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Consequences of Positivism

A Pragmatist Assessment

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The aim of this article is to highlight the baleful consequences of positivism in contemporary political science. The author focuses on Gary King, Robert Keohane, and Sidney Verba's *Designing Social Inquiry* because it clearly and consistently articulates positivist commitments that tacitly and so often inconsistently, animate much work in the discipline. The author first establishes the positivist commitments that animate King, Keohane, and Verba's argument and then shows that precisely because of those commitments, their theory of inquiry generates two highly regrettable consequences: It hinders our ability to make sense of successful quantitative analysis generally and of causal explanation in particular. More diffusely, but no less important given King, Keohane, and Verba's practical aims, their theory of inquiry is self-defeating in its efforts to impart intellectual unity to the discipline of political science.

Keywords: *explanation; causal mechanisms; inference; positivism; pragmatism*

Political scientists typically deploy the labels *positivist* and *positivism* for solely rhetorical purposes. For many, *positivist* is a badge of honor, worn

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oneself or bestowed on others, but intended to identify those whose research is seen—if not actually, then at least potentially—as embodying the virtues of rigor, clarity, and solidity. For others, *positivist* is a generalized term of abuse, a sufficient reason to dismiss entire brands of research and those who conduct them as abstract, sterile, and politically dogmatic in disciplinary and extradisciplinary terms.

One would barely know from all this disputation that positivism is a philosophical position, one that not only can be defended but actually has been since roughly the mid–18th century. My aim is neither to trace the history of this philosophical position nor to resurrect it. I think positivism is a mistaken doctrine, one that insofar as it, usually unreflectively, infiltrates social and political inquiry, has had baleful practical consequences for both ongoing research in the discipline and debates concerning how that research is assessed. My aim is to identify some of those consequences as a first step toward figuring out how we might rectify them.

I focus in what follows on the influential treatise that Gary King, Robert Keohane, and Sidney Verba (KKV; 1994) wrote in the hopes of extending lessons developed for "quantitative" social science to more "qualitative" political research. KKV articulate in an admirably clear and consistent way the positivist commitments that tacitly and so often inconsistently, animate much work in contemporary political science. What is interesting about their treatise then, is not just that they themselves tacitly endorse contestable philosophical commitments but also the fact that in their responses to KKV, a set of accomplished political scientists largely take those commitments for granted.[1] In their haste to address KKV's advice on matters of research design, most of the reviewers pass over without comment the philosophical commitments embedded in KKV's effort to establish sound descriptive and causal inference—in short, "generalization"—as the nearly exclusive aim of good social science.

As is well known, KKV argue that although there are many quite diverse "styles" of research, all social and political inquiry shares, or at least should

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1. King, Keohane, and Verba (KKV; 1994) have attracted extensive commentary. See, for instance, Bartels (1995), Brady (1995), Caporaso (1995), Collier (1995), Laitin (1995), McKeown (1999), Rogowski (1995), Shively (1995), and Tarrow (1995). Many of these essays have been reprinted along with several new essays by the editors in Brady and Collier (2004). I retain citations to the original sources.

share, a common underlying "logic" of inquiry (pp. 3-4).[2] That underlying logic consists in a commitment to a quite specific conception of descriptive and more important, causal inference. After advancing and defending that conception, KKV issue a considerable amount of practical advice on research design that they insist will help social scientists to more fruitfully pursue the task of inference.

KKV insist that their "goal is practical: designing research that will produce valid inferences about social and political life" (p. 3). Several commentators suggest that they pursue this goal with conspicuously religious enthusiasm. Theirs, we are told, is "primarily a missionary effort" (Shively, 1995, p. 1193). Others attribute to KKV the "aim of evangelizing" (Rogowski, 1995, p. 467). Still others depict KKV as delivering a "homily," one that "puts forth a simple straightforward faith" (Brady, 1995, p. 12). Such portraits capture the spirit of the enterprise. The central article of faith for KKV is the crucial, indeed defining, importance of inference to social inquiry and the potential power of research design to improve our inferences. This faith derives from their interpretation of the way quantitative research, when properly conducted, proceeds in social science. The congregation to whom KKV most directly preach is composed of those scholars committed by predilection, necessity, or both to qualitative research.[3]

In this article, I raise a set of objections to the views that KKV articulate and, thereby, to the underlying philosophical current to which they give voice. I do not couch my arguments as a defense of qualitative research against incursions from quantitative approaches for at least three reasons. First, like KKV, I believe that there is an underlying unity to social inquiry. Although I do not argue the point fully here, that underlying unity revolves around the effort to produce causal explanations of social and political events where this involves specifying the underlying, usually unobservable mechanisms that generate observed patterns. We need not embrace the positivism that KKV endorse to defend that broad program. Indeed, their positivism is a standing hindrance to the task of generating causal explanations specifically and of establishing the centrality of that task to political and social inquiry. Second, many qualitative researchers, whether tacitly or explicitly, them-

2. All otherwise unattributed citations are to KKV (1994). Except where explicitly indicated, all stress or emphasis is in the original.

3. Although KKV sometimes portray their audience broadly as consisting of "all qualitative and quantitative researchers" (p. 3), it is quite clear that it really is considerably more restrictive. Hence, they explain in the preface that "our goal in writing this book is to encourage qualitative researchers to take scientific inference seriously and to incorporate it into their work" (p. ix). And if one somehow overlooked that statement, they soon reassert that "our main concern in this book is making qualitative research more scientific" (p. 18).

selves strike a broadly positivist stance. There is no unique mapping from the broad research "style" one adopts to the commitments one makes regarding underlying philosophical matters. Finally, a major part of the difficulty with KKV's positivist commitments is that they induce them to misconstrue what *quantitative* research actually entails. That misconstrual generates negative consequences for substantive social and political inquiry and for efforts to identify what factors actually unify it.

As already noted, KKV view themselves as engaged in the practical endeavor of improving qualitative social research. Conversely, they insist from the outset that they hope to avoid "abstract philosophical debates" (p. 3). Some readers may therefore suspect that my argument is miscast. I not only characterize the positivist views KKV so nicely articulate as a philosophical position but also, as my subtitle suggests, intend to assess those views from a competing philosophical perspective, namely, pragmatism. Suspicious readers, however, should consider two sorts of initial warrant for my argument. First, despite their protestations, KKV do not actually "sidestep . . . issues in the philosophy of social science" (p. 6). Not only do they tacitly adopt a contestable positivist view of such issues but also their positivism thoroughly colors both the way they understand the logic of social inquiry and their preoccupation with research design. Second, as a philosophical view, pragmatism approaches theories and concepts instrumentally and assesses them in terms of their consequences. So my criticism is not simply that KKV articulate a positivist approach to social and political inquiry. My claim, rather, is that in so doing, their argument generates decidedly negative practical consequences.

I do not offer a full-fledged pragmatist alternative to KKV's positivist approach to social and political inquiry. Pragmatists and positivists differ pretty much across the board on important matters of scientific inquiry.[4] But pragmatists differ among themselves in various ways too. Whatever their intramural differences, however, pragmatists see science, like other human practices, as a problem-solving activity (Laudan, 1981; Rorty, 1999, p. xxi). Pragmatists, thus, straddle the instrumentalist-realist dichotomy that political scientists unhelpfully treat as received wisdom in philosophy of science. They treat theories and methods as tools for solving problems and so judge them instrumentally in terms of their consequences (Dewey, 1948, p. 143). They also talk about unobservable entities such as causal mechanisms and see such talk as central to our ability to navigate the "real" world

4. Hacking (1983, pp. 58-65) sketches some of the intramural differences among pragmatists. On what separates pragmatists from positivists, Hacking states bluntly, "The differences arise from the roots" (p. 63; see also Laudan, 1990).

that such mechanisms constitute (Hacking 1983, pp. 63-64).[5] In this sense, pragmatists see success at solving both empirical and conceptual problems as equally important to assessing scientific progress (Johnson, 2002, 2003; Laudan, 1981).

My argument conjoins these features of pragmatism. KKV offer a vision of social science method, or what a pragmatist might call a *theory of inquiry*.[6] Pragmatists will judge that theory, just as they assess other conceptual tools, by its consequences. Among the most important of those consequences is that KKV define *causality* and *explanation* in ways meant to inoculate social and political inquiry from the purportedly dire effects of unobservable factors. This means that there is no room in their theory of inquiry for causal explanation understood as requiring analysts to identify the causal factors that generate variation in the phenomena being studied. As a consequence, KKV's theory of inquiry cannot make sense of repeated demands by those engaged in quantitative political science for just that sort of explanation.

The rest of the article consists of six sections. First, I briefly characterize positivism and show how KKV adopt a broadly but consistently positivist stance. The next section enters some clarifications and caveats intended to ensure that my portrait of KKV as positivists is both charitable and clear. In particular, I show that KKV advance a particular interpretation of causal inference and that that interpretation aims to remedy problems that emerge as central only on a positivist construal of scientific inquiry. The subsequent section shows that the positivist commitments KKV articulate are widely if inexplicitly accepted among mainstream political scientists. The next two

5. Even neopragmatists such as Richard Rorty (1999), who mistakenly seek to reduce pragmatism to a version of postmodern antirepresentationalism, allow that we "can make a distinction between hope and knowledge in cases where knowledge of causal mechanisms is available" (p. 165, n. 14).

6. For pragmatists,

> the rules of method are themselves theories or conjectures about how objects like us living in a world like this one might sort and choose between the ideas that occur to them. Our theories about inquiry, as embodied in the methodology of science, reflect long efforts by trial and error at figuring out how to produce dependable knowledge. (Laudan 1990, p. 102)

Some neopragmatist philosophers insist that notions of *the* scientific (let alone the *social* scientific) method do not designate any distinctive set of rules or procedures (Rorty, 1999, pp. xxi, 35-36, 95). Although correct, this observation is largely beside the point. The aim of social science—like that of all inquiry—is, as pragmatists have long noted, to fix beliefs in the face of real, practical doubt. Hence, we ought to assess methods not by some preexisting set of criteria but instead, by their consequences in attaining that aim. In that sense, what we require is not some essentialist criterion of "scientific method" but simply a recognition that our theories of inquiry generally allow us to identify better from worse instances of inquiry (Putnam, 1995, pp. 463-491). On this general matter, see Laudan (1981, 1990, 1996).

sections of the article identify important negative consequences of these shared positivist commitments. I argue that in important respects, the consequences of positivism are much less salutary than either KKV or commentators on their treatise commonly suppose. In the conclusion, I seek to preempt objections to my argument by sketching a position that takes causal explanation as a defining aim of social and political inquiry and suggest how that aim sustains a commitment to both the value of generalization and a common logic of social and political inquiry. This position proceeds from pragmatist commitments distant from KKV's positivism without embracing any of the "postpositivist" views now fashionable in the discipline.

Preaching Positivism

As a philosophical position, positivism encompasses a family of commitments. Consider the following succinct characterization:

> The key ideas are as follows: (1) An emphasis upon *verification* (or some variant such as *falsification*): Significant propositions are those whose truth or falsehood can be settled in some way. (2) *Pro-observation*: what we can see, feel, touch, and the like provides the best content or foundation for all the rest of our non-mathematical knowledge. (3) *Anti-cause*: There is no causality in nature, over and above the constancy with which events of one kind are followed by events of another kind. (4) *Downplaying explanation*: Explanations may help organize phenomena, but do not provide any deeper answer to *Why* questions except to say that the phenomena regularly occur in such and such a way. (5) *Anti-theoretical entities*: Positivists tend to be non-realists, not only because they restrict reality to the observable but also because they are against causes and are dubious about explanations. . . . (6) Positivists sum up items (1) to (5) by being against *metaphysics*. Untestable propositions, unobservable entities, causes, deep explanation—these, says the positivist, are the stuff of metaphysics and must be put behind us. (Hacking, 1983, pp. 41-42)

KKV endorse variations on each of the first five ideas. Whether they understand how these commitments hang together as a philosophical position is unimportant here. Indeed, KKV rarely so much as use the words *positivism* or *positivist* in the book. Nor is it important whether they embrace an antimetaphysical stance in other domains. However, it is important to note that just insofar as KKV ground their case for the methodological unity of social and political inquiry in this constellation of commitments, they hardly avoid messy philosophical issues. Instead, they tacitly embrace a philosophical position, and a highly contestable one at that. Although KKV may be

preaching, then, their sermon is grounded in a more or less systematic theology. Any assessment of the consequences of their proselytizing must account for the influence of those underlying theological ideas on their view of political inquiry.

To appreciate how closely the position KKV endorse tracks Hacking's (1983) sketch of positivism, consider the following set of interrelated claims by KKV:

> [1] There are some general ways to evaluate and improve the usefulness of a theory. . . . First choose theories that could be wrong. . . . Second, to make sure a theory is falsifiable, choose one that is capable of generating as many *observable implications* as possible. (p. 19)

> [1′] We should always design theories that are vulnerable to falsification. . . . However, for *evaluating* existing social scientific theories, the asymmetry between verification and falsification is not as significant. Either one adds to our scientific knowledge. The question is less whether, in some general sense, a theory is false or not—virtually every interesting social science theory has at least one observable implication that is wrong—than *how much of the world the theory can help us explain*. (p. 101)

> [2] We have emphasized that every theory, to be worthwhile, must have implications about the observations we expect to find if the theory is correct. . . .
>
> We should ask of any theory: what are its observable implications? We should ask about any empirical investigations: are the observations relevant to the implications of our theory, and if so, what do they enable us to infer about the correctness of the theory? In any social scientific study, the implications of the theory and the observation of the facts cannot be considered reliable if they are not based on theory and data in strong connection with one another and forged by formulating and examining the observable implications of a theory. (pp. 28-29)

> [3] Scientific research is designed to make descriptive and explanatory inferences on the basis of empirical information about the world. Careful descriptions of specific phenomena are often indispensable . . . but our particular definition of science requires the additional step of attempting to infer beyond the immediate data to something broader that is not directly observed. That something may involve descriptive inference—using observations from the world to learn about unobserved facts. Or that something may involve causal inference—learning about causal effects from the data observed. (pp. 7-8)

> [3′] We defined causality in terms of a causal effect: the mean causal effect is the difference between the systematic component of a dependent variable when the causal variable takes on two different values. (p. 85)

> [4] At its core, real explanation is always based on causal inference. (p. 75, n. 1)

> [5] We should choose observable, rather than unobservable concepts whenever possible. Abstract, unobserved concepts . . . are often used in social science theories. They can play a useful role in theory formulation; but they can be a hindrance to empirical evaluation of theories and hypotheses unless they can be defined in a way such that they, or at least their implications can be observed and measured. . . .
>
> In no way do we mean to imply . . . that concepts like intentions or motivations are unimportant. We only wish to recognize that the standard for explanation in any *empirical* science like ours must be *empirical* verification or falsification. Attempting to find empirical evidence of abstract, unmeasurable, and unobservable concepts will necessarily prove more difficult and less successful then for many imperfectly conceived specific and concrete concepts. The more abstract our concepts, the less clear will be the observable consequences and the less amenable the theory will be to falsification. (pp. 109-110)

The recurring theme in these passages is clear. They convey a preoccupation with verification and observation, skepticism toward causality and explanation, and distrust of theory insofar as it invokes unobservable factors. For KKV, seeing is believing.[7] Yet the claims they make here require interpretation if we hope to appreciate fully their positivist provenance.

Clarifications and Caveats

KKV are not positivist in a naive or dogmatic sense. Thus in contrast to "the over-enthusiastic claims of early positivists" (p. 41), KKV do not seek to dispense altogether with unobservable theoretical entities. As we shall see, given the scheme of causal inference they endorse, they cannot do so. Rather, KKV seek to tightly circumscribe the potentially malign effects of unobservable factors on social and political inquiry by tightly embracing the positivist penchant for observation. They insist that abstract classifications and unobservable concepts although unavoidable, are justified insofar as they contribute to generality. However, such theoretical entities also make positivists such as KKV apprehensive. Accordingly, the best way to immunize social research from the potentially damaging influence of unobservable factors is to tightly link whatever concepts we employ to their "*observable consequences*" (p. 111).

7. KKV ask, "Social scientists who focus on only overt, *observable* behaviors are missing a lot, but how are we to know if we cannot see?" (p. 41). It is useful to juxtapose their query with this remark: "Positivism results from the conception that seeing is believing" (Hacking, 1983, p. 63).

This may seem only so much methodological common sense. Yet it reflects the extent to which KKV are animated by deep and quite thoroughgoing positivist commitments. This becomes clear when we explore their conception of causality and the view of "explanation" that it sustains. First, however, we should be clear about the nature of their undertaking.

Because KKV's primary audience consists of those engaged in qualitative research, it will help to start by recalling how they demarcate that domain. According to KKV, "qualitative research" consists in social and political inquiry "where numerical measurement is either impossible or undesirable" (pp. ix, 4, 53).[8] It is this predicament, they insist, that "bifurcates the social sciences into a quantitative-systematic-generalizing branch and a qualitative-humanistic-discursive branch" (p. 4). And it is just this dichotomous understanding against which KKV importune. To remedy the underlying predicament, they suggest that we focus on inference, or more colloquially, generalization. As KKV eventually indicate,

> Inference is the process of using facts we know to learn about facts we do not know. The facts we do not know are the subjects of our research questions, theories and hypotheses. The facts we do know form our (quantitative or qualitative) data or observations.
>
> In seeking general knowledge, for its own sake or to understand particular facts better, we must somehow avoid being overwhelmed by the massive cacophony of potential and actual observations about the world. Fortunately, the solution to that problem lies precisely in the search for general knowledge. (p. 46)

Here again we encounter the primacy of observation. But we also see that inference consists basically in the instinct to generalize. Thus KKV insist that "as soon as the subject becomes causal or descriptive inference, . . . we are interested in observations and generalizations that are expected to persist" (p. 112). What is more, the remedy for problems of improper or invalid inference consists solely in improved research design. "A *research design* is a plan that shows, through a discussion of our model and data, how we expect to use our evidence to make inferences" (p. 118). KKV do not portray the process of formulating such a plan as a mechanical or rule-bound undertaking. However, they do insist that any sound research design "must take place according to explicit procedures consistent with the rules of inference" (pp. 12, 99). So here we see that their two preoccupations—inference and research design—are mutually constraining. Proper inference depends on improved research

8. Collier, Brady, and Seabright (2003) provide a more nuanced, pragmatist treatment of the quantitative-qualitative distinction.

design, whereas sound research designs are grounded in a proper grasp of the rules of inference.

In certain respects, much of this seems unexceptionable. Indeed, it is not difficult to find leading practitioners of qualitative research who endorse observation and generalization in seemingly similar ways. Consider a perhaps improbable example. Clifford Geertz (1973), whose own methodological pronouncements are intimately and unfortunately colored by the positivist impulses against which he felt it necessary to protest and who KKV themselves properly present as an exemplar of interpretive social inquiry, hardly disagrees with the broad strictures they advance regarding the necessity of observation and generalization.[9] After all, he is widely credited with reforming the notoriously slippery idea of "culture," hoping thereby to render it useful to social and political inquiry precisely by conceptualizing it in ways more amenable to public observation (Ortner, 1984, p. 129). Geertz also insists that any interpretation be subject to "explicit canons of appraisal" (p. 24) even as he laments the dearth of resources with which to conduct such assessments. Moreover, Geertz reminds us that anthropologists "don't study villages (tribes, towns, neighborhoods . . .); they study *in* villages." And he expressly concedes that although they are "particular," the findings of ethnographic case studies are "not privileged," punctuating his point by noting that "where an interpretation comes from does not determine where it can be impelled to go" (pp. 22-23). Clearly, Geertz's "interpretive science" (p. 27) aims at generality. The issue is how one understands the commitment to generalization.[10]

The disagreement between positivists such as KKV and those who espouse interpretive approaches revolves around the different ways they respond to the demand for causal explanation. KKV endorse one highly contestable view of explanation that they think allows them to circumvent the idea of causality as commonly understood. I return to their view below. Advocates of interpretation, by contrast, regularly—and mistakenly in my estimation—deny that they are involved in the enterprise of causal explanation at all. The irony, of course, is that despite their supposed differences, committed positivists and advocates of interpretation both abjure the task of

9. Compare Fenno (1990), for whom not only observation but also the quest for generalization is a recurrent theme. Likewise, Theda Skocpol (1979) aims to "generalize about social revolutions, to develop explanations of their causes and outcomes" (p. 35) based on evidence systematically gathered via comparative historical inquiry.

10. Hence, Geertz (1973) warns that the difficulty of moving "from local truths to general visions" on his view represents "a major methodological problem, and for the most part a badly handled one" (p. 21). The point here is not that Geertz himself (or his interpretive progeny for that matter) have resolved these difficulties, only that they are fully aware of them.

causal explanation. I will not pursue this point here. At the moment it is more important to get clear about the view of causality and explanation that KKV advance.

One might take statements such as the following as grounds for claiming that KKV depart from the sketch of positivism I offer in the previous section:

> Social science research, whether quantitative or qualitative, involves the dual goals of describing and explaining. . . . Each is essential. We cannot construct meaningful causal explanations without good descriptions; description, in turn, loses most of its interest unless linked to causal relationships. (p. 34)

In passages like this, KKV apparently endorse just the sort of things—explanation and causal relations—about which positivists are especially dubious. But words such as *cause* and *explain* can be misleading if they are used in ways that fail to pick out commonly understood concepts.

KKV adopt and amend a particular view of causal inference. They do so with little fanfare.[11] Their approach concentrates on identifying what they call the "causal effect" of some explanatory or independent variable on some other dependent variable. I will not rehearse the nuances of their view because KKV lay them out concisely and clearly (pp. 75-85, 91-99). Instead, I want to call attention to its revisionist implications. The view KKV endorse purportedly redefines causality in terms of observable outcomes—the causal effect. This, in turn, allows them to reduce the task of explanation (recall [3'] and [4] in the previous section) to the estimation of causal effects. In this way, passages such as the one quoted above notwithstanding, KKV do not depart from the basic positivist commitments Hacking (1983) identifies. If seeing is believing, then one task for positivists simply is to redefine any suspect philosophical concepts in terms that render them more fully susceptible to the protective force of observation.

Although KKV present all this in a matter-of-fact manner, their position actually is contentious. Philosophers disagree quite vigorously among themselves when it comes to matters of causality and explanation and how the two are related. Despite their disagreements, however, they tend to agree on a

11. The view KKV adopt derives from Holland (1986). KKV acknowledge this debt in passing (p. 76, n. 2). My point in what follows is assuredly not that they are at all dishonest in relegating their debt to Holland largely to footnotes. Nevertheless, by proceeding as they do, KKV create the false impression that Holland presents an uncontroversial account of how causality properly enters social and political research. Goldthorpe (2001) nicely situates the view KKV endorse in the context of competing understandings of causality and explanation in social inquiry. But note that he is preoccupied with the statistical provenance of these competing conceptions and refers not at all to the philosophical commitments that animate them.

central issue: Explanation involves the effort to identify how observed phenomena are brought about, to specify the causal mechanisms that generate those features of the world for which we wish to account. In stark contrast to the view KKV advance, then, there appears to be broad agreement among philosophers that "citing causes explains while citing effects does not" (Hausman 1998, pp. 161-163). Although this consensus in itself hardly constitutes a sufficient reason to reject KKV, it does raise plausible suspicions about the views they advance. These suspicions in turn are borne out when we come to examine the consequences of their positivism in the fifth and sixth sections below.

In this respect, two features of KKV's position invite comment. First, the concerns that drive it really are positivist all the way down. Both their analysis of causal effects and how to estimate them, as well as virtually all their practical advice about research design, are animated by the specter of what they call "the fundamental problem of causal inference."[12] This difficulty emerges at the center of their definition of a causal effect (recall [3'] in the last section). On their account, a causal effect consists just in the difference between the value some dependent variable $\mathbf{Y}_*^{\mathrm{I}}$ takes on (in their example, the percentage of generic districtwide—specified by subscript *—vote some candidate garners) when the relevant independent or explanatory variable $\mathbf{X}$ assumes one characteristic (the candidate is an incumbent) and the value $\mathbf{Y}_*^{\mathrm{N}}$ takes on when $\mathbf{X}$ assumes another, counterfactual characteristic (the candidate is not an incumbent). So the causal effect, according to KKV, is a "theoretical quantity" equivalent to $\mathbf{Y}_*^{\mathrm{I}} - \mathbf{Y}_*^{\mathrm{N}}$ (pp. 76-79). Because in any empirical inquiry one of these values will be counterfactual, the "fundamental problem" arises because this crucial difference is in principle *unobservable*.[13] It hardly is unfair to characterize KKV's entire enterprise as a single-minded effort to inoculate social and political inquiry from the malign influence that such unobservable factors—to say nothing of the "untestable assumptions" demanded by any effort to systematically circumvent them—might have.[14] Neither is it unfair to suggest that "the fundamental problem of causal inference" looms especially large for KKV just insofar as it runs afoul of their positivist commitments.

12. This phrase is due to Holland (1986, p. 947) whose usage KKV first borrow and then amend.

13. On this point, see Holland (1986, pp. 946-947) and KKV (pp. 76-82).

14. KKV stress that this problem really is "fundamental," openly acknowledge that their practical recommendations regarding research design are virtually all aimed at helping researchers "avoid" or "circumvent" it, and concede that all such strategies of evasion trade on "some untestable assumptions" (pp. 79-80, 91). The specific assumptions KKV make—"unit homogeneity" and "conditional independence"—are unimportant for present purposes (pp. 91-95).

Second, although it is important to let KKV speak for themselves, we need not take their views at face value. They begin by announcing that their book "focuses on *the* essential logic underlying all social scientific research" (p. 3, italics added) and punctuate their conclusion with the message that "valid inference is possible only so long as *the* inherent logic underlying all social scientific research is understood and followed" (p. 230, italics added). Yet despite their matter-of-fact presentation, KKV endorse a disputable approach to scientific inference.

Unfortunately, most mainstream critics pass over without comment KKV's claim that they identify *the* logic of causal inference. This acquiescence places critics at a disadvantage not only in responding to KKV but also in grasping their enterprise in the first place. The best way to understand the problems this generates is to see that KKV hardly are alone.

The Choir

KKV quite clearly are preaching positivism and the published reception of their methodological reflections by mainstream political scientists suggests that they are preaching to the choir. The clearest indication of this tacit positivist consensus is the fact that none of the critics who specifically addressed KKV's treatise when it first appeared so much as acknowledge the philosophical commitments that animate the treatment of inference in the first half of their book. Instead, nearly all the "critics" rush to the final three chapters to challenge this or that aspect of the advice KKV offer on matters of research design. This is unsurprising because such matters concern the sort of empirical problem—of observable evidence and how we might discipline it—with which political scientists are unjustifiably preoccupied (Johnson, 2002, 2003).

Even the rare commentator who does address more conceptual problems never calls into question KKV's underlying positivism. Henry Brady (1995), for instance, starts by lauding KKV's argument as "lucid and well-organized" yet almost immediately concedes that it is "not exactly clear how 'explanation' fits into KKV's categories of descriptive and causal inference" (pp. 12-13, n. 6). Turning to the example of incumbency advantage KKV use to illustrate their concept of causal effect, Brady rightly suggests that that concept does nothing to illuminate "what aspects of incumbency create this advantage" (p. 14). In short, KKV never "specify the causal mechanisms" needed to sustain a persuasive explanation. Brady notes that KKV insist they are not averse to this task. Indeed, using good positivist language, KKV repeatedly allow that the effort to identify causal mechanisms "makes intu-

itive sense" (p. 85). But Brady then lets KKV off the hook; he never presses them to demonstrate how explanation actually fits into their methodological scheme. And other than supplying a bit of concessive hand waving, KKV never do so (pp. 85-87).

Insofar as KKV remain true to their positivist commitments, their honest response to Brady's (1995) perplexity is that their scheme of inference simply cannot accommodate explanation as he depicts. It is not, as Brady allows, that KKV simply do not "pay enough attention" to problems of explanation, as though they might persuasively append meaningful concern for causality and explanation onto their argument (see also Laitin, 1995, pp. 455-456; Rogowski, 1995, pp. 469-470). Rather, KKV's philosophical commitments prevent them from doing so in a consistent, systematic manner. For it is those commitments that impel KKV to *define* causality in terms of causal effects and so reduce the task of explanation to the estimation of those effects. This much should be clear from the analysis of previous sections. But for the sake of clarity, the reasoning runs like this. For KKV, as for other positivists, seeing is believing. Hence, they place a premium on observation and are thoroughly suspicious of explanation because it invokes unobservable processes. As a result, although they acknowledge that explanation is a central goal of social inquiry, they first redefine it in ways that reduce it to the task of inference or generalization and then construct a scaffolding of precepts for research design intended to immunize that task from the threats posed by unobservables. In other words, rather than offering sound advice about how to construct reliable causal explanations, they redefine the enterprise in such a way that it is satisfied once we estimate the relevant causal effect. This, finally, allows KKV for rhetorical purposes to relegate even secondary concern for causal mechanisms—which typically are *un*observable—to the nebulous realm of intuition.

The perplexity Brady (1995) evinces is, to use his language, a direct result of neglecting the "theology" that sustains KKV's preaching. This leads Brady (pp. 11, 13) to dramatically understate the importance of being "scrupulous" not just about the arguments KKV advance but the basis on which they advance them as well. For as I hope to have shown, it is the *philosophical* position that KKV adopt—more precisely the highly questionable positivist underpinnings of their views—that account for both the specific "substantial defects" Brady rightly identifies in their work (e.g., "such as equating explanation with causal inference") and those aspects of it (e.g., their single-minded preoccupation with "increasing the number of observations") that because he finds them merely perplexing, generate only "vague worries" (Brady, 1995, pp. 13, 18).

The problem posed by the tacit positivism prevalent among mainstream political scientists is not simply that it leaves even perceptive critics like Brady at something of a loss. If I am correct that positivism is a standing hindrance to clear thinking about the task of causal explanation, this insight might help clarify other lingering problems in the discipline. I briefly address just one of those now. It has to do with the troubling state of quantitative political methodology. This problem is important in its own right, but it also serves the useful purpose of directing our attention back to the presumption common to both KKV and their mainstream critics that what they identify actually is *the* logic of social and political inquiry.

Achen (2002a) reiterates and sharpens what has emerged as a persistent complaint in assessments of quantitative methods. He criticizes common practice among political methodologists (and their eager clients in the discipline) for failing to ground their techniques in any plausible "substantive theory" of underlying social and political processes where, more specific, this involves providing "legitimate theoretical microfoundations" for their "statistical procedures" (Achen, 2002a, pp. 436, 424). The result of this common practice, according to Achen, has been that innovations in quantitative methods have been largely "irrelevant to genuine empirical advance in the discipline" (p. 437). Presumably, providing the sorts of conceptual underpinnings he suggests would rectify this dire situation.[15]

This is a persuasive, if damning, assessment. But let us be charitable toward both the quantitative methodologists among us and those who rely on their work. Suppose Achen's indictment is somewhat exaggerated. The situation he portrays nevertheless is quite ironic. It surely should lead one to wonder why complaints like his largely have fallen on deaf ears. Why have political methodologists not devoted their efforts to worrying about conceptual foundations rather than proposing new, increasingly specious estimators? The matter will perhaps look slightly less puzzling if we recognize that like other subfields in the discipline, political methodology is infused with the same positivism that KKV articulate. If, as KKV assert and as their various mainstream critics concede virtually without complaint, they identify *the* logic of causal and descriptive inference and if their interpretation of that logic is as thoroughly informed by positivist proclivities as I have suggested, then it would be extremely difficult to heed the call for a conceptually well-founded quantitative political methodology. This is so for just the reasons I

15. It is important to see that the problems Achen (2002a) identifies are essentially *conceptual*. Laudan (1981) differentiates between empirical and conceptual problems and argues that they are equally important in assessing ongoing research traditions. Neglect of conceptual problems regularly confounds political inquiry (Johnson, 2002, 2003).

outlined in my discussion of Brady's (1995) perplexity. The primary aim of positivist philosophical commitments is to inoculate social and political research from just those arduous theoretical tasks that Achen insists we must actively pursue.

KKV's claim to have articulated *the* logic of scientific inference and to have shown how attention to that logic allows us to evade or circumvent troublesome theoretical and conceptual matters is tendentious. Kevin Hoover (1990), for instance, argues that any econometric approach to the task of causal inference must make reference to the notion of a "causal order" that "is not a property of the statistically observed relations between variables" but rather, "of the underlying and not directly observable data-generating process" under investigation (p. 213). Likewise, John Goldthorpe (2001) defends a view of "causation as generative process" in which causal inference consists in identifying specific causal mechanisms that "even if not perhaps directly observable . . . actually generate the causal effect of X on Y" (pp. 8-9) and, thereby, the data available for statistical analysis.[16] This line of argument comports poorly with the premium positivists (recall [1] and [5] from the above section) place on observation or their corresponding aversion to unobservable theoretical entities. Yet it is just that premium and that aversion that drives KKV's methodological enterprise and that the choir to whom they are preaching finds so consoling. The point, if it needs making, is that whatever solace positivism affords mainstream political scientists is perverse to the extent that it derives from philosophical commitments that generate and sustain persistent practical difficulties in the discipline.

It remains to address what the theoretically disinclined positivist surely will deem the obvious question: So what? My pragmatist response to such obdurate shoulder shrugging is to identify two important ways the positivist interpretation of social and political inquiry has large, actively harmful consequences. Given their own practical aspirations, positivists surely should find these consequences quite disconcerting.

What are the consequences of KKV's positivism? I think their argument has two consequences that are perverse given their own practical aspirations. First, their views hinder our ability to make sense of successful quantitative

16. It is important that Goldthorpe (2001, pp. 8-9, 12-15) advances this view of causal inference as a "corrective" to the view KKV endorse and not simply as a "complement" to it. In this regard, he recognizes that one cannot simply *add on* a discussion of mechanisms to the definition of *causality* KKV advance. Goldthorpe insists too that this distinctive view of causal inference acknowledges both the persistence of unobservables and the theoretical tasks involved in modeling them. It is ironic that Brady and Collier (2004), after invoking Goldthorpe as warrant for their arguments, proceed to define *causal inference* as the "process of reaching conclusions about causation on the basis of *observed* [italics added] data" (pp. 276, 291).

social inquiry and so call into question the advice they impart regarding qualitative research. Second, and no less important given KKV's practical aims, their theory of inquiry is self-defeating in its efforts to impart intellectual unity to the discipline of political science. I examine the first of these consequences next and then turn to the second in the subsequent section.

First Consequence

KKV explicitly posit (recall [1'] above) explanatory scope as key to the assessment of social and political theories. As they assert, once we ensure that a theory can be falsified and specify it as concretely as is possible, we should take care to formulate it "so that it explains as much of the world as possible" (p. 113). With this directive in mind, we justifiably can ask whether KKV's own theory of inquiry explains the actual practice of social and political inquiry. As it turns out, this is a somewhat complicated task. On one hand, insofar as KKV articulate positivist philosophical commitments that are widely accepted by mainstream political scientists, it would be quite surprising if their theory of inquiry failed to capture important aspects of current practice. On the other hand, it is important not to judge the consequences of KKV's methodological program by wholly external criteria. Fortunately for present purposes, the commitment to positivism among political scientists, although broad, is largely inchoate and so inconsistent. It, thus, is fairly easy to demonstrate that the theory of inquiry KKV advocate fails to capture a recurrent feature of exemplary quantitative research, namely, the persistent aspiration to *explain* robust empirical generalizations.

Consider two examples that are perhaps as close to robust empirical generalizations as we have in political science. The first example is what commonly is termed the Lipset hypothesis, postulating a "strong positive linkage from prosperity to the propensity to experience democracy" (Barro, 1997, p. xii). Although this generalization is strongly supported empirically, not only do we lack an adequate theoretical account of the mechanisms that sustain the observed regularity but also those who explore the matter in quantitative terms view this shortcoming as an ongoing source of dissatisfaction (e.g., Barro, 1997, pp. 61, 86-87; Przeworski, Alvarez, Cheibub, & Limongi, 2000, pp. 6-7, 78, 101, 112-117).

The second example is Duverger's law, which asserts roughly that electoral systems characterized by plurality rule in single-member districts sustain two party systems. This regularity too is well established empirically. And much work has gone into specifying the conditions under which it holds (Cox, 1997). But simply as an observed regularity, Duverger's law also has

long generated theoretical dissatisfaction (e.g., Riker, 1982, p. 761). Although the mechanisms that sustain it are perhaps more clearly understood than those that underwrite Lipset's hypothesis, they remain subject to considerable disagreement (e.g., Cox, 1997, pp. 29-30). For positivists such as KKV, what is perhaps most disturbing is that this disagreement revolves almost exclusively around unobservables. The most commonly accepted mechanisms each presume that some political actors (elites or voters or both) act strategically. And this, of course, drives a wedge between their "observable" behavior and the reasons (beliefs and preferences) that motivate it.

Those tempted by KKV's positivism should find these examples of ongoing quantitative research discomfiting. For in both instances, those engaged in quantitative research demand a substantially more expansive conception of explanation than KKV are able to capture in their preoccupation with estimating causal effects. Specifically, they rightly suppose that explanation requires us to identify and examine the causal mechanisms that generate regularly observed empirical patterns. Consider quantitative inquiry into the relation of development and democracy. There one regularly encounters comments like these:

> Lipset was right in thinking that the richer the country, the more likely it is to sustain democracy. . . . Clearly this fact cries for an explanation. (Przeworski et al., 2000, p. 101)

> The positive relation between democracy and prior measures of prosperity—the Lipset hypothesis—is well established as an empirical regularity. Given the strength of this relation, it is surprising that convincing theoretical models of this mechanism do not exist. Thus development of such a theory is a priority for future research. (Barro, 1997, pp. 86-87)[17]

Such laments illustrate a more general point. Even when they are statistically well established, regularities like those Lipset and Duverger identify do not so much *provide* an explanation as *stand in need* of one (Goldthorpe, 2001, p. 11; Little, 1991, pp. 159-179). Even when, as is rare enough, political scientists manage to establish robust empirical generalizations between say, economic development and democracy, or between electoral rules and features of party systems, they repeatedly demand an explanation for ob-

17. The demand for explanatory mechanisms underlying the Lipset hypothesis is not restricted, of course, to those committed to large-*N* quantitative research. See, for instance, Bardhan (2005, pp. 87-88) and Wedeen (2005, p. 283).

served patterns rather than express a sense of satisfaction or accomplishment that might accompany success at providing one.[18]

If this dissatisfaction is not "merely philosophical," it nevertheless underscores the predicament that positivists like KKV confront, namely, that the philosophical stance they adopt affords scant resources for addressing it. As a theory of inquiry then, the program KKV endorse seems wrong just insofar as it fails to capture a central aim of the best quantitative research in the discipline. And this, of course, is deeply troubling insofar as KKV aim to improve qualitative research by imparting essential lessons derived from quantitative inquiry.

KKV might respond, as they do in a reply to critics, that they are concerned with the evaluation or assessment of theories rather than with how they are formulated or constructed (King, Keohane, & Verba, 1995, pp. 475-476). The most obvious problem with this response is that this is not strictly speaking true. KKV offer all sorts of advice about the formulation of theories. Most of that advice (again, recall passages [1] and [5] above, for instance) subordinates the task of formulating theory to the anticipated demands of testing or assessing it—at least as KKV interpret those demands. In that sense, their advice suffers from the mistaken notion that theory adds nothing to a plausible explanation over and above what can be gleaned from confronting some hypothesis with what they call "the hard facts of empirical reality" (King et al., 1995, p. 475). But the second, more telling problem is that this reply does not actually address the demand for explanation that shadows even our most robust empirical generalizations. For reasons I already have given, neither their portrait of causal inference nor the methodological advice that flows from it say much of anything about the task of causal explanation.

18. One might question whether the Lipset hypothesis and Duverger's law in fact are examples of reliable empirical generalizations. On my view, they are our best candidates. If these two examples are unpersuasive instances of generalization, then it is arguable that the discipline has produced none. That said, notice where the burden of argument lies here. If one were to challenge my examples, one would be under pressure not only to identify alternatives but also to demonstrate that they are not susceptible to the same sort of analysis I offer here.

Achen (2002a, p. 442), for example, mentions two additional "reliable empirical generalizations," namely, the democratic peace and the positive relationship between party identification and voting behavior. I will not discuss the literatures that these observations have generated in any detail. Yet both regularities stand in need of explanation. As one recent survey of the democratic peace hypothesis reminds us, the debates it has generated stem from the suspicion that "the absence of war between democratic states (even if one acknowledges it) is merely an empirical oddity for which no convincing theoretical explanation or rationale exists" (Ray, 1998, p. 39f). In an exemplary article, Achen (2002b) takes up the task of elaborating the explanatory mechanisms that might sustain the robust empirical literature on party identification. It seems, then, that these generalizations too conform to the pattern I identify in the text.

Here KKV also might respond by reminding us that theirs is a reformist or therapeutic enterprise. They might well suggest that demands for explanation of the sort we have just encountered overstate what it involves and that properly understood, explanation simply reduces to the estimation of causal effects. KKV of course never actually provide an argument for that position and I suspect that they cannot provide one. Moreover, as it turns out, they themselves leave the door open to demands for more expansive explanatory aspirations insofar as they rightly acknowledge that social and political theories pose "why" questions.[19] Yet although estimating a causal effect may establish that some independent variable **X** is causally relevant to some dependent variable **Y**, it surely does not establish why or how this relation operates (Brady, 1995). If my analysis is persuasive, however, KKV will be unable to address that discrepancy in a way consistent with the positivist commitments that animate their scheme of inference.

This is clear if we translate the Lipset hypothesis and Duverger's Law into KKV's scheme of causal inference. For Lipset, the causal effect of development on democracy for some country (denoted by subscript $_*$) at some time would be $\mathbf{Y}_*^{P} - \mathbf{Y}_*^{NP}$ where P indicates a situation in which the value of **X** for country $_*$ is prosperous and NP indicates nonprosperous. For Duverger, the causal effect of electoral rules on the number of parties for some electoral unit (denoted by subscript $_*$) at some time would be $\mathbf{Y}_*^{SMD} - \mathbf{Y}_*^{\sim SMD}$ where SMD indicates a situation in which the value of **X** for unit $_*$ is single-member district with plurality voting and ~SMD indicates some other electoral rule.

What does this translation exercise tell us? If, like KKV in their unguarded moments, we presume that "explanation—connecting causes and effects—is the ultimate goal" (p. 34) of social and political inquiry, how does their scheme "connect" the causes and effects that preoccupy Lipset and Duverger? The answer is that it does not do so. In both instances, their scheme helps establish presumptive causal relevance but nothing more. It does nothing to satisfy the curiosity of those, including KKV themselves, who want to know why or how in either case the particular causes generate their respective effects. Charitably, it seems that if explanation is the ultimate or less strongly, a crucial goal of inquiry, estimating causal effects falls far short of that aim.

Charity, however, has its limits. It arguably is more accurate to say that the positivist commitments that animate KKV's theory of inquiry present a

19. Early on, KKV suggest that "a social science theory is a reasoned and precise speculation about the answer to a research question, including a statement about *why* the proposed answer is correct" (p. 19, italics added). They subsequently assert that "causal theories are designed *to show the causes* [italics added] of a phenomenon or set of phenomena" (p. 99). Here their views echo the philosophical views I mentioned earlier in the text. Of course, KKV never make clear how these tasks are adequately discharged by establishing causal effects.

standing hindrance to commonly expressed aspirations to explain empirical regularities. Those positivist commitments drive a preoccupation with observation and generalization that in turn, leads KKV to embrace a revisionist definition of causality, a corresponding understanding of explanation, and a litany of methodological advice that simply leaves no room for the crucial conceptual tasks demanded by explanation as it commonly is understood. This shortcoming surely is serious enough in itself. Rather than dwell on it, however, I will briefly note two of its consequences.

First, the inability to grasp the demand for explanation leads KKV to obscure the usefulness of qualitative research. For example, they treat case-based research primarily as a way of generating additional "observations" and so by implication, as deficient relative to large-*N* quantitative inquiry. In so doing, however, they allow their positivist theory of inquiry to divert attention from the potential of case-based research as a means to analyze the workings of causal mechanisms, especially those that have heretofore remained unappreciated or poorly specified.[20]

Second, the sort of positivism KKV espouse underwrites widespread unreflective atheoretical propensities in the discipline and so interferes with our ability to assess the performance of quantitative research. Entire research traditions in mainstream political science proceed on very much the positivist terrain KKV map out. Here one need only consider ongoing work on political culture. Advocates of such research traditions regularly claim great empirical prowess even as their claims turn out, on examination, to be unwarranted precisely because they neglect the task of explanation and the properly conceptual problems that that task involves (Johnson, 2002, 2003). Yet having set out to immunize social and political research from the sorts of mysterious conceptual entities to which we must refer when explaining empirical regularities, KKV and those who subscribe to similar positivist theories of inquiry lack the resources to evaluate those claims. In this respect, the practical consequences of the positivist agenda that KKV so effectively articulate are perversely counterproductive.

Second Consequence

In addition to the regrettable impact KKV's theory of inquiry has for our understanding of quantitative research, I fear that the positivist commitments that animate their analysis have exacerbated unproductive polemics in the

20. Tarrow (1995, p. 472) and McKeown (1999, pp. 173-174, 184) criticize KKV for this. In fairness, KKV simply are inconsistent on the matter (see pp. 86, 225).

discipline. Insofar as KKV afford mainstream political scientists with reasons, however unwarranted, to embrace with renewed vigor positivist views of what constitutes "good" research, they unwittingly have contributed to ongoing rounds of mutual suspicion, recrimination, and misunderstanding. This clearly is ironic given KKV's intentions. But scholars on the qualitative side of our disciplinary divide—especially those who endorse one or another postpositivist or "antipositivist" philosophical stance—continue to take such positivist views as an invitation to reassert with renewed fervor well-rehearsed philosophical and methodological complaints whose most distinctive feature is their antipositivist genealogy.[21]

Although I cannot argue my case at length, it is easy enough to discern symptoms of this dynamic. Consider, for example, a recent survey of courses in "qualitative methods" offered in various graduate programs. After bemoaning the "positivist bias" in such courses, the author offers as evidence the fact that KKV has emerged as required reading in them. In this sense, then, KKV has come to exemplify the "New Orthodoxy" (Yang, 2003, p. 29). Likewise, by my count, KKV provide the explicit foil for seven of the "95 Theses" with which Anne Norton (2004) now seeks to subvert "hierarchy and . . . orthodoxy" in the discipline. They might easily provide fodder for twice that number.[22] I will not address Norton's arguments here other than to say that she offers precisely the sort of response that I think KKV and other positivists invite. The religious metaphor on which her title trades is an apt counterpoint to KKV's religious faith.

Mainstream political scientists might dismiss these as idiosyncratic examples. Yet if we turn more generally to the matters that most agitate outspoken perestroikans in the discipline, we find that many coordinate on a shared antipathy to "the idea that political science exists as a unitary enterprise dedicated to the accumulation of an expanding knowledge base of universal, decontextualized generalizations about politics" (Schram, 2003, pp. 836-837; see also Kasza, 2001, p. 597). KKV and those who adopt a positivist stance similar to theirs might well insist reactions such as these trade on caricatures of their views. Such a response would be disingenuous. For as I hope to have made clear, their view places a very high premium on observation and generalization and places nearly all methodological advice at the service of unifying the discipline on a particular interpretation of those goals. Even if they were to grant that, however, positivists such as KKV surely will protest that they never intended to provoke such discord and that it is unfair to hold

21. Geertz exemplifies this pattern. But it is a general problem in debates regarding the philosophy and methodology of social science (Laudan, 1996).

22. KKV appear in Theses 16, 53, 64, 68, 77, 79, and 84 but might have appeared in each of Theses 51 through 68 inclusive (see Norton, 2004).

them responsible for the reaction of dissenters. Such a response too would be disingenuous insofar as our positivist evangelists would just as surely be willing to claim credit for having contributed to unifying the discipline if qualitative researchers of various persuasions had enthusiastically embraced their philosophical commitments and methodological advice. Thus once one appreciates the unacknowledged positivist commitments that lead KKV to privilege observation and generalization at the expensive of theory and explanation, one can sympathize with the dissenters without accepting their reactionary views as wholly coherent or persuasive. Reactionary views rarely are. In this instance, however, a central feature of mainstream research to which perestroikans are reacting is its prevalent, if unselfconscious positivism.[23]

Conclusion

My aim has been to highlight the consequences of positivism. I focus on KKV only because they articulate in a laudably clear way positivist commitments that many mainstream political scientists embrace in less self-reflective and so even less persuasive ways. I first establish the positivist commitments that animate their views. I then argue that precisely because of those commitments, their theory of inquiry generates two highly regrettable consequences. It hinders our ability to make sense of successful quantitative analysis. And more diffusely but no less important given KKV's practical aims, their theory of inquiry is self-defeating in its efforts to impart intellectual unity to the discipline of political science.

The urge to focus on consequences is a pragmatist one. I have not, however, claimed to offer a fully worked out pragmatist alternative to the positivism that prevails among political scientists. I clearly cannot pursue that task here. I instead briefly discuss one crucial component of any such alternative, namely, a focus on causal explanation. This focus could deflate unhelpful polemic because as I note earlier, neither side of the positivist/antipositivist divide adequately faces that task. It also could mitigate the persistent unrequited dissatisfaction that even our most robust generalizations elicit. It would require that we work hard to elaborate theories of the causal mechanisms underlying observed regularities.[24]

23. I surely need to preempt complaints about depicting perestroikans as reactionary so let me quote the opening line of one recent manifesto that begins, "The Perestroika movement is a reaction against . . ." (Kasza, 2001, p. 597; cf. Norton, 2004, pp. 139-141).

24. There is a large theoretical literature on the importance of causal mechanisms in social science. In addition to the contributions to Hedström and Swedberg (1998), see, for example,

For present purposes, a mechanism **m** is a usually unobservable component of some more encompassing theory **T**. It typically operates at an analytical level below that which **T** seeks to explain and makes **T** more credible in the sense that **m** renders more fine-grained the explanations that **T** generates.[25] Note that insisting on the need for causal mechanisms does not mean that we abandon a commitment to either generalization or observation. It simply revises the way we construe those enterprises. Consider the following characterization:

> Mechanisms are composed of both *entities* (with their properties) and *activities*. Activities are the producers of change. Entities are the things that engage in activities. Activities usually require that entities have specific types of properties. . . . The organization of these entities and activities determines the ways in which they produce the phenomenon. Entities often must be appropriately located, structured and oriented, and the activities in which they engage must have a temporal order, rate and duration. . . . Mechanisms are regular in that they work always or for the most part in the same way under the same conditions. (Machamer, Darden, & Craver, 2000, p. 3)

As is clear from this passage, the theoretical challenge consists in not merely conceptualizing the more or less particular mechanisms that sustain causal explanations but also identifying the range of conditions under which we might reasonably expect those causal mechanisms to generate effects (Hoover, 1990; Knight, 1995; Little, 1991, 1993). We are seldom, if ever, in a position to delineate the necessary and sufficient conditions that the operation of a given mechanism presumes. But this does not imply that we must be resigned to generating catalogues of more or less disparate mechanisms (Elster, 1989). Careful attention to underlying conditions can enable researchers to place their explanatory bets with greater confidence when confronted with a multiplicity of possible mechanisms and to think systematically about how various mechanisms might interact. It provides as well an impetus to theoretically informed observation because we will want to establish not just whether our expectations are or are not borne out but also, in

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Elster (1989), Goldthorpe (2001), Johnson (2002, 2003), Knight (1995), Laudan (1981), Little (1991, 1993, 1998), Steel (2004), Stinchcombe (1991), and Tilly (2001). Virtually none of this makes its way into the discussion of KKV in the literature. See, for instance, the various contributions to Brady and Collier (2004). That said, I make no claim here to address, let alone resolve, all the difficulties raised by a mechanisms approach to explanation.

25. This characterization raises an important question of whether as some think (e.g., Elster, 1989; Little, 1993, 1998), mechanisms need to operate at the individual level or whether as others think (Hoover, 2001a, 2001b; Tilly, 2001), this insistence on microfoundations is misplaced.

either event, to try to ascertain why. On this view of social and political inquiry observation, causal explanation and generalization are complementary rather than placed in tension.

Positivists, of course, have objections to this competing if only partially specified theory of inquiry. In closing I consider two. One objection goes like this: "All this talk about mechanisms amounts to little more than reminding us about the importance of intervening variables and there is nothing in our positivist commitments that prevents us from acknowledging such factors." The problem with this objection is that it actually redefines the terms of debate in questionable if predictable ways. It is a crucial feature of any variable, whether we identify it as dependent, independent, "intervening," or otherwise, that it or more precisely the values assigned to it, are not just observable but observed (e.g., Brady & Collier, 2004, p. 312). But that is just what is at issue here. Achen (2002a), for example, insists that if we hope to remedy the shortcomings of political methodology, we must concentrate on providing "*theoretical* microfoundations." Likewise, both Hoover (1990) and Goldthorpe (2001) insist that the logic of causal inference in quantitative social research unavoidably involves the theoretical task of constructing models of unobservable causal processes and mechanisms. Thus it is question begging to reconceptualize the properly theoretical task of specifying mechanisms as an empirical exercise of identifying intervening variables.[26] This line of argument reflects the unfortunate tendency in the discipline to convert all problems of social and political inquiry into empirical problems. Yet both the putative warrant for that tendency and its unfortunate consequences for substantive social and political inquiry remain largely unexamined (Johnson, 2002; 2003; Laudan 1981). Minimally those who press this first positivist challenge must provide some argument to justify their assumption that causal mechanisms are always or even usually observable in the ways they must be to treat them as variables. Beyond that they must defend the presumptive primacy of observation that animates this objection.

26. Even those who are explicitly critical of KKV succumb to this preoccupation. Hence, as already noted, Brady and Collier (2004, pp. 252-255) invoke Goldthorpe (2001) as warrant for their approach to causal inference. But although Goldthorpe readily acknowledges that specifying causal mechanisms is a theoretical task that involves conceptualizing unobservable factors, Brady and Collier immediately turn the search for mechanisms into the quest for "causal process observations" that can complement the sorts of "data set observations" familiar in quantitative research. Neither Brady and Collier nor any of the other contributors to their recent volume offer a sustained discussion of the theoretical and conceptual issues surrounding the search for causal mechanisms. Nor do they—any more than do KKV or for that matter, Goldthorpe—consider the underlying philosophical issues involved. So although the tacit justification for their enterprise differs from KKV's positivism, Brady and Collier remain preoccupied with empirical problems.

A second positivist objection to claims that explanatory accounts must specify underlying mechanisms or processes warns that we quickly encounter a troubling "regress." KKV articulate this complaint as follows:

> If we posit that an explanatory variable causes a dependent variable, a "causal mechanisms" approach would require us to identify a list of the causal links between the two variables. This definition would also require us to identify a series of causal linkages, to define causality for each pair of consecutive variables in the sequence, and to identify the linkages between any two of these variables and the connections between each pair of variables. This approach quickly leads to an infinite regress, and at no time does it alone give a precise definition of causality for any one cause and one effect. (p. 86)

It is not clear, however, why pragmatists should find this criticism especially troubling. In the first place, as KKV articulate it, this complaint treats mechanisms as variables. It therefore is question begging in precisely the same way as the first positivist criticism. To repeat, why presume that causal mechanisms are observable in the ways they must be to treat them as variables? Second, this complaint trades on a double standard. The inferences sanctioned by their own positivist theory of inquiry are not, as KKV concede, final or complete or certain. Where, then, is the warrant to demand that explanatory accounts informed by a competing pragmatist theory need be final or complete or certain? Those who defend a "causal mechanisms" approach to explanation readily admit that any explanatory account will remain incomplete and provisional (Goldthorpe, 2001, p. 9). Finally, once we acknowledge that the effort to identify causal mechanisms inevitably will be incomplete, we also can recognize that it simply "bottoms out" in ways determined by the purposes of the inquirers (Machamer et al., 2000, p. 13).[27] Those purposes can be challenged by others, as can the extent to which those purposes are realized in any given study. But there is no reason to expect a hard and fast methodological rule to determine when to call a halt to efforts to identify mechanisms in any given instance. Likewise, there is no reason to worry that the effort to identify mechanisms will go on, without reason, indefinitely.

I hope it is clear from all this that a pragmatist theory of inquiry departs markedly from the views of KKV and other less self-reflective positivists insofar as it insists both that the task of specifying "a theory of the underlying structures or mechanisms that produce the explanandum" is central to the

27. This, of course, follows from the fact that explanations, more generally, "are relative to human interests" (Hacking, 1983, p. 53; Hausman, 1998).

explanatory aspirations of social inquiry and that any such theory will "postulate unobservable mechanisms and structures" (Little, 1991, p. 7).[28] Pragmatists, thus, approach questions of causation and of explanation in ways bound to make positivists quite uneasy. Such uneasiness, however, is an artifact of philosophical commitments that themselves actively hinder the task of explaining the social and political phenomena. In response to this uneasiness, pragmatists recommend that we abandon the positivist endeavor to *inoculate* our research against unobservable theoretical entities and instead pay explicit attention to how we can usefully *incorporate* them into our explanations.

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28. A theory of inquiry that focuses on causal mechanisms and the conditions under which they operate just as clearly departs from the antipositivist views of interpretive types such as Geertz, postmodern types such as Norton, and others insofar as it insists on the centrality of causal explanation even as it redefines the search for regularities in ways that itself departs from the implausible features of "covering law" models of generalization (Little, 1993). I cannot pursue this theme here. Johnson (1997, 2000, 2002) offers some initial direction.

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