

# Political Methodology

## Committee on Concepts and Methods Working Paper Series

31

February 2011

### Varieties of Qualitative Methods

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## Introduction

In 1978, Gary Zukav claimed that there are two types of people in the world. Those that have a predisposition to logic and precision and those that prefer a more free-flowing study of the world. The first group find themselves attracted to the sciences and the latter to the liberal arts (Zukav 1978). For Zukav, a person's ontology drove them towards a specific type of education. While a clean, elegant notion, the divisions within social science as to methodological orientation belie it. For any discipline, perhaps especially ones at the nexus between "hard" science and humanities, there are a multitude of methods to choose from and research styles as varied as the individuals who practice them.

In political science, however, the division among researchers has, for the past couple of decades, been described in much the same way. Indeed, the rift between those that use quantitative methods and those that use qualitative had become so entrenched that, much like Zukav, it is often referred to as representing separate cultural enclaves (Goertz and Mahoney 2006) and even different religions (Beck 2008; Schrodt 2008). Concentrating upon this divide, however, has given rise to a false understanding that these groups are homogenous. It is now often assumed that qualitative research is a uniform entity, and that qualitative researchers have arguments only about substantive issues and not about methodology.<sup>1</sup> This homogenization of the "qualitative methodologist" is both harmful and unwarranted. In this article, we seek to rectify this burgeoning assumption before it becomes further entrenched in the field. Rather than a "tale of two cultures," we argue that the practice of qualitative methods within the social

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<sup>1</sup> Whether or not the same assumption holds true within the quantitative realm is questionable as well, but beyond the scope of this discussion. Though the language of mathematics provides a unifying element, the distinctions between statistical analysis and formal modeling may be large enough to warrant a less holistic understanding of the quantitative tradition.

sciences is actually a tale of many sub-cultures, and that these dissimilarities emerge as a result of differences in ontological and epistemological assumptions.

### **The Initial Divide**

It is common to come across the acronym KKV in the opening remarks of books and articles on qualitative methods. For the novice qualitative methodologist, this is an acronym that must be learned early on for it is sure to appear regularly. KKV refers to the King, Keohane and Verba volume *Designing Social Inquiry: Scientific Inference in Qualitative Research*<sup>2</sup> wherein the authors lay out the schematics for qualitative research design, while also providing a synthesis of mainstream quantitative methods.<sup>3</sup> The advice within the volume urged qualitative researchers to take cues from their quantitative counterparts in order to augment the inferential leverage a researcher has on empirical tests.

Though many debates about the veracity of statistical versus case study research had been raging within the social sciences for decades, the publication of DSI<sup>4</sup> in 1994 engendered a number of well-reasoned responses, some in favor of the volume's recommendations, some more critical of the privileged position granted to the quantitative template. Over the next decade the critical voice grew in volume and culminated in the edited piece by Henry Brady and David Collier, *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (hereafter RSI). RSI reprinted a number of earlier articles questioning the recommendations of DSI, while also presenting new work on qualitative methods. Sections include critiques of the quantitative template, discussions of tools for qualitative research, and suggestions on how to combine

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<sup>2</sup> Brady and Collier have largely avoided the use of the acronym 'KKV' to refer to the volume, preferring instead 'DSI' to focus the reader's attention on the book itself, not the authors behind it.

<sup>3</sup> Brady, Collier and Seawright "The Quest for Standards: King, Keohane and Verba's *Designing Social Inquiry*" in Brady and Collier *Rethinking Social Inquiry: Diverse Tools, Shared Standards* 2004

<sup>4</sup> In keeping with Brady and Collier's shorthand, we will hereafter refer to the volume as DSI.

quantitative and qualitative approaches within one research design. The contributing authors have gone on to publish numerous books and articles on quantitative and qualitative methods. Indeed, one could observe that RSI spawned a veritable cottage industry in writings on qualitative methods, and is part of a recent reinvigoration of serious rethinking of qualitative and mixed-methods approaches.<sup>5</sup>

The immense contribution of RSI notwithstanding, qualitative methodologists still refer to DSI, often in the opening lines of their treatise, and still frame the current methodological debate as one of qualitative vs. quantitative approaches, or DSI vs. RSI. However, since RSI's publication in 2004 and the subsequent or perhaps concomitant growth of writings on qualitative methods, there have emerged several, and we feel critical, cleavages within the field of qualitative methods. This paper addresses these cleavages and attempts to put forth what we feel are the ontological and epistemological sources of division.

The paper is divided into four parts. The first part synthesizes the critique of DSI found in RSI as a starting point for the qualitative divisions that would later emerge. The second part provides an overview of the major divisions in qualitative methods as we define them: the quantitative emulation approach, eclectic within-case and small-N comparative approach, set theoretic/Boolean approach, and empirical interpretivism. We also identify the source of these divisions: different understandings of the nature of causation and knowledge claims rooted in different ontological and/or epistemological standpoints.<sup>6</sup> We will argue that the empirical interpretivist approach has often been excluded from consideration as a qualitative approach largely due to epistemological differences, while the other three camps, though often lumped

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<sup>5</sup> CQR, IQMR, the qualitative methods section in APSA (now known as the mixed and qualitative methods section).

<sup>6</sup> While there are other epistemes within the social sciences (non-empirical interpretivism, such as Derrida for example), we limit our scope to those that consider it possible to study the empirical world.

together, differ on important ontological issues. The third part discusses one issue raised in RSI's critique of DSI (selection bias) and how each of the four camps treats this issue. We conclude with a discussion of what we feel to be the most important difference between the four approaches, namely, divergent standards, along with some interesting commonalities, such as the use of counterfactuals, an iterative process as a prescription for research design pitfalls, and, most importantly, a close engagement with the cases.

### **RSI's Critique of DSI**

The 2004 publication of RSI was a critical moment for the field of qualitative methods. Since the behavioral revolution, little had done so much to remind scholars that qualitative methods were not only useful for research, but could also be rigorous, more than just "telling stories." To be sure, many scholars had not forgotten this, but curriculum in graduate departments and publication trends in leading journals had come to privilege quantitative approaches over qualitative ones.<sup>7</sup>

For those utilizing qualitative methods, the definitive guide to research design was, until 2004, DSI and its admonition to qualitative researchers to walk and talk more like quantitative ones. However, with RSI and the elegant critique of its many contributing authors, among them both qualitative and quantitative methodologists, those researchers wishing to use qualitative methods had a new source for guidance. RSI set the standard for qualitative research, enumerating several problems with transplanting the quantitative template wholesale to small-N studies, while also detailing how qualitative research programs, such as case studies, process

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<sup>7</sup> This point has not been lost on many qualitative researchers. George and Bennett, Yanow and Peregrine Shaw, Mahoney

tracing, and small-N cross-case comparisons, can shed light on entrenched theoretical debates. This section summarizes the crux of RSI's rejoinder to DSI.

The contributors to RSI approved of DSI's central mission to present political science with a set of criteria for establishing good research, agreeing that creating standards to which all research could be held was a valuable goal.<sup>8</sup> They also acknowledged that DSI provided much practical advice to the social scientist. This included such notions as: contributing to a scholarly literature, constructing falsifiable theories that are logically consistent, understanding that both description and causal analysis require making inferences, and seeking to make research replicable (Collier, Seawright and Munck 38-44). However, there remained hearty disagreement: out of the 54 pieces of advice that Collier, Seawright and Munck distilled from DSI, they claimed that the contributors to RSI agreed with 14 of them. In other words, while there was much to learn from DSI, there was much more that was contrary to research advice from the qualitative tradition. Particular points of divergence were grouped into five categories:

1. Under the topic of causal inference, the contributors argued that while paying attention to the assumptions that comprise the "logical foundations" of inference was necessary as DSI suggests, King Keohane and Verba did not go far enough in spelling out what this meant (RSI 46). Specifically, though the topic of omitted variables is dealt with in DSI, not enough is said to help researchers distinguish between extraneous variables that can be left out of a causal story and those whose absence would have a confounding effect upon a theory. Likewise, the exhortation to avoid endogeneity leaves unanswered why and in what ways endogeneity may pose substantive or theoretical problems. RSI argues that the solutions to such problems offered by DSI are derived from quantitative techniques, and may, therefore not apply in a small-N and/or case study environment. Further, RSI points out that such

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<sup>8</sup> This idea of a shared standard will be more fully analyzed later in the paper.

advice is also offered without recognition of the difficulties causal inference entails both for qualitative and quantitative scholars (RSI 46-47).

2. Many important methodological issues are mentioned in DSI, but given insufficient attention. Topics that are neglected include:
  - a. How to limit one's analysis to the range of variation that is relevant to a researcher's theory,
  - b. How to solve issues of indeterminacy through such qualitative means as "strong theory"
  - c. How to ensure that measurements of one's variables are valid and reliable within the context of a given research design (RSI 47).
3. Many pieces of advice that DSI offers cannot be applied in practice. One example deals with measurement error, which DSI reminds scholars to avoid as much as possible and to pay attention to the implications it has for causal inference. According to Bartels, however, it is often difficult to know what the consequences of measurement error may be even in a quantitative framework, let alone a qualitative one where the notion of measurement itself may be more malleable. Similarly, Brady and McKeown take issue with the advice that where causal inference may be challenging a researcher should stick to descriptive inference. They do so because causal inference is generally considered superior (and indeed is argued to be the ultimate goal even in RSI) in political science, and therefore to end with description may doom a research project (47).
4. Much of the advice given in DSI entails trade-offs that the authors are mostly silent about. For example, DSI suggests increasing the number of observations in a study, especially in cases where indeterminacy may be an issue, but they do not fully acknowledge or offer solutions to the problems of independence and causal homogeneity that this practice is likely



to cause (47). Another trade off is choosing concepts that are concrete, and hence easily measured. While making the research project easier to carry out, much may be lost about the question that the researcher wants to answer, as most questions in political science grapple with complex or contested concepts.

5. Finally, RSI points out that DSI overlooks many valuable tools present in the qualitative tradition – practices that could benefit quantitative projects as well as qualitative ones. These include concept formation and process-tracing techniques (48). Moreover, interpretation, as a technique for gaining fine-grained description is wrongly dismissed according to RSI as being no less stylized than formal modeling, since (argues DSI) even the thickest of descriptions pales in comparison to reality (48).

Overall, the authors in RSI attempt to show that the quantitative framework offered in DSI has many drawbacks and potential difficulties even for researchers using regression techniques, relies upon several assumptions that may not hold true especially in studies of small N phenomena, and may be completely inappropriate for many research questions. The seminal points are that there are times when selecting cases upon the dependent variable is essential for conducting good research, no-variance research designs have their place in good scholarship, increasing the number of observations should not be done indiscriminately, researchers should be paying as much attention to concept formation as concept measurement, and as much attention to causal mechanisms as causal inference. DSI may or may not have fulfilled its mission to create a reference book for scholars wishing to conduct good social science, but it did bring the qualitative world together and forced these researchers to become more self-aware of

their own methodology and more systematized in presenting their methods to the discipline at large.

### **Some Cleavages Emerge**

Given some of the striking differences that have emerged within the field, perhaps qualitative methodologists were not brought together for long. The thrust of our argument is that many of the developments in qualitative methods, particularly, but not limited to the last ten years, have tended to find qualitative methodologists divided into one of four camps: quantitative emulation, eclectic small-N/within case comparativism, set-theory/Booleanism, and empirical interpretivism.<sup>9</sup> At the heart of these cleavages is a fundamental disagreement over ontology and epistemology that, we feel, leads to divergent understandings of the standards qualitative work should be judged by.

For the quantitative emulator (QE), the statistical or experimental template is seen as the gold standard that all studies should try to live up to, though they acknowledge that some questions cannot be answered using these techniques alone. King, Keohane and Verba certainly fall into this camp, as does Gerring and several chapters of the RSI volume. This should not imply that QE feel qualitative work should take a backseat to quantitative studies, but that the two are complementary, and that as far as possible when utilizing qualitative techniques, the evaluative tools used in the quantitative analysis should be applied to qualitative studies as well. At its heart, QE holds that quantitative and qualitative researchers share ontological and epistemological assumptions about the world, but should embrace methodological diversity as a means to enhance analytical leverage on empirical puzzles. Within the QE approach, the highest goal of research is prediction and the approach is variable-oriented.

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<sup>9</sup> We are indebted to Colin Elman for suggesting these categories.

The second camp we call eclectic small-N comparativism. This camp overlaps in some ways with the three others. With the QE approach, they share the search for common standards with quantitative work. With the set-theorists (and empirical interpretivists for that matter), they share a serious skepticism regarding generalizability. With the empirical interpretivists they share a mechanistic view of causation, directing them to focus more on causal mechanisms than effects of treatments or causal conditions. Eclectics, more so than other camps, rely on identifying tightly coupled causal sequences in order to explicate causal processes. The goal of this research is elucidation and its approach is process-oriented.

The third camp is one we call the set-theoretic or Boolean approach. Members of this camp tend to disagree with both QE and eclectics on specific issues, such as the suitability of using mixed-methods to answer certain questions. Set-theorists also employ a different notion of causation (i.e., deterministic) emphasizing its often asymmetric nature as being unsuited for measurement tools that are meant to uncover linear and additive types of causal relationships. The goal of research within this camp is explanation, and research is case-oriented.

The fourth camp is empirical interpretivism, an approach that emphasizes the constructed nature of social reality. Interpretivists find problematic attempts by all other camps (and certainly, quantitative methodologists as well) to observe political phenomena and gather facts about it, arguing that the social world is created through interaction and known through understanding. The goal of this type of research is understanding, and the approach is meaning-oriented.

What are the roots of these differences in the qualitative tradition, and what are their commonalities? We argue that the differences stem from divergent ontological and epistemological assumptions, but the commonality on which they hang is a close engagement

with cases. For QE and set-theorists, their differences are mostly ontological, that is, they differ in their understandings of the nature of causation. While QE's approach to causation resonates with the quantitative template, set-theorists insist that the type of causation a qualitative researcher is most concerned with cannot be captured by regression analysis. Both the QE and eclectic approach has a more encompassing take on causation, deploying methodological tools to capture both probabilistic and deterministic causation. Set-theorists, on the other hand, while not dismissing probabilistic causation in general, tends to have at its center a greater concern for research agendas that emphasize deterministic causal conditions. However, unlike the QE set-theoretic approach, eclectics tend to emphasize more the mechanisms through which a cause exerts its effect. The tools of QE and set-theory, regression and fuzzy set/QC analysis respectively, while capable of identifying the 'what' of a problem, are less well-suited to isolating the 'why' and 'how'. In other words, both regression and fuzzy set/QC analysis can identify the causes of effects and the effects of causes, but they cannot tell us how a cause exerts its effect. It can only tell us that it does exert an effect.

Empirical interpretivists, on the other hand, differ widely from the three other camps in terms of both ontology and epistemology. However, we argue that it is the *epistemological* assumptions, not the ontological differences, of empirical interpretivism that serve to set a wider gulf between this approach and the three others. Ontologically speaking, empirical interpretivism draws on constructivism, in the sense that the social world is thought of as one in which reality is constructed through the interactions of individuals and the mutually constitutive relationship between agency and structures. Epistemologically, interpretivists feel that knowledge is generated through understanding and interpretation of the world around us. This goes against positivist assumptions that the world is an entity that exists outside of the individual, and that, as

researchers, we can know the world through objective fact gathering. While this differs widely from the other camps ontological and epistemological assumptions, we argue that the methodological distinction of the interpretivists stems largely from epistemological differences, not ontological ones. We argue that the ontological assumptions that form the foundation of the interpretivist approach are not irreconcilable with either the QE, eclectic or set-theoretic world view. Rather, it is the requirements of the interpretive epistemology that lead to divergent methodological practices. Additionally, we feel that empirical interpretivists rightly belong in the qualitative camp due to their close engagement with cases and use of standard qualitative tools, such as counterfactual analysis and process tracing.

We wish to be clear that these different approaches, as we describe them, are not meant to indicate that there exist four monolithic, homogenous, mutually exclusive frameworks. There are many scholars whose work can be described with reference to more than one approach, and within each approach there are disagreements over some issues. The purpose of this paper, however, is to discuss some of the diversity within the overall qualitative tradition, in an attempt to resituate certain debates that have taken place between either the quantitative-qualitative or positivist-interpretivist divides.

### **Quantitative Emulation**

In order to better unpack the notion of QE it may be easiest to conceive of it as a family resemblance concept. A minimalist definition of this camp would include a drawing from the following characteristics<sup>10</sup>:

- concern with generalizable findings

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<sup>10</sup> Some of these characteristics will be used by the other camps as well.

- use of probabilistic logic
- use of case studies solely (mostly?) for hypothesis testing
- adherence to the concept of the scientific method
- acceptance of a “real world,” or use of “as if” assumption; rejection of subjective reality arguments
- prioritizing causal inference to description

DSI, itself, is perhaps the quintessential example: the application of quantitative methods to qualitative studies. However, most mainstream political science that follows a mixed-method template or uses process-tracing/case study techniques as a moment for hypothesis formation rather than theory testing would fall into the QE approach. Indeed, the ability to “cross-fertilize” is a hallmark of the QE project. While DSI make the move from quantitative to qualitative, Gerring and many of the authors represented in RSI make the point that qualitative tools can (and should, according to RSI) be utilized in quantitative studies. For example, Gerring writes, “Process tracing and pattern matching may be undertaken with large samples and standard (“dataset”) observations” (Gerring, forthcoming, 268). Indeed, this idea of a shared ontology/epistemology lies at the heart of the RSI project, where the authors nearly tie themselves into definitional knots in an attempt to argue against DSI’s advice while finding standards of causal inference that can be shared across the discipline (more on this later).

When a different epistemology arises, such as is overtly the case with interpretivism, QE may dismiss these pursuits as no longer truly scientific (Gerring forthcoming).<sup>11</sup> By making this move, it is possible for the quantitative emulator to continue claiming “shared standards”

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<sup>11</sup> This is not to argue that these projects are not seen as important contributions to the field, but rather that they are entirely different pursuits that are not commensurate with the rest of political science.

between the quantitative and qualitative frameworks.<sup>12</sup> QE is thus tightly wedded to the scientific method, even when certain circumstances lead them away from its traditional tenets, such as random case selection. As a side note, though they may be associated more with a QE project, neither the use of numbers nor sample size necessarily dictates which framework a qualitative study will follow. QE projects may stem from a few case studies, while QCA, which utilizes Boolean algebra, may have many cases and translates qualitative measures into numbers ranging from 0 to 1.

Beyond the belief in a shared epistemology (and possibly a shared ontology with the quantitative world, it is the preeminence of certain criteria within the standards for a valid causal inference that define QE. Causal inference, for QE, is generally seen as predicated upon falsifiability and generalizability. A QE perspective on causality is rooted in probability and based upon counterfactual reasoning. For example, DSI defines causality, or a causal effect as the “difference between the systematic component of observations made when the explanatory variable takes one value and the systematic component of comparable observations when the explanatory variable takes on another value (81-82). In arriving at these two values, however, it is often necessary, they argue to generate a counterfactual example against which the observed data can be compared.

Similarly, Gerring states that “to say that  $X$  is a cause of  $Y$  is to say that a change in  $X$  generates a change in  $Y$  relative to what  $Y$  otherwise would be (the counterfactual condition), given certain background conditions and scope conditions (*ceteris paribus* assumptions). Another way of saying this is that a cause, if it is indeed a cause, raises the prior probability of an

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<sup>12</sup> This does not seem an entirely unprejudiced move, and akin to the quantitative field dismissing qualitative work for lacking a mathematical basis.

outcome occurring,” (Gerring 336). Though Gerring acknowledges that the logic of necessity and sufficiency would generally seem to go against this definition, he argues that in reality one need only understand a necessary cause as having a probability equal to zero when the cause is absent, and a sufficient cause as having a probability equal to one when the cause is present (Gerring forthcoming). This is quite distinct from the set-theorists’ perspective, as will be discussed below.

The definition given in RSI for a causal effect is also similar: “the impact of a given explanatory variable on a particular outcome. More specifically, other things being equal, the causal effect is the difference between the two values of the dependent variable that arise according to whether an independent variable assumes one of two specific values,” (RSI, 275-6).<sup>13</sup> This would follow from the argument that QE sees a shared ontology and epistemology with quantitative research, as RSI claims.

This understanding of causation leads the QE framework to a concern over variation, often resulting in the rejection of a no-variance research design, and, by extension, studies in which  $N=1$ . It is acknowledged that no-variance designs may have their place, such as in discerning a necessary cause, but for the QE approach, without a control against which to test this, the necessary condition may remain trivial. Further, the problem of confounders becomes especially problematic in such research designs. As Gerring states:

In observational settings, it is highly desirable to identify settings in which  $Y$  actually does vary (rather than is simply free to vary, from some hypothetical perspective). Here, we cannot manipulate  $X$  and we may have much less assurance about  $Y$ ’s capacity to

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<sup>13</sup> Compare this to Mahoney’s conception of causality, under which the notion of the counterfactual allows for only one type of cause: necessity. As he writes, “a cause is something that—when counterfactually taken away under *ceteris paribus* conditions—fosters a different outcome ... Although useful, this approach cannot accommodate sufficient causes. With sufficient causes, the counterfactual absence of the cause may not change the outcome in an individual case and thus could be interpreted as not exerting an effect under the necessary cause definition. (Mahoney 2008, 418).



respond to a given change in *X*. Confounding factors are legion, and a stable outcome could therefore be credited to many things.

Finally, the notion of the scientific method leads QE toward another goal that the other camps may not value. This is generalizability.

### *Generalizability*

One of the standards of good causal inference insisted upon in DSI is the ability to make generalizable statements from a limited amount of evidence. Many of the suggested techniques generated from the quantitative framework are attempts at achieving this goal. In this regard QE will agree with the advice of King, Keohane and Verba. However, this is usually because the goal is one of generality, and not necessarily because a researcher has a predisposition for the quantitative framework. For example, Gerring argues explicitly that a goal of scientific research is to be general, to make as broad a claim as possible (Gerring, forthcoming).<sup>14</sup> That this is an *ontological distinction* is made apparent by a comparison between the ways in which QE and eclectics or set-theorists conceive of classic small-N studies. Skocpol's work on social revolutions is generally held as a model example of a small-N study, though the set-theorists also see within it a great use of determinist logic, utilizing the logic of necessity and sufficiency. It is believed to explain an entire universe of cases and be limited to these cases. However, if one is wedded to the project of generalizing, then one would see in Skocpol the ability to create a generalizable theory that could predict social revolutions in other contexts. (Indeed, Gerring makes this argument (Gerring, forthcoming). In regards to the topic at hand, whether this is true or not is less important than whether Skocpol's work would be less important if it is not true. QE would tend to relegate any theory incapable of at least a certain amount of generalizability to a

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<sup>14</sup> To be precise, Gerring acknowledges that this comes with a *ceteris paribus* caveat, and often this is a caveat that is not always easily met.

substandard position, whereas eclectics or Booleans would see in it a simple adherence to scope conditions, and empirical interpretivists as a condition of remaining closely attached to the subject's view of reality. The first two camps would not fault a project for being non-generalizable, seeing it more as a prudent move, while the interpretivists might very well dismiss a project for attempts at generalization.

### *Concepts and measurement*

A final area where QE may be distinguished from set-theoretic or empirical interpretivist approaches is with concepts. On the surface, one marker of the ontological distinction between the QE approach and qualitative scholars in the set-theoretic and interpretivist camp is that concepts are often referred to using quantitative jargon, such as the use of the phrase “independent variable” instead of “causal condition” or “dependent variable” instead of “outcome.”<sup>15</sup> This may seem a superficial distinction, but different phrasing of a theory may very well lead to (or perhaps indicate, or constitute) a different sort of causal inference, which (as noted above) may be an important difference.

A deeper distinction, though, arrives with the problems of measurement. While many from the QE camp use thick descriptions and precise definitions, in much the same way as other qualitative scholars, measurement is often more of a concern. This may lead to the use of instrumental variables or indicators more frequently than one might see in, for example, set-theoretic work.

To summarize, the QE framework is usually in epistemic agreement with a quantitative world view, seeing causation as probabilistic and remaining tightly bound to the notion of the scientific method and its search for generalization and external validity. On occasion, this leads

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<sup>15</sup> Eclectics tend to use both sets of jargon.

to the attempt at methodological cohesion with the quantitative framework. On some points QE agrees with the eclectic and Boolean approach, especially epistemologically, but on many specific research concerns it remains a disparate project. With interpretivism, the QE framework shares relatively little, as they are unwilling to make a move away from the precept of objectivity.

### **Within-case/small-N Eclectic Approach**

Small-N and within case comparativists have in common with QE the belief that all social science works from a similar epistemology, though different research methods abound.

For example, George and Bennett argue:

While case studies share a similar epistemological logic with statistical methods and with formal modeling that is coupled with empirical research, these methods have different methodological logics. Epistemologically, all three approaches attempt to develop logically consistent models or theories, they derive observable implications from these theories, they test these implications against empirical observations or measurements, and they use the results of these tests to make inferences on how best to modify the theories tested (George and Bennett 2005, pp 6).

However, within this pluralistic approach, eclectics tend to resort to identifying temporal sequences and causal mechanisms through the use of process tracing and paired comparisons. Indeed, identifying the causal links leading to an outcome is the basis for causal inference in these comparative studies. The more closely the linkages can be made the better the case for causation. This stands in distinction to QE's reliance on the number of observations to build causal inference.

Additionally, whereas the set-theoretic approach looks for instances of equifinality (where multiple causal pathways lead to the same outcome in different cases), eclectics tend towards a typological approach that attempts to explain a distinct path taken in each case to a variety of different outcomes. For instance, a set-theoretic approach might look at different

empirical instantiations of a particular outcome, and identify the different types of causal combinations that lead to this same type of outcome in different cases. Skocpol's work on social revolutions would be one such example.

The eclectic approach, on the other hand, often allows for more variation on the dependent variable than either set-theory or QE, and describes how the varied outcome occurs in each, unique case. Peter Evans's *Embedded Autonomy* is one example, where he explains economic development in late industrializing states as a function of whether the state is both embedded and autonomous. He identifies the concept of an embedded autonomous state, one in which the state has connections with, but is still insular from, business. He then identifies South Korea as a state that was able to industrialize despite its late start because of its embedded autonomous state, and then traces the process by which this occurs, explicating the mechanisms that link embedded autonomy to economic development in late industrializing states. Were Evans interested in equifinality, he might then identify other instances of successful economic development in late industrializing states, looking for different causal combinations that led to this particular outcome. Instead, he looks at other late industrializers that were not as successful as South Korea and demonstrates that this was because the states in question were either embedded but not autonomous (Brazil), or autonomous but not embedded (India).

A final attribute that this eclectic camp shares, in part, with the set-theorists is a serious interest in concept formation. Less concerned with measurability than QE, the eclectics focus instead on accuracy, resonance and scope, and tend to work with thick, multi-dimensional concepts. Skocpol's concept of a social revolution is, again, a good example of this work.

### **Set-theoretic/Boolean approach**

The set-theorist split from the QE camp, and to a lesser extent the eclectic approach, is premised on ontological grounds. Mahoney and Goertz draw on Hume's discussion of causation to illustrate the two different approaches to causation. They refer to Hume's definition of cause:

We may define a cause to be an object followed by another, and where all the objects, similar to the first, are followed by objects similar to the second. [definition 1]...Or, in other words, where, if the first object had not been, the second never would have existed. [definition 2] (David Hume in *Enquiries Concerning Human Understanding, and Concerning the Principles of Morals* 1975 [1777] (Cited by Goertz and Mahoney 2010).

Mahoney and Goertz call these two definitions the "counterfactual definition" and the "constant conjunction definition" and argue that this distinction underlies some of the primary differences between quantitative and qualitative approaches. Indeed, for Goertz and Mahoney (2006), practitioners of the two methods reside in different 'cultures' and it is diverse cultural understandings that represent diverse practices and lead to communication difficulties. They contrast research traditions between the two approaches in ten areas, such as approaches to explanation, use of multivariate models, case selection, scope and causal generalization, and concepts of causation. It is this last issue that we focus here as being fundamental to the difference between QE methodology and set-theorist methodology.

As we have discussed above, the QE and eclectic approach stresses the "shared standards" but "diverse tools" of both quantitative and qualitative research traditions. This search for diverse tools amidst shared standards tends to preclude the problematization of just what type of causation our diverse tools are meant to capture. Mahoney and Goertz (2006) contrast quantitative and qualitative approaches in terms of, among other things, causation, claiming that quantitative scholars think of causation as correlative and additive, while qualitative scholars think of cause in terms of logic and set theory.

We feel, however, that when Mahoney and Goertz discuss 'qualitative scholars', they are really referring to *set-theoretic* qualitative scholars. While the QE and often the eclectic approach

certainly make room for logic, its adherents do not consider this to be the only way to think of causation. For example, George and Bennett (2005), while claiming that case studies are "a powerful means of assessing claims of necessity or sufficiency," (26), warn against relying too much on claims of necessity, stating that, among other things, "whether a factor is necessary to an outcome in a case is a separate issue from *how much* it contributed to the magnitude of the outcome," (27, emphasis in the original). This caveat calls to mind what Mahoney and Goertz claim to be one of the hallmarks of the quantitative template: on-average effects causation, where the increase in the value of the independent variable produces a concomitant increase or decrease in value on the outcome (Mahoney and Goertz 2006:232).

Earlier in the paper we refer to the set-theoretic approach as one that has as its research goal *explanation* and is case-, rather than variable-, oriented. To elaborate on this first point, we point again to Mahoney and Goertz's work on the cultural differences between research traditions. They argue that another major difference between quantitative and qualitative research (and, as we claim, with the QE and eclectic approach as well) is the means of explanation each employs. According to Mahoney and Goertz's cultural approach, quantitative scholars favor an "effects-of-causes" approach, while qualitative scholars prefer a "causes-of-effects" approach. Statistical research designs attempt to recreate the controlled experiment, where "one seeks to estimate the average effect of one or more causes across a population of cases. The explanation of specific outcomes in particular cases is not a central concern," (Mahoney and Goertz 2006: 230). Estimating average effects of causal variables in order to generalize to a larger population brings the quantitative researcher closer to the ultimate goal of prediction. In other words, quantitative (and QE) researchers do not attempt to estimate the average causal effect of economic development on democracy in order to understand why democracy *did occur* in

economically developed countries, but rather to better understand what effect, if any, future economic development might have on democratizing countries. Prediction lies at the heart of this endeavor.

The eclectic approach to predictive theories is much more moderate and emphasizes the importance of both historical explanation and nomological generalization: "While social scientists should aspire toward predictive theories...they should also recognize the value of good historical explanations of cases as well as that of law-like generalizations," (George and Bennett 2005: 131). Here, prediction is the highest goal, though it is acknowledged that this goal is not always achievable, given the reflexive nature of social subjects and their ability to act strategically to change their social environment. Prediction, in that it extends to the future, is thought of as more broad and relevant than theories that only explain the past (Gerring 2001:125).

But the set-theoretic approach is more grounded in the tradition of explanation, or "effects-of-causes." Mahoney, Kimball and Koivu<sup>16</sup> argue that explanation is the goal of qualitative research and provide a model, the elaboration model, to assist researchers with the difficult task of evaluating competing historical explanations. The model relies on set theory and logic coupled with causal process observations to develop logically precise statements of the relative importance of causal conditions in single cases. The undertaking is premised by the idea that qualitative social scientists primarily "seek to explain particular outcomes in one or more specific cases."<sup>17</sup>

The Boolean approach also embraces fuzzy set and qualitative comparative analysis (fs/QCA), a tool for establishing necessary and sufficient (or jointly sufficient) causal conditions

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<sup>16</sup> 2009

<sup>17</sup> Mahoney, Kimball and Koivu 2009: 1

using set theory and Boolean algebra.<sup>18</sup> This technique relies on Hume's counterfactual definition of cause and highlights the causal complexity and asymmetric nature of social phenomena over linear, additive, and probabilistic approaches.

Finally, a comparison of the ways QE, eclectics and set-theorists analyze Skocpol's famous work on social revolutions exemplifies the ontological distinctions between these three camps. Epistemologically, all three camps appear to agree that Skocpol's mechanism of case study research is satisfactory for creating knowledge. However, for QE, as was indicated earlier in the paper, Skocpol has generated a theory on social revolution that is precise and generalizable enough to use in predicting other potential social revolutions. For the eclectics, it is a quintessential comparative study that uses close sequential analysis of causal mechanisms to explain how social revolutions occurred in France, Russia and China, whereas for the set-theorists, Skocpol is best understood as defining the necessary and sufficient conditions for social revolutions.

### **Empirical Interpretivist Approach**

The empirical interpretivist methodological approach is grounded in specific ontological and epistemological assumptions, in opposition to various strands of positivism, though often specifically critical and logical positivism. The positivist worldview includes the "notion that theory is the result of observation, the result of systematization of a series of inductive generalizations, and the result of the accumulation of an interrelated set of scientific laws, theory is logically prior to the observation of any similarities or regularities in the world; indeed, theory is precisely that which makes the identification of regularities possible," (Hawkesworth 2006: 32). Interpretivism, on the other hand, embraces constructivist ontology, that is, that social reality

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<sup>18</sup> For a detailed discussion of fs/QCA techniques, see Ragin 2000, Ragin 2008, and Rihoux and Ragin 2009.



is created through shared interactions, and an interpretivist epistemology, that we 'know' through understanding and interpretation. The social sciences are unique in the respect that both the subject of study and the performer of the study are the same type of creature, and ones that make meaning at that. "Unlike...rocks, animals and atoms, humans make, communicate, interpret, share, and contest meaning. We act; we have intentions about our actions; we interpret others' actions; we (attempt to) make sense of the world: We are meaning-making creatures," (Yanow 2006: 9).

We argue that, while the differences in ontological assumptions between interpretivists and qualitative positivists are by no means insignificant, it is not on these grounds that interpretivists diverge in their methodological leanings. The constructed nature of *social* reality is not, by itself, completely foreign to positivist qualitative methodologists. If this construction meant *also* that the action of the universe was not of a causal nature, then positivist qualitative methodologists would be skeptical. But this constructivist ontology does not preclude the existence of causal phenomenon:

Interpretive researchers are coming to better understand the ways in which they can reclaim this powerful term (causation) on their own ground. Two interrelated moves are possible. One is to reorient the understanding of causality from general laws to specific cases. Telling quite specific causal stories about how a policy was implemented (as done e.g., by Pressman and Wildavsky 1973) is not inconsistent with interpretive presuppositions; far from it. This is "Sherlock Holmes causality"- a careful mapping of clues in context, a tracing of connections among events. Interpretive methods lend themselves very well to demonstrating such causality.

The related move, which strengthens the previous one considerably, is to note the role that human meaning making plays in action. Human meaning making and beliefs are understood as "constitutive of actions," and this view of causality can be used to explain not only individual actions but also the "broad patterns of behavior associated with social movements" (Bevir 2006). In sum, interpretive researchers have a lot to contribute to understandings of causality in ways that broaden the conceptualization of causality beyond the variables-based, explanation-prediction, general law model. (Schwartz-Shea 2006:108-9)

Instead, it is the epistemological assumptions that underwrite the interpretivist approach that lead them to diverge methodologically from the three other camps. The process of *knowing*, that is, interpretation, requires interpretivists to take seriously language, symbol systems, and individual perception.

And it is not just the researcher that interprets and makes meaning of the world around her, but her research subjects. Weldes's subject of study is not just the Cuban missile crisis, but why the individuals involved perceived the situation as a crisis (1999). For Weldes, not only is the knowability of the Cuban missile crisis captured through her own interpretation and sense-making processes, but *what* she wishes to know about the crisis is how others interpreted and made sense of it. Interpretation, then, is not only the *method* of study, but also the *object* of study. Separate from questions of the reality status of the subjects of study, it is this aspect of interpretivism, based on this specific approach to knowability, which requires the empirical interpretivist to systematize a set of methods apart from their more positivist-oriented counterparts in the QE, eclectic or set-theory traditions.

### **How the Assumptions You Make Affect the Methodological Advice You Give**

We have, until this point, spoken of the four qualitative approaches in broad terms. We turn now to a discussion of how these methodological differences, rooted in differences of ontological and epistemological assumptions, lead to variation in the prescriptions for resolving particular methodological sticking points. We take up an issue that has been discussed at length within RSI's critique of DSI and numerous other scholarly works: selection bias, and the larger issue of case selection.

Though many scholars from the QE camp agree fully with the advice found in DSI, all four qualitative approaches contain elements that disagree with DSI's overall prescription for resolving selection bias in small-N studies. Moreover, except for QE, the camps agree that DSI does not adequately identify the problem of selection bias in small-N analysis, instead transplanting wholesale the issues quantitative researchers bump against in statistical analysis. For the QE approach, there is no attempt to deny that selection bias in quantitative studies is very real, in the sense that truncating your sample on the dependent variable can flatten the slope of the regression line, leading the researcher to underestimate the causal effects of the independent variable in question. Furthermore, in cross-case analysis, QE tends to see a correlative moment to regression analysis. "[I]n some respects," write Collier, Mahoney and Seawright in reference to cross-case analysts, "[they can] be seen as doing 'intuitive regression,' and correspondingly, the issue of selection bias arises" (RSI 94).

The other camps agree with QE, as presented in DSI, however, that in small-N analysis random sampling cannot solve for the problem of selection bias. George and Bennett discuss the trade-offs and pitfalls associated with case studies and though selection bias can be an issue, it doesn't happen in the ways the quantitative approaches predict. Selecting cases on the dependent variable can enable the researcher to determine whether particular causal conditions are not necessary or sufficient, or can act as a heuristic tool in the early stages of research to identify potential causal paths or previously omitted variables. However, selection bias can become a problem for the small-N researcher if "Cases whose independent *and* dependent variables vary as the favored hypothesis suggests, ignoring cases that appear to contradict the theory, and overgeneralizing from these cases to wider populations," (George and Bennett 2005: 24).

Gerring, likewise, sees selection bias as a serious problem; though for him it is a subset of the larger issue of the assignment problem. In large N studies, he argues that the only solution to selection bias is randomization, writing: "...random (or as-if random) assignment is a necessary *but not sufficient* condition for valid causal inference." (Gerring, forthcoming: 217). In case study research, however, he is in agreement with DSI and RSI, writing that randomization would likely *lead* to problems of selection bias rather than solving for them (Gerring 2007). Rather, he suggests that cases be chosen on the basis of several possible techniques, including choosing for a typical case, a diverse set of cases, or a crucial, deviant, or extreme case to list just a few (Gerring 2007). In any study, however, the assignment problem remains, and must be dealt with for Gerring. This is representative of the QE perspective in which any case selection will represent a larger population.

The set-theoretic approach to selection bias and case selection more generally, points to an additional problem of selection bias, and this is a direct result of its underlying assumptions of deterministic causation. While set-theorists agree that selection bias can be a problem, they add that it can also lead you to *overestimate* your causal effects sometimes if you *do not* truncate your sample; in fuzzy sets, when running tests of necessity (for example) you must exclude from testing all cases for which the outcome is not present (or has zero membership). Any causal condition/outcome pair in which the membership score on the causal condition is greater than or equal to the membership score on the outcome is counted as a necessary cause. However, if the score on the outcome is zero, any score on the causal condition will be greater than or equal to it. Your results will be biased towards accepting a causal condition as necessary rather than rejecting it. This is similar to discussions of trivial necessary causes, where a causal condition is present irrespective of the outcome. However, in order to guard against this type of bias, you

must be careful about selecting on the causal conditions (which is not a problem with regression analysis).

Empirical interpretivists rarely discuss issues of selection bias in the sense that set-theorists, QE, and quantitative researchers do. They are concerned with the bias of the researcher,<sup>19</sup> since the way a researcher “knows” their research subject is by interpreting it through their own theoretical lens, biased with their presuppositions.<sup>20</sup> Indeed, many interpretivists question whether objectivity is even possible, as interpretivist presuppositions include the lack of division between the researcher and the social world they study. But this is perhaps a separate topic, better discussed under the heading of *objectivity* rather than *selection bias*.

Case selection, on the other hand, most certainly belongs in a discussion of selection bias, for it is through our procedures for selecting cases that we attempt to avoid bias. Adcock (2006) indicates that interpretive researchers, particularly in the Geertzian tradition, use macro-societal comparisons as a way to “locate particular viewpoints in more general perspective,” (61). One way of doing this is by selecting research subjects that are ‘experience-distant’ as well as subjects that are ‘experience-near’. A similar technique is to choose subjects that represent both dominant discourses and subordinate discourses in order to gain leverage on concepts such as power and domination.

Wedeen (2008) lists four reasons why she felt Yemen was an ideal case for her study of nationalism: 1) lack of prior political arrangements that regulated membership in a state, 2) an

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<sup>19</sup> Interestingly, George and Bennett raise this issue as well (28) as a problem of bias not acknowledged by DSI.

<sup>20</sup> This is not necessarily a problem for interpretive research, in fact, it is inescapable, as the social world can only be known and understood through personal experience that, necessarily, varies from individual to individual. Interpretive researchers eschew the positivist requirement of objectivity because their epistemological assumptions do not allow for the possibility.

accompanying democratic transition, 3) ineffective state institutions incapable of inculcating national values, and 4) the experimental nature of Yemen's national unity advances our understandings of national identity formation more generally. (2-4)

### **Conclusion: Diverse Standards, Shared Tools?**

When RSI claimed that all social science should seek to meet the same standards of scholarship, but that different tools could be used, this did not seem a contentious claim. It is likely that King, Keohane and Verba felt the same about their contention that “the differences between the quantitative and qualitative traditions are only stylistic and are methodologically and substantively unimportant” (DSI 4).

Of course we all want rigorous studies that are capable of offering interesting and innovative knowledge to the field. But if this is all that the contributors to RSI meant, then their search for shared standards is so broad as to be meaningless. What then is the standard to which DSI and RSI hold political scientists? Are they the same between these two seminal works, and if so, do these standards represent *all* of political science? To begin, the difference between a standard and tool needs to be clearly demarcated, since they are so integrally related as to be obfuscating. The following are all definitions for a standard:

- something used as a measure, norm, or model in comparative evaluations (Oxford)
- a basis for comparison; a reference point against which other things can be evaluated (web)
- something set up and established by authority as a rule for the measure of quantity, weight, extent, value, or quality (Websters)
- criterion: the ideal in terms of which something can be judged (web)

A tool, on the other hand, is

- something (as an instrument or apparatus) used in performing an operation or necessary in the practice of a vocation or profession (web)

According to RSI, shared standards are “commonly accepted methodological norms for the conduct of research. The *overarching goals* of valid description and causal inference and of building theory are central to the idea of shared standards” (RSI 305). This overarching goal is placed in contrast to intermediate goals that are rife with trade-offs. Intermediate goals are those generally argued to be the standards of good research by other methodological handbooks, however, and conceivably form the basis for understanding the overarching goals. That is to say, exactly what constitutes “valid” inference remains indefinable without reference to these intermediate goals.<sup>21</sup> While the sentiment that “the idea of shared standards centrally involve the search for common criteria in evaluating and managing [the] trade-offs [among intermediate goals]” (305-6) sounds appealing, it essentially means that these common criteria have yet to be established or agreed upon. Moreover, these trade-offs form the crux to many of RSI’s arguments against the advice offered in DSI. Importantly, if this represents the QE perspective, and the eclectics, set-theorists and interpretivists share even less in common with the quantitative framework, then shared standards may not be reachable. For example, if one research tradition gives precedence to generality and another to precision, then research that fulfills one goal but not the other will be judged differently by these two traditions. And not just at an “intermediate” level, but as inferring something “valid” about the world. How complex this search for standards becomes, is made clear by a brief look at the criteria generally used.

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<sup>21</sup> Even within this definition, we are left with the ambiguous term “valid,” which is usually broken down into internal and external validity. These forms of validity are generally considered to be in tension, as research usually gains internal validity the deeper it is into a single case and external validity the broader it gets.

In summarizing what they hoped to push scholars to achieve through following their advice, DSI listed the following six goals as being especially important:

1. Explain as much as possible with as little as possible (parsimony)
2. Ensure the research question is falsifiable
3. Make the research replicable (use publically known procedures)
4. Test a theory with evidence not used to generate the theory [is this a tool or goal]
5. Minimize bias (accuracy)
6. Estimate uncertainty

Implicit within their work is the additional goal of generalizability.

RSI shared some of these prescriptions, advising scholars to contribute to a scholarly literature, construct falsifiable theories, be logically consistent, and seek replicability. But the notion of trade-offs receives serious attention, afflicting the goal of parsimony, accuracy and generalizability.

In a more recent work on social science methodology, in an attempt, perhaps, to find common standards, Gerring offers only two general criteria: discovery and appraisal. The standard of discovery judges whether a study is innovative, explorative or novel. It asks the question: “Is it new?” Appraisal, according to Gerring, is whether a study can be verified and justified: “Is it falsifiable?” (Gerring, forthcoming). Beyond these general criteria, though, Gerring offers other, rather more extensive, lists for judging a causal argument and analysis:

1. For the argument
  - a. Truth
  - b. Precision
  - c. generality
  - d. boundedness
  - e. parsimony



- f. coherence
  - g. commensurability
  - h. relevance
2. Analysis
- a. Accuracy (validity, precision, uncertainty, internal/external validity) ▪
  - b. Sample selection (size, representativeness) ▪
  - c. Cumulation (standardization, replication, transparency) ▪
  - d. Theoretical fit (construct validity, severity, partition, elimination)

In accordance with RSI, Gerring acknowledges that many trade-offs exist among the above mentioned criteria, but that they function according to *ceteris paribus* logic – all things being equal, all these goals are good. Individual researchers must decide when and to what degree to exchange such things as parsimony for accuracy. This leaves the standards up to significant debate, as the extensive work in RSI shows.

To reiterate, all of the lists above are standards, *not tools*: one can design studies that meet these criteria without any reference to methods such as statistics, process-tracing or particular techniques such as increasing the number of observations. Yet many of these standards are questioned in RSI, along with many specific tools. In particular, RSI is highly concerned with the trade-offs that may be present among the standards, citing in particular the problems between parsimony, accuracy, generality and causation (9). Moreover, standards that go against common qualitative research practices also receive criticism. For example, induction, used to some degree by both quantitative and qualitative researchers but most often in process-tracing or case-study analysis, almost by definition goes against the use of different evidence to generate and test a theory. Induction, as a tool, is given great weight in RSI, but it seems to go without notice that this means that a standard by which to judge research has been altered.

To conclude, RSI argues that “[w]hile analysts have diverse tools for designing, executing and evaluating research; it is meaningful to seek shared standards for employing such

tools. These shared standards can facilitate recognition of common criteria for good research among scholars who use different tools. Methodological pluralism and analytic rigor can be combined” (RSI 7). While we heartily agree with the last sentiment, a quick overview of current methodological works and handbooks belies the first idea. Indeed, even the authors of RSI go a great distance towards undermining this claim once it is agreed that the overarching goal is rooted in the criteria from their intermediate goals.

Ragin, in *Redesigning Social Inquiry*, pushes this argument further, showing that the set-theoretic camp does not choose the same standards, even as those chosen in RSI, writing “set theoretic arguments – the bread and butter of social science theory – should be evaluated on their own terms, that is, as (asymmetric) set relations and not as (symmetric) correlational arguments” (draft, p 2). To generalize his argument, any given style of methodology must be evaluated upon its own criteria. This holds true not only for the qualitative-quantitative divide, but also for the multitude of qualitative divisions. In the same way that RSI argues against DSI’s *belief in a* “shared logic of inference,” many interpretivists argue against the idea that causation should be seen as a higher goal than description. Indeed, they might well agree with DSI that where good causal arguments cannot be made a researcher should stay with good description. The caveat would be that good causal arguments are nearly impossible to make. This is not, however, because of an ontological assumption that precludes causality. This is due instead to epistemological commitments; to the knowability of reality when it is for each of us individually constructed: “Because constructivist ontology rejects the notion of an objective reality against which analysts test the accuracy of interpretations, “falsifiability” cannot be the goal.

Researchers can do no more than contrast interpretations against other interpretations. Not all interpretations are equally valid.”<sup>22</sup>

It appears, increasingly, that as different methodological traditions advance both in self-awareness and in actual technique that the schisms between them grow. The importance of this is twofold. First a researcher needs to know what makes good research. If we are correct in our assessment that different standards accompany different methodologies, then it matters a great deal how a researcher intends to pursue a question, and that she explicitly seeks to live up to the standards of the tradition(s) in which she finds herself. Second, it is unfair (although it will inevitably happen) to critique a research project against criteria that it never intended (nor needs) to live up to. To label an interpretivist piece as shoddy because it fails to meet positivist standards of external validity is a failure to judge it against the standards that interpretivism lives by. Likewise, a set-theorist’s study that does not have variation on the dependent variable is not immediately doomed, as Ragin and others have shown extensively. An awareness of the complexity of standards that pervade qualitative methodology is important as the discipline moves forward. Attempting to confine qualitative methods to a homogenous entity will only lead to bitter divisions in the discipline, hampering potential and fruitful discussions across these different traditions.

### *Commonalities*

Along with a close engagement of the cases, we feel that there are commonalities between all four approaches. Interestingly, though researchers across the various qualitative approaches may not share the same standards, they do seem to share the same tools. Case

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<sup>22</sup> Klotz and Lynch p. 106

studies, comparative and/or historical analysis, process tracing and counterfactual analysis are suggested by most qualitative methodologists, regardless of their ontological or epistemological bent. Interpretivists, where the gulf is widest from the positivist qualitative methodologists on both epistemological and ontological grounds, at the end of the day use tools that look remarkably similar to what qualitative positivists use. These techniques include triangulation, looking for negative evidence, checking the meaning of outliers, ruling out spurious relations, checking out rival explanations, replicating findings, and using extreme cases.<sup>23</sup> The QE, eclectic, and set-theoretic approaches alike would find this to be familiar advice. In her interpretive study of nationalist identity Wedeen discusses her field work methods: "I adopt a practice-oriented approach to language and other symbolic systems, while blending participant observation techniques with careful readings of texts," (2008:17). She elaborates on what she means by this, going on to say that she has conducted eighteen months of fieldwork over the last six years, and this fieldwork consisted of open-ended interviews with "ordinary men and women from diverse regions and class backgrounds, including politicians from various political parties," (17-18). Additionally, she participated in everyday Yemeni life, attending social gatherings and electoral events, as well as examined informal speech acts, all the while recording her observations in a field journal. Open-ended interviews, examination of texts and documents, and participation in social life are also part of fieldwork conducted by positivist qualitative researchers as well. It would appear that, though the other camps may believe that the researcher *knows* through observation and data collection, while the interpretivists feels the researcher *knows* through understanding and interpretation, they all go about these dissimilar processes by performing the same tasks.

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<sup>23</sup> Miles and Huberman 1994, cited in Schwartz-Shea 2006 98-9

Specifically, all four camps advocate the use of counterfactuals, particularly when the laboratory of the empirical world is so unkind as to present us with only one instance of a substantively important case, such as World War One. A counterfactual thought experiment is one in which, in order to determine whether X causes Y, one imagines a world in which X was not present, and then attempts to determine if Y could still have possibly occurred. They differ, however, in the way that they understand counterfactuals.

For QE, counterfactual reasoning is a necessary component of causal inference. When determining if an independent variable has impacted the dependent variable, it is often necessary to imagine what might have happened had the independent variable *not* existed. Gerring sees counterfactuals as taking the place of repeatable experiments where each variable could be manipulated to see the magnitude of effect it had upon an outcome. Since such experiments are not often possible in the social world, reasoned imagining must take their place. Importantly, probability often becomes a key component in counterfactuals within the QE framework. Since the presence of X is understood as raising the likelihood of Y occurring, when conceiving of the counterfactual, the question is often phrased in terms of “how likely” it would be to see Y in the absence of X. In essence, this changes the practice of the counterfactual exercise by broadening the thought experiment beyond a search for necessity (as the set-theorists would argue), but drawing the discussion away from other mechanisms that might also lead to the same outcome, as both the eclectics and set-theorists would be likely to do.

The eclectic approach also advocates for the use of counterfactual reasoning, but emphasizes its role in projects that seek to advance historical explanations for particular events. An important part of the eclectic undertaking, however, is first making sure that the case in question is explained through a "well-validated and explicit theory," (George and Bennett 2005

p. 168). The type of cause the independent variable is thought to exert is also important. Does it exert its effect individually, or in conjunction with other causal conditions? Does it exert its effect in a probabilistic manner? All these concerns must be taken into account, though they should not, in the eclectic perspective, preclude the use of counterfactuals entirely.<sup>24</sup> Rather, the eclectic approach emphasizes a logic of appropriateness, indicating that the best use of the counterfactual is in cases where "one or only a few decisive points in the historical case determined the outcome. Short-term causation is generally easier to address with a counterfactual than causation that involves a longer-term process," (George and Bennett 2005, p. 169).

The set-theoretic framework offers a similarly nuanced approach to the counterfactual on the grounds of types of causation, namely that it emphasizes that only a certain type of cause can be discerned through a counterfactual thought exercise: a necessary cause. A necessary cause is one in which the outcome could not occur without the presence of the cause, but the presence of the cause itself does not guarantee the outcome. As the purpose of the counterfactual is to imagine a world in which X was not present but Y did occur (thus indicating that X was *not* a cause of Y), this would only be revelatory if the relationship between X and Y was one of necessity. However, if the relationship between the two events was one of sufficiency, then imagining a world in which X was not present would not reveal anything about outcome Y. A sufficient causal relationship is one in which the presence of X guarantees the outcome of Y, but Y could have occurred through other means as well. In other words, though a counterfactual that finds a world in which X was not present and Y still occurred plausible, rules X out as a necessary cause of Y, X could still be a sufficient cause of Y. In order to perform a

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<sup>24</sup> For an excellent discussion of the usefulness and pitfalls of counterfactual analysis, see James D. Fearon "Counterfactuals and Hypothesis Testing in Political Science," *World Politics*, Vol. 43, No.2 (January 1991), pp. 169-195 along with Tetlock and Belkin's edited volume, *Counterfactual Thought Experiments in World Politics*.

counterfactual thought experiment that evaluated relationships of sufficiency, one must imagine a world in which X is present, and yet Y still occurs. This would rule out a causal relationship of sufficiency between X and Y.<sup>25</sup> However, such an experiment would suffer from the reverse problem, as it would only be meaningful if the causal relationship is one of necessity. For the set-theorists, both thought experiments must be performed.

Empirical interpretivists also utilize counterfactual analysis. Jutta Weldes does this implicitly when she asks why, given the information that was available to the Kennedy administration, did they assume from the outset that the missiles in Cuba were a threat to national security. Contained in this question is an attempt to imagine a world in which all the elements were present, but the outcome was something other than a depiction of the event in terms other than a national security threat. Klotz and Lynch (2007) include counterfactual analysis, along with process tracing and even statistical studies, as methodological choices amenable to constructivism. So long as these methods remain sensitive to social and historical context, "We encourage *creative* use of all methods, as long as researchers do not claim objectivity or universal truth," (Klotz and Lynch, 2007, p. 108). This would appear to be another commonality between the empirical interpretive approach and the eclectic approach: a logic of appropriateness.

Beyond these shared techniques, we also identify a common theme that runs through the three qualitative approaches, that of the importance of research as an *iterative* process. Within each approach we have found, in one form or another, instructions for the researcher to alternate between the general and the specific when conducting their research. Adcock and Collier (2001)

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<sup>25</sup> This is quite similar to Goertz and Mahoney's "possibility principle", where one should choose negative cases (I.e. cases in which the outcome in question did not occur) based on whether causal conditions present were such that the outcome was likely to occur. Though they are speaking specifically about negative case selection, they are essentially identifying a method by sufficient causal relationships can be evaluated counterfactually.

advise researchers to go between background concepts, systematized concepts, indicators, and scores when forming concepts. Ragin's (2000 and 2008) technique for calibrating scores on cases relies fundamentally on the researcher going from the empirical case to the membership scores and back. Some interpretivists (with perhaps the exception of post-modernists) may avail themselves of the process of the hermeneutic circle, where parts of a text are understood in reference to the whole, and the whole is established by reference to the individual parts. Lieberman's oft-cited piece on nested analysis (2005) compellingly advises the researcher using mixed-methods (in this case, regression analysis and case studies) to alternate between one and the other, taking the findings from the previous round of data analysis to drive the subsequent rounds. Even Kaplan's (1965, 53) paradox of conceptualization calls to mind an iterative process for theory and concept development: "Proper concepts are needed to formulate a good theory, but we need a good theory to arrive at the proper concepts... The paradox is resolved by a process of approximation: the better our concepts, the better the theory we can formulate with them, and in turn, the better the concepts available for the next, improved theory."<sup>26</sup> While the emphasis on iteration as a means of fine tuning our theories, concepts, and analysis is perhaps not a *shared standard*, it certainly seems to be a *shared prescription*.

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<sup>26</sup> Cited in Adcock and Collier, 532



## Bibliography

- Adcock, Robert. 2006. "Generalization in Comparative and Historical Social Science: The difference that interpretivism makes." in *Interpretation and Method: Empirical Research Methods and the Interpretive Turn* eds. Dvora Yanow and Peregrine Schwartz-Shea. Armonk, New York: M.E. Sharpe.
- Adcock, Robert and David Collier. 2001. "Measurement Validity: A shared standard for qualitative and quantitative research. *The American Political Science Review*, Vol. 95, No. 3 (Sep., 2001), pp. 529-546.
- Bartels, Larry. 2004. "Some Unfulfilled Promises of Quantitative Imperialism" in *Rethinking Social Inquiry: Diverse Tools, Shared Standards* eds. Henry E. Brady and David Collier. Lanham, MD: Rowman and Littlefield.
- Bevir, Mark. 2006. "How Narratives Explain" in *Interpretation and Method: Empirical Research Methods and the Interpretive Turn* eds. Dvora Yanow and Peregrine Schwartz-Shea. Armonk, New York: M.E. Sharpe.
- Brady, Henry E. and David Collier. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, MD: Rowman and Littlefield.
- Collier, David, James Mahoney and Jason Seawright. 2004. "Claiming too Much: Warnings about selection bias" in *Rethinking Social Inquiry: Diverse Tools, Shared Standards* eds. Henry E. Brady and David Collier. Lanham, MD: Rowman and Littlefield.
- Collier, David, Jason Seawright and Gerardo L. Munck. 2004. "The Quest for Standards: King, Keohane, and Verba's *Designing Social Inquiry*" in *Rethinking Social Inquiry: Diverse Tools, Shared Standards* eds. Henry E. Brady and David Collier. Lanham, MD: Rowman and Littlefield.
- Gerring, John. 2001. *Social Science Methodology: A criterial framework*. Cambridge: Cambridge University Press.
- 2007. *Case Study Research: Principles and Practices*. Cambridge: Cambridge University Press.
- 2008. "Case Selection for Case Study Analysis" in *The Oxford Handbook of Political Methodology* eds. Box-Steffensmeier, Janet M., Henry E. Brady and David Collier. Oxford: Oxford University Press.
- Forthcoming. *Social Science Methodology: A criterial framework, 2<sup>nd</sup> Ed*. Cambridge: Cambridge University Press.
- George, Alexander L. and Andrew Bennett. 2005. *Case Studies and Theory Development in the Social Sciences*. Cambridge, MA: MIT Press.
- Goertz, Gary and James Mahoney. 2010. Two Cultures: Hume's Two Definitions of Cause. *Qualitative and Multi-Method Research*, Spring 2010 24-27.

- Hawkesworth, Mary. 2006. "Contending Conceptions of Science and Politics: Methodology and the Constitution of the Political" in *Interpretation and Method: Empirical Research Methods and the Interpretive Turn* eds. Dvora Yanow and Peregrine Schwartz-Shea. Armonk, New York: M.E. Sharpe.
- Kaplan, Abraham. 1964. *The Conduct of Inquiry*. San Francisco: Chandler.
- King, Gary, Robert O. Keohane and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton, NJ: Princeton University Press.
- Lieberman, Evan. 2005. Nested Analysis as a Mixed Method Strategy for Comparative Research. *American Political Science Review*, 99:435-52.
- Mahoney, James. 2010. "After KKV: The New Methodology of Qualitative Research," *World Politics* 62:1 (January 2010).
- James Mahoney and Gary Goertz. 2006. "A Tale of Two Cultures: Contrasting Quantitative and Qualitative Research," *Political Analysis* 14:3 (Summer): 227-249.
- Mahoney, James, Erin Kimball and Kendra L. Koivu. 2009. The Logic of Historical Explanation in the Social Sciences. *Comparative Political Studies*. 42:1 (January), pp. 114-146.
- Pressman Jeffrey L. and Aaron Wildavsky. 1973. *Implementation*. Berkeley: University of California Press.
- Ragin, Charles C. 2000. *Fuzzy-Set Social Science*. Chicago: The University of Chicago Press.
- 2008a. *Redesigning Social Inquiry: Fuzzy Sets and Beyond*. Chicago: The University of Chicago Press.
- 2008b. "Measurement vs. Calibration: A set-theoretic approach" in *The Oxford Handbook of Political Methodology* eds. Box-Steffensmeier, Janet M., Henry E. Brady and David Collier. Oxford: Oxford University Press.
- Ragin, Charles C. and Benoit Rihoux. 2009. *Configurational Comparative Methods: Qualitative Comparative Analysis (QCA) and related techniques*. London: Sage.
- Schwartz-Shea, Peregrine. 2006. "Judging Quality: Evaluative Criteria and Epistemic Communities" in *Interpretation and Method: Empirical Research Methods and the Interpretive Turn* eds. Dvora Yanow and Peregrine Schwartz-Shea. Armonk, New York: M.E. Sharpe.
- Skocpol, Theda. 1979. *States and Social Revolutions: A comparative analysis of France, Russia and China*. Cambridge: Cambridge University Press.
- Wedeen, Lisa. 2008. *Peripheral Visions: Public, power and performance in Yemen*. Chicago: University of Chicago Press.
- Weldes, Jutta. 1999. *Constructing National Interests: The United States and the Cuban Missile Crisis*. Minneapolis, MN: University of Minnesota Press.

Yanow, Dvora. 2006. "Thinking Interpretively: Philosophical Presuppositions and the Human Sciences" in *Interpretation and Method: Empirical Research Methods and the Interpretive Turn* eds. Dvora Yanow and Peregrine Schwartz-Shea. Armonk, New York: M.E. Sharpe.

Yanow, Dvora and Peregrine Schwartz-Shea. 2006. *Interpretation and Method: Empirical Research Methods and the Interpretive Turn*. Armonk, New York: M.E. Sharpe.

Zukav, Gary. 1979. *The Dancing Wu Li Masters*. New York: Bantam Books.