the narrative provides the relevant historical background, but does not on its own explain anything. This is because much of the explanation, according to Greif, requires accounting for what did not occur. (p. 26) And though there is a long tradition of qualitative work dealing with counter-narratives, Greif chooses to rely exclusively on formal modeling for this purpose. The stark dichotomy between theory and narrative can be seen throughout many of the essays in the volume. Consequently the game theoretic models do the heavy lifting and the narrative offers no real feedback in terms of the theoretical argument. Thus, to the extent that some bridge-building occurs here, it is at best a “cantilevered bridge” (Sil 2000b).

Moreover, the qualitative dimension of the work often gives selective treatment of the history or context, featuring claims or interpretations that many qualitative
researchers with expertise in the area would consider contestable. As Lustick (1996) notes, working with historical materials requires careful attention to the complexity of historiography and judgment in the handling of multiple narratives. This is not possible when a case-study is conducted in a hurried manner for the sole purpose of illustrating a causal logic derived from a formal model.

To be sure, the narratives greatly enrich the formal modeling. But while the use of qualitative narratives may demonstrate the potential utility of a general model, the actual construction of a causal story is driven by the model, not the gathering of qualitative data. Analytical narratives do represent a case of MMR, but the two methods used serve quite different purposes; and the more important purpose, related to the elaboration of a causal explanation, is achieved primarily through the formal model.

*Formal-Quantitative Approaches*

While those partial to a probabilistic view of the world have generally stayed away from formal theory, those engaged in formal modeling have long been interested in deploying statistical analysis to test hypotheses axiomatically derived from their models. This is evident, for example, in the studies done by Bruce Bueno de Mesquita and his colleagues on such problems as the causes of war (Bueno de Mesquita and Lalman 1992) and the political durability of authoritarian rulers with poor performance records (Bueno de Mesquita, Smith, Siverson and Morrow 2003). In the first study, a game theoretic framework is deployed for the purpose of constructing a formal theory of international
interactions which is used to demonstrate how foreign policy is driven more by domestic institutions than by power relations in the international system; and statistical analysis and historical cases are subsequently introduced to demonstrate the strength of the theory vis-à-vis alternative accounts. In the latter study, a theory of political incentives and institutional choice is developed on the basis of rational-choice assumptions, and empirical assessments are provided primarily through statistical analysis (although historical examples are also thrown in to buttress the argument).

Both of these studies represent classic examples where formal models are developed axiomatically and hypotheses are deduced before being subjected to rigorous empirical tests primarily on the basis of quantitative methods. They also appear to reflect practical applications of Goldthorpe’s (1996) call for an explicit “sociological alliance” between the quantitative analysis of large-scale data sets and the elaboration of hypotheses deduced from rational actor theory. What is interesting to note here is that the juxtaposition of methods is aimed at different phases of research. The theory-building component of the approach, which could (in principle, at least) stand on its own, is largely based on what Shapiro and Wendt (2005, 24-8) would call a logicist epistemology, one that draws inspiration from Carl Hempel’s formulation (1966) of the deductive-nomological model, with the emphasis placed on the deduction of testable causal propositions from axiomatic covering laws. In a logicist view of social science, temporal and intellectual priority is explicitly assigned to the construction of concepts and the articulation of assumptions that inform internally consistent theoretical axioms from which predictive and explanatory claims are logically derived. The problem is that the methods subsequently used to test the emergent theory are designed on the basis of an
empiricist view of the world as is typical of most probabilistic approaches using large-N
data sets. The only way these two approaches can be juxtaposed is if the theory-building
and theory-testing phases of the research process are regarded as distinct entities. Thus,
while multiple methods are indeed used, they are used for different purposes, with one
method clearly assigned intellectual priority and another relegated to the role of providing
external validation (but not refutation, since the primary basis for the quality of the theory
is held to be its internal logical consistency).

Quantitative-Qualitative Approaches

Attempts at marrying statistical & case based analysis are perhaps the most
canonical form of MMR today. One of the more influential examples of this is
Lieberman’s “Nested Analysis” in which small-N case study analysis (SNA) is
embedded within large-N statistical analysis (LNA). Such an approach may indeed yield
a rigorous research design, but it is questionable to what extent this represents a
genuinely mixed method approach. First, in Lieberman’s framework, the research must
begin with quantitative analysis. In fact, he maintains that “a prerequisite for carrying out
a nested analysis is availability of a quantitative dataset, with a sufficient number of
observations for statistical analysis” (p. 438). Thus, if a problem-driven orientation is one
of the goals of MMR, nested analysis will necessarily depart from that, as the procedure
is only applicable to questions for which large datasets exist. Second, in conducting a
nested analysis, the case studies serve primarily to confirm or disconfirm the findings of
the statistical analysis. Though Lieberman does provide some feedback mechanism in which the SNA would inform the development LNA, ultimately the findings of the study are what can be established through statistical analysis, relegating the case studies to a subsidiary position in terms of the analytical contributions. In the first instance and in the last, it is the quantitative dimension that does the explaining in a nested analysis approach.

Another example of the challenges of qualitative-quantitative combinations can be found in Yoshiko Herrera’s work on *Imagined Economies*. Herrera uses regression analysis to test standard economic explanations and to demonstrate that the correlations are not as robust as the existing arguments would suggest. But her own explanation relies almost entirely on content analysis of regional understandings of two areas of roughly similar objective economic conditions (Sarmara and Sverdlosk). While Herrera’s study demonstrates some creativity, it is not clear that there is a methodologically sound reason for choosing her argument over the existing explanations since the same test is not applied. One method is used to challenge prevailing economic explanations, but the alternative explanation developed is not then evaluated in the same way. At the same time, if content analysis were employed to simultaneously test existing economic arguments and develop a more complex theoretical framework that subsumes those arguments, then the resulting claims would have been more compelling. Thus, what is gained by using multiple methods appears to be marginal compared to what is lost by not applying a single method more expansively.
IV. Cautionary Notes: What is Gained and What is Lost

The current shift toward MMR research is promising in many respects. It represents arguably the most rigorous sustained engagement between the methods to date. It also has created space to explore work that is problem-driven, but at the same time methodologically self-conscious, two dimensions that had long been considered trade-offs of different approaches. Works attempting to employ MMR have shown the potential advantages of such an orientation. Certain combinations of methods, it has been noted, can increase the validity of findings (Tarrow, 1995). MMR can also improve the theoretical insights of a project by allowing researchers to demonstrate both causal effects and causal mechanisms (George and Bennett, 2005). An orientation toward MMR can also enhance the scope of scholarship, by allowing scholars trained in multiple methods to approach research questions from multiple angles (Cappocia, 2007).

Certain familiar issues however continue to hinder the full-scale realization of MMR. Despite its potential advantages, scholars have already begun sounding the alarm. So far, most critiques have dealt with the investment of time and energy in doing all methods "right." But there still seems to be some agreement that, all things being equal, using more than one method will produce better results. (Cappocia, 2007) The practicalities of MMR are no doubt an important consideration, but this is not in our opinion the most fundamental problem. In the practice of MMR we identify two sets of problematic issues that scholars ought to consider before committing to a research project predicated on the application of more than one method.
One of the problems we perceive is that MMR cannot avoid the trap of ultimately relying upon some sort of methodological hierarchy. Lieberman’s notion of “nesting” offers another image of the relationship, but the basic fact remains that in most instances, it is one method doing the heavy analytical lifting with the other providing raw material. While it is often the case that such material significantly informs the research project, it is not the methodological source of the substantive contribution. In the case of analytical narratives, the main theoretical insights are drawn from the formal modeling. In the case of Lieberman’s nested games, it is the quantitative analysis that is ultimately the basis of scientific inference. And in Herrera’s work, the regression analysis serves to disconfirm alternative explanation but does nothing to confirm her own. For that, she relies primarily on more qualitative methods. In all of these cases, the value-added from the work comes from a single method.

The tendency toward hierarchy we argue will be an inevitable feature of MMR. This is due to the fact that different methods reflect competing, sometimes incommensurable, foundational perspectives. Some methods are more appropriate for identifying lawlike regularities, while others are more appropriate for outlining specific processes or capturing universal logics. Each of these approaches may seek to know the “truth” in some way, but the level of generality and the fallibility of the truth claim can vary dramatically depending on the assumptions in play. Thus, simply deploying methods together to construct a singular model or narrative is not necessarily a straightforward proposition since care will have to be taken to manage and adjust the philosophical assumptions upon which each of the methods proceeds. The same objections that have been raised in the past to pluralism and cumulation can be
reconstituted in the case of multi-method research. (Johnson 2002; Harvey and Cobb 2003; Lichback 2003, 7)

What often results under the heading of MMR is that tools conventionally associated with one method are appropriated by a project of a different methodological orientation. While a case study can be embedded in a broader statistical study, a regression can be used to tell part of a story, and interviews can be used to inform a model, this does not in our assessment constitute a genuine mixing of methods, where the methods are on equal footing. Moreover, it does little to bridge the epistemological divide between the methods or develop an independent MMR approach to research. In this context, the case study, the regression, and the interview serve as tools of data collection. These tools may improve the quality of the scholarship (no small accomplishment in itself), but to the extent that the main engine of explanation relies on a single method, this cannot be seen a genuine move to a methodological common ground.

It should also be noted that the trend towards MMR is not necessarily consistent with another trend that has become increasingly more visible in recent years: the attention being devoted to the definition and analysis of causal mechanisms. For KKV, the study of causal effects is logically prior to and more reliable than analysis of unobservable mechanisms, and the value of mechanisms is limited to their ability to generate new observations that may influence the level of confidence in causal inferences (KKV 1994, 85-87, 225-227). McKeown criticizes KKV for conflating scientific inference with quantitative inference, and notes that KKV’s simultaneous commitments to a covering law approach and to causal analysis featuring a role for mechanisms “creates a strong tension that is never confronted, let alone resolved” (McKeown 2004,
George and Bennett (2005), who adopt a scientific realist approach, emphasize that causal mechanisms operate at the ontological level and can be neither conflated with, nor subsumed under, hypothesized causal effects. Waldner goes further in accusing KKV of reducing mechanisms to mere “servants of inferences,” only relevant insofar as the search for mechanisms inspires scholars to uncover more information with which to generate support for hypothesized causal effects (Waldner 2007, 154). These criticisms point to an important limitation of Brady and Collier’s (2004) efforts to elaborate on the possibilities for shared standards for “diverse tools”: those tools that are aimed at uncovering mechanisms ask questions and design research projects in ways that are fundamentally different from tools employed to identify empirical associations between observable social phenomena. A method such as process-tracing, which aims to identify the sequence of links leading from a specific set of initial conditions to a particular outcome, can certainly be used in conjunction with a small-N or large-N analysis of variables. But, the process-tracing component of the analysis can independently generate outputs that have little or nothing to do with the outputs of a small-N or large-N analysis of observable data.

Finally, it should be noted that this critique is in no way meant to stifle intellectual creativity or hinder a pluralistic view towards methods. What we wish to advocate is a constrained pluralism which takes seriously the comparative strengths and weaknesses of a given method but also recognizes the fundamental incommensurability of concepts and analytic principles conventionally associated with particular methodological approaches. Moreover, in the absence of greater attention to integrating components of diverse theoretical frameworks, the attempt to triangulate methodological
approaches ultimately risks reproducing the very method-driven scholarship that it was meant to rectify. Rather than mechanically inserting different methodological exercises into a single research product, it may be more worthwhile to focus attention on the ways in which concepts and mechanisms drawn from different theoretical traditions can be translated, compared, and partially integrated, regardless of whether the number of methods used is one, two, or more.

This is the case, for example, with the “analytic eclecticism” recently articulated by Katzenstein and Sil (2008; see also Sil and Katzenstein, forthcoming). Analytic eclecticism, in contrast to MMR, is focused on the construction of original problem-specific frameworks that capture the complexity of socially important substantive issues, and it primarily aims to reveal connections between portable and context-specific mechanisms that are normally analyzed in separate theoretical languages reflecting the foundational assumptions of separate research traditions. In principle, such a project can be advanced (though not culminated) by the flexible application of a single method -- be it game theory, regression analysis, case studies, or ethnography – so long as the problem and the emergent theory or narrative encompass components drawn from research puzzles and substantive analyses developed in separate research traditions. The construction of eclectic analytic frameworks certainly benefits from movement in the direction of methodological pluralism, but the concrete triangulation or mixing of methods should not be confused with, or subsume, the more complicated task of identifying theoretical and empirical connections between puzzles, findings, and causal stories developed in alternative research traditions grounded in diverse epistemological perspectives.
V. Conclusion

On the whole, MMR has been a blessing for the discipline, especially in promoting a diffuse spirit of methodological pluralism and creating awareness of the comparative strengths and trade-offs of particular methods. The initial reactions to DSI had revealed entrenched battle lines, with discrete communities of scholars convinced that their methodological skills were not only useful for the social sciences but more useful than skills employed by others. This also implied that certain research products would be inherently more valued by those partial to a given method, while others would be at best tolerated as secondary contributions because they happen to be designed for the application of a different method. This state of affairs, thankfully, has now passed.

However, the value of MMR is best understood in functionalist terms. MMR has played a crucial role in helping to correct for unnecessarily rigid and competitive postures driven by commitments to particular methodological traditions. With new generations of scholars now taking seriously the notion of mixing or triangulating multiple methods, there are now more and deeper channels of communication among those interested in a given problem, and there are more incentives to consider the value-added of theories, narratives and data devised by those partial to alternative methodological traditions. The move towards MMR has also made individual scholars more appreciative, or at least more patient, when they encounter research products reflecting the application of a method they are not themselves adept at using.

But, the practical applications of MMR also reveal why it should not be held up as
the new “one best way” or the new “gold standard” for good scholarship. The differences over epistemology and the related issues of commensurability across methods and types of data remain just as significant as they have always been. MMR has not solved this fundamental problem in social scientific research. Those works that do employ two methods or more appear to employ those methods that already have in common certain basic ontological and epistemological assumptions. This is perhaps why the most common expressions of MMR involve the juxtaposition of statistical analysis, usually to establish some variation in outcome, and case studies to develop the causal stories through which the hypothesized causal factors produce this variation. Other attempts to combine methods traditionally founded on competing epistemologies, however, end up adopting the epistemological perspective of one of these methods and thus relying primarily on that method in order to establish the most crucial features of analysis.

Most importantly, the quest for MMR should not interfere with the individual scholar’s enjoyment of research. A scholar who thoroughly enjoys doing ethnographic research or developing models based on the logic of games can make useful contributions without necessarily incorporating other methods into a single research project. The unit of analysis for assessing scholarly research need not be the individual, especially if we view the relationships of different kinds of research products as a sophisticated division of labor. There is no reason why a single research product must incorporate the applications and findings associated with different methods. Collaborations among individuals or simply the mutual recognition of the contributions of different scholars using different methods can also constitute pathways to methodological pluralism at the
level of the discipline writ large. If MMR is treated as the new "one best way," we risk forcing individuals who excel at (and enjoy using) a given approach to invest time, energy and emotion in shoe-horning a separate methodological exercise that someone else could have undertaken with greater ease and enjoyment. The important contribution of MMR is to keep the lines of communication among scholars practicing different methods, not create intellectually unjustified pressures upon individual scholars who fear that their careers will depend upon demonstrating their competence in applying different methods.

In sum, mixed-method research can help us cultivate a deeper awareness of the trade-offs of different methodological approaches in relation to different kinds of intellectual problems. But, it cannot overcome the fundamental problems of incommensurability unless it is designed in conjunction with simultaneous efforts to construct problem-specific analytic frameworks. More importantly, to the extent that different methods can be triangulated in relation to a given problem, the actual application of a given method need not be done at the level of the individual scholar or the individual research product. It is incumbent upon the discipline as a whole to recognize the distinctive intellectual payoffs of particular methods. In this regard, the rise of MMR has proven to be a tremendous asset. What we need to guard against now is its use as basis for a new hierarchy in which those who acquire and deploy different methodological skills are presumed to be more valuable members of the discipline than those who master a single method and enjoy using it time and again to develop original findings.
References


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