

# Qualitative Methods

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Organized Section on Qualitative Methods

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## *Letter from the President*

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I am pleased to report that APSA-QM is off to a running start, and is already among the most active sections in the Association. Below is a quick update of some of the things the section has been up to.

### **Annual Meeting Activity**

The section turned out an overflow crowd for its first business meeting and reception. The high point of the meeting was the presentation of an award to David Collier, founding transitional president of the section, for his outstanding efforts in forming the section, as well as for his remarkable substantive contributions to qualitative methods.

The section also elected its first officers, including Vice President Elizabeth Kier and Secretary-Treasurer Colin Elman. In addition, the section elected Executive Council Members David Collier, Henry Brady, Theda Skocpol, Kathleen Thelen, and Deborah Yashar. Thanks are due to Paul Pierson (chair), Lynn Eden, Margaret Keck, Deborah Larson and Ron Mitchell for serving on the nominations committee.

As a result of the efforts of our 2003 Program Chair Cindy Skach, the section fielded six well-attended panels (see <http://asu.edu/clas/polisci/cqrm/apsa2003.htm>). In fact, the 2003 mean-adjusted attendance for the qualitative methods section was the highest of any APSA section. We also co-sponsored a short course on Field Methods, and the section thanks Evan Lieberman, Julia Lynch and Marc Howard for leading the seminar (see <http://www.apsanet.org/mtgs/shortcourselist03.cfm>).

Due to the very high attendance at the 2003 program, the section has been allocated 11 panels for APSA 2004. As this almost doubles the number of panels the section fielded in 2003, it represents a great opportunity for section members and other APSA members to submit paper and panel proposals on qualitative methods, on how these methods compare and contrast to or can be combined with quantitative or formal methods, and on how qualitative methods have contributed to particular research programs. The program co-chairs for 2004 are James Mahoney and Nina Tannenwald, both of Brown University. Their call for papers can be found at: <http://>

www.apsanet.org/mtgs/divisions/division47.cfm. We also anticipate that the short course on Field Methods will be offered again in 2004.

### Newsletter

Thanks to John Gerring's editorial leadership, the section newsletter is already among the best of its kind. The newsletter combines up to date information on section activities, substantial discussion of methodological questions, and useful information about developments and publications in the discipline. This second issue is being sent only to section members — if you got it from a colleague, we encourage you to sign up for the section (see below) to receive the newsletter in the future. The first issue can be downloaded at: <http://asu.edu/clas/polisci/cqrm/Newsletter.html>.

### Section Membership and How to Join

The section's membership doubled from August to October and now stands at more than 475 members. If you are not already a member, in order to receive this newsletter and enjoy the other benefits of section membership, please sign up now. The procedure for doing so is quick and easy and can be found at: <http://asu.edu/clas/polisci/cqrm/JoinSection.html>.

### Book, Article, and Paper Awards

The section has established the Giovanni Sartori Book Award, the Alexander L. George Article Award, and the Sara and George McCune Sage Paper Award. In the coming weeks the section will set up committees for these awards, announce them to section members via email, and post them on the web site. Further details on these awards can be found at <http://asu.edu/clas/polisci/cqrm/Awards.html>.

### Web Site

The section's web site (<http://asu.edu/clas/polisci/cqrm/QualitativeMethodsAPSA.html>) is a work-in-progress, but is already getting lots of traffic. As noted above, it has links for joining the section as well as links on the newsletter and the section's awards. The web site is jointly organized with the Consortium on Qualitative Research Methods (CQRM) and has links to CQRM's January Institute on Qualitative Research Methods as well as syllabi and working papers.

That's it for now. If you have any suggestions for moving the section forward, I hope you'll share them with me.

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## Symposium: Interpretivism

Responses to any of the issues discussed in this symposium or any future symposium may be posted on our web site or, pending space, in the next issue of the newsletter. (Letters should be sent to the editor as an email attachment.)

### Interpretations of Interpretivism

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What is interpretivism?<sup>1</sup> As is common with broad methodological debates, much hinges on matters of definition. Interpretivism might be defined residually — as non-positivism. However, this scarcely clarifies the matter, as noted by Robert Adcock and David Dessler in their contributions to this symposium. We might start with David Laitin's suggestion that interpretivism refers to interpretation or clarification — rendering the ambiguous into a clearer form. This is true enough, so far as it goes. However, in current usage the term seems to carry a good deal more freight.<sup>2</sup>

At an epistemological level, interpretivism might be looked upon as a form of truth-construction that relies on tests of *coherence* rather than (or in addition to) *correspondence* with external reality. Thus, interpretivists derive meaning by looking at the context of an action or event, its connection to a surrounding set of actions, events, and interpretations.

Interpretivism, classically, involves an appeal to an imagined whole. The term is also sometimes employed as a broad rubric encompassing all (or most) non-quantitative forms of analysis within the social sciences. All interpretivists will agree that they are in search of "inter-subjective" meanings, which are contrasted with "subjective" meanings and "objective" facts. All interpretivists agree on the importance of closely attending to the meanings that are attached to a set of actions or events by the participants under study; interpretivism is self-consciously actor-centered. For this reason, interpretivists sometimes refer to their object of study as *textual*. Although it may not comprise a written text, it nonetheless is approached in much the same way as a literary critic (or biblical exegete) approaches a text. Hence, the link to biblical hermeneutics and the *Verstehen* tradition (Gadamer 1975).

In many respects, interpretivism may be regarded as occupying a middling position between the ideals of naturalism (aka 'positivism') and post-modernism. It is consistent with the practice of old-fashioned literary criticism, particularly that focusing on authorial intentions (Hoy 1982), and with the disciplines of anthropology and history. It occupies a small, or perhaps not so small (again, the matter of definition is crucial), niche in political science and sociology.

The reader will note from the foregoing discussion that my definition of the subject relies primarily on interpretivism as it took form in the 1950s, 1960s and 1970s in work by Hans-Georg Gadamer (1975), Clifford Geertz (1973, 1979a, 1979b), E.D. Hirsch (1967), Albert Hirschman (1970), Alasdair MacIntyre (1971), Charles Taylor (1985), Georg Henrik von Wright (1971), and Peter Winch (1958).<sup>3</sup> Regrettably – from the point of view of clarity – the term has been appropriated in recent years for a range of usages that are difficult to differentiate from post-modernism or post-structuralism. However, since the work of Derrida, Lacan, Deleuze, Lyotard, and Foucault is fundamentally at odds with the work of Gadamer et al., I think it makes sense to keep these two methodological/epistemological subjects in separate boxes.

With this preliminary definition of the subject on the table, I shall now turn my attention to several methodological questions raised by the theory and practice of interpretivism. Most of these issues have been vetted previously. Even so, it may be helpful by way of entrée to our symposium to run through the catechism in a peremptory fashion. Many of these issues will be touched upon at greater length – and with considerably greater subtlety – in the remaining, more focused, contributions to this symposium. Consider this an entrée *outré* into a difficult subject, a probing of difficult questions. There will be ten, in all.

To begin with, it might be questioned whether interpretivism is better approached as a philosophy of social science, or even an epistemology, rather than a specific research agenda or method. There are few interpretivist “hand-books,” and many philosophical declamations. This is beginning to change, as recent Sage catalogs attest. Even so, the status of interpretivism seems somewhat different from its paradigmatic competitors, rational choice and behavioralism, which eschew philosophical discussion and are eminently practical (in the sense of showing practitioners how to practice the trade). Behavioralism and interpretivism are reverse-images of each other in this respect; one offers a ‘bare-foot’ program of research, the other a program of reflection with some suggestions about the practical business of studying social phenomena. Indeed, the most influential interpretivists (e.g., Gadamer, Hirschman, MacIntyre, Ricoeur, Taylor, Winch) are best known for their general-philosophical statements, not for their fieldwork (Geertz is a notable exception). Many are philosophers by trade. Thus, one might wonder whether we are comparing apples and oranges when we compare interpretivism with its supposed rivals. Arguably, the intention of interpretivism is to question the epistemological underpinnings of all work in the social sciences, not to show anyone how to do it. It addresses the question, What is it that we are doing when we do social science?, not How should we do social science? I do not intend to engage these epistemological debates here; I raise them only because they bear directly on this symposium. Here, we are treating interpretivism as a purported *method*. Whether it is appropriate to do so is the first question that we ought to consider. If it is not, then we have embarked on a fundamentally misguided venture.

Second, does interpretivism refer simply to non-causal explanations (aka descriptive propositions), proximal causal arguments or causal processes?<sup>4</sup> Or, alternatively, does interpretivism also invite speculation about deep-rooted (‘structural’) causal factors? Plausibly, close attention to actor rationality and to the particulars of a given research setting preclude the consideration of long-distance causes. Indeed, one finds few such arguments in the interpretivist canon. Here, causes are often treated as ‘constitutive’ (Wendt 1998, 1999), or as indistinct ‘processes’ where the cause and effect are so intermingled that it is difficult to tell one from the other. There are few exogenous causal factors in the interpretive universe. At the same time, interpretivists do not appear to have given up on big-C causation. Thus, it remains a point of ambiguity.

Third, does interpretivism generate propositions (descriptive or causal) that are *falsifiable*? Do their research programs, invoking a phrase from Lakatos, culminate in testable arguments? Alternately stated, how many interpretations of a given social phenomenon are possible, and how might one determine good interpretations from bad ones? This discussion is clouded over by a degree of ambiguity surrounding the concept of falsifiability, which Popper saw, quite properly, as a matter of degrees (Popper 1934/1968), and which subsequent work has further amended (e.g., Lakatos 1978). But the general idea is not in doubt. In order to be considered true, or possibly true (Popper himself affected an extreme epistemological skepticism), an argument must risk the possibility of being false. This means, in practical terms, that propositions, and the research programs that they are embedded in, should be specific about the outcomes that they seek to describe or explain; they should identify rival hypotheses; they should identify a research design (a specifiable procedure) to test the proposition, as well as rival hypotheses; and this research design should be replicable, so that future scholars can revisit the matter. I am employing scientific jargon here, but the basic ideas are not alien to rational thought in all areas, as Popper constantly sought to emphasize. In any case, there is room for argument over the extent to which interpretivists a) embrace the norm of falsifiability and b) achieve it in their work.

Fourth, does interpretivism exclude a consideration of factors that lie outside the ‘intersubjective’ realm – i.e., they are not constituted primarily by actors or by language but are, rather, objective (existing independently of human perception)? What about, for instance, the role of biological features of the human condition such as cognitive processing (Lakoff 1987); geography (Diamond 1997); or abstract institutions such as an electoral system (Taagepera and Shugart 1989)? Consider, as well, motivations that are in some sense universal rather than culturally constructed such as self-interest (the cornerstone of rational choice). Are all these issues outside the purview of interpretivism? Of course, interpretivists might respond that these factors enter the realm of interpretivism *insofar as they are culturally constructed*. Even so, this means that interpretivists are limited to examining the particular within the universal. Thus, while interpretivists might acknowledge, with Chomsky, that language contains some purely cognitive

(pre-cultural) factors, language evidently takes different forms at different times and places. The interpretivist would presumably be interested in the latter, not the former. Ditto for factors such as geographies and electoral systems; they provide a universal structure within which differences appear.

This discussion raises an important issue of a substantive nature. Does interpretivism commit one to a view of the world that grants causal priority to intersubjectivity (i.e., to culture, language, or thought) vis-à-vis various objective/structural factors? Evidently, one is not likely to be terribly interested in studying intersubjective meanings if these are largely super-structural. For the Marxist, the rational-chooser, and the cultural materialist (e.g., Harris 1979), intersubjective meanings are interesting only as dependent variables. In the writings of interpretivists, one finds implicit agreement on what one might label the relative autonomy of culture or intersubjectivity. Turtles rest upon turtles rest upon turtles; there is no ultimate ground. Social science is all about interpretations, ultimately.

Fifth, is quantitative analysis excluded from the interpretivist palette? Does one leave the realm of interpretivism as soon as one begins to count? For example, one might question why a study that involves talking to a small number of people in an unstructured manner (Hochschild 1981; Lane 1962) is interpretivist while talking to a large number of people in a more structured format (Campbell et al. 1960; Verba et al. 1995) is not. The issue of numeracy, particularly as pertains to survey research, is addressed at length in Laura Stoker's and Dvora Yanow's contributions to this symposium. Although it seems to be generally understood that interpretivists do not rely primarily on things that can be counted to reach insight into a topic (is there a single example of an interpretivist work based primarily on quantitative analysis?), they certainly do not exclude themselves from the realm of quantitative analysis. Again, one encounters an element of ambiguity in how interpretivism should be defined.

Sixth, how much leeway should the observer be granted in the act of interpretation? Are meanings understandable by the participants to be granted priority over meanings that lie outside the consciousness and language of participants? How much 'abstraction' from the brute facts should be allowed? Interpretivists do not generally go as far as ethnomethodologists (Garfinkel 1967) in this regard; they believe that some sort of reconstruction is necessary. But it is rather unclear (and rarely addressed) how much analytical license may be granted, while still remaining within the interpretive tradition. To clarify this zone of ambiguity, interpretivists may wish to appeal to the proper boundaries of a given intersubjective realm ('culture'); they are particularly reticent to generalize on a global scale (MacIntyre 1971). Yet, the appeal to cultural specificity presumes that cultures have clear boundaries. As it happens, this is *not* an assumption that most contemporary interpretivists would feel comfortable in making. Hence, the enduring problem of abstraction. If cultures are now global, does that mean that we can (must?) move to a global scale?

A seventh issue is closely related. To what extent does interpretivism allow one to generalize about the world (in either a causal or descriptive vein)? Interpretivism is usually

regarded as a particularizing art. However, it is important to emphasize that this matter, like all judgments of size, is relative. Thus, the question is not whether to generalize, but *how much* to generalize. Indeed, relative to post-modernist/post-structuralist work (Clifford 1988; Rosenau 1992), interpretivists are unabashed generalizers. Yet, against the backdrop of mainstream social science interpretivists usually find themselves on the particularistic end of the spectrum. They tend to emphasize the shortcomings of 'grand' theory, the extent to which local practices resist generalization, the particularity of context, the contingency of human action (see essay by Mark Bevir in this symposium). The very notion of 'context,' an important term in the interpretivist lexicon, suggests a smaller setting than is usual for the nomothetically-inclined social sciences. Local knowledge is prized; general knowledge distrusted.

Quite obviously, if grand generalizations are wrong then they are wrong and should simply be abandoned. This is, arguably, a question of fact. However, it seems more likely that we are facing what one might call a question of academic aesthetics rather than a question of ontological truth. Interpretivists do not deny the universal features of human cognition, geography, and 'institutions'; they feel strongly, however, that this is not where the action is. Non-interpretivists feel just as strongly that their job is to abstract from the particular; that the really interesting (theoretically fruitful) facts are those which are common to a number of disparate settings. It is difficult to avoid the conclusion that one is at the horns of a particularly irresolvable dilemma about what sort of truths are useful truths. Should we aim to explain a great deal about a little bit, or a little bit about a great deal? Each, one might say, offers a simplification of reality, but these two approaches – the 'splitting' and 'lumping' – simplify in quite different ways.<sup>5</sup> This point is nicely underscored in Dessler's essay (below).

An eighth issue is even harder to pin down, but deserves vetting anyway. Is interpretivism a left-wing, or 'critical,' method? There is a general sense that interpretivists occupy the left wing of the contemporary social sciences. This was not always the case; certainly it was not so in the time of Dilthey, when science was still very much on the side of Progress and Progress was still on socialism's side. We should recall that Marxism still carried scientific credentials in the nineteenth and early-twentieth centuries. Otto Neurath, a leading member of the Vienna Circle (whose members came to be referred to as Logical Positivists), was a Marxist sympathizer and, according to Gillies (1993: 24), was "in charge of central planning in the temporary Spartacist government set up in Bavaria after the first world war." Other members of the Vienna Circle also leaned toward the Left in the interwar years.

However, by the end of the twentieth century, with the onset of the Cold war, the upheavals of the 1960s, and the apparent demise of communism as a progressive ideology, science changed sides, beginning to look more and more like a handmaiden of the establishment. I do not wish to imply, therefore, that an interpretivist method conditions a critical or left-wing attitude toward the mainstream. Evidently, the relationship between method and substance changes over time and

may yet change again. Nor do I wish to suggest that the practice of an interpretivist method, by itself, inclines one to a critical opinion vis-à-vis the status quo. One can be a left-wing interpretivist as well as a right-wing interpretivist. It seems more likely that the association between interpretivism and radicalism flows from a more general feature of late-twentieth century academics in the US (and perhaps elsewhere): those in the humanities are more left-wing than those in the 'hard' sciences (Ladd and Lipset 1975). The fact is worth mentioning only because it is my impression that political/ideological issues lie just below the surface of many debates between interpretivism and its (right-wing?) twin, positivism.

Ninth, there are set of questions pertaining to interpretivism's proper place in contemporary political science (or social science more generally), an issue addressed by all the participants in this symposium (at least implicitly). Is interpretivism a distinctive way of doing business, as implied by Adcock and Bevir and argued strongly by Yanow, or is it better understood as indicating an important element of *all* (or most) social-scientific work, as argued by Laitin? If the former, should we think of interpretivism as a complement to other ('positivist') methods, as Stoker and Dessler suggest, or an opponent? Do interpretivist studies cumulate with non-interpretivist work? Or are they – to take the most radical view – incommensurable? If the latter, how might we adjudicate between studies of the same general subject that are interpretivist and non-interpretivist (or is it, essentially, a non-rational leap of faith)? Are interpretivist studies 'better' than non-interpretivist studies? Do we need more or less interpretivism, or are the proportions, at present, just about right?

Evidently, to claim that interpretivist work is valuable and important and deserving of an honored place in the social science universe is not to say that all other sorts of work should cease to exist. (I anticipate that few interpretivists would take this position.) My hunch is that much of the agonized debate over interpretivism – as with the equally agonized debate over rational choice – may be understood as a matter of achieving a proper proportion, and a mutually complementary interplay, among diverse research approaches. This observation does not resolve methodological debate; indeed, it invites debate (for how else are we to sort out the proportions?). However, it does put the resulting debate into a more hopeful, and more practical, frame.

Finally, there are ongoing and seemingly irresolvable questions about how to define 'interpretivism.' We return, thus, to the opening topic of this short essay. I raise the definitional question again at this juncture only to remind readers that much of what has been said is contingent upon a certain definition of what interpretivism is. Indeed, in circulating early drafts of this essay I was struck by how often I received the response that, while true of some interpretivists, what I had to say was not true of others.<sup>6</sup> I am quite sure that this is indeed the case.

Perhaps this is the proper point to end this initial foray into a complex and important subject, and to open the floor to other interpretations of interpretivism.

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### Endnotes

<sup>1</sup>I am grateful for feedback on various early drafts of this essay from Robert Adcock, Mark Bevir, David Dessler, David Laitin and Dvora Yanow. It has greatly benefited from their careful, and not uncritical, attention.

<sup>2</sup>I understand Laitin's essay to be arguing in a prescriptive mode; this is what we *should* be defining as interpretivism, he seems to be arguing. Here, I shall approach the subject from the perspective of normal usage. Interpretivism is a) what people say it is and b) what so-called interpretivists do.

<sup>3</sup>For useful compendiums, see Gibbons (1987), Natanson (1962), and Rabinow and Sullivan (1979).

<sup>4</sup>Sometimes, this issue is framed in terms of a dichotomy between explanation and understanding, as Adcock notes. However, since interpretivists do not seem to equate the former with causal argument and the latter with arguments of a descriptive sort, these terms are not helpful.

<sup>5</sup>A more complicated methodological issue beckons. While interpretivists generally present the particularizing move as the safer move, since it does not presume cross-cultural equivalences, it is important to note that particularizing statements also – by implication – generalize. To say, for example, that Germany is fascist is to imply that either that a) other countries are not fascist or b) that they are. Either (a) nor (b) is always implied; otherwise, the proposition about German fascism (and indeed, the very meaning of fascism) is nonsensical. In this limited sense, particularistic statements are also general statements.

<sup>6</sup>In this respect, the debate over interpretivism reiterates a persistent feature of the ongoing debate over rational choice. As the reader may recall, Don Green and Ian Shapiro (1994) issued a widely read and sharply critical evaluation of work in this genre nearly a decade ago. This was followed by a series of defenses and counter-attacks, many of which tried to show that Green and Shapiro's defi-

inition of rational choice was narrow and un-representative (Cox 1999). Again, much seemed to hinge on matters of definition.

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## Interpretation

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If we mean by an interpretation a rendering of an ambiguous input into a clear but partial output [or as the OED puts it, "expound the meaning of... something abstruse...to render meanings...clear and explicit"], then all social scientists are interpretivists.<sup>1</sup> Those in the social sciences who divide the methodological universe into interpretivists and (say) positivists are creating what Dewey has called an "untenable dualism." It would be like, as Pitkin has ironically observed in a related context, dividing up the world into herrings and fruits.<sup>2</sup> It can be done, but with little profit for understanding the world.

Interpretation involves a mix between art and logic. Ambiguous inputs (for example, a year of *New York Times* articles) lend themselves to a near infinity of partial meanings. Seeing something original, a pattern that has yet been seen before, involves artistry. However, seeing if the pattern is real, and not merely like one of Hamlet's interpretations of a cloud formation that Polonius accepts wholeheartedly (though a bit obsequiously), requires a set of diagnostic tests that are ruled by logic.

All science entails both the artistic and logical elements of interpretation, though there is no standard sequence, as these are two interactive processes. The artistic element is essentially theory, in Sheldon Wolin's sense of an encompassing vision.<sup>3</sup> The logical element requires a set of diagnostics. Interpreters of data want to know if the general principle that lies behind their interpretation is valid for a range of cases beyond the case at hand. The more violations – or as Thomas Kuhn calls them, anomalies – the less confidence one would have in the interpretation.<sup>4</sup> Interpreters will also want to know whether the logical implications of their vision are valid. Deriving observable implications of a vision and seeing whether they are valid is a standard tool of interpretation, whether performed by self-designated interpretivists or positivists.

To demonstrate the proposition of the universality of the two elements of the interpretive act, I draw on the iconic classic of political interpretation (Karl Marx's *Eighteenth Brumaire of Louis Napoleon*) and of sociological positivism (Emile Durkheim's *Suicide*). Marx's journalistic essay exemplifies the best in theoretically based narrative. The sardonic irony that pervades every section makes this essay gripping, the highest form of theoretically guided narrative. But the attention to historical detail, the weaving of a coherent story linked to larger historical forces, and the attention to apparent anomalies, make

*Eighteenth Brumaire* a model of the narrative form. The statistical method at the time of Durkheim's examination of suicide was still at a primitive stage – he did not have a standard tool to run regressions or even measure variance. Yet he invented algorithms that were apposite to his problem allowing him to systematically evaluate rival theories.

Using distinct methods in these two works, both Marx and Durkheim were interpretive masters. Rather than reifying a methodological dualism, a reading of these two works demonstrates the futility of carving out a separate methodological realm for qualitativists called interpretation.<sup>5</sup> Though one was immersed in narrative and the other in statistics, both Marx and Durkheim perform the same sorts of tests that add confidence to their interpretations. This essay thus appeals to the universality of the scientific method rather than to a divisive pluralism, where we all fend off innovation by appeals to the sanctity of our particular “ism”.

### Marx's *Eighteenth Brumaire*

Marx's principal thesis in *Eighteenth Brumaire*, as is the case in his entire corpus, is that class struggle is the motor of history and will lead in contemporary times to a revolution against the bourgeoisie. The events of December 1851 in which Louis Bonaparte, through a preemptive coup, restored France to a monarchy, however, are an apparent anomaly, for several reasons. First, it appears that with this coup, Bonaparte hoodwinked the bourgeois republicans, suggesting that history can be moved by great men (the Caesarist interpretation) rather than through class struggle. Second, it appears that history is moving backwards, from bourgeois republicanism to monarchy – rather than progressively in a way that would have the proletariat overthrowing a descending bourgeoisie. A fundamental goal of this narrative is to challenge these rival interpretations and to add support to his own.

The narrative strategy of reducing error compels Marx to show first that class struggle was the motor for regime change; second that despite the monarchical restoration, the bourgeoisie remained ascendant; and third, that the preemptive coup did not weaken the opportunities for the proletariat to successfully rebel and to achieve political hegemony. On the first point, Marx implies that an observable implication of the Caesarism interpretation would be that Bonaparte brilliantly exploited his strategic opportunity. To show the failure of this interpretation, Marx dredges up every piece of evidence he could find (though he resorts to sardonic irony to cement his case) to demonstrate that Bonaparte was a buffoon, and that it would be a gross misinterpretation of contemporary French politics to see his actions as a sign of individual greatness. Marx also argues that the divided bourgeoisie wanted to abdicate from power, as a restored empire would do its dirty work at no cost to them. It didn't take a Caesar to wrest control from abdicators!

In contrast to the Caesarist interpretation, Marx's analysis of the political landscape in which the key actors represented self-interested social classes made for a coherent narrative, with less central anomalies. If the so-called Party of

Order were to be analyzed merely as one that was pro-monarchy but divided between Legitimists and Orleanists (a non-class interpretation), it would miss their deep source of difference. Both were bourgeois, but the Legitimists controlled finance while the Orleanists controlled industry. Division among bourgeois interests compelled them to abdicate before each destroyed the other. Meanwhile, the party known as the Montagne was a coalition of proletariat and petty bourgeois. Party labels were only a thin veil covering vital class interests.

Another potential anomaly remains. Could there not be class struggle, yet with history taking a U-turn? Marx therefore asks whether the preemptive coup took France backwards in history? Marx argues no. In modern capitalism, he argues, the bourgeoisie cannot rule directly due to the complexity of the state apparatus that overwhelms the ministries. But given their internal divisions, the bourgeoisie cannot rely on elections to capture the presidency. They therefore supported an electoral law that effectively disenfranchised a significant percentage of the proletariat. Still they were divided, and found the best solution to be to rule indirectly through a populist leader who could get support from the lumpen elements in the cities (criminals and n'er-do-wells), along with the small peasantry in the rural areas who cannot organize themselves. Bonaparte was therefore a stooge of the bourgeoisie rather than a great man restoring Caesarism.

What does this say about the proletariat, defeated in 1848, and without any apparent power or energy in the parliamentary era? Here Marx argues that its inactivity amid the crisis of 1851 helped to overthrow bourgeois-controlled parliamentary power, and thus to give power to a single executive representing a single target, “in order to concentrate all its forces of destruction against it.” When the subsequent destruction occurs, Marx still optimistically predicts, “Europe will leap from its seat and exult” (VII-2). But like a juggler, Marx argues, by bringing surprises, with a new coup every day, “Bonaparte throws the whole bourgeois economy into confusion... makes some tolerant of revolution and makes others lust for it, and produces anarchy in the name of order, while at the same time stripping the entire state machinery of its halo, profaning it and making it a once loathsome and ridiculous” (VII-9). Thus the bizarre history of the 18<sup>th</sup> Brumaire was a preface to a proletarian revolution. Showing class (and not a great man) to be a motor of French politics from 1848-51, the bourgeoisie in ascendancy (and history not moving backwards), and the future closer to a proletarian revolution (and not a marginalization of workers in the face of peasant and lumpen power), Marx interprets the ambiguous events of 1851 such that contemporary France was not off the predicted path of proletarian revolution, thereby increasing confidence in his theory.<sup>6</sup>

### Durkheim's *Suicide*

Durkheim's principal thesis in *Suicide* is that its causes are “social”. Suicide rates vary depending on the degree of social integration. When it is very high, individuals who have a tendency to suicide will be willing to give up their lives for the betterment of their social group. This is altruistic suicide.

When it is very low, two mechanisms yield higher suicide rates. Individuals not linked to social networks lose any sense of the meaning of life, and if they have suicidal tendencies, these will be activated. The result is egoistic suicide. And also under conditions of low social integration, individuals who have a tendency to suicide feel no constraints from their social network to avoid such anti-social acts. The result is anomic suicide.

With this social theory of suicide, Durkheim is directly challenging a set of alternative interpretations. Theories based on individual psychology, race, gender, and religion pervaded the scientific literature – these interpretations had to be challenged for Durkheim’s interpretation to succeed.

Using data from state statistics, Durkheim sought to provide support for his interpretation, both through tests of observable implications of rival theories as well as his own, and through elaborations of his own theory to reduce error. As for tests, Durkheim went through rival interpretations one by one to demonstrate that the observable implications of those theories were inconsistent with the data. If psychological illness was the basis for suicide, an observable implication would be that those who had higher rates of psychosis (e.g. women) should have higher rates of suicide. However, the data show that men are far more likely to commit suicide than are women. As for racial theories, Durkheim reanalyzes a previous study by Morselli that showed a correlation between race (Cymries vs. Celts in France) and suicide rates (much higher for the Cymries). The test was ingenious – Cymries were taller than Celts, and shorter men were rejected for armed service. Morselli then correlated suicide with rejection rates in the French army and found the inverse relationship. An observable implication of this finding is that since all French are of different racial mixes between these two stocks, where there are pure Cymries there should be very high suicide rates. But in Scandinavia, where the Cymry stock is relatively pure, suicide rates are low. This leads Durkheim to interpret Morselli’s results as a spurious correlation, offering a rival interpretation of the data, namely that where Cymries live in France has lower social integration than where Celts tend to live, and more likely to produce egoistic suicide. These are but two tests of the observable implications of rival interpretations of suicide rates – each provides a devastating blow.

Durkheim also offers a plethora of tests of the observable implications of his own theory. If lack of social integration takes away all incentives to live, men should commit suicide at time when they are most commonly in society, namely at the workplace. Alternatively, women were most often in society on Sundays, and if they were devoid of a social network, this fact would be most revealed on the weekend. Indeed the data support the observable implication – men commit suicide on weekdays while women do on Sundays. While many critics have disputed whether the theory makes a prediction on the likely day of suicides by gender, my point here is that Durkheim is creatively interpreting his theory in the search for testable observable implications. In another test, Durkheim infers that education in modern society gives incentives for individuals to think for themselves, outside of tradition. An

observable implication, largely confirmed, is that higher levels of free public education would be associated with higher rates of suicide.

A simple theory linking low social network exposure to suicide rates clearly defeated its rivals. But Durkheim recognized that his interpretation faced several anomalies that made for bad predictions. For example, men in some armies willingly gave up their own lives to improve the situation of their comrades. This is an anomaly in a theory that initially focused only on anomic suicide: not only low but very high levels of integration make for suicide. Thus the introduction of altruistic suicide, thereby making the social integration theory curvilinear. A “U” shaped function for social integration reduces error and makes his interpretation stronger.

Throughout *Suicide*, Durkheim employs the language of “interpretation” to mean that he is providing a rendering of an ambiguous input (a maze of suicide data) into a clear output (a curvilinear relationship between social integration and suicide rates). Durkheim doesn’t advertise that his interpretation, though clear and powerful, is only partial. He doesn’t explain all the variance; he needs to add rather ad hoc assumptions (e.g. that women have a less developed mental life than men) to handle unexplained variance. Yet *Suicide* demonstrates that there is no absence of interpretive work in high-n statistical exegeses.

Interpretation for Durkheim, as it was earlier for Marx, involved gathering evidence on the observable implications of his theory and rival ones, and elaborating his own theory in order to reduce error. Taking ambiguous events and coalitions in order to produce a clear (yet partial) output is the modus operandi of *Suicide* as it was in *Eighteenth Brumaire*. Both Marx and Durkheim elaborate patterns and test the observable implications of those patterns, all in the name of interpreting an ambiguous world. Durkheim was no less an interpretivist in his positivistic treatise than Marx was in his journalistic masterpiece.

Is interpretation a distinct intellectual enterprise associated with qualitative narrative work? An examination of exemplary work in the narrative and statistical traditions has answered this question with a firm negative.

## Endnotes

<sup>1</sup>Webster’s “New Collegiate” gives the same: “to explain or tell the meaning of, present in understandable terms”. OED provides these exemplars: 1863, Fawcett in *Political Economy*: “The law interprets...his wishes with regard to the disposal of his property;” and 1795, Cowper: “The child who knows no better than to interpret by the letter a story of a cock and bull.” However, Webster also has: “to act as an interpreter between speakers of different languages.” This use, though common especially in UN circles, is not quite loyal to the core notion of interpretation, as it implies a direct mapping between equally ambiguous inputs and outputs.

<sup>2</sup>Hanna Pitkin, *Wittgenstein and Justice* (Berkeley: University of California Press, 1972), pp. 219-20.

<sup>3</sup>Sheldon Wolin, *Politics and Vision* (Boston: Little, Brown, 1960).

<sup>4</sup>Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

<sup>5</sup>I am aware that in the philosophy of interpretation (hermeneutics), its universality in science is a core theme. See Paul Ricoeur (1978) "Explanation and Understanding" in Charles E. Reagan and David Stewart, eds. *The Philosophy of Paul Ricoeur* (Boston: Beacon), pp. 149-66 for an argument paralleling the one in this essay. However, Ricoeur's definition of interpretation (p. 98, in the essay "Existence and Hermeneutics") is more mystical than need be.

<sup>6</sup>See Jon Elster, *Making Sense of Marx* (Cambridge: Cambridge University Press, 1965), pp. 388, 418. Elster demonstrates failures in the coherence of Marx's narrative, in inattention to mechanisms and in not addressing the anomaly of Germany, with similar conditions as France but with different results. This critique is asking for a stronger interpretation that failed due to Marx's "inherent lack of intellectual discipline" (*ibid.*, p. 390).

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## *Interpretive Empirical Political Science: What Makes This Not a Subfield of Qualitative Methods<sup>1</sup>*

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The ideal study in political science today would be the comparative study of health regulation of noodles in one hundred and fifty countries. In this way you have a sufficiently large mass of material to reach generalizations, and you don't ever have to have eaten a noodle—all you need is that data.

— Stanley Hoffmann  
(quoted in Cohn 1999)

The new section on Qualitative Methods draws attention to the fact that "Political Methodology," as the "old" methods section is called, does not encompass the whole range of research methods available to and used by scholars doing political research. And yet some researchers feel that "qualitative" methodology itself does not capture the full range of non-quantitative methods used by political science researchers. This was especially clear in the treatment of "qualitative methods" by several of the articles in the premiere issue of the newsletter: they did not reflect the character of the work that is increasingly being subsumed under the heading "interpretive research methods." It seems appropriate, then, to delineate what interpretive research entails by contrasting it with qualitative methods.

Such a discussion rests on an understanding of what is meant by "science" and whether there is, or can and should be, only one version of science in the area of study that we call political science. Contra Keohane (2003:11), the "standards of science held up to us by the natural sciences and espoused by economics and psychology," which Keohane thinks political science should reach, are not the only the only way to do

political science (nor is there, for that matter, a single way to do natural science, and phenomenological approaches are increasingly being heard in economics, especially among European scholars, and, even more strongly, in psychology). In sum, there are differences in procedure and in rationale for such procedures, which reflect differences of ontological and epistemological presuppositions. This is what keeps interpretive methods from being a subfield of qualitative methods: they do not live under the same philosophical umbrella.

I should note at the outset that my understanding of interpretive methods is informed by its presence in the three political science subfields in which I read most widely (public policy, organizational studies, and public administration), as well as by my readings in feminist theories and anthropology. The empirical and theoretical interpretive literature in these fields reflects work in Continental philosophy and US anthropological and sociological theory and philosophy (hermeneutics, phenomenology, some critical theory, symbolic interactionism, ethnomethodology, and pragmatism, among others). Other fields doing constructivist-interpretive research may draw on other influences, which would explain differences that I have not captured here: interpretive approaches are not (yet) unitary in their arguments and claims.

On the one hand, they encompass two dozen or more modes of analyzing data, each with its own particular systematic method, much as "quantitative" methods encompass a wide range of analytic forms. On the other hand, all interpretive researchers do not speak with one voice on some of the central philosophical and procedural issues. Interpretive philosophies have only been available to the English-reading world since the mid-1960s or so. Their explicit, conscious, and intentional extension into the world of research methods began much more recently than that, with the effort to argue for their standing in the world of science. The internal debates and intellectual arguments are still unfolding.

Lastly, "interpretive" methodologists make no claim to analytic exclusivity in their use of that term. All empirical scientists interpret their data. The reason for this particular usage is part of the argument that follows. Moreover, political theorists are also engaged in interpretive acts. Although there are overlapping hermeneutic processes in analyzing theoretical texts and contemporary policy and agency documents, for example, "interpretive research methods" as used in methodology commonly refers to empirical social science.

### **Nomenclature background**

The two-part taxonomy of "quantitative" and "qualitative" methods became entrenched during a specific historical moment, with the development of survey research, statistical analysis, and behaviorist theory, and was solidified with improvements in computer processing and the growing capacity to manipulate large amounts of numerical data with increasingly less human effort. The structural logic of the language of "quantitative" drew "qualitative" into play by counterdistinction: if statistics and "large n" studies (increasingly enabled by computer abilities) were quantitative analy-

sis, then “small n” studies using non-statistical methods – field-based interviewing and observing – must be “qualitative” analysis.

What “qualitative” originally designated was the features characteristic of traditional, Chicago-school-style field studies – ethnographies in anthropology departments and participant-observations in sociology departments, as those two separated and carved out distinct turf. Chief among these features are, one, word-based modes of accessing data, through observing (with whatever degree of participating; see Gans 1976), extended over time which immersed the researcher in the language and culture of the study’s domain, and “conversational” (a.k.a. “in-depth” or “unstructured,” or even “semi-structured”) interviewing, supplemented where appropriate by a close reading of research-relevant documents; two, word-based modes of analyzing word-data (rather than “translating” them into numbers for statistical analysis, e.g.); and, three, a richly detailed narrative form of communicating both data and findings, in which tables, figures, photographs and the like supplement and/or illustrate the data and/or analysis rather than presenting them. Moreover, traditional qualitative methods require a flexible response in the moment to observational (including participational) and interviewing circumstances, and so they are not “rigorous” in the literal sense of that word – they do not follow a stepwise course in the way that quantitative studies are described as doing. This does not mean, however, that such methods are not systematic, a point I return to below.

The requisite flexibility also means that the research design often changes in the face of research-site realities that the researcher could not anticipate in advance of beginning the research. For this reason, it is accepted methodological practice not to begin such a study with a formal hypothesis, which is then “tested” against field “realities.” Researchers in this mode more commonly begin their work with what might be called informed “hunches,” grounded in the research literature and in some prior knowledge of the study setting. Understanding and concepts are allowed (indeed, expected) to emerge from the data as the research progresses.

Increasingly, however, “qualitative” is being used to refer *not* to the traditions of meaning-focused or lived experience-focused research, but to small-n studies that apply large-n tools. Such studies have been coming under pressure to conform to the validity and reliability criteria that characterize quantitative methodologies. What is problematic here is that quantitative methods are, by and large, informed by positivist philosophical presuppositions and *their evaluative criteria have grown out of these ontological and epistemological presuppositions*, whereas traditional qualitative methods are informed, explicitly or not, by interpretive philosophical presuppositions. It is the struggle with the robustness of data, for example, *under the requirements of positivist science* that leads King, Keohane, and Verba (1994), e.g., to call for increasing the number of observations in order to improve small-n studies. However, it is a fallacy that small-n studies entail a small number of observations: they may entail a small number of research sites – one is not uncommon outside of explicitly

comparative work – but field studies of communities or organizations or polities entail large-n data points in their sustained observation (with whatever degree of participation) over extended periods of time, extended and repeated conversational interviews, and/or multiplicity of agency, policy or other documents read and analyzed.<sup>2</sup>

Moreover, the on-site flexibility and less step-wise research design that characterize traditional qualitative methods have been taken to mean that these methods are not systematic, as noted above, although this is hardly the case, as attention to the care with which settings and/or interview subjects and/or research-question-relevant documents are identified, considered, and selected; observations and interviews, carried out; and analyses, conducted will attest (see, e.g., Murphy 1980 on the former, Feldman 1994 on the latter). Neither “qualitative” nor “interpretive” means “impressionistic.” Along with procedural systematicity, the work entails a “philosophical rigor”<sup>3</sup> – a rigor of logic and argumentation – rather than merely a procedural “rigor.”

The view that this work is not systematic and the pressure to conform to quantitative criteria come out of an understanding that true and proper social “science” means one and only one thing – that set of ideas that developed through the 19<sup>th</sup> century as social positivists, and then evolutionary positivists, and then critical positivists, followed by 20<sup>th</sup> century logical positivists, argued that if universal principles were discoverable, through the application of human powers of reasoning and methodical, systematic observation, for the natural and physical worlds, surely they were discoverable for the human, social world; and set about to detail what the elements of such a science should be.<sup>4</sup> Early 20<sup>th</sup> century and subsequent critics of this approach argued along the lines that humans are different from trees and planets – for one, we are capable of self-reflection and of meaning-making – and that, therefore, human science requires the capability to explore reflexivity and meaning, at the same time that we enact those qualities in research practices.

The two-part qual-quant taxonomy has become a placeholder, a shorthand surrogate, for differences between positivist and interpretivist philosophical presuppositions concerning the character of social realities and their “knowability.” What we are increasingly looking at these days methodologically is, instead, a tripartite division, among quantitative, positivist-qualitative, and traditional qualitative methods. The latter have increasingly been termed “interpretive” methods because of their intentional, conscious grounding in or family resemblance to the ontological and epistemological presuppositions of the Continental interpretive philosophies of phenomenology and hermeneutics (and some critical theory<sup>5</sup>) and their American counterparts of symbolic interactionism, ethnomethodology, and pragmatism, among others. Despite differences of specific method, they share a constructivist ontology and an interpretive epistemology. They could as well, then, more fully be called constructivist-interpretive methods; because of the prevalence of the phrase “the interpretive turn” in social science (cf. Rabinow and Sullivan 1979, 1985) and the cumbersomeness of the full term, they are more commonly

referred to only as “interpretive” methods, although one also finds reference to “constructivist” or “constructionist” methods. This does not mean that these presuppositions are necessarily arrived at *prior to* methods; it is equally possible – and, in my experience, far more likely – that methodological inclinations of whatever sort are arrived at without any conscious attention to their philosophical groundings (especially when graduate programs do not include philosophy of science discussions in core courses). “Pre-supposition” should be taken in a conceptual or logical sense, rather than a chronological one, to mean what one must suppose about social realities and their knowability in order logically to hold particular methodological positions.

### Interpretive Presuppositions and Data Accessing and Analysis

Phenomenology provides a constructionist (or constructivist) ontology centered on the primacy of context; such context-specificity is fundamental to case-based research, and it is completely antithetical to a positivist scientific insistence on universal, generalizable laws or principles. Hermeneutics provides an interpretive epistemology rooted in the potential for multiple possible meanings of language, acts or physical artifacts; also context-specific, such potential multiplicities and their possible incongruences are what leads field researchers to access data from a variety of sites (neighborhoods, agency divisions, etc.) across a research setting. Interpretive researchers accord legitimacy to the local knowledge possessed by actors in the situations under study of their own circumstances, language, etc., exploring apparent discrepancies between word and deed across various sub-sites within the research setting (e.g., neighborhoods, classes, occupations, organizational levels, agencies). And it both is open to the possibility of multiple interpretations of events and analyzes these multiplicities.

The work is, in short, conducted as “sustained empathic inquiry” (Atwood and Stolorow 1984: 121), in which empathy constitutes an intentional grasping of the other’s meaning. Studying the “lifeworld” (phenomenology’s *lebenswelt*) of research site members and the political, organizational, and/or communal artifacts they embed with meaning, as hermeneutics would argue, requires a decentering of expertise on the part of the researcher: accessing local knowledge of local conditions shifts the researcher’s expert role from technical-rational subject-matter expertise to process-expertise; it is a radically democratic move (see Dryzek 1990, Schneider and Ingram 1997, Yanow 2000, 2003).

Moreover, interpretive researchers are aware of the extent to which their research formulations, choice of observational sites and persons interviewed, analytic frame, and writing all constitute the subject of study, rather than objectively reflecting it. All these are “ways of worldmaking,” as Nelson Goodman (1978) put it. Interpretive research reports often include researchers’ reflections on this process, which itself constitutes a significant departure from positivist-qualitative writing. Many go further than that, reflecting also on the di-

mensions of power that are inscribed through this process on the setting and/or participants in question (see, e.g., Behar 1993 for one example).

Interpretive research attends to data of three broad sorts: language (spoken by actors in the situation or in written form such as in state documents or individuals’ diaries); acts and interactions (including nonverbal behavior); and physical objects used in these acts or in written language (such as governmental buildings, census questionnaires, mission statements). These three classes of artifact are seen as existing in a symbolic relationship with their underlying meanings (values, beliefs, feelings). Meanings are not, as a rule, accessed directly: they are too abstract, and inquiring about abstractions leads to abstract generalizations, rather than situation-specific usages and practices. What can be accessed are the more concrete artifacts, and meanings are inferred from them. Data are accessed through one or more of three methods: observing/ participating, interviewing, and reading documents.<sup>6</sup>

Distinguishing between methods of accessing data and methods of analyzing data highlights the fact that “qualitative” methods may be used in keeping either with positivist or interpretive presuppositions. The list — suggestive, but hardly exhaustive — of word-based techniques for analyzing word-data in an interpretive vein includes category analysis, content analysis (close textual reading, rather than quantitative incidence-analysis such as that of Lasswell and de Sola Pool), conversation analysis, discourse analysis, dramaturgic analysis (building on the work of Kenneth Burke), ethnographic semantics, ethnomethodology, ethnoscience, frame-reflective analysis, grounded theory, metaphor analysis, myth analysis, narrative analyses of various sorts (oral histories, story-telling), (participatory) action research, semiotics, space analysis, textual analyses such as deconstruction, and value critical analysis, not to mention the more general analytic processes entailed in participant-observation and ethnographic research (see, e.g., Feldman 1994, Yanow 2000 for examples of some of these and further references).

One of the reasons that qualitative research has come under attack and that there is confusion over what interpretive methods entail is that researchers have not been as clear about their methods as we might be. Such methods are still typically taught and learned inferentially-inductively, through reading examples of others’ work and the apprenticeship-like quality of one’s own first field study. The teaching of them and the practice entail significant amounts of tacit knowledge (in Polanyi’s, 1966, sense). Nevertheless, we can, I think, do a better job than we have to date of making procedures and rationales more explicit, which will more fully reveal their systematicity. We need writings that are more reflexively explicit and transparent about how it is we do what we do, whether in field-work mode or in “desk-work” or “text-work” modes (Yanow 2000), and we need to continue to develop an articulated set of criteria based in interpretive presuppositions for judging the “goodness” of interpretive research (see, e.g., Brower, Abolafia, and Carr 2000 or Erlandson et al. 1993, p. 186 for suggestions of more appropriate assessment modes). I, for one, am not prepared to yield the label of “science” for

interpretive work, not at this moment in time: the work adheres to what I see as the two central characteristics of science overall, systematicity and testability<sup>7</sup>, and the claim to scientific status still carries significant societal weight.

Interpretive research is not new. It has a long history within political science, albeit not under that label, in the form of community studies, analyses of bureaucratic agencies, case-based studies of policy implementation, field-based studies of conditions for development, and the like. More recent interpretive analyses have studied budgeting and accounting practices (e.g., Czarniawska-Joerges 1992) and the role of built space in communicating state meanings (e.g., Goodsell 1988, Lasswell 1979, Yanow 1995). The extent and prevalence of this work varies across subfields; in many, it has been eclipsed by the rising focus on rational choice, formal modeling, and advanced statistical analyses. This has led many of its practitioners to find intellectual homes outside of “political science” departments, journals, book series and publishers, and associations, lending the work a certain invisibility within the discipline. Despite this marginalization, most researchers in an interpretive vein do not argue for replacing quantitative-positivist methods wholesale. Such a universalizing move would contradict one of our central philosophical tenets: good contextualists that we are, we mostly argue in favor of letting the research question drive the choice of methods, itself an implicit argument (and sometimes made explicitly) that positivist-informed methods are good for some questions, interpretive-informed methods, for others. Positivism, itself, is not the enemy: I am much happier living in a post-metaphysics world under positivist implications for class, race-ethnic, gender, and religious equalities and bureaucratized decision-making than I would have been in a pre-positivist one (ruled by monarchic whim and metaphysical explanations of the world and their inegalitarian applications).

What *does* concern interpretive researchers is the danger that methods-driven research narrows the range of questions that political science can usefully entertain and explore. In short, if the research question calls for sensitivity to contextually-specific meanings, it is likely to be addressed more usefully by some form of interpretive method. If it is important to the research question to know what the eating of noodles means to the lives, national identities or self-constructions of residents of each of Hoffmann’s 150 countries (in the epigraph) for their cultural or administrative practices, and if this meaning-making is to be allowed to emerge from the data themselves (i.e., through conversational interviews and/or participant-observation intended to elicit an insider’s understanding of the cultures involved), then statistical analysis of data gathered through a survey is unlikely to be helpful.<sup>8</sup>

For some methodologists, the problem of contesting approaches is resolved by the use of both positivist-informed and interpretive methods in a single research context, each one informing the other. My own view on this question is that given their contradictory ontological and epistemological holdings, the two approaches are incompatible. Indeed, as I examine such studies and talk with colleagues who propose them, I find that it is *not* the same research question that is being en-

gaged: when shifting from one approach to another, the research question itself is reformulated, although the two formulations are both engaging the same general topic. In that sense, then, both approaches can be useful in informing knowledge on the topic of concern, but the research itself proceeds differently in both cases, starting from the character of the knowledge that the researcher is interested in accessing. (Such a shift is actually illustrated by implication, in fact, in my tongue-in-cheek reformulation of the noodle study from a survey project to an interpretive one.)

In sum, interpretive methods are not concerned with some of the issues that appear to claim qualitative researchers’ attention currently: establishing concepts to be tested in the field; problems of measurement; sample size; theory-testing (in the scientific method sense). For interpretive researchers, data are legitimated in their word form as they are derived from (participant-) observation, conversational interviewing, and texts, rather than translated into measures. Researchers start from the assumption that bias cannot be reduced or avoided – the interviewer’s presence and/or reading may very well affect the interaction (in fact, we would be surprised if it didn’t, and many interviewers use that effect)<sup>9</sup> – rather than seeking to control against it. This is inherent in a constructivist view of data: they are seen as being co-produced in the interaction, rather than as objectified, free-standing entities that can be removed (“collected”) from the field setting.

Tools and techniques do not exist in a conceptual void. Methods are linked to methodologies, which themselves are understandings of or stances concerning the reality status of what those methods allow us to study and the knowability that we presume about that world. From a sociology of knowledge and sociology of the professions perspective, calling attention to “qualitative methods” in a professional association that until now has had only one, methodologically limited “political methodology” section as the “unmarked” case begins to move toward regrounding methods in methodologies, and methodologies in the philosophies of science and social science. Deconstructing the qual-quant taxonomy and raising the visibility of constructivist-interpretive methods within political science research practices takes us further toward the conceptual complexity that marks the human sciences.

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### Endnotes

<sup>1</sup>My thanks to Tim Pachirat and Peri Schwartz-Shea for their comments on an earlier draft, which helped me make some of my tacit knowledge about these methods more explicit, and to John Gerring for his editorial suggestions. The epigraph comes from a post by Tom Nichols to the Perestroika listserv (October 3, 2003).

<sup>2</sup>One could count, for example, the large number of hours of observation, the number of conversations held, the number of interactions, and the ensuing number of segments of interaction and/or conversation analyzed over the course of the research project. In some sense, each one of these constitutes an "observation" as that term is used in quantitative analyses.

<sup>3</sup>The phrase is Mark Bevir's, made in the closing discussion at the recent (2003) APSA roundtable on constructivist and interpretive methods.

<sup>4</sup>For discussions of these debates and references to original sources, see, e.g., Abbagnano (1967), Hawkesworth (1988), Polkinghorne (1983), Rabinow and Sullivan (1979). Although Hawkesworth's book is addressed to the field of policy analysis, the first half constitutes an excellent delineation of the philosophical issues at hand.

<sup>5</sup>Many critical theorists have accused phenomenologists, in particular, of disregarding issues of power and structure in their focus on the Self. Whereas this criticism may well hold at the level of philosophy, once one brings interpretive philosophies into the practical realm of political studies, one can hardly escape questions of power and structure. Hence, the overlap of concerns with some critical theory.

<sup>6</sup>Such data are "accessed" more than they are "gathered" or "collected." Neither acts nor, one hopes, objects or agency documents are removed from the field setting in which they occurred. What is brought back are the researcher's copious interview and/or observational notes, although copies of documents, interview tapes, and the like may be brought out of the field. This makes creating a database for other researchers' use problematic.

<sup>7</sup>I thank Tim Pachirat (personal correspondence, 2003) for drawing my attention to the fact that positivist and interpretive researchers most likely understand "testability" — as reflected in the statement "I was wrong about my findings" — in different ways. The difference reflects, at least, the distinction between seeing findings as reflections of objective reality and seeing them as constructions of that reality. I think his observation is correct. I have in mind the willingness to subject one's findings to scrutiny in an attitude of humility in the face of the possibility that one might be wrong, coupled with the passionate conviction that one is right (cf. Yanow 1997). I do not have the space to develop that point more fully here.

<sup>8</sup>But such a meaning-focused study is unlikely to tackle 150 disparate cultural sites!

<sup>9</sup>This point about readers' interpretations lies at the heart of "reader-response" arguments in literary theory; see, e.g., Iser (1989). The strongest argumentation for using aspects of the interviewer to elicit responses, including for responding critically when told something one disagrees with, is a contested issue among interview researchers. See, e.g., Holstein and Gubrium (1995) for one view.

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## *Is it Possible to do Quantitative Survey Research in an Interpretive Way?*

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I've been asked to address the question, Is it possible to do quantitative survey research in an interpretive way?, which requires first identifying what an "interpretive way" is. I will consider three ways of distinguishing interpretive research — in terms of its objectives, epistemological assumptions, and modes for analyzing empirical materials. My question, thus, becomes, Is quantitative survey research, as ordinarily practiced, capable of achieving interpretive research objectives,

reflecting interpretive epistemological assumptions, and enabling interpretive modes of analysis? And, To what extent can survey research practices be modified to incorporate an interpretivist project? I offer these comments as a quantitative survey researcher who is struggling to understand the challenges posed to survey research by interpretive approaches.

### Research Objectives

Let's say that the objective of interpretive political research is to understand how people make sense of political phenomena, to understand what any given political problem or policy or action or event means to them. If so, we immediately confront a disjuncture with survey research as it is ordinarily practiced. Much survey research in political science is *not* focused on how people make sense of politics, how they experience and understand it. Instead, it focuses on how some measurable property, A (e.g., a person's level of education), relates to another measurable property, B (e.g., a person's level of support for affirmative action policies), by way of testing causal hypotheses about the force exerted by A on B.

But, still, lots of other survey-based research in political science *is* directly interested in depicting what people believe and how they feel about political phenomena, not merely in establishing regularities between pairs of "objective" attributes. The numerous examples include work on how people understand and explain racial inequality, the "party images" research on how people understand the policies and values political parties advocate and the constituencies they serve, and the studies of Americans' dissatisfaction with campaigns and government.

Moreover, even if the question of interest to survey researchers concerns the causal link between attributes—say, that between education and opinion on affirmative action—the lingering question of mechanism shifts researchers attention back to peoples' ideas and experiences. Attempting to understand why education influences opinions on affirmative action leads survey researchers to ask how people who differ in their educational achievement come to differ in their beliefs, feelings, and experiences concerning affirmative action, which then produce differences in opinion. In short, mechanism questions invite rather than preclude exploration of the subjective.

Even so, such an enterprise—fitting "meaning" into an explanation of the link between two objective attributes—may still be found wanting. It either implicitly or explicitly entails that meaning is shared among those who share objective attributes like education levels, and only differ in important ways (so as to make them worthy of study) among those who differ in objective attributes. Further, it privileges objective attributes as the prime movers in the story of where politically consequential outcomes come from.

### Epistemological Assumptions

Even if survey research hones in on an interpretive objective, perhaps it will fail to honor an interpretive epistemology.

I offer four such objections to quantitative survey research.

1. Survey research's attempts to uncover people's beliefs and feelings impose a view of the relevant phenomenon on the respondent. This is usually by way of "closed-ended" questions that impose the researcher's own beliefs and commitments—be they articulated (as, for example, via operational definitions of concepts), or implicit (as, for example, via ideological bias). To take an example, survey researchers have long been interested in whether citizens' policy views have a self-interested basis. For any given policy, researchers usually first stipulate what kinds of interests are at stake, and then stipulate ("measure") for whom the policy is advantageous and for whom it is detrimental. They do not ask people what interests they see at stake, or how they see their own interests furthered or hindered by a policy.

They could, of course, move in this direction. Indeed, the survey tool of the "open-ended": question is designed to accommodate this objection.<sup>1</sup> In some pilot work that I conducted, using open-ended questions in a large-scale survey, I found that people's own sense of their interests did not conform to the conceptualization usually provided by researchers. For example, people worried a lot about whether a national health care plan would restrict their freedom to choose health insurance coverage levels, providers, doctors, and the like. The typical approach to studying self-interest ignores beliefs concerning freedom of choice, placing it in the realm of values and not interests.<sup>2</sup>

2. Survey research, with its emphasis on standardized interviews and interviewing techniques, treats the research enterprise as obtaining a "pure" or "true" response from the subject, and fails to appreciate that the researcher and subject, or at least the interviewer and subject, are engaged in an interaction that jointly produces the results. Particularly if one tries to move away from closed-ended toward more open-ended questioning, interviewers must be recognized as actively involved in the creation and interpretation of survey responses.

Survey researchers are, of course, aware of this to some extent, as reflected in work on "interviewer effects." Scholars like Lynn Sanders and Darren Davis, for example, have studied how the race of one's interviewer seems to shape survey responses and interpret these findings substantively, as the result of racially charged interactions in the survey interview setting. Still, most survey researchers view interviewer effects in a manner much less compatible with the interpretivist epistemological perspective: interviewers can be sources of both random and systematic error, which to the extent possible must be eradicated through improved training and interviewing techniques.

3. Survey research is doomed to fail in truly discovering how people make sense of politics and where those meanings come from because it abstracts individuals from their social and institutional contexts. My sense, and I confess I am not at all clear on this, is that scholars pursuing interpretive work believe that one cannot rightly come to understand what any one person believes without studying the beliefs of other people who are embedded in her life, and the beliefs of others that are embedded in her life through the media, curricula, novels, and

the like. The inherent intersubjectivity of meaning cannot be revealed by the methodological individualism of traditional survey research.

Perhaps this criticism of survey research, if valid, could be accommodated by moving to research designs that introduce network sampling and that link survey and media data. Put another way, it does not appear to be survey research, itself, that is the problem here but the typical research design that uses survey-based data collection. This might be remedied by sampling members of a relevant community (or network) and analyzing the data by connecting part to whole, as well as by linking survey data on individuals to data on the media (and other texts) they consume.

4. Survey research is incapable of fully revealing meaning because it relies merely on self-reports, i.e. language, and ignores how meaning is represented in other artifacts—acts, objects—that need to be observed, directly. Without joining observational data and self-report data any understanding of the subjective lives of individuals will be impoverished. Moreover, survey research doesn't even do a good job at getting at meaning as revealed in language. One cannot simply ask people questions and expect meanings to pop out. This is for one or both of two reasons. (A) People cannot consciously access and articulate all that they feel and believe.<sup>3</sup> (B) The question and response mode of survey research, even when open-ended questions are employed, is inadequate to the task of revealing how people do make sense of political phenomena. One needs to elicit stories or narratives, for example, and employ the many other conversational tools of in-depth interviewing in order to generate the empirical materials capable of illuminating meaning.

### Modes of Analysis

A final possibility is that the modes of interpretive analysis are incompatible with the quantitative analysis of survey data.<sup>4</sup> At least two different ideas are involved here.

The claim that important beliefs are inaccessible, mentioned above, emerges from a variety of psychological theories including those of psychodynamics and cognitive linguistics. Used to inform interpretive work, they suggest ways of analyzing texts that seem blatantly at odds with standard quantitative coding techniques. Take cognitive linguistics, for example. From a cognitive linguistic perspective, if you are interested in discovering how people think about something complex and abstract like the harm of pornography you must begin by getting the right kind of data; you must elicit a wide range of impressions about pornographic media and its use by individuals and across society. But then these verbal materials must be scrutinized for how the language people use reveals the metaphors they are implicitly but not explicitly using, and which reveal unarticulated, inarticulatable beliefs. This kind of ("latent coding") analysis is certainly at odds with the manifest coding of open-ended materials that typifies the survey researcher's work.

The more standard criticism of quantitative survey analysis involves the classic variable-centered vs. case-centered

(nomothetic vs. idiographic) dichotomy. Even if one were able to expand the verbal materials obtained in large-n research and subject them to sophisticated latent coding prior to a quantification step, any subsequent statistical analysis would link variable to variable. It would focus on how individual differences of one kind (variable 1) relate to individual differences of another kind (variable 2). From an interpretive perspective one might argue that two analytical errors are thus made. Because the chosen variables are abstracted from the rest that make up the case as a whole no single case is adequately represented in the analysis.<sup>5</sup> At the same time, the between-person analysis technique does not illuminate what is going on within individuals.

Of course, survey analysts can respond to some extent by building in complex statistical interactions between variables, which moves them in the direction of a more case-centered analysis (looking at how outcomes vary across individuals defined by collections of attributes simultaneously). But the statistical tools currently available won't take one far in this direction.

### Final Thoughts

I think it stimulating and productive to move questions like the conflict between interpretive and quantitative research to the concrete plane of actual practice. Such a dialogue is capable of enhancing researchers' methodological self-awareness and perhaps even spurring innovation—improving the work of those in each mode and maybe even motivating a substantive (not methodological) dialogue between them. Ultimately, I believe that the epistemology of interpretivism is so roundly incompatible with the positivism underlying many quantitative survey research practices that there is no way to fully reconcile the two. Still, as I have tried to suggest, there are points of accommodation. Speaking as a survey researcher, I think I benefit from thinking about the interpretivist challenge (which I cannot claim to have gotten right here). I have a better, and more reflective, sense of the particular epistemological viewpoint that underlies that data collection and analysis tools I use, and I am at least sometimes stirred to alter or abandon them in the face of their limitations.

### Endnotes

<sup>1</sup>Still, even open-ended questions embed researchers' assumptions and choices, and studies have shown that minor variation in question wording can elicit widely different responses.

<sup>2</sup>My favorite published example of survey-based public opinion research that seriously attempts to grapple with this general criticism from interpretivists is Elizabeth Theiss-Morse, Amy Fried, John L. Sullivan, and Mary Dietz's "Mixing Methods: A Multistage Strategy for Studying Patriotism and Citizen Participation" (in *Political Analysis*, ed. James Stimson, 3: 89-121. 1991). Rather than simply designing survey questions to tap "patriotism" as they, themselves, define it, Theiss-Morse et al. first try to uncover how "ordinary Americans" think about patriotism using small-n, intensive research and modes of analysis. They then develop survey questions based on the insights generated in this stage of their research.

<sup>3</sup>But clues to people's beliefs and feelings may still be found in the language they use, as I discuss below.

<sup>4</sup>My distinction between "epistemological assumptions" or issues and "modes of analysis" is artificial and the latter might best be subsumed under the former. But what I had to say in the last section was about the data survey research provides while in this section I am focusing in on what quantitative analysts do with that data.

<sup>5</sup>Of course, one need not know "everything" about a case before one knows anything; indeed, such a claim is incoherent. But I think that interpretivist analysis requires simultaneously taking into account many different aspects or attributes of a case.

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## What Might It Mean to Be an "Interpretivist"?<sup>1</sup>

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Attention to "interpretivism" has become a vital topic in the methodological conversations that this new APSA section both reflects and promotes. However it remains unclear just what is meant by talk of an interpretive approach. This ambiguity leaves some political scientists skeptical as to whether interpretivists study topics or employ methods that involve more than a rhetorical break with established practices in the discipline. I believe that interpretivism can and should be taken as a distinctive approach to political science, but that it is incumbent upon interpretivists to articulate more specifically just what their underlying commitments are and how these diverge from those of other approaches. As I see it, the test of any such articulation will be whether it can persuade skeptics that something distinctive really is being proposed.

In this essay, I consider several points that have been emphasized in articulations of what it might mean to be an "interpretivist." I first suggest that some common points of emphasis fail to win over skeptics because they simply do not identify a criterion that persuasively differentiates interpretivism from other approaches to political science. I then turn to interpretivism's concern with the intersubjective dimension of meaning. I believe that this is the point most capable of providing a differentiating criterion, but that efforts to articulate interpretivism on this basis also bring out divisions among interpretivists—in particular, between structuralist and intentionalist variants of interpretivism. I close by proposing that, if they wish to persuasively articulate a divergence from established practices in political science, intentionalists should supplement their claims about intersubjectivity with attention to the character of explanation.

Articulations of interpretivism commonly emphasize the anti-positivism of the approach. However, this point fails to speak to skeptics. The meaning of "positivism" is no clearer

than that of "interpretivism" (on positivism, see Dessler's contribution to this symposium) and hence the content of any such dichotomous contrast is highly ambiguous. Moreover, since few political scientists today identify themselves with positivism, an emphasis on anti-positivism does not communicate to this audience any specifics about just how interpretivists might differ from them.

Some more headway, but not much more, is made by articulations that emphasize taking meanings seriously. Such articulations do differentiate interpretivists from scholars who explicitly bypass meanings in favor of a focus on "objective" features of society, polity, and/or economy (ex. Skocpol 1979: chap. 1). But they leave unclear how interpretivism stands in relation to established political science research traditions whose practitioners would also claim a concern with meanings—conceptualized variously in terms of ideas, opinions, attitudes, values, beliefs, etc. Political scientists on the whole have never given widespread support to approaches that reject attention to meanings; and few, if any, in the discipline ever bought into the argument that a concern with meanings is incompatible with the drive to be scientific. Indeed many practitioners of two of the discipline's most self-consciously "scientific" research traditions—behavioral survey research and rational-choice scholarship—would assert that meanings are basic to their approach. A persuasive articulation of the claim that interpretivism is something tangibly different must hence go beyond merely emphasizing attention to meanings. It must specify just how an interpretive approach breaks with the manner in which established traditions conceptualize meanings, conduct research into them, and employ the findings of such research in their pursuit of further goals of inquiry.

When we shift away from the generic point of attending to meanings to more specific questions about how meanings are conceptualized, a break between interpretivism and other approaches does come into focus. Indeed, if pressed to choose a criterion differentiating contemporary interpretivists, there is perhaps no better candidate than their commitment to incorporating intersubjectivity into the way that we conceptualize meanings. From Charles Taylor's classic "Interpretation and the Sciences of Man" (1971), to Paul Rabinow and William Sullivan's overview of "The Interpretive Turn" (1979), to the more recent formulations of such figures as Alexander Wendt (1992), scholars charting the commitments of interpretivism consistently turn to the notion of intersubjectivity. They argue that meanings should not be conceptualized solely in *subjective* terms as something that exists in the minds of individuals considered in isolation from one another, but also need to be understood in *intersubjective* terms as something bound up with concrete contexts of shared social practices and interacting individuals.

Now, pausing to give our skeptics a chance to respond, I imagine that they would acknowledge talk of intersubjectivity as marking at least a rhetorical break, but would also follow up with nagging worries as to what exactly this talk adds up to. When so pressed to explicate their commitment to intersubjectivity, interpretivists seem to me to have two different responses available. On the one hand, they may flesh

out intersubjectivity in terms of “meaningful structures” (Geertz 1973: 7) or quasi-structures, conceived of as having some kind of existence independent of the subjective realm of individual minds. Alternatively, if wary of so postulating a supra-individual domain of intersubjective meanings, interpretivists may instead flesh out intersubjectivity in terms of the argument that to understand (or, for that matter, to explain) any individuals’ subjective meanings we must locate them against the network of their other beliefs and in a concrete context of social practices and interactions with others (Bevir 1999: chap. 2). Drawing on the language of John Searle (1983) and other philosophers who discuss the mind in terms of “intentional states,” we might call this second response “intentionalist interpretivism.” It is marked out from more structuralist variants of interpretivism by a refusal to grant ontological status to meanings conceived of as located anywhere other than in the intentional states of individuals. Intentionalists may sometimes pragmatically employ talk of supra-individual meanings as a short hand when descriptively summarizing a complex interactive reality, but they understand such talk in nominalist rather than realist terms.<sup>2</sup>

With regard to persuading skeptics of the distinctiveness of interpretivism, the more structuralist response has one clear advantage. The move to locate a basic dimension of meanings in an intersubjective structure or quasi-structure floating behind, beneath, or beside individuals’ intentional states establishes a very tangible differentiation from methodologically individualist behavioral survey research or rational-choice scholarship. But, I for one, retain some doubts as to whether this response does not tend toward the determinist *aporias* that have long plagued the structuralist urge (whether in its “objective” or “intersubjective” variants), and thus prefer the intentionalist response. In its methodological or procedural individualism this stance, however, remains quite akin to survey research and rational-choice. For intentionalists, the task of persuading the skeptic of interpretivism’s distinctiveness thus remains incomplete at this point. After all, was not detailed study of the intentional states of individuals located in a concrete context of interactions and social practices a leading concern of Paul Lazarsfeld and his collaborators’ pioneering mid-century community studies of public opinion?<sup>3</sup> Do intentionalist interpretivists simply favor revival of this earlier tradition in public-opinion research—with its practice of repeated interviews with many individuals from a single area—or do they propose research practices that differ tangibly from those of the Columbia sociologists? Now, while some intentionalist interpretivists might respond here with concerns about the application of formalized interview techniques across diverse individuals, I believe that a more promising ground for clarifying commitments and articulating difference will come with a shift of focus. Rather than focusing further on the way meanings are conceptualized and information about them is gathered (where I suspect that the differences here may ultimately be relatively minor), I now shift the conversation to consider instead how such information is employed in the pursuit of further goals of inquiry, most specifically that of explanation.

To even begin to consider how interpretivists pursue explanation, we must avoid the sidetracking that results when *understanding* and *explanation* are framed as alternative goals of inquiry and interpretivism identified with the former alone.<sup>4</sup> This framing would only make sense if interpretivists—whether structuralist or intentionalist in their leanings—employ the information they gather with little to no interest in explanation. Though this may hold for some scholars, it fundamentally misconstrues the agenda of many others who, far from disclaiming explanatory pursuits, instead work with their own distinctive commitment as to what explanation giving in the human science involves: they make understanding a prerequisite of explanation rather than an alternative to it. This commitment is, of course, far from new, echoing as it does Weber’s vision of interpretive sociology as “a science concerning itself with the interpretive understanding of social action and thereby with a causal explanation of its course and consequences” (1978: 4). In recent decades, efforts to articulate a distinctive interpretive form of explanation have been particularly well developed among intentionalists. A number of philosophers and political theorists have explored and elaborated the logic of an interpretive form of explanation grounded in individual intentional states such as beliefs and desires; alternatively labeling the kind of explanations that result as “intentional,” “rational,” “rationale,” or “reason” explanations.<sup>5</sup>

An intentional account of explanation giving in the human sciences offers intentionalist interpretivists a well-developed basis on which to respond to skeptics of their distinctiveness.

On this basis they can articulate an approach to explanation that diverges from the ways that most mainstream political scientists pursue this goal. From the perspective of an intentional conception of explanation, all practices of covariational analysis—from qualitative small-N comparisons to the most complex forms of regression analysis—have quite limited value, or even none at all, in the pursuit of explanation. Indeed, such practices may be seen as positively detrimental to the extent that they encourage elisions of the basic distinction between making descriptive generalizations and giving explanations. The intentional perspective rejects as *prima facie* non-explanatory any proposed explanatory claim that cannot be unpacked in the terms of intentional explanation: that is, in terms of the outcome (or aggregated outcome) of identifiable political actors acting upon intentional states they might plausibly hold. Moreover, even when the *form* of a proposed claim passes this test, as with most of the claims made by rational-choice scholars<sup>6</sup>—the intentional perspective retains critical purchase on the *content* of that claim. It asks whether adequate evidence has been given that the actors of concern do in fact hold the intentional states attributed to them.

So, in conclusion, just what might it mean to be an “interpretivist” in political science today? In this essay, I have suggested that interpretivists might be best distinguished by their commitment to incorporating attention to intersubjectivity into the way that meanings are studied. This is, however, a thin common ground. Interpretivists are no less susceptible to

disagreement on the recurring issues of methodological individualism vs. holism than are non-interpretivists. These disagreements lead into a divergence between structuralist and intentionalist ways of explicating what attention to intersubjectivity involves in the study of meanings. Structuralist interpretivism postulates intersubjective meaningful structures or quasi-structures, and construes the classic interpretive task of understanding in terms of the “thick description” (Geertz 1973) of these structures. Intentionalist interpretivism approaches meanings as intentional states of individual minds, to be understood by locating them within the broader network of an individual’s other intentional states and in a concrete context of social practices and interactions with others. Approached in this way, the task of understanding is in turn an essential prerequisite for meeting the demands of an intentional conception of explanation giving in the human sciences.

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### Endnotes

<sup>1</sup>Thanks to Mark Bevir, John Gerring, and Alison Kaufman for their helpful comments on drafts of this essay.

<sup>2</sup>A longer discussion would need to consider whether there is also a third, more dialectical way of fleshing out intersubjectivity that overcomes the dichotomizing of possibilities that I engage in here. On my reading, Wedeen’s (2002) effort to sketch out a space between Geertz and methodological individualism explores just this option. However, whether or not we choose to add a third dialectical interpretivism to the structuralist and intentionalist variants focused on here, I do think that we should avoid the limiting of possibilities (and the scope for disagreement and debate) within interpretivism implied by Wedeen’s occasional framing of methodological individualism and interpretivism as exclusive positions (ex. 2002: 718). Where, after all, would that leave Weber?

<sup>3</sup>Lazarsfeld, Berelson, and Gaudet (1948); Berelson, Lazarsfeld, McPhee (1954). Lazarsfeld’s engagements with such standard-issue “interpretive” concerns as the theory of action and critical theory should challenge any notion that systematic quantitative research is necessarily incompatible with such engagements. In particular, see the essays collected in Lazarsfeld, 1975.

<sup>4</sup>For such a dichotomy, see Hollis and Smith (1990).

<sup>5</sup>The elaboration of such a conception has been a longstanding concern of some philosophers and political theorists. See Skinner (1972), Moon (1975), Searle (1983), Farr (1987), Fay (1996), and Bevir (1999).

<sup>6</sup>For a recent presentation of rational-choice as following the form of intentional explanation, see Fearon and Wendt (2002).

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## Interpretivism: Family Resemblances and Quarrels

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All political scientists offer us their interpretations of the world. Interpretive approaches differ from many others in that they offer us interpretations of interpretations; they concentrate on meanings, beliefs, languages, discourses, and signs, as opposed to, say, laws and rules, correlations between social categories, or deductive models. Of course, this distinction between interpretive approaches and others is not an all or nothing affair: sensible interpretivists allow that the study of laws, correlations, and models can play a role in our explora-

tion of practices; and sensible institutionalists, behavioralists, and rational choice theorists allow that their typologies, correlations, and models can do explanatory work only in so far as they can be unpacked in terms of the actual beliefs and desires of actors. Nonetheless, we can distinguish a family of interpretive approaches to political science that stand out in that they focus on meanings and beliefs – a family that includes decentred theory, ethnography, poststructuralism, practical philosophy, and social constructivism, and that overlaps with other approaches such as the constructivist and ideational forms of institutionalism.

Interpretivists are far from alone, of course, in paying attention to meanings or beliefs. They are more distinct, surely, in the extent to which they imply that political science is about meanings or beliefs, as we might say, all the way down. Interpretivists account for meanings or beliefs by locating them in wider webs of meanings or beliefs in large part, I suspect, because of assumptions about, first, the constitutive nature of beliefs in relation to actions and, second, the holistic nature of beliefs or meanings. Consider the constitutive nature of beliefs in relation to actions. When other political scientists study voting behavior by means of surveys of the attitudes of voters, or models of rational action given certain beliefs and preferences, they differentiate meanings or beliefs from actions in order to seek a correlation or deductive link between the two. Interpretivists often suggest, in contrast, that such surveys and models cannot tell us why, say, raising one's hand should amount to voting, or why there would be uproar if someone forced someone else to raise their hand against their will, or why only certain people should be regarded as eligible to vote. We can explain these sorts of things, they continue, only if we appeal to the intersubjective beliefs that underpin the practice of concern to us: we need to know, for example, that voting is associated with making a free choice and so with a particular concept of the self; and we need to know what counts as an infringement of free choice and who is regarded as being capable of making such a choice. Practices and beliefs are constitutive of one another: practices could not exist if people did not have appropriate beliefs; and beliefs or meanings would not make sense in the absence of the practices to which they refer.

Consider now the holistic nature of meanings or beliefs. Many interpretivists imply that people hold beliefs for reasons of their own, so we can make sense of their beliefs only by locating them in the context of the other beliefs that provide reasons for their holding them. Hence, they argue that even if other political scientists established a correlation between, say, a positive attitude to social justice and voting Democrat, they still could not properly explain people's voting Democrat by reference to this attitude; after all, people who have a positive attitude to social justice still might vote Republican if they believe still more strongly in conservative values, or if they believe the Democrats will not implement their policies. To grasp why someone with a positive attitude to social justice votes Democrat, we have to unpack the other relevant beliefs and desires that relate that attitude to that vote. To explain an action, we cannot merely correlate it with a single

isolated attitude; rather, we must interpret it in relation to a whole set of beliefs and desires. A wide range of political scientists typically treat beliefs, meanings, ideas, norms, and the like as if, first, they could be differentiated from actions, and, second, they could be related individually to actions. Interpretivists stand out somewhat in their insistence that meanings or beliefs form holistic webs which are constitutive of actions and so practices.

Interpretivists share with one another sympathy for bottom-up forms of social inquiry as well as a focus on meanings or beliefs. Typically they believe that people in the same situation can hold very different beliefs if only because their experiences of that situation can be laden with very different prior theories. No abstract concept, such as a class or an institution, can properly explain people's beliefs, interests, or actions; rather, it can represent only an abstract stand-in for the multiple and complex beliefs and actions of the individuals we classify under it. Interpretivists often conclude, for such reasons, that practices require bottom-up studies of the actions and beliefs out of which they emerge. No doubt constructivist institutionalists are more willing than poststructuralists to bypass bottom-up studies so as to focus on the way institutions operate in a given setting. Yet even when constructivists postulate institutional unity, they usually conceive of it as an emergent property based on individual actions in the context of intersubjective norms, which, at least in principle, could be contested. Interpretivists favor bottom-up studies of the ways in which social practices are created, sustained, and transformed through the interplay and contest of the beliefs and meanings embedded in human activity.

Another theme shared by interpretivists is an emphasis on the contingency of social life. Typically they believe that because people in any given situation can interpret that situation and their interests in all sorts of ways, political scientists are pressed to allow that no practice or norm can fix the ways in which its participants will act, let alone how they will innovate in response to novel circumstances. Our practices are thus radically contingent in that they lack any fixed essence or logical path of development. This emphasis on the contingency of social life explains why interpretivists denaturalize alternative theories. Interpretivists believe that political scientists efface the contingency of social life when they attempt to ground their theories in apparently given facts about the nature of reasoning, the path-dependence of institutions, or the inexorability of social developments; and they therefore try to expose these facets of social life as contingent if other social scientists represent them as natural or inexorable.

Even as interpretivists exhibit these family resemblances, so they are engaged in family quarrels. Important questions arise here, I believe, over the composition and the recentring of practices. The problem is that interpretivists are confused about the nature of the meanings that inform actions and practices. Sometimes meanings appear to exist as quasi-structures governed by a semiotic code or random fluctuations of power. At other times meanings are understood in terms of the beliefs of individuals, with the concept of an ideology, discourse or language referring only to a cluster of intersubjective beliefs.

More often than I would like, interpretivists combine these analyses of meaning by paying-lip service to the capacity for agency while writing empirical studies that concentrate on the ways in which traditions and practices create forms of subjectivity to the almost total exclusion of the ways in which agents create traditions and practices.

Many interpretivists concentrate on the construction of individuals because they want to repudiate autonomy. However, we should distinguish here between autonomy and agency. Autonomous individuals are able, at least in principle, to have experiences, to reason, to adopt beliefs, and to act outside all contexts. Agents, in contrast, can reason and act in novel ways, albeit only against the background of contexts that influence them. Most interpretivists reject autonomy because they believe, say, that all experiences and all reasoning embody theories, so people can adopt beliefs only against the background of a prior set of theories, which, at least initially, must be made available to them by a social discourse or tradition. However – and this is significant – this rejection of autonomy does not entail a rejection of agency. On the contrary, we can accept that people always set out against the background of a social discourse or tradition and still conceive of them as agents who can act and reason in novel ways so as to modify this background. Even if a linguistic context forms the background to people's statements, and a social context forms the background to their actions and practices, the content of their statements and actions does not come directly from these contexts; it comes instead from the ways in which they replicate, use, or respond to these contexts in accord with their intentions.

The confusion among interpretivists on the nature of meanings reappears in the ways in which they suggest political scientists might recentre bottom-up studies. On the one hand, because interpretivists sometimes appear to reduce meaning and agency to a semiotic code, they can seem to use concepts such as discourse and power/knowledge as ways of recentring accounts of practices. On the other, their emphases on contingency and particularity can inspire a concern to challenge the validity, or at least naturalness, of all terms of recentring, including presumably those of discourse and power/knowledge. No doubt interpretivists, like everyone else, should use the abstract concepts that they believe best describe the world. If they find that networks are multiplying, they might invoke a "network society"; if they find that people are increasingly dealing with risk through personalized health plans, pension provisions, and the like, they might invoke an "individualizing power"; and if they find that a group of people express similar ideas, such as a belief in freedom, the market, the importance of the consumer, and the need to roll back the state, they might invoke a "discourse of the New Right". In general, if our aggregate concepts are merely descriptive, the worth we attach to them will depend on whether or not we believe the broad patterns exist. However – and this too is significant – if they are merely descriptive, they do not do explanatory work.

So, the question of recentring becomes awkward for interpretivists with respect to explanatory concepts, not de-

scriptive ones. The more we emphasize the contingency and particularity of beliefs, actions, and practices, the harder it becomes to explain them by reference to an aggregate concept or a social process. Indeed, if interpretivists deploy discourse or power to do explanatory work, these concepts are liable to imply a neglect of agency – if a discourse purports to explain a pattern of belief or speech, the implication is surely that the discourse defines, rather than just limits, the content of the beliefs or intentions people can hold – and this neglect of agency will be likely to lead to further confusions. For example, the use of discourse as an explanatory concept would appear to imply a determinism that can barely account for change: if individuals arrive at beliefs, and even construct themselves, in accord with a fixed and disembodied ideology, they appear to lack the capacity to modify that ideology, so such modifications seem to be inexplicable. Of course, interpretivists sometimes criticize structuralism for exhibiting just such determinism while implying that they themselves view such transformations in terms of an instability that is inherent within the structure – an instability that threatens the structure and puts it into contradiction with itself; but, alas, in doing so, they merely elide the question of whether we are to understand such instabilities, contradictions, and transformations as necessary qualities of a disembodied discourse or as contingent properties and products of individual subjects, their beliefs, their reasoning, and their action.

Interpretivists struggle to recenter their theories of governance in ways that have explanatory power. They might do so by drawing on the contrast between agency and autonomy. When we reject autonomy, we accept that individuals necessarily experience the world in ways that reflect the influence on them of a tradition, ideology, or discourse. Hence our explanatory concepts should indicate how social influences permeate beliefs and actions even on those occasions when the speaker or actor does not recognize such influence. To accept agency is, however, to imply that people possess the capacity to adopt beliefs and actions, even novel ones, for reasons of their own, where these beliefs and actions then can transform the social background. Interpretivists might be well advised, therefore, to think of the social context in terms of, say, traditions rather than language or discourse: after all, the concept of a tradition evokes a social structure in which individuals are born and which then acts as the background to their beliefs and actions even while they might modify, develop, and even reject much of their inheritance. At the very least, interpretivists should care that use of concepts such as language, discourse, and power/knowledge does not lead them to neglect agency and individual reasoning either in their abstract theorizing or in their analyses of political practices.

## The Positivist-Interpretivist Controversy

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“Positivism” and “interpretivism” are complex intellectual traditions in social science. Both have long histories, uncertain boundaries, and multiple tracks of development. They confront social scientists with two main questions. First, does positivism provide a viable framework for the study of human affairs? Second, is positivism consistent with (despite its differences from) interpretivism? In this brief essay, I suggest that both questions can be answered in the affirmative.

My argument is divided into two main sections. In the first, I consider and reject a well-known argument to the effect that positivism is ill-suited to the study of human affairs because of an ontological discontinuity between nature and society. In the second, I characterize positivism and interpretivism in their modern forms, relying on the distinction between the search for generalizing causal knowledge and the pursuit of particularizing interpretive accounts. In the conclusion to the essay, I note that the positivist-interpretivist divide does not concern research methods per se but rather the larger epistemic projects within which these methods are put to use.

### Is a Positivist Social Science Even Possible?

The most influential argument against the very idea of a positivist social science is ontological in character. Roughly, this argument holds that because of ontological differences between the natural and social realms, though positivism may be an appropriate analytical paradigm for the study of nature, it falls short of what is needed in the study of human affairs.

Two well-known articles in political science advance this argument. In “A State of an Art on an Art of the State,” Friedrich Kratochwil and John Gerard Ruggie assess the study of international regimes in the early-to-mid 1980s. They note that “the prevailing epistemological position in regime analysis is almost entirely positivistic in orientation. Before it does anything else, positivism posits a radical separation of subject and object” (Kratochwil and Ruggie, 1986: 764). They argue that “a positivist epistemology simply cannot accommodate itself to so intersubjective an ontology” as we find in the study of norms, rules, and principles in international relations (*ibid.*: 765). “The impact of norms within international regimes is not a passive process, which can be ascertained analogously to that of Newtonian laws governing the collision of two bodies,” Kratochwil and Ruggie write. “Hence, the common practice of treating norms as ‘variables’ . . . should be severely curtailed” (*ibid.*: 768).

Gabriel A. Almond and Stephen J. Genco sketch a similar argument in their paper “Clouds, Clocks, and the Study of Politics.” They organize their thesis around a paper written

by the philosopher Karl Popper. “Popper’s piece presents us with three ways of conceptualizing social reality—as a clock, as a cloud, and as a system of plastic controls,” Almond and Genco observe. “Political reality. . . is clearly best captured by the third conceptualization. It consists of ideas—human decisions, goals, purposes—in constant and intense interaction with other ideas, human behavior, and the physical world. The relations among these events is not simply reactive, as are the encounters of physical objects; they are not readily amenable to cause-and-effect ‘clocklike’ models or metaphors” (1977: 492).

For Almond and Genco, the anti-positivist implications of these claims are clear. “These ontological properties of political affairs are plain for all to see; they are not matters on which reasonable people can differ. . . The implication of these complexities of human and social reality is that the explanatory strategy of the hard sciences has only a limited application to the social sciences” (1977: 493). Like Kratochwil and Ruggie, Almond and Genco hope to foster a political science more sensitive to the ideational aspects of politics and less wedded to what they see as mechanistic models of explanation inspired primarily by Newtonian mechanics. In both articles, the authors justify their recommendations for a new approach to the study of politics with the argument that it provides in some way a better “fit” with the ontology of social and political life.

The main problem with this argument is that the notion of an epistemology “fitting” or “agreeing with” an ontology is vague and underdeveloped. Kratochwil and Ruggie argue that a positivist epistemology “fundamentally contradicts” the ontology of social life heavily imbued as it is with intersubjective meaning. They do not explain, however, just what “contradiction” means in these circumstances. To contradict is to assert or imply the contrary or opposite of something. What remains unexplained is how an epistemology (a theory of knowledge and its justification) can contradict an ontology (a theory of the nature of being). More problematically, the authors of both articles appear to have smuggled in certain epistemological claims of their own to make the ontological case against positivist epistemology. Kratochwil and Ruggie, for example, say that “we *know* regimes by their principled and shared understandings. . . Hence, the ontology of regimes rests upon a strong element of intersubjectivity” (1986: 764, original emphasis). In a similar fashion, Almond and Genco write of “ontological properties of political affairs [that] are plain for all to see” (1977: 493). The authors of these articles then move from these claims to make their case that positivism’s epistemology fails to do justice to the ontology of the social world. However, as these quotes make clear, it is a prior epistemology (signaled by the words “know” and “see”) that does the real work of bringing positivism into question, since it is through these epistemological lenses that social ontology is constructed the way it is in these two essays. In the end, the notion of a desired “fit” between ontology and epistemology remains elusive.

The danger in asking for a fit between epistemology and ontology is that it might be understood as a request for a sci-

ence that reinforces our commonsense concepts of the world and never challenges our intuitions about it. Surely no theory of matter and motion provided a more natural fit between the concepts people used to understand the natural world and the kinds of behavior they saw there than the Aristotelian paradigm of earth, air, fire, and water. Newton's theory, which postulated a gravitational force between distant objects for which no physical mechanism could be identified, was as vulnerable as any theory in science has been to the charge that its epistemology (specifically, the set of concepts it advanced for understanding gravitation) was badly out of line with known ontology (the reality familiar to all that there was no "action at a distance," that only through contact could one object exert a force on another). Science might be most valuable where it most directly causes us to rethink the obvious. For this reason alone, the requirement that a science's epistemology must somehow line up with its ontology should be pursued with great care.

### Positivism and Interpretivism in Their Modern Forms

As noted at the beginning of this essay, "positivism" and "interpretivism" are complex philosophies with long and intricate histories. Neither can be summarized or defined in a straightforward way. Most accounts of positivism or interpretivism list three or four features that, in the eyes of the author, seem most important in shaping the character of the philosophy. For example, in an important book, Georg Henrik von Wright lists as "the tenets of positivism" (1) methodological monism ("the idea of the unity of scientific method amidst the diversity of subject matter of scientific investigation"), (2) "the view that the exact natural sciences. . . set a methodological ideal," and (3) the claim that scientific explanation consists "in the subsumption of individual cases under hypothetically assumed general laws of nature" (1971: 4). In another influential work, Leszek Kolakowski identifies as logical empiricism's "permanent features" (i) rationalism ("the idea that only those statements about the world whose content can be controlled by means accessible to all are entitled to the name of knowledge," (ii) nominalism (in its theories of knowledge and meaning), (iii) an anti-metaphysical attitude, and (iv) scientism (an assertion of "the essential unity of the scientific method") (1972: 207). Such definitions of positivism or empiricism can be multiplied, and what is most striking about the resulting list is that no two accounts are identical.

The same holds true for the various accounts of interpretivism. Claims that define interpretivism include: (a) Human behavior is to be understood (made sense of) rather than explained (attributed to causes). (b) Social scientists, unlike natural scientists, study a realm already constituted by meanings. (c) Social action is to be analyzed not as the effect of causes but as the conformity of behavior to rules. (d) The social world is constituted by intersubjective meanings, not brute facts. (e) The vocabulary of causal analysis is inconsistent with the vocabulary needed for a proper interpretive understanding of social life. (f) Prediction is not a sensible or reasonable goal of social inquiry. (g) In the study of society,

theory is used not to predict but to uncover or clarify the import of symbolic acts. And so on.<sup>1</sup>

The lesson here is that there is no non-arbitrary set of beliefs constitutive of either positivism or interpretivism. Put another way: There are many positivisms and many interpretivisms in social science; no particular account of either is canonical. Rather than add to the existing list of definitions or attempt to adjudicate among them, I will take the approach here of discerning two broad patterns of research practice and connecting them to positivism and interpretivism respectively. In other words, rather than defining positivism and interpretivism in the abstract and then searching for connections to social research, I will start with two generic patterns of empirical research and link them to positivism and interpretivism.

The first pattern is the *search for causal generalizations*. In a statistical setting, this takes the form of the search for causal effects (King, Keohane, and Verba, 1994). More broadly it takes the form of *modeling*. As Charles Lave and James March put it in a classic work,

A model is a simplified representation of the real world. Models are created by speculating about processes that could have produced the observed facts. Models are evaluated in terms of their ability to predict correctly new facts. . . . [A] simple model has only enough detail to make it applicable to other situations (1993: 19).

The search for causal generalizations is positivist social science in its modern form. Whether pursued through statistics or through models, the strategy is to isolate causes and gain an understanding of their individual workings. The analyst abstracts from the concrete complexities of social life to focus on the fundamental causal processes underlying them. The abstraction required to isolate specific causal processes means that verisimilitude is not the measure of a model's worth. "[M]odels that aim to explore fundamental processes should be judged by their fruitfulness, not by their accuracy," writes Robert Axelrod. "For this purpose, realistic representation of many details is unnecessary and even counterproductive." Modelers must keep things simple in order to cover the broadest range of cases possible. "The intention is to explore fundamental social processes. Although a particular application may have motivated a given model, the primary aim is to undertake the exploration in a manner so general that many possible settings could be illuminated" (1997: 6).

The second pattern of empirical research is the *reconstruction of detailed historical episodes*. Here the technique is not abstraction but concretization; the researcher's aim is not the isolation of fundamental causal processes but an account of the arrangement and working of the various causes that converged in a specific episode of interest; the purpose of analysis is not to generalize over similar cases but to gain detailed knowledge of the historical case in its uniqueness. Particularization leads to theory (generalizable knowledge) not in the positivist sense of knowledge that subsumes similar cases but in the quite different sense of an account that ties together details *within* the episode at hand (Wohlforth, 1998: 678). As

Clifford Geertz puts it in one of the single most quoted lines of interpretive methodology, “the essential task of theory building here is not to codify abstract regularities but to make thick description possible, not to generalize across cases but to generalize within them” (1973: 26). Though Geertz uses the term “description,” what he really means here is thick *generalization*. “The aim [of theorizing within cases] is to draw large conclusions from small, but very densely textured facts,” he writes (p. 28).

Note that the difference between positivism and interpretivism in this account is *not* the difference between a focus on causes and an emphasis on meanings. (See also Robert Adcock’s contribution to this symposium on this point.) No instance of social research can make sense without relying on both cause and meaning: each presupposes the other. Weber’s famous definition of sociology suggests just this dependence: “Sociology. . . is a science which attempts the interpretive understanding of social action *in order thereby* to arrive at a causal explanation of its course and effects” (1947: 88, emphasis added). Because we cannot know causes in social life without understanding the meanings constitutive of that life, any attempt to divide various instances of social research along the fault line of cause and meaning is doomed to fail. Inevitably such attempts can only lead back to arguments for the ultimate compatibility of cause and meaning in social analysis and the need to synthesize them (Moon, 1975; Apel, 1984).

Is there, then, any inconsistency between positivism and interpretivism? The brief answer is: not necessarily. A researcher using abstract models to capture fundamental causal processes in the world need have no complaint against the scholar working to reconstruct in painstaking empirical detail an important historical episode. She need have no complaint because nothing she does—no assumption she makes, no goal she sets in the process of determining causal effects or building causal models—is undermined by the activity of the researcher focused on detailed historical reconstruction. The same holds for the latter researcher vis-à-vis the first scholar’s project. Neither need be bothered by what the other does. Here we have *difference* in research aims but no necessary *disagreement* between the researchers.

However, should the first scholar *value* generalizing research over the particularizing alternative, while the second scholar sees more merit in historical particularization than causal generalization, we would have not just difference but a genuine disagreement. Here we confront one of the most important and under-discussed questions in social research: How should we decide which research questions to pursue? Are some ways of framing research questions better (more valuable, more useful) than others? Surely most scholars would agree that “a research project should pose a question that is ‘important’ in the real world” (King, Keohane, and Verba, 1994: 15). But this leaves open the issue of *how* such questions are to be framed. Do we phrase research questions so that they prompt us to search for causal generalizations? Should we instead be more interested in historical particular-

izations? Is one form of knowledge more significant than the other? If so, on what grounds? If not, how do we choose?

These questions open up a vast terrain that remains mostly uncharted in modern political science. Yet as a community we are acutely aware of their importance. We have all heard the call for research that is “question-driven” rather than “method-driven.” This is clearly a plea for greater *significance* in our scholarship. Too much research is seen by too many scholars in our discipline as uninteresting, possibly even irrelevant. Obviously, the complex tangle of issues raised by the call for relevance cannot be tackled in the present paper. But the analysis above of the divide between positivist and interpretivist research is instructive. We saw there that if positivists and interpretivists disagree, it is because they value differently the various answers one might seek in social research, not because they disagree over epistemology or ontology or other deep philosophical matters within science. This suggests that the answer to the question of the social relevance of science—what makes a research question worth pursuing, not simply in terms of intrinsic scientific merit, but as a contribution to society?—is not to be found *within* science. Science, after all, cannot provide its own social justification. The validation of science must ultimately be sought from the outside, from a judgment we make about the role and potential efficacy of social science in society.

## Conclusion

These observations leave us finally with the question of method and methodology. It is useful to distinguish between these terms (Harding, 1987: 2-3). A “method” is a technique or procedure for gathering and analyzing data. Methods include random sampling, content analysis, statistical hypothesis testing, survey research, regression analysis, structured interviews, archival research, and so on. “Methodology” is the study of method. It is epistemology articulated at a level very close to the practice of empirical research.<sup>2</sup> Methodology tells us why the methods we use in science work the way they do.

Are there methods distinctive of positivist and interpretivist research strategies? It is hard to identify any, since methods (e.g., survey research, content analysis, interview methods) can be used to answer a multiplicity of research questions. In general, projects take on their distinctive philosophical shape—positivist, interpretivist, feminist, constructivist, postmodern, and so on—at a level of discourse higher than that at which methods are developed, debated, and deployed. A method is simply a technique for gathering and analyzing data; as such, it is largely neutral in the paradigm wars that rage above it. Foucault, for example, visited the archives and assembled and used archival data in much the same way as the social theorists he criticized in his many works on the human sciences.

In an important essay on feminist method, Sandra Harding comes to much the same conclusion. She argues “against the idea of a distinctive method of feminist research,” suggesting that “it is *not* by looking at research methods that one will be

able to identify the distinctive features of the best of feminist research” (1987: 1, 3, original emphasis). Similarly, that there are no distinctive positivist methods, or interpretivist methods, or constructivist methods, and so on, though there are quite clearly feminist research *projects*, positivist *projects*, and the like. One methodological distinction that seems worth preserving is that between quantitative and qualitative methods. This division concerns the type of data or level of measurement upon which the method relies (Collier, Seawright, and Brady, 2003). We distinguish between qualitative and quantitative methods because we find it useful to distinguish among the different types of data we collect and analyze. But we do not distinguish between positivist method and (say) feminist method because the difference between positivist projects and feminist ones cannot be grasped through contrasts in method.

In closing, it is worth noting that the distinction between method and methodology helps explain the logic behind the creation of the new Qualitative Methods section of the American Political Science Association (APSA-QM). Some observers have wondered how a new methods section could be justified when the APSA already had a section called “Political Methodology.” The answer lies at least in part in the definitions offered above. The new section has been created around a distinctive set of *methods* dealing centrally with qualitative data. Its focus is not the logic of inference writ large, which is the domain of *methodology*. It is instead the use of procedures and techniques of qualitative empirical analysis that are distinctive and numerous enough to warrant study outside the sphere of large-N quantitative research. Members of APSA-QM will of course take up methodological issues. They cannot talk sensibly and systematically about qualitative methods without doing so. But the section is not organized around a qualitative *methodology*. Political methodology covers the domain of qualitative research as much as it does that of quantitative analysis.<sup>3</sup>

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## Endnotes

<sup>1</sup>Some of the more important works on interpretivism are Winch (1958), Taylor (1971), Skinner (1972), and Rabinow and Sullivan (1979).

<sup>2</sup>Definitions in the Oxford English Dictionary suggest the close relationship of epistemology and methodology. The OED defines methodology as “[t]he science of method. . . Also, the study of the direction and implications of empirical research, or of the suitability of the techniques employed in it.” Epistemology is defined as “[t]he theory or science of the method or grounds of knowledge.”

<sup>3</sup>The problem here may lie simply in the incompleteness of the section names. If the first section were named “Political Methodology and Quantitative Methods” and the second called “Political Methodology and Qualitative Methods,” the logic of their fit might be clearer.

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## Interview with Clifford Geertz

*Clifford Geertz is Professor Emeritus of Social Science, Institute for Advanced Study, Princeton New Jersey. John Gerring was a Member of the School of Social Science at the Institute, 2002-03. This interview was conducted on July 25, 2003, at the Institute. Transcription by Jennifer Jefferis; editing by John Gerring.*

*Gerring:* It is perhaps fair to say that the quantitative side of political science has taken most of its cues from economics, while the qualitative side of the discipline has taken its cues from anthropology and history. For the latter group, there is

no tradition more esteemed than interpretivism, and there is no one more esteemed within the interpretivist tradition than you. Your work is assigned routinely in courses and referenced continually in the political science literature. So it is of enormous interest to the discipline, what you think about all this. The question we're struggling with is how to understand interpretivism in the context of political science and the social sciences more generally. That's the question I will be circling around one way or another as we go through this interview.

I'll say one other thing by way of preface. As the discipline of political science has become more self-consciously scientific, more quantitative, and so forth, I think there is a sense on the part of people, even people who do quantitative work, that something has been lost in the process. But I don't think we're very clear on what exactly it is. Let's start on this question, then. You've lived through a lot of the history. It seems to me that there was a time back in the fifties and sixties when the different social sciences really were talking to each other quite frequently. The SSRC served as a focus for much of this interdisciplinary discussion, which I believe you were a part of . . .

*Geertz:* I think the inter-disciplinarity of the 1940s, 1950s, and 1960s owed a great deal to the war-time experience. During the war (I was just an ordinary seaman in the Navy), most of the faculty of the social sciences – the leading people, almost to a man and woman — were involved in one capacity or another. And so for the first time they were thrown together. This was particularly dramatic for anthropologists, because they were thrown in with political scientists and economists and engineers and everyone else. You would get someone working on propaganda, or some other thing involving the war effort, and it would be an anthropologist, a sociologist, a psychoanalyst, you know. And for anthropologists, particularly, that was a radical change, because before that it was a very self-enclosed discipline concerning tribes and pre-history and so forth. All of a sudden, it was this general ferment. The war was short, three years or so for most people, so they came back and this experience changed their whole notion of what social science ought to be about. It was a very lively period, and I went to Harvard just as everyone came back to the academy. Interdisciplinarity was blooming forth all over. This was also the time when the big foundations – Ford, Rockefeller, Carnegie, and so forth – were beginning to support social science on a major scale. SSRC was part of that but was not the only center of it. So this led to a tremendous concern with team research and the task of relating quantitative and qualitative work.

The Social Relations department at Harvard is a good example of this. Sam Stouffer was there, along with Talcott Parsons, Harold Lasswell, Clyde Kluckhohn, Jakobson, and many others. Norbert Weiner was down the street at MIT. I don't say there was no abrasiveness, but they were all in the same business. People were interacting, and it transformed the social sciences. At that point, everything was mixed together. Not that individuals didn't go off every once in a while, but they could also get along. It was a period of great opti-

mism. Social science was about to begin. And a great deal of it was interdisciplinary research. So it was a great period. And that's where I was formed. I came in through philosophy and the humanities. There was plenty of room for that sort of thing at the time.

Later on, when I moved to Chicago, the interdisciplinary, inter-method, conversation continued. David Easton was there, David Apter, Leo Strauss, Hans Morgenthau, the Rudolphs. They were all prima donnas. Everybody was doing his or her stuff. In fact, it was not so bad. They got along in a way because they did not have to deal with one another very often. The department was entirely run by a woman who was secretary of the department. She gave out the grants, and she just made sure that all the prima donnas got their share of the loot. And the prima donnas just taunted and held forth, and they were big figures.

*Gerring:* It was a heroic time. I'm very fascinated by that moment in time. It just seems so . . . I don't know, so rich, so fertile in many ways compared with the segmentation of disciplines and sub-disciplines today. I'm curious whether you think that that moment of ferment was made possible by an underlying theoretical consensus, e.g., the theory of modernization, or something else.

*Geertz:* Parsons of course always wanted to have a general theory, but I don't really know how much it animated the others. I think it had more to do with the war-time experience of working together. Academic life before the war, long before your time, was a very protected environment. Scholars did their work without being really in the world. The war brought them into the world and they never left it after that. You could no longer just sit in an office and know all there was to know about the Navajo.

*Gerring:* Subsequently, in the 1960s, it does seem to me that part of what happened was a de-legitimation of social science and a suspicion of the heretofore rather tight connection between government and the academy.

*Geertz:* Some of that is true, and the Cold War and Vietnam really did that. But it didn't lead back to the old system. People didn't retreat back to the ivory tower. Instead, we are faced with the question of scientism, which is another issue altogether, about which I don't quite know what to say. People had always thought of themselves as scientists, but they had a rather capacious notion of science. Now the difference between people is not whether they think they're scientists or not, but what they think science is. To some people it's only statistical tests, and that's it. For other people, anything is science. Just go out and talk to people and come back and say what they said; that's science. So the whole conception of what science is became problematic. Then you get to the 1970s and you get post-modernism and all that. By then the American anthropologists have 86 sections, psychology has 105 — it's total fragmentation, except that everybody doesn't stay in the fragments. I mean there's almost no anthropologist that is

only an anthropologist. People like myself, Mary Douglas, we do all sorts of things. Anybody in every field is sort of all-purpose. It's very rare that you find someone that's just in one of those little chapters.

*Gerring:* I think that's true. However, by reference to the past, the career paths that you find to day are relatively narrow. Let's talk a little about interpretivism. This label, which now is very common, is actually fairly new, isn't it?

*Geertz:* Well, I myself never talk about interpretivism, but that's all right, it doesn't matter. It's hermeneutics that we're really talking about. That's what it comes out of. I think the clearest statement is Charles Taylor's, in a famous essay in the *Review of Metaphysics* ["Interpretation and the Sciences of Man," *Review of Metaphysics*, 25 (1971), 3-51] in which he talks about the tradition of biblical interpretation. As Chuck says in this piece, the hermeneutic approach starts with the notion of something as unclear and tries to bring it into clarity. When I work in the field on anything, whether it's something sort of airy-fairy like religion or something more concrete like a market, I start with the notion that I don't understand it. Then, I try to understand it better by tacking back and forth between large and little things. And that's what you really do when you "interpret." It is a little like when you learn a language that you don't know. During the first few days you get a few sentences and that's about it. As you talk, get corrected, and correct others, you really begin to get into it, until eventually you master the language. And the same thing is true when you're trying to understand foreign enterprises.

I didn't understand why people were so fascinated with cockfights in Bali, because they're really rather boring to a Westerner. It's just two chickens pecking at each other. And you have eight fights in an afternoon everyday. It's one of those situations where if you've seen one you've seen them all. The cocks just go at it, eventually one is dead, and there's blood all over the place. It's exciting for the first two or three times maybe. But why are these people so absorbed in it? So the question that somebody like me asks, whether I'm an interpretivist or whatever the heck I am, is *What is going on here?* Something is going on here that I don't see, that I can't understand. What sort of story can I tell about this apparently meaningless activity? Well, perhaps this has to do with masculine competition, and so forth. That becomes the model. It's the same process whether one is working with a marketplace, a ritual, or family life. The notion is that you start with something about which you have a slight grasp, like you do with a foreign language, but you really don't understand it.

I studied a number of languages and the two that I think about all the time are Javanese and Arabic. They're really quite different experiences. To make it simple-minded, if you're learning Indonesian, it gives the impression of being easy. In a couple of lessons you can go out in the street and begin to talk. But it's a very subtle language and a lot of people who think they speak it well do not. They don't understand what is really going on. Arabic is the opposite. It's so morphologically complex that although there are almost no irregu-

larities you can't really say anything for the first year because you always get it wrong and it doesn't make any sense. However, anyone who has the patience to learn to speak Arabic correctly speaks it pretty well by the time they actually get to that point. Whereas a lot of people in Indonesian never get beyond the primary stage because they don't realize they're not very good at it. The point is, these are two different experiences. However, in both situations you're starting with something you don't understand and slowly trying to learn to speak. That's the model I think of as emblematic of what you call interpretivism.

*Gerring:* So, interpreting an action or a set of events is like learning a language in a sense. Of course there is always the question of whether you know how to speak it or not and that's . . .

*Geertz:* The way you learn how to speak it or not is also whether you can *communicate*, whether when you speak, people understand. It takes a long time to be able to tell a joke in Arabic, and if you get a laugh, well . . . And the same things true about working with cockfights. If you really can act so that you get intelligible responses from your informants; if you say things about the cockfight or the market or the ritual that they regard as intelligent, then you are obviously beginning to get a hold of it.

*Gerring:* I think telling a successful joke would be a good example of a fusion of horizons, in Gadamer's sense. So here's a question related to that. I think it's true that when we think about interpretivism we often think of rendering the exotic familiar, or making sense of something that is ostensibly nonsensical. How does interpretivism deal with situations that seem commonsensical? I'm thinking about the question of what a political anthropology of American politics might look like.

*Geertz:* I was just reading a book by Sherry Ortner, an anthropologist, who worked on the graduating class of 1958 [*New Jersey Dreaming: Capital, Culture, and the Class of '58*, Duke University Press, 2003]. I myself have never worked in the United States. There's no question that it's harder. You take so much for granted when you study your own kind. You have to *de-familiarize* yourself. You have to get the distance. You have to realize that you *don't* understand. It's obviously hard to put yourself in the frame of "I don't get this." But otherwise there's no real difference. It's just that, somehow, you have to artificially make it strange. For political scientists, I think they just have to learn how to see, not the mystification of reality, but mystery in the sense that there's a lot more there than ordinary concepts might suggest. I mean you can just read the papers and realize that.

*Gerring:* What do you find left out of standard social scientific accounts of contemporary politics that a more anthropological effort might illuminate?

*Geertz:* One example of this Putnam's book on Italy, where he talks about civic traditions [*Making Democracy Work: Civic Traditions in Modern Italy*, Princeton University Press, 1993]. He is after something that most political scientists don't even think about. I'm afraid that "political anthropology" doesn't produce an image in my mind of anything in particular.

*Gerring:* Yet, the appeal of Putnam to political science, and to sociology and economics, is the general theory of social capital. It's not what he has to say about Emilia Romano. I raise that only because it raises a question that we've talked about before, you and I, about whether or to what extent an ethnographic/anthropological/interpretive approach generalizes, wants to generalize, can generalize — or whether it's more of a differentiating art.

*Geertz:* I don't think that's the opposition I would construct. It isn't a matter of whether you're just going to do particular studies or general studies. It doesn't work that way, at least for me. The enterprise of social science is inherently comparative. What you learn about one case you then try to look at another to illuminate both the differences and similarities. Let's say you're interested in bazaar-like markets, which I first studied in Morocco. Now, there's a whole range of studies of markets around the world, including Ted Bestor's recent work on fish markets in Japan [*Tokyo's Marketplace: Culture and Trade in the Tsukiji Wholesale Market*, University of California Press, forthcoming]. So there's a beginning of a critical mass of studies of a general topic in particular settings — a big fish market in Tokyo, a clothing market in a small town in Morocco, and so forth. These are not going to be exactly the same, but they are very similar and they have to do with communication and the symmetry of markets. (I could go on for hours, there's a whole theory on this.) As these cases are written, one begins to see similarities and differences among markets and it deepens one's ability to grasp the one situation one started out looking at. I think Ted would say he'd learned something from reading my stuff on Morocco, and I have certainly learned something new from him. You don't start out with some general abstract theory of markets (or stratification) that holds everywhere. What you get is a richer picture of the variations and the similarities, the continuities and the differences, and some principles of markets and stratification systems that, even if they don't apply everywhere, apply often enough to be of some use. You don't have to explain everything to explain anything.

*Gerring:* What would be a good example of a generalization or a principle of that nature?

*Geertz:* Well, again we come back to the market example. I tried to argue that there's a difference between a market search across a firm economy and in a bazaar. In the latter, the action is not between buyers or between sellers. It's between a buyer and a seller who bargain with each other. In a firm economy, that's not the case. You have advertising, you have set prices and the competition comes between the producers. So, the

difference is that the information problem is different. The search problem is different. You have an intensive search rather than an extensive search. The used car market is like a bazaar. You have to know a lot about the car and the guy that sold it to you, because you have to pursue that particular case. If you're buying a new refrigerator, a washing machine, or a box of cereal in a firm economy you simply compare prices, look it up in consumer digest and see which one's the best, and you have advertising and the prices are the same. It's not negotiated. To go and buy breakfast in Morocco means a trip to the market and negotiating with the negotiating party.

*Gerring:* So there is a general phenomenon which is instantiated in different ways across cultures. Let's move onto a question that is often raised in the context of interpretivism, the question of causal explanation. You know it's sometimes said that interpretivism is about describing things, or offering interpretations, which is a certain kind of explanation, but not causal explanation.

*Geertz:* This presupposes certain philosophical conclusions about what causation means. That aside, if you get interpretation right, I believe the causes will fall out. If you understand the cockfight, you'll understand why people are engaging in it, why things are happening the way they are happening. It must be clear, of course, whether you're talking about a cause or a causal law. There's a big difference. Everything is caused. On the molar level there is no uncaused behavior. If I look at the cockfight and something happens, I don't say "There's no cause for this, it just happened." I don't write that way, and no one really writes that way. So that isn't what we're doing. But the question about causal laws gets more complicated. There's one issue concerning the difference between causality and determinism. If you are familiar with Elizabeth Anscombe's work, you will understand that the search for causes is close to detective work. You come in and you find the pitcher has fallen to the floor and there's glass. Did the cat push it, or did the wind blow it over? The one thing you know is that there is a cause. It's on the floor and there's milk all over. But whether the cat did it or the wind did it, or you put it down in a way that made it tumble later on — there are evidently lots of possibilities. The point is, you need to have the story of what happened. But you don't have to have a causal law. There's no causal law that cats tip over milk.

To be sure, you can correlate behavior. But this doesn't usually get you very far. An interpretivist tends not to ask that sort of question first. One is trying to get a story, a meaning frame to provide an understanding of what is going on. You want to understand what it is that's motivating people, or cats, to do these (unaccountable) things. So we look for a motive and feelings and emotions and ideas and concepts and all that jazz, which you don't need to do if you take 70,000 people and see how their movements correlate with each other. I mean you could do that for traffic flows. I'm not denying that this can be done, or that it is not useful to do it, if you really want to figure out the traffic flows in New York. I don't think you should spend a lot of time asking each individual driver what

they were doing. It might help to give some understanding but, in general, I would agree the way to do it is to pick a place and measure the number of cars that go by and correlate it with the time of day and find out how the traffic flow works. So it's not an opposition of that sort – correlation versus causation. It's just that a sheer correlation between two people's behavior is not usually very interesting.

*Gerring:* Let me move on to a much bigger question. Can't we all live together? I mean, those who do interpretive work, and those who do a different kind of work. I'm not sure how this works in anthropology, but in our field people from different methodological fields are oftentimes at each other's throats.

*Geertz:* Yeah, that's what this school (the School of Social Science) is dedicated to overcome. I think there has grown a kind of militancy in the social sciences. You see it in the evolutionary psychologists, you see it in the evolution theorists, you see it in economics. Here, people adopt a position and try to take things over. Once you get any group of people who eliminate everything else then it polarizes the situation, the opposition organizes, and you get civil wars. I would say you need a historical interpretivist approach to understand why it is there has been this methodological movement and where it comes from. My own sense is that it comes from a kind of utopian vision of science — that somehow science is about to begin and they're going to finally set it all straight. Obviously, I'm not persuaded by this, but I don't try to stop them from doing it. They try to stop everybody else — well, at any rate, some of them do. (A lot of them are really quite tolerant.) As a friend of mine says, the problem with the rational choice people is not what they do but that they practice it inside departments. I mean they just choose their own people and they make rational choices in terms of aggrandizing their own power, and I think that's bad for academia, regardless of the field. I have some questions about rational choice theory (game theory), but I acknowledge its place in the academy. We have lots of people who do that sort of work here. But to say, "If you're not doing game theory you're not doing science," well that tends to make people unhappy. I don't think it's necessarily that much worse than in the past, but there has been a kind of militancy in the last ten years or so that I don't fully understand the reason for. The level of tolerance has declined a bit. I'm talking about scientism, science as an ideology. It's still not clear what happens to turn good scientists into scientistics.

*Gerring:* Let me ask you one more question. This is a very open ended question and you may answer of course in any way that you wish. This newsletter is written primarily for political scientists and I'm wondering if you have any advice or thoughts on the study of politics.

*Geertz:* Well I've done a lot of study on it. I've written a book and I'm about to write another one. I think this is, again, a question of scientism. I would suggest that at least some

people in political science ought to get away from toy problems and start addressing real ones. For example, can there be a functioning multiethnic state such as India or Nigeria? What do we know about this from 50 years of experience with extremist states? Certainly, not as much as we ought to. Political scientists should engage problems that are there. I don't mean practical problems in the sense that one should take up social work. But they ought to take problems as they come from the world. Take Adam Ashforth's recent work as an example [Ashforth, a visiting member at the School of Social Science, is at work on a book on witchcraft in southern Africa]. He goes to South Africa and finds that people have been driven to witchcraft everywhere — including Mandela, Tutu, and the politics of AIDS — and he attacks that problem. I would suggest at least some of your people begin to do that more than they have. This means you have to be able to accept a lot more ambiguity, a lot more uncertainty. You never know quite what you'll find and when you'll be entirely wrong about what you thought was true. It is hard. It's more complicated. There are no ready devices off the shelf that you can use. You have to make them up for yourself. You have to try to interpret the evidence, to understand what the hell they're saying when they talk about witchcraft. But that's the advice I would give, to engage with the political world that one confronts. That's what Putnam did [in Italy], and [Robert] Dahl did in New Haven. They tried to talk about what's going on there. It doesn't mean that none of the sharpened tools of social science won't be useful, but I am always more concerned about arbitrarily simplified accounts that make it possible for me to show the exercising of some particular methodological skill. I think that doing something because you can do it or because there's a technique for it or a program for it — this strikes me as a very bad way to spend a life.

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## Book Notes

*Note:* Jennifer Jefferis compiled this list of books from a variety of databases. Descriptions were drawn from jacket blurbs and publisher's web sites. We tried hard to find those books (published after January 2000, our arbitrary start-date) that had interesting methodological approaches or explicit commentary on methodological issues — excluding qualitative methods textbooks (covered in the last issue) and books devoted to purely statistical issues. We are quite sure that we missed many valuable contributions to the broadly defined genre of qualitative methods. Therefore, we beseech readers to send us suggestions of additional books that we can include in future book notes (self-nominations are welcome).

Abbott, Andrew. 2001. *Time Matters: On Theory and Method*. Chicago: University of Chicago Press. \$25.00

This text focuses particularly on questions of time, events and causality. The author grounds each essay in straightforward examinations of social scientific analysis.

Anderson, Lisa. 2003. *Pursuing Truth, Exercising Power: Social Science and Public Policy in the Twenty-First Century*. New York: Columbia University Press. \$27.50

As dean of Columbia University's School of International and Public Affairs, Lisa Anderson has a unique vantage point on the intersection of the social sciences, particularly political science, and the formation and implementation of public policy. How do, or should, the research and findings of the academy affect foreign or domestic policy today? Why are politicians often quick to dismiss professors as irrelevant, their undertakings purely "academic", while scholars often shrink from engagement as agents of social or political change? There is a tension at work here, and it reveals a deeper compromise that arose as the modern social sciences were born in the nursery of late nineteenth century American liberalism: social scientists would dedicate themselves to the pursuit of objective, empirically verifiable truth, while relinquishing the exercise of power to governments and their agents. Anderson argues that this compromise helped underwrite the expansion of American influence in the twentieth century, and that it needs serious reexamination at the dawn of the twenty-first.

Brady, Henry E. and David Collier (eds). 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham: Rowman & Littlefield.

Qualitative and quantitative research have complementary strengths and weaknesses. Social science must draw on both traditions to make progress. Brady, Collier, and collaborators show how the two traditions can be brought together, with shared standards for using diverse research tools. The book takes as a point of departure the approach of "mainstream quantitative methods," which is exemplified by King, Keohane, and Verba's *Designing Social Inquiry (DSI)*. *DSI* makes a major contribution to social science methodology, yet in seeking to impose a conventional quantitative template on qualitative research, the authors ignore major strengths of qualitative tools. They likewise overlook key weaknesses of conventional quantitative methods—weaknesses that are quite evident from the broader perspective of statistical theory. Brady and Collier rethink these methodological issues, proposing an alternative framework that holds more promise for both qualitative and quantitative investigation. They argue that qualitative analysis must be defended in its own terms, and that scholars can also develop a rationale for qualitative research based on statistical theory.

Ekiert, Grzegorz and Stephen E. Hanson. 2003. *Capitalism and Democracy in Central and Eastern Europe*. Cambridge: Cambridge University Press. \$85.00

This volume presents a shared effort to apply a general historical-institutionalist approach to the problem of assessing institutional change in the wake of communism's collapse in Europe. It brings together a number of leading senior and junior scholars with outstanding reputations as specialists in post-communism and comparative politics to address central theoretical and empirical issues involved in the study of post-communism. The authors address such questions as how historical 'legacies' of the communist regime are defined, how their impact can be measured in methodologically rigorous ways, and how the effects of temporal and spatial context can be taken into account in empirical research on the region. Taken as a whole, the volume makes an important contribution to the growing literature by utilizing the comparative historical method to study key problems of world politics.

Elman, Colin and Miriam Elman. 2003. *Progress in International Relations Theory: Appraising the Field*. Cambridge: MIT Press. \$24.95

All academic disciplines periodically appraise their effectiveness, evaluating the progress of previous scholarship and judging which approaches are useful and which are not. Although no field could survive if it did nothing but appraise its progress, occasional appraisals are important and if done well can help advance the field. This book investigates how international relations theorists can better equip themselves to determine the state of scholarly work in their field. It takes as its starting point Imre Lakatos's influential theory of scientific change, and in particular his methodology of scientific research programs (MSRP). It uses MSRP to organize its analysis of major research programs over the last several decades and uses MSRP's criteria for theoretical progress to evaluate these programs. The contributors appraise the progress of institutional theory, varieties of realist and liberal theory, operational code analysis, and other research programs in international relations. Their analyses reveal the strengths and limits of Lakatosian criteria and the need for metatheoretical metrics for evaluating scientific progress

Flyvbjerg, Bent. 2001. *Making Social Science Matter: Why Social Inquiry Fails and How it Can Succeed Again*. Cambridge: Cambridge University Press. \$20.00

This book presents an exciting new approach to social science, including theoretical arguments, methodological guidelines, and examples of practical application. Why has social science failed in attempts to emulate natural science and produce normal theory? Bent Flyvbjerg argues that the strength of social science is in its rich, reflexive analysis of values and power, essential to the social and economic development of any society. Richly informed, powerfully argued, and clearly written, this book provides essential reading for all those in the social and behavioral sciences.

Geddes, Barbara. 2003. *Paradigms and Sand Castles: Theory Building and Research Design in Comparative Politics*. Ann Arbor: University of Michigan Press. \$20.00

This book shows the relationship between thoughtful research design and the collection of persuasive evidence to support theory building. It teaches the craft of research through interesting and carefully worked out examples in the field of comparative development.

The book describes in a lively and provocative manner the methodological pitfalls most characteristic of the study of developing countries, and it shows through the use of examples how to avoid them. Some believe that the aspiration displayed in that name *social science* is not only unrealistic but undesirable. For those who find "science as a vocation" a compelling goal, however, the advice in this book aims to prevent casual, uninformed, or unintended ventures off the long path leading to that goal. It aims not to provide a set of mechanical rules of research design, but rather to foment thoughtful and innovative ways of using the inadequate and fuzzy evidence actually available to build sturdy theories.

George, Alexander and Andrew Bennett. 2004. *Case Studies and Theory Development*. Cambridge: MIT Press. \$18.95

The book provides both practical "how to" advice on carrying out case study research and an analysis of the strengths and limits of case studies relative to other methods. Topics include case study research design, process tracing, typological theory, comparative methods and structured focused comparison, the congruence method,

the development of policy relevant theories, and the philosophy of science basis of case study research.

Gerring, John. 2001. *Social Science Methodology: A Critical Framework*. Cambridge: Cambridge University Press. \$22.00

This book is an introduction to methodological issues in the social sciences that is appropriate for advanced undergraduates, graduate students, and general readers with some background in social science subjects. It is a concise and readable guide to doing and evaluating work in anthropology, economics, history, political science, psychology, and sociology.

Goertz, Gary. 2003. *International Norms and Decision Making: A Punctuated Equilibrium Model*. New York: Rowman and Littlefield. \$29.95

This book presents a punctuated equilibrium framework for understanding the nature of policy decision-making by governments as well as a theory on the creation, functioning, and evolution of international norms and institutions.

Goertz, Gary and Harvey Starr (eds). 2002. *Necessary Conditions: Theory, Methodology and Applications*. New York: Rowman and Littlefield. \$75.00

This anthology represents a first: the first book ever devoted to the implications of necessary conditions for social science research, logic, methodology, research design, and theory. Rarely is the contrast between the prevalence of a concept in scholarship and its absence in methodology texts so wide. This book presents literally hundreds of necessary condition hypotheses representing all areas of political science and all methodologies, and authored by many of the most influential political scientists of the last 50 years. Thus, this volume brings together under one cover essential work that deals not only with the analysis of common methodological, logical, and research design errors, but also the proper means—qualitative and quantitative—to analyze the many ramifications of necessary condition hypotheses and theories.

Grofman, Bernard (ed). 2001. *Political Science as Puzzle Solving*. Ann Arbor: University of Michigan Press. \$20.95

Why would a political leader gamble on a vote of confidence that he did not need to call and the failure of which would seriously harm his party's future prospects? Why would a trade union conduct a strike it knows it cannot win? How could the Soviet Empire have dissolved so quickly? These are some of the questions considered here. Contrary to standard methodological treatises, which tend to treat puzzle solving as distant from what social science is about, the contributors to this volume strongly demonstrate that an emphasis on specific puzzles will influence the research choices of future political scientists.

Harvey, Frank P. and Michael Brecher (eds). 2002. *Evaluating Methodology in International Studies*. Ann Arbor: University of Michigan Press. \$24.95

This book offers a unique collection of original essays by world-renowned political scientists. The essays address the state of the discipline in regard to the methodology of researching global politics, focusing in particular on formal modeling, quantitative methods, and qualitative approaches in International Studies. The authors reflect on the strengths and weaknesses of current methodology and suggest ways to advance theory and research in International Studies.

This volume is essential reading for methods courses and will be of interest to scholars and students alike.

Kitcher, Philip. 2003. *Science, Truth and Democracy*. Oxford: Oxford University Press. \$17.95

Noted philosopher of science Philip Kitcher examines the intense debate about the role that science plays in shaping our lives. He examines in particular the sharp divide that separates purists who believe that the pursuit of scientific knowledge is always valuable and necessary, on the one hand, from others who believe that it invariably serves the interest of those in positions of power, on the other. He rejects both perspectives and works out a more realistic image of the sciences — one that allows for the possibility of scientific truth, but nonetheless permits social consensus to determine which avenues to investigate. He then proposes a democratic and deliberative framework for responsible scientists to follow. Kitcher's nuanced yet controversial analysis and conclusion will be of interest to a wide range of readers interested in the sciences.

Klandermans, Bert and Suzanne Staggenborg (eds). 2002. *Methods of Social Movement Research*. Minneapolis: University of Minnesota Press. \$29.95

Citing the critical importance of empirical work to social movement research, the editors of this volume have put together the first systematic overview of the major methods used by social movement theorists. Original chapters cover the range of techniques: surveys, formal models, discourse analysis, in-depth interviews, participant observation, case studies, network analysis, historical methods, protest event analysis, macro-organizational analysis, and comparative politics. Each chapter includes a methodological discussion, examples of studies employing the method, an examination of its strengths and weaknesses, and practical guidelines for its application.

Lewis-Beck, Michael and Tim Ruting Liao, and Alan Bryman. 2003. *The Sage Encyclopedia of Social Science Research Methods*. Sage Publications. \$450.00

This unique multi-volume reference set offers readers an all-encompassing education in the ways of social science researchers. Written to be accessible to general readers, entries do not require any advanced knowledge or experience to understand the purposes and basic principles of any of the methods. The Encyclopedia features two major types of entries: definitions, consisting of a paragraph or two, provide a quick explanation of a methodological term; and topical treatments or essays discussing the nature, history, application/example and implication of using a certain method. Also included are suggested readings and references for future study.

Lichbach, Mark Irving. 2003. *Is Rational Choice Theory All of Social Science?* Ann Arbor: University of Michigan Press. \$59.50

The author addresses the question of the place of rational choice theory in the social sciences in general and in political science in particular. He presents a typology of the antagonists as either rationalist, culturalist, or structuralist and offers an insightful examination of the debate. In the process he reveals that the rationalist bid for hegemony and synthesis is rooted in the weaknesses, not the strengths, of rationalist thought. He concludes that the various theoretical camps are unlikely to accept the claimed superiority of the rationalist approach but that this opposition is of value in itself to the social sciences, which require multiple perspectives to remain healthy.

Mahoney, James and Dietrich Rueschemeyer (eds). 2003. *Comparative Historical Analysis in the Social Sciences*. Cambridge: Cambridge University Press. \$25.00

This book systematically investigates the past accomplishments and future agendas of contemporary comparative-historical analysis. Its core essays explore three major issues: the accumulation of knowledge in the field over the past three decades, the analytic tools used to study temporal process and historical patterns, and the methodologies available for making inferences and for building theories. The introductory and concluding essays situate the field as a whole by comparing it to alternative approaches within the social sciences. *Comparative Historical Analysis in the Social Sciences* will serve as an invaluable resource for scholars in the field, and it will represent a challenge to many other social scientists - especially those who have raised skeptical concerns about comparative-historical analysis in the past.

McAdam, Doug, Sidney Tarrow, and Charles Tilly. 2001. *Dynamics of Contention*. New York: Cambridge University Press. \$22.50

Over the past two decades the study of social movements, revolution, democratization and other non-routine politics has flourished. And yet research on the topic remains highly fragmented, reflecting the influence of at least three traditional divisions. The first of these reflects the view that various forms of contention are distinct and should be studied independent of others. Separate literatures have developed around the study of social movements, revolutions and industrial conflict. A second approach to the study of political contention denies the possibility of general theory in deference to a grounding in the temporal and spatial particulars of any given episode of contention. The study of contentious politics are left to 'area specialists' and/or historians with a thorough knowledge of the time and place in question. Finally, overlaid on these two divisions are stylized theoretical traditions - structuralist, culturalist, and rationalist - that have developed largely in isolation from one another.

Ragin, Charles. 2000. *Fuzzy Set Social Science*. Chicago: University of Chicago. \$20.00

In this innovative approach to the practice of social science, Charles Ragin explores the use of fuzzy sets to bridge the divide between quantitative and qualitative methods. Paradoxically, the fuzzy set is a powerful tool because it replaces an unwieldy, "fuzzy" instrument (the variable, which establishes only the positions of cases relative to each other) with a precise one (degree of membership in a well-defined set). Ragin argues that fuzzy sets allow a far richer dialogue between ideas and evidence in social research than previously possible. They let quantitative researchers abandon "homogenizing assumptions" about cases and causes, they extend diversity-oriented research strategies, and they provide a powerful connection between theory and data analysis. Most important, fuzzy sets can be carefully tailored to fit evolving theoretical concepts, sharpening quantitative tools with in-depth knowledge gained through qualitative, case-oriented inquiry. This book should revolutionize research methods not only in sociology, political science and anthropology but in any field of inquiry dealing with complex patterns of causation.

Rosenbaum, Paul R. 2002. *Observational Studies*. 2nd edition. New York: Springer. \$79.95

An observational study is an empiric investigation of the effects caused by a treatment, policy, or intervention in which it is not

possible to assign subjects at random to treatment or control, as would be done in a controlled experiment. Observational studies are common in most fields that study the effects of treatments on people. The second edition of "Observational Studies" is about 50 percent longer than the first edition, with many new examples and methods. There are new chapters on nonadditive models for treatment effects (Chapter 5) and planning observational studies (Chapter 11) and Chapter 9, on coherence, has been extensively rewritten.

Ruben, David-Hillel. 2003. *Action & Its Explanation*. Oxford: Oxford University Press. \$45.00

David-Hillel Ruben mounts a defense of some unusual and original positions in the philosophy of action. Written from a point of view out of sympathy with the assumptions of much of contemporary philosophical action theory, his book draws its inspiration from philosophers as diverse as Aristotle, Berkeley, and Marx. Ruben's work is located in the tradition of the metaphysics of action, and will attract much attention from his peers and from students in the field.

Scott, Joan W. and Debra Keates (eds). 2001. *Schools of Thought: Twenty-Five Years of Interpretive Social Science*. Princeton: Princeton University Press. \$26.95

This book brings together a cast of prominent scholars to assess, with unprecedented breadth and vigor, the intellectual revolution over the past quarter century in the social sciences. This collection of twenty essays stems from a 1997 conference that celebrated the twenty-fifth anniversary of the Institute for Advanced Study's School of Social Science. The authors, who represent a wide range of disciplines, are all associated with the School's emphasis on interpretive social science, which rejects models from the hard sciences and opts instead for a humanistic approach to social inquiry. Following a preface by Clifford Geertz, whose profound insights have helped shape the School from the outset, the essays are arranged in four sections. The first offers personal reflections on disciplinary changes; the second features essays advocating changes in focus or methodology; the third presents field overviews and institutional history; while the fourth addresses the link between political philosophy and world governance. Two recurring themes are the uses (and pitfalls) of interdisciplinary studies and the relation between scholarship and social change. This book will be rewarding for anyone interested in how changing trends in scholarship shape the understanding of our social worlds.

Sil, Rudra and Eileen Doherty (eds). 2000. *Beyond Boundaries? Disciplines, Paradigms, and Theoretical Integration in International Studies*. Albany: State University of New York Press. \$23.50

This book represents a critical yet constructive reappraisal of the role, and the limits, of the boundaries that define and separate disciplines and subfields in the social sciences, as well as the boundaries that divide distinct research traditions or paradigms in the analysis of international life. It provides an integrative and eclectic examination of the virtues of a more flexible division of labor, a division that facilitates more meaningful communication among scholars of different methodological persuasions investigating similar problems in international life.

Stigler, Stephen M. 2002. *Statistics on the Table: The History of Statistical Concepts and Methods*. Cambridge: Harvard University Press. \$19.95

Stigler's useful, readable, and valuable book, with its numerous illuminating illustrations and plentiful insights, is an authoritative and definitive work in the early development of mathematical statistics, and a delightful examination in witty detail of the contributions of Gauss, Laplace, deMoivre, Bayes, Galton, Lexis, James Bernoulli, Quetelet, Edgeworth, and others. With humor and conviction, Stigler describes vividly the events leading to the emergence of statistical concepts and methods. —D. V. Chopra, Choice

Tooley, Michael. 2000. *Time, Tense, and Causation*. Oxford: Oxford University Press. \$37.00

The main goal of Michael Tooley's groundbreaking book is to establish a position intermediate between the tenseless theory of time and the standard tensed theory of time. Tooley argues for a novel version of the tensed theory of time, namely, that the future is unreal and the present and past real, and yet that reality consists only of tenseless facts. The question that naturally arises for the reaper concerns an apparent paradox: how could the tensed theory of time be true (and time be "dynamic") if reality consists only of tenseless facts?

Turner, Mark. 2001. *Cognitive Dimensions of Social Science: The Way We Think About Politics, Economics, Law and Society*. Oxford: Oxford University Press. \$19.95

What will be the future of social science? Where exactly do we stand, and where do we go from here? What kinds of problems should we be addressing, with what kinds of approaches and arguments? In *Cognitive Dimensions of Social Science*, Mark Turner offers an answer to these pressing questions: social science is headed toward convergence with cognitive science. Together they will give us a new and better approach to the study of what human beings are, what human beings do, what kind of mind they have, and how that mind developed over the history of the species. Turner, one of the originators of the cognitive scientific theory of conceptual integration, here explores how the application of that theory enriches the social scientific study of meaning, culture, identity, reason, choice, judgment, decision, innovation, and invention. About fifty thousand years ago, humans made a spectacular advance: they became cognitively modern. This development made possible the invention of the vast range of knowledge, practices, and institutions that social scientists try to explain. For Turner, the anchor of all social science - anthropology, political science, sociology, economics - must be the study of the cognitively modern human mind. In this book, Turner moves the study of those extraordinary mental powers to the center of social scientific research and analysis.

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## Article Notes

*Note: Jennifer Jefferis compiled these articles, followed by their abstracts, from a variety of databases. We tried hard to find those articles (published after January 2000, our arbitrary start-date) that had interesting methodological approaches or explicit commentary on methodological issues that might be of interest to qualitative researchers. Please let us know of additional articles (including your own) that fall into this broadly defined category so that we can incorporate them into our next edition of article notes.*

Achen, Christopher H. 2002. "Toward a New Political Methodology: Microfoundations and Art." *Annual Review of Political Science* 5:423-450.

The past two decades have brought revolutionary change to the field of political methodology. Steady gains in theoretical sophistication have combined with explosive increases in computing power to produce a profusion of new estimators for applied political researchers. Attendance at the annual Summer Meeting of the Methodology Section has multiplied many times, and section membership is among the largest in APSA. All these are signs of success. Yet there are warning signs, too. This paper attempts to critically summarize current developments in the young field of political methodology. It focuses on recent generalizations of dichotomous-dependent-variable estimators such as logit and probit, arguing that even our best new work needs a firmer connection to credible models of human behavior and deeper foundations in reliable empirical generalizations.

Adcock, Robert and David Collier. 2001. "Measurement Validity: A Shared Standard for Qualitative and Quantitative Research." *American Political Science Review* 95:3 (September) 529-46.

Scholars routinely make claims that presuppose the validity of the observations and measurements that operationalize their concepts. Yet, despite recent advances in political science methods, surprisingly little attention has been devoted to measurement validity. We address this gap by exploring four themes. First, we seek to establish a shared framework that allows quantitative and qualitative scholars to assess more effectively, and communicate about, issues of valid measurement. Second, we underscore the need to draw a clear distinction between measurement issues and disputes about concepts. Third, we discuss the contextual specificity of measurement claims, exploring a variety of measurement strategies that seek to combine generality and validity by devoting greater attention to context. Fourth, we address the proliferation of terms for alternative measurement validation procedures and offer an account of the three main types of validation most relevant to political scientists.

Aspinwall, M.D. and G. Schneider. 2002. "Same Menu, Separate Tables: The Institutional Turn in Political Science and the Study of European Integration." *European Journal of Political Research* 38:5 (August) 1-36.

Recent research on European integration has largely profited from the institutional turn in political science. Theoretical progress has, however, been hampered by the diverse understandings of this new research tradition. This paper tries to tackle the conceptual diversity in a positive way. We first analyze the neo-institutionalist turn in political science and European studies and then move on to a detailed analysis and comparison of the three competing approaches — sociological, historical, and rational choice institutionalism. Next, we will show that the main differences are as much epistemological as theoretical. A convergence towards a unifying institutionalist approach can thus only be possible if some sort of a methodological convergence takes place. We sketch how a synthesis between the competing schools might appear.

Aydinli, E. and J. Mathews J. 2000. "Are the Core and Periphery Irreconcilable? The Curious World of Publishing in Contemporary International Relations." *International Studies Perspectives* 1:3 (December) 289-303.

While divisive inter- and intraparadigm debates over theories and methodology abound in the discourse of International Relations, issues surrounding geographically based divides between developed and developing world International Relations scholars have received considerably less attention. Trends of globalization and internationalization in the past decade have strengthened the argument that such divides must be bridged. This article first investigates whether there have been changes in the level of dialogue between core and periphery IR scholars throughout the 1990s by looking at publishing practices in twenty leading IR journals worldwide over seven years. It suggests explanations for the continuing lack of communication based on interviews with IR scholars from the developing world.

Beck, Nathaniel. 2001. "Time-Series—Cross-Section Data: What Have We Learned in the Past Few Years?" *Annual Review of Political Science* 4:271–293.

This article treats the analysis of "time-series-cross-section" (TSCS) data, which has become popular in the empirical analysis of comparative politics and international relations (IR). Such data consist of repeated observations on a series of fixed (non-sampled) units, where the units are of interest in themselves. An example of TSCS data is the post-World War II annual observations on the political economy of OECD nations. TSCS data are also becoming more common in IR studies that use the "dyad-year" design; such data are often complicated by a binary dependent variable (the presence or absence of dyadic conflict). Among the issues considered here are estimation and specification. I argue that treating TSCS issues as an estimation nuisance is old-fashioned; those wishing to pursue this approach should use ordinary least squares with panel correct standard errors rather than generalized least squares. A modern approach models dynamics via a lagged dependent variable or a single equation error correction model. Other modern issues involve the modeling of spatial impacts (geography) and heterogeneity. The binary dependent variable common in IR can be handled by treating the TSCS data as event history data.

Bochner, A.P. 2001. "Narrative's Virtues" *Qualitative Inquiry* 7:2, (April) 131-157.

Reacting to the charge that personal narratives, especially illness narratives, constitute a "blind alley" that misconstrues the essential nature of narrative by substituting a therapeutic for a sociological view of the person, this article speaks back to critics who regard narratives of suffering as privileged, romantic, and/or hyperauthentic. The author argues that this critique of personal narrative rests on an idealized and discredited theory of inquiry, a monolithic conception of ethnographic inquiry, a distinctly masculine characterization of sociology, and a veiled resistance to the moral, political, existential, and therapeutic goals of this work. Layering his responses to the critique with brief personal stories regarding the suppressed emotionality that motivates academics to oppose innovations, the author examines his own motives as well as those of the critics, concluding that multiplicity is easier to pronounce than to live and urging a commitment to a social science that can accommodate diverse desires.

Braumoeller, Bear F. 2003. "Causal Complexity and the Study of Politics." *Political Analysis* 11:3 (Summer) 209-233.

Theories that posit complex causation, or multiple causal paths, pervade the study of politics but have yet to find accurate statistical expression. To remedy this situation I derive new econometric procedures, Boolean probit and logit, based on the logic of complexity. The solution provides an answer to a puzzle in the rational deter-

rence literature: the divergence between theory and case-study findings, on the one hand, and the findings of quantitative studies, on the other, on the issue of the role of capabilities and willingness in the initiation of disputes. It also makes the case that different methodological traditions, rather than settling into "separate but equal" status, can instead inform and enrich one another.

Burian, Richard M. 2001. "The Dilemma of Case Studies Resolved: The Virtues of Using Case Studies in the History and Philosophy of Science." *Perspectives on Science: Historical, Philosophical, Social*. 9:4 (Winter) 383-404.

Philosophers of science turned to historical case studies in part in response to Thomas Kuhn's insistence that such studies can transform the philosophy of science. In this issue Joseph Pitt argues that the power of case studies to instruct us about scientific methodology and epistemology depends on prior philosophical commitments, without which case studies are not philosophically useful. Here I reply to Pitt, demonstrating that case studies, properly deployed, illustrate styles of scientific work and modes of argumentation that are not well handled by currently standard philosophical analyses. I illustrate these claims with exemplary findings from case studies dealing with exploratory experimentation and with interdisciplinary cooperation across sciences to yield multiple independent means of access to theoretical entities. The latter cases provide examples of ways that scientists support claims about theoretical entities that are not available in work performed within a single discipline. They also illustrate means of correcting systematic biases that stem from the commitments of each discipline taken separately. These findings illustrate the transformative power of case study methods, allow us to escape from the horns of Pitt's "dilemma of case studies," and vindicate some of the post-Kuhn uses to which case studies have been put.

Capps, P. 2000. "Incommensurability, Purposivity and International Law." *European Journal of International Law* 11:3 637-661.

Within international law, theory is often considered peripheral to more pressing practical problems. In the first part of this article, it is argued that refusal to take account of theoretical and methodological issues entails that particular descriptions of international law lack validity, and, hence, rational reasons cannot be provided as to why one account should be considered preferable to any other. This problem of rational justification, which emerges in a variety of forms, is referred to as the incommensurability thesis. The argument is illustrated with respect to the ninth edition of Oppenheim's *International Law*. In part two, a methodology is advanced which demonstrates how a justifiable account of international law can be generated which avoids the incommensurability thesis. This methodology states, specifically, that international lawyers must develop (a) a coherent understanding of the kind of function international law performs in maintaining social order in the relations between states and (b) a substantive conception of social order. Therefore, in order for a particular account of international law to possess validity over rival accounts, international lawyers must take account of social theory and moral and political philosophy. The final part of this article discusses the concepts of international law offered by Weil and Kant, which can be understood as examples of the methodological approach offered in this article.

Crozier, M. 2001. "A Problematic Discipline: The Identity of Australian Political Studies." *Australian Journal of Political Science* 36:1 (March) 7-26.

Political science as a distinct discipline only emerged in the Australian academy after the Second World War. By the end of the 1950s it was attracting thousands of undergraduates. Into the 1990s and beyond, political science has remained a relatively 'successful' discipline. However, it would be difficult to argue that the identity of Australian political science has been forged by disputes over the 'scientific' core of the discipline. Some Australian political scientists have claimed that this deficit is a fundamental weakness, denying the discipline both a set of common professional values and a basis for the integration of research. By contrast, this paper will argue that the issue of identity is better considered from a historically sensitive perspective rather than purely in terms of profession and methodology.

Diermeier, Daniel and Keith Krehbiel. 2003. "Institutionalism as a Methodology." *Journal of Theoretical Politics* 15:2 123-144.

We provide a definition of institutionalism and a schematic account that differentiates between institutional theories (in which institutions are exogenous) and theories of institutions, in which some (but not necessarily all) institutions are endogenous. Our primary argument is that institutionalism in the contemporary context is better characterized as a method than as a body of substantive work motivated by the so-called chaos problem. Secondary arguments include the following. (1.) While it is important to differentiate sharply between institutions and behavior, institutionalism presupposes a well-defined behavioral concept. (2.) When making the challenging transition from developing institutional theories to developing theories of institutions, it is essential to hold behavioral axioms fixed and to choose a form of equilibrium that exists for the class of games studied. (3.) For most research programs today, a form of Nash equilibrium has the requisite properties while the core and structure induced equilibria (SIE) that rely on the core, often lack the requisite properties.

Dowding, K. 2000. "Institutionalist Research on the European Union: A Critical Review." *European Union Politics* 1:1 (February) 125-144.

This article critically examines the recent wealth of institutionalist rational choice literature on the EU. It appraises the major fault lines and debates. It argues that non-cooperative game theory provides a thorough set of tools to examine the effects of different sorts of institutions upon the powers and limitations of different institutional actors. In certain areas scholars have not fully utilized the models applied to other political systems, but EU scholarship has taken a great leap forward in the past few years. Fully specified models with proper predictions are now being developed, though there is a tendency for modellers to introduce too many new assumptions that make empirical comparison with earlier models problematic, as it is sometimes hard to see which new assumptions are of most import. But we are into a new phase of normal science rational choice institutionalist explanation of the EU.

Edwards, Mickey. 2003. "Political Science and Political Practice: The Pursuit of Grounded Inquiry." *Perspectives on Politics* 1:2 (June) 349-354.

I recognize, as will my readers, that I'm in a fairly strange—but hopefully not untenable—position in attempting to assess the study and teaching of politics. Having been a member of Congress, and in various positions of congressional leadership, for 16 years, and having been more recently a teacher for 10 years, I am, for this purpose, both the student and the studied, the bug and the entomologist. I do

understand that it is difficult for me to view this relationship between scholar and subject in a completely objective manner; I hope to do justice to both sides of this exchange, but I recognize that I am much more likely to bring to this discussion a bug's-eye view, gazing up at the scientist as he squints through his microscope and shaking my head in wonderment as he purports to find great complexity in my most simple movements, and great simplicity in my most complex undertakings. Nonetheless, as one who occupies a position on the inner periphery of the academic circle, I am pleased to have been asked by the editors of this journal to share a few thoughts on the relationship—and on the present and potential value of the relationship—between the two distinct worlds of political science and political practice.

Elman, Colin and Miriam Fendius Elman. 2002. "How Not to Be Lakatos Intolerant: Appraising Progress in IR Research." *International Studies Quarterly* 46 (June) 231-262.

Despite the popularity of Imre Lakatos' ideas and numerous references to his *Methodology of Scientific Research Programs* (MSRP), IR scholars often misstate and misapply his criteria for appraising theoretical development. This article provides a more complete description of Lakatos's metric, addresses a number of critiques related to its use, and surveys how MSRP has been used to evaluate IR research. It suggests that IR proponents of Lakatos's methodology could better appreciate the limits of its application, and that those who use his metric could do so in a more informed way. The article argues for a sustained discussion about the promises and difficulties of theory appraisal, and suggests that MSRP may be a useful point of departure for that dialogue. It calls for IR theorists to undertake comparative analyses of different rationalist metrics to provide the basis for making informed judgments about their different strengths and weaknesses in helping to produce better theories.

Freeman, M. 2001. "Is a Political Science of Human Rights Possible?" *Netherlands Quarterly of Human Rights* 19:2 (June) 123-139.

The political science of human rights rests on a philosophical contradiction. The contemporary conception of human rights derives from the 'classical' conception of natural rights, which was based on the philosophy of natural law. Modern social science derives from nineteenth-century positivism, which arose precisely to refute the philosophy of natural law, and to exclude the concept of natural rights from scientific discourse. The concept of human rights retains from its natural-law heritage its inherent prescriptive character, but it is, for that reason, not a proper object of scientific study by the canons of positivism. I show how this philosophical confusion affects the contemporary political science of human rights. I argue that the political science of human rights should be interdisciplinary between philosophy and social science, between the social sciences, and between political science and international relations. Law should be removed from its hegemonic role in human-rights studies, and relocated correctly within this interdisciplinary approach.

Gerring, John and Paul A. Barresi. 2003. "Putting Ordinary Language to Work: A Min-Max Strategy of Concept Formation in the Social Sciences." *Journal of Theoretical Politics* 15:2 201-232.

This article proposes a 'min-max' strategy of definition applicable to all concepts intended for general usage within some language region. The min-max strategy relies on the conjoined use of minimal and ideal-type definitions. A minimal definition identifies

the bare essentials of a concept with traits sufficient to bind it extensionally while maintaining all non-idiosyncratic meanings associated with it. An ideal-type definition includes all attributes that together define the concept in its purest, most 'ideal' form. Minimal definitions are minimal in their attributes, but maximal in their phenomenal range, while ideal-type definitions are maximal in their attributes, but minimal in their phenomenal range. This min-max strategy serves to bind a concept in semantic and referential space, providing the most satisfactory general definition for that concept. We illustrate this strategy with the keyword culture. We identify the minimal and maximal definitions of 'culture', within which all definitions developed in particular research settings fall, thus resolving the conceptual ambiguity that has plagued the use of the term 'culture' and demonstrating the utility of the min-max approach as a strategy of general definition.

Gibson, Gregory, A. Cladeira, and Lester Kenyatta Spence. 2002. "The Role of Theory in Experimental Design: Experiments without Randomization." *Political Analysis* 10:4 (Fall) 362-375.

Some who have written about the logic of experimentation argue that random assignment of subjects to treatment conditions is an essential attribute of an experiment. Others disagree. Rather than treating this as a matter of dueling definitions, we consider experiments without randomization from a theoretical perspective. Our central contention here is that, for some research questions, theory dictates *systematic* (not random) assignment of respondents to experimental conditions. Two such areas of inquiry are research on political tolerance and on institutional legitimacy. This article gives cursory attention to the former body of work and detailed attention to the latter, based on an experiment conducted in a survey in 2001 on the consequences of the American presidential election for institutional legitimacy. Because in both instances theory requires nonrandom assignment, the problem becomes one of identifying the costs of nonrandomization (threats to internal validity) and specifying analytical techniques that might ameliorate those costs. Consequently, we present results from a statistical approach that addresses the problem of nonrandomization. The most important claim of this article is that theory ought to specify research design, including experimental designs, and that dogmatic attachment to one definition of *experiment* will not serve the discipline of political science.

Hillman, A.J. 2002. "Public Affairs, Issue Management and Political Strategy: Methodological Issues that Count — A different view." *Journal of Public Affairs* 1:4 (January) 356-361.

In the preceding article, Doug Schuler makes two challenges to scholars in the areas of public affairs, issues management and corporate political strategies to advance the state of empirical knowledge: (1) to make use of more sophisticated methodology, and (2) to adopt a grand theory to guide our empirical efforts. This paper is a commentary on these challenges. In it, I agree that our field of inquiry requires additional sophistication in methods, measures and statistical tools. However, I disagree that a grand theory would aid us in advancing our field. Instead, I propose that advancement is more likely to occur with the acceptance of a common dependent variable to guide our efforts. I propose that until we agree why we study what we do, we will have difficulty building a coherent stream of knowledge that speaks to the managerial audience. I advocate adopting firm performance as our ultimate dependent variable and defining our area of inquiry as 'What makes some firms outperform others in the nonmarket environment?' and ultimately, 'How does nonmarket performance affect overall firm performance?'

Hug, Simon. 2003. "Selection Bias in Comparative Research: The Case of Incomplete Data Sets." *Political Analysis* 11:3 (Summer) 255-274.

Selection bias is an important but often neglected problem in comparative research. While comparative case studies pay some attention to this problem, this is less the case in broader cross-national studies, where this problem may appear through the way the data used are generated. The article discusses three examples: studies of the success of newly formed political parties, research on protest events, and recent work on ethnic conflict. In all cases the data at hand are likely to be afflicted by selection bias. Failing to take into consideration this problem leads to serious biases in the estimation of simple relationships. Empirical examples illustrate a possible solution (a variation of a Tobit model) to the problems in these cases. The article also discusses results of Monte Carlo simulations, illustrating under what conditions the proposed estimation procedures lead to improved results.

Laitin, David D. 2003. "The Perestroika Challenge to Social Science." *Politics and Society* 31:1 (March) 163-184.

Political science faces a challenge from a "Mr. Perestroika," who decries the hegemony of formal and statistical analysis in the discipline. Although not connected with this movement, Bent Flyvbjerg makes the best case for a renewed dominance for qualitative and case study work throughout the social sciences. This article challenges Flyvbjerg's call for a *phronetic* as opposed to an *epistemic* discipline. It challenges as well the unqualified call for pluralism advocated by many in the perestroika movement. It offers instead an integrated tripartite method in which narrative, statistics, and formal modeling fill in a scientific frame.

Laver, Michael and Kenneth Benoit and John Garry. 2003. "Extracting Policy Positions from Political Texts Using Words as Data." *American Political Science Review* 97:2 (May) 311-331.

We present a new way of extracting policy positions from political texts that treats texts not as discourses to be understood and interpreted but rather, as data in the form of words. We compare this approach to previous methods of text analysis and use it to replicate published estimates of the policy positions of political parties in Britain and Ireland, on both economic and social policy dimensions. We "export" the method to a non-English-language environment, analyzing the policy positions of German parties, including the PDS as it entered the former West German party system. Finally, we extend its application beyond the analysis of party manifestos, to the estimation of political positions from legislative speeches. Our "language-blind" word scoring technique successfully replicates published policy estimates without the substantial costs of time and labor that these require. Furthermore, unlike in any previous method for extracting policy positions from political texts, we provide uncertainty measures for our estimates, allowing analysts to make informed judgments of the extent to which differences between two estimated policy positions can be viewed as significant or merely as products of measurement error.

Lieberman, Robert C. 2002. "Ideas Institutions and Political Order: Explaining Political Change." *American Political Science Review* 96:4 (December) 697-712.

Institutional approaches to explaining political phenomena suffer from three common limitations: reductionism, reliance on exogenous factors, and excessive emphasis on order and structure. Ideational approaches to political explanation, while often more sensi-

tive to change and agency, largely exhibit the same shortcomings. In particular, both perspectives share an emphasis on discerning and explaining patterns of ordered regularity in politics, making it hard to explain important episodes of political change. Relaxing this emphasis on order and viewing politics as situated in multiple and not necessarily equilibrated order suggests a way of synthesizing institutional and ideational approaches and developing more convincing accounts of political change. In this view, change arises out of "friction" among mismatched institutional and ideational patterns. An account of American civil rights policy in the 1960s and 1970s, which is not amenable to either straightforward institutional or ideational explanation, demonstrates the advantages of the approach.

Mahoney, James. 2000. "Rational Choice Theory and the Comparative Method: An Emerging Synthesis?" *Studies in Comparative International Development* 35:2 (Summer) 83-94.

It is argued that the recent engagement of rational choice theorists with the comparative method is a welcome development for the field of comparative politics. Certain issues in recent rational choice writings related to the comparative method are examined.

Mahoney, James. 2000. "Path Dependence in Historical Sociology," *Theory and Society* 29:4 (August), pp. 507-548.

Comparative-historical researchers frequently describe their arguments as path-dependent. Yet, they often lack a clear understanding of the meaning and theoretical underpinnings of the concept "path dependence." This paper attempts to clarify the meaning and uses of path dependence in comparative-historical analysis. It argues that path dependence occurs when an initial period of contingency triggers a subsequent sequence that is deterministic. Within this framework, two distinct types of path-dependent patterns are identified: self-reinforcing sequences and reactive sequences. The paper explores how these different modes of path dependence suggest an important research agenda for social and political analysis by providing substantive meaning to the assertion "history matters."

James Mahoney. 2000. "Strategies of Causal Inference in Small-N Analysis," *Sociological Methods and Research* 28: 4 (May), pp. 387-424.

Much debate concerning small-N analysis has centered on the question of whether this research tradition has powerful tools for assessing causality. Yet recent contributions make it clear that scholars are not in consensus regarding the more basic issue of what procedures and underlying logic are, in fact, used in small-N causal assessment. Focusing on the field of comparative-historical analysis, this paper attempts to clarify these procedures and logic. Methods associated with three major strategies of small-N causal inference are examined: nominal comparison, ordinal comparison, and within-case analysis. The paper argues that the use of these three strategies within particular small-N studies has led scholars to reach radically divergent conclusions about the logic of causal analysis in small-N research. One implication of this argument is that methodologists must sort out the interrelationship among strategies of causal inference before arriving at conclusions about the overall strengths and limitations of small-N analysis.

McDermott, Rose. 2002. "Experimental Methodology in Political Science." *Political Analysis* 10:4 325-342.

Experiments offer a useful methodological tool to examine issues of importance to political scientists. The historical and cultural differences between experiments in behavioral economics and social psychology are discussed. Issues of central concern to experimentalists are covered, including impact versus control, mundane versus experimental realism, internal versus external validity, deception, and laboratory versus field experiments. Advantages and disadvantages of experimentation are summarized.

Morse, Janice. Michael Barrett, Maria Mayan, Karin Olson, and Jude Spiers. 2002. "Verification Strategies for Establishing Reliability and Validity in Qualitative Research." *International Journal of Qualitative Methods* 1:2.

The rejection of reliability and validity in qualitative inquiry in the 1980s has resulted in an interesting shift for "ensuring rigor" from the investigator's actions during the course of the research, to the reader or consumer of qualitative inquiry. The emphasis on strategies that are implemented during the research process has been replaced by strategies for evaluating trustworthiness and utility that are implemented once a study is completed. In this article, we argue that reliability and validity remain appropriate concepts for attaining rigor in qualitative research. We argue that qualitative researchers should reclaim responsibility for reliability and validity by implementing verification strategies integral and self-correcting during the conduct of inquiry itself. This ensures the attainment of rigor using strategies inherent within each qualitative design, and moves the responsibility for incorporating and maintaining reliability and validity from external reviewers' judgments to the investigators themselves. Finally, we make a plea for a return to terminology for ensuring rigor that is used by mainstream science.

Mosquera, M., Y. Zapata, K. Lee, C. Arango and A. Varela. 2001. "Strengthening User Participation Through Health Sector Reform in Colombia: A Study of Institutional Change and Social Representation." *Health Policy and Planning* 16:2 (June) 52-60.

The challenge of achieving community participation as a component of health sector reform is especially great in low- and middle-income countries where there is limited experience of community participation in social policy making. This paper concentrates on the social representations of different actors at different levels of the health care system in Colombia that may hinder or enable effective implementation of the participatory policy. The study took place in Cali, Colombia and focused on two institutional mechanisms created by the state to channel citizen participation into the health sector, i.e. user associations and customer service offices. This is a case study with multiple sources of evidence using a combination of quantitative and qualitative social science methods. The analysis of respondents' representations revealed a range of practical concerns and considerable degree of skepticism among public and private sector institutions, consumer groups and individual citizens about user participation. Although participation in Colombia has been introduced on political, managerial and ethical grounds, this study has found that health care users do not yet have a meaningful seat around the table of decision-making bodies.

Moules, Nancy J. 2002. "Hermeneutic Inquiry: Paying Heed to History and Hermes: An Ancestral, Substantive, and Methodological Tale." *International Journal of Qualitative Methods* 1:3.

Hermeneutic or interpretive inquiry is a living tradition of interpretation with a rich legacy of theory, philosophy, and practice. This paper is not intended to be a treatise on the right way to view and practice this tradition, but an exploration of the legacies that inform the philosophy of practice as the author has taken it up. In this explication, the author examines the ancestral, philosophical, and methodological histories that inform a current practice of hermeneutic inquiry.

Munck, Gerardo L. 2001. "Game Theory and Comparative Politics: New Perspectives and Old Concerns." *World Politics* 53:2 (January) 173–204.

In an effort to take stock of the claims put forth by advocates of game theory, this article offers an assessment that considers game theory both as a set of theoretical principles that extends rational choice theory to interdependent decision making and as a type of formal methodology. Some important strengths of game theory are identified, such as its emphasis on actors and strategic choices and its ability to generate predictions in a logically rigorous and internally consistent manner. But many shortcomings are also discussed. One shortcoming is that the effort to develop a theory of action falls short, both in the sense of failing to provide a full explanation of actions and in the sense of not applying to domains of great significance. A second shortcoming is the failure of the procedures used in formal modeling to offer guidance pertaining to a critical step in the process of modeling: the conceptualization of the model. Thus, the challenge facing scholars in comparative politics is to consider the new perspectives offered by game theory and draw upon its strengths, but to do so without losing sight of a series of old concerns in the social sciences that game theory is not suited to tackle.

Nikitin, D. 2001. "Russian Social Science in Transition: Applied Political Research and Social Criticism." *Sociological Practice* 3:2 (June) 157-173.

One means by which the Soviet state maintained hegemony was through control over the production and movement of information. This function created ambiguity, on the one hand, and contradiction on the other, as sociological inquiry progressed within the Soviet system. This circumstance extended to both the appropriate subject matter and the methodology of sociological research. While sociology practiced in the West operated more or less within broad boundaries of free inquiry, such was not the case for most of twentieth century Russia. This paper explores this history of "permissible" sociological research in Russia, and then turns an eye on the more recent developments in sociology in light of the new Russian state. In particular, the development of democratic processes in Russia have led to a concomitant growth in research centers, institutes, and consultancies, all of which have significant practical and applied objectives for the sociological product. A listing of Russian research centers on the web is included.

Nowotny, H. 2000. "Transgressive Competence: The Narrative of Expertise." *European Journal of Social Theory* 3:1 (February) 5-21.

Relying on a powerful collective narrative through which political, legal and social decision-making is guided in the name of science, the authority of scientific experts reaches beyond the boundaries of their certified knowledge base. Therefore, expertise constitutes and is constituted by transgressive competence. The author argues that (1) changes in the decision-making structure of liberal Western democracies and changes in the knowledge production system diminish the authority of scientific expertise while increasing the context-dependency of expertise - thereby altering the nature of

its predictive claims; (2) the societal distribution of expertise, while displaying emancipatory features of empowerment of citizens, also raises issues of quality control; and (3) in order to regain a balance between public and private, i.e. individual-based societally distributed expertise, future expert systems will need to adopt a longer time-perspective. The author also reflects on directions in which future expert systems might evolve.

O'Neill, J. 2003. "Unified Science as Political Philosophy: Positivism, Pluralism and Liberalism." *Studies in History and Philosophy of Science* 34:3 (September) 575-596.

Logical positivism is widely associated with an illiberal technocratic view of politics. This view is a caricature. Some members of the left Vienna circle were explicit in their criticism of this conception of politics. In particular, Neurath's work attempted to link the internal epistemological pluralism and tolerance of logical empiricism with political pluralism and the rejection of a technocratic politics. This paper examines the role that unified science played in Neurath's defense of political and social pluralism. Neurath's project of unified science addressed problems that lie at the center of recent debates around liberalism concerning the possibility of social cooperation in conditions of pluralism. His response is distinctive in calling upon an empiricist tradition that differs from Kantian proceduralist approaches that have predominated in recent liberalism. While Neurath's position has problems, it deserves reconsideration, especially in so far as it questions the Kantian assumption that a thin language of abstract rights provides the best basis for the cosmopolitan lingua franca required by conditions of social pluralism. An investigation of the role that unified science plays in Neurath's politics also gives reasons for revising common misconceptions about the nature of the unity of science program itself.

Pierson, Paul. 2000. "Not Just What, But When: Timing and Sequence in Political Processes." *Studies in American Political Development* 14:1 (Spring) 72-92.

Many perceive the clash between those advocating rational choice theory and their critics to be the dominant cleavage in contemporary political science. At least as fundamental, if much less widely discussed, is the divide over the role of historical analysis (or the investigation of temporal processes). Most social scientists take a "snapshot" view of political life. How does the distribution of public opinion affect policy outcomes? How do individual social characteristics influence propensities to vote? How do electoral rules affect the structure of party systems? Disputes among competing theories center on which factors ("variables") in the current environment generate important political outcomes. Variable-centered analyses are based, however, on some questionable assumptions about how the social world works. The significance of such variables is frequently distorted when they are ripped from their temporal context. There is often a strong case to be made for shifting from snapshots to moving pictures. Placing politics in time systematically situating particular moments (including the present) in a temporal sequence of events and processes can greatly enrich our understanding of complex social dynamics.

Smith, S. 2000. "The Discipline of International Relations: Still an American Social Science?" *British Journal of Politics and International Relations* 2:3 (October) 374-402.

This article reviews the state of the discipline of international relations. It starts from statements made by the editors in their editorial published in the first issue of this journal. The editors noted that there seemed to have been less adherence to positivism in international relations than in other areas of political science and that there

was both more opposition to positivism and more methodological and epistemological openness in international relations than in political science generally. The article outlines the current state of the field, focusing on the rationalist mainstream and then on the reflectivist alternatives, before looking at social constructivism, seeing it as the likely acceptable alternative to rationalism in the mainstream literature of the next decade. It then turns to examine whether international relations is still an American social science, before looking at the situation in the United Kingdom. It concludes that the editors' comments were indeed accurate, but that the fact that there is both more opposition to positivism in international relations and more openness in the UK academic community does not mean that the mainstream US literature is anything like as open or pluralist. The UK community is indeed more able to develop theory relevant to the globalized world at the new millennium, but the US academic community still dominates the discipline.

Tetlock, Philip and Richard Ned Lebow. 2001. "Poking Counterfactual Holes in Covering Laws: Cognitive Styles and Historical Reasoning." *American Political Science Review* 95:4 829-843.

We report a series of studies of historical reasoning among professional observers of world politics. The correlational studies demonstrate that experts with strong theoretical commitments to a covering law and cognitive-stylistic preferences for explanatory closure are more likely to reject close-call counterfactuals that imply that "already explained" historical outcomes could easily have taken radically different forms. The experimental studies suggest that counterfactual reasoning is not totally theory-driven: Many experts are capable of surprising themselves when encouraged to imagine the implications of particular what-if scenarios. Yet, there is a downside to openness to historical contingency. The more effort experts allocate to exploring counterfactual worlds, the greater is the risk that they will assign too much subjective probability to too many scenarios. We close by defining good judgment as a reflective-equilibrium process of balancing the conflicting causal intuitions primed by complementary factual and counterfactual posings of historical questions.

Thies, C.G. 2002. "A Pragmatic Guide to Qualitative Historical Analysis in the Study of International Relations." *International Studies Perspectives* 3:4 (November) 351-372.

Researchers using qualitative methods, including case studies and comparative case studies, are becoming more self-conscious in enhancing the rigor of their research designs so as to maximize their explanatory leverage with a small number of cases. One aspect of qualitative research that has not received as much attention is the use of primary and secondary source material as data or evidence. This essay explores the potential problems encountered by political scientists as they conduct archival research or rely on secondary source material produced by historians. The essay also suggests guidelines for researchers to minimize the main problems associated with qualitative historical research, namely, investigator bias and unwarranted selectivity in the use of historical source materials. These guidelines should enable advanced undergraduates and graduate students to enhance the quality of their historically minded political science scholarship.

Tilley, S.A. 2003. "Challenging" Research Practices: Turning a Critical Lens on the Work of Transcription." *Qualitative Inquiry* 9:5 (November) 750-773.

This article interrogates transcription work in the context of qualitative research. Although it is common practice in academe for someone other than the researcher to transcribe tapes recorded for purposes of data collection, the author argues the importance of researchers taking seriously the ways in which the person transcribing tapes influences research data. She suggests that the transcriber's interpretive/analytical/theoretical lens shapes the final texts constructed and as a result has the potential to influence the researcher's analysis of data. Specifically, the article explores the experiences of Ken, a person hired to transcribe audiotapes of focus group interviews conducted for a larger research study. The numerous challenges Ken faced during the work are addressed. His use of voice recognition software to simplify the task is discussed as well as the educational potential transcription work holds for graduate students.

Tillmann-Healy, L.M. 2003. "Friendship as Method." *Qualitative Inquiry* 9:5 (October) 729-749.

In this article, the author discusses friendship as a method of qualitative inquiry. After defining friendship and positing it as a kind of fieldwork, the methodological foundations of friendship as method are established. Next, it is proposed that friendship as method involves researching with the practices, at the pace, in the natural contexts, and with an ethic of friendship. Finally, the author describes this method's strengths and considerations for both researcher and participants.

Tilly, Charles. 2001. "Mechanisms in Political Processes." *Annual Review of Political Science* 4:21-41.

Ostensibly theoretical disputes in political science often involve competing approaches to explanation, including skepticism, covering law arguments, reconstructions of propensities, system models, and explanations featuring causal mechanisms. Mechanism- and process-based accounts, including cognitive, environmental, and relational effects, deserve more attention than they have received in recent political science. Analyses of democratization illustrate these points.

Wedeen, Lisa. 2002. "Conceptualizing Culture: Possibilities for Political Science." *American Political Science Review* 96:4 713-728.

This essay makes a case for an anthropological conceptualization of culture as "semiotic practices" and demonstrates how it adds value to political analyses. "Semiotic practices" refers to the processes of meaning-making in which agents' practices (e.g., their work habits, self-policing strategies, and leisure patterns) interact with their language and other symbolic systems. This version of culture can be employed on two levels. First, it refers to what symbols do—how symbols are inscribed in practices that operate to produce observable political effects. Second, "culture" is an abstract theoretical category, a lens that focuses on meaning, rather than on, say, prices or votes. By thinking of meaning construction in terms that emphasize intelligibility, as opposed to deep-seated psychological orientations, a practice-oriented approach avoids unacknowledged ambiguities that have bedeviled scholarly thinking and generated incommensurable understandings of what culture is. Through a brief exploration of two concerns central to political science—compliance and ethnic identity-formation—this paper ends by showing how culture as semiotic practices can be applied as a causal variable.

Yauch, C.A. and H.J. Steudel. 2003. "Complementary Use of Qualitative and Quantitative Cultural Assessment Methods." *Organizational Research Methods* 6:4 (October) 465-481.

The organizational cultures of two small manufacturers were analyzed using qualitative and quantitative assessment methods. This article describes not only how qualitative and quantitative data contributed to the validity of the results through triangulation but also how the qualitative and quantitative research paradigms were used in a complementary fashion to produce a more complete understanding of the organizational cultures. Using methods from both research paradigms enabled a greater understanding of cultural artifacts and behaviors but more important of the underlying cultural values and assumptions. Based on this experience, it is recommended that qualitative and quantitative methods be used to produce more robust results than could be accomplished using a single approach for cultural assessment.

**Symposium: Interview Methods** *Political Science and Politics* 35:4 (December, 2002).

1. Leech, Beth L. "Asking Questions: Techniques for Semistructured Interviews." 665-668

In an interview, what you already know is as important as what you want to know. What you want to know determines which questions you will ask. What you already know will determine how you ask them.

2. Goldstein, Kenneth. "Getting in the Door: Sampling and Completing Elite Interviews." 669-672

Many factors are important when it comes to conducting high quality elite interviews. As my colleagues have noted in their presentations in San Francisco and in their essays in this issue, gaining valid and reliable data from elite interviews demands that researchers be well prepared, construct sound questions, establish a rapport with respondents, know how to write up their notes, and code responses accurately and consistently. Improving these skills will certainly reduce the amount of measurement error contained in interview data. Unfortunately, none of these skills matter if you do not get the interview. In other words, everything that my colleagues have talked about depends on getting in the door, getting access to your subject. A well-prepared personable researcher who would be able to control an open-ended and wide-ranging interview, while establishing a strong informal rapport with an elite respondent will never get to demonstrate his or her interviewing skills—or ability to decrease measurement error—if the meeting never takes place. Furthermore and fundamentally, systematic error will also be introduced if researchers only get access to certain types of respondents.

3. Aberbach, Joel D. and Bert Rockman. "Conducting and Coding Elite Interviews." 673-676

In real estate the maxim for picking a piece of property is "location, location, location." In elite interviewing, as in social science generally, the maxim for the best way to design and conduct a study is "purpose, purpose, purpose." It's elementary that the primary question one must ask before designing a study is, "What do I want to learn?" Appropriate methods flow from the answer. Interviewing is often important if one needs to know what a set of people think, or how they interpret an event or series of events, or what they have done or are planning to do. (Interviews are not always necessary. Written records, for example, may be more than adequate.) In a case study, respondents are selected on the basis of what they might know to help the investigator fill in pieces of a puzzle or confirm the proper alignment of pieces already in place. If one aims to make inferences about a larger population, then one must draw a systematic sample. For some kinds of information, highly structured interviews using mainly or exclusively close-ended questions may be an excellent way to proceed. If one needs to probe for information and to give respondents maximum flexibility in structuring their responses, then open-ended questions are the way to go.

4. Woliver, Laura. "Ethical Dilemmas in Personal Interviewing." 677-678

There are many issues of ethics and openness in elite interviewing that I have learned how to deal with through the years. My work has focused on people who cause trouble: protesters, litigants, defendants, sidewalk counselors, rescuers, and abortion providers, to name a few. Of course, in dealing with people you are studying you must be honest and ethical. It is important to remember that their activism comes from something they deeply feel. Their activism is because of their beliefs, opinions, experiences, and sense of community. They do not exist as activists so that you can add more lines to your vita or finish your dissertation. You must leave them in the same position in which you found them. You must do no harm to them.

5. Berry, Jeffrey. "Validity and Reliability Issues in Elite Interviewing." 679-682

Many of the early important empirical works on policymaking in Washington were built around elite interviews. We first learned about how Congress really operates from pioneers in elite interviewing such as Lewis Anthony Dexter (1969), Ralph Huitt (1969), and Donald Matthews (1960). No less revered is the scholarship of Richard Fenno (1978), John Kingdon (1995), and Robert Salisbury (1993), who have produced enduring and respected work from elite interviewing. Yet there are few other contemporary political scientists working on public policymaking who have built reputations for their methodological skills as interviewers. Elite interviewing is still widely used as the basis for collecting data, but most interviewing depends on a few trusted templates. Most commonly, elites in a particular institution are chosen at random and subjected to the same interview protocol composed of structured or semistructured questions. For example, state legislators are asked a series of questions about their attitudes on particular issues or institutional practices. Or policymakers involved in certain issues are selected and then quizzed about those matters. Some confident and skilled interviewers, like William Browne (1988) and Richard Hall (1996), combine different interview approaches in their work but they are the exceptions and not the rule.

6. Rivera, Sharon Werning, and Polina M. Kozyreva, and Eduard G. Sarovskii. "Interviewing Political Elites: Lessons From Russia." 683-688.

The past decade has opened up unprecedented opportunities for scholars of post-communist countries. Throughout much of Eastern Europe and the former Soviet Union, scholars can now engage policymakers and other elites directly through interviews—probing their decision calculi and obtaining unpublished information and data. Yet there are gaps in the scholarly literature that would prepare researchers for interviewing highly placed individuals in these countries.

**Symposium: American Political Development** *Studies in American Political Development* 17:1 (Spring, 2003)

1. Gerring, John. "APD from a Methodological Point of View." 82-102

2. Bense, Richard. "The Tension between American Political Development as a Research Community and as a Disciplinary Subfield." 103-106

3. Skowronek, Stephen. "What's Wrong With APD?" 107-110

4. Smith, Rogers M. "Substance and Methods in APD Research." 111-115.

## Announcements

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### Munck & Verkuilen Win Concepts Award

The Committee on Concepts and Methods (C&M) of the International Political Science Association (IPSA) is happy to announce that its 2003 Award for Conceptual Innovation in Democratic Studies was awarded to: Gerardo L. Munck and Jay Verkuilen, "Conceptualizing and Measuring Democracy: Evaluating Alternative Indices," *Comparative Political Studies*, Vol. 35, No. 1, February 2002, pp. 5-34.

The award, set at 1,500 USD, is sponsored by the IPSA Committee on Concepts and Methods (C&M) and the Facultad Latinoamericana de Ciencias Sociales (FLACSO) in Mexico City.

For more information on the tri-annual award, please visit to the committee's website ([www.concepts-methods.org](http://www.concepts-methods.org)).

The 2003 jury was composed of Professor Terry Karl (Chair), Stanford University, Professor Evelyne Huber, University of North Carolina at Chapel Hill, and Professor Axel Hadenius, University of Uppsala.

The article by Gerardo Munck and Jay Verkuilen was chosen from an exceptionally strong set of top-quality submissions. The Committee on Concepts and Methods thanks all authors, colleagues, and publishers who submitted their own work, the work of their peers, or their authors' work to the committee.

The citation reads as follows:

"This article makes a contribution in the fields of conceptualization and measurement, and it provides an important service to the scholarly community involved in the

study of democracy because it analyzes the conceptualization, measurement, and aggregation rules followed by the authors of the most frequently used indices of democracy. Given that quantitative studies of democracy and democratization have become a growth industry, it is crucial that scholars be aware of the comparative strengths and weaknesses of the various indices available for their analyses. The article goes beyond an evaluation of existing indices by offering a comprehensive framework for the generation of data and thus elucidating one of the crucial components of research aimed at causal inference. Finally, it makes useful suggestions for improving the quality of data on democracy."

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### *Forthcoming in the Spring Issue:*

Symposium I: Field Research

Symposium II: The Problem of Cumulation in Qualitative Methods

## *Calendar of Upcoming Events*

### **2004 Annual Meeting of the American Political Science Association**

September 2-5, 2004

### **Annual Training Institute on Qualitative Research Methods**

Arizona State University, January 5-16, 2004.

### Qualitative Methods

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