Eclecticism proposes that epistemic differences need not matter. It leapfrogs over them to a problem-driven pragmatism—use what works, in whatever combination it works.

Pluralism embraces diversity as a virtue, and seeks to find the limits of association and commensurability between several equally valid epistemes.

The section needs proponents of all three strategies taking part in the discussion. In particular, pluralists are needed to balance the monists' one-sized-fits-all, and the eclectics' rejection of foundations.

Without the check of pluralism, pursuing monism will be an unpleasant and ultimately self-defeating process. Whichever of the approaches emerges victorious from the struggle, the section will be impoverished by the absence of those it managed to exclude. As for eclecticism, it may be liberating for individuals to declare "badges, we don't need no stinking badges." But a strategy of decoupling methodology from epistemology simply delays the inevitable reckoning when lurking incommensurability surfaces.

While I believe that pluralism is a necessary part of the conversation, I acknowledge that it is also a hard choice. It requires learning about other research traditions, and being tolerant of differences. It mandates the sharing of scarce resources. It also means becoming more careful about overly broad claims, whether made in the positive ("this approach is the future of political science") or the negative ("scholars studying a few or single cases are historians not political scientists"). Innocuous in the context of a single episteme, these types of statements are staggeringly insensitive to other research traditions, and antithetical to a section that strives to be inclusive.

As the incoming President, I want to make the argument that the section's breadth is an opportunity to be embraced. It is not just a problem to be overcome by a retreat to homogeneity, or to be ignored by proceeding as if our differences do not matter.

Symposium: Cautionary Perspectives on Multi-Method Research

Is Multi-Method Research Really "Better"?

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Recent scholarship in political science attests to the rapid proliferation of approaches engaged in multi-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.[1] The general notion that different types of methods can be employed to advance or test a particular theory is not in itself new. Multi-method approaches have long been a feature of social science research, taken up usually out of necessity (e.g. Jick 1979). Where data conducive to one method was not available, scholars would turn to another in order to fill the gap.

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. The method wars in political science have given way to an apparent consensus on the primacy of MMR as a way to achieve better results and overcome the limitations of particular methods.

This trend is most apparent in comparative politics where some of the field's leading figures have called for MMR as a means of overcoming the limitations of single-method research (SMR). According to George and Bennett (2005), the differences between different methodological approaches give them "complementary comparative advantages." Many have gone beyond this, however, to argue that MMR in fact represents a better research strategy, producing greater inference and more reliable findings than SMR (Brady and Collier 2004; Lieberman 2005; Laitin 2002; George and Bennett 2005).

While the increased appreciation of diverse methodological approaches is a welcome trend, there is much to be alarmed about in the recent rush to assert the primacy of MMR. What began as a movement to employ "all means necessary" in the service of problem-driven research is quickly turning into a new dogma that researchers *must, or ideally should,* incorporate "all means available" to validate their work. Practitioners and advocates of multi-method approaches explicitly argue that, all things being equal, multi-method research is better than single-method research. The goal of this essay is to assess this claim. While "better" can be understood in many different ways, our interest is particularly in the epistemological viability of the claim. Do we in fact learn more by combining different methods? Does MMR actually increase the number or strength of the inferences we can derive in the course of carrying out an individual research project? Can multi-method research actually increase the validity of findings? We are not concerned with the limitations or particular shortcomings evident in works using MMR. We are more interested in working towards an understanding of whether in principle MMR should be privileged over SMR at all times. In other words, is good

MMR always better than good SMR?

Our argument is straightforward: The claim that MMR is inherently better than SMR is built on the faulty premise that one method can offer external validity for the findings of another. Different methods can at best corroborate each other's findings, but this does not yield a more compelling inference. We do not know more or know better as a result of triangulating different methods because different methods rest upon incommensurable epistemological foundations that even the most heroic attempts at translation cannot overcome. Though combining methods may and often does produce good scholarship, we find that MMR holds the same epistemological status as separate projects addressing the same question, and that SMR is no less likely to produce good scholarship. The "goodness" of scholarship ultimately depends on the care and originality with which research is designed and executed, not on the number of methods that are deployed. Thus, although MMR is certainly valuable for social science research and should be welcomed as a part of a broad repertoire of methods available to scholars, there is no epistemologically sound reason to elevate it above others. Below, we make this case by examining some common forms of MMR and then considering some of the hidden costs associated with prodding individual scholars to adopt a strategy of MMR in the context of a single project.

Statistical Analysis and Varieties of Case-Study Approaches

Despite the great expectations of social scientists, practical applications of MMR have revealed the limitations of trying to use different methods to introduce measures of external validity. Because methods are premised on different epistemological commitments, they tend to employ different types of variables or mechanisms and privilege different dimensions of social reality. They also tend to focus on concepts that can be operationalized within certain boundary conditions and thus may not easily translate to other modes of inquiry premised on different sets of theoretical priors. For these reasons combining methods often results in findings that are incommensurable, frustrating efforts to offer external validity.

Take for example one of the most popular forms of MMR in use today: the combination of statistical and case study analysis. Proponents of such an approach argue that it offers greater analytical leverage, as one method compensates for the limitations of the other. Statistical analysis is limited in its ability to identify a causal mechanism as it tends to focus on causal effects.[2] Case study analysis, on the other hand, is well suited to identifying a causal mechanism, but is has limited generalizability. The combination of the two, it is argued, gives us the best of both worlds. Statistical analysis can be used to identify a general distribution of causal effects whereas the case studies may be employed for the purpose of identifying causal mechanisms and revealing separate links in a causal chain (Lieberman 2005; George and Bennett 2005).

First, it should also be noted that efforts to combine statistical and case-study analysis are only limited to certain kinds of case studies—those designed on the basis of an empiricist epistemology. Empiricism, which emphasizes the temporal priority of positive empirical observations and thus privileges inductive logics, provides a foundation for the probabilistic worldview of statistical analysis and is also consistent with the use of case studies for testing hypotheses or developing hypotheses that can be subject to quantitative tests. But, case studies have a variety of purposes. Some are constructed so as to support a more deductive orientation to theory building, as in the case of "analytic narratives" (Levi 2004) used in conjunction with game theoretic models. Others are designed on the basis of more hermeneutic or phenomenological approaches that stress the interpretation of meanings held by actors within distinctive contexts (Yanow 2006).

But, even with a common empiricist orientation, the claim that combining the two methods offers greater analytical leverage is difficult to sustain on epistemological grounds. This is because in moving from one mode of inquiry to another the basic conceptual categories will necessarily shift. McKeown (2004: 140–146) makes the point that the kind of inference that is privileged in the quantitative worldview cannot be conflated with the broader process of scientific inference which, in the case of a case study, focuses attention on a fundamentally different task: explaining how a set of initial conditions enable particular mechanisms to have particular effects in one or more contexts. Thus, in moving from large-N statistical analysis to case study analysis, the case study will by its very nature introduce variables not present in the statistical analysis (for example, variables that are not quantifiable but whose effects can be observed within a given context). It is precisely the depth of inquiry that enables case study analysis to identify causal mechanisms and reveal their effects in relation to a given outcome. However, it is also this feature that makes the two modes of inquiry incommensurable. They are effectively examining two different sets of variables. Thus in this scenario, the case study cannot be said to either confirm or falsify the finding of the statistical analysis.[3] Though it may offer a plausible story, at an epistemological level, it offers no corrective for the built-in limitations of the statistical analysis. In such combinations, the statistical analysis and case study analysis effectively represent independent (thought perhaps complementary) intellectual exercises. The findings of one cannot be said to validate the other.

Following the reverse sequence does not make this problem any less intractable. For example, if one begins with case study analysis to establish a causal mechanism and then tests the causal relationship with large-N statistical analysis, the latter cannot serve as a source of external validity for the former. As George and Bennett (2005: 138) note, causal mechanisms operate at the ontological level and can be neither conflated with, nor subsumed under, hypothesized causal effects. Statistical analysis can validate the relationship between hypothesized causal effects and generalize it across cases, but it cannot show that the causal mechanism found in the original case study analysis operates in the same manner and produces the same effect across different spatial and temporal contexts. Without case study analysis of each case, one cannot verify that the same causal mechanism is at work in all, let alone approximate the general size of its effect upon a given dependent

variable. Both can provide independent analysis of related research questions, but one does not validate the other.

Formal Modeling and Empirical Analysis

Another popular mode of MMR features the combination of formal modeling with some form of empirical analysis. Proponents of such approaches also make the claim that the combination of the two methods produces greater insight, as one can be used to compensate for the limitations of the other. The formal model is thought to provide analytical rigor while the empirical analysis grounds the investigation in some social context. With this sort of combination the different methods tend to have somewhat more defined roles: the formal model is used for deductive theory-building and the empirical analysis is used for the purposes of illustration or theory-testing. However, the two sets of methodological operations involve quite different foundations. Even where the empirical analysis and formal model are both conceived of as broadly positivist enterprises, the former follows from empiricism and the latter from logicism (Shapiro and Wendt 2005). Using either case studies or statistical analysis in conjunction with the construction of a formal model would require completely ignoring the foundational principles on which the latter is built. The empirical analysis is also constructed specifically to test deductive models rather than to generate alternative causal stories since the causal structure and explanatory logic of the model depends more on axiomatic principles and internal consistency rather than on inferences from observed regularities.

In the case of "analytic narratives" (Bates et al. 1998), for example, the case-specific narratives certainly provide context, but the causal story does not emerge from this context, and the interpretations of the contexts are not evaluated against the strength of other interpretations of the same contexts. Moreover, since "theory means formal theory" (Bates et al. 1998: 3), there is the question of how the deductivist logic of formal theorizing can be meaningfully combined with the interpretive logic informing the construction of a context-bound narrative. While it is indeed admirable that scholars are able to demonstrate their use of both extensive-form game theory and case-specific research (sometimes involving fieldwork or archival research), the core causal logic of the explanation is given by the assumptions and logics built into a particular game-theoretic model rather than a balanced process of moving between theory and data (Sil 2000: 375). To be sure, empirical analysis can reveal flaws in an existing model and potentially inform a new model, but this can be done in differently designed SMR projects motivated by different objectives as part of our collective efforts to further knowledge. There is no inherent value to insisting that a single study incorporate both a formal model, premised on a logical positivist worldview and partial to deductive theorizing, and a case-specific narrative developed through interpretive methods designed to generate a deeper understanding of a given context rather than a more general explanation. Thus, while both game theory and case study serve important roles, those roles are distinct, each dictated by the nature of the methods combined, and each producing distinct, fundamentally incommensurable research products.

In the case of the marriage between statistical and formal approaches (e.g. Goldthorpe 1997), too, the use of one method to validate the findings of another is highly problematic. Although statistical analysis and formal modeling both proceed from a broadly positivist foundation, as noted above, the inductively oriented empiricism undergirding the former is fundamentally at odds with the deductively oriented logicism of the latter. The empirical analysis may corroborate the findings of the formal model by capturing the expected distribution of outcomes, but it cannot capture the effects of cognitive mechanisms that are predicated on the assumption of instrumental rationality.

Statistical analysis can determine a general pattern corroborating the findings of a formal model, and case study analysis may offer an illustration of a particular dynamic, but neither mode of inductive inquiry can offer external validation of a model deduced from a priori axioms. Again the problem of epistemological incommensurability presents an obstacle in the quest for external validation. Though considering empirical evidence maybe a useful heuristic for formal modeling, the juxtaposition of the two does not strengthen the inference we can draw from the former or the confidence we can have in a given model.

The Hidden Costs of MMR

The argument up to this point is *not* that there is no value in examining a substantive issue through the application of different methods. It is that, in the common forms of MMR considered above, what one can learn by juxtaposing two or more methods within a single research product is not fundamentally different from what we would learn from separate studies using different kinds of SMR to address the same question. This is not to say that the findings of a single research product featuring MMR are not useful. But, epistemologically speaking, there is no intrinsic gain from insisting on always triangulating different types of methods within a single approach rather than encouraging scholars interested in different methods to use those methods to explore the same substantive question as part of a larger collective effort to generate insights into the question. The combination of methods in a single study does not resolve the problem of epistemological incommensurability, and thus cannot eliminate the tradeoffs built into each of the methods employed at various stages of a multi-method project. Thus it cannot be said that we know more or know better when multiple methods are deployed.

This critique may ring hollow to some. Even if MMR is simply a juxtaposition of different intellectual projects within the same work, it might be argued, is this not in itself an advance over SMR? More significantly, is it not more efficient to have a single scholar generate findings using different methods when investigating the same sorts of substantive problem? Our conservative answer to both questions is: perhaps in some rare instances, but generally not.

Here, the main argument concerns the ways in which research is organized in the discipline writ large and the ways in

which individual scholars conceive of their roles and contributions within the discipline. One must consider the amount of time and energy that is involved in MMR in terms of the methodological training, the fluidity in applying each of the methods, and the total time spent conducting research. Here, three possibilities exist. First, a scholar may end up taking the same amount of time to produce a single product using MMR as it would have taken to apply different methods in different research products. Second, time pressures related to grant deadlines, promotions, or other professional considerations may force researchers to spread a more limited amount of effort over different pieces of the research product. This may ultimately hurt the quality of the scholarship, producing thin case studies, shoddy datasets, and unsophisticated models hastily put together to round out a multi-method project. Third, it may actually end up taking *more* time as a single scholar shifts gears from one phase of a project featuring one method to a different phase using another method, especially if the scholar requires more "retraining" or "retooling" to effectively apply different methods. In all three scenarios, either some component of the research product will suffer, or a heavy burden will be placed on individual scholars.

Besides the costs to the individual researcher, there are considerable costs to the discipline as well. As researchers become more and more diversified in their methodological skills, they will likely become less and less specialized, spending less time on the approach they are most skilled at or most passionate about. This will have the inevitable effect of diluting the pool of expertise in the field and decreasing our collective efficiency as each of us feels compelled to maintain proficiency in the application of quite different types of methodological tools. Where there is a pool of labor available, it is not at all clear what is gained by making all of the members of that pool make the same kind of investment in gaining the same array of skills. If anything, the gains to efficiency from more specialized training and from iterated applications of the same method are lost. While specialization can certainly be taken too far, there is no reason to think that the entire discipline will gain by having all its members trained to do multi-method research.

Finally, there is a danger that the move towards MMR will result in decreased scrutiny of the core assumptions and epistemic commitments underlying the methods we choose to employ. Because MMR holds out the hope that by the very act of combining methods one can somehow overcome their individual limitations, it also holds the danger that scholars will feel justifiably absolved from questioning the foundations of the multiple methods they employ. This is perhaps the greatest danger of MMR: that we as a community of scholars will lose our critical eye towards methods and, in the process, lose our awareness of the fundamental challenges that have accompanied social scientific inquiry from its very inception.

Conclusion

The notion that MMR is always better than SMR assumes that if MMR is done well, one method can be used to validate the other, and a single project can generate more robust results or more compelling findings. The reality of social science research, however, is that all methods have limitations. This is no less true when they are deployed simultaneously in order to investigate a certain research problem. The problem of epistemological incommensurability that has long plagued exchanges across different research traditions is not being resolved by MMR; it is simply being transferred from the level of the discipline or subfield to the level of the individual scholar and research project. In this context, combining methods may produce complementary results, which may be valuable from a pragmatic point of view in generating support for a particular proposition. However, from an epistemological point of view, such combinations have the same structure and value as separate studies addressing the same research question. Putting the burden of producing these separate studies on a single scholar may not produce any gains to efficiency and may, in fact, come with some significant costs both for the scholar and for the discipline as a whole. In light of these concerns, MMR should be thought of not as the new gold-standard in research but as part of a diverse repertoire of methodological approaches that may be useful to some scholars depending on the nature of the research question—and on the preferred skill-sets and intellectual passions of the individual scholar.

Notes

[1] This essay is part of a longer article that is currently under preparation. It also builds on Amel Ahmed and Rudra Sil, "The Logic(s) of Inquiry: Reconsidering Multi-Method Approaches." Working Paper No. 16 of the Committee on Concepts and Methods, International Political Science Association (November 2008).

[2] King, Keohane, and Verba (1994, 85–87, 225–227) view causal effects as logically prior to and more reliable than unobservable mechanisms. For KKV, the value of mechanisms is limited to their ability to generate new observations that may influence the level of confidence in causal inferences. David Waldner (2007: 154) interprets this position as flawed in that it fails to recognize the epistemological function of mechanisms, reducing them to mere "servants of inferences."

[3] Moreover, as explained by Rohlfing (2008) in the cases of "nested analysis," the introduction of case studies introduces bias potentially exaggerating findings.

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The Challenge of Conceptual Stretching in Multi-Method Research

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Multi-method research (MMR) has gained enthusiastic support among political scientists in recent years. Much of the impetus for MMR has been based on the seemingly intuitive logic of convergent triangulation: two tests are better than one, since a hypothesis that had survived a series of tests with different methods would be regarded as more valid than a hypothesis tested only a single method. In their seminal *Designing Social Inquiry*, King, Keohane, and Verba (1994) argue that combining qualitative and quantitative methods is useful because it increases the amount of data used to test a specific theory or hypothesis. While critical of specific prescriptions in KKV, Brady and Collier (2004) and Gerring (2007, 2009) reiterate the mantra of epistemological monism, shared standards, and logical consistency between qualitative and quantitative methods.

This paper, though, warns that what Sartori (1970) calls the "stretching" or "straining" of concepts between qualitative and quantitative domains has potentially damning implications on MMR. Simply because qualitative and quantitative findings point in the same direction—statistical significance and coefficient signs match the outcome of a case study—does not make them any more likely to be true, since the concepts applied in one methodological component are not equivalent to those applied in the other. It is impossible for qualitative and quantitative methods *to say the same thing* because they are talking *about different things*. An alternative schema, though, is possible based on seeing the component of an MMR design as representing two distinct cultures of inquiry that are complementary, rather than corroborating.

Conceptual Stretching and Causal Analysis

The topic of conceptual stretching was first broached as a warning against the proliferation of quantitative, statistical methods and the incessant drive to substitute quantifiable variables in place of qualitative categories. Conceptual stretching occurs, according to Sartori, when "*denotation is extended by obfuscating the connotation*" (Sartori 1970: 1041). The term itself is used in the context of the ladder of abstraction, in which concepts are mapped along two inversely-related dimensions: the *intension* (connotation), the systematic and explicit definition of the characteristics of the concepts, and the *extension* (denotation), the range of cases which can be categorized as meeting the conceptual definition. Increasing a concept's extension by incorporating more cases under its rubric leads to stripping away some of its necessary intension, the specificity of characteristic involved in the concept. While ascending or descending the ladder is critical for theory building, Sartori deplores attempts to expand a concept's extension without acknowledging concomitant diminishment in intention. Collier points out some exceptions to the law of inverse relation, but maintains the fundamental argument that in travelling to cover new cases, conceptual definitions can suffer unacknowledged distortion (Collier and Mahon 1993; Adcock and Collier 2001).[1] Such distortion makes a concept's terms, definitions, and referents inconsistent, violating a crucial criterion in evaluating social scientific work (Gerring 2009: 112).

To understand the nature of conceptual stretching in MMR better, a closer comparison of qualitative and quantitative approaches to conceptualization is necessary. Coppedge (1999), an early and eloquent advocate of MMR, describes qualitative concepts as "thick," having complex definitions developed iteratively through examination of a small realm of cases. In contrast, quantitative concepts are "thin," with relatively simple conceptual definitions. Conceptual thickness/thinness is inversely related to narrowness/breadth of extension. Because of their definitional intricacy and high intension, qualitative concepts are designed to apply to only a small number of cases. The proliferation of vocabulary about various democratic and authoritarian regime types and subtypes is exemplary of the type of highly descriptive conceptual categorization used in qualitative analysis. Quantitative scholars, by comparison, rely on datasets like Freedom House or POLITY, which reveal only that two countries are equally democratic (or undemocratic) and have no substantive meaning to the distance between intervals (Munck and Verkuilen