Using Qualitative Methods for Causal Explanation

Joseph A. Maxwell

Field Methods 2004 16: 243
DOI: 10.1177/1525822X04266831

Using Qualitative Methods for Causal Explanation

JOSEPH A. MAXWELL
George Mason University

The view that qualitative research methods can be used to identify causal relationships and develop causal explanations is now accepted by a significant number of both qualitative and quantitative researchers. However, this view is still controversial, and a comprehensive justification for this position has never been presented. This article presents such a justification, addressing both recent philosophical developments that support this position and the actual research strategies that qualitative researchers can use in causal investigations.

Keywords: *cause; philosophy; qualitative; realism; validity*

The ability of qualitative research to address causality has been a contested issue for some time. Divergent views on this question are currently held within both the qualitative and quantitative traditions, and there is little sign of a movement toward consensus. However, the emergence of realism as a distinct alternative to both positivism/empiricism and constructivism as a philosophical stance for social science (Layder 1990; Sayer 1992; Baert 1998) has provided a new way to address this issue. I will first outline the positivist/empiricist and constructivist positions on qualitative research and causal explanation and then describe a realist approach that avoids many of the problems created by these positions.

The positivist/empiricist position regarding research on causality is that qualitative research methods cannot by themselves be used to establish causal relationships or causal explanations. The narrow version of this position, as stated by Light, Singer, and Willett (1990), is that "to establish a causal link, you must conduct an *experiment*. . . . Of the three research para-

This article has gone through many revisions and has benefited from the comments of too many graduate students and colleagues to acknowledge individually, as well as one anonymous reviewer for Field Methods*. In addition, I am grateful for the encouragement given by Carol Weiss, who also provided a temporary home for the article as a Working Paper of the Harvard Project on Schooling and Children, and Russ Bernard, who kept nagging me to revise the article and get it out the door.*

Field Methods, Vol. 16, No. 3, August 2004 243–264
DOI: 10.1177/1525822X04266831

digms we discuss [descriptive, relational, and experimental], only experimental inquiries allow you to determine whether a treatment *causes* an outcome to change" (pp. 5–6, emphasis in original).

A broader version of this view is that nonexperimental quantitative methods, such as structural equation modeling, can also be used to make causal claims (Blalock 1961; see Shadish, Cook, and Campbell 2002:392–414). Most proponents of these views hold that qualitative methods are limited to suggesting causal hypotheses or providing supporting data for "causal" quantitative research (e.g., Shavelson and Towne 2002).

Both of these versions of the positivist/empiricist position derive from David Hume's analysis of causality, as further developed by philosophers such as Carl Hempel (Baert 1998:176–9). Hume argued that we cannot directly perceive causal relationships, and thus, we can have no knowledge of causality beyond the observed regularities in associations of events. For this reason, causal inference requires some sort of systematic comparison of situations in which the presumed causal factor is present or absent, or varies in strength, as well as the implementation of controls on other possible explanatory factors.

This idea that causality is fundamentally a matter of regularities in our data was the "received view" in philosophy of science for much of the twentieth century. It was codified by Hempel and Oppenheim (1948) in what they called the "deductive-nomological" model of scientific explanation, which held that scientific explanation of particular events consists of deducing these from the initial conditions and the general laws governing relationships between the relevant variables; Hempel later added models of statistical explanation to this (Salmon 1989:1–25). This "regularity" theory, with modifications, has been the dominant causal theory in quantitative research in the social sciences (Mohr 1996:99); the demise of positivism as a viable philosophy of science had little impact on quantitative researchers' ways of addressing causality.

This concept of causation also had a far-reaching effect on qualitative research. Some qualitative researchers accepted the strictures that it implies and denied that they were making causal claims that were more than speculative (e.g., Lofland and Lofland 1984:100–2; Patton 1990:490–1). Becker (1986) has described the detrimental effect of Hume's theory on sociological writing, leading researchers to use vague or evasive circumlocutions for causal statements, "hinting at what we would like, but don't dare, to say" (p. 8).

Other qualitative researchers reacted to this position by denying that causality is a valid concept in the social sciences (Layder 1990:9–12). A particularly influential statement of this position was by Lincoln and Guba (1985), who argued that "the concept of causality is so beleaguered and in such serious disarray that it strains credibility to continue to entertain it in any form approximating its present (poorly defined) one" (p. 141). They proposed replacing it with "mutual simultaneous shaping," which they defined in the following way:

> Everything influences everything else, in the here and now. Many elements are implicated in any given action, and each element interacts with all of the others in ways that change them all while simultaneously resulting in something that we, as outside observers, label as outcomes or effects. But the interaction has *no directionality*, no *need* to produce *that particular outcome*. (p. 151, emphasis in original)

Guba and Lincoln (1989) later grounded this view in a constructivist stance, stating that "there exist multiple, socially constructed realities ungoverned by natural laws, causal or otherwise" (p. 86) and that "'causes' and 'effects' do not exist except by imputation" (p. 44).

These two reactions to the regularity view have been so pervasive that the 1,000-page second edition of the *Handbook of Qualitative Research* (Denzin and Lincoln 2000a) has no entries in the index for cause or explanation. The only references to causality are historical and pejorative: a brief mention of "causal narratives" as a central component of the attempt in the 1960s "to make qualitative research as rigorous as its quantitative counterpart" (Denzin and Lincoln 2000b:14) and a critique of the "causal generalizations" made by practitioners of analytic induction (Vidich and Lyman 2000:57–8).

However, the positivist rejection of using qualitative research for causal explanation was challenged by some qualitative researchers (e.g., Denzin 1970:26; Britan 1978:231; Kidder 1981; Fielding and Fielding 1986:22; Erickson 1992:82). Miles and Huberman (1984; see Huberman and Miles 1985) took an even stronger position:

> Until recently, the dominant view was that field studies should busy themselves with description and leave the explanations to people with large quantitative data bases. Or perhaps field researchers, as is now widely believed, can provide "exploratory" explanations—which still need to be quantitatively verified.
>
> Much recent research supports a claim that we wish to make here: that field research is far *better* than solely quantified approaches at developing explanations of what we call *local causality*—the actual events and processes that led to specific outcomes (Miles and Huberman 1984:132, emphasis in original).

They suggested that given multisite data, qualitative methods can develop rather powerful general explanations and can confirm causal models suggested by survey data.

Likewise, although most quantitative researchers still deny that qualitative methods can by themselves answer causal questions (e.g., Shavelson and Towne 2002), some have moved away from this view. For example, Rossi and Berk (1991), after advocating the use of randomized experiments in program evaluation, state, "This commitment in no way undermines the complementary potential of more qualitative approaches such as ethnographic studies, particularly to document why a particular intervention succeeds or fails" (p. 226). And Shadish, Cook, and Campbell (2002), although committed to experiments as the best method for causal investigation under most conditions, see no barrier in principle to using qualitative methods for causal inference (pp. 389–92, 500–1).

However, the view that qualitative research can rigorously develop causal explanations has never been given a systematic philosophical and methodological justification. There are two essential tasks that such a justification must accomplish. First, it must establish the philosophical credibility of this position, since the traditional, positivist/empiricist view is grounded in a philosophical understanding of causation that inherently restricts causal explanation to quantitative or experimental methods. Second, it must address the practical methodological issue of how qualitative methods can identify causal influences and credibly rule out plausible alternatives to particular causal explanations, a key tenet of scientific inquiry. I will first discuss two developments that support the legitimacy of causal explanation based on qualitative research: the rise of realist approaches in philosophy that see causation as fundamentally a matter of processes and mechanisms rather than observed regularities, and the development of a distinction between variable-oriented and process-oriented approaches to explanation (see Maxwell 2004a). I will then turn to the strategies that qualitative researchers can use in their research to establish causal explanations.

A REALIST APPROACH TO CAUSAL EXPLANATION

There has been a significant shift in the philosophical understanding of causality in the last fifty years (Salmon 1998), one that has not been fully appreciated by many social scientists. This shift is, in large part, the result of the emergence of realism as an alternative to both positivism/empiricism and constructivism as a philosophy of science (Layder 1990; Putnam 1990; Sayer 1992; Pawson and Tilley 1997; Archer et al. 1998; Baert 1998).

Realists typically understand causality as consisting not of regularities but of real (and in principle observable) causal mechanisms and processes, which may or may not produce regularities. For the philosophy of science in general, this approach to causality has been most systematically developed by Salmon (1984, 1998). For the social sciences, it is often associated with (but by no means limited to) those calling themselves "critical realists" (Sayer 1992; Archer et al. 1998). Realism's critique of the "regularity" conception of causation has challenged not only its restriction of our knowledge of causality to observed regularities but also its neglect of contextual influences (Sayer 1992:60–1; Pawson and Tilley 1997) and mental processes (Davidson 1980, 1993; McGinn 1991) as integral to causal explanation in the social sciences and its denial that we can directly observe causation in particular instances (Davidson 1980; Salmon 1998:15–6).

This realist view of causation is compatible with, and supports, all the essential characteristics of qualitative research, including those emphasized by constructivists. First, its assertion that some causal processes can be directly observed, rather than only inferred from measured covariation of the presumed causes and effects, reinforces the importance placed by many qualitative researchers on directly observing and interpreting social and psychological processes. If such direct observation is possible, then it is possible in single cases rather than requiring comparison of situations in which the presumed cause is present or absent; this affirms the value of case studies for causal explanation. Second, in seeing context as intrinsically involved in causal processes, it supports the insistence of qualitative researchers on the explanatory importance of context and does so in a way that does not simply reduce this context to a set of "extraneous variables." Third, the realist argument that mental events and processes are real phenomena that can be causes of behavior supports the fundamental role that qualitative researchers assign to meaning and intention in explaining social phenomena and the essentially interpretive nature of our understanding of these (Blumer 1956; Maxwell 1999, 2004a). Fourth, in claiming that causal explanation does not inherently depend on preestablished comparisons, it legitimizes qualitative researchers' use of flexible and inductive designs and methods.

Realism is also compatible with many other features of constructivism and postmodernism (Baert 1998:174; Maxwell, 1995, 1999, 2004b), including the idea that difference is fundamental rather than superficial, a skepticism toward "general laws," antifoundationalism, and a relativist epistemology. Where it differs from these is primarily in its realist ontology—a commitment to the existence of a real, although not "objectively" knowable, world—and its emphasis on causality (although a fundamentally different concept of causality than that of the positivists) as intrinsic to social science.

Putnam (1990), one of the major figures in the development of contemporary realism, states that

> whether causation "really exists" or not, it certainly exists in our "life world." What makes it real in a *phenomenological* sense is the possibility of asking "Is that really the cause?" that is, of *checking* causal statements, of bringing new data and new theories to bear on them. . . . The world of ordinary language (the world in which we actually live) is full of causes and effects. It is only when we insist that the world of ordinary language (or the Lebenswelt) is defective . . . and look for a "true" world . . . that we end up feeling forced to choose between the picture of "a physical universe with a built-in structure" and "a physical universe with a structure imposed by the mind." (p. 89, emphasis in original)

VARIANCE THEORY AND PROCESS THEORY AS FORMS OF CAUSAL EXPLANATION

The philosophical distinction between positivist/empiricist and realist approaches to causality is strikingly similar to, and supports, an independently developed distinction between two approaches to research, which Mohr (1982, 1995, 1996) labels variance theory and process theory. Variance theory deals with variables and the correlations among them; it is based on an analysis of the contribution of differences in values of particular variables to differences in other variables. Variance theory, which ideally involves precise measurement of differences and correlations, tends to be associated with research that uses probability sampling, quantitative measurement, statistical testing of hypotheses, and experimental or correlational designs. As Mohr notes, "the variance-theory model of explanation in social science has a close affinity to statistics. The archetypal rendering of this idea of causality is the linear or nonlinear regression model" (Mohr 1982:42).

Process theory, in contrast, deals with events and the processes that connect them; it is based on an analysis of the causal processes by which some events influence others. Process explanation, since it deals with specific events and processes, is less amenable to statistical approaches. It lends itself to the in-depth study of one or a few cases or a relatively small sample of individuals and to textual forms of data that retain the chronological and contextual connections between events.

Similar distinctions between variance and process approaches in the social sciences are those between "variable analysis" and the "process of interpretation" (Blumer 1956), variable- and case-oriented approaches (Ragin 1987), and factor theories and explanatory theories (Yin 1993:15ff.). And Gould (1989) describes two approaches in the natural sciences: one is characteristic of physics and chemistry, fields that rely on experimental methods and appeal to general laws; the other is characteristic of disciplines such as evolutionary biology, geology, and paleontology, which deal with unique situations and historical sequences. He argues that

> the resolution of history must be rooted in the reconstruction of past events themselves—in their own terms—based on narrative evidence of their own unique phenomena. . . . Historical science is not worse, more restricted, or less capable of achieving firm conclusions because experiment, prediction, and subsumption under invariant laws of nature do not represent its usual working methods. The sciences of history use a different mode of explanation, rooted in the comparative and observational richness of our data. (pp. 277–9)

Both types of theories involve causal explanation. Process theory is not merely "descriptive," as opposed to "explanatory" variance theory; it is a different approach to explanation. Experimental and survey methods typically involve a "black box" approach to the problem of causality; lacking direct information about social and cognitive processes, they must attempt to correlate differences in output with differences in input and control for other plausible factors that might affect the output. Qualitative methods, on the other hand, can often directly investigate these causal processes, although their conclusions are subject to validity threats of their own.

A striking example of the difference between variance and process approaches is a debate in the *New York Review of Books* over the scientific validity of psychoanalysis. Crews (1993) and Grünbaum (1994) denied that psychoanalysis is scientific because it fails to meet scientific criteria of verification, criteria that even common-sense psychological explanations must satisfy:

> To warrant that a factor X (such as being insulted) is causally relevant to a kind of outcome Y (such as being angered or feeling humiliated) in a reference class C, evidence is required that the incidence of Y's in the subclass of X's is *different* from its incidence in the subclass of non-X's. . . . Absent such statistics, there is clearly insufficient ground for attributing the forgetting of negative experiences to their affective displeasure, let alone for ascribing neurotic symptoms to the repression of such experiences. (Grünbaum 1994:54; emphasis in original)

Nagel (1994a, 1994b) agreed with Grünbaum that Freud's general explanations for many psychological phenomena are suspect but saw Freud's main contribution not as the promulgation of such a general theory but as the development of a method of understanding that is based in individual interpretations and explanations. He also agreed "that psychoanalytic hypotheses are causal, and require empirical confirmation; but we differ as to the kind of evidence that is most important" (Nagel 1994b:56). The type of explanation that Nagel defended as characteristic of both commonsense psychology and psychoanalysis involves a specific understanding of particular cases based on a general interpretive framework, an understanding based on the "fitting together" of pieces of evidence in a way that elucidates how a particular result occurred rather than the demonstration that a statistical relationship exists between particular variables.

Qualitative researchers have provided numerous illustrations of how such a process approach can be used to develop causal explanations. For example, Weiss (1994) argues that

> in qualitative interview studies the demonstration of causation rests heavily on the description of a visualizable sequence of events, each event flowing into the next. . . . Quantitative studies support an assertion of causation by showing a correlation between an earlier event and a subsequent event. An analysis of data collected in a large-scale sample survey might, for example, show that there is a correlation between the level of the wife's education and the presence of a companionable marriage. In qualitative studies we would look for a process through which the wife's education or factors associated with her education express themselves in marital interaction. (p. 179)

A second example is provided by a mixed-method study of patient falls in a hospital (Morse and Tylko 1985; Morse, Tylko, and Dixon 1987) that included qualitative observations of, and interviews with, elderly patients who had fallen, focusing on how they moved around in the hospital environment and the reasons they fell. The researchers used these data to identify causes of falls, such as the use of furniture or IV poles for support, that had not been reported in previous quantitative studies. This identification was made possible by the study's focus on the process of patient ambulation and the specific events and circumstances that led to the fall rather than on attempting to correlate falls with other, previously defined variables.

Developing causal explanations in a qualitative study is not, however, an easy or straightforward task. Furthermore, there are many potential validity threats to any causal explanation, threats that will need to be addressed in the design and conduct of a study. In this, the situation of qualitative research is no different from that of quantitative research; both approaches need to identify and deal with the plausible validity threats to any proposed causal explanation. This ability to rule out plausible alternative explanations or "rival hypotheses" rather than the use of any specific methods or designs is widely seen as the fundamental characteristic of scientific inquiry in general (Popper

1959; Platt 1966; Campbell 1986:125). Thus, I turn now to how qualitative research can accomplish these tasks.

DEVELOPING CAUSAL EXPLANATIONS AND DEALING WITH THREATS TO CAUSAL INFERENCE

Miles and Huberman (1994:245–87) provide a detailed discussion of strategies for drawing and verifying conclusions in qualitative research. In what follows, I describe strategies that are particularly relevant to causal inference and causal validity in qualitative research. All of these strategies are most productive if they are informed by, and contribute to, a detailed theory (which can be inductively developed) of the causal process being investigated (Bernard 2000:55–6). Causal explanation, from a realist perspective, involves the development of a theory about the process being investigated, a process that will rarely be open to direct observation in its entirety. Such a theory assists in designing the research, identifying and interpreting specific evidence supporting or challenging the theory, and developing alternative theories that need to be ruled out to accept this theory.

I am not arguing that these methods are either thoroughly developed or foolproof. Becker (1970) argued more than thirty years ago that "these methods have all kinds of problems, some because their logic has never been worked out in the detail characteristic of quantitative methodologies; others because you gather your data in the middle of the collective life you are studying" (p. vi). My presentation of these methods is partly a call for more systematic exploration and development of such methods as strategies for causal explanation.

I have grouped these strategies into three categories. First, there are strategies that are generally associated with quantitative or variance approaches but that are nonetheless legitimate and feasible for developing and assessing causal claims in qualitative research. Second, there are strategies based on the direct observation or indirect identification of causal processes. Third, there are strategies that are useful in developing alternative explanations of the results and deciding between these.

Strategies Usually Associated with Variance Approaches

Intervention. Although some qualitative researchers see deliberate manipulation as inconsistent with qualitative approaches (e.g., Lincoln and Guba 1985), this view is by no means universal. The integration of qualitative investigation with experimental intervention has a long history in the social sciences (e.g., Milgram 1974; Trend 1978; Lundsgaarde, Fischer, and Steele 1981) and is becoming increasingly common in so-called mixed-method research (e.g., Cook, Hunt, and Murphy 2000). The issues of quantification and of experimental manipulation are independent dimensions of research design (Maxwell, Bashook, and Sandlow 1986) and are not inherently incompatible (Maxwell and Loomis 2003).

However, interventions can also be used within more traditional qualitative studies that lack a formal control group. For example, Goldenberg (1992), in a study of two students' reading progress and the effect that their teacher's expectations and behavior had on this progress, shared his interpretation of one student's failure to meet these expectations with the teacher. This resulted in a change in the teacher's behavior toward the student and a subsequent improvement in the student's reading. The intervention with the teacher and the resulting changes in her behavior and the student's progress supported Goldenberg's claim that the teacher's behavior, rather than her expectations of the student, was the primary cause of the student's progress or lack of it. The logic of this inference, although it resembles that of time-series quasi-experiments, was not simply a matter of variance theory correlation of the intervention with a change in outcome; Goldenberg provides a detailed account of the process by which the change occurred, which corroborated the identification of the teacher's behavior as the cause of the improvement in a way that a simple correlation could never do.

Furthermore, in field research, the researcher's presence is always an intervention in some ways (Maxwell 2002), and the effects of this intervention can be used to develop or test causal theories about the group or topic studied. For example, Briggs (1970), in her study of an Eskimo family, used a detailed analysis of how the family reacted to her often inappropriate behavior as an "adopted daughter" to develop her theories about the culture and dynamics of Eskimo social relations.

Comparison. While explicit comparisons (such as between intervention and control groups) for the purpose of causal inference are most common in quantitative, variance-theory research, there are numerous uses of comparison in qualitative studies, particularly in multicase or multisite studies. Miles and Huberman (1994:254) provide a list of strategies for comparison and advice on their use. "Controlled comparison" (Eggan 1954) of different societies is a longstanding practice in anthropology, and research that combines group comparison with qualitative methods is widespread in other fields as well. Such comparisons (including longitudinal comparisons and comparisons within a single setting) can address one of the main objections raised against using qualitative case studies for causal inference—their inability to explicitly address the "counterfactual" of what would have happened without the presence of the presumed cause (Shadish, Cook, and Campbell 2002:501).

In addition, single-setting qualitative studies, or interview studies of a single category of individual, often incorporate less formal comparisons that contribute to the interpretability of the case. There may be a literature on "typical" settings or individuals of the type studied that make it easier to identify the relevant causal processes in an exceptional case, or the researcher may be able to draw on her or his own experience with other cases that provide an illuminating comparison. In other instances, the participants in the setting studied may themselves have experience with other settings or with the same setting at an earlier time, and the researcher may be able to draw on this experience to identify the crucial mechanisms and the effect that these have.

For example, Regan-Smith's (1992) study of exemplary medical school teaching and its effect on student learning included only faculty who had won the Best Teacher award; from the point of view of quantitative design, this was an uncontrolled, preexperimental study. However, all of the previously mentioned forms of informal comparison were used in the research. First, there is a great deal of published information about medical school teaching, and Regan-Smith was able to use both this background and her own extensive knowledge of medical teaching to identify what it was that the teachers she studied did in their classes that was distinctive and the differences in student responses to these strategies. Second, the students Regan-Smith interviewed explicitly contrasted these teachers with others whose classes they felt were not as helpful to them.

Observation and Analysis of Process

Becker (1966), in discussing George Herbert Mead's theory of society, states that in Mead's view,

> The reality of social life is a conversation of significant symbols, in the course of which people make tentative moves and then adjust and reorient their activity in the light of the responses (real and imagined) others make to those moves. . . . Social process, then, is not an imagined interplay of invisible forces or a vector made up of the interaction of multiple social factors, but an observable process of symbolically mediated interaction. (p. 69)

However, Becker then makes a fundamental point about the observation of social processes: "Observable, yes; but not easily observable, at least not for scientific purposes" (p. 69). Dunn (1978) argues similarly that "there are still no cheap ways to deep knowledge of other persons and the causes of their actions" (p. 171).

Observing (and analyzing) social processes is hard work, requiring both substantial time and methodological skill. Most books on qualitative methods discuss the skills involved in such observation (a particularly detailed example is Emerson, Fretz, and Shaw 1995) although usually without directly relating these to causal inference. I see three strategies as particularly useful in this latter task: intensive, relatively long-term involvement; collecting "rich" data; and using narrative or "connecting" approaches to analysis.

Intensive, long-term involvement. Becker and Geer (1957) claim that long-term participant observation provides more complete data about specific situations and events than any other method. Not only does it provide more, and more different kinds, of data, but the data are more direct and less dependent on inference. Repeated observations and interviews and sustained presence of the researcher in the setting studied can give a clearer picture of causal processes, as well as helping to rule out spurious associations and premature theories. They also allow a much greater opportunity to develop and test causal hypotheses during the course of the research. Finally, such involvement is usually essential to the following strategy—the collection of rich data.

For example, Becker (1970:49–51) argues that his lengthy participant observation research with medical students not only allowed him to get beyond their public expressions of cynicism about a medical career and uncover an idealistic perspective but also enabled him to understand the processes by which these different views were expressed in different social situations and how students dealt with the conflicts between these perspectives.

Rich data. Rich data (often, and erroneously, called "thick description"; see Maxwell 1992:288–9) are data that are detailed and varied enough that they provide a full and revealing picture of what is going on and of the processes involved (Becker 1970:51ff.). In the same way that a detailed, chronological description of a physical process (e.g., of waves washing away a sand castle or the observations of patient falls described above) often reveals many of the causal mechanisms at work, a similar description of a social setting or event can reveal many of the causal processes taking place. In a social setting, some of these processes are mental rather than physical and are not directly observable, but they can often be inferred from behavior (including speech).

Regan-Smith's (1992) study of medical school teaching, described above, relied on lengthy observation and detailed field notes recording the teacher's actions in classes and students' reactions to these. In addition, she used what might be called indirect observation of causal processes through interviews: the students explained in detail not only what it was that the exemplary teachers did that increased their learning but also how and why these teaching methods were beneficial. (Indirect observation is, of course, subject to its own validity threats.)

In addition, Becker (1970) argues that rich data "counter the twin dangers of respondent duplicity and observer bias by making it difficult for respondents to produce data that uniformly support a mistaken conclusion, just as they make it difficult for the observer to restrict his observations so that he sees only what supports his prejudices and expectations" (p. 53). In both cases, rich data provide a test of one's developing theories, as well as a basis for generating, developing, and supporting such theories.

Narrative and connecting analysis. Causal explanation is dependent on the analysis strategy used as well as the data collected. The distinction between two types of qualitative analysis, one using categorization and comparison and the other identifying actual connections between events and processes in a specific context, is becoming increasingly recognized.

Smith (1979) provides a particularly clear explanation of these:

> I usually start . . . at the beginning of the notes. I read along and seem to engage in two kinds of processes—comparing and contrasting, and looking for antecedents and consequences. The essence of concept formation [the first process] is . . . "How are they alike, and how are they different?" The similar things are grouped and given a label that highlights their similarity. . . . In time, these similarities and differences come to represent clusters of concepts, which then organize themselves into more abstract categories and eventually into hierarchical taxonomies.
>
> Concurrently, a related but different process is occurring. . . . The conscious search for the consequences of social items . . . seemed to flesh out a complex systemic view and a concern for process, the flow of events over time. (p. 338)

Similar distinctions are made by other researchers. Seidman (1991:91ff.) describes two main strategies in the analysis of interviews: the categorization of interview material through coding and thematic analysis and the creation of several different types of narratives, which he calls "profiles" and "vignettes."

Patton (1990) distinguishes between content analysis and case studies, Weiss (1994) between "issue-focused" and "case-focused" analysis, Dey (1993) between "categorization" and "linking," and Maxwell and Miller (n.d.; Maxwell 1996) between "categorizing" and "connecting" strategies.

These distinctions are closely related to the distinction between variance and process approaches discussed above. While categorization in qualitative research is quite different from categorization in quantitative research, for causal explanation its value is primarily comparative, identifying differences and similarities and relating these to other differences and similarities. (Ragin's [1987] integration of case- and variable-oriented approaches, using Boolean algebra, is one example of such a strategy.) A different type of analysis is needed for processual explanation—one that elucidates the actual connections between events and the complex interaction of causal processes in a specific context. Narrative and case analysis can accomplish this; although many narratives and cases are not explicitly concerned with causality, the tools they use can be applied to the purpose of elucidating causal connections. Similarly, what Erickson (1992) calls ethnographic microanalysis of interaction, "begins by considering whole events, continues by analytically decomposing them into smaller fragments, and then concludes by *recomposing* them into wholes. . . . [This process] returns them to a level of sequentially connected social action" (p. 217).

Agar (1991:181) describes a study in which the researchers, using a computer program called The Ethnograph to analyze interviews with historians about how they worked, provided a categorizing segment-and-sort analysis that decontextualized their data and allowed only general description and comparative statements about the historians. This analysis failed to meet the client's need for a connecting analysis that elucidated how individual historians thought about their work as they did it and the influence of their ideas on their work. Similarly, Abbott (1992) gives a detailed account of how a reliance on variance theory distorts sociologists' causal analyses of cases and argues for a more systematic and rigorous use of narrative and process analysis for causal explanation.

However, Sayer (1992:259–62) notes that narratives have specific dangers. They tend to underspecify causality in the processes they describe and often miss the distinction between chronology and causality; their linear, chronological structure tends to obscure the complex interaction of causal influences; their persuasive "storytelling" can avoid problematizing their interpretations and deflect criticism. Researchers need to be aware of these issues and address them in drawing conclusions.

Developing and Assessing Alternative Explanations

The three preceding strategies are most useful for developing causal explanations; they do not usually address the problem of generating plausible alternatives to these explanations, deciding between two or more explanations that are consistent with the data, or testing an explanation against possible validity threats. Numerous specific ways in which validity threats can be assessed or rendered implausible in qualitative research are given by Becker (1970), Kidder (1981), Lincoln and Guba (1985), Patton (1990), Miles and Huberman (1994), and Maxwell (1996). I discuss four strategies that are particularly useful in dealing with causal validity: the "modus operandi" approach, searching for discrepant evidence, triangulation, and "member checks."

The modus operandi approach. This strategy, originally proposed by Scriven (1974), resembles the approach of a detective trying to solve a crime, an inspector trying to determine the cause of an airplane crash, or a physician attempting to diagnose a patient's illness. Basically, rather than trying to deal with validity threats as variables, by holding them constant in some fashion or attempting to statistically "control for" their effects, the modus operandi method deals with them as processes. The researcher tries to identify the potential validity threats, or alternative explanations, that would threaten the proposed explanation and then searches for "clues" (what Scriven called the "signatures" of particular causes) as to whether these processes were operating and if they had the causal influence hypothesized.

Consider a researcher who is concerned that some of her interviews with teachers had been influenced by their principal's well-known views on the topics being investigated rather than expressing their actual beliefs. Instead of eliminating teachers with this principal from her sample, the researcher could consider what internal evidence could distinguish between these two causal processes (such as a change in voice or behavior when these issues were discussed) and look for such evidence in her interviews or other data. She could also try to find ways to investigate this influence directly through subsequent interviews.

The main difficulty in using this strategy in qualitative research is coming up with the most important alternative explanations and specifying their operation in enough detail that their consequences can be predicted. As Miles and Huberman (1994) note, "it's usually difficult for anyone who has spent weeks or months coming up with one explanation to get involved seriously with another one" (p. 275). Feedback from others is particularly useful here, as is the next strategy, looking for discrepant evidence and negative cases.

Searching for discrepant evidence and negative cases. The use of the modus operandi approach depends on the researcher's willingness to search for evidence that might challenge the explanation she has developed. There is a strong and often unconscious tendency for researchers to notice supporting instances and ignore ones that do not fit their prior conclusions (Shweder 1980; Miles and Huberman 1994:263). Identifying and analyzing discrepant data and negative cases is a key part of assessing a proposed conclusion. Instances that cannot be accounted for by a particular interpretation or explanation can point out important defects in that account, although the supposed discrepant evidence must itself be assessed for validity threats. There are times when an apparently discrepant instance is not persuasive, as when the interpretation of the discrepant data is itself in doubt. Physics is full of examples of supposedly "disconfirming" experimental evidence that was later found to be flawed. The basic principle here is to rigorously examine both the supporting and discrepant data to assess whether it is more plausible to retain or modify the conclusion.

One technique that supports this goal has been termed "quasi-statistics" by Becker (1970:81–2). This refers to the use of simple numerical results that can be readily derived from the data. A claim that a particular phenomenon is typical, rare, or prevalent in the setting or population studied is an inherently quantitative claim and requires some quantitative support. Quasi-statistics can also be used to assess the amount of evidence that bears on a particular conclusion or threat, from how many different sources they were obtained, and how many discrepant instances exist. This strategy is used effectively in a classic participant-observation study of medical students (Becker et al. 1961), which presents more than fifty tables and graphs of the amount and distribution of qualitative observational and interview data supporting and challenging their conclusions.

Triangulation. Triangulation—collecting information from a diverse range of individuals and settings or using a variety of methods—reduces the risk of systematic biases because of a specific source or method (Denzin 1970) and "puts the researcher in a frame of mind to regard his or her own material critically" (Fielding and Fielding 1986:24). For example, Regan-Smith (1992) did not rely entirely on interviews with medical students for her conclusions about how exemplary teaching helped students to learn; her explanations were corroborated by her own experiences as a participant-observer in these teachers' classes and by the teachers' explanations of why they taught the way they did. In addition, she deliberately interviewed students with a wide variety of characteristics and attitudes to ensure that she was not hearing from only one segment of the students.

However, Fielding and Fielding (1986:30–5) point out that triangulation does not automatically increase validity. First, the methods that are triangulated may have the same biases and thus provide only a false sense of security. For example, interviews, questionnaires, and documents are all vulnerable to self-report bias. Second, researchers may consciously or unconsciously select those methods or data sources that would tend to support their preferred conclusions or emphasize those data that "stand out" by their vividness or compatibility with their theories; both of these are examples of what is usually called "researcher bias." Fielding and Fielding emphasize the fallibility of any particular method or data and argue for triangulating in terms of validity threats. In the final analysis, validity threats are ruled out by evidence, not methods; methods need to be selected for their potential for producing evidence that will adequately assess these threats.

Member checks. Soliciting feedback from others is an extremely useful strategy for identifying validity threats, your own biases and assumptions, and flaws in your logic or methods. One particular sort of feedback is systematically soliciting responses to one's data and conclusions from the people you are studying, a process known as "member checks" (Lincoln and Guba 1985). This not only serves as a check on misinterpretations of their perspectives and meanings but also can provide alternative interpretations of observed events and processes. Regan-Smith (1992) used this technique in her study of medical school teaching, conducting informal interviews with the students she studied to make sure that she understood what they were trying to tell her and whether her conclusions made sense to them.

However, Bloor (1983) warns that "members' reactions . . . are not immaculately produced but rather are shaped and constrained by the circumstances of their production" (p. 171). He describes a number of problems that he encountered in using this technique, including members' lack of interest, their difficulty in juxtaposing their own understanding to that of the researcher, the influence of the member's relationship with the researcher, the member's ulterior purposes, and the member's need to reach consensus with the researcher and other conversational constraints. These validity threats must themselves be evaluated and taken into account.

CONCLUSION

The strategies described above are ones that many conscientious qualitative researchers use regularly, although they are rarely described explicitly in empirical research publications. I argue that they can be legitimately applied to the development and testing of causal explanations. The identification of causal influences through qualitative methods involves its own pitfalls and validity threats, however, as described above. In addition, Patton (1990) warns that

> one of the biggest dangers for evaluators doing qualitative analysis is that, when they begin to make interpretations about causes, consequences, and relationships, they fall back on the linear assumptions of quantitative analysis and begin to specify isolated variables that are mechanically linked together out of context. . . .Simple statements of linear relationships may be more distorting than illuminating. (p. 423)

Miles and Huberman (1984) emphasize that qualitative research aims at understanding local, contextualized causality rather than "general laws" linking isolated variables and can only develop general models on the basis of valid site-specific explanations.

Field researchers are often interested in knowing what goes on in the settings they study, not only to advance their theoretical understanding of these settings but also because ultimately, they want to contribute to their improvement. To accomplish either of these tasks, they must be able to identify the causal processes that are occurring in these settings and to distinguish valid explanations for outcomes from spurious ones. Philosophical and methodological prohibitions against using qualitative approaches for this task are unjustified. By employing available strategies for understanding causal processes and addressing validity threats to causal conclusions, qualitative researchers can, in many circumstances, provide causal explanations.

REFERENCES

Abbott, A. 1992. What do cases do? Some notes on activity in sociological analysis. In *What is a case? Exploring the foundations of social inquiry*, edited by C. C. Ragin and H. S. Becker, 53–82. Cambridge, UK: Cambridge University Press.

Agar, M. 1991. The right brain strikes back. In *Using computers in qualitative research*, edited by N. G. Fielding and R. M. Lee, 181–94. Newbury Park, CA: Sage.

Archer, M., R. Bhaskar, A. Collier, T. Lawson, and A. Norrie. 1998. *Critical realism: Essential readings*. London: Routledge.

Baert, P. 1998. *Social theory in the twentieth century*. New York: New York University Press.

Becker, H. S. 1966. The life history and the scientific mosaic. Introduction to *The Jack-roller*, by C. Shaw. Chicago: University of Chicago Press. (Reprinted in H. Becker, *Sociological work: Method and substance.* New Brunswick, NJ: Transaction Books, 1970.)

———. 1970. Fieldwork evidence. In *Sociological work: Method and substance*, edited by H. S. Becker, 39–62. New Brunswick, NJ: Transaction Books.

———. 1986. *Writing for social scientists: How to start and finish your thesis, book, or article*. Chicago: University of Chicago Press.

Becker, H. S., and B. Geer. 1957. Participant observation and interviewing: A comparison. *Human Organization* 16 (3): 28–32.

Becker, H. S., B. Geer, E. C. Hughes, and A. L. Strauss. 1977. *Boys in white: Student culture in medical school*. New Brunswick, NJ: Transaction Books. (Originally published by the University of Chicago Press, 1961.)

Bernard, H. R. 2000. *Social research methods: Qualitative and quantitative approaches*. Thousand Oaks, CA: Sage.

Blalock, H. M. 1961. *Causal inferences in nonexperimental research*. New York: W. W. Norton.

Bloor, M. J. 1983. Notes on member validation. In *Contemporary field research: A collection of readings*, edited by R. M. Emerson, 156–72. Prospect Heights, IL: Waveland.

Blumer, H. 1956. Sociological analysis and the "variable." *American Sociological Review* 22:683–90. (Reprinted in H. Blumer, *Symbolic interactionism: Perspective and method*, 127–39. Berkeley: University of California Press, 1969.)

Briggs, J. 1970. *Never in anger: Portrait of an Eskimo family*. Cambridge: Harvard University Press.

Britan, G. M. 1978. Experimental and contextual models of program evaluation. *Evaluation and Program Planning* 1:229–34.

Campbell, D. T. 1986. Science's social system of validity-enhancing collective belief change and the problems of the social sciences. In *Metatheory in social science: Pluralisms and subjectivities*, edited by D. W. Fiske and R. A. Shweder, 108–35. Chicago: University of Chicago Press.

Cook, T. D., H. D. Hunt, and R. F. Murphy. 2000. Comer's school development program in Chicago: A theory-based evaluation. *American Educational Research Journal* 37:535–97.

Crews, F. 1993. The unknown Freud. *New York Review of Books*, November 18.

Davidson, D. 1980. *Essays on actions and events.* Oxford: Clarendon.

———. 1993. Thinking causes. In *Mental causation*, edited by J. Heil and A. Mele, 3–17. Oxford: Clarendon.

Denzin, N. K. 1970. *The research act*. Chicago: Aldine.

Denzin, N. K., and Y. S. Lincoln, eds. 2000a. *Handbook of qualitative research*, 2nd ed. Thousand Oaks, CA: Sage.

———. 2000b. Introduction: The discipline and practice of qualitative research. In *Handbook of qualitative research*, 2nd ed., edited by N. K. Denzin and Y. S. Lincoln, 1–28. Thousand Oaks, CA: Sage.

Dey, I. 1993. *Qualitative data analysis: A user-friendly guide for social scientists*. London: Routledge.

Dunn, J. 1978. Practising history and social science on "realist" assumptions. In *Action and interpretation: Studies in the philosophy of the social sciences*, edited by C. Hookway and P. Pettit, 145–75. Cambridge, UK: Cambridge University Press.

Eggan, F. 1954. Social anthropology and the method of controlled comparison. *American Anthropologist* 56:743–63.

Emerson, R. M., R. I. Fretz, and L. L. Shaw. 1995. *Writing ethnographic fieldnotes*. Chicago: University of Chicago Press.

Erickson, F. 1992. Ethnographic microanalysis of interaction. In *The handbook of qualitative research in education*, edited by M. D. LeCompte, W. L. Millroy, and J. Preissle, 201–25. San Diego, CA: Academic Press.

Fielding, N. G., and J. L. Fielding. 1986. *Linking data*. Thousand Oaks, CA: Sage.

Goldenberg, C. 1992. The limits of expectations: A case for case knowledge of teacher expectancy effects. *American Educational Research Journal* 29 (3): 517–44.

Gould, S. J. 1989. *Wonderful life: The Burgess Shale and the nature of history*. New York: W. W. Norton.

Grünbaum, A. 1994. Freud's permanent revolution: An exchange. *New York Review of Books*, August 11, 54–5.

Guba, E. G., and Y. S. Lincoln. 1989. *Fourth generation evaluation*. Newbury Park, CA: Sage.

Hempel, C. G., and P. Oppenheim. 1948. Studies in the logic of explanation. *Philosophy of Science* 15:135–75.

Huberman, A. M., and M. B. Miles. 1985. Assessing local causality in qualitative research. In *The self in social inquiry: Researching methods*, edited by D. N. Berg and K. K. Smith, 351–81. Thousand Oaks, CA: Sage.

Kidder, L. H. 1981. Qualitative research and quasi-experimental frameworks. In *Scientific inquiry and the social sciences*, edited by M. B. Brewer and B. E. Collins, 226–56. San Francisco: Jossey-Bass.

Layder, D. 1990. *The realist image in social science*. New York: St. Martins.

Light, R. J., J. Singer, and J. Willett. 1990. *By design: Conducting research on higher education*. Cambridge, MA: Harvard University Press.

Lincoln, Y. S., and E. G. Guba. 1985. *Naturalistic inquiry*. Newbury Park, CA: Sage.

Lofland, J., and L. Lofland. 1984. *Analyzing social settings: A guide to qualitative observation and analysis*, 2nd ed. Belmont, CA: Wadsworth.

Lundsgaarde, H. P., P. J. Fischer, and D. J. Steele. 1981. *Human problems in computerized medicine*. Number 13. Lawrence: University of Kansas Publications in Anthropology.

Maxwell, J. A. 1992. Understanding and validity in qualitative research. *Harvard Educational Review* 62:279–300.

———. 1995. Diversity and methodology in a changing world. *Pedagogía* 30:32–40.

———. 1996. *Qualitative research design: An interactive approach*. Thousand Oaks, CA: Sage.

———. 1999. A realist/postmodern concept of culture. In *Anthropological theory in North America*, edited by E. L. Cerroni-Long, 143–73. Westport, CT: Bergin & Garvey.

———. 2002. Realism and the role of the researcher in qualitative psychology. In *The role of the researcher in qualitative psychology*, edited by M. Kiegelmann, 11–30. Tuebingen, Germany: Verlag Ingeborg Huber.

———. In 2004a. Causal explanation, qualitative research, and scientific inquiry in education. *Educational Researcher* 33(2)3–11.

———. In 2004b. Re-emergent scientism, postmodernism, and dialogue across differences. Special issue of *Qualitative Inquiry* 10(1):35–41.

Maxwell, J. A., P. G. Bashook, and L. J. Sandlow. 1986. Combining ethnographic and experimental methods in educational evaluation: A case study. In *Educational evaluation: Ethnography in theory, practice, and politics*, edited by D. M. Fetterman and M. A. Pitman, 121–43. Thousand Oaks, CA: Sage.

Maxwell, J. A., and D. Loomis. 2002. Mixed methods design: An alternative approach. In *Handbook of mixed methods in social and behavioral research*, edited by A. Tashakkori and C. Teddlie, 241–71. Thousand Oaks, CA: Sage.

Maxwell, J. A., and B. Miller. n.d. Categorizing and connecting as components of qualitative data analysis. Unpublished manuscript.

McGinn, C. 1991. Conceptual causation: Some elementary reflections. *Mind* 100 (4): 573–86. (Reprinted in C. McGinn, *Knowledge and reality*, 152–67. Oxford: Clarendon Press, 1999.)

Miles, M. B., and A. M. Huberman. 1984. *Qualitative data analysis: A sourcebook of new methods*. Newbury Park, CA: Sage.

———. 1994. *Qualitative data analysis: An expanded sourcebook*. Thousand Oaks, CA: Sage.

Milgram, S. 1974. *Obedience to authority: An experimental view*. New York: Harper & Row.

Mohr, L. B. 1982. *Explaining organizational behavior*. San Francisco: Jossey-Bass.

———. 1995. *Impact analysis for program evaluation*, 2nd ed. Thousand Oaks, CA: Sage.

———. 1996. *The causes of human behavior: Implications for theory and method in the social sciences*. Ann Arbor: University of Michigan Press.

Morse, J. M., and S. J. Tylko. 1985. The use of qualitative methods in a study examining patient falls. Paper presented at the annual meeting of the Society for Applied Anthropology, Washington, DC.

Morse, J. M., S. J. Tylko, and H. A. Dixon. 1987. Characteristics of the fall-prone patient. *The Gerontologist* 27 (4): 516–22.

Nagel, T. 1994a. Freud's Permanent Revolution. *New York Review of Books*, May 12, 34–8.

———. 1994b. Freud's Permanent Revolution: An Exchange. *New York Review of Books*, August 11, 55–6.

Patton, M. Q. 1990. *Qualitative evaluation and research methods*, 2nd ed. Thousand Oaks, CA: Sage.

Pawson, R., and N. Tilley. 1997. *Realistic evaluation*. London: Sage.

Platt, J. 1966. Strong inference. *Science* 146:347–53.

Popper, K. 1959. *The logic of scientific discovery*. New York: Basic Books.

Putnam, H. 1990. *Realism with a human face*. Cambridge, MA: Harvard University Press.

Ragin, C. C. 1987. *The comparative method: Moving beyond qualitative and quantitative strategies*. Berkeley: University of California Press.

Regan-Smith, M. G. 1992. The teaching of basic science in medical school: The students' perspective. Unpublished doctoral dissertation, Harvard Graduate School of Education.

Rossi, P. H., and R. A. Berk. 1991. A guide to evaluation research theory and practice. In *Evaluation and effective risk communications: Workshop proceedings*, edited by A. Fisher, M. Pavlova, and V. Covello, 201–54. Washington, DC: Interagency Task Force on Environmental Cancer and Heart and Lung Disease.

Salmon, W. C. 1984. *Scientific explanation and the causal structure of the world*. Princeton, NJ: Princeton University Press.

———. 1989. Four decades of scientific explanation. In *Scientific explanation*, edited by P. Kitcher and W. C. Salmon, 3–219. Minneapolis: University of Minnesota Press.

———. 1998. *Causality and explanation*. New York: Oxford University Press.

Sayer, A. 1992. *Method in social science: A realist approach*, 2nd ed. London: Routledge.

Scriven, M. 1974. Evaluation perspectives and procedures. In *Evaluation in education—Current perspectives*, edited by W. J. Popham, 68–84. Berkeley, CA: McCutchan.

Seidman, I. E. 1991. *Interviewing as qualitative research: A guide for researchers in education and the social sciences*. New York: Teachers College Press.

Shadish, W. R., T. D. Cook, and D. T. Campbell. 2002. *Experimental and quasi-experimental designs for generalized causal inference*. Boston: Houghton Mifflin.

Shavelson, R. J., and L. Towne, eds. 2002. *Scientific research in education*. Washington, DC: National Academy Press.

Shweder, R. A., ed. 1980. *Fallible judgment in behavioral research*. San Francisco: Jossey-Bass.

Smith, L. 1979. An evolving logic of participant observation, educational ethnography, and other case studies. *Review of Research in Education* 6:316–77.

Trend, M. G. 1978. On the reconciliation of qualitative and quantitative analyses: A case study. *Human Organization* 37:345–54. (Reprinted in T. D. Cook and C. S. Reichardt, eds., *Qualitative and quantitative methods in evaluation research.* Newbury Park, CA: Sage, 1979.)

Vidich, A. J., and S. M. Lyman. 2000. Qualitative methods: Their history in sociology and anthropology. In *Handbook of qualitative research*, 2nd ed., edited by N. K. Denzin and Y. S. Lincoln, 37–84. Thousand Oaks, CA: Sage.

Weiss, R. S. 1994. *Learning from strangers: The art and method of qualitative interviewing*. New York: Free Press.

Yin, R. 1993. *Applications of case study research.* Thousand Oaks, CA: Sage.

JOSEPH A. MAXWELL is an associate professor in the Graduate School of Education at George Mason University, where he teaches courses on research design and methods; he has also worked extensively in applied settings. He is the author of Qualitative Research Design: An Interactive Approach *(Sage, 1996); "Causal Explanation, Qualitative Research, and Scientific Inquiry in Education" (in* Educational Researcher*, March 2004); and (with Diane Loomis) "Mixed Methods Design: An Alternative Approach" (in the* Handbook of Mixed Methods in Social and Behavioral Research*, Sage, 2003), as well as papers on research methods, sociocultural theory, Native American society, and medical education. He has presented seminars and workshops on teaching qualitative research methods and on using qualitative methods in various applied fields and has been an invited speaker at conferences and universities in the continental United States, Puerto Rico, Europe, and China. He has a Ph.D. in anthropology from the University of Chicago.*