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Abstract

Most contemporary political science researchers are advocates of multi-method research, however, the value and proper role of qualitative methodologies, like case study analysis, is disputed. A pluralistic philosophy of science can shed light on this debate. Methodological pluralism is indeed valuable, but does not entail causal pluralism. Pluralism about the goals of science is relevant to the debate and suggests a focus on the difference between evidence for warrant and evidence for use. I propose that case study research provides *evidence for use* through providing information that bears on the applicability of causal generalizations and risk assessment.

Keywords

case studies, methodology, political science methodology, pluralism, causality

Introduction

My use of the phrase “evidence for use” originates in Nancy Cartwright’s 2006 article, “Well-Ordered Science: Evidence for Use.” There she suggests that philosophers of science direct their energies away from “highly general

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questions of warrant . . . to much more specific questions about particular methods and their problems of implementation, their range of validity, their strengths and weaknesses, and their costs and benefits” (Cartwright 2006, 982). Cartwright advocates pluralism about the goals of science, as well as pluralism about evidence. In addition, Cartwright advocates a version of causal pluralism—the view that no one account of causality can sustain the various causal inferences that we make.

Various pluralisms of the sort that Cartwright advocates are much discussed in contemporary philosophy of science. Can pluralism be used as a tool for thinking about a current methodological debate in political science? It seems plausible that different methodologies might be appropriate for different goals or purposes. Multimethod researchers—those who advocate roles for both qualitative and quantitative methodologies in political science—sometimes claim that the underlying justification for methodological pluralism is that different methodologies are appropriate for different goals. For example, they argue that case study research, a form of research that incorporates a variety of qualitative methodologies and focuses on individual examples, plays an important role in knowledge production and use that other methodologies in political science, such as formal (mathematical) modeling and statistical methods, cannot. Results from formal and quantitative modeling may be useful if one wants to make broad generalizations about correlations, to offer general explanations, or perhaps even to make claims about causal connections between variables. But if one is seeking to understand how such causes function “on the ground” and in particular contexts, only the sort of specific, intensive, and detailed information that case study research provides will serve. Given this line of argument, I offer that understanding pluralism about methods, evidence, and goals, as well as the ways in which they are related to each other can help us understand this methodological debate in political science. In particular, this line of analysis can shed light on the role of case study research in political science.

The structure of the article is as follows. In section 1, I show that the debate over case study research has been framed by scholars who embrace methodological monism based on a questionable philosophy of science. In section 2, I consider criticisms of that monism, consider what appear to be pluralistic claims made in support of case study research, and introduce an alternative conception of the goals of science found in the more recent work of pluralistic philosophers of science such as Nancy Cartwright. Section 3 develops the discussion of pluralism around the question of causality. Julian Reiss’s work is used to clarify some different versions of causal pluralism. Section 4 explores an example in which theoretical claims and knowledge of statistical regularities is supplemented by qualitative knowledge to support an inference to a particular course of action. The example is interpreted as a case where evidence

for the truth of general statistical claims needs to be distinguished from evidence for use. In section 5, I conclude that causal pluralism is not required for understanding the role of case study research and methodological pluralism does not commit us to causal pluralism. However, case study research provides evidence for use in a number of ways that support methodological pluralism.

I. Framing the Debate: Designing Social Inquiry

Most political scientists no longer work solely within the qualitative tradition, but avail themselves of a variety of methods—most notably formal modeling (deductive and mathematical in nature), quantitative analysis (essentially econometric), as well as qualitative research, including a variety of comparative case and narrative methods. Consequently, most political scientists are multimethod researchers. However, the power of nonqualitative methods, particularly econometrics, has those who use qualitative methods on the defensive. John Gerring, a political scientist who does multimethod research, puts it this way:

Thus, a paradox: Although much of what we know about the empirical world is drawn from case studies and case studies continue to constitute a large proportion of the work generated by the discipline, the case study *method* is held in low regard or is simply ignored. Even among its defenders there is confusion over the virtues and vices of this ambiguous research design. (Gerring 2004, 341)

The “low regard” with which case study work is held stems from questions about how case studies can provide warrant for belief in causal and descriptive generalizations that are thought to be the goal of science. At least two philosophical/epistemological concerns have been raised about case studies. The first is a concern about the inductive support that case studies can offer for a descriptive (empirical) or causal regularity. Case studies typically involve looking at one or a small number of cases in detail, but drawing a conclusion from a few cases, or perhaps just one, would seem to be bad inductive reasoning. This is the problem of the small *n*. The second concern has to do with the *relevance* of the cases, as opposed to how *many* cases are examined. Focusing on relevance raises questions about how cases should be selected and the problem that any selected case may or may not be representative of the larger population from which it is drawn. The way that these epistemological issues have been tackled in the methodological literature in political science is closely wedded to the particular philosophy of science assumptions that continue

to shape the discussion about scientific method and evidence in political science. Specifically, these two concerns are in the foreground if the primary epistemic question is thought to be how case studies can provide warrant for belief in causal claims.

The contemporary debate about qualitative research and case study research in particular has been shaped by the influential *Designing Social Inquiry: Scientific Inference in Qualitative Research* by Gary King, Robert O. Keohane, and Sidney Verba (*DSI* hereafter), published in 1994. The book is part polemical treatise, part handbook for the researcher, and intended as a general guide to make qualitative research more rigorous. As it turns out, “more rigorous” means “more like quantitative research.”

The authors state their motivation for writing the book in the introduction:

Our main goal is to connect the traditions of what are conventionally denoted “quantitative” and “qualitative” research by applying a unified logic of inference to both. The traditions appear quite different; indeed they sometimes seem to be at war. Our view is that these differences are mainly ones of style and specific technique. The same underlying logic provides the framework for each research approach. (King, Keohane, and Verba 1994, 3)

Given their premise that there is “one logic” underlying all social science research, the authors of *DSI* (King, Keohane, and Verba) outline the key elements of that logic and thereby provide a way to apply it to qualitative research. *DSI* appears to be committed to the view that the logic of inference is exemplified by statistical/quantitative research, and if qualitative methods are to be employed they should be designed using these quantitative methods as a template (Brady and Collier 2004). “We argue that nonstatistical research will produce more reliable results if researchers pay attention to the rules of scientific inference—rules that are sometimes more clearly stated in the style of quantitative research” (King, Keohane, and Verba 1994, 6). *DSI* goes on to specify what is intended by “rules of scientific inference” identifying four characteristics of scientific research: (1) the goal is inference; (2) the procedures are public; (3) the conclusions are uncertain; (4) the content is the method (King, Keohane, and Verba 1994, 8-9).¹

¹My statement of these theses interprets their views in a narrow sense. It may be that *DSI* interprets the logic of justification more broadly than this statement implies, but I will work with this narrow interpretation as a way of more clearly identifying the possible points of disagreement and the ways in which the defenders of case study research have proceeded.

The first three are fairly self-explanatory and uncontentious, although if the authors of *DSI* mean by (1) that the *only* goal of science is inference to warrant there may be some debate and I will return to this later. The claim (2) that procedures be public commits *DSI* to no more than that the methods should be replicable and not subjective. The claim (3) that the conclusions are uncertain is also straightforward; scientific knowledge is always subject to revision in the light of new evidence. It is the fourth claim—that the content is the method—that merits the closest scrutiny.

The content of “science” is primarily the methods and rules, not the subject matter, since we use these methods to study virtually anything. . . . The unity of all science consists alone in its method, not its material. (King, Keohane, and Verba 1994, 9)

DSI thus presents a strong methodological unity of science thesis consisting of three key elements: (1) there is one logic of inference; (2) there is one goal of science; and (3) science is unified by method.

2. Critics and Philosophy of Science

Three books from the mid-2000s provide an overview of the range of responses to *DSI*. Henry E. Brady and David Collier’s edited volume, *Rethinking Social Inquiry*, offers articles covering the spectrum of positions and includes a response from *DSI*’s authors, King, Keohane, and Verba. *Case Studies and Theory Development in the Social Sciences* (2005) by Alexander L. George and Andrew Bennett also offers an alternative approach to qualitative methodology. In addition, John Gerring’s *Case Study Research: Principles and Practice* (2007) provides a practical analysis and typology of cases.²

It is worth noting that many of these responses to *DSI* from within political science do not directly consider or challenge the claim that there is one logic of inference. For example, George and Bennett note an ambiguity in the claim and suggest that they might agree with this core thesis of *DSI* depending on the interpretation.

If this logic of inference refers in a broad sense to the epistemological logic of deriving testable implications from alternative theories, testing these implications against quantitative or case study data, and modifying

²Other sources for this discussion are from the Symposium on Rethinking Social Inquiry in a 2006 issue of *Political Analysis* and the 2008 *Oxford Handbook for Political Methodology*.

theories or our confidence in them in accordance with the results, then perhaps on a very general level there is one logic that is the modern successor of the still-evolving positivist tradition . . .

If, however, the logic of inference refers to specific methodological injunctions on such issues as the value of single-case studies, the procedures for choosing which cases to study, the role of process-tracing, and the relative importance of causal effects (the expected change in the dependent variable given a unit of change in an independent variable) and causal mechanisms as the bases for inference and explanation . . . then we disagree with the overall argument . . . (George and Bennett 2005, 10-11)

On either of these suggested interpretations, the authors of *DSI* are committed to the nonpluralistic view of science as I have characterized it in the previous section, but it would seem that George and Bennett are as well, since they are prepared to embrace the weaker version of the claim. They reject *DSI*'s account of causal inference and do so on philosophical grounds, because they *do* embrace a broader understanding of case study methodology and its value for causal inference. Nonetheless they hold views that are fairly similar to those in *DSI* on the nature and role of inference in science.

DSI's understanding of the methodological unity of science traces its roots to some of the key tenets of early twentieth-century philosophy of science: logical positivism; logical empiricism; and Popper's critique of them. The key features of this view are the unity of science thesis, the suggestion of some sort of demarcation thesis (science is what uses the scientific method), which includes a commitment to the view that there is a context of discovery which is distinct from a context of justification, combined with the specific recommendation that case studies should be used only in the discovery phase of research design and, perhaps, the testing of hypotheses. There are also elements of reductionism (though the unity of science thesis is primarily a methodological thesis in *DSI*), but this is not explicit. Timothy McKeown, one of *DSI*'s critics, notes these philosophical commitments explicitly in his 1999 review of the book:

Although they disclaim any interest in philosophy of science, King, Keohane, and Verba adopt essentially Popperian positions on many important questions. In particular, they emphasize a clear distinction between forming or stating hypotheses and testing them, an accompanying

reluctance to treat hypothesis formation as anything other than an artform, their stress on the need for simplicity in theories, and their insistence on subsuming each case within a class of cases are all highly consistent with logical positivism or Karl Popper's reworking of it. (McKeown 1999, 162)

Although McKeown does not offer an alternative philosophy of science, he does give an analysis of what he thinks has gone wrong.

Because the statistical worldview embodied in King, Keohane, and Verba's assumption is usually not the worldview that animates case studies, their approach leads to a series of misconceptions about the objectives of case studies and their accomplishments. These misconceptions are constructive, however, in the sense that exposing them leads to a clearer notion not only of the underlying logic behind case studies but also of the importance of nonstatistical thinking and research activity in all research domains—even those dominated by classical statistical data analysis. (McKeown 1999, 162)

McKeown is suggesting that one way of understanding methodological pluralism is through recognizing a plurality of goals that various different methodologies seek to achieve. In his view, one reason why *DSI* misses the value of case study research is its failure to be sensitive to differences in goals because of its commitment to the unity of science thesis. There is indeed one line of debate around multimethod research that would seem to show support for McKeown's diagnosis. A number of researchers have argued that case studies provide unique evidence for causal processes, evidence that is not produced through quantitative means (George and Bennett 2005; Collier, Brady, and Seawright 2004; Bennett and Elman 2006; Mahoney 2003). That political science should seek causal processes, not just causal regularities, is implicated in the debate.

While the idea that research seeks causal processes and not just causal regularities is not usually explicitly formulated as a commitment to causal pluralism, sometimes it is. For example, Mahoney writes,

Important contrasts in the logic of causal inference underlie nominal and ordinal strategies. A nominal strategy implicitly or explicitly assumes a nonlinear understanding of causation built around the idea of

necessary and sufficient conditions. This understanding of causation is quite different from that employed by most large-*N* researchers . . . (Mahoney 2003, 338)³

Mahoney is contrasting two concepts of causality: one he identifies as defined through necessary and sufficient conditions and another with statistical regularities, which he calls a linear conception of causality. He discusses both cross-case and within-case analysis and the various methods through which different sorts of causal claims can be established (or refuted), including “process tracing,” a method that he describes as particularly useful for establishing causal mechanisms.⁴

Mahoney and Goertz (2006) offer an alignment of methodologies with conceptions of cause that is consistent with the idea that different methodologies play different roles in supporting different sorts of causal claims. Qualitative methodology is paired with necessary and sufficient conditions and quantitative methodology with what they refer to as “correlational causes” and a “probability/statistical theory.” They identify the former as most closely associated with a “causes-of-effects” approach to explanation, whereas the latter is seen as more compatible with an “effects-of-causes” approach, further elaborating on the idea that different methodologies are more appropriate to different goals. “Methodologists working in the statistical tradition have seen clearly the difference between the causes-of-effects approach, in which the research goal is to explain particular outcomes, and the effects-of-causes approach, in which the research goal is to estimate average effects” (Mahoney and Goertz 2006, 230-31). *DSI* is identified as explicitly adopting the “effects-of-causes” approach. But what Mahoney and Goertz mean by different “conceptions of cause” is not clear and elsewhere their discussion suggests that they are really talking about ways in which methodologies can converge to provide evidence for the same cause.

³Nominal and ordinal strategies would both make comparisons across highly aggregated units such as nation-states. They differ in that nominal strategies make comparisons using nominal units (categories that are compared are named and not quantitatively identified), whereas ordinal strategies use ordinal units (quantitative).

⁴Following George and McKeown (1985), the effort to infer causality through the identification of causal mechanisms can be called “process tracing” (Mahoney 2003, 363). A number of multimethod researchers embrace the use of case studies but only to test causal claims—not to provide evidence in support of them. I have not addressed this debate, but rather I have focused on researchers that do accept the possibility that evidence from case studies can serve a positive role.

We believe that both approaches are of value; in fact, they complement one another. Ideally, an explanation of an outcome in one or a small number of cases leads one to wonder if the same factors are at work when a broader understanding of scope is adopted, stimulating a larger-N analysis in which the goal is less to explain particular cases and more to estimate average effects. Likewise, when statistical results about the effects of causes are reported, it seems natural to ask if these results make sense in terms of the history of individual cases; one wishes to try to locate the effects in specific cases. (Mahoney and Goertz 2006, 231)

This passage seems to be describing two lines of evidence converging on one cause. If so, then perhaps this discussion is better read as one in which seemingly differing conceptions of cause are really differing conceptions of how to support causal claims. Nonetheless, there is at least a *prima facie* case to be made that the debate revolves, in part, around connections between methodological pluralism, plurality of goals, and some sort of causal pluralism.

To explore this line of investigation, I turn to causal pluralism as advocated by Nancy Cartwright (2007) and Julian Reiss (2009). Can causal pluralism help us better understand the epistemological role of case studies and does the methodological pluralism advocated by multimethod researchers require a commitment to causal pluralism?

3. Pluralism and Causality

The authors of *DSI* identify causal inference as one of the principle goals of political science (“descriptive inference” is the other). They also support the view that causal inferences can be made using quantitative methods even though, they note that

. . . [m]any social scientists are uncomfortable with causal inference. They are so wary of the warning that “correlation is not causation” that they will not state causal hypotheses or draw causal inferences, referring to their research as “studying association and not causation.” (King, Keohane, and Verba 1994, 75)

They go on to say that they think that this attitude fails to acknowledge what they refer to as the Fundamental Problem of Causal Inference, a phrase that they take from Paul Holland (1986), which is that causal conclusions are always uncertain. Though we will always be uncertain about causal inferences, this

is not a reason that we should not make them. However, they warn that we should be “cautious about detailing our uncertainty when we do” make inferences. They offer a definition of causality following the approach used in Holland (1986), with modifications taken from Suppes (1970).⁵

Without going into the details of the account, there are three aspects of it that are relevant for this discussion: (1) it is stochastic/statistical; (2) both quantitative and qualitative examples of causal inference are given as illustrations, so the authors of *DSI* present it as methodologically neutral; and (3) it is an account of causal effects, not causal processes or mechanisms. The authors of *DSI* do discuss causal mechanisms and processes, noting that while they have an “intuitive” appeal,

Identifying causal mechanisms requires causal inference. . . . That is, to demonstrate the causal status of each potential linkage in such a posited mechanism, the investigator would have to define and then estimate the causal effect underlying it. To portray an internally consistent causal mechanism requires using our more fundamental definition of causality . . . for each link in the chain of causal events. (King, Keohane, and Verba 1994, 86)⁶

In short, the authors of *DSI* reconfirm their commitment to one logic of (causal) inference and thus to one concept of “cause.”

Offering an alternative account, George and Bennett address the question of causal inference directly. Using the work of Wesley Salmon on causal processes, they argue that it is only through in-depth and intensive case study work that causal processes and mechanisms are identified and that it is actually in this so-called “process tracing” that the evidence for causal inferences lies. They argue that a probabilistic or statistical account of the sort that the authors of *DSI* advocate as fundamental can never fully establish causal connections, noting problems such as common causes and conjunctive forks.

⁵I have focused on two ways of talking about cause that are prevalent in the literature of the debate and thus have simplified the discussion of causal pluralism by focusing primarily on a broad statistical/probabilistic interpretation of causality (all of which are not the same) and causal processes. Some of the political science literature does not distinguish sharply between causal processes and causal mechanisms and I have not done so either.

⁶This is the “effect-of-causes” approach that Mahoney and Goertz identify in the passage above.

The idea that causal processes are revealed through intensive case study work appears elsewhere in this literature as well. For example, Brady, Collier, and Seawright (2006) argue for the role of case studies in establishing causal processes. They claim that this is something that cannot be achieved through statistical methods. However, others—Nathaniel Beck (2006), for example—argue that they are mistaken and that there is nothing additional case studies provide that is not established through quantitative methods. Beck’s view is that case study research is appropriate to inform research design, but not in the justification of knowledge.

In summary, *DSI* proposes that there is one logic of (causal) inference and that the way it works in quantitative cases should provide a template for qualitative inference. Anything that we might want to say about causal mechanisms or processes in case studies depends on prior demonstration of the claim through this one legitimate mode of causal inference. George and Bennett and other critics, by contrast, argue that *DSI* only succeeds in capturing one sense of causality and that there are other concepts that are not only legitimate but that can only be captured through intensive case analysis.

In *Hunting Causes and Using Them*, Nancy Cartwright argues that no one, universal account of causality is adequate to all the different relations that we call “causal.”

There are, I maintain, a variety of different kinds of causal relations that we might be pointing to with the label “cause” and each different kind of relation needs to be matched to the right methods for finding out about it as well as with the right inference rules for how to use our knowledge of it. (Cartwright 2007, 9)

This way of thinking about causality does not support a “one size fits all” or as Cartwright refers to it an “off the shelf” methodology to be applied in all contexts.⁷ In so far as the authors of *DSI* are advocating one logic of (causal) inference, causal pluralism appears to be incompatible with their view. To explore whether causal pluralism has resources to offer, I turn to Julian Reiss’s account in “Causation in the Social Sciences: Evidence, Inference, and Purpose” (2009).

Reiss argues for pluralism about the concept of cause, a view that he shares with Cartwright. Reiss also follows Cartwright in arguing that all extant accounts of causality face unresolvable counterexamples when they are treated

⁷Her particular target is the idea that randomized clinical trials (RCTs) can serve this role in the social sciences.

as universal accounts of causality. Each account may cover particular cases well, but each is also vulnerable to counterexamples. He considers familiar counterfactual, regularity, probabilistic, mechanistic, and interventionist accounts providing counterexamples to their necessity and sufficiency in each case to support his claim.

Plurality of concepts of “cause” does not follow directly from this argument, however. It could be the case that each of these senses of causality provides a different sort of evidence for a cause or causes, but subsumed under one common concept of “cause.” On this view, there are different paths to discovery, each producing different sorts of evidence. But the underlying conception of cause remains common. Reiss calls this “evidential pluralism.” He notes that there are a number of multimethod political scientist researchers who seem to hold this view. I have mentioned the ambiguous account in George and Bennett, for example. Reiss suggests that Gerring (2005) is also an evidential pluralist.

Reiss contrasts evidential pluralism with conceptual pluralism, the view that there are many different concepts of cause. If multimethod researchers in political science are evidential pluralists, then the various different methods in the social sciences are all worth pursuing because they are all various ways of confirming or disconfirming the causal hypothesis. We might generate statistical evidence of causal connection through multiple regression analysis, a core statistical technique in political science research, but a case study that traced a causal process or identified a causal mechanism would make us more confident that we had indeed established a cause. Something like this view seems to underlie the work of many multimethod researchers. For instance, we find it in the work of James Fearon and David Laitin in their research on civil war. They advocate large-N statistical work using regression analysis, but note that such research is often unable to establish causality.

. . . [C]ase studies can be extremely useful as a method for assessing whether arguments proposed to explain empirical regularities are plausible. One selects particular cases and examines them in greater depth than was required to code values on the outcome and explanatory variables of interest. . . . [T]he case study will entail a narrative account of what led to the outcome, including an assessment of what role the proposed causal factors played. (Fearon and Laitin 2008, 757)

The idea is that the statistical work and the case study research are methods that support the conclusion about the *same* causal connections. And yet the

widespread advocacy of methodological pluralism in political science is ambiguous in that it is consistent with either evidential pluralism or conceptual pluralism.

Reiss argues that not all differences between causal concepts can be resolved through evidential pluralism. Identifying a cause includes establishing a causal claim, which, in turn, supports causal inferences. Establishing causal claims under one concept of causality does not always secure the sorts of inferences that we desire to make (namely, those in which we are interested). For example, Reiss argues that two senses of cause, such as a probabilistic one and cause that is characterized through the manipulation of some X through which we can control some Y, may coincide, but they need not. He offers the following example: economists identified a causal relationship between inflation and unemployment in the 1960s (lower inflation was associated with higher unemployment). However, attempts at controlling (manipulating) unemployment through macroeconomic policy appeared increasingly unsuccessful. Reiss concludes that conceptual monism cannot be saved through privileging some one concept of causality; we cannot always support causal conclusions understood under one sense of cause from evidence that is relevant under another sense of cause.

Thus Reiss decides in favor of conceptual pluralism and concludes that methodologies and the evidence they produce establish particular types of causes and are evidence specific to those sorts of causes. This discussion suggests the possibility that case study research may be a methodology that reveals some types of causes but not others. While concepts of cause may overlap at times, they do not always do so. In Reiss's example of inflation and unemployment, the inference to *use* of the causal knowledge provided through the statistical analysis is not warranted because the type of causality established through the methodology applied did not support an interventionist causal claim.

4. Causal Pluralism Reconsidered: An Example

While this analysis is suggestive, the question of whether case study research could establish a different type of cause or merely different evidence for the same cause in political science research remains to be determined. As I have already noted, at least some political scientists who do multimethod research think that they are looking at alternative methods for establishing evidence for the same underlying causal mechanism. Others seem to be arguing that such research gets at causal mechanisms and causal processes that are best thought

of as covered by different concepts of causality. A closer examination of both a typology of cases and typology of causes would be useful for investigating this issue more thoroughly.⁸ For the moment, I am going to stick with the fairly coarse-grained distinction between statistical/probabilistic causality as defined in *DSI* and causal process/mechanism notions of causality, where such processes are taken to stand in fundamental contrast to the way they are conceived of in *DSI*.

Reiss has suggested (following Cartwright) that a clear way of determining if there are different types of causes (causes under different concepts) is through looking at the inferences that causal claims support. In his example, the inference to use is not supported by the evidence that establishes causality in the sense of a causal generalization linking inflation and unemployment. Looking at an example of an argument for use—an argument for a particular policy decision—offers a way of exploring this suggestion.

In September 2006, James Fearon, a political scientist at Stanford University and an expert on civil war, testified before U.S. House of Representatives, Committee on Government Reform, Subcommittee on National Security, Emerging Threats, and International Relations on “Iraq: Democracy or Civil War?” This testimony took place prior to the “surge”—the increase in the number of troops in Iraq—and was part of the public debate surrounding it. Fearon presented expert testimony⁹ supporting the claim that Congress should not authorize the deployment of additional troops. Here is the core of his policy recommendation argument from the executive summary of the testimony:

1. The current US strategy in Iraq aims to help put in a place a national government that shares power and oil revenues among parties closely linked to the combatants in the civil war. The hope is that our presence will allow the power-sharing agreement to solidify and us to exit, leaving a stable, democratic government and a peaceful country.
2. The historical record on civil war suggests that this strategy is highly unlikely to succeed, *whether the US stays in Iraq for six more months or six more years (or more)*. Foreign troops and advisors can enforce power-sharing and limit violence while they are present, but it appears

⁸Gerring (2007) offers a typology of cases, which could provide a good starting point.

⁹It is relevant that Fearon is not an “area” expert but rather studies civil wars more generally, as he notes at the beginning of his testimony. “I am not a specialist on the politics of the Middle East, but I have spent a lot of time studying the politics of civil wars” (Fearon 2006, 1).

to be extremely difficult to change local beliefs that the national government can survive on its own while the foreigners are there in force. In a context of many factions and locally strong militias, mutual fears and temptations are likely to spiral into political disintegration and escalation of militia and insurgent-based conflict if and when we leave.

3. Thus, *ramping up* or “*staying the course*” amount to *delay tactics*, *not plausible recipes for success as the administration has defined it*.
4. Given that staying the course or ramping up are not likely to yield peace and a government that can stand on its own, I argue for gradual redeployment and repositioning of our forces in preference to an extremely costly permanent occupation that ties our hands and damages our strategic position in both the region and the world.¹⁰

Fearon is *not* a particularly strong advocate of multimethod research; in fact, his focus has been primarily on the use of formal models and statistical methods. As discussed in the previous section, he also seems to hold the view that when case studies provide evidence it is in the form of additional evidence for a causal relation that has already been suggested through other evidence. The central part of his argument—his evidence—rests in the historical record that he refers to. This is a body of descriptive, statistical evidence that he offers supporting the causal inference that one course of action is more likely to have a favorable outcome than another. I base the claim that he is making causal inferences on his use of particular terms—what Cartwright refers to as the “thick” causal terms of ordinary language (Cartwright 2007, 9). So, for instance, in (2) we see “Foreign troops and advisors *can enforce* power-sharing and *limit* violence . . .” and “it appears to be extremely difficult to *change* local beliefs . . .” by which he means there is statistical evidence that outside intervention can enforce settlements but not change local beliefs. Also in (4), we see Fearon’s claim that “an extremely costly permanent occupation . . . *ties* our hands and *damages* our strategic position in both the region and the world,” again a claim that is at least amenable to test (that multiple interventions reduce the ability to undertake new ones or has other

¹⁰All excerpts from the testimony come from a version available at The Freeman Spogli Institute for International Studies at Stanford University (http://fsi.stanford.edu/publications/iraq_democracy_or_civil_war/) accessed March 14, 2010. The numbering is mine and does not appear in the executive summary.

adverse effects). Fearon is making (tentative and probabilistic) causal claims about the effects of particular policies. The claims are being made relative to the goal (1) and so the argument is pragmatic and strategic in the sense that it is an argument for a particular course of action.

Fearon's claims are backed up by statistical findings based on his quantitative study of civil wars. But to make the argument that the U.S. should follow a particular course of action in Iraq he has to show that these claims about regularities are significant for the case of Iraq and *how* they are to be interpreted and applied in this case. One element of his argument is to read the situation *in Iraq* as a civil war. He begins by noting that the conflict in Iraq (at that time) qualified as a civil war on generally agreed upon understanding of "civil war". He then proceeds to give an account of the likelihood of achieving successful power-sharing agreements as a resolution to such conflicts by summarizing statistical arguments in the following way:

Civil wars typically last much longer than international wars. For civil wars beginning since 1945, the average duration has been greater than 10 years, with fully half ending in more than seven years (the median). The numbers are fairly similar whether we are talking about wars for control of a central government, or wars of ethnic separatism. . . .

When they finally do end, civil wars since 1945 have typically concluded with *a decisive military victory* for one side or the other. In contests for control of the central state, either the government crushes the rebels (at least 40% of 54 cases), or the rebels win control of the center (at least 35% of 54 cases). . . .

Quite often, in perhaps 50% of these cases, what makes decisive victory possible is the provision or withdrawal of support from a foreign power to the government or rebel side. For example, the long civil war in Lebanon ended in 1991 after the US and Israel essentially changed their positions and became willing to see the Syrian-backed factions win control if this would lead to peace. International intervention in civil wars is extremely common and often determines the outcome. Power-sharing agreements that divide up control of the central government among the combatants are far less common than decisive victories. I code at most 9 of 54 cases, or 17%, this way. Examples include El Salvador in 1992, South Africa in 1994 and Tajikistan in 1998. (Fearon 2006, 3)

The evidence provided is in the form of descriptive statistics and not causal claims, but he clearly intends that causal conclusions be drawn for policy in

Iraq.¹¹ The arguments that are made are in fact supported by more detailed multiple regression analysis that purports to make stronger causal claims. But even these findings are ultimately probabilistic in nature. While knowing that the probabilities for success are low given the evidence that he presents, knowing whether *Iraq* is more or less likely to be well-explained by the general findings is crucial for drawing the conclusions Fearon wants to draw.

Fearon offers two examples to aid in locating Iraq in the distribution of civil war cases. Here is the second:

Right now representatives of Shiite political factions with ties to different clusters of militias share power in the national parliament and across government ministries. The expectation that US forces would act to prevent illegal grabs of power at the national level, and wholesale attacks by, say, Mahdi Army militias against Badr Brigade militias over territorial control in Baghdad and other cities, is making for an armed and fractious peace between Shiite factions. Regardless of written constitutional rules and procedures, after the US leaves these Shiite factions and their affiliated militias will fear power grabs by the other and be tempted by the opportunity themselves. An intra-Shiite war is thus a plausible scenario following US withdrawal, whether that should come in six months or five years. (Fearon 2006, 5)

These are counterfactual projections informed by the sort of historical narrative account that is characteristic of political science case study research. The evidence that he brings to bear is qualitative. In effect, he argues that the (causal) process that he sees the proposed surge as resulting in will ultimately result in events that are adverse to American interests. What Fearon offers is based not only on general causal claims or statistical findings but an argument that links those causal claims to the details of this particular situation. The thick causal concepts that are used (“prevent illegal grabs of power” “is making for”) indicate that particular actions are likely to have particular effects. The argument from the statistical claims to use of this knowledge depends on correctly identifying the details and is made through a narrative

¹¹In addition to the arguments that appear in the testimony, I draw on the reasons why Fearon was called to give testimony, which have to do with his area of expertise. So, for example, his work on civil war with David Laitin includes their 2003 article that makes claims both about what have been thought to be causes of civil war, but are not (i.e., ethnicity), and what are the causes of civil war (conditions giving rise to insurgency).

that purports to describe a process or mechanism through which particular outcomes will occur. The role of identifying the process does not seem to be to introduce an additional type of causality into the discussion, but rather to provide evidence that *this* case is one in which a particular outcome will occur. In this testimony, both sorts of evidence contribute to the conclusion, but it follows from neither on its own. Nor does it follow (deductively) from both together. However, together, the arguments (and the different sorts of evidence that they bring to bear on the conclusion) make a stronger case for a particular course of action.

Thus, one read on the convergence of qualitative and quantitative evidence in his testimony is that the qualitative evidence supports a conclusion about how to apply the information garnered through quantitative research. The evidence works together to support a conclusion about causes and hence the same conclusion about policy. This would be consistent with evidential pluralism. We might consider fleshing out this reading in the following way. The qualitative evidence that informs his testimony gives information about how to partition the reference class so that it is relevant to the particular case (Iraq in 2006) in which we are interested. The information about the particular circumstance—the sort of knowledge that comes from case narrative—is providing evidence for the causal claims under the same concept of causality by providing evidence for the appropriate partition.

Reiss's argument for causal pluralism depends on the idea that while evidence can converge it will not always do so. I have argued that there is a sense in which the evidence does converge in this case, but, of course, this example may not be comparable to Reiss's. If we think back to unemployment and inflation, and identify a parallel issue here, the worry is that the general statistical causal claims may or may not be relevant for the particular case of Iraq, which may not conform perfectly—or even at all—with the general findings. The narrative that traces the causal process provides the evidence of relevance. It does this through linking the elements of the Iraq case with the appropriate statistical evidence and providing a narrative that helps us place Iraq in the relevant reference class. This reading does not require different concepts of causality but rather links the statistical evidence with the causal process evidence to make the case for a particular action. It is possible that the reason for this difference is simply that my example is not like Reiss's example, but it is also possible that Reiss's example could be given a similar alternative reading. I believe it is the latter.

Reiss has argued that different concepts of causality are aligned with different sorts of causal claims, not all of which can support the kinds of causal inferences that we need to make. But, in fact, his argument only shows that

some causal claims made about the connection between unemployment and inflation were not supported. He does not show that there is an alternative interventionist claim that *would* support some other conclusion and hence some alternate intervention. He concludes that the failure of inference is a failure of evidence for use, but perhaps it is the failure to have *found* evidence for use. He has not established that the failure is due to the need for evidence for a different type of cause. His example of the failure of inference could be read as a situation where the problem is that more evidence is needed, not evidence for a different type of causality. Following my discussion above, the evidence would be specifically evidence that would aid in partitioning the reference class or that would clarify what features of this particular case are relevant.¹² But even if we are able to relevantly partition the reference class in a way that more accurately supports our inferences, the statistical evidence would not be able to establish that the case of Iraq is like all other cases known to be in that class (i.e., civil wars).

This line of reasoning returns us to what appears to be the core issue in the debate: the idea that there is one goal of science. One of the clearest expressions of the “one goal of science” view found in *DSI* appears in Beck (2006).¹³ He asks whether political scientists should be concerned with discovering general lawlike regularities or explaining particular events. He believes that the primary focus should be on the former, but if so, we will rarely, if ever, be able to draw conclusions about an appropriate course of action in a particular case from the knowledge that we have. Interestingly, Beck does not consider the question of which of these goals would be more useful for informing policy. He clearly supports the notion that the goal of science is inference (inferences supported by generalizations), and thus clearly embraces evidence for warrant over evidence for use. This is the view we see in *DSI*, which frames the debate.

These considerations suggest that the question of plurality of causes is a red herring. The methodological dispute is really about the nature of science and the plurality of goals. Is the “method the content” of science or should we be looking at the *use* of scientific knowledge more closely when we consider methodology? And if so, what sort of evidence is “evidence for use”? I return

¹²I have framed the following discussion in terms of partitioning of reference classes, which would seem to commit me to a statistical/probabilistic account of causality. I do not intend to so commit myself, but have used this language because it is consistent with the simplified contrast between the two “concepts” of causality as I have set it up.

¹³Beck’s view is that case studies have only a limited use and are properly used only for testing of causal hypotheses.

to the idea “evidence for use” in relation to a plurality of both goals and methods to answer these questions.

5. Evidence for Use and Methodological Pluralism

One strength of detailed intensive case study work is, in part, that it provides additional evidence for causal relations and causal claims. When the goal is practical, we do not always use the generalization or theory that we believe most likely to be true or accurate in *all* circumstances, but the generalization or theory that we think is most relevantly true in *this* circumstance. The generalizations that *DSI* (and Beck) identify as the goal of scientific inquiry are not the whole story. While one aspect of what the case study tells us is about which variables are relevant in this circumstance, information necessary for good research design, the qualitative work in case studies also provides information on how likely it is that a particular case is likely to be explained by a more general causal claim. Good case study work does this by showing us how the case in question is similar to or different from others that fall within the reference class and/or by suggesting further relevant partitions. In this way, cases not only provide causal evidence; they also provide *evidence for use*. They give us clues about the limits of inference from generalizations.

Two examples illustrate how it is that evidence for use is not the same as evidence for warrant. The first comes from Cartwright’s discussion of randomized clinical trials (RCT) as the gold standard of evidence (2006). While we may have excellent evidence from RCTs that a particular fertilizer is safe and effective when used in a country where users cannot read the directions and the geology and weather differ dramatically from the conditions of testing, we cannot predict either the effectiveness or safety under its conditions of use.¹⁴ Nancy McHugh makes a similar point (forthcoming) when she analyzes

¹⁴First from philosopher and sociologist of science Jerry Ravetz, who specializes in questions of use: we may have excellent evidence, from randomized controlled trials even, that a particular fertilizer is both safe and effective. Then we send the fertilizer in bags with English language instructions to a distant country with dramatically different geology—say very steep slopes with vast runoff—and no culture of fertilizer use. There it is applied just before the huge rains come at 10 or 12 times the tested doses. The river is poisoned, people grow sick, animals die, and no good is done for the crops. (Cartwright 2006)

the effects of Agent Orange in rural communities in Vietnam, noting that victims are unable to have their claims considered seriously when the evidence from animal trials shows Agent Orange to be safe, even though the conditions under which it was used in Vietnam diverge so dramatically from the circumstances in which the evidence from animal studies was collected.

Neither of these cases provides evidence for causal pluralism however, though they both do provide evidence for methodological pluralism. In both cases, it is very possible that generalizations from one setting might not be portable to others and would require more detailed local knowledge, in effect, replication in a different setting. However, it is not necessary to appeal to different types of causes to make this claim. Nor would an argument for “process tracing” circumvent “The Fundamental Problem of Causality”—the fundamental uncertainty in causal inference. In fact, it could well be that the search for something to fill that “gap”—to establish *the* causal connection—is wrong-headed, as Christopher Hitchcock has suggested.¹⁵

However, the “gap,” or more neutrally, the uncertainty that persists in causal inference, does tell us something else about what needs to be taken into consideration when making the move from warrant to use. And so there may be yet another reason to pursue case study research. Even once we have the best available partition of the reference class (or all the available relevant information) we cannot directly infer what to do. And it does not seem that there is any conception of causality that would allow us to do so. When Fearon argued against the surge, he argued based on his best understanding of both the statistical generalizations and what sort of reference class was appropriate to the case of Iraq given his understanding of both Iraq and other cases of civil war. But this is not the only evidence needed for drawing a conclusion about what the best course of action is in this case. We need not only information about what variables have an effect but which effects are relevant to our goals. One way of thinking about this is through risk assessment. We need to know something about both the potential costs and benefits of the failure to take a certain action and the potential costs and benefits of taking that action. This means understanding not only the details of the case but of the purposes as well—what do we want the knowledge for. This kind of particularized knowledge, a knowing-how, rather than an knowing-that, requires seeing how things function “on the ground.” Making judgments about risks requires

¹⁵In his article “Of Humean Bondage,” Hitchcock (2003) argues that looking for *the cause* has led to flawed philosophical accounts of causality and that “cause” refers to a variety of different relations not some one specific causal relation.

the sort of close local knowledge that is provided by case study research—but need not commit us to different concepts of cause.

In all three of the examples that I have discussed, the application of generalized knowledge without full consideration of the evidence needed to justify that application in particular circumstances is problematic. Actions take place at a specific place and time. As such, they require attention to relevant particulars. At the very least, the case study research methodology develops attention to and respect for specific circumstances and thus an awareness of relevant differences (the extent to which a general theory may not pertain) as well as relevant similarities (the extent to which it does). We do not need to appeal to a conceptual causal pluralism to recognize the uncertainty of causal conclusions in the social sciences cannot be fully eliminated through better evidence. The sensitivity to detail that good risk assessment requires gives a further reason for case study research.

I have argued that the debate about case studies research in political science does not provide support for conceptual causal pluralism, though it does appear to provide additional means to acquire important evidence for causal claims. I have also argued that case study research is important for providing information needed for good risk assessment and that the inference to use (as opposed to warrant) requires risk assessment. The way the debate over methodology is currently framed, a philosophy of science strongly influenced by the legacy of Popper and positivism still dominates the discussion. A more pluralistic and pragmatically oriented contemporary philosophy of science offers better resources for understanding the role of methodologies in the social sciences. But pluralism about methodology need not commit us to a conceptual pluralism about causes. There is crucial information that case studies often provide that is better characterized as evidence for use. Pluralism about the aims of science—recognizing use as well as warrant as goals—makes clearer the need for methodological pluralism and supports an important role for case study research in political science.

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Bio

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