WHAT IS A CASE?

Exploring the foundations of social inquiry

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Introduction: Cases of "What is a case?"[1]

CHARLES C. RAGIN

The precept of case analysis

Social science methodology is anchored by a number of basic precepts that are rarely questioned by practitioners. One precept that is central to the logic of analysis is the idea of having *cases.* Social scientists use terms like "*N* of cases," "case study," and "sample of cases" with relatively little consideration of the possible theories and metatheories embedded in these terms or in the methods that use cases and make conventional forms of analysis possible. For example, we may describe an investigation as a case study because it involves ethnographic research in one setting, without ever considering what constitutes a case study or whether there are methodologically decisive differences between case studies and other kinds of studies. Another example: In quantitative research, we use the terms "cases" and "units of analysis" interchangeably without considering the problems that might come from conflating data categories and theoretical categories (Ragin 1987:7–9). One researcher may use *families* as data-collection sites in a survey study; another may write a book called *What Is Family?* (Gubrium and Holstein 1990). A third example: A study that uses interviews of employees to construct a picture of the informal organization of a firm looks superficially like one that uses interviews of employees to address variation in job satisfaction. Both studies use interviews of employees as the primary data source, but the first is about the firm as a whole, while the second is about employees' subjective states. In short, the term "case" and the various terms linked to the idea of case analysis are not well defined in social science, despite their widespread usage and their centrality to social scientific discourse.

Implicit in most social scientific notions of case analysis is the idea that the objects of investigation are similar enough and separate enough to permit treating them as comparable instances of the same general phenomenon. At a minimum, most social scientists believe that their

methods are powerful enough to overwhelm the uniqueness inherent in objects and events in the social world. The idea of comparable cases is implicated in the boundary between dominant forms of social science and other types of discourse about social life (e.g., journalism and, in many quarters, history). The audiences for social science expect the results of social scientific investigation to be based on systematic appraisal of empirical evidence. Use of evidence that is repetitious and extensive in form, as when it is based on observations of many cases or of varied cases, has proved to be a dependable way for social scientists to substantiate their arguments. Social scientists who conduct case studies argue that their cases are typical or exemplary or extreme or theoretically decisive in some other way. Thus, even in case-study research the principle of repetition is often implicated in statements concerning the relation between the chosen case and other cases. [For an extended discussion of this issue, see Feagin, Orum, and Sjoberg (1991) and especially Sjoberg et al. (1991).]

No matter which case or unit investigators use in their empirical analyses, they typically invoke additional units in the presentation of their research. An analysis of individual-level survey data from a sample of adults in the United States, for example, provides a foundation for statements about individuals and about the United States (in addition to the range of units in between these two poles – e.g., communities, cities, social classes). A study of this type can be seen both as an extensive analysis of many cases (the sample of individuals) and as an intensive case study of the United States. Further, the United States may be seen as a case in several different senses. For example, it may be seen as a member of a larger set of broadly defined objects (e.g., advanced societies), or as an instance of an important theoretical concept or process (e.g., partial implementation of meritocratic principles), or as an intrinsically interesting historical or cultural entity in its own right (e.g., a country that is changing in historically specific or decisive ways). This example shows as well that while it is tempting to see the case study as a type of qualitative analysis, and perhaps even to equate the two, virtually every social scientific study is a case study or can be conceived as a case study, often from a variety of viewpoints. At a minimum, every study is a case study because it is an analysis of social phenomena specific to time and place.

When presenting their results, investigators manipulate both empirical cases and theoretical cases, and these different cases may vary by level, as when they are nested or hierarchically arrayed, and they may vary in specificity. A given body of empirical evidence also can be constructed into a variety of different substantive cases (a case of mismanagement, or a case of overwhelming external pressures, or a case of inertia, etc.). To the question "What is a case?" most social scientists would have to give multiple answers. A case may be theoretical or empirical or both; it may be a relatively bounded object or a process; and it may be generic and universal or specific in some way. Asking "What is a case?" questions many different aspects of empirical social science.

Conceptions of "cases" and social science discourse

The various usages and meanings of the term "case" have far-reaching implications for the conduct of social science; this fact alone is enough to justify questioning its status. The issue also deserves careful consideration because different conceptions of the term "case" are central to the enduring gulf between quantitative and qualitative social science. The term "case" is one of many basic methodological constructs that have become distorted or corrupted over time. The typical pattern is for a key methodological term to gain multiple and sometimes contradictory meanings. Consider, for example, the term "cause." A fundamental rule of quantitative social science is that "correlation is not causation." Yet social scientists routinely make statements that one variable *causes* variation in another, when the evidence is based entirely on correlational patterns. The term "cause" has been permitted multiple, nonoverlapping meanings. It is only when critics challenge a researcher who uses correlational evidence that "cause" in the sense of documented empirical connections is addressed (see Becker's discussion, Chapter 9). The term "control" offers another example. Originally, "control" referred almost exclusively to experimental designs, where causal factors are directly manipulated by the investigator (hence the appropriateness of the term "control"). Today we use the term "control" to refer as well to arithmetic adjustments based on assumptions of causal linearity and additivity in analyses of naturally occurring data. [A conceptually oriented overview of these practices is offered by Lieberson (1985).] Thus, we say that we control statistically for the effect of parental social class when we assess the effects of public-versus-private-school attendance on student performance, when in fact we have no real control at all over who attends which school. The term has been broadened in meaning to refer to very different research activities.

The same holds true for the term "case." The view that quantitative researchers look at many cases, while qualitative researchers look at only one or a small number of cases, can be maintained only by allowing considerable slippage in what is meant by "case." The ethnographer

who interviews the employees of a firm in order to uncover its informal organization has at least as much empirical data as the researcher who uses these same interviews to construct a data set appropriate for quantitative assessment of variation among employees in job satisfaction. Both have data on employees and on the firm, and both produce findings specific in time and place to that single firm. Further, both researchers make sense of their findings by connecting them to studies of other firms. Yet the ethnographer is said to have but one case and to be conducting a case study, while the quantitative researcher is seen as having many cases.

In this light, much of what is considered large-*N* research also must be seen as case-study research, and the tendency to conflate *qualitative study* and *case study* should be resisted. To apply the same term to vastly different methodological constructs serves only to increase the perception that the different kinds of social science are irreconcilable and that their practitioners speak mutually unintelligible languages. We need to strive for greater clarity in what we mean by "case" and differentiate its various meanings. This emendment of current practices will simplify the task of linking qualitative and quantitative research and bring greater richness and unity to the conduct of social science (Ragin 1991).

Consider this book a first step in confronting this important task. Collectively, the contributors have questioned the term "case" from a variety of viewpoints; their contributions to this volume can be seen as the groundwork for future efforts toward refining the various meanings of "case." As the contributions show, it is difficult to ask "What is a case?" without addressing other bases of social scientific methodology. Asking this question initiates a long-overdue conversation about the foundations of social science and the meanings of the terms we use to describe what we do.

Conversations about "What is a case?"

The conversation about the term "case" presented in this volume had its origins in other conversations. The issue of cases came up often in a logic-of-analysis workshop that Howard Becker and I conducted in the winter and spring of 1988. This particular workshop had its origins in still other exchanges, based on our shared reaction to the unexamined status of the case in social science methodology.

This peculiar status of the "case" was clear to me in my work *The Comparative Method* (Ragin 1987). In that work I showed how conventional variable-oriented comparative work (e.g., quantitative cross-national research), as compared with case-oriented comparative work,

disembodies and obscures cases. In most variable-oriented work, investigators begin by defining the problem in a way that allows examination of many cases (conceived as empirical units or observations); then they specify the relevant variables, matched to theoretical concepts; and finally they collect information on these variables, usually one variable at a time – not one case at a time. From that point on, the language of variables and the relations among them dominate the research process. The resulting understanding of these relations is shaped by examining patterns of covariation in the data set, observed and averaged across many cases, not by studying how different features or causes fit together in individual cases.

The alternative, case-oriented approach places cases, not variables, center stage. But what is a case? Comparative social science has a ready-made, conventionalized answer to this question: Boundaries around places and time periods define cases (e.g., Italy after World War II). In comparative and historical social science, there is a long tradition of studying individual countries or sets of theoretically or empirically related countries conceived as comparable cases. The conventionalized nature of the answer in macrosocial inquiry made it simple to contrast variable-oriented and case-oriented approaches. It could just as easily be argued, however, that not countries but rather parallel and contrasting event sequences are cases (see Abbott's contribution, Chapter 2), or that generic macrosocial processes, or historical outcomes, or macro-level narratives are cases. "What is a case?" is problematic even where researchers are confronted at every turn by big, enduring, formally constituted macrosocial units such as countries.

The problem of "What is a case?" is even more crucial when the contrast between variable-oriented and case-oriented approaches is transferred to other research domains, because in most research areas the answers are less conventionalized. Is a social class a case or a variable? (See Platt's discussion in Chapter 1.) This is not a trivial question for scholars interested in social movements and the future of inequality. Is an analysis of United States census data a study of many cases (individuals) or one case (the United States)? As I pushed my ideas about case-oriented research into new substantive areas, I found that I had a lot of new questions about cases. The logic-of-analysis workshop provided a good setting for exploring these questions.

Howard Becker brought many questions about cases to the workshop, too. His concerns overlap with mine, but also differ qualitatively (see Chapter 9). In the workshop, and later in the symposium where the essays in this volume were first presented, he persistently pulled the rug out from under any possible consensus about "What is a case?" From

his perspective, to begin research with a confident notion of "What is a case?" (or, more precisely, what *this* – the research subject – is a case of) is counterproductive. Strong preconceptions are likely to hamper conceptual development. Researchers probably will not know what their cases are until the research, including the task of writing up the results, is virtually completed. What *it* is a *case of* will coalesce gradually, sometimes catalytically, and the final realization of the case's nature may be the most important part of the interaction between ideas and evidence.

In short, Becker wanted to make researchers continually ask the question "What is this a case of?" The less sure that researchers are of their answers, the better their research may be. From this perspective, no definitive answer to the question "What is a case?" can or should be given, especially not at the outset, because *it depends*. The question should be asked again and again, and researchers should treat any answer to the question as tentative and specific to the evidence and issues at hand. Working through the relation of ideas to evidence answers the question "What is this a case of?"

Thus, while I hoped the workshop would bring the start of an answer to my questions about cases, Becker, my co-conspirator, hoped to keep the question on the floor, unanswered. These contrasting orientations made for a lively workshop, with some participants sharing my concern for answers, however tentative and incomplete, and others sharing Becker's concern for keeping the question alive. Still, we all left the workshop with a strong sense of unfinished business. This sense that there was much more that could be mined from the topic was our primary motivation for organizing a symposium on the topic.

We decided to keep the conversation about cases going by posing the question to a select group of eight social scientists in the fall of 1989, to be followed by a symposium on the topic in the following spring. Our primary selection principles were diversity and originality. Convincing eight diverse social scientists to come to Northwestern University and offer their thoughts on "What is a case?" was easier than we had anticipated. Potential participants were eager to take a crack at our question. As a lure we suggested the following topics to our participants as possible issues for discussion:

1. Alternative definitions of cases, of case study, and of case analysis.
2. The contrast between observational and explanatory units and the implications of this distinction for research findings as representations of social life.
3. The hierarchical nature of units and the implications of this structure for case analysis and the goal of generalization.
4. The place of theoretical and purposive sampling in social science and the relation between these sampling strategies and case study.

5. The relationship between analysis of cases and analysis of research literatures.
6. The different uses of case studies in social science.
7. The boundary between social science and other forms of discourse and the place of the concept of the case in supporting this boundary.
8. What is a good case study?

Our lure produced a collection of contrasting answers to the question "What is a case?" and an avalanche of new questions as well. During the two-day conference, which involved not only the invited scholars but also many of the original workshop participants and a lively group of students and faculty from Northwestern and other universities in the Chicago area, it was difficult to separate our questions about cases from a wide array of issues concerning the foundation of modern social science methodologies.

The present collection of essays includes all eight prepared specifically for the symposium. However, the essays have been modified in response to the discussion at the symposium and in response to each other as well.

Starting points for answering "What is a case?"

Before discussing the different responses, it is important to note that none of the symposium participants offered what might be considered conventional answers to the question. For example, no one pushed methodological individualism or the idea that social life can be understood only from the perspective of individual actors. Nor did any of the participants attempt to defend textbook treatments of cases – the idea that there are populations of cases (observations) "out there" waiting for social scientific analysis. (However, acceptance of this position is implied in Lieberson's essay; see Chapter 4.) Nor did anyone endorse the idea that the definition of a set or population of cases was the purely practical task, specific to each research endeavor, of defining the universe of possibly relevant or comparable observations. When sampling came up for discussion at the symposium, the primary focus was on theoretical or purposive sampling, not random sampling from a population. Correspondingly, there was little discussion of issues in estimating population parameters. The concept of "the case" is logically prior both to the concept of "the population" and the concept of "the variable." In a context where the concept of "the case" is made problematic, these other concepts appear impenetrable.

While the answers to "What is a case?" were diverse, they displayed common themes. Participants agreed that the precept of case analysis is

fundamental to the conduct of social science and that it has a special, unexamined status. They agreed that individual social scientists answer the question "What is a case?" in remarkably different ways and that answers to this question affect the conduct and results of research. And all agreed that cases may be multiple in a given piece of research: What the case is may change both in the hands of the researcher (during the course of the research and when the results are presented) and in the hands of the researcher's audiences.

This general agreement on the importance and indeterminate nature of the term "case" should not be taken as evidence that striking differences in emphases do not exist among the eight responses. In fact, the differences are dramatic. At the most general level, the contributions differ in whether the question stimulated a critique of current practices or a reflection on research experiences. Four of the eight contributions focus on critiques of common practices, and four focus on their own experiences. Crosscutting this descriptive dichotomy, however, are more fundamental differences which reflect different starting points for answering the question. To understand these different starting points, consider two key dichotomies in how cases are conceived: (1) whether they are seen as involving empirical units or theoretical constructs and (2) whether these, in turn, are understood as general or specific.

The first dichotomy (whether the question of cases involves empirical units or theoretical categories) is common in discussions of social science methodology and overlaps with the philosophical distinction between realism and nominalism. Realists believe that there are cases (more or less empirically verifiable as such) "out there." Nominalists think cases are theoretical constructs that exist primarily to serve the interests of investigators. A realist sees cases as either given or empirically discoverable. A nominalist sees cases as the consequences of theories or of conventions.

The second dichotomy concerns the generality of case categories. Are case designations specific (e.g., the "authoritarian personality" or the "anti-neocolonial revolution") and developed in the course of research (e.g., through in-depth interviews or historical research) or are they general (e.g., individuals, families, cities, firms) and relatively external to the conduct of research? In many areas of research, generic units are conventionally treated as cases, and case categories are neither found nor derived in the course of research. They exist prior to research and are collectively recognized as valid units by at least a subset of social scientists. Specific case categories, by contrast, emerge or are delineated in the course of the research itself. What the research subject is a "case of" may not be known until after most of the empirical part of the

Table I.1. *Conceptual map for answers to "What is a case?"*

Understanding of cases	Case conceptions: Specific	Case conceptions: General
As empirical units	1. Cases are found (Harper)	2. Cases are objects (Vaughan)
As theoretical constructs	3. Cases are made (Wieviorka)	4. Cases are conventions (Platt)

project is completed. To a limited extent, this second dichotomy overlaps with the qualitative-quantitative divide in social science. The cases of quantitative research tend to exist as conventionalized, generic categories independent of any particular research effort. The cases of qualitative research tend to coalesce as specific categories in the course of the research. "What is this – the research subject – a case of?" is a question that is best asked in qualitative social science.

The cross-tabulation of these two dichotomies (Table I.1) yields four possible starting points for answering the question "What is a case?" Consider the nature of "cases" from the perspective of each cell of the cross-tabulation:

Cell 1: Cases are found. In the first quadrant, researchers see cases as empirically real and bounded, but specific. They must be identified and established as cases in the course of the research process. A researcher may believe that "world systems" (networks of interacting and interdependent human societies) are fundamentally important empirical units for understanding the history of human social organization and therefore may seek to determine the empirical boundaries of various historical world systems (verifiable, e.g., through evidence of trade in bulk goods between peoples of differing cultures). Researchers who approach cases in this way see assessment of the empirical bounding of cases as an integral part of the research process. Among the eight contributions, the clearest advocate of this view of cases is Douglas Harper (Chapter 6). Harper makes the empirical unit "community" problematic and attempts to delineate communities inductively, through individuals. Communities are bounded in different ways depending on their nature, and the boundary of a single community may be fluid and ever-changing.

Cell 2: Cases are objects. In the second quadrant, researchers also view cases as empirically real and bounded, but feel no need to verify

their existence or establish their empirical boundaries in the course of the research process, because cases are general and conventionalized. These researchers usually base their case designations on existing definitions present in research literatures. A researcher interested in explaining contemporary international inequality, for example, would accept nation-states (as conventionally defined) as appropriate cases for his or her analysis. Often coupled with this view is an instrumental attitude toward cases – they exist to be manipulated by investigators. Diane Vaughan's contribution (Chapter 8) offers the best example of this approach. Her empirical cases are conventional units such as organizations and families. She argues that by exploring generic processes (e.g., misconduct) across different types of generic empirical units (e.g., families and formal organizations), it is possible to develop better theories.

Cell 3: Cases are made. Researchers in this quadrant see cases as specific theoretical constructs which coalesce in the course of the research. Neither empirical nor given, they are gradually imposed on empirical evidence as they take shape in the course of the research. A cell-3 investigator interested in tyranny, for example, would study many possible instances of tyranny. This investigation might lead to an identification of an important subset of instances with many common characteristics, which might be conceived, in turn, as *cases* of the same thing (e.g., as cases of "patrimonial praetorianism" or as cases of "modern tyranny"). Interaction between ideas and evidence results in a progressive refinement of the case conceived as a theoretical construct. At the start of the research, it may not be at all clear that a case can or will be discerned. Constructing cases does not entail determining their empirical limits, as in cell 1, but rather pinpointing and then demonstrating their theoretical significance. Michel Wieviorka's contribution (Chapter 7) offers a good example of this understanding of cases (as does John Walton's; see Chapter 5). Wieviorka's essay shows how the interaction of ideas and evidence in his research on terrorism made it possible for him to identify its sociologically decisive features.

Cell 4: Cases are conventions. Finally, in the fourth quadrant, researchers see cases as general theoretical constructs, but nevertheless view these constructions as the products of collective scholarly work and interaction and therefore as external to any particular research effort. A researcher, for example, might conduct research on "industrial societies," recognizing that the assignment of empirical cases to this theoretical category is problematic and that the theoretical category

itself exists primarily because of collective scholarly interest. In this view, cases are general theoretical constructs that structure ways of seeing social life and doing social science. They are the collective products of the social scientific community and thus shape and constrain the practice of social science. This view of cases is the basis for Jennifer Platt's contribution (Chapter 1). Among other things, she shows how the cases of a given study may shift over time as intellectual fashions change in social science and past work is selectively reconstructed by the social scientific community.

This fourfold division of case conceptions is not absolute. A researcher could both use conventionalized empirical units, accepting them as empirically valid (cell 2), and try to generate new theoretical categories or case constructs (cell 3) in the course of his or her research. Frustrations with conventional case definitions and practices (cell 4) could lead researchers to intensify their empirical efforts and to define cases and their boundaries in a more inductive manner (cell 1). In fact, most research involves multiple uses of cases, as specific or general theoretical categories and as specific or general empirical units. These multiple uses occur because research combines theoretical and empirical analysis, and the two kinds of analyses need not use parallel cases or units. The point of Table I.1 is not to establish boundaries between different kinds of research, but to establish a conceptual map for linking different approaches to the question of cases.

The eight answers

As noted, independent of starting point, the contributions split equally into two main groups. The first four are critiques of conventional practices. The second four are analyses of research experiences.

Jennifer Platt (Chapter 1) searches broad expanses of the terrain of social research in her effort to explore the diverse ways sociologists use the term "case." This sweep includes both qualitative and quantitative empirical research from the past and present. She uncovers surprisingly little consistency. Both empirical and theoretical cases are multiple within most research efforts, and investigators only occasionally seem concerned to match theoretical cases and empirical cases. For example, much theorizing about social classes as cases has occurred over the history of sociology and political science. Yet many recent efforts to study classes empirically use survey data from samples of residents (often males only) and infer classes and their characteristics by aggregating the characteristics of individuals. The distance between these artificial statistical constructions and the theoretical categories are obvi-

ously great, especially when viewed from the Marxist perspective of classes as historical actors. Yet this way of studying classes has become conventionalized (Platt 1984). Another confounding factor in social scientists' uses of cases is whether investigators see the research setting itself as a meaningful historical case (e.g., Great Britain in the 1970s) or as just one among many possible equivalent settings for research (a postindustrial society). Platt also notes that in many studies crucial arguments depend on evidence about other cases in other studies; thus the evidence used to support a conclusion may be secondhand or even thirdhand. Researchers may construct arguments from contrasts between their own cases and those of other researchers, even when this contrast involves using secondary cases in ways that conflict with their original uses. This feature of social research underscores the communal nature of case use in social science.

Andrew Abbott's critique of conventional practices (Chapter 2) focuses on the tendency for most social scientists to conflate the dichotomy of "population" (or large *N*) versus "case study" with the dichotomy of "causal analysis" versus "narrative account." He argues that social scientists should conduct narrative analysis across many cases. After dissecting several "population/analytic" studies (large-*N*, variable-oriented investigations), Abbott concludes that these conventional forms of analysis cannot systematically address action, agency, and complex event sequences. These studies describe cases as acting only in crude and ad hoc ways. For example, some version of the rational actor model may be invoked to explain an anomalous statistical relationship. Abbott contends that investigators should ask "What do cases do?" first and that narratives, as cases, are the appropriate units. Inductively, the investigator constructs narrative accounts and explanations from events, which in turn are found in colligations of occurrences and other evidence. Thus, narratives can be discerned in specific empirical evidence. The end product is not a mere collection of narratives, however. Abbott points to the possibility of building generic narrative steps and generic plots from the events and sequences that make up individual narratives [see also Abbott and Hrycak (1990) on patterns and sequences of events].

Harrison White (Chapter 3) bases his analysis of social scientists' use of cases on an examination of "worldly" conventions in their use – how nonscientists use them. He finds three basic worldly uses: (1) to establish identity, (2) to explain or resolve by invoking general principles, and (3) to account for why events unfold in one way and not another, with the idea that such knowledge can be used to control situations or to fix them in some way. These different worldly uses of cases are paralleled in different kinds of social scientific work. An example of the first type is Immanuel Wallerstein's *Modern World System* (1974), which establishes the world capitalist system as a singular and fundamental unit for social scientific thinking. An example of the second type is Barrington Moore's *Social Origins of Dictatorship and Democracy* (1966), which accounts for a range of political outcomes with a single explanatory framework. Jane Mansbridge's *Why We Lost the ERA* (1986) offers an excellent example of the third type. White shows that there is no simple correspondence between these different goals in the use of cases and the kinds of evidence or types of empirical units used in a study. Mansbridge's book, for example, a classic "control" case study in White's framework, includes survey data on individuals showing broad-based support for the equal-rights amendment and the ideas it embodied. White argues forcefully that scholarly recognition of these worldly conventions in the use of cases would improve social scientific thinking.

The relative utility of different types of empirical units for formulating or testing causal arguments is Stanley Lieberson's key concern (Chapter 4). He is troubled by what might be called pseudo-causal analysis (Mill's method of agreement and his indirect method of difference) mechanically applied to small numbers of cases. Researchers working with small *N*'s have argued that Mill's methods permit rudimentary causal analysis [see, e.g., the exchange between Nichols (1986) and Skocpol (1986)]. Lieberson disagrees and frames his contribution to this volume as a cautionary tale: Although it may be tempting to do causal analysis of small *N*'s using Mill's methods, the end product will be a seductively deterministic, and probably faulty, causal generalization. Lieberson attempts to demonstrate that when *N*'s are small, the possibility of formulating causal statements that are both general and reasonable (e.g., "state breakdown is a cause of social revolution") is greatly diminished. The problems that crop up resemble those that occur in quantitative analysis when researchers attempt to maximize the proportion of explained variation (Lieberson 1985). While this cautionary tale bypasses discussion of the ways in which Mill's methods have been superseded (e.g., Ragin 1987:85–123), it is significant because much of the discussion of "What is a case?" focuses on small *N*'s. Lieberson's implicit message is that investigators who want to make valid causal generalizations should use generic empirical units and conduct large-*N*, variable-oriented investigations. Lieberson illustrates his arguments not through analyses of examples of small-*N* research, but by showing what might happen if these methods were applied mechanically to artificially constructed, generic empirical units – automobile accidents contrasted with nonaccidents.

John Walton (Chapter 5) argues that cases are made by invoking theory. The process of justifying a case – as a case of something important – involves showing that the case belongs to a specific family of phenomena. This family, in turn, is important because of its relevance to general social scientific thought ("theory"). This theoretically grounded character of cases explains both why they are central to the advancement of theory – why case studies appear prominently in the history of social thought – and why cases can be made and remade as new theories are applied to known cases (a point also made by Jennifer Platt in Chapter 1). What cases are "cases of" may change as our theories change. To demonstrate his arguments, Walton describes how "the case" shifted in his own study (Walton 1991) of "California's little civil war," an episode of conflict between the residents of Owens Valley and the City of Los Angeles over rights to the valley's water. The case had obvious historical significance, but its sociological significance was at first elusive. Moving back and forth between theoretical ideas and historical evidence, Walton eventually found a suitable theoretical context for framing his case sociologically.

Douglas Harper's contribution (Chapter 6) offers a clear example of how cases are conceived in much of qualitative sociology. He plumbs the boundaries of communities through intensive study of individuals. In effect, the individual provides a window on the community. In the process of finding communities through individuals, empirical limits are established. For example, through ethnographic investigation of "Willie," a rural handyman, Harper (1987) unearths a complex web of formal and informal exchanges and interdependences. Willie's skill in making repairs and creating useful objects out of refuse is a feature of this community, not simply an aspect of Willie (i.e., Willie's human-capital endowment). Harper's answer to "What is a case?" argues, in essence, that cases can be found inductively, pieced together from the lived experiences of individuals. When collaborating with researchers who see communities as givens (i.e., defined by formal political boundaries or census tracts), Harper chafes and struggles because he feels that an important part of the research – finding and delimiting cases – has been assumed away.

Michel Wieviorka opens his discussion (Chapter 7) by examining the factors that make something a "good case," focusing first on the peculiar status of the good case in medicine. A medical case is "good" when it is both rare and diagnosable; it embraces both an individual, the patient, and an important or new category in the professional literature. Wieviorka then moves to the contrast between good cases in history and sociology. What makes a case good in history often differs from

what makes it good in sociology, even though the two approaches may be applied to the same historical facts. A historical case is good because of its significance for subsequent events; a sociological case is good because of its theoretical significance. These two don't always go hand in hand, however; the sociologically decisive aspects of the case may not be relevant to questions about why the case followed one historical path and not another. To illustrate his arguments, Wieviorka presents his own work on terrorist groups, where his primary concern was to identify their theoretically decisive character (i.e., a feature that was general to these groups and that clearly differentiated them from other groups) (see Wieviorka 1988). This search led to the development of a new theoretical case or construct, which in turn allowed him to differentiate among terrorist groups. As Wieviorka explains, some groups which originally had been classified as terrorist in journalistic accounts could now be seen to differ from terrorist groups marked by the presence of theoretically decisive features. Thus, the end result of empirical research for Wieviorka is a new or refined theoretical case.

Diane Vaughan's contribution (Chapter 8) focuses on general empirical units as cases – families, organizations, nation-states, and so on. But she sees in this diversity of empirical units a great possibility for studying specific processes in vastly different types of settings. She opens by noting that the typical academic career requires social scientists to know more and more about less and less and that this specialization also often entails a focus restricted to a single empirical unit (e.g., the family). This restriction impedes theory development and elaboration because many of the phenomena that interest social scientists and their audiences appear in many different types of empirical units, at various levels of complexity and size. Patterns observed studying a phenomenon in small units (e.g., misconduct in families) can lead to theoretical and analytic insights in the study of the same phenomenon in larger units (e.g., organizations). This creative symbiosis is further strengthened because different kinds of evidence are available in different types of units. For example, it is difficult to do an in-depth interview of a formal organization, but possible to do so for the members of a family [as in Vaughan's study (1986) of couple breakups using principles from organizational theory]. Formal organizations, by contrast, leave many written records of their day-to-day operations; families, relatively few. Thus, Vaughan's answer to "What is a case?" celebrates the diversity of generic empirical units and the many opportunities for social scientists this affords.

Looking ahead

One of our authors commented at the symposium that the question "What is a case?" was like a Rorschach test and capable of producing a variety of responses from social scientists, even like-minded ones. It is true that the question produces diverse responses. It is true as well that the question is a prism. As Becker shows in his reflections on the contributions (Chapter 9), it is possible to see the practices of social science in new ways through this prism. The issue of cases touches basic questions in how we, as social scientists, produce results and seem to know what we know. The essays in this collection emanate from the use of this prism in diverse realms of social scientific practice.

In many respects "What is a case?" is a conversation that for us has no real beginning or end. But we also feel that in some respects it has been a missing conversation in the social sciences, because all too often the "case" is a basic, taken-for-granted feature of social science research. It is important to examine taken-for-granted features because they limit our understanding and vision both of social life and of social science. In this sense, "What is a case?" is one question among many others (e.g., "What is a population?" or "What is a variable?") waiting for serious attention. We hope that our project has given this needed conversation new life and that this collection will stimulate new efforts both to answer "What is a case?" and to ask other basic questions about taken-for-granted elements of social science research.

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