A Critical Summary of "When Multi-Method Research Subverts Methodological Pluralism – or, Why We Still Need Single-Method Research" by Amel Ahmed and Rudra Sil

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Summary
This article seeks to introduce a more balanced discussion of the value of multi-method research (MMR). Perceiving a "growing consensus over the virtues of MMR," particularly in international relations and comparative politics, Amel Ahmed and Rudra Sil argue that a blind acceptance of MMR as the superior methodological approach is flawed (pg. 935). Instead, "MMR is best thought of not as a magic bullet for overcoming trade-offs across methods, but rather as a valuable addition within a diverse repertoire of methodological approaches, one that comes with its own trade-offs" (pg. 948). This critical summary provides a brief overview of Ahmed and Sil's argument and then provides a brief critical interjection.

First Problem with MMR: The Threat of Methodological Hegemony
Ahmed and Sil value MMR insofar as it "implies that formal, quantitative, and qualitative methods are all valuable tools" (pg. 936). What the authors consider dangerous is the David Laitin approach, where MMR is treated as a ""best practice," accompanied by the expectation that a single scholar will produce better research by using two or more methods in executing a single project" (ibid). One of the outcomes of this increasingly hegemonic commitment to MMR is the fact that graduate students are increasingly incentivized and rewarded for MMR, hired at research universities and tenured for using MMR, and published in top journals based on employing a variety of methods in the research (ibid).

Second Problem with MMR: The Presence of Ontological Incommensurability
Ahmed and Sil assume that methods are not ontologically-neutral; that is to say, they are derived from a particular theory of the social world. As such, two or more methods can be used in unison, and may strengthen one another, only "when they happen to proceed on the basis of ontologies that are compatible if not similar, and only when these ontologies incorporate similar conceptions of causality cast as the same level of abstraction" (pgs. 938-939). This is what Ahmed and Sil refer to as the "problem of incommensurability" (pg. 939).

Why is it that different ontological conceptions matter? "Ontological assumptions imply certain boundary conditions for the investigation of particular questions, assign priority to certain types of observations and certain aspects of social reality, specify certain understandings of whether and when some set of observations constitute causation, and suggest certain strategies for presenting and assessing research products" (ibid). In short, there is a limit to the number of methods that can be triangulated in order to explore and verify a given substantive claim. Note that this should not be taken to by synonymous with the incommensurability between qualitative and quantitative methods: in fact, qualitative methods may also "be at a significantly greater distance from each other in terms of their ontologies than one of these approaches is from a regression analysis or a game-theoretic model" (pg. 939).

Ultimately, what matters is what type of social analysis we are conducting, and where it fits on a nomothetic-idiographic continuum (i.e. from more positivist, generalizable, law-like, natural-science-type propositions to more subjective, contextual, contingent, semantic approaches). How is one to combine a Geertzian cultural hermeneutic ethnographic approach with statistical analysis, for

example? (pgs. 942; 944). Similarly, a within-case process-tracing analysis cannot reliably be used to construct a formal model, for it requires "relaxing the very conception of a formal model […] a formal model by definition proceeds from a set of pure concepts and first principles […] If such elements were to be open to reformulation on the basis of a qualitative case study, it would no longer be a formal model in the proper sense of the term" (pg. 943). The opposite problem is illustrated in the "analytic narrative" approach, for here the stylized model explains while the narrative illustrates; and if the narrative is not allowed to enrich the causal analysis, then it is no more than window-dressing (pg. 944).

Third Problem with MMR: It Cannot Be Assumed to Reduce Error

Even if we move beyond an ontological discussion, it is important to remember that MMR does not necessarily reduce error. On occasion, in fact, error can be compounded via the use of MMR. As an illustrative example, Ahmed and Sil cite the case of "when a variable is excluded from the within-case analysis because a slight misspecification of the qualitative model makes it appear to be statistically insignificant" (pg. 941). This is, in large part, because different methods are suited to evaluating truth-claims at different levels of abstraction/generality.

For example, the authors cite Evan Lieberman's work (namely his books, *Boundaries of Contagion* and *Race and Regionalism*), which has been heralded as a model of the promise of a nested-analytic MMR approach. They argue that Lieberman's work is impressive, but his different methodologies are assessing truth claims at varying levels of abstraction. Thus his use of within-case process-tracing and statistical analysis 'speak past one another,' as it were: "[Statistical analysis' cannot possibly offer external validation for claims related to the shifting motivations of various groups across critical junctures that are defined in relation to country-specific historical processes" (pg. 942). Here, method 1 complements findings uncovered by method 2, but method 1 should not be taken to "increase the validity of a particular finding" using method 1 (ibid).

Fourth Problem with MMR: Discouraging the Mastery of a Single Method

Ahmed and Sil highlight that mastering methods is costly: "[c]onsider, for instance, the challenge of maintaining the language skills required to carry out archival research or ordinary language interviewing in a given locale. It is no less of a challenge to have to continually update one's technical skills to make use of the latest statistical models or the most sophisticated variants of game theory" (pg. 946). As a result, it is ultimately impossible to expect MMR practitioners to have mastered each method in their toolkit. The costs of learning methods "create pressures on individuals pursuing MMR to work with the less complicated variants of selected methods in the interest of being able to "do it all"" (ibid). The authors highlight that there should not be a presumption against single-method mastery. To illustrate this, they highlight three highly influential works in political science that employ single-method research (SMR), or close to it: Larry Bartels' *Unequal Democracy* (employing sophisticated statistical analyses); Lisa Wedeen's *Peripheral Visions* (which leverages a context-sensitive interpretive analysis); and Elinor Ostrom's *Understanding Institutional Diversity* (whose greatest contribution lies in its use of game theoretic models). In the end, Ahmed and Sil are particularly concerned for qualitative-idiographic SMR, which is "likely to be context-bound and hence more difficult to triangulate with causal generalizations produced by formal or large-N approaches" (pg. 947).

A Critical Interjection

Ahmed and Sil's overall premise and comments are well taken. But the authors seem to conflate theory with method. It is true that rational choice theorists are by far the most prolific users of formal and quantitative methods whereas practitioners of cultural hermeneutics usually rely on ethnographic

research. But we should be careful not to assume that the presence of a regression model in a paper implies a rational-choice approach or that a chapter in a book with some thick description disqualifies its author from being a rational-choice theorist. In particular, the problem of ontological incommensurability is a theoretical/perspectival problem, not an inherently methodological one.

If we instead distinguish theory and method – if, for example, we assume that a game theoretic model does not require a rational choice approach – then a whole set of possibilities open up. For example, consider the following research project. First, a cultural hermeneutic approach based on extensive ethnographic research is used to understand the content of notions of "rationality," "altruism," "self-interest," "public good," and "community" within a particular cultural setting. Second, a game-theoretic model is constructed, leveraging these culturally-sensitive (and context-specific) notions to model behavior and generate observable implications. Third, observable implications are collected and a statistical analysis is conducted across jurisdictions that are most similar to the cultural setting subjected to the intensive ethnographic research in the first stage, allowing the researcher to test the generalizability of the model. Here, we combine multiple methods – including both formal and quantitative methods – without resorting to a rational choice approach.

In the end, what is needed is a two-tiered discussion, one that begins by focusing on the challenges and opportunities of theoretical pluralism (or theoretical amalgamation) and only then proceeds to discuss the implications for the use of MMR or SMR.