

Qualitative & Multi-Method Research

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Letter from the Editor

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One of the most common symposium formats in this newsletter has been discussion of a newly published book of interest to the QMMR membership. The current issue introduces an adaptation of this recurrent format with the appearance of our first multi-book symposium. The symposium “A New Wave of Qualitative Methodology” draws together into one discussion the authors of five recently published books: Gary Goertz and James Mahoney, *A Tale of Two Cultures: Qualitative and Quantitative Research in the Social Sciences* (Princeton University Press, 2012); Derek Beach and Rasmus Brun Pedersen, *Process-Tracing Methods: Foundations and Guidelines* (University of Michigan Press, 2013); Carsten Q. Schneider and Claudius Wagemann, *Set-Theoretic Methods for the Social Sciences: A Guide to Qualitative Comparative Analysis* (Cambridge University Press, 2012); Joachim Blatter and Markus Haverland, *Designing Case Studies: Explanatory Approaches in Small-N Research* (Palgrave Macmillan, 2012); and Ingo Rohlfing, *Case Studies and Causal Inference: An Integrative Framework* (Palgrave Macmillan, 2012). The symposium evolved from an initially planned single-book symposium, and I would like to thank Gary Goertz and James Mahoney for so warmly welcoming the adaptation of the intended discussion of their *A Tale of Two Cultures* into the multi-book discussion that appears here. Finally, I would like to thank Joachim Blatter for his assistance in suggesting just how this discussion might be organized. The symposium has ended up organized as follows: it begins with three focused commentaries on Goertz and Mahoney’s book from other authors, next turns to general commentaries from Rohlfing and from Goertz and Mahoney that reach across all the books, and then closes with a round of responses to issues raised in the commentaries.

Symposium: A New Wave of Qualitative Methodology

Review of *A Tale of Two Cultures*

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Goertz and Mahoney's (GM) *A Tale of Two Cultures* can be seen as the logical capstone of the debates between quantitative and qualitative methods that was sparked by King, Keohane, and Verba's (KKV) 1994 publication of *Designing Social Inquiry*. A definitive, though selective, answer to KKV's one logic is put forward, with GM clearly illustrating core foundational differences between quantitative and qualitative "cultures" of research, including approaches to the symmetry/asymmetry of causal relationships, focus on effects-of-causes or causes-of-effects, and set-theory versus statistical correlations and probability theory. As such, the book is a welcome counter to recent monistic pronouncements about research methods within the social sciences, including Gerring's 2011 *Social Science Methodology*.

Beyond defining what differentiates the two cultures from each other, GM ambitiously set out to define what they believe is a shared core set of elements that define the qualitative culture. At the outset they exclude more interpretive approaches from their review, focusing on causal-oriented qualitative case study methodologies that can at least in principle communicate with quantitative, causal-oriented research. The book then defines what can be thought of as a "monist" definition of qualitative, causal-oriented research focused on set-theory, coupled with correlational and counterfactual understandings of causal relationships.

Naturally such an attempt to impose a "monist" definition upon such a broad field as causal case study methods will spawn significant debate amongst qualitative scholars. However, a fair metric by which GM's monist definition of qualitative culture can be evaluated with is whether it accurately reflects the emerging "state of the art" as demonstrated in recent works on qualitative, causal case study methodology.

Here we believe that GM's attempt to impose a monist definition does not reflect recent work in the field, where we are increasingly coming to appreciate the *diversity* of the methodological tools we have in the qualitative case study toolbox. This diversity was first displayed in George and Bennett's now classic 2005 book, but can be seen in more recent methodological contributions, including the other books reviewed in this newsletter, which either focus on developing the foundations and uses of particular case study methods (like QCA or process-tracing), or that develop different causal case study methods in a way that respects diversity (Blatter and Haverland 2012; Rohlfing 2012).

Following George and Bennett (2005), we contend that there are (at least) three distinct causal case study methodologies: (1) small-n, structured, focused comparative methods, (2) congruence, and (3) process-tracing (PT) case studies. While we agree with GM that all three methods share certain common ontological and epistemological foundations, they also differ on a range of issues in ways unacknowledged by GM. These differences have major implications for how we can use each method, along with their comparative strengths and weaknesses.

Yet by painting over these differences, GM's monist definition defines away many of the comparative strengths (and weaknesses) of individual causal case study methods. Additionally, GM's monist definition does not offer qualitative scholars tools to justify why they chose a particular causal case study method in a specific research situation, and why it offers the most analytical bang-for-the-buck in comparison to other methodological tools in the toolbox.

In the following we first discuss what we believe are the many strong points of the book. We then explore the implications of GM's monist definition in detail, using their treatment of PT methodology as an example, where it is our contention that GM's monist definition defines away many of the comparative strengths of PT.

Strong Points of the Book

The book is designed for teaching, with concise individual chapters on a range of subjects that handily can be assigned as core readings. Many of the chapters are simply the best treatments of certain methodological topics that have been written to date. For example, there are excellent discussions of the topics such as causal complexity, equifinality, and asymmetric causation that are wonderfully concise, but that also have extensive suggestions for further reading, making the book able to be used in teaching from the graduate level and upwards. The introduction to set theory is also excellent, and should be required reading for all scholars (quantitative and qualitative). Given the comparative expertise of the authors, it is also not surprising that there is a very strong chapter on conceptualization and the implications that set theory has for defining concepts. In particular, the treatment of the principle of unimportant variation is very concise and should make clear for students what is a very complex topic. Therefore, many of the chapters will become standard references for methodological courses on qualitative methods.

Furthermore, GM's ambition to facilitate communication by recognizing and appreciating methodological differences *across* the two cultures should be commended (p. 2). While we disagree with the decision to impose a monist definition of the qualitative "culture" (see below for more), this decision makes a lot of sense if our intention is to communicate with quantitative scholars. Too often the two communities talk past each other. Many (but by no means all!) quantitative scholars define "scientific" research as designs that either approximate experiments, or analyze large samples or populations using statisti-

cal analysis. A good example of this disdain for qualitative methods was a statement made by one of the founding fathers of the so-called “scientific approach” to the study of IR, David Singer, in a roundtable at the 2007 International Studies Association. Here he stated that there are two approaches to studying IR: the “scientific” one (involving statistics), and “poetry” (everything else). On the other hand, many qualitative scholars look disparagingly at the statistical results published in top journals, contending that they tell us very little about causal relationships among important theoretical phenomena.

Better understanding of each other’s methods and how they differ from the one (statistical/experimental) logic promulgated by KKV or Gerring is therefore vital for improving communication between the two cultures. Whilst qualitative scholars in their training do learn a lot about quantitative methods in mandatory courses on statistics, quantitative scholars often have little training in qualitative methods, and if they do, it is often a KKV-based course that teaches them very little about what qualitative case study methods actually are. Despite our disagreements with certain elements, we therefore contend that GM’s book should be required reading for quantitative scholars in order to improve their understanding of differences across the two cultures of research.

The Downside of a Monist Definition of Qualitative Methods

We contend that by defining away the differences *within* causal case study methodology, GM end up downplaying the comparative strengths (and weaknesses) of particular methods. We will explore this using GM’s treatment of PT methodology as an example, showing that they end up with a very minimalistic definition of PT that defines away the comparative strength of the method in comparison to other case study methods. Given the centrality that they place on PT within the qualitative culture (p. 103), we believe that GM’s demotion of the method is particularly problematic for several reasons.

First, GM define PT merely as a method “used to evaluate hypotheses about the causes of a specific outcome in a particular case. The method is built around two main kinds of tests: Hoop tests and smoking gun tests” (p. 93). They put causal process observations at the heart of the method (p. 90-92; Brady and Collier originated the term in 2004 in *Rethinking Social Inquiry*).

Yet test types and types of empirical material are not what define a methodology. There is an emerging consensus in the literature that PT as a distinct research method is defined by its ambition to study the causal *mechanisms* by which X contributes to produce Y (George and Bennett 2005; Checkel 2008; Bennett 2008, 2010; Glennan 1996; Bunge 1997; Beach and Pedersen 2013). While GM do mention causal mechanisms, their minimalist definition of PT does not include mechanisms but instead focuses on test types and CPOs. This omission probably stems from their reluctance to wade into the often heated (and confusing) debate about the nature of causal mechanisms (p. 100), but by never defining what mechanisms are we are left with an “anything goes” understanding, where a mechanism can be anything from a set of intervening variables between X and Y to a set of events—neither of which

can be counted as a causal mechanism based upon how the term is widely defined in the qualitative literature (e.g. Bennett 2008; Waldner 2012; Beach and Pedersen 2013).

Understanding mechanisms as intervening variables is problematic for many reasons, although the most important one is that it prescribes designing research focused on maximizing variance instead of focusing our attention on the *linkages* between parts of a mechanism that transmit causal forces from X to Y. Not surprisingly, Bunge has termed an intervening variable understanding as “grey boxing” mechanisms (1997). Even more problematic is treating mechanisms as a series of events. Unless events are the observable manifestations of the existence of parts of a mechanism, they lack any theoretical significance. PT is a theory-guided method where we are tracing whether a theorized causal mechanism (or mechanisms) was present in a case. Merely tracing events between the occurrence of X and Y is just descriptive analysis, and does not enable us to make inferences about theorized causality.

However, the promise of PT as a methodological tool is that it enables the researcher to study more or less directly the causal mechanism linking a causal condition (or set of conditions) and an outcome, allowing us to open up the “black box” of causality and focus on how X contributes to producing an outcome through the operation of a causal mechanism. To reap these benefits we need to have a clear definition of what mechanisms are. Surprisingly therefore, GM do not even discuss the “mechanismic” understanding of causation, focusing solely on correlation and counterfactuals understandings.

Second, downplaying mechanisms defines away the crucial differences *between* what are widely accepted as two distinct within-case methods: PT and congruence. The core difference between the two deals with what type of causal relationship we are making inferences about: a causal theory ($X \rightarrow Y$) or the parts of a causal mechanism between X and Y ($X \rightarrow M \rightarrow Y$). Congruence methods involve producing multifaceted predictions of empirical patterns we should observe in a case for a given *causal theory* ($X \rightarrow Y$), whereas PT involves producing predictions about observable implications that we should find in a given case for *each part of a causal mechanism*, in effect unpacking the causal arrow into a series of parts that together transmit causal forces from X to Y (see George and Bennett 2005; Khong 1992; Beach and Pedersen 2013).

GM then put forward Tannenwald’s 1999 study as an exemplary work of PT (p. 91). Yet her study has no ambition to study causal mechanisms, which is how most methodologists define PT methodology (e.g. Bennett, 2008, 2010; Checkel, 2008; George and Bennett, 2005). Instead she puts forward a strong empirical prediction (taboo talk) or what she should find if the hypothesized causal theory (norms \rightarrow non-use of nuclear weapons by the US) is present in the cases. She then investigates the match between this prediction and the empirical record using a temporal analysis of four cases (t_0, t_1, \dots, t_n), but at no point does she analyze mechanisms between X and Y. Therefore Tannenwald is actually using the congruence method, not PT.

Conflating the two within-case methods masks their comparative strengths and weaknesses, and results in quite murky

prescriptions for proper research design for within-case analysis. Congruence analysis in many respects is more analogous to quantitative analysis of causal effects, in that the research question is *whether* a given X (or set of Xs) had an impact upon Y. Tannenwald's question was whether norms mattered for the US non-use of nuclear weapons. In contrast, PT asks *how* a given X (or set of Xs) contributes to produce Y. Reframed in this fashion, if Tannenwald was doing PT she would have gone beyond just measuring "taboo talk" by actors, and instead probed how they mattered by opening up the psychological decision-making mechanisms whereby norms impacted upon behavior.

Additionally, other key elements of PT that are essential to understand if we are to use PT also disappear in GM's monist definition. It is particularly surprising that there is no mention of Bayesian reasoning in relation to within-case analysis. A quantitative-oriented reader will be left wondering how we can use CPOs as manifestations of empirical predictions made using different types of tests to make causal inferences when there is no variation in values of X and Y to assess. Bayesian logic is implicit in van Evera's treatment of the four different possible types of tests, and has been made explicit in recent work on PT (Bennett 2008, 2010; Beach and Pedersen 2013; Rohlfing 2012). Yet without developing how Bayesian logic can be used to underpin strong causal inferences in within-case designs that lack variation, quantitative scholars will not be convinced, nor will qualitative scholars understand how they can improve the strength of the inferences that they are able to make by designing tests that, for example, maximize the likelihood ratio (uniqueness) (see Beach and Pedersen 2013).

Finally, the description of PT in practice looks a lot like historical institutionalist (HI) theories. Unfortunately, there is a strong tendency in the methodological literature for scholars to import their own theoretical ideas into their descriptions of PT methodology. For instance rational choice-oriented scholars argue we have to conceptualize mechanisms using Coleman's bathtub model, where all forms of phenomenon should be linked with micro-level processes. In parallel, we can see the clear imprint of HI theorization when GM write that in PT we, "inevitably carry out an over-time, processual analysis of the case... The analyst will normally identify historical junctures when key events directed the case toward certain outcomes and not others. She or he may well pause to consider how small changes during these junctures might have led the case to follow a different path... The overall explanation likely will be rich with details about specific events, conjunctures, and contingencies" (p. 89). All of this looks like a description of HI theory. In our opinion there is no reason for us to bias our methods towards particular theories, be they RC or HI. A theory is not a research methodology, and vice versa.

The conclusion is that imposing a monist definition results in the reader being left in the dark regarding what exactly we are doing when we engage in PT. What exactly are we tracing, and how does PT differ from other case study methods? How can we make causal inferences when we have no variation? When should we use PT, and when should we use small-n comparative methods?

Moving the Discussion Forward: What We Now Want to Know

GM's book is an excellent introduction to the debates that have raged for the past twenty years between quantitative and qualitative methodologists. On topics like conceptualization, GM have produced accessible chapters that are also important contributions to the literature. As argued in the introduction, GM's book should therefore best be seen as the logical capstone of the KKV-inspired debates between quants and qualis. This does not mean that we denigrate the important contribution that GM's book makes, but simply that causal case study methodology has post-GM's book reached a level of maturity where we no longer need to define our identity solely by how we differ from quantitative methodology.

What qualitative scholars now want to know is how causal case study methods themselves are similar and different to each other both as regards their ontological and epistemological foundations, the research situations in which they can be utilized, and the guidelines and best practices for their use. A new post-KKV generation of methodologists has begun asking the question "and now what," shifting the focus of work towards defining the nature and uses of different causal case study methods on their own terms instead of constantly juxtaposing them with quantitative methods. Here Rohlfing's and Blatter and Haverland's books are good examples of this next generation of methodological scholarship. Both books appreciate the plurality of methods within causal case study research, and attempt to illustrate critical similarities and differences and their methodological implications along a number of parameters such as different understandings of causality and logics of causal inference.

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Are We All Set?

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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 stemming from 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 meaningful research plan 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 data. 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 defining 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 relations 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: covariational 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 disagree. 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: $AB + CD \rightarrow Y$.

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Two Cultures and Beyond: A Plea for Three Approaches

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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: Qualitative and Quantitative 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

comes to the two methodological cultures they describe but we are less convinced by their labeling, because a significant part of what makes research qualitative is not captured by their approach. We will come back to this point when we present our more critical remarks about the book.

First of all, we would like to stress that Goertz and Mahoney's work, which is partly condensed in their new book, has had a tremendous influence on how we think about, teach, and practice concept formation, case studies and Qualitative Comparative Analysis, and that we strongly believe that the (more explicit) use of logic and set theory will improve case study research tremendously. Next, we want to point to some highlights within the *Tale of Two Cultures* in order to show why the book includes some very helpful resources for teaching social science methods:

(a) In the Introduction they provide an important insight and a convincing argument for why it is adequate to talk about two cultures. On page 8 they acknowledge that it might "be possible for quantitative researchers to mimic qualitative practices and vice versa" (e.g., the analysis of necessary conditions with statistical tools). Yet they correctly point to the fact that "[w]hile one might conceive ways of extending the tools of one culture to do what is easily accomplished in the other culture, these extensions are unnatural and usually purely hypothetical."

(b) Right after the "Mathematical Prelude" the authors start with distinguishing "two different ways to ask and address causal questions" (p. 41) and present the "causes of effects" (X-centered) versus the "effects of causes" (Y-centered) approach. Although we think that the dichotomy is too simplistic (since it ignores a third way that is based on coherent theories as "constitutive and causal schemas"), we fully agree that different kinds of research interests represented in distinct types of questions and the corresponding approaches "have tremendous downstream methodological consequences." In line with scholars who connect epistemological/methodological reflections with generic social theory (in contrast to empiricist "causal models")—e.g., Alexander Wendt (1999: 78)—we stress in our book even more than the authors that the kind of questions that we ask should determine the research design and the methodological tools in empirical studies. Instead, very often the methodological approach to which scholars are socialized determines the questions they ask.

(c) Pages 51–54 in the chapter on Causal Models represent our favorite example for the authors' capability to translate similar concepts (here: the formulation of a causal model) into diverse languages which are used within the different cultures (for the qualitative side they provide formulations in ordinary language, in logical notation with Boolean operators, and in the notation of set theory). Furthermore, it nicely illustrates that translation within a culture is rather easy whereas a simple translation across cultures is quite tricky and can be misleading. Nevertheless, at this point the downside of their brief treatments of many topics shows up. On page 54 they provide some arguments in order to show why causal configurations within a set theoretical model are not the same thing as interaction terms in statistical analysis. They can point to the

Mathematical Prelude in which they have laid out in more detail that the analogy between the logical AND in set theory and multiplication in statistical models is only partial, and point to the fact that in set-theoretical models there is no intercept term. But they gloss over the fact that in set-theoretical models there is also no error term. The latter has been at the heart of recent methodological criticism of QCA (Hug 2012). It is a pity that they do not address this fact—especially since they provide valuable insights on the different treatments of errors in their chapter on concept formation.

This leads to our more critical comments. We feel that the second part of the book that deals with "within-case analysis" does not give credit to the existing variety of approaches. This does not mean that this section does not contain important messages that we fully endorse. For example, given the fact that since the 1970 case study methodology has been dominated by the comparative method proposed by Przeworski and Teune (1970) and Lijphart (1975), and that cross-case analysis is still predominant in some recent case study books (Gerring 2007, Rohlfing 2012), it is an important statement when the authors claim that "[i]n small-N qualitative research, the main leverage for causal inference derives from within-case analysis, with cross-case methodologies sometimes playing a supporting role" (p. 88). Furthermore, they go on with some promising descriptions of what within-case analysis is when they state that "[i]n the effort to formulate good explanations, the case-study researcher will inevitably carry out an over-time, process-oriented analysis of the case. Many different observations at different points in time will be considered. The analyst will normally identify historical junctures when key events directed the case toward certain outcomes and not others" (p. 89). This sounds pretty much like the way we describe the "causal-process tracing" approach in our book, highlighting the important role of time and timing for a method with the term "process" in its title (Blatter and Haverland 2012: 79–143; see also Blatter and Haverland, forthcoming).

Unfortunately, Goertz and Mahoney do not take this into account anymore afterwards. Instead, they follow the lead of Bennett (2008) and Collier (2011) when they argue that process tracing "is built around two main kinds of tests: hoop tests and smoking gun tests" (p. 93). De facto, they reduce process tracing and within-case analysis to the application of logic and set theory in a deductive research endeavor (the inductive use is only briefly mentioned in a footnote), which means that the second defining element of process tracing—the role of timing and temporality—gets lost. But this aspect clearly dominated when Alexander George (1979; 1985 with Timothy McKeown) introduced the term process tracing to the social sciences. In the book that Alexander George wrote with George Bennett, we find a broad and diverse set of elements that is supposed to characterize process tracing. They introduced process tracing as "an operational procedure for attempting to identify and verify the observable within-case implications of causal mechanisms" and defined "causal mechanisms as ultimately unobservable physical, social, or psychological processes through which agents with causal capacities operate, but only in specific contexts or conditions, to transfer energy, information, or

matter to other entities” (George and Bennett 2005: 137–138). Furthermore, they stressed that explanations via causal mechanisms have affinities to a scientific realist epistemology—which basically means that it is assumed that the reality exists independent from the observer; that it implies a commitment to micro-foundations—fundamental assumptions about the behavior of actors which form the basis for most basic social science theories; and that explanations via causal mechanisms draw on spatial contiguity and temporal succession (George and Bennett 2005: 137–145). The latter defining characteristic got lost in the more recent—single-authored—work by Bennett (2008) where he started to connect process tracing to van Evera’s typology of tests for deduced hypotheses and to Bayesian up-dating, which paved the way for a very different understanding of process tracing (e.g., Collier 2011, Beach and Pedersen 2013).

We have nothing against a deductive approach to qualitative and case study research and see also an important role for Bayesian thinking within such an approach (see Blatter and Haverland 2012: 167, 177, 198–202, and page 194 for a revealing example of what a difference Bayesian thinking makes in drawing conclusions from case studies). But it is unfortunate to see these elements as characteristic for process tracing, because it leads to the crowding out of exactly those epistemological and methodological fundamentals that make (causal-) process tracing a highly valuable and indispensable complement to more deductive research approaches (Blatter 2012).

In our book and in various contributions, we make the case for the recognition of two distinct alternatives to the statistical worldview and the corresponding co-variational (COV) approach to case studies: the “causal process tracing approach (CPT)” and the “congruence analysis approach (CON)” (Blatter and Blume 2008, Blatter and Haverland 2012). The latter has been stimulated by George and Bennett’s “congruence method” (George and Bennett 2005: 181–204), but goes much further in spelling out a theories-driven (plural!) case study approach which is based on a systematic comparison between the correspondence of a plurality of expectations that we can derive from one abstract theory (defined as a specification of a coherent worldview) with clusters of empirical evidence and the correspondence of another set of expectations that we derive from another theory with the relevant set of empirical evidence (Blatter and Haverland 2012: 144–204). We understand and describe causal-process tracing (CPT) as a separate, full-fledged and consistent research approach that combines “configurational thinking” (Ragin 2008: 109–114) with a scientific realist ontology and epistemology. The latter means that we reemphasize that the spatio-temporal continuity and contiguity of social processes serves an important “natural basis” for drawing causal inferences (George and Bennett 2005: 133–145).

The difference that it makes when we add this scientific realist underpinning of within-case analysis to the formal logic of set-theory shows up when we look at how we in contrast to Goertz and Mahoney, describe Henry Brady’s famous analysis of the effect of the early media call that proclaimed an Al Gore victory in Florida in the 2000 U.S. presidential election (Brady

2004). Brady shows that causal-process tracing based on causal-process observations can provide more convincing estimations of the electoral consequences than statistical analysis based on data set observations (he showed that Bush might have lost between 28 and 224 votes and not 10,000 as the statistical analysis implied). In our book (Blatter and Haverland 2012: 124–127) we point to this example in order to argue that methodologists with a strong affinity to co-variational thinking do not really recognize the value and epistemological foundation of causal-process tracing. We argue that Brady’s observations are anything but “isolated observations” (Gerring 2007: 177). Like Goertz and Mahoney, we show that Brady’s observations provide information for calculating how many people had fulfilled those conditions that had been necessary and together sufficient to be swayed by the media. We formulate the logic behind Brady’s observations only in terms of necessary and sufficient conditions, whereas Goertz and Mahoney use the terminology and the logical notations of set theory and argue that Brady carries out a series of “hoop tests.” We count five necessary conditions (Blatter and Haverland 2012: 126), whereas Goertz and Mahoney point to three (p. 55) or four conditions (p. 93). But the core differences are (a) that we highlight the importance of time for Brady’s calculation and (b) that we show that Brady’s argumentation is so convincing because he provides all the information that makes a mechanism-based explanation complete.

At this place we cannot address the second aspect since we would have to delve deep into the discussion on useful understandings of “causal mechanisms.” Goertz and Mahoney avoid this debate in their book and we advocate an understanding that aims to connect mechanism-based research to a cumulative process of generic theory building in the social sciences (Blatter and Haverland 2012: 95–97). But we want to highlight that in Brady’s calculation the temporal fact that the polls were open just ten more minutes when the media call happened plays the crucial role. He introduces a cluster of diverse observations in order to calculate the number of people in the Panhandle counties who had not yet voted but intended to vote. With the help of these observations, he could reduce the number of people who possibly could have been influenced to 4,200. Using temporal sequences as a cornerstone for drawing causal inferences within a causal process tracing approach fits the asymmetrically deterministic logic of set theory (it is not just not probable but outright impossible that people who voted before the media call could have been swayed). Nevertheless, we should not reduce the former to the latter when we do not want to lose the “scientific realist” insight that “causation is a relation in nature, not in logic” (Wendt 1999: 81). We think that logic/set theory is a helpful tool for drawing causal inferences through within-case analysis, but it is not the core epistemological basis.

We want to end with some general remarks. It is certainly useful for the development and for the promotion of logic and set theory in the social sciences to compare its fundamentals and applications with similar tools in statistics. Nevertheless, the resulting dualism cannot capture the epistemological and methodological diversity on which qualitative and case study

research draws. Such a dualism has strange consequences, e.g., when it forces Goertz and Mahoney to call the combination of a statistical analysis with process tracing an example of multi-method research whereas the combination of QCA and process tracing is seen as purely qualitative research (pp. 106–109). Furthermore, such an approach runs the risk that methodological discourse gets self-referential; the authors might be able to convince quantitative methodologists that research based on logic and set theory should be taken seriously; but that valuable goal might disconnect them from the real praxis of case study research. Goertz and Mahoney (p. 9) point to the fact that in statistical research the model that has gained the most prominent position in the methodological discourse has only a limited influence on the praxis. As far as we can see (and we include but do not limit our view to US-American journals), the same is true for qualitative and case study research: the works they refer to (e.g. Brady and Collier 2004) have gained a dominant position in the Anglo-Saxon methodological discourse, but they certainly do not cover the diverse praxis (especially if one looks beyond the field of comparative politics/studies). Finally, the concentration on alternative mathematical foundations helps to provide a common thread through a book that covers many topics. But this might be a little bit too essentialist and parsimonious for distinguishing two research cultures. In our book, we use a much broader set of elements to characterize a coherent research approach: specific kinds of research goals/questions, distinct ways to select cases, diverse techniques of data generation and data analysis as well as alternative directions for drawing conclusions beyond the cases under investigation. These elements should be aligned into a coherent research approach, but they cannot be reduced to a single meta-principle.

Overall, we believe that *A Tale of Two Cultures* will help scholars who do qualitative and case study research to be more reflective and explicit about their methodological approach. With its focus on logic and set theory it leaves room for other books to do the same by introducing or explicating further methodological approaches and tools. Within this joint endeavor, the main contributions of our book are (a) to highlight the existing diversity in case study methodology and praxis, and (b) to point to the many advantages that we gain in terms of internal coherence and external complementarity when we complement the classic co-variational approach, which builds on cross-case analysis with two distinct approaches to within-case analysis. As coherent research approaches, causal-process tracing and congruence analysis do not only differ in respect to their epistemological presuppositions and to their techniques for drawing causal inferences, but also in respect to the kind of questions they help us to answer and to the ways we draw generalizing conclusions beyond the cases under study.

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A Tale of Four Books, or: Principles and Practice of Social Science Methods

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The field of qualitative methods has experienced a remarkable degree of development over the past 20 years. The five books that are under review in this symposium bear witness to this process. In my contribution on these books and *A Tale of Two Cultures* in particular (henceforth referred to as *Two Cultures*), I choose to focus on the distinction between the *practice* and *principles* of social methods and its role in the books under review here. As I also hope to show in the following sections, there are multiple reasons that I find it crucial to clearly distinguish between principles and practice. Quite often, people criticize the application of methods in empirical research and then directly jump to the inference that the method is deficient. This is, of course, to be avoided because a method cannot be blamed for how it is applied.

For empirical researchers seeking methodological guidelines, it is equally important to understand whether they have read something about methodological principles or practice. It is safe to argue that the community develops certain routines in the application of methods that might result, for instance, in a qualitative and quantitative culture. Information about these routines is valuable because it allows the discipline to reflect about its state of affairs and identify bad and good practices. However, the development of a culture entails that some of the ways in which a method can be done are rarely pursued, if at all. For instance, qualitative methodologists repeatedly emphasize that hypothesis tests via case studies can invoke most-likely cases (e.g., page 183 in *Two Cultures*). The conventional reading is that if the test fails, one has cast strong doubt on the hypothesis because it should have mastered the test. It is true that quantitative research often relies on random or convenience samples and that one does not know how a passed or failed test affects our confidence in a hypothesis. However, this is only a matter of practice and there are instances of quantitative studies that explicitly draw on most-likely reasoning in justifying sample selection (Levy 2007).¹

In the following sections, I highlight the importance of the distinction between principles and practice by raising some thoughts on how these two issues figure in the four books. This is a useful exercise in order to avoid the erroneous equation of arguments on practice with principled claims and to highlight where each of the books stands on the practice vs. principle dimension. I first discuss two *Two Cultures* (2012) by Goertz and Mahoney (GM), which is clearly concerned with the practice of social science methods. I then proceed with *Explanatory Approaches* (2012) by Blatter and Haverland (BH), followed by *Set-Theoretic Methods* by Schneider and Wage-mann (SW, 2012), and *Process-Tracing Methods* by Beach and Pedersen (BP, 2013).

Two Cultures: The Practice of Qualitative and Quantitative Methods...Or Not?

Two Cultures is an impressive extension of GM's widely well-received *Political Analysis* article from 2006. The book extends the number of dimensions on which the quantitative and qualitative cultures are compared and discusses each dimension in much detail. It does not take much to argue that their book will leave a significant imprint on the field, and this rightly so because GM offer the first comprehensive and comparative discussion of quantitative and qualitative methods. However, I have a few concerns about GM's message in *Two Cultures* and the meaning that readers might get from it. With regard to the latter point, one should understand that *Two Cultures* concerns the way in which GM perceive the common practice of qualitative and quantitative research. GM are clear about this, but it is worth emphasizing because the way qualitative and quantitative research can be done is much broader than the two allegedly existing cultures.

In order to understand why I say "allegedly existing cultures," I have to elaborate my preferred view of *Two Cultures*: it is a descriptive empirical study of how social science methods are practiced. GM distinguish multiple dimensions on which one can describe an empirical study such as the breadth of concepts (thin vs. thick), the nature of utilized evidence and so on. They then develop the overarching hypothesis that all these dimensions load on a single qualitative-quantitative dimension, i.e., one dimension suffices to sort how social science methods are practiced. Given the diversity of ways in which quantitative and qualitative research can be practiced, this is an interesting and strong hypothesis. Unfortunately, *Two Cultures* stops at the formulation of the hypothesis and confines itself to the presentation of illustrative evidence. Although *Two Cultures* offers many insightful arguments and presents illuminating examples, the claim that a single dimension characterizes the field of social science methods remains untested. In terms of our knowledge about the landscape of social science methods, at the end of *Two Cultures*, we are more or less at the point where we started.

The next step one should take, either GM or anyone else, is to review selected parts of the empirical literature, gather the required data, and test whether the dimensions distinguished by GM indeed load on one dimension (the appendix in *Two Cultures* operates on a more abstract level and does not achieve this). Absent such a project, my hunch is that *Two Cultures* will be a mind-reinforcing book, but not a mind-changer. Readers who shared the perception of social science methods with GM will still do so after having read *Two Cultures*. People who already disagreed with the message of the *Two Cultures* article will also continue to do so and point to illustrative examples that are in discord with GM.

The absence of an empirical analysis of the methodological landscape also makes it difficult, in my eyes at least, to engage with the book on a substantive level. GM admit that their distinction of two cultures is very broad and that there are subcultures within each culture. For every argument that one makes against one of the claims in *Two Cultures*, proponents of the two-cultures argument can point to a subculture

that does not exactly fit with the root culture, but nevertheless belongs to it.

Nevertheless, I have an opportunity here: I find it difficult to follow the subculture argument in every respect. For instance, it would be interesting to know whether researchers applying Bayesian statistics are really happy with being dubbed a subculture of frequentist research that GM mainly have in mind when talking about the quantitative culture. Similarly, I have serious concerns with subsuming QCA under the qualitative culture. QCA certainly shares some characteristics with the latter, e.g., the focus on set relations. However, if one takes QCA as a method as opposed to an approach (Schneider and Wagemann 2012, chap. 1) and examines hundreds or thousands of cases (Cooper and Glaesser 2011), it is likely to share characteristics with the quantitative culture such as reliance on thin concepts. The fact that QCA as a method does not fit squarely is one reason for my believing that the large-n vs. small-n dimension (n being the number of cases) is also needed for properly mapping social science methods. Unless a thorough review of the practice of qualitative and quantitative research is done, we neither know if my hunch is true, nor whether there is a qualitative vs. quantitative dimension à la *Two Cultures* in the first place.

Explanatory Approaches: A Broad Philosophical and Narrow Covariational View

BH claim to present three coherent approaches—covariance, process tracing, congruence approaches—that are attributable to different epistemologies—empiricism/positivism and critical rationalism, constructivism/conventionalism and critical theory, and pragmatism/naturalism and critical realism. The main message is a plea for an “anti-fundamentalist and pluralist” epistemology and a middle ground that tears down the walls between the three approaches (section 1.3.4). The argument for a pluralist view on philosophy of science and case studies is well taken. As for example, Hall (2003) and Hay (2006) vividly described it; ontology and methodology should match each other and different ontologies demand a different (case study) methodology and method. It would be nice if a plea for pluralism would mean beating a dead horse representing pleas for a unified ontology, methodology, and epistemology. In the face of calls for a unified—ologies (e.g., Gerring 2012), however, this is not the case at present. What I am less convinced of is BH’s idea of an epistemological middle ground, as the philosophies of science that underlie the three approaches are inherently incompatible in many respects (see Jackson 2010). Regrettably, the notion of an epistemological middle ground remains underdeveloped (also in chapter 5), though this is a central argument that deserved more elaboration.

While I concur with BH’s call for pluralism, I have concerns with regard to the presentation of their epistemologies (which might be better called philosophies of science) and the covariational approach in particular. I am not a philosopher of science, but the short discussion of epistemologies underlying the three approaches is not overly convincing. Philosophies that generally are kept separate—e.g., pragmatism and

critical realism—are lumped together and in turn tied to one case study approach. For example, this contrasts with Jackson’s (2010) recent and, in my view, more reflected distinction of four philosophies of science and associated methodologies and methods that distinguishes between neopositivism, critical realism, analyticism, and reflexivity.

Leaving the fundamental philosophical issues aside, I have difficulties with seeing the three approaches as coherent, in particular, the depiction of the covariational approach. For instance, the covariance approach is described as focusing on the link between a single independent variable and the dependent variable. The process-tracing approach is characterized by a focus on temporality and conjunctural causality. What happens now when I test a covariational hypothesis involving a conjunction, i.e., an interaction effect? Or when I do a case study that is focusing on temporality and the individual sufficiency of mechanisms? Are these covariance case studies or process-tracing case studies? The congruence approach is defined by the evaluation of the relative strengths of competing theories. This might characterize the congruence approach, but this can also be done within a covariance approach by invoking Bayes’ theorem, which is a non-issue in BH’s book (and largely ignored in *Two Cultures*), but is a central topic in BP’s and my book. This is not to say that the covariational approach (or neopositivist approach more generally) is superior to the other two, because the appropriateness of the methodology and method hinges on the underlying ontology. However, the misrepresentation of the covariational approach in these and other respects might give the reader of *Explanatory Approaches* an incorrect picture of what you can and cannot achieve with it.

Set-Theoretic Methods: Principles of QCA by Example

Set-Theoretic Methods by SW and *Process-Tracing Methods* by BP are clearly discussing principles of methods as opposed to practice. SW’s *Set-Theoretic Methods* is a long-awaited book on good standards in QCA. Charles Ragin’s contribution to the development and spread of QCA in the social sciences cannot be overestimated. From the viewpoint of empirical researchers and teachers of QCA, though, the gradual development of this technique comes at a cost in that there is no single reference offering insights on all elements of QCA. *Set-Theoretic Methods* fills this gap and, additionally, advances QCA in multiple directions. The only issue I have with the methodological arguments is that they often work by example. Of course, this is better than reasoning void of any empirical illustration. However, it would be even better to know the general conditions under which a methodological problem occurs. Such guidelines are lacking, making it difficult for empirical researchers to determine whether a specific problem pertains to their analysis.

Process-Tracing Methods: Two Strong Principles with Underconsidered Alternatives

Process-Tracing Methods by BP is very explicit about its philosophical anchorage and its goal of introducing principles of process tracing. Much like *Set-Theoretic Methods*, *Pro-*

Process-Tracing Methods has been long overdue. Process tracing and causal mechanisms are a growth industry on the methodological and applied side of qualitative research, but a coherent discussion reflecting the most recent developments has so far been lacking. BP take a clear position in three respects and I want to briefly elaborate on each of them. First, BP borrow the ontological conception of causal mechanisms from Machamer, Darden, and Craver (2000). Their distinction between entities that undertake an activity which, in turn, prompts another entity to perform an activity is central to discussions about mechanisms in biology (Machamer et al. invented the definition for the field of biology) and philosophy of science more generally. My point is not so much that BP follow Machamer's et al. conception, but that they do this a little bit over-hastily. In light of the growing body of literature on mechanisms, I see an increasing need to give a balanced discussion of the conceptions of mechanisms that are on offer, in particular when one takes a clear stand in support of one conception. In this view, I find it particularly surprising how quickly GM and BH rush to the discussion of causal mechanisms in *Two Cultures and Explanatory Approaches* and jump to the adoption of one specific definition. In contrast, BP offer a more reflected read of the state of the debate about mechanisms.

Still, I have some reservations as regards BP's preferred conception. For example, it is surprising that Glennan's (2002) conception of mechanisms is favorably referred to in BP's discussion of mechanisms. The notion of "invariant, change-relating generalizations" is central for Glennan and entails two elements that stand in direct contrast to BP's account of mechanisms. The change-relating relation takes place between two intervening variables, a concept flatly rejected by BP as incompatible with their idea of a mechanism (incorrectly, I think). Moreover, Glennan requires that mechanisms are regular, while BP also elaborate on case-specific mechanisms that do not travel beyond the examined case. The latter point is also in discord with Machamer's et al., who share with Glennan the premise that mechanisms are regular. Part of the problems that I see with BP's discussion of mechanisms is that they import core ideas from the natural sciences. As Machamer et al. themselves point out, a conception of mechanism that does work for biology need not be appropriate for other scientific disciplines. Unfortunately, BP do not reflect on the overarching question as to what extent a mechanistic conception of biology, where organisms exhibit regularities in their functioning, can be extended to the social sciences.

The second general commitment of BP pertains Bayesian causal reasoning. BP make a strong case for Bayesianism that I find wanting for two reasons. First, their rejection of frequentism as a viable form of causal reasoning in process tracing is wrong (section 5.2 in *Process-Tracing Methods*). The fallacy rests on confusing principles of causal reasoning with its practice. BP link frequentism to mainstream quantitative research, as many others have done before. This perspective ignores that frequentism, much as Bayesianism, represents a general logic of causal reasoning that holds independently of any particular method. (While I cannot go into the details of this argument here, I invite the reader to take a close

look at chapters 1 and 8 of *Case Studies and Causal Inference* [2012] in which I deal with this topic in detail.) To avoid misunderstandings, my point is not to argue in favor of either frequentist or Bayesian reasoning. Instead, case study researchers should be aware that both types of reasoning are available in small-n research and that they entail different implications for important elements of case studies such as case selection (see chapter 3 in *Case Studies and Causal Inference*).

The second point in which I find the discussion of Bayesianism wanting concerns the one-sided discussion of its philosophical and epistemological foundations. BH make a plea for subjective Bayesianism, meaning that there are no constraints on the probabilities a researcher assigns to a hypothesis or the expectation of gathering specific evidence given a hypothesis. Within the field of Bayesianism, the alternative is objective Bayesianism based on the premise that there are rational constraints on probabilities. This is an ongoing discussion in the Bayesian literature (Williamson 2010), but does not find resonance in *Process-Tracing Methods*. Similarly, philosophy of science currently witnesses debates about frequentism and Bayesianism. Deborah Mayo (e.g., 2010) rejects Bayesianism and is a major proponent of frequentism for principled reasons, a position not reflected on in BP. Certainly, the debates about subjective vs. objective Bayesianism and Bayesianism vs. frequentism are involved discussions in philosophy of science, but I think one can hardly avoid them if one wants to make a strong argument in favor of subjective Bayesianism.

All in all, the five books reflect the richness of qualitative methods and should appeal to a broad audience. They also offer the basis for the advancement of the field in multiple directions and it will be very interesting to see what the next developments will be.

Notes

¹ Goertz and Mahoney explain that they focus on common practices because techniques rarely transfer from the quantitative to the qualitative domain and vice versa. This might be because there are too few discussions and examples for such transfer, implying that current common practices reproduce themselves.

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The Third Wave of Qualitative Methodology

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Qualitative methods in political science is in the midst of a third wave of research. The first wave consisted of works on case study and comparative methodology from the late 1960s and 1970s (e.g., Smelser 1967; Verba 1967; Przeworski and Teune 1970; Sartori 1970; Lijphart 1971; Campbell 1975; Eckstein 1975; George 1979). Another outpouring of work occurred in the aftermath of the publication of King, Keohane, and Verba’s *Designing Social Inquiry* (1994). Reactions to that work helped to spur a second wave of qualitative methodology that includes Mahoney and Rueschemeyer (2003), Brady and Collier (2004), George and Bennett (2005), Goertz (2005), Gerring (2007). Now, a new surge of publications on qualitative methods is taking place, as witnessed by this symposium of five new books (which are only a part of many new publications on qualitative methods). Yet it remains an open question whether this new work represents an amplification of themes from the second

wave or a departure toward new issues.

One theme that runs across all of this work—old and new—has been that authors have to define, implicitly or explicitly, their position vis-à-vis statistics as an approach to research design and analysis. Reactions to statistical work have varied quite a lot, including the question of its compatibility with qualitative research. In our book, *A Tale of Two Cultures: Qualitative and Quantitative Research in the Social Sciences*, we present a view of qualitative methods that stresses its identity as separate from and independent of quantitative research. We suggest that many qualitative methods can be used without recourse to statistics. And we provide a justification for qualitative methods that does not draw on statistical theory.

Our book is about contrasting quantitative with qualitative research, and as such it focuses centrally on differences in methodological practice. We adopt a pluralist approach in which there is an important place for qualitative research, quantitative research, and various kinds of mixed-method research. We reject a “monist” position in which all research can be best understood in terms of statistical theory (see Elman 2013).

For the purposes of this symposium, which focuses on qualitative research, we propose a dialogue *within* the qualitative culture. This within-culture focus is also the perspective adopted by the other authors. For example, when Derek Beach and Rasmus Brun Pedersen describe us as “monists,” they are not suggesting that we embrace a single template to all research. They are asserting that we do not pay enough attention to differences within the qualitative culture (i.e., to qualitative subcultures). In particular, they argue that our discussion of process tracing focuses on only one set of tools associated with this method, overlooking other key elements of the method.

In their book, *Process-Tracing Methods: Foundations and Guidelines* (2013), Beach and Pedersen distinguish three uses of process-tracing methods. Each type is linked to the analysis of a causal mechanism, which is a central concept for their understanding and definition of process tracing. *Theory-testing process tracing* explores whether a causal mechanism functions as specified in advance. *Theory-building process tracing* tries to identify the mechanism between X and Y. Finally, *explaining outcome process tracing* seeks to develop a mechanistic explanation for a particular outcome. In our book, we focused mainly on theory-testing process tracing in which the goal is to make a causal inference. Hence, we looked closely at process tracing tests, such as hoop tests and smoking gun tests. These tests are not exactly how Beach and Pedersen characterize theory-testing process tracing, because they link process tracing so closely to the study of mechanisms.

We see Beach and Pedersen’s explaining outcome process tracing as normally requiring theory-testing process tracing and often drawing on theory-building process tracing as well. To identify the causes of a specific outcome, one normally must engage in theory-testing process tracing. For example, well-done narrative explanations of outcomes can be recast in terms of process-tracing tests. Thus, we would prefer to see outcome explanation as one potential goal for which a

researcher might use theory-building and theory-testing process tracing.

Beach and Pedersen's concerns about the monist nature of our work stems naturally from their effort to differentiate varieties of process tracing. If one presents an entire book on process tracing, it is not surprising that one might view our short discussion of process tracing as incomplete. Yet, for our argument, distinguishing different kinds of process tracing was not critical. The key point we needed to make was that process tracing is very important in qualitative research and far less important in quantitative research. If this point seems obvious to qualitative researchers, that simply underscores the salience of the contrast. Because our audience includes both qualitative and quantitative researchers, however, this key point may not always be obvious, and we risked obscuring it with a complex discussion of the varieties of process tracing. Discussing kinds of process tracing is an important topic, but it is an issue *within* the qualitative culture.

Set-Theoretic Methods for the Social Sciences: A Guide to Qualitative Comparative Analysis (2012) by Carsten Q. Schneider and Claudius Wagemann offers the most up to date and comprehensive discussion of set-theoretic methods in the social sciences. Several features of the book make it stand out from existing publications. First, the authors focus on set-theoretic methods as a large family of methods. These methods include Millian matching methods (e.g., the method of agreement), typological theory, methods of sequence elaboration, as well as crisp and fuzzy-set QCA. The authors thus use and integrate recent writings on set-theoretic methods that fall outside of the formal QCA tradition. As a result, this book casts a wide net while still focusing squarely on set-theoretic methods.

A second notable feature of the book is that Schneider and Wagemann avoid invidious comparison with other methods, such as regression analysis. While at various stages they compare set-theoretic methods to regression methods, the purpose is to clarify their presentation of set-theoretic methods, not to suggest that set-theoretic methods are inherently superior. We see the comparisons they make between set-theoretic methods and mainstream quantitative methods as following the spirit of our two cultures argument.

Third, the authors do an admirable job of presenting their ideas in a way that is clear but sophisticated. They cover both the basics and more complex issues. They present many useful illustrations and helpful textboxes. The presentation is well organized, and readers of all levels can gain from engaging this material.

In their commentary on our book, Schneider and Wagemann raise some questions about our claim that logic and set theory are the mathematical tools that underpin qualitative methods. As they note, many qualitative scholars may not see this description as applying to them. Many qualitative methodologists argue that statistical theory—not logic and set theory—provides the mathematical foundation for both qualitative and quantitative research. This important group represents the true monists in the qualitative tradition. We believe that the integration of qualitative research with statis-

tics is a valuable undertaking. Certainly, many researchers want to use both statistics and case studies, and it is important to think about how to do this well. However, the justification of qualitative methods on the basis of statistical theory is not our approach.

Schneider and Wagemann agree with us that logic and set theory are at the heart of many qualitative methods. They note that some qualitative methods, such as rules for data collection, may not be best characterized using logic and set theory. Our point, however, is that set theory and logic are at the core of qualitative methods, including Mill's methods of agreement and difference, process-tracing tests, typological theory, counterfactual analysis, and Qualitative Comparative Analysis (QCA). We believe that Schneider and Wagemann agree with us on this matter.

We do have some real debates with Schneider and Wagemann *within* the QCA culture. They argue that we do not acknowledge that researchers establish qualitative differences between cases by using what they regard as the critically important 0.50 crossover point in fuzzy set analysis. They see this point as the dividing line for "qualitative differences" among cases. Thus, for them, a case of 0.49 is very different from a case of 0.51.

But we remain skeptical of the idea of that the 0.50 value reflects a qualitative difference rather than a slight difference in degree of membership. To be sure, fuzzy logic does not assume that all differences are equal: there are regions in the data that have the same semantic meaning. But crossing the 0.50 membership point does not constitute a "qualitative" difference nor a "difference in kind." For example, it is awkward and misleading to assert that a case with .51 membership in the category of democracy is a democracy. It is more appropriate and informative to say that the case has slightly more membership in democracy than not democracy but is still quite borderline. Fuzzy-set analysis is designed precisely to be able to recognize these kinds of nuances.

In their book *Designing Case Studies: Explanatory Approaches in Small-N Research* (2012), Joachim Blatter and Markus Haverland argue that case study research is characterized by three distinct approaches. They label one approach covariational (COV), which has obvious parallels to statistical analysis. The second approach is called causal-process tracing (CPT), which is clearly rooted in the qualitative tradition. We see substantial overlaps between these two approaches and many of the distinctions we make in the book. Thus, for example, their COV approach fits well with cross-case work that seeks to estimate the average effect of variables, whereas their CPT approach corresponds to within-case work that seeks to explain outcomes using configurations of variables.

In their discussion of the COV approach, Blatter and Haverland do a nice job of linking this analysis to the experimental template. They also nicely contrast experimental control versus observational control, probabilistic versus deterministic causation, and individual variable effects versus configurational causality. They are careful to avoid conflating set-theoretic methods such as Mill's methods of agreement and difference with the COV approach.

It is interesting to compare Blatter and Haverland's discussion of process tracing with that of Beach and Pedersen. Unlike Beach and Pedersen, Blatter and Haverland focus almost exclusively on theory-testing modes of process tracing (not theory formation). They emphasize four different goals: outcome explanation, evaluating the effect of a cause, identifying the preconditions for an outcome, and identifying causal mechanisms. Thus, they have an even more fine grained set of distinctions than Beach and Pedersen. Like Beach and Pedersen, Blatter and Haverland express concerns that our analysis does not attend enough to the variety of approaches that characterize qualitative research. From our perspective, however, the interesting point to note is that these distinctions are meaningful within the qualitative culture where the focus is on specific outcomes, within-case analysis, and identifying causal mechanisms.

Blatter and Haverland's third approach to case studies is what they call "congruence" (CON). CON involves using case studies to evaluate the relative strengths and weaknesses of two or more theoretical approaches. We feel that this is something that in fact occurs in well-done case studies of the COV or CPT types as well. We think that one purpose of a case study is to illuminate theoretical debates. We see the CON approach as normally requiring COV or CPT in order to do its work. Thus, in substantive research, one arrives at an evaluation about the strengths and weaknesses of alternative theories using covarational analysis and/or process tracing.

Blatter and Haverland raise interesting questions about the distinction between data-set observations and causal-process observations, which was formulated by Collier, Brady, and Seawright (2004) and is used in our book. Blatter and Haverland develop the alternative concepts of "variable-scoring observation" and "process-tracing observation" (pp. 20–23). We agree that more discussion is needed concerning the kind of observations used in qualitative versus quantitative research. Nevertheless, all parties in this debate seem to concur that the kinds of data that are gathered and used in qualitative and quantitative research are different.

Finally, Blatter and Haverland make interesting connections between qualitative methodology and work in the philosophy of science. We found especially important their argument for a "middle ground" that rejects strong monist positions and embraces methodological pluralism. As they stress, different research goals require different methods. One of the key points of our book is that the divergent goals of qualitative and quantitative research yield many downstream contrasts.

In *Case Studies and Causal Inference: An Integrative Framework* (2012), Ingo Rohlfing offers a new comprehensive treatment of the case study method. This book is an important advance in the multimethod research area, a domain in which many students want to write dissertations. This book can be quite usefully compared to John Gerring's *Case Study Research* (2007), which defines the current state of the art. Whereas Gerring grounds his argument thoroughly in the principles of statistical theory, Rohlfing relies heavily on both statistical theory and logic/set theory to make sense of case study meth-

odology. Students who seek to appreciate differences within the multimethod research community would do well to read these two books side by side.

Nevertheless, if one has objections to Gerring's approach, some of those concerns will carry over to Rohlfing. For example, he believes that: "A case study starts with a specification of a population of cases for which a causal relationship is expected to hold and to which causal inferences are generalized after the empirical analysis" (p. 203). He also follows a long tradition in discussing types of case studies in light of statistical results; for case selection, these results help to identify typical cases, deviate cases, extreme cases, and so on. Hence, much of his discussion of multimethod research has quantitative analysis in the leading role and the case study cast as a supporting actor.

An alternative perspective is suggested by the Beach/Pedersen and Blatter/Haverland books. These authors place the focus of case studies more squarely on the analysis of causal mechanisms. For them, case selection is motivated by the effort to identify and analyze causal mechanisms. Here the qualitative analysis is in the driver's seat, with process tracing possibly standing alone as a method of inference. We would encourage a broader discussion among all of these authors about case selection practices.

Like Schneider and Wagemann, Rohlfing treats set theory separately from statistical theory. We believe that there are many unexplored tensions and complementarities between statistical and set-theoretic approaches. An important aspect of the third wave of qualitative methodology will involve sorting out this relationship more completely.

The relationship of Bayesian statistics to the set-theoretic approach also raises important questions. For example, while Rohlfing's chapter 8 starts with process-tracing tests, the majority of the chapter is on Bayesian reasoning. Usually when one thinks about using Bayes theorem, one imagines having multiple observations that gradually strengthen (or weaken) a conclusion. Here the connection between hoop tests and Bayesian reasoning would involve the fact that passing hoop tests gradually increases one's confidence in a hypothesis. Failing a hoop test eliminates a hypothesis, and this element of process tracing may be less congruent with Bayesian reasoning.

In his commentary, Rohlfing suggests that the differences that we discuss between qualitative and quantitative may not be correlated with one another. In fact, however, we think the empirical evidence points in the opposite direction (e.g., Mahoney and Terrie 2008). More generally, we find it difficult to believe that Rohlfing does not really think that many if not all of the dimensions that we discuss are intimately related given what he argues in his book.

In summary, we see all of the books in this symposium as complementary. We think that our *A Tale of Two Cultures* can help situate the other books by placing their methods within the larger context of the qualitative–quantitative divide. At the same time, we think these other books are part of what will surely become an even larger third wave of research on qualitative methodology.

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Response to Commentaries

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We would like to respond to several of comments made by the other contributions to this symposium. First, and most generally, we would like to state that our book on process-tracing (PT) was heavily inspired by George and Bennett's chapter on PT (2005). In many respects our book can be understood as our take on what the result would have been if George and Bennett, instead of only using a chapter, had the luxury of devoting a book-length manuscript to developing PT, where they would have had more space to both develop the ontological and epistemological foundations of PT, and develop practical guidelines for its use.

We therefore are quite surprised that some qualitative scholars do not view our take on PT as "mainstream," especially our focus on tracing causal mechanisms. However, we have a difficulty seeing what PT, understood as a social science method, would be tracing unless it is explicitly tracing causal mechanisms. If we are not tracing a theorized mechanism, what then is the "process" between X and Y? If we are merely tracing events that are not the manifestations of an underlying mechanism (a theory), then what we are engaging in is a narrative of events that "just happened" between the occurrence of X and Y. While relevant for making descriptive inferences, we can make no causal claims based upon this type of narrative story. Therefore, if we are engaging in theory-guided social science with ambitions to make causal claims, we should be tracing the theorized process between a cause and an outcome—which is exactly what a causal mechanism is. This is why PT is widely defined in the methodological literature as a case study method for tracing causal mechanisms (Bennett 2008b; Bennett and Checkel forthcoming; Mahoney 2012; Waldner 2012).

Where we do diverge slightly from recent interpretations of PT is that we make explicit the distinction that can be found within qualitative case-study research regarding whether our focus is on generalizable theorization or is case-centric. This distinction captures a core ontological and epistemological divide with the social sciences, where we find on the theory-centric side both neo-positivist and critical realist positions, and analyticism and pragmatism on the case-centric side (Jackson, 2011). Case-centric scholars operate with a very different understanding of the social world, viewing it as very complex and extremely context-specific, making generalizations become difficult, if not impossible. Therefore the ambition for PT in this understanding becomes to account for particularly puzzling and historically important outcomes. PT case studies focus on

explaining a particular historical outcome, for example by working backward from the known outcome to uncover the causal mechanisms that can account for the outcome. Outcomes here are not understood to be a “case of” some theoretical concept (e.g. a war), but instead are understood in a much more inclusive, holistic fashion as the Cuban Missile Crisis, or World War I. The shared core of case-centric PT is well expressed by Evans, who writes, “Cases are always too complicated to vindicate a single theory, so scholars who work in this tradition are likely to draw on a mélange of theoretical traditions in hopes of gaining greater purchase on the cases they care about.” (1995: 4). Given these large differences in how theories are used and the analytical goals of research across this ontological and epistemological divide, we chose to make these differences clear in our book, at the same time acknowledging as suggested by Goertz and Mahoney that one can proceed in more testing or building modes of analysis when engaging in theory-centric PT.

Returning to the question of mechanisms, Rohlfing objects to our understanding of mechanisms as being invariant systems. Here we would like to clear up several misunderstandings. Machamer does not claim that mechanisms need to be regular; indeed he explicitly states that they do not need to be regular in the first endnote in his 2004 article. Further, our understanding of mechanisms is not just a curious idea imported from the natural sciences, but is an understanding widely used by social scientist scholars who take the study of mechanisms seriously, and in particular the idea that mechanisms are *systems* that cannot necessarily be reduced to their individual components (e.g. George and Bennett 2005: 222; Bennett 2008b; Waldner 2012). Beyond this systems-orientation we are inherently pragmatic about the nature of mechanisms in our book, and believe it should be our research questions and the nature of our theories that drive our methodological choices about factors such as the level of analysis we choose when conceptualizing mechanisms (macro, micro, or a mix).

Finally, we question whether frequentist reasoning can be appropriate when engaging in within-case analysis using process-tracing. To make an inference based on frequentist reasoning requires variation, which we per definition do not have when we are tracing a mechanism within a case using an invariant, single case design. This being said, numbers and patterns can be relevant to make inferences depending upon what type of empirical predictions we are making when testing whether a part of a mechanism is present or not. If part of our mechanism hypothesizes that an international institution had privileged information relative to governments in a given negotiation, one way we could test this would be to put forward the prediction that we should find that the institution had more study papers in key issues than governments, and that the content would be longer in the institution’s papers. However, the actual inference would still be made using Bayesian reasoning, and in particular by evaluating the likelihood ratio where we would evaluate how likely it is we would find this pattern in the evidence in light of what we know about the case ($p(e|h)$) in relation to the likelihood of finding this evidence if any other plausible explanation was valid ($p(e|\sim h)$). Bayesian reasoning

gives us a helpful language for describing the research process where we update our confidence in the validity of a given theory based on the strength of empirical tests that we are able to deploy.

However, as qualitative scholars we do not believe that we possess the type of data that would enable us to make “objective” predictions regarding prior probabilities and likelihood ratios that quantitative Bayesian statisticians can make, which is why we chose not to engage with this literature—especially given that our audience is qualitative case study scholars and not quantitative statisticians. Instead, we refer our readers to discussions about Bayesian reasoning and subjectivity to Howson and Urbach (2006: 265–272) and Chalmers (1999: 177–192). Here we pragmatically suggested, as many social scientists before us, that Bayesian reasoning focuses our attention on what elements our tests need to include that we need to evaluate qualitatively in order to make as strong causal inferences as possible. This is why many process-tracing methodologists either explicitly (Bennett 2008a) or implicitly (Mahoney 2012) refer to Bayesian reasoning.

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*Fuzzy Sets are Sets—
A Reply to Goertz and Mahoney*

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The Common Ground

As stated in our first comments, we share G&M's vision of qualitative research as being rooted in set theory. Precisely because we think that this is a plausible proposition with potentially even more fruitful and intriguing implications than mentioned in G&M's book, we expressed some concern as to whether or not G&M will get their message through in the way their argument is formulated. By and large, our apprehension is triggered by a similar observation formulated by Elman (2013) who argues that some of G&M's propositions about the set-theoretic nature of qualitative research are prescriptive while others are descriptive. We believe that in the discussions to follow, more clarity in distinguishing between what qualitative research currently is and what it ought to be would help to strengthen G&M's vision of qualitative research as mainly being set-theoretic.

We take up G&M's quest and focus this round of debate more on issues that arise if and when one accepts set theory as the foundational system of thought of qualitative research. Above all, we respond to G&M's claim that fuzzy sets do not establish qualitative differences between cases that fall above and below the fuzzy set membership score of 0.5. In addition, we provide two examples, going beyond G&M, of how the notions of sets and their relations can be used to inform standard issues in qualitative social research: case selection in multi-method research and theory evaluation.

Qualitative Thresholds in Fuzzy Sets

G&M challenge the claim made by us and others in the field (e.g., Ragin 2000) that fuzzy sets, first and foremost, establish qualitative differences between cases ('differences in kind') and that this difference is established by whether a case holds a fuzzy set membership of higher or lower than 0.5 in a given fuzzy set. They, instead, find it "awkward and misleading" to interpret the difference between a case with a fuzzy value of 0.49 and another one with 0.51 as a qualitative one. For them, this difference is only a matter of degree with no particular relevance for the conceptual status of the cases in question. They claim that our insistence on a qualitative difference takes away the advantage of fuzzy sets, namely to recognize nuances. In the following, we provide arguments why we think G&M's position is either a misunderstanding of our claim or leads to untenable consequences.

First, and perhaps most importantly, all sets, by their very definition, establish a qualitative distinction between those cases that are members and those that are not. This also applies to fuzzy sets; otherwise, they would not be sets. The only information fuzzy sets add to this qualitative distinction is that cases can be ranked as to how much they are (not) members of a set. As a matter of fact, we think that much of the plausibility of G&M's postulate "qualitative research = set-theoretic research" precisely stems from the fact that sets reflect qualitative properties of cases and qualitative researchers are interested in, well, the cases' qualitative properties and their relations. Consider the Latin origin of the words "qualitative" and "quantitative," respectively. *Quantum* is translated as "how many" or "how much." Objects are assessed as to how much of a certain property they possess. The word *qualis*, instead, is translated as "what" or "of what kind/sort/nature." Qualitative statements about objects establish *qualitative* differences between objects that are in a given set and those that are out. We deem this property of sets both uncontroversial and constitutive for the vision of qualitative research as being rooted in set theory.

With crisp sets, all this seems pretty uncontroversial. Cases have either full membership or full non-membership, and the Rule of the Excluded Middle conveniently rules out any confusion between these two mutually exclusive and jointly exhaustive qualitative states in which cases can be. What seems confusing is whether or not the properties of fuzzy sets—partial membership scores and the breakdown of the Rule of the Excluded Middle—unavoidably mean that (a) qualitative distinctions between cases cannot be made; and, relatedly, that (b) one and the same case should be allowed to hold high membership in two or more mutually exclusive sets.

We believe that (a) fuzzy sets first and foremost express which cases are qualitatively identical and different, and only afterwards to which degree cases are members of a given set; and (b) that cases cannot simultaneously be good empirical instances of two or more sets that have been created precisely because researchers believe that there are two or more qualitatively distinct phenomena worth being given different names. Applied to G&M's example, if we believe that democracies are not only qualitatively different from autocracies, but also from anocracies, and we therefore introduce this new term into the literature, then it seems confusing to us that the exact *same* empirical property of a case (e.g. a Polity score of 7) qualifies a case as being a good instance of two conceptually distinct sets (e.g., democracy and anocracy).

We are not sure how exactly G&M position themselves on these two issues. They do argue that partial membership in two or more mutually exclusive sets should be possible and they have no issue with such partial membership being higher than 0.5 in more than one set. We agree that partial membership in mutually exclusive sets is unproblematic—if and only if, however, a case's membership does not exceed 0.5 in more than one of these sets. This is where we disagree with G&M. This allows two interpretations. G&M might claim that fuzzy sets do not establish any qualitative distinctions and that all is just a matter of degree. For reasons outlined above, this runs

so much counter to the notion of sets and the whole logic of their book that we dismiss this as a plausible interpretation of their argument.

Alternatively G&M might claim that the qualitative distinction is not made by the 0.5 but some other membership score. There are two candidates. One might want to claim that only full members of a set (1) are qualitatively different from the rest. Or one might claim that only full non-members (0) are qualitatively different from the rest. Both options seem problematic to us. Claiming that only cases with full membership in the set of, say, democracy should count as democracies often means that most analyses are exclusively performed on non-democracies, for often times there are no ideal-typical instances of democracy in the data. Likewise, using 0 membership as the qualitative threshold would often mean that researchers exclusively have democracies in their data. While these arguments run against 0 or 1 as qualitative anchors, the following provides an argument in favor of this anchor being at 0.5. By pure mathematical necessity, one and the same case can hold membership in one, and only one, of all the logically possible conjunctions between two or more sets. Although one and the same case will have fuzzy set membership scores of higher than 0 in most, and often even all, of the logically possible types, it can be exclusively attributed to only one of the mutually exclusive and jointly exhaustive ideal types—if and when 0.5 is interpreted as the qualitative anchor. This property of fuzzy sets is constitutive for the so-called fuzzy-set truth table algorithm (Ragin 2008), which, in turn, is at the heart of virtually all applied Qualitative Comparative Analysis.

It should also be noted that regardless of where G&M believe the qualitative threshold is located (and it must, we contend, be somewhere), one of their arguments against 0.5 as a qualitative anchor is not tenable, namely that small differences in fuzzy set membership (e.g., 0.49 vs. 0.51) should not establish qualitative differences. The same small difference in set membership scores of 0.02 would also establish a qualitative difference if the threshold was at 0 or at 1. If we accept that sets establish qualitative differences and thus that there must be a threshold, then differences between two fuzzy values have to be interpreted in different manners, depending on whether the two values are on the same side of the threshold or on two different sides.

Saying that fuzzy sets first and foremost establish qualitative distinctions does not mean that researchers lose sight of differences in degree. Saying that a case with 0.51 in the set of democracies should count as a democracy while another case with 0.49 should not does not prevent a researcher from seeing that the 0.51 case is much more ambiguous than a third case with a membership of, say, 0.91, which is a much clearer empirical manifestation of the ideal typical democracy. This should make clear why 0.5 is also sometimes called “the point of maximum ambiguity (i.e., fuzziness)” (Ragin 2008: 30). Note as well that this information on differences in degree among qualitatively identical cases never gets lost in applied fuzzy set social science, not even in QCA, which, as mentioned, crucially rests on the qualitative distinction imposed by the 0.5 anchor. For instance, all the formulas for calculating the parameters of fit

for set-relational statements make use of the fuzzy set membership scores of each case in all sets involved.

Last but not least, the use of 0.5 as a cutoff for qualitative distinction does not seem to be a peculiarity of social scientists using fuzzy sets. For illustration, consider Figure 1 borrowed from Lakoff’s (1987) book, *Women, Fire, and Dangerous Things*. Wavelength is the empirical information used for calibrating the fuzzy sets of different types of spectral colors—just as Polity scores are used for calibrating membership in different types of political regimes. Sure enough, any given wavelength produces partial membership in more than one type of color. Yet, no single wavelength yields a membership of higher than 0.5 in more than one type of color. At worst, a given wavelength produces maximum ambiguity by yielding fuzzy set membership scores of exactly 0.5 in two color sets (e.g., 590 NM as 0.5 fuzzy membership in both yellow and red). Confronted with such ambiguous classification, researchers—or language groups for that matter—might decide to introduce a new type of color, say “orange,” and define a case with 590 MN wavelength as having above 0.5 membership in orange and below 0.5 in both red and yellow—just as researchers introduce anocracy as a new type of political regime for those cases that otherwise would have an ambiguous status vis-à-vis already existing types of regime.

A Set-Theoretic Perspective on Case Selection and Theory Evaluation

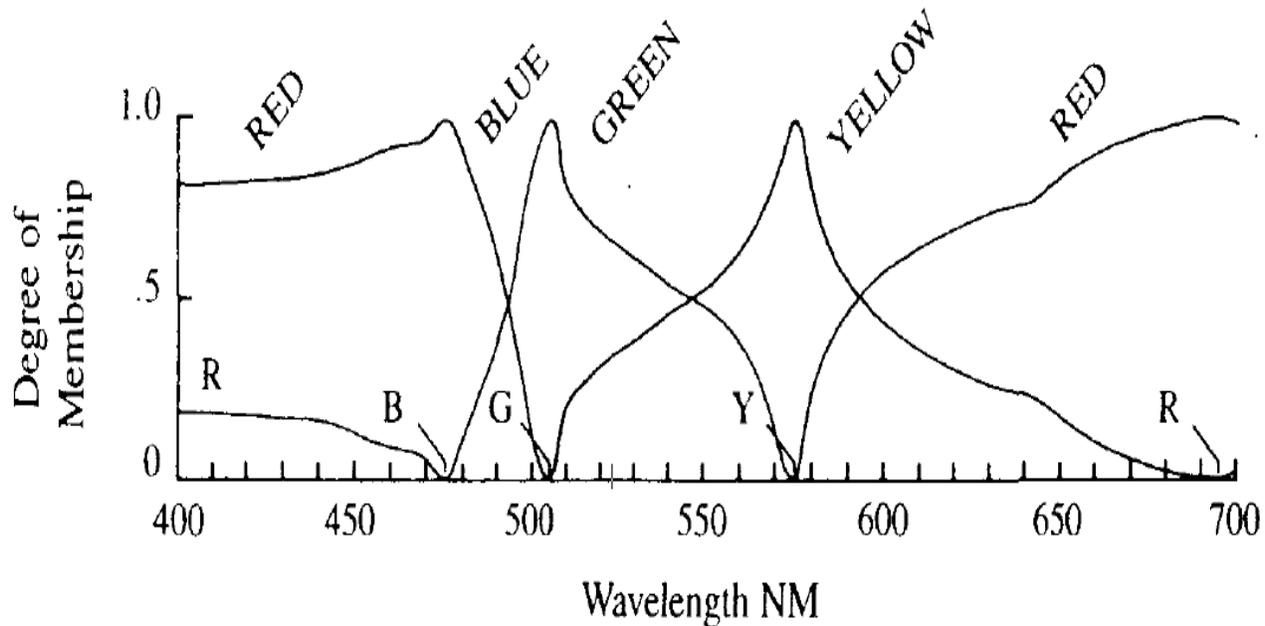
No doubt, the range of topics addressed by G&M is breathtaking. Yet, looking at social science research from a set-theoretic angle promises to lay the ground for an even more extensive methodological agenda. We highlight only two such issues that remain underdeveloped in G&M’s book and the literature at large: set-theoretic multi-method research (MMR) and set-theoretic theory evaluation.¹

MMR is *en vogue*. Yet, the literature is only slowly responding to the task of formulating principles and practices of one specific form of MMR: a set-theoretic cross-case analysis (read: QCA) followed by within-case analysis. The emerging literature on this topic (Rohlfing and Schneider 2013; Schneider and Rohlfing 2013) identifies different types of cases, some of which are similar to regression-based multi-method research (e.g. typical cases), while others are different (e.g., irrelevant cases, deviant cases consistency, or deviant cases coverage). The distinction between these types crucially rests on their membership above or below the 0.5 qualitative anchor in the condition and outcome sets. At the same time, this literature also proposes mathematical formulas for identifying the *best available* cases for each of these types and these formulas rely on the degree of membership of cases. This, incidentally, provides further evidence that fuzzy sets can do both: establishing qualitative differences and expressing differences in degree. Since set-theoretic MMR principles and practices are decidedly different from the better-known regression-based MMR, further research needs to go into how to perform this type of MMR.

The same also holds for another topic often linked to qualitative research: theory evaluation (as opposed to theory test-

Figure 1: Wavelength and Membership in Types of Color

Source: Lakoff (1987: 27)



ing, which is at the heart of most quantitative research). Early on, Ragin (1987: 118–121) alerted researchers to the possibility of using Boolean algebra for “evaluating theoretical arguments” derived by set-theoretic methods. In essence, researchers present both their theoretical hunches (T) and their empirical findings (E) in the form of Boolean expressions. The intersection between T and E (TE) yields a Boolean expression that describes where theory and empirics overlap (i.e., this is the part of the theoretical expectations that is supported by empirical evidence). The intersection between T and $\sim E$ ($T\sim E$), instead, yields a Boolean expression of which part of the theory is not supported by the empirical findings; and $\sim TE$, in turn, is where the empirical analysis revealed conjunctural causes for the outcome not foreseen by the theory.

This basic template of theory evaluation needs to be further refined and made compatible with recent developments in QCA. Most importantly, virtually no applied QCA yields fully consistent results, nor are always all instances of the outcome explained, or covered. In such a scenario, the different intersections between theory and empirical findings sketched out above will have different meanings depending on whether they yield Boolean expressions that mostly describe cases that are members of the outcome under study or non-members thereof. In our book (Schneider and Wagemann 2012: 295–305), we make a first attempt at spelling out this extended and, admittedly, more complicated and intricate version of set-theoretic theory evaluation. More research has to be done. For instance, much of what we write resonates with well-established notions such as “least likely” or “most likely” cases and the guidance they provide for case selection. In addition, more would need to be written on how theory evaluation is influenced by the common QCA practice of making assumptions

on so-called logical remainders, and here especially easy counterfactuals.

In sum, we agree with G&M that an explicit set-theoretic take on social science research opens an exciting and largely unexplored research agenda, and have sketched out some details of that agenda. This agenda should not be dismissed simply because not everybody would like to, nor needs to, be subsumed under a set-theoretic perspective.

Note

¹ We address these and other issues, such as “robustness tests in set-theoretic methods,” more extensively in our book (Schneider and Wagemann 2012).

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Methodologies—Theories—Praxis

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We are grateful for the thoughtful discussion of our book by Goertz and Mahoney and the valuable remarks of other contributors. In our final statement we want to address three points. First, we would like to revisit our distinction between three approaches to explanatory case studies. This is triggered by Goertz and Mahoney's statement that our congruence approach usually requires elements of one of our two other approaches, the covariational approach and causal-process tracing. Secondly, in a response to comments by Rohlfing we want to briefly clarify what we mean by a co-variational approach to explanatory small-N research. Finally, we would like to take up the notion of Goertz and Mahoney that we are witnessing a third wave of qualitative methodology. We will argue that work of this new wave is particularly promising when it comes to closing the gap between those interested in the methodological per se and those looking for concrete guidelines and advice for actually conducting qualitative research.

Goertz and Mahoney address our distinction between three approaches to explanatory case study research. They argue that to achieve the goal of congruence analysis, which is the evaluation of the explanatory power of alternative theories, we usually need to draw either on the covariational approach or on causal process tracing. We would like to stress that we take a broader and more pluralistic epistemological perspective on theory development in the social sciences than the other books and this implies that congruence analysis is not just a mix between the other two approaches.

When dealing with theories in the context of congruence analysis, we have theories in mind that operate on a higher level of abstractions than theories (or explanations) that are typically utilized in research adhering to the co-variational approach and or causal-process tracing. These theories are often explicitly linked to paradigms, they are not oriented towards a specified population, and they often embody propositions about constitutive concepts. Such theories are for instance prominent in International Relations (think of rationalism and social constructivism) and they often link empirical research to political philosophy.

The co-variational approach has more affinities with what could be called "empiricist" theories. This understanding of theories fits the view presented in the books of Goertz and Mahoney, Rohlfing, and Schneider and Wagemann. Empiricist

theories operate on a lower level of abstraction and apply to large or medium-sized, more or less clearly delineated populations of similar cases. These theories are for instance prominent in Comparative Politics. The theories are represented by variables that operate on one or a few levels of analysis. Compare the corresponding "data set observations" with the diverse and non-standardized observations yielded in congruence analysis. Moreover, while co-variational analysis heavily relies on comparisons across cases, congruence analysis is based on comparisons of bundles of empirical observations with predictions and propositions deduced from multiple theories.

Causal-process tracing has affinities to mechanism-oriented approaches where theories operate on a low level of abstraction, focusing on a single case or small populations. This concept of theory is more akin to Beach and Pedersen's view. Note also that crucial to our causal-process tracing approach is the very idea of processes. Although in congruence analysis propositions and predictions can concern processes, it is by no means necessary to do so. In short, we would argue that congruence analysis is sufficiently distinct from both co-variational analysis and causal-process tracing to merit a separate treatment.

Our second point relates to Rohlfing's comment that we "misrepresent" the co-variational approach and that we do not discuss Bayes' theorem. Perhaps we could have been more clear from the outset than when we talk about the co-variational approach, we do not talk about a general approach to the social sciences. Rather our co-variational approach is a very specific approach that is (a) tailored to a small-N setting; (b) informed by the experimental template; and (c) has the goal of causal inference. It draws on what Lijphart has described as "the comparative method" (1971). We believe that if we had called this approach "the comparative method," it would have raised more eyebrows. Bayes theorem is indeed not part of this approach to explanatory case study research. But it is actually discussed in the context of our congruence analysis approach (Blatter and Haverland 2012: 176–177, 194).

Our final point moves somewhat beyond the confines of methodology and focuses on the extent to which the principles and guidelines we are discussing will trickle down to the practice of case study research. Goertz and Mahoney have subsumed the books of this symposium under what they call the "third wave of qualitative methodology." This wave succeeds the work on (comparative) case studies in the 1960s and 70s, such as Lijphart's article mentioned above, and the responses to the attempt by King, Keohane, and Verba's *Designing Social Inquiry* (1994) to fit qualitative research into the statistical template, such as Brady and Collier's edited volume *Rethinking Social Inquiry* (2004). We believe that generally speaking, the third wave of qualitative methodology as represented in this symposium does more than the first two waves to contribute to closing the gap between methodological discourse on the one hand, and concrete guidelines and advices for those primarily interested in conducting case studies on the other hand.

For one, the third wave as represented here consists of

monographs. This very format helps to put into practice what we preach. Although many scholars as well as better graduate students might be able to synthesize the disparate and article-length methodological pieces, a common format of the first two waves, into a coherent approach for their own work, many others will fare well by book-length treatments written by one or two scholars. Such a format allows for in-depth treatment, integrated accounts, and consistency in methodological language.

Second, all authors make ample use of published real world research to illustrate their arguments. This is an important step forward. As users of methodological advice we were often discouraged by the artificial character of the hypothetical examples with which methodologists accompanied their ideas. Benefitting to some extent from the achievements of the first and second wave of qualitative methodological thinking, we and the other contributors were able to draw on an emerging set of best practices.

Finally, some books take additional efforts to get their message across to practitioners of case study research. Schneider and Wagemann's book on set-theoretic methods offers features like an "easy reading guide," a "how-to section," and a link to online learning material. Beach and Pedersen's book on process tracing provide a lot of useful advice regarding the often neglected topic of data collection as well as a user-friendly checklist at the end of the book. In our own book we tried to mirror the research process as closely as possible, starting from research goal and questions and concluding with modes of generalization and the format of presentation.

These features of the third wave should help to pass what should be the litmus test of any methodological debate: whether the principles and guidelines we are preaching will result in an improved practice of case study research.

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Principles and Practice of Social Science Methods Reloaded

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The central theme of my first contribution to the symposium is the distinction between the practice and principles of social science methods (or, in the terminology of *Two Cultures* (chap. 1), typical practice vs. possible and best practice). The existing discussions of *Two Cultures*, including those in the recent symposium in *Comparative Political Studies* (Goertz and Mahoney 2013), emphasize the salience of this distinction for two reasons that I focus on in the following. First, I need to correct Goertz and Mahoney's (GM) potentially misleading characterization of the way in which I discuss principles of case selection in qualitative research in *Case Studies and Causal Inference* (CSCI). Second, in light of GM's contribution to this symposium, I should clarify and reiterate what I agree and disagree with regarding *Two Cultures*.

Case Selection in Qualitative Research

GM assert about *CSCI* that it "also follows a long tradition in discussing types of case studies in light of statistical results; for case selection, these results help to identify typical cases, deviate cases, extreme cases, and so. Hence, much of his discussion of multimethod research has quantitative analysis in the leading role and the case study cast as a supporting actor." This statement is potentially misleading for multiple reasons.

First, GM might appear to infer from a discussion of types such as the typical case or the deviant case that one is engaged with case selection on the basis of statistical results. This ignores that these types also figure in set-relational multimethod research (Schneider and Rohlfing, forthcoming), which is largely disregarded in *Two Cultures* and chapter 9 on multimethod research in particular. More importantly, GM ignore here the fact that typical cases, deviate cases, and most-likely and least-likely cases are established types of cases in the qualitative literature. They were devised by Eckstein (1975) and Lijphart (1971), decades before the advent of multi-method research. They thus have a much longer tradition in qualitative research than in multi-method studies. In fact, then, multimethod research of the correlational and set-relational type borrowed these types from the case study literature. The salience of these types of cases for qualitative research is additionally attested to by discussions in other publications, including George and Bennett (2005) and Beach and Pedersen (2013).

Second, readers of this newsletter for whom "statistical analysis" specifically calls to mind regression analysis (as it does for me due to GM's reference to Gerring's [2007] largely regression-based discussion of case selection) would be misled by the claim that "quantitative analysis [is] in the leading

role” in *CSCI*. My text neither includes a single regression table nor mentions residuals as the main vehicle for regression-based case selection a single time. One major message of *CSCI* is that *stand-alone* case studies can result in strong causal inferences when they follow certain guidelines (chapter 1 and 10). In contrast to Blatter and Haverland, Beach and Pedersen, and GM, all of whom extend to multi-method research, this is exactly the reason why my book does not elaborate on the role of case studies in multi-method research. As I emphasize in *CSCI*, I concur that multi-method research has benefits, but I also underscore that the widely held negative view of causal inference via stand-alone case studies is unwarranted. My main argument is thus fully in line with one message of *Two Cultures*, i.e., that qualitative research (at least partially) has its distinct features and is suitable for strong inferences when it follows best practices (that are not necessarily typical practices).

Third, what I do discuss in chapter 3 of *CSCI* is the choice of cases on the basis of continuous variables. Some probably hold a very broad understanding of “quantitative” for which the use of continuous variables alone suffices, and only such a broad sense could sustain reading my work as giving a lead role to quantitative analysis. In chapter 2, I explain that correlational causal statements can relate differences in degree to each other, such as the higher the level of globalization, the more encompassing liberalization is (as opposed to high levels of globalization leading to massive liberalization) (see Jakobsen 2010). This might not be how GM believe that case studies are usually done, but they *can* be done in this manner and *CSCI* is about possible and best practices as opposed to typical practices. A point on which GM and I (and the other contributors to this symposium) fully agree is that decisions made at the level of ontology, theory, and research design have implications for subsequent steps of the research process. In this spirit, the formulation of hypotheses (if we presume hypothesis testing) that establish differences in degree *mandates* the choice of cases on the basis of continuous variables. If we formulate such a hypothesis but then turn to categorical causes and/or outcomes, we create an apparent mismatch between theory and measurement that is to be avoided.

What might have given the impression that parts of chapter 3 are about multi-method research is the fact that one might need to establish a simple bivariate scatterplot for case selection.¹ For instance, when we want to find the mechanism underlying the link between higher levels of globalization and liberalization, we evidently need to select two cases establishing this correlation. On a broader level, it would be nice to have some confidence as to whether such a relationship generally holds in the first place (see also below). But exactly the same is done in set-relational research—for a good reason, I might add. Suppose we are now interested in the mechanism that connects high globalization to high levels of liberalization (a relation of sufficiency); we then need to select a case that displays this pattern. Again, it would be good to know whether most countries that are globalized maintain high levels of liberalization (presuming we are not going for deviant cases). I cannot see anything wrong with this procedure, which actu-

ally is discussed in *Two Cultures* (chapter 14), and it certainly does not assign the leading role to quantitative (or set-relational) research. As I explain in chapter 1 of *CSCI*, the cross-case analysis is simply the *means* for making an informed choice of cases for process tracing. (Without using my means-end distinction, the general logic is also detailed by Beach and Pedersen in their discussion of case selection.)

Fourth, it is always interesting to see the reaction prompted by a reference to a population of cases. In my experience, such a reference has the air of traditional, old-school, quantitative research, naively looking for (universal) patterns. For GM, it suffices to put me in Gerring’s (2007) corner, but the sense in which this is correct is one which GM fail to specify: the philosophical basis of my book, like Gerring’s (and GM’s), is neo-positivism. This is another term that usually elicits interesting responses because it is linked to quantitative research and also appears to be outdated, old-school, etc. However, among others, Jackson (2010: chap. 3) nicely works out that the well-known and sometimes harshly fought methods debate of the past decades took place on common philosophical ground, namely neo-positivism. GM explicitly exclude interpretivist and other qualitative research from the qualitative paradigm, meaning that *Two Cultures* is also based on neo-positivism and thus buys into its fundamental premises and implications.

One of these implications is that causal analyses cannot be confined to a substantively important case without a more general theoretical ambition in terms of the number of cases about which one aims to make inferences. This is not to deny that the analysis of a single case can be a valuable endeavor and it is not a dismissal of philosophies of science implying a concern with single cases (as, for example, discussed by Beach and Pedersen, and Blatter and Haverland). However, a case-explaining case study does not fit squarely with the basic philosophical premise of neo-positivism that *CSCI* and *Two Cultures* share.

In this context, I do not understand GM’s problem with my statement about populations, partly because they present it without explicating their concerns. *Two Cultures* actually entails the claim that qualitative research involves populations of cases (chapter 16), whereas GM assert that populations are smaller in qualitative research, which might or may not be true. (I note here that I develop a methodological rationale for small populations in chapter 9 of *CSCI*.) Unless one is able to study all cases in the population, case studies involve the generalization of causal inferences, which is a point that Goertz and Mahoney readily accept in other articles (Mahoney and Goertz 2004: 654).

Furthermore, GM’s quote from *CSCI* is taken out of context because I explain that the idea of what the population is might change over the course of the research process (Rohlfing 2012: 24). Nevertheless, qualitative research at minimum always starts with an idea about a general phenomenon that one wants to explain, such as democratization or welfare state retrenchment. While I cannot elaborate on this argument in detail here, the definition of the outcome of interest at least implicitly delineates the population of cases. Since many case studies are driven by an interest in a specific phenomenon

and, in fact, cannot be conducted without an understanding of what the outcome of interest is, all neo-positivist case studies effectively delineate the population once the positive and negative outcomes are demarcated. Even if one does not follow my line of reasoning here, without an understanding of what the population is at a given stage of the research process, it is difficult to do an analysis in the first place. For instance, how can I know that a condition is sufficient and select a case for process tracing when I do not know what cases to include in the 2x2 table in the first place (such as Goertz and Mahoney 2012:213)?

(Unknown) Typical Practice vs. Best Practice

GM state that “we find it difficult to believe that Rohlfing does not really think that many if not all of the dimensions that we discuss are intimately related given what he argues in his book.” As they suggest, I do think that many dimensions of a research design are intimately related to each other, with ontological and theoretical premises at the top level (though I do not think that the systematic relation describing typical practice [see Goertz and Mahoney 2012: 1] is the relation that follows from best practices). Indeed, this is one of the main points of *CSCI* (chapter 1). However, my initial contribution to this symposium is obviously aimed in another direction. Similarly, the intimate relation of dimensions is not the main point of *Two Cultures*, as this alone would be a statement about best practices. Instead, GM assert that the dimensions are interrelated and that the bundle of dimensions they describe represents the typical practice of qualitative research. My previous contribution only aimed at the latter point because this is not substantiated by *Two Cultures* due to the lack of an appropriate review of qualitative and quantitative research practices. Neither the review in *Two Cultures*, the review by Mahoney and Terrie (2008), nor any other review of which I am aware delivers data that comes close to an empirical assessment of *Two Culture’s* central message. Thus, there is still much empirical work to do if we are to assess the extent to which the *Tale of Two Cultures* represents actual typical practices of social science research.

Note

¹ If we understand statistical analysis specifically as regression analysis, then a bivariate scatterplot does not suffice to make a qualitative study of those cases qualify as multimethod.

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Announcements

APSA Short Courses Sponsored (or Co-Sponsored) by Division 46: Qualitative and Multi-Method Research Wednesday, August 28, 2013, Chicago, Illinois

The fee for each short course is \$10 for faculty, \$5 for graduate students.

Multi-Method Research: Criticism and Progress in Methodology 9:00 AM–1:00 PM

Instructors: David Collier, University of California, Berkeley (DCollier@Berkeley.edu); Thad Dunning, Yale University (Thad.Dunning@Yale.edu); and Jason Seawright, Northwestern University (J-Seawright@Northwestern.edu).

This course builds on the spirit of the well-known book on scientific methodology by Lakatos and Musgrave, *Criticism and the Growth of Knowledge*. Critiques of methods contribute to improving research tools, as well as to establishing criteria for selecting among alternative tools. Course sessions will focus on the criticism of quantitative methods that was crucial in launching the qualitative methods movement of the past 15 years, as well as on subsequent critiques of regression analysis, natural experiments, process tracing, and diverse tools for case study and medium-N analysis.

**Field Research and Analytic Transparency:
Collecting Data, Analyzing Evidence, and
Drawing Inferences in Qualitative Research
2:00 PM–7:00 PM**

Instructors: Diana Kapiszewski, University of California at Irvine (dianakap@uci.edu); Naomi Levy, Santa Clara University (nlevy@scu.edu); Colin Elman, Syracuse University (celman@maxwell.syr.edu).

This short course addresses a variety of data-collection techniques, and discusses ways in which scholars can demonstrate that qualitative data of the type collected during field research support their analytic claims. The course has two linked foundational premises. First, designing, conducting, and analyzing data collected during fieldwork are overlapping and inter-dependent processes. Second, it is impossible to evaluate—let alone replicate—research without knowing how, and how transparently, those processes were carried out.

With regard to field research, we explore how research design and fieldwork interact, address preparing for field research, and discuss multiple data-collection techniques—both interactive (surveys, experiments, interviewing, oral histories, focus groups, participant observation, and ethnography) and non-interactive (observation, archival research, and collecting documents and statistics). Throughout, we provide scholars with strategies to help them anticipate and address challenges involved in designing and conducting field research, for instance, (1) converting their research design into a “to get” list; (2) accessing elusive data and data sources; (3) evaluating data’s evidentiary value; (4) organizing and managing data; and (5) analyzing data both in and out of the field.

During the last hour of the course, we discuss strategies for achieving analytic transparency in qualitative research. Engaging in analytic transparency requires scholars to identify the inferential structures invoked in their research design, and demonstrate that their data support the inferences in their written work in a manner appropriate to that design. We also introduce participants to a promising approach for achieving analytic transparency in qualitative research: active citation (Moravcsik 2010).

Although fieldwork is usually associated with “studying politics abroad,” we discuss techniques that may be applied inside and outside the U.S. The course includes several hands-on activities. Participants will also be provided with a large packet of handouts, including document templates, sample correspondence, etc. The course is valuable for students planning dissertation projects, for scholars who would like to develop or improve their data-collection and analysis skills, and for those who teach classes on research methods.

**Process-Tracing Methods
2:00 PM–6:00 PM**

Instructor: Derek Beach, University of Aarhus, Denmark (derek@ps.au.dk).

This short course introduces recent advances in process-tracing (PT) case study methodology, giving participants a set of methodological tools to utilize the method in their own research. The relative strength of PT methods is that it enables us to study causal mechanisms using in-depth case studies. Causal mechanisms are theories that detail how an outcome is produced, opening up the black box of causality by describing what happens in the arrow in between X and Y.

The course starts by differentiating PT from other methods; including small-n methods such as analytical narratives and comparative case studies. The course then goes briefly into the ontological debates on how causal mechanisms can be understood. Here the focus

is always on the practical methodological implications that adopting different understandings have. The course then discusses how we can conceptualize causal mechanisms in a manner that enables them to be studied empirically in case study research. Conceptualization deals with translating a causal theory of an $X \rightarrow Y$ relationship into a theorized causal mechanism that can explain how X produces Y through the working of a series of parts of a mechanism, each composed of entities engaging in activities.

The third session of the course deals with how we can trace mechanisms empirically using in-depth case studies. The course introduces the Bayesian logic of inference that many scholars contend underpins PT, contrasting it with the frequentist logic of inference. We then move to a discussion of different test types and show how Bayesian logic enables us to update our confidence in the presence of mechanisms in a single case.

The course concludes with a discussion of case selection techniques and how strategic selection enables us to nest our single case into a broader analysis, enabling us to make inferences beyond the single case to the rest of the population of the given theoretical phenomenon.

The course will be particularly valuable for scholars who are considering using PT in their own research, as it provides a set of practical methodological tools along with an exposition of the strengths and weaknesses of the method. The course will also be useful for scholars who teach research methods as it reviews recent debates about case study methodology.

**Using Qualitative Information to Improve Causal Inference
2:00 PM–5:00 PM**

Instructors: Adam N. Glynn (aglynn@fas.harvard.edu) and Nahomi Ichino, Harvard University (nichino@gov.harvard.edu).

This course introduces a new approach to mixed methods based on the Rosenbaum (2002, 2009) approach to observational studies. We demonstrate how to use this approach to address causal questions with small to medium sample sizes while retaining the ability to produce p-values and confidence intervals. Furthermore, we show how to incorporate qualitative information into the analysis to ameliorate the effects of difficult-to-measure outcomes, to construct qualitative confidence intervals, and to consider the robustness of the results in a sensitivity analysis.

The short course will be organized into three parts. The first two parts will be in a lecture format with Q&A periods and demonstrations throughout. The first part will present the Rosenbaum framework for observational studies. The second part will show how to include qualitative information in this approach, with a demonstration of free software for this method. Finally, course participants will work through how to test their hypotheses in a workshop format.

This course will be valuable for students at the dissertation-planning stage and other scholars whose goal is causal inference and work with non-experimental data. It will be particularly useful for studies where the key explanatory variable cannot be randomized, such as analyses of the effects of institutions in a given set of countries or subnational units. By the end of the course, participants will have walked through a checklist of the steps towards a robust research design.

Readings: We recommend, but do not require, that participants read the notes we will distribute ahead of the short course.

**APSA Panels/Roundtables Sponsored (or Co-Sponsored) by
Division 46: Qualitative and Multi-Method Research
August 29–September 1, 2013, Chicago, Illinois**

**Author Meets Critics: Michael Coppedge's
*Democratization and Research Methods***

Chair: Anibal Perez-Linan, University of Pittsburgh
Participants: Barbara Geddes, University of California, Los Angeles;
Michelle Taylor-Robinson, Texas A&M University; Michael
Coppedge, University of Notre Dame

**Causal Inference: Bayesian Approaches,
Counterfactuals, and Motivations**

Chair: Andrew Bennett, Georgetown University
Derek Beach and Rasmus Pedersen, Aarhus University, "Turning
Observations into Evidence: Using Bayesian Logic to Evaluate
Empirical Material."
Kendra L. Koivu, University of New Mexico, "Taking Asymmetry
Seriously: Logical Parallels, Multifinality, and Counterfactuals of
Sufficiency."
Ingo Rohlfing, University of Cologne, "The Importance of Being
Probably Wrong in Bayesian Case Studies."
Alan M. Jacobs, University of British Columbia, and Macartan
Humphreys, Columbia University, "Mixing Methods: A Bayesian
Unification of Qualitative and Quantitative Approaches."
Joseph O'Mahoney, Brown University, "Why Did You Do That?:
The Methodology of Motive Attribution."
Discussants: Andrew Bennett, Georgetown University; Colin Elman,
Syracuse University

**Comparative-Historical Analysis in the Social Sciences I:
Agenda-Setting Work**

(Co-Sponsored with Comparative Politics Division)

Chair: Nancy Bermeo, Oxford University
Stephan Haggard, University of California, San Diego, "The Devel-
opmental State."
Steven Levitsky, Harvard University, and Lucan Way, University of
Toronto, "The Origins of Durable Authoritarianism."
Jane Gingrich, University of Minnesota, Twin Cities, "Coalitions,
Policies, and Distribution: Esping-Andersen's Three Worlds of
Welfare Capitalism."
James Mahoney, Northwestern University, "Theory and Method in
Comparative-Historical Analysis: Three Strategies Revisited."
Discussant: Nancy Bermeo, Oxford University

**Comparative-Historical Analysis in the Social Sciences II:
Tools for Temporal Analysis**

(Co-Sponsored with Comparative Politics Division)

Chair: Daniel Carpenter, Harvard University
Paul Pierson, University of California, Berkeley, "Path Dependence
Revisited."
Giovanni Capocchia, Oxford University, "Critical Junctures and the
Analysis of Institutional Origin."
Jacob Hacker, Yale University, and Kathleen Thelen, Massachusetts
Institute of Technology, "Change without Reform, Reform with-
out Change: The Hidden Faces of Institutional and Policy Trans-
formation."
Evan Lieberman, Princeton University, "Multi-Method Compar-
ative-Historical Analysis: Trends and Possible Trajectories."
Discussant: Daniel Carpenter, Harvard University

**Conceptualization and Measurement in
Cross-National Research**

Chair: Tony P. Spanakos, Montclair State University
Heidi Jane M. Smith, "Conceptualizing and Measuring Decentraliza-
tion."
Daniel Neep, University of Exeter, "Concept Formation and Author-
itarianism: Ideal Types and Typologies in Qualitative Political Sci-
ence."
Jens Borchert, University of Frankfurt, "The Democratic Promise
Meets the Democratic Doubt: The Concept of Political Equality,
its Three Dimensions, and its Historical Development."
Jon D. Carlson, University of California, Merced, "A War by Any
Other Name ... Reduces the Size of Your 'N' by 20%: Case Selec-
tion and the Correlates of War (COW) Inter-State War Data Set."
Mariana Borges, Northwestern University, "Measuring Clientelism:
A Concept Analysis."
Discussants: Tony P. Spanakos, Montclair State University; Amy R.
Poteete, Concordia University

**Conceptualizing "The Empirical": A Conversation among
Interpretive Empirical Researchers and Political Theorists**

(Co-Sponsored with Interpretive Methodologies
and Methods Related Group)

Chair: Peregrine Schwartz-Shea
Participants: Lisa J. Disch, University of Michigan; Farah Godrej,
University of California, Riverside; Paul A. Passavant, Hobart and
William Smith Colleges; Brent J. Steele, University of Kansas;
Dvora Yanow, Wageningen University; Rochana Bajpai, SOAS,
University of London

**Contributions of Fuzzy-Set / Qualitative Comparative
Analysis: Some Questions and Misgivings**

(Co-Sponsored with Comparative Politics Division)

Chair: David Collier, University of California, Berkeley
Zachary Elkins, University of Texas, Austin, "Radial Taxonomy in
Practice."
Thad Dunning, Yale University, "Measurement and Causal Infer-
ence: Some Concerns about fs/QCA."
Marcus Kurtz, Ohio State University, "The Promise and Perils of
Fuzzy-Set/Qualitative Comparative Analysis: Measurement
Error and the Limits of Inference."
Jason Seawright, Northwestern University, "Warranting Methods: A
Proposed Standard with Application to QCA."
Sherry Zaks, University of California, Berkeley, "When Goldilocks
Gets it Wrong: Origins, Assumptions, and Extensions of fs/QCA."
Discussants: Bear Braumoeller, Ohio State University; Evelyne Huber,
University of North Carolina, Chapel Hill

APSA Committee Panel

**Data Access and Research Transparency (DA-RT) in
Political Science Roundtable 1 of 2:
Policy and Implementation**

(Co-Sponsored with Political Methodology Division)

Chair: Arthur Lupia, University of Michigan
Participants: Allan Dafoe, Yale University; Brian Humes, National
Science Foundation; Diana Kapiszewski, University of California,
Irvine; John Ishiyama, University of North Texas; Rick Wilson,
Rice University

**APSA Committee Panel
Data Access and Research Transparency (DA-RT) in
Political Science Roundtable 2 of 2:
Persuasion and Promise**

(Co-Sponsored with Political Methodology Division)

Chair: Colin Elman, Syracuse University
Participants: George Alter, University of Michigan; Simon Jackman, Stanford University; Lisa Wedeen, University of Chicago

Deconstructing Social Science Concepts

(Co-Sponsored with R-IPSAC-IPSA Research Committee #1
[Concepts and Methods])

Chair: Rudra Sil, University of Pennsylvania
Jillian Schwedler, University of Massachusetts, Amherst, "Puzzling Out 'Puzzles.'"
Douglas Dow, University of Texas, Dallas, "The Concept of 'Concept.'"
Frederic Schaffer, University of Massachusetts, Amherst, "Questions about 'Causes.'"
Amel Ahmed, University of Massachusetts, Amherst, "Analyzing 'Data.'"
Amy Linch, Pennsylvania State University, "Talking about 'Events.'"
Discussants: Dvora Yanow, Wageningen University; Ivan Ascher, University of Wisconsin-Milwaukee

**Field Research on African Politics in the 21st Century:
Methodologies of Power and Persuasion**

Chairs: Lauren MacLean, Indiana University, Bloomington; Parakh Hoon, Virginia Tech
Participants: Robert Bates, Harvard University; Catherine Boone, University of Texas, Austin; John Harbeson, Johns Hopkins University/SAIS; Parakh Hoon, Virginia Tech; Lauren MacLean, Indiana University, Bloomington; Leonard Wantchekon, New York University

Field Research: Principles, Applications, and Techniques

Chair: Brian C. Rathbun, University of Southern California
Katya Drozdova, Seattle Pacific University, "Qualitative Methods for National Security Applications: Archival Research and Contemporary Sources."
Lauren M. MacLean, Indiana University, Bloomington, Diana Kapiszewski, University of California, Irvine, and Benjamin L. Read, University of California, Santa Cruz, "Field Research and the Production of Political Knowledge."
Jeffrey W. Paller, University of Wisconsin, Madison, "Ethnography as Empirical Strategy: Process, Networks, and Spontaneity."
Anna Katherine Boucher, Ahmar Maboob, and Lydia Dutcher, University of Sydney, "Power and Solidarity in Elite Interviews: Building a Rubric for Analysis."
Discussants: Brian C. Rathbun, University of Southern California; Jaimie Bleck, Notre Dame University

Issues in Research Design: Levels, Units, Cases

Chair: John S. Odell, University of Southern California
Jan Erk and Wouter Veenendaal, Leiden University, "Putting all our Eggs in the 'Freedom House' Basket: Democracy and Democratization Research on Small States."
Giovanni Capoccia, Oxford University, and Laura Stoker, University of California, Berkeley, "Choosing and Combining Units in Political Research."
Parakh Hoon, Virginia Tech, "Revisiting Theories of African Political and Economic Development and Botswana's Miracle."
John Andrew Donaldson, Singapore Management University, "Going Extreme: Systematically Selecting Extreme Cases for Study through Qualitative Methods."
Discussants: John S. Odell, University of Southern California; Barbara Geddes, University of California, Los Angeles

The Methods Café

(Co-Sponsored with Interpretive Methodologies
and Methods Related Group)

Chairs: Dvora Yanow, Wageningen University; Peregrine Schwartz-Shea, University of Utah
Mary Bellhouse, Providence College, and Ilan Danjoux, University of Calgary, "Analyzing Visual Materials: Paintings, Photographs, Political Cartoons."
Gerald Berk and Dennis Galvan, University of Oregon, "APD/Institutional Analysis: Methodological Issues."
Emily Hauptmann, Western Michigan University, "Archival Research."
Kevin Bruyneel, Babson College, "Collective Memory Studies: Methodological Issues."
Stephen Marshall, University of Texas at Austin, and Ronald Schmidt, California State University, Long Beach, "Critical-Interpretive Race and Immigration Studies."
Mary Hawkesworth, Rutgers University, "Feminist Methods."
Katherine Cramer Walsh, University of Wisconsin, Madison, and Dorian Warren, Columbia University, "Field Research I: United States."
Edward Schatz, University of Toronto, "Field Research II: 'Overseas.'"
Jörg Strübing, University of Tübingen, "Grounded Theory."
Cecilia Lynch, University of California, Irvine, "Interpreting International Politics."
Lee Ann Fujii, University of Toronto, "Interviewing."
Shaul Shenhav, Hebrew University of Jerusalem, and Lisa Wedeen, University of Chicago, "Narrative Analysis/Discourse Analysis."
Peregrine Schwartz-Shea, University of Utah, and Dvora Yanow, Wageningen University, "Research Design for Interpretive Projects."
Adria Lawrence, Yale University, "Teaching Qualitative-Interpretive Methods."
Frederic Charles Schaffer, University of Massachusetts, Amherst, and Cyrus Ernesto Zirakzadeh, University of Connecticut, "Why Compare? Comparison and Concepts in Interpretive Research."

Multi-Method Research

Chair: Jason Seawright, Northwestern University
Uriel Abulof, Tel-Aviv University, "Public Political Thought: Doxa, Discourse and Normative Concepts Analysis."
Peter L. Lorentzen, University of California, Berkeley, and M. Taylor Fravel, Massachusetts Institute of Technology, "Evaluating Formal Models with Qualitative Evidence."
David S. Patel, Cornell University, "Making Space for GIS in Multi-method Research."
Shivaji Kumar, Ohio State University, "Benefits of Sequential, Multi-method Designs for Researching Public Opinion Influences on the Foreign Policies of Emerging Asian Powers."
Ajay Verghese, Stanford University, "Sequencing Multi-Methods Research."
Discussants: Amy G. Mazur, Washington State University; Jason Seawright, Northwestern University

Narratives, Discourse, and Interpretation

(Co-Sponsored with Interpretive Methodologies and Methods Related Group)

Chair: Peregrine Schwartz-Shea, University of Utah
Jelena Subotic, Georgia State University, "The Politics of Space and Place: Narrative Analysis in International Relations."
Craig A. Parsons, University of Oregon, "Constructivism and its Alternatives."
M.J. Peterson, University of Massachusetts, Amherst, "Institutional Self-Presentation in a Changing Ideational Context: The World Bank and the UNDP Define their Roles in Promoting Development."
Natalia V. Kovalyova, University of North Texas, "Political Russian and the Legacy of the Authoritarian Discourse."
Christian Rudolf Thauer, Freie Universität Berlin, "Re-reading Thucydides: An English School Approach."
Discussants: Peregrine Schwartz-Shea, University of Utah; Shaul Shenhav, Hebrew University of Jerusalem

Path Dependence and Critical Junctures: Methodological Benefits and Limits

Chair: Hillel David Soifer, Temple University
Zeki Sarigil, Bilkent University, "Showing the Path to Path Dependence."
Jael Goldsmith Weil, Northwestern University, "Arroz sin leche, ¿La Dictadura Acabar? The Persistence of State Subsidized Milk during a Neoliberal Dictatorship: A Case of Failed Path Reversal."
Clayton J. Cleveland, University of Oregon, "Eventful and Critical Junctures: What is the Difference and Why is it Important?"
Michael Bernhard, University of Florida, "Institutional Syncretism and the Limits of Path Dependence: A Theory of Regime Instability."
Discussant: Hillel David Soifer, Temple University

Qualitative Comparative Analysis: Critiques, Enhancements, and Applications

Chair: Ingo Rohlfing, University of Cologne
Alrik Thiem, ETH Zurich, "When More than Time is of the Essence: Enhancing QCA with eQMC."
Ursula Hackett, University of Oxford, "The Exclusive-OR and its Applications in Qualitative Comparative Analysis."
Jack Paine, University of California, Berkeley, "Can Qualitative Comparative Analysis (QCA) Achieve Its Goals of Causal Inference?"
Mathias Poertner, Donghyun Choi, and Christopher Kroglund, University of California, Berkeley, "Fuzzy Sets, Shaky Ground: Testing Calibration, Measurement, and Specification Sensitivity in fsQCA."
Discussant: Ingo Rohlfing, University of Cologne

Subnational Comparative Analysis: Progress and Prospects

(Co-Sponsored with Comparative Politics Division)

Chair: William Hurst, Northwestern University
Participants: Richard Snyder, Brown University; Caroline Arnold, Brooklyn College-CUNY; Catherine Boone, University of Texas, Austin; Edward Gibson, Northwestern University; William Hurst, Northwestern University; Rachel Riedl, Northwestern University

New Editor for the Committee on Concepts and Methods Working Papers Series

The Committee on Concepts and Methods (C&M) announces that Andreas Schedler of CIDE Mexico City is stepping down as editor of our two C&M working paper series and that Cas Mudde of the University of Georgia will be taking over as the new editor.

Founded by Giovanni Sartori and friends, C&M was the first research committee recognized by the International Political Science Association (IPSA) in 1970. It promotes conceptual and methodological discussion in political science and provides a forum of debate for adherents of all methodological schools, who otherwise tend to conduct their deliberations at separate tables.

C&M publishes two series of working papers that readers may consult and download for free at the C&M website (www.concepts-methods.org):

Political Concepts contains work of excellence on political concepts and political language. It seeks to include innovative contributions to concept analysis, language usage, concept operationalization, and measurement.

Political Methodology contains work of excellence on methods and methodology in the study of politics. It invites innovative work on fundamental questions of research design, the construction and evaluation of empirical evidence, theory building, and theory testing.

Founded by Andreas Schedler in 2005, C&M has published close to one hundred working papers that have collectively been viewed and downloaded many thousands of times. Putting out the two working paper series is now perhaps the most visible and important activity of C&M.

The working paper series is open to excellent and original work from all sub-disciplines and methodological approaches. In particular, we are looking to increase the work in American Politics, Comparative Politics outside of Europe and Latin America, International Relations and Political Theory as well as research using interpretive and quantitative methods. All suitable submissions will be reviewed by two peers, who will be experts on the methodological aspect of the paper (after all, the innovation should be methodological rather

than substantial). Importantly, they are asked to review the submission *constructively*, looking to *improve* rather than reject the paper, understanding that it is work-in-progress rather than the final product.

The WP series also aims to be(come) *the* place for high-quality original, conceptual and methodological work that is unlikely to be published (in that detail) in top journals. We are thinking in particular of extensive conceptual and methodological discussions in PhD dissertations, (reliability and validity) assessments of important datasets, and robustness tests of existing research using conceptual or methodological innovations. Unlike articles in most academic journals, WP papers can address *what* and *how* questions; e.g., what is the best conceptualization of party ideology? How can religiosity best be operationalized cross-regionally? What dataset provides the most reliable and valid results in studies of political mobilization? Further, WP papers that do address *why* questions do not necessarily have to provide empirically substantiated alternative explanations for the theories they critically assess. Most importantly, the critiques should predominantly be conceptual or methodological.

For more information on the C&M Working Papers series, and to download the published papers for free, please visit the website: <http://www.concepts-methods.org/WorkingPapers>.

If you are not sure whether your paper will fit the series, please feel free to contact the editor, Cas Mudde (mudde@uga.edu), to discuss your paper. If you want to submit a manuscript to the C&M Working Paper series, send it to wp@concepts-methods.org.

Introduction to the South-North Network for Qualitative and Multi-Method Research in Latin America

To date, few efforts have been made to formally bring together scholars from South and North America to assess and develop qualitative and mixed-methods approaches to the study of Latin America. The growing rigors of social science research, together with the increasing number of social scientists graduating from excellent Latin American universities, make this kind of collaboration increasingly relevant. The South-North Network for Qualitative and Multi-Method Research in Latin America (“South-North Network”) was formed by a group of graduate students and junior faculty members in 2012 as a first step toward promoting greater cross-national engagement on qualitative and multi-methods research.

The South-North Network seeks to develop a common research agenda that pushes its members to engage in an explicit and self-reflexive discussion of the methodological choices that we make as scholars. Toward that end, it has two primary objectives: (1) to critically analyze and publicly debate the practical, ethical, and epistemological challenges of applying different methodologies to our study of the region, and (2) to develop and refine our methodological toolkit so that we can more effectively address central questions for the region. In fulfillment of these objectives, the South-North network will promote the creation of spaces where scholars can formally discuss research on the region that is explicitly guided by qualitative and multi-methods.

A first expression of this regional collaboration will take place at the XXXI International Congress of the Latin American Studies Association (LASA), which takes place on 29 May–1 June, 2013, in Washington, DC. The congress brings together thousands of social science and humanities scholars from throughout the world to present their work on Latin America and the Caribbean. Founding members of the network have organized a panel on the processes and actors associated with the state and state-building in Latin America. The panel will examine, among other things, the unintended consequences of local land dispute resolutions on long-term state development, the

contemporary state and the territorial organization of power in the region, and the consequences of localized conflicts between the state and its challengers on violence, rule-of-law, and citizenship. These works make a substantive contribution to our knowledge of the role of the state in Latin America. They are also explicitly reflective about the methodologies that are most fruitful for studying the state and its institutions.

In addition to organizing methods-focused panels at academic conferences, the South-North Network intends to serve as a forum for virtual and real-time discussions of the methodological innovations that affect the study of Latin America. As a cross-national network, we hope that these discussions can transcend borders. We want the most recent methodological insights to be integrated into the course curricula of Latin American universities. We hope to build and grow ties to regional training institutes, such as the Summer School for Mixed Methods of the Southern Cone. The school took place for the first time this past January with great success at the Pontificia Universidad Católica de Chile (<http://www.stateness.com/espanol-escuela-de-verano-de-metodos-mixtos>). It has benefited a wide range of regional students and investigators through exposure to cutting-edge research on qualitative, quantitative, and mixed-method techniques. Interacting with local and foreign faculty and students, participants were able to explore the usefulness of multi-method techniques, as well as the pitfalls of these approaches. We seek to join forces with initiatives such as this one in order to strengthen the connections between methodologically like-minded scholars and students, in an effort to improve the scope, the substance, and the rigor of our work.

Currently, the South-North Network consists of faculty members and graduate students from a variety of public and private universities in South and North America. Affiliated individuals include Juan Pablo Luna, Andreas Feldman, and Julieta Suárez-Cao (Pontificia Universidad Católica de Chile; Núcleo Milenio para el Estudio de la Estatalidad y la Democracia en América Latina); Juan Bogliaccini (Universidad Católica del Uruguay; María Paula Saffon (Columbia University); Jennifer Cyr (University of Arizona); and Sara Niedzwiecki (University of North Carolina at Chapel Hill). If you are interested in hearing more about our efforts, or if you would like to join our network of scholars, please contact Juan Pablo Luna (jpluna@icp.puc.cl) or Juan Bogliaccini (juan.bogliaccini@correo.ucu.edu.uy).

Qualitative and Multi-Method Research

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