Interpretation and Method

Empirical Research Methods and the Interpretive Turn

Second Edition

Edited by
Dvora Yanow and
Peregrine Schwartz-Shea

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Interpretation and Method

. . . human behavior always comes with meaning,
with intentions and purposes.
That is to say, the way others see and react to what you do
is powerfully affected by what they think you mean,
what you are trying to do
and why.
So if the social scientist wants to describe the sequence of the interaction,
he cannot omit what it means to the actors.
He studies not just behavior
but action.

—*Sam Beer*, "Letter to a Graduate Student"

As a culture, we were aware of the seals and the walruses on top of the ice,
but we didn't know what they were doing underneath the ice.

—*Joe Seungetuk*, Inupiaq artist and writer
Address to the Public Administration Theory Annual Conference,
University of Alaska-Anchorage, June 19, 2003

Acknowledgments

Our collaboration began with a curiosity—and a concern—about what the present generation of doctoral students was learning in the methods classroom and how textbook authors and publishers were treating the notion of "methods." This led us, at the 1999 Seattle meeting of the Western Political Science Association (WPSA), to tour the book display room to see what was being marketed to conferees. That, in turn, led to a conference paper the following year at the San Jose WPSA and to a subsequent effort to condense a narrative content analysis of twelve textbooks into ten pages for *PS: Political Science and Politics*. We are grateful to our San Jose co-panelists Martha Feldman and Sandra Kensen and respondent Janet Flammang and to those who showed up for the morning session, including the late Rita Mae Kelly, Kirstie McClure, and Jane Bayes. Their lively engagement with methodological matters encouraged us to think that others might find questions of method and truth claim justifications as intriguing as they had become to each of us. We are particularly thankful to the *PS* editor who turned down that manuscript and especially to the author of the acerbic review who found our essay a "polemic under the garb of some sort of analysis" in an argument that had been "lost over 40 years ago."

Our bemused outrage at these comments spurred us on to develop a more fully reasoned article for *Political Research Quarterly* and a subsequent Interpretive Research Methods workshop at the 2003 WPSA. We were tremendously gratified by the turnout, but even more so by the comments we received from both faculty and students, which continued to arrive long after we had returned to our respective homes. Our thanks to the participants in that event and to our WPSA colleagues Bill Haltom (that year's local arrangements chair), Tim Kaufman-Osborn (president and program chair), and Betty Moulds (executive director) for making it happen. The enthusiastic response to both article and workshop convinced us that the time was right to pull together the 2006 edition of this book. It is thanks to our editor at M.E. Sharpe, Harry Briggs, and the enthusiasm, support, and collaboration of chapter authors both old and new that you hold in your hands a second edition.

Identifying authors who could write engagingly, who were involved with empirical research, and who were grappling with methodological issues—which for us meant not just tools and techniques but the philosophical questions that make tools and techniques matter—was not easy. For both of us, this project has continued to be an education beyond our own subfields. One of us (Dvora) felt about the first edition as if she were earning her second doctorate. We are indebted to Ido Oren for directing our attention to many interesting scholars in the field of international relations, some of whom became chapter authors; and to Joe Soss, Mary Hawkesworth, Patrick Jackson, and Robert Adcock for helpful suggestions that widened the net even further. Ed Schatz, Ernie Zirakzadeh, and Ted Hopf posted methodologically related comments to various Internet lists that helped sharpen the focus of the book. The new chapters in this second edition came about from our own networks established since the appearance of the first edition—in particular, through the Methods Café, which we started at the 2005 WPSA meeting and then took to the American

Political Science Association in 2006, and the Interpretive Policy Analysis conference, launched in Birmingham (UK) in 2006.

We thank the authors, old and new, of the chapters collected here, both for their patience with our editorial requests, suggestions, and demands and for teaching us a great deal about topics and ways of thinking new to us. At M.E. Sharpe, in addition to Harry Briggs for the invitation to prepare a second edition of the book, we thank associate editor Elizabeth Granda Parker for advice negotiating the minefield of permissions and other support and copyeditor Laurie Lieb for her close reading of the manuscript.

Dvora adds: Most of the work thinking through the scope and shape of the first edition of the book was done while I was on sabbatical, sitting in Lauren Edelman's office as a visiting scholar at the Center for the Study of Law and Society at the University of California, Berkeley's Boalt Hall School of Law. Laurie not only set in motion my stay at the center; she also kindly made a place for me there while she took her own sabbatical leave elsewhere. I extend my thanks as well to members of the center—then-director Bob Kagan, associate director Rosann Greenspan, Malcolm Feeley, Jonathan Simon, KT Albiston, Kristen Luker—and other visiting scholars that year—Liz Borgwardt, Hila Keren, Ron Harris—along with Eleanor Swift at Boalt, for their gracious hospitality and for making time in their busy schedules to talk about evidence, methods, and other matters. In addition, I am enormously grateful for the colleagueship of two scholars who were then commencing their academic careers and are now fully launched: Robert Adcock, who was a sounding board for many of my thoughts, directing me to the history and sociology literatures and catching my imprecisions in word choice; and Tim Pachirat, for supremely intelligent, thoughtful, and humane critique in all of our wide-ranging conversations. And my ongoing thanks to Peri, for her enthusiasms and cautions, and for bringing perspectives from both research and teaching that have kept the project on course and that continue, unfailingly, to complement my own quite different background and reading practices.

And Peri says: I owe a strange debt to colleagues from my early years in the political science discipline, John Orbell, Randy Simmons, and the late Robyn Dawes, who tutored me in rational choice theory and experiment analysis—giving me the foundation for understanding both the strengths and the limits of those approaches. I want, also, to express my gratitude here to two then–graduate students who first introduced me to feminist scholarship, Debra Burrington and Melanee Cherry, and to other feminist colleagues such as Eloise Buker, the late Rita Mae Kelly, and MaryAnne Borrelli, who offered encouragement over the years. More recently, in both my research design and philosophy of social science courses, I have enjoyed my interactions with numerous University of Utah graduate students who have challenged me to more fully explain my thinking on methodological issues. Additionally, my departmental colleagues John Francis, Matthew Burbank, and Mark Button provided feedback at critical moments, as have two colleagues from other institutions, Diane Singerman and Valerie Hudson, who provided venues for thinking out loud about the possibilities of new methodological directions. And kudos to my husband, Tim, and my children, Carl and Tierney, for preventing me from being a workaholic. My thanks, of course, to Dvora for her unfailing collaborative skills, her gracious manner, and, above all, those many wonderful intellectual discussions that were part of coediting both the first and second editions.

Lastly, we owe Robert Adcock the inspiration for the title of this book. He suggested it initially as the name for a listserv created in the winter of 2003–4 as a home for the discussion of methods such as these and the philosophical issues they raise. As the book developed, it seemed an appropriate umbrella for the ideas expressed in these several chapters, and so we have "stolen" it for this project as well. As Robert did then, we acknowledge Hans-Georg Gadamer as the source for these borrowings. We hope our readers will hear in the title the echoes of his work and see it as a mark of our respect for our predecessors and our indebtedness to his, and their, thinking.

Wherefore "Interpretive"

An Introduction

WHAT'S "INTERPRETIVE" ABOUT INTERPRETIVE METHODS?

Why designate a set of research tools and procedures "interpretive"? Isn't all science engaged with the interpretation of data? What's "interpretive" about interpretive methods?

It is to the challenge of responding to these sorts of questions that we have set ourselves in this book. Indeed, researchers in all sciences—natural, physical, and human or social—interpret their data. Moreover, interpretive processes for analyzing texts in what might be called literary social science, such as what is done by historians, political theorists (or political philosophers), and feminist theorists, overlap with those used, for example, by "empirical" social scientists analyzing contemporary governmental and organizational documents. "Interpretive" social scientists make no claim to conceptual exclusivity in their use of that term.

However, "interpretation" has a particular meaning at this point in time in methodological discussions concerning empirical social science. Although research methods are often taught and learned as if they were tools and techniques alone, divorced from any methodological context provided by the history of ideas in science and attendant questions concerning the reality status (ontology) of the subject of study and its "knowability" (epistemology), such matters are increasingly being raised for explicit attention and discussion. We take up this concern for such contextualization in this book, focusing on empirical research across the human, or social, sciences.

The challenge of contextualizing methods in matters methodological has been becoming more acutely felt in recent years in response to two not unrelated developments in the philosophy and sociology of science: the so-called interpretive turn, and the increasing cross- or interdisciplinarity of research questions.

THE "INTERPRETIVE TURN" IN THE SOCIAL SCIENCES

As more and more of the work of late nineteenth- and early to mid-twentieth-century Continental philosophers became available in English translation, their thinking fueled what became widely known in the latter part of the twentieth century, especially in US writings, as the "interpretive turn" or the "interpretive paradigm" in the social sciences (see, e.g., Burrell and Morgan 1979; Hiley, Bohman, and Shusterman 1991; Rabinow and Sullivan 1979, 1985). The "turn" metaphor developed a life of its own, finding expression also in the linguistic turn (e.g., Rorty 1967; Van Maanen 1995), the rhetorical turn (e.g., D. McCloskey 1985), the narrative turn (see L. Stone 1979), the historic turn (T.J. McDonald 1996), the metaphorical turn (Lorenz 1998), the argumentative turn (Fischer and Forester 1993), the cultural turn (Bonnell and Hunt 1999), even the practice turn (Schatzki, Knorr Cetina, and Savigny 2001).[1] Yet the implications for empirical human and social science research practices of these philosophical, conceptual, and theoretical turns have only

rarely been spelled out, let alone in a way that links them to their ontological and epistemological presuppositions. That is one of our aims in this book.

These various "turns" share not only an orientation toward "taking language seriously" (J.D. White 1992; see also Edelman 1977), but also an overarching appreciation for the centrality of meaning in human life in all its aspects and a reflexivity on scientific practices related to meaning making and knowledge claims. The methodological ideas and the specific methods discussed in these pages have generally been subsumed under the "interpretive" heading because of the ideas and language of the interpretive turn, which became a phrase in good currency, and the philosophies that inspired or undergird it. Because those philosophies developed largely in debate with ideas from critical and logical positivisms, and because these ideas inform the so-called scientific method and its reliability and validity criteria, "interpretive" methods and methodologies contend with methodological positivism and with the quantitative methods that enact positivist philosophical presuppositions. The turning, then, is, by and large, twofold: it is a turning away from, if not against, the idea of a social scientific practice in which humans are conceptualized as objects, much as rocks or plants are in the physical and/or natural sciences, thereby erasing, too, the human traits of researchers; and it is a turning toward a rehumanized, contextualized set of practices. This provides one account of the use of the phrase "human sciences" to refer to anthropology, sociology, and so on: its emphasis on their meaning-focused and person-centered concerns, as distinct from the more behavioralist connotations of "social sciences."[2]

Much of the work arguing on behalf of interpretive approaches has focused on critiquing the positivist philosophies and ideas that inform the "pre-turn" practices of the social sciences, decrying their limitations and those of their associated methods. Although such critique, well founded in our view, is both warranted and needed, it has had the unintended consequence of establishing a negative tone to the conversation, with authors seemingly always on the attack or in a defensive posture with respect to the contributions of interpretive methods. As a consequence, the positive (in a contributory sense) delineation of interpretive techniques and their philosophical grounding has been shortchanged.

We wish not only not to repeat the critique at length, but also to present in a more positive vein the contributions of interpretive methodologies and methods to empirical social science. Part I of this book straddles the two directions of the turning—the turning against and the turning toward: It includes some critique, but that is purposed more to explicate than to be dismissive. Parts II and III detail the turn toward a rehumanized scientific practice, and Part IV reflects on the whole enterprise. An impression appears to exist that "interpretive" describes "a type of social science that is only remotely empirical and concerned primarily with problems of meaning or hermeneutics" (Ragin 1987, 3). We are not quite certain what interpretive research Charles Ragin and others might have in mind, but we present work here, particularly in Parts II and III, that is closely, even intimately, empirical *and* concerned with problems of meaning, conceived of and analyzed hermeneutically or otherwise. These works bear on action as well as understanding, whether that action is intended to foster reconceptualization and other changes within the academy or it is aimed at changes within policy-analytic and other domains of extra-academic practice. Indeed, not only do we see no contradiction between empirical research and meaning-focused analysis, as the book subtitle is intended to suggest; we are also of the view that the central focus of much empirical social science *should be* on problems of meaning. Moreover, as discussed below and in various chapters (especially Chapters 2 and 7), the meaning-making activity of human actors is central to understanding significant dimensions of causality that are obscured in positivist epistemological and ontological conceptions of that key idea.

We do not see the relationship between interpretive philosophical ideas and research methods

as causal or chronological: Philosophical presuppositions do not necessarily lead directly to methodological ones or precede them in the timeline of a researcher's analytic or theoretical development. In fact, many social scientists working with interpretive methodologies came to their philosophical presuppositions, or to know and understand them, only *after* extended involvement in empirical research. It has often been their very grounded, empirically based dissatisfaction with the explanatory power of "traditional" quantitative methods for their research questions, rather than a more general philosophically grounded inquiry, that has led researchers to explore and engage interpretive epistemologies and interpretive-inflected methods. As well, interpretive ideas and orientations have increasingly become part of the "ether" in which contemporary social science debates take place, and they have often been picked up from that context without prior immersion in the philosophical literature.

Yet many issues in empirical research practice do become clearer in light of interpretive presuppositions—once these are articulated. Here is an additional reason for the negative treatment and even outright denigration, in textbooks and elsewhere, of interpretive or so-called qualitative methods: Those of us doing that kind of research have, until fairly recently, not done a good job on the whole of articulating how it is that we do what we do, and why. This is another aim of this book—not only to identify the links between what interpretive researchers do and the ontological and epistemological arguments of interpretive philosophies, but also to identify the varieties of interpretive methods and to spell out what deliberations and procedures some of these entail.

MAKING METHODS EXPLICIT

Most researchers using interpretive methods have just set about doing the work, writing the tale, without explicit reflection on their methodological considerations, choices, and decisions. Historians write history; comparativists describe societies and governments and states; organizational studies theorists analyze issues in management practices; policy analysts analyze policies. This makes eminent sense when scholars write to communities with well-established practices of evidentiary proceedings. Increasingly, however, disciplinary and subfield walls are breaking down, and readerly practices bestride such divisions. Here lies a second impulse behind this book.

As long as a researcher is writing for a community of readers sharing the same presuppositions and assumptions, there is little or no need to be explicitly reflective about what was done either in generating data or in analyzing them, beyond a simple description of settings and sources. But when writing for other interpretive (or meaning or discourse or epistemic) communities, or across communities (as in interdisciplinary and cross-disciplinary fields of inquiry), or within communities with no agreed-upon procedural norms or when such norms are under contestation, explicit statements of methodological concerns and methods procedures become more necessary. The need for explicit statements slides along a continuum; there is no "bright line" indicating where one must come down on the side of methodological disclosure.[3] Relatively more transparency, however, may have a variety of beneficial effects. It enables a more fully developed engagement with methodological positivists (such as, for example, around discussions of objectivity). Over time, greater disclosure is likely to provide enhanced insights for improving theorizing about the ways in which researcher positionality may impact the accessing of sources and the generating and analyzing of data.[4] And increased methodological transparency, by improving understanding across disciplinary divides, may provide a better foundation for interdisciplinary work and more solid grounds for challenging existing divisions of scholarly inquiry into "fields" and "disciplines," which at this point in time are based as much on organizational inertia as on substantive grounds (Kaufman-Osborn 2006; Klein 1993; D.W. Smith 2003; Wallerstein 1999).

The lack of explicit methodological statements in research writing parallels curricular practices: Unlike training in various forms of quantitative analysis, which is explicit, stepwise, and prescriptive, ethnographic, participant-observer, historical, and textual-analytic methods have, by and large, been taught and learned inductively (although we see this beginning to change, more in some fields of study than in others, as attention to methods, in general, develops). Ethnographic and participant-observer research methods in particular have largely been learned through a kind of apprenticeship, through reading others' work in a series of topical courses and through a kind of trial-and-error learning by doing (the "drop the graduate student in the field and see if he swims" sort of teaching). Both teaching and practice entail significant degrees of tacit knowledge (in M. Polanyi's 1966 sense). This is not to say that such researchers have not reflected on their processes, but that they have felt no need to do so in great detail on the written page. As long as they were writing for a community of practice in which methods and their presuppositions were shared *and were accepted as "scientific,"* this was not problematic. The spreading impact of behaviorism on the social sciences since the 1970s, however, accompanied by the growing use of increasingly more technical statistical methods—seen in a range of academic work-related practices, from position descriptions to publication gatekeeping—coupled with the growth of cross-disciplinary reading and research, has made it increasingly necessary to make such reflection explicit. The lack of such explicitness about their systematicity and the disinclination to situate methods discussions in their philosophical grounding has contributed to the (false) impression that interpretive methods are not serious scholarship: that their procedures are not rigorous or systematic and that their findings are not trustworthy, being little more than "opinion."[5]

Part of our motivation in designing this book has been to make methodological concerns more explicit in a way that is both reflective on (and thereby consistent with one of interpretive methods' own philosophical tenets) and illustrative of what interpretive philosophies and methods have to offer. Combining these two impulses—the drive toward procedural transparency, together with insights garnered from the philosophical mandates of the interpretive turn—has led us and many of the authors of chapters included here to problematize the received wisdom concerning certain terms within "research methods."

Specifically, several chapters in this book explore the meanings of the concepts of science and scientific reason (Mary Hawkesworth in Chapter 2); explanation and causality (Robert Adcock's Chapter 5); rigor (Dvora Yanow in Chapter 6); and generalizability, validity, and reliability (Peregrine Schwartz-Shea's Chapter 7). In a related vein, Kirstie McClure, in Chapter 3, investigates the very basis on which statistical science was initially legitimated. When interpretive researchers and methodological positivists use the same terms but mean different things, without attending to those differences, noncommunicative debates ensue, to the detriment of the subject.[6] It has been thought, and taught, for instance, that interpretive methods lack rigor, do not concern themselves with causality, are not reliable or valid, and so on. Many, if not most, methods textbooks support this view (Schwartz-Shea and Yanow 2002). Discussions of these and other terms in this book's chapters, however, show that this received wisdom reflects, at times, a construal of these concepts' meaning that has been narrowed from their meanings in their earlier or original contexts (e.g., the meaning of "rigor" in formal logic). This narrowing has led methods discussions off in one direction, leaving behind a wide range of interpretive analytic methods that are, in point of fact, logically rigorous, concerned with causality, and pursuant of research trustworthiness. In contestations over these terms, we appear to have a version of partisan battles over ownership of the national flag—except that the defending camp (in this case, those doing qualitative and interpretive work) has by and large accepted the other camp's (re)definition of important concepts and ceded that terrain, not recognizing that under earlier (if not original) definitions, both (or all)

parties' practices would enjoy an equal claim. This capitulation may be due to a lack of training in disciplinary history and in the philosophy of (social) science,[7] as well as to the ahistorical treatments of methods in curricula and in textbooks.

In problematizing these terms, the authors of these chapters return the discussion to earlier meanings underlying their usage. This is not an "original intent" argument—we have no constitution to interpret—but it is an effort to reclaim lost meaning within the philosophy of science, whether social-human or natural-physical (as philosophy of science in the social-human realm must contend with philosophy of science in the natural-physical realm, in particular in the form of "the scientific method" that is assumed in the former to derive from the latter). It is our hope at least to flag the problem of miscommunication due to incompatible terminological meanings as a concern and to suggest that serious attention is needed to concept usage—not that usage is in error, or even "merely" different or idiosyncratic (interpretive researchers are not Humpty Dumpty, making words mean whatever we want them to), but that these meanings represent a shared, collective usage within a community of thought and practice, informed by different philosophical stands. It is, after all, shared practices that produce the research *gestalts* that render particular terms intelligible *within* a particular research community, but which also make them misunderstood or opaque *across* communities, as Schwartz-Shea illustrates with the thought experiment that opens Chapter 7. To the extent that "methodology" is usefully seen as "applied ontology and epistemology," we seek to make these shared practices more transparent.

SO WHAT'S WRONG WITH "QUALITATIVE"?

"Qualitative" methods as a category and descriptor increasingly does not capture the full range of non-quantitative methods used in empirical social science research, and, in particular, methods of the sort presented here. 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" (a point taken up in Chapters 1 and 2) and whether there is, or can and should be, only one version of science in the social sciences. Contra Keohane (2003, 11), the "standards of science held up to us by the natural sciences and espoused by economics and psychology" are *not* the only way to do political and other social sciences; nor is there, for that matter, a single way to do natural science, as Knorr Cetina's (1999) comparative study of particle physics and molecular biology makes clear. Phenomenological approaches are increasingly being heard in economics, especially among European scholars, and, even more strongly, in various subfields of psychology (see, e.g., Atwood and Stolorow 1984). There are established interpretive positions in anthropology and in sociology, as well as critical theoretical, feminist, and other approaches that share a focus on meaning. In other areas of these disciplines, as well as in interdisciplinary fields such as educational studies, social (or human) geography, organizational studies, and urban planning, "qualitative" methods are under increasing pressure to adhere to the characteristics of large 'n' research. In hiring descriptions for faculty positions, "qualitative" increasingly refers to research using focus groups, structured interviews, Q-sort, and other similar techniques, rather than ethnographic, participant-observer, ethnomethodological, semiotic, narrative, and other such approaches. There are differences both in procedure and in rationales for such procedures between these two families of methods, reflecting differences in ontological and epistemological presuppositions. This is what keeps interpretive methods from being a subfield of qualitative methods: The two increasingly do not travel under the same philosophical umbrella when it comes to their respective procedural enactments of assumptions about the reality status and knowability of their subjects of inquiry.

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, solidified by improvements in computer processing and its growing capacity to manipulate large amounts of numerical data with increasingly less human effort and involvement. The structural logic of the language of "quantitative" drew "qualitative" into play by counter-distinction: If statistics and "large 'n'" studies (increasingly enabled by computer abilities) were to be understood as quantitative analysis, then "small 'n'" studies using nonstatistical methods—field-based observing and interviewing—must be "qualitative" analysis.

What "qualitative" originally designated, then, were the features characteristic of Chicago School-style field studies of the early to mid-twentieth century—ethnographies in anthropology departments and participant-observations in sociology departments, as those two separated and carved out distinct turfs, and their extension to political, organizational, educational, and other studies.[8] Often thought of as "traditional" qualitative methods, such research attends to data of three broad sorts: language (spoken by actors in the situation or written in such forms as organizational correspondence, government documents, or individuals' diaries); acts and interactions (including nonverbal communication elements such as gestures and vocalizations); and the physical settings and objects used in these acts or referenced in language (such as governmental buildings, census questionnaires, paintings, and organizational mission statements). These three classes of artifact are analyzed to infer the meanings conveyed through them (Chapter 1 elaborates on this).

Chief among the features of such work are:

1. word-based modes of generating data, through observing (with whatever degree of participating; see Gans 1976) extended over time, which immerses the researcher in the language, values, beliefs, and other cultural aspects of the study's domain (see Ellen Pader, Chapter 10, this volume); talking, including "conversational" or "discursive" (a.k.a. "in-depth" or "unstructured" or even "semi-structured") interviewing (see Joe Soss, Chapter 8, and Frederic Schaffer, Chapter 9); and the close reading of research-relevant documents and/or other materials (see, e.g., Ilan Danjoux, Chapter 20);
2. word-based modes of analyzing word data (rather than "translating" them into numbers for statistical analysis, for instance; see Jutta Weldes, Chapter 12, and Dean McHenry, Chapter 13, as well as the chapters in Part III, this volume); and
3. a richly detailed narrative form of communicating both data and findings, in which tables and figures, when used, supplement and/or illustrate the data and/or analysis—or constitute the data—rather than presenting them in summarized form.

Traditional qualitative methods require a flexible response "in the moment" to observational (including participational) and talk or 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 (see Chapters 1 and 6 for further discussion). That 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 (Schwartz-Shea and Yanow 2012). For this reason, it is accepted interpretive methodological practice not to begin such a study with a formal hypothesis that is then "tested" against field "realities." Researchers in interpretive modes more commonly begin their work in an abductive logic of inquiry, with puzzles or a sense of tension between expectations and prior observations, grounded in the research literature and, not atypically, in some prior knowledge of the study setting (Agar 2010;

Locke, Golden-Biddle, and Feldman 2008; Schwartz-Shea and Yanow 2012, 27–34; Van Maanen, Sørensen, and 2007). Understanding and concepts are allowed (indeed, expected) to emerge from the data as the research progresses.[9]

Unfortunately, the on-site flexibility and less stepwise research design that characterize traditional qualitative methods have been taken to mean that these methods are not systematic. This is hardly the case, as attention to the care with which settings, 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., Feldman 1995; Feldman et al. 2003; or Murphy 1980 for further discussion of this point). Neither "qualitative" nor "interpretive" means "impressionistic." Along with procedural systematicity, interpretive work entails a "philosophical rigor" (in Mark Bevir's phrase [2003])—a rigor of logic and argumentation—rather than merely a procedural "rigor." The chapters in Parts II and III document the systematicity of interpretive methods.

The difficulty with the "qual-quant" nomenclature, however, goes beyond a misleading understanding of what constitutes "qualitative" research. Increasingly, that term 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 (e.g., King, Keohane, and Verba 1994; cf. Brady and Collier 2004, 2010). Interpretive methods are not troubled by some of the issues that appear to concern these and other qualitative researchers following methodologically positivist approaches: establishing concepts to be tested in the field; theory-testing of a priori, deductive theories; problems of measurement and sample size; or building qualitative databases. For interpretive researchers, concepts are part of the everyday talk, lives, and written or depicted record of situational actors and/or embedded within a literature, becoming part of the historical background that forms the context for scholarly thinking; the attempt to specify them a priori, as universal constructs, violates interpretive presuppositions about the historical locatedness of scholars and actors (as Oren illustrates in Chapter 17, this volume). Interpretive researchers also conceptualize the development and use of theory differently: developed inductively or abductively, theory can be assessed against particular research contexts, as a potential resource for understanding, rather than as an apparatus of causal, predictive laws. Word- and visual-data, as the chapters here show, need not be translated into numerical indices or measures to achieve analytic power or legitimacy. Finally, building qualitative databases is problematic, because data are seen as being coproduced in and through field-based interactions rather than as objectified, free-standing entities available ("given") for "collection" divorced from their field setting.

Efforts to "improve" the quality (from the point of view of methodological positivism) of meaning-focused studies have brought them under pressure to conform to the evaluative criteria—validity and reliability—that characterize quantitative methods and 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 and have their own evaluative standards (see Chapter 7).

It is the struggle to produce satisfyingly robust data, for instance, *under the requirements of positivist science* that leads King, Keohane, and Verba (1994), for example, to call for increasing the number of observations in order to improve small 'n' studies (see also Chapter 7). 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 in field research outside of explicitly comparative work—but a field study of a single community or organization or polity entails a large 'n'

of data points in its sustained observation (with whatever degree of participation) over extended periods of time, often in and of various locations within the research site, extended and repeated conversational interviews, and/or a multiplicity of agency, policy, or other documents read and analyzed.[10] One might imagine counting, for example, the large number of hours of engaged observation, the number of conversations held, the number of interactions, and the ensuing number of segments of observation and/or conversation and/or interaction analyzed over the course of the research project—any one of which would yield a large 'n,' indeed. In her study of a single organization, for example—a single 'n' study, by traditional reckoning—sociologist Rosabeth Moss Kanter spent over 120 "personal on-site contact days," during which she conducted over 120 "more than momentary conversations" in which she asked her interlocutors to describe other people (uncounted) and their situations (also not tabulated) (Kanter 1993, 337). This account omits the countless hours of observing and "momentary" conversations, all of which contributed to informing her research and analysis. In some sense, each one of these constitutes an "observation," although not necessarily as that term is used in quantitative analyses.[11]

The pressure to adopt a more "quantitative" methodology is leading to the growing delimitation of the term "qualitative" to connote methods other than what it initially designated—the sorts of field studies generated by Chicago School-style anthropologists, sociologists, and others and case study developers across the social sciences (e.g., H.S. Becker et al. 1977 [1961]; Blau 1963 [1953]; Crozier 1964; Dahl 1961; Dalton 1959; H. Kaufman 1960; Lofland 1966, 1969; Powdermaker 1966; Whyte 1955 [1943]). The distinction is a misnomer: "Quants" interpret their data; "quals" count things that they study. But even more than this: The binomial quantitative-qualitative taxonomy has become a placeholder, a surrogate shorthand standing in as a symbolic representation of a much broader issue than the question of who counts. The language of quantitative and qualitative has increasingly become a proxy for differences, largely unarticulated, 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 their less explicit but nonetheless recognizable family resemblance to the ontological and epistemological presuppositions of the Continental interpretive philosophies of phenomenology and hermeneutics (and some critical theory) and their American counterparts of symbolic interactionism, ethnomethodology, and pragmatism, among others.[12] 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 and the cumbersomeness of the doubled term, they are more commonly referred to only as "interpretive" methods, although one also finds reference to "constructivist," "constructionist," or phenomenological methods.[13]

To understand what is being captured symbolically by "qualitative" and "quantitative," we must go back to the initial purpose of social research: Researchers are making claims to knowledge. To claim that something is knowable entails a related claim in regard to its "reality status." Epistemological and ontological claims are mutually implicating—*and* they implicate methodological choices. If one claims that a door is objectively real (its existence is independent of and external to the observer) and that it is knowable through external ("objective") observation, then methodological positivism's scientific method is a reasonable methodological procedure to choose for establishing and supporting truth claims emerging from research into some aspect of that door.[14] If one cannot claim knowledge of an organization or a community on the basis of external obser-

vation *alone*, then one needs a different methodology and different methods for producing and supporting knowledge claims.

The "quant-qual" division, in sum, demarcates a distinction between epistemological and ontological claims that rest on positivist philosophical presuppositions and those claims influenced by schools of thought that put human meaning making at the center of their concerns. The latter have been subsumed under the term "interpretive." What we have then, in binomial terms, is a "quantitative-interpretive" methods divide.[15]

This binomial becomes especially clear when we separate methods of *accessing* data sources or *generating* data from methods of *analyzing* those data once they are accessed and/or generated. For those human sciences that rule out laboratory or other experimental methods (whether for ethical or for practical reasons, such as the difficulty or impossibility of establishing control groups), data are accessed and generated through observing events and the actors in them (with whatever degree of participation), through talking with those actors about those events, and/or through close readings of documentary and other material sources (e.g., film, agency buildings) identified as central to the research question—or some combination of all three. Part II of the book includes discussions of these three modes of generating data.

In our introduction to Part II we draw the distinction between collecting, accessing, and generating data, but we note here our preference for talking about "generating" data rather than the more widely used "collecting" data. The latter term is laboratory language, in which butterflies or potsherds or other artifacts are physically gathered up and brought back to the lab for analysis; whereas in the nonexperimental, field-based studies[16] conducted in the human sciences, the primary "data" and their sources are left in their locations of origin (or should be). What are brought back are the researcher's copious interview and/or observational notes and/or notes on documents and other materials, such as paintings (e.g., Bellhouse 2011), although copies of documents, interview tapes, and the like may be brought out of the archive or the field.[17] This formulation makes clear that the data of such studies are not the people themselves, or the events and conversations and settings and acts, or even the documents, but rather the researcher's views of these, as encapsulated in her notes. "Data," in this approach, are not things given (*datum*, *data*, from the Latin "to give"), but things encountered, often by surprise, observed, and made sense of, interpreted.[18] What is accessed are sources of data; the data themselves are generated, whether by the researcher interacting with visual/tactile/spatial sources or coproduced in conversational and/or participatory interactions. This understanding of "data" as constituted by human researchers' observations renders problematic the creation of databases of interpretive data for other researchers to use. So-called raw data may be the "least interpreted" form (in contrast to its character in succeeding stages of the research process), but the "interpretive moment" cannot be escaped: It colors all stages of the research process, such that human science data are never really "raw" and "unprocessed." In getting an interpretive (or qualitative) researcher's data, other researchers would be getting processed, not "raw," data—"cooked" and filtered through the initial researcher's interpretive schema.[19]

VARIETIES OF INTERPRETIVE ANALYTIC METHODS

At this point (if not before; see below), "data" in hand, methodologies part company, largely around the question of the legitimacy of "word data." At the epistemological level, methodological positivists posit the superiority of quantitative data over word data. From this perspective, words are *best* translated into numbers for purposes of statistical analysis.[20] Interpretive researchers reject the assumption of the superiority of numerical data over other forms of data (e.g., sound, visual imagery, built space materials). They do *not* reject quantitative data per se. Instead, they

take an interpretive perspective on numbers: That communities choose to count particular phenomena reveals much about what communities value and the problems that are, or are not, recognized as central to their identities and concerns (see, e.g., Czarniawska-Joerges 1992 for an interpretive analysis of budgets).[21] Interpretive researchers, then, respect the form or *genre* of the data, and word and other types of data are retained in their original form for purposes of interpretive analysis.

It is common knowledge that there is a wide range of "advanced" methods of statistical analysis: Markov chain Monte Carlo ideal point estimation; multinomial logit analysis and multinomial probit analysis; ARIMA (autoregressive integrated moving average), MANCOVA (multivariate analysis of covariance), and MANOVA (multivariate analysis of variance); cluster analysis, factor analysis, principal components analysis, and more.[22] It is less widely known *outside of the interpretive research community* that there is a broad range of methods for the interpretive analysis of word and other data genres, some of them (e.g., semiotic squares) no less "advanced" or complex than nonparametric random-effects analysis or Poisson regression. Several of these are explored in Part III of the book. Table I.1 provides a partial list of the "infinite" variety of interpretive methods. We emphasize that the list is suggestive and by no means complete.[23]

This variety also illustrates the fact that interpretive researchers do not all speak with one voice on some of the central philosophical and procedural issues. Interpretive philosophies have only been available beyond the German- (and, to a lesser extent, French-) reading world since the mid-twentieth century or so. Their explicit, conscious, and intentional extension into the world of research methodology, and along with it the effort to argue for the standing of such methodologies and methods in the world of science, began much more recently than that. The internal debates and intellectual arguments are still unfolding. Indeed, one might array the analytic methods listed in Table I.1 along a continuum, from more descriptive to more explicitly critical-theoretical (the latter being more intentionally directed at the effects of institutional structures and power on individual meaning making). Case study, grounded theory, life and oral histories, and participant-observation analyses—to make a gross generalization—might more commonly be found at the descriptive end; action research, critical theory, deconstruction, discourse, and post-structural analyses might be at the other end; frame and value-critical analyses might be more toward the center on the critical side; and so forth. But the inadequacy of such a distinction is highlighted when one notes that interpretive work of all kinds, in rendering tacit knowledge explicit, makes silenced discourses speak, thereby perforce engaging questions of power. Any interpretive analytic method, in other words, has the capacity to move fully across the descriptive-critical continuum.[24]

What leads a researcher to choose to follow an interpretive or some other path is largely the set of ontological and epistemological presuppositions undergirding the initial shaping of the research question. The linguistic structure of the word does not mean that these *pre*suppositions are necessarily arrived at *prior to* methods. It is equally possible—and, in our experience as researchers, teachers, and readers of others' work, 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). "Presupposition" should be taken in a conceptual or logical sense, then, rather than in a chronological one, to mean what one must suppose—even if one does so un- or subconsciously—about social realities and their knowability in order logically to hold particular methodological positions. A phenomenological approach to this matter suggests a way to engage the question, Where do presuppositions come from? They are not necessarily explicitly known, and often not explicitly reflected on or consciously chosen. There seems, instead, to be a relationship between presuppositional inclinations—toward research that resonates with one's