

⁵I 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.

⁶See 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).

Interpretive Empirical Political Science: What Makes This Not a Subfield of Qualitative Methods¹

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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.²

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”³ – 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 19th century as social positivists, and then evolutionary positivists, and then critical positivists, followed by 20th 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.⁴ Early 20th 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 theory⁵) 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.⁶

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 testability⁷, 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.⁸

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)⁹ – 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

¹My 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).

²One 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.

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

⁴For 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.

⁵Many 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.

⁶Such 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.

⁷I 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.

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

⁹This 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.

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,