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Are We All Set?

Carsten Q. Schneider
Central European University, Budapest
schneiderc@ceu.hu

Claudius Wagemann
Goethe University, Frankfurt
wagemann@soz.uni-frankfurt.de

With their book, Gary Goertz and James Mahoney (henceforth G&M) make an important and useful contribution to the often only vaguely defined battle over the differences and similarities of so-called quantitative and qualitative methods in the social sciences. This debate not only frequently pops up in graduate seminars, but also sometimes risks to represent a real fracture in the social sciences, with whole departments taking side or simply being divided into two (or more, as we argue) methodological camps. Much of this general debate has long suffered from the fact that one culture—the quantitative—was defined in much clearer terms than the other. Often times, "qualitative" was simply understood as the residual category "not-quantitative." For us, the major contribution of G&M consists in providing a clear and systematic understanding of the qualitative culture. Simply put, for G&M, the common denominator for qualitative research is the analysis of sets and their relations. Needless to say, sorting out the understanding of "qualitative research" unavoidably leads to, well, sorting out, or at least under-emphasizing, several features of qualitative research that some scholars would consider crucial. We therefore argue that G&M's vision of qualitative research as being rooted in set theory, next to being a boon, might turn into a bane unless it is clearly spelled out what their equation "qualitative research = set/theoretic research" does not entail. In the following, we share some thoughts on this and also discuss some common issues in set-theoretic research that remain under-developed in G&M's book.

The Boon: A Crisp Understanding of Qualitative Research

In our view, it is one of the most important merits of their text that it offers a clear image of what is meant when the term "qualitative" is invoked. G&M see qualitative research squarely rooted in *set theory* (pp. 16ff.)—regardless of whether

or not practitioners of qualitative research themselves are aware of this fact. We see two main virtues in this.

First, this allows for a crisper contrast with non-qualitative methods, for as G&M convincingly show, sets and interest in set relations do not feature high in the quantitative culture. Second, G&M have carved out a language for all those scholars that do not feel at ease with applying statistical principles and practices to their research. We are certainly not the only ones who are frequently confronted with student papers or PhD *prospecti* written by students with a clear qualitative research intention who, however, resort to a terminology squarely rooted in the quantitative camp, such as variables, correlations, hypotheses testing, control variables, and so on. More often than not, this leads to incoherent, if not incomprehensible, research designs. For those students, a thorough reading of G&M will be a boon in their struggle for coming up with a coherent research design and an adequate terminology.

The Bane: The Role of Set Theory in the Social Sciences is Both Broader and More Narrow

As proponents of set-theoretic methods ourselves, we have much sympathy with the argument that this system of thought ought to play a major role in qualitative research. We think, however, that readers of their book might have two issues with G&M's view on set theory's role in the social sciences. In a sense, it seems that they attribute both too little and too much importance to the role this mathematical system is playing. To us, G&M could clarify more which of the activities and features that clearly belong to the qualitative culture they do *not* subsume under the set-theoretic umbrella. At the same time, they tend to downplay the role set theory can and should play within the quantitative culture.

Not All Qualitative Research Activities are Grounded in Set Theory

We share G&M's vision of set theory being at the heart of much of qualitative research (Schneider and Wagemann 2012: 2f.). What might be misleading to some readers is that G&M, in our impression at least, do not make sufficiently clear that they restrict this claim to those aspects of qualitative research that aim at drawing inference based on already existing. Large chunks of qualitative research, however, consist in generating data, which is typically time consuming. In the eyes of some, data generation rather than drawing causal or descriptive inference ought to be the definig core of qualitative research. For those scholars G&M's claim that qualitative work is always grounded in set theory is confusing, because, needless to say, much of such field work is not, and does not have to be, rooted in set theory. Our point here is not that G&M claim otherwise, but that it should have been made clearer that they do *not* pretend that qualitative scholars who gather data in the field, who unearth hitherto unknown facts, and who summarize them in long and oftentimes complex narratives should do so using set theory as their guide.

Certainly, space in a book is limited and G&M's juxtaposition of set-theoretic qualitative research vs. covariational quantitative research does shed useful light on many hidden assum-

ptions, differences, and similarities among these cultures. Yet, leaving unmentioned the uneasy fit of this reductionist view on qualitative and quantitative research might have unintended consequences. Ironically, with their focus on set theory and the downplaying of field work and the intimate study of cases, G&M might lead at least some readers to believe that the qualitative culture looks much more like the "armchair research" approach commonly attributed to the quantitative culture. This is ironic in two ways. For one, it runs counter to G&M's (correct, we believe) message that most of the inferential leverage in qualitative research must come from within-case rather than cross-case analysis. It is also ironic because it might send the wrong message to qualitative scholars at the beginner's level that armchair research is a viable option in qualitative research—as long as it is framed in terms of set relations.

Some Quantitative Research Can and Should be Set-Theoretic

While not all qualitative research is set-theoretic, some quantitative research can and should be. If a large chunk of social science theories stipulates set relations, as argued by Ragin (2000), then adequate quantitative tests of these theories would need to be set-theoretic in nature as well. We think G&M could have emphasized more the pervasiveness of set-theoretic claims in social science theories, and the misfit between theories and methods—and thus also between the ontology of social research and its methodology (Hall 2003)—that arises if quantitative researchers ignore set relation in their empirical tests.

G&M do acknowledge that there are attempts at modeling set relations within the statistical framework but that they are far from mainstream, to say the least (see esp. chapter 4). To us, this contains several important messages. First, contrary to often implicit, and sometimes explicit (Clark, Gilligan, and Golder 2006), beliefs, the mainstream statistical toolbox is not adequate for analyzing set relations. Second, trying to analyze set relations with the statistical tools requires quite distinct and pretty complex tools (for illustration, see Braumoeller 2003). Third, since most of these attempts at addressing set relations within the statistical camp have been developed in the past decade or so, it is reasonable to infer that it was the increasing focus on set relations in the qualitative literature (Ragin 2000, 2008b) that has triggered this interest among quantitative scholars. To us, this is a contribution of the qualitative camp to the general methodological debate that deserves recognition and could have been underlined even more strongly by G&M.

Really Two Cultures?

Not everybody agrees with social science research being a tale of two cultures. Some argue that there are more, others that there are less.

Della Porta and Keating (2008: 32) identify four approaches and related methodologies in the social sciences. Blatter and Haverland (2012) differentiate between three approaches: co-variational analysis, causal process tracing, and congruence analysis. Indeed, congruence analysis in particular appears to be an addition to the discussion which overcomes the focus on statistically inspired approaches on the one hand and within-case analysis on the other. Even Ragin's idea of "moving beyond qualitative and quantitative strategies"—the subtitle of his book from 1987—is already an attempt at defining a third culture. Ironically, in so doing, he popularized precisely those set-theoretic approaches which G&M now identify as being at the core of one of the two methodological cultures. This begs the question where Ragin's proposed third variant has ended up.

While several authors argue that there are more than two cultures, others, such as Gerring (2012), put emphasis on methodological monism. In fact, even authors like Brady and Collier (2004) and most of the contributors to their volumes, while being in disagreement over many issues stemming from King, Keohane, and Verba (1994), seem to accept the claim that there really is just one unifying framework, as expressed in their subtitle "Diverse Tools, Shared Standards."

A related issue is the nature of single case studies. For many scholars, single case studies count as qualitative research *par excellence*. We believe that G&M subscribe to this view as well. It is, however, not directly obvious how set theory and the search for set relations are meaningfully applied when only one case is at hand. G&M certainly have interesting thoughts to share on how set theory makes sense with an *N* of 1. Their chapter 8 on causal mechanisms provides some clues, as do other publications by the authors (Mahoney, Kimball, and Koivu 2009; Mahoney 2012). Yet, a more explicit treatment of single case studies would have been good. Not least because other scholars who have dedicated more extensive thought to the nature of single case studies, such as Gerring (2007: 187ff.) or Rohlfing (2012), seem to challenge the claim that all single case studies rest their descriptive or causal inference on set theory. Rohlfing (2012: 4), for instance, explicitly argues that single case studies can fruitfully draw on both correlational or set-relational notions and research practice, thus cutting across G&M's two cultures.

Set-Theoretic Perspective with Some Blind Spots

Even if not everybody might subscribe to the claim that set theory is the unifying framework for all qualitative research aiming at causal inference, it is, we believe, unquestionable that some, perhaps most, of this research is set-theoretic in nature. How good of a guide is G&M? In the following, we discuss several issues where G&M's treatment of set theory might have some blind spots.

Semantic Transformation and Causal Properties

In the qualitative culture, data transformation purely driven by technical reasons (such as logging skewed data) is considered bad practice and replaced by what G&M call "semantic transformation" (p. 140), otherwise known in the set-theoretic literature as the "calibration" of sets (Ragin 2008b). We agree, but think it important to underline a feature of this semantic transformation that remains hidden in G&M's account.

Oftentimes, the very meaning of a concept already embodies a causal component. Hence, when calibrating a set, qualitative scholars also take into account their knowledge or

assumptions about the causes or effects of that very set. For illustration, imagine a researcher is interested in the conditions for being a member of the outcome set of "persons being married." One of the conditions in the analysis is the set of "rich people." Now, when transforming the semantic meaning of "being rich" into set membership scores, qualitative scholars routinely will also ponder what the meaning of "rich" is in the context of finding a spouse, that is, what it is about a person's wealth that is expected to be causally relevant on the mating market. That meaning is different from the meaning of "being rich" in a study interested in finding out who is, say, a member of the set of "private jet owners." It is not difficult to imagine that this crucial and common component of the semantic transformation—taking expectations on the causal effect into account when coding the data—causes disbelief among quantitative scholars, so explicitly mentioning it seems important when carving out different cultures.

Qualitative Differences Trump Differences in Degree—Even in Fuzzy Sets

Another observation with regard to set calibration is that G&M tend to downplay the fact that even with fuzzy sets, researchers first and foremost establish qualitative differences between cases, and only after that differences in degree with regard to each case's membership in a given set. This qualitative difference is established by the so-called 0.5 cross-over point (Ragin 2008a). Cases above this anchor are qualitatively different from those below in terms of their membership in the set. For instance, assigning the membership score of 0.8 in the set of "democratic polities" means that the researchers consider this to be a democracy—full stop. The case simply is not an ideal typical democracy, which would be signaled by full membership. Likewise, assigning the score of, say, 0.3 means that the country under investigation is not a democracy—full stop. It is, however, not fully out of the set.

For an illustration that G&M downplay the role of the 0.5 qualitative anchor and of the analytic intricacies this can create, consider their figure 13.4 (p. 167). We agree with G&M's observation that one and the same case usually has partial membership in more than one regime type. We disagree, however, that one and the same case should be allowed to have partial membership of *higher than 0.5 in more than one* type. This is the message of figure 13.4, though. A country with a Polity score of 7.8 would hold a fuzzy set membership score of 0.6 both in the category of anocracy and democracy. We find this conceptually and research-practically misleading.

Conceptually, when asked, which ideal-typical regime form such a case resembles most, researchers would have to respond that, qualitatively, it counts both as a democracy and an anocracy. Assigning one case to more than one ideal type runs counter not only to intuition, but also to established practices in set-theoretic research. For instance, at the core of QCA is the so-called truth-table algorithm (Ragin 2008b: chap. 7; Schneider and Wagemann 2012: chap. 7). It is based on assigning each case to one, and only one, truth table row, which, in turn, are considered to represent all logically possible, mutually exclusive, and jointly exhaustive ideal types built by the

conditions under study. Likewise, the post-fsQCA case selection criteria formulated by Rohlfing and Schneider (Rohlfing and Schneider 2013; Schneider and Rohlfing 2013) crucially rest on attributing cases to ideal types based on their membership scores being above or below 0.5 in a given set. Allowing cases with multiple scores above 0.5 not only leads to substantive confusing, but also annihilates these set-theoretic procedures.

The insights that even with fuzzy sets researchers maintain the notion of a dichotomous concept, and that this dichotomy is established by the 0.5 anchor, have further implications worth mentioning. First, the closer a case's membership score comes to the 0.5 anchor, the less information we have about its qualitative status (0.5 is also called the "point of maximum ambiguity," Ragin 2008b: 30). Hence, the farther away from 0.5 a case's membership score, the more certain we are about its conceptual status. Second, when performing the semantic transformation, the most consequential decision is where to locate the 0.5 qualitative anchor rather than the 0 or 1 qualitative anchor. Third, and related to this, the robustness of set-theoretic findings vis-à-vis equally plausible semantic transformations is affected, if at all, by the location of the 0.5 anchor rather than that of the other anchors or the specific functional form (Schneider and Wagemann 2012: 284ff., Skaaning 2011).

Equifinality, Causal Homogeneity, and Scope Conditions

G&M argue that qualitative researchers pay more attention to the appropriate scope of their findings and that they usually try to establish causal homogeneity by limiting the scope of their argument (Goertz and Mahoney 2012: 108f., 205ff.). This resonates with other authors' take on that matter (see, e.g. Ragin 2000: chap 2 on constitution populations). We agree, in principle, but wonder how, in practice, this is reconciled with another constitutive feature of qualitative research, namely the emphasis on causal equifinality. If qualitative researchers routinely find that different causes produce the same outcome, where, if anywhere, is causal homogeneity?[1]

This is a genuine question and we have only a tentative answer. In our view, in the presence of equifinality researchers can only maintain the claim of causal homogeneity if and when they formulate explicit arguments that all sufficient terms (cross-case) or mechanisms (within-case) are functional equivalents of a higher-order concept. For instance, if the result of an analysis is that both combinations A*B and C*D are sufficient conditions for Y,[2] then—for maintaining the claim of causal homogeneity—researchers would have to argue that both conjunctions are different empirical manifestations of one and the same higher-order, more general concept. Schneider (2009), for instance, finds several sufficient paths towards the consolidation of different third-wave democracies, but all of these paths are different expressions of the same principle: the fit of political institutions to the societal contexts in terms of their respective degrees of power dispersion. Hence, what looks like causal heterogeneity and equifinality at a lower level of abstraction is explicitly interpreted as causal homogeneity and unifinality at a higher, more general level of abstraction. This implies that causal homogeneity cannot be simply assumed in qualitative

research, but must be established through conceptual arguments *after* having generated equifinal results.

Conclusion

G&M have done a great service to both students and users of social science methodology. Their message is clear, innovative, and helpful in many respects. Not everybody will agree—some because they hear G&M claiming things that, we think, they do not; others because they fundamentally agree. But because G&M's is an internally coherent proposition, the lively debates this book will surely trigger can be expected to be productive and coherent as well.

Notes

[1] Equifinality can occur at the cross-case level—*different* (combinations of) conditions are sufficient for the *same* outcome—and/or at the within-case level—*different* causal mechanisms are operative among cases with the *same* sufficient condition(s). In the framework of necessary conditions, causal heterogeneity comes in the form of SUIN conditions (Mahoney, Kimball, and Koivu 2009; Rohlfing and Schneider 2013).

[2] Using Boolean logic, we would write: AB + CD →Y.

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Two Cultures and Beyond:
A Plea for Three Approaches

Joachim Blatter
University of Lucerne, Switzerland
joachim.blatter@unilu.ch

Markus Haverland
Erasmus University, Rotterdam
haverland@fsw.eur.nl

Gary Goertz and James Mahoney are masters in presenting methodological messages in an accessible, lucid, and at the same time focused and precise style. Their *Tale of Two Cultures: Quantitative and Qualitative Research in the Social Sciences* is another impressive example of this quality. In this book they juxtapose the statistical and the set-theoretical ways of thinking as two distinct and internally "relatively coherent cultures of research" (footnote 2, p. 5). They feel obliged to label these two cultures "quantitative" and "qualitative" because "the qualitative-quantitative distinction is built into nearly everyone's vocabulary in the social sciences, and it serves as a common point of reference for distinguishing different kinds of work" although they admit that those labels "are quite inadequate for capturing the most salient differences between the two traditions" (p. 5). For Goertz and Mahoney, the two cultures differ basically in two respects: (a) whereas quantitative research is focusing primarily on cross-case analysis, inferences in qualitative studies are drawn primarily on the basis of within-case analysis; (b) quantitative scholars use the sophisticated techniques of statistics; qualitative scholars, in contrast, use—albeit often implicitly—logic and set theory. We agree with Goertz and Mahoney when it