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Bridging Positivist and Interpretivist Approaches to Qualitative Methods

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The usual juxtaposition of qualitative research against quantitative research makes it easy to miss the fact that qualitative research itself encompasses at least two traditions: positivist and interpretivist. Positivist work seeks to identify qualitative data with propositions that can then be tested or identified in other cases, while interpretive work seeks to combine those data into systems of belief whose manifestations are specific to a case. In this paper, I argue that discovering causal relationships is the province of positivist research, while discovering causal mechanisms is the province of interpretivists. I explain why absolutist claims for one or the other approach are mistaken, and argue that the combination of both makes more sense. Finally, I offer suggestions for combinations of positivist and interpretive work, both at the level of thought experiment and in actual data collection and analysis. Throughout, I draw my examples from recent studies of poverty, a field in which a small but distinguished tradition of qualitative studies of the poor has been joined by a growing body of both positivist and interpretive work.

From case studies to econometric analysis, policy research has a long tradition of employing both qualitative and quantitative methods, but the usual juxtaposition of qualitative research against quantitative research makes it easy to miss the fact that qualitative research itself encompasses a multitude of different approaches. Qualitative work can be positivist: It can attempt to document practices that lead consistently to one set of outcomes rather than another, to identify characteristics that commonly are related to some policy problem, or to find strategic patterns that hold across different venues and with different actors. Qualitative work also can be interpretivist: It can seek to understand what general concepts like "poverty" or "race" mean in their specific operation, to uncover the conscious and unconscious explanations people have for what they do or believe, or to capture and reproduce a particular time, culture, or place so that actions people take become intelligible.¹

The differences between interpretive and positivist work easily can be seen by way of a comparison. Kathryn Edin (1991) and Carol Stack (1974) both have done extensive research on the coping strategies of welfare recipients. Based on extended, repeated interviews with welfare recipients, both studies describe the difficulty mothers have in supporting themselves on welfare payments, the frustration they feel, and the many methods they develop to stretch an insufficient income, but Edin and Stack make different uses of this information. Edin's approach is primarily positivist. She observes what welfare recipients do, which is to solicit contributions from family and to work in either undocumented or underground work to meet their basic needs. She suggests that it is reasonable to assume that other recipients cope in the same way as her respondents, given similar gaps between welfare payments and costs of living in other locations across the country. Her argument's force, in fact, depends on the generalizability of this pattern. If what she found indeed is repeated for other welfare recipients in other locales, her argument for reform is strengthened. Stack, by contrast, has an

interpretivist approach. She documents one type of coping strategy—kin-based support networks—and explains how practices of exchanging food and clothing, shared childrearing, and fictive kin relationships all reinforce a sense of mutual obligation, even when such obligations are quite costly to particular individuals.² Her argument derives its force from the accuracy with which she has described these practices, and from its ability to account for the range of behaviors and beliefs in the community she describes. Unlike Edin, however, Stack makes no claims as to the generality of the specific practices—one kind of child exchange, one kind of welfare sharing—that she identifies as elements of kin-based support networks. Such networks could be formed around different practices in other locations; her argument is about how kin-based support works, not about what it comprises.

The differences in interpretivist and positivist qualitative work thus are differences in the questions one asks of the data and the types of conclusions one wishes to draw. Both forms of qualitative work look for details about preferences, motivations, and actions that are not easily made numeric. Positivist work, however, seeks to identify those details with propositions that then can be tested or identified in other cases, while interpretive work seeks to combine those details into systems of belief whose manifestations are specific to a case. While both in the end can comment about general principles or relationships, positivist work does so by identifying general patterns, while interpretivist work does so by showing how the general pattern looks in practice.

These differences in the use and the conclusions of interpretive and positivist work have led purists in both camps to assert that these two systems of inference cannot be combined. In their book, *Designing Social Inquiry: Scientific Inference in Qualitative Research*, Gary King, Robert Keohane, and Sidney Verba (1994, pp. 40, 42) suggest that interpretivist work can “help us ask the right questions and even give us additional confidence in our conclusions. But only with the methods of scientific inference will we be able to evaluate the hypothesis and see whether it is correct.” For their part, Yvonna Lincoln and Egon Guba (1985, p. 33) assert in their book, *Naturalistic Inquiry*, that the interpretive approach, or what they call postpositivism, is “an entirely new paradigm, not reconcilable with the old.... What is needed is a transformation, not an add-on. That the world is round cannot be added to the idea that the world is flat.” Perhaps the only assertion on which all of these authors would agree is one that King, Keohane, and Verba (1995, p. 479) advance: that only when “there is a *single* unified logic of inference ... [is it] possible effectively to combine different methods” (emphasis added).

Such an approach, however, runs the risk of subordinating understanding to dogma. In fact, it is precisely because the logics of inference are different, and suited for answering different questions, that research combining both logics is effective. Positivist work can identify the existence of causal relationships that are present in data, with some degree of probability. What it cannot do is to explain how the mechanism implied by a particular causal relationship works. Interpretivist work, by contrast, can produce detailed examinations of causal mechanisms in the specific case, explaining how particular variables interact. Without positivist work, however, one does not know how widespread the existence of similar cases might be—a question that often is of special interest to students of policy.

This combination does more than add substantive content that neither approach could create alone. As a consequence of creating more clarity about

mechanism and relationship, the combination of positivist and interpretivist work also helps to correct for biases that each approach suffers from separately. In the absence of knowledge about how a mechanism operates, positivist work, which must "tell a story" about the significance of the causal relationships it identifies, easily falls back on intuitively plausible assumptions about the case at hand. When the case is one about which the researcher has intimate knowledge, such assumptions often are accurate, but when the researcher is an outsider—and, in the case of policy research, this often is the situation—the researcher's sense of the plausible can be tainted by prejudice, ignorance, or mistaken inferences. By contrast, interpretive work reconstructs categories that are organic to the context it studies, and thus is much less likely to be led astray by preconceived notions that stem from inappropriate generalizations.³ But here as well there is a bias. Having uncovered specific manifestations of a general category, interpretivists can be led to two corresponding mistakes. One is to assume that the general case is always made specific in the ways that they describe, and thus to suggested solutions that are not appropriate for cases other than their own. The other error is to appreciate the complexity of the causal mechanism too much, and thus to forswear any possibility of policy reform that does not address all the layers of a particular problem.

In the first section of this paper, I explain why causal relationships are peculiarly the province of positivist research, while causal mechanisms are peculiarly the province of interpretivists.⁴ I explain why the absolutist claims of preference for one or the other approach are mistaken, and argue instead that they should be seen as complements. Finally, I offer suggestions for combinations of positivist and interpretive work, both at the level of thought experiment and in actual data collection and analysis. Throughout, I draw my examples from recent studies of poverty, a field in which a small but distinguished tradition of qualitative studies of the poor has been joined by a growing body of both positivist and interpretivist work. The problems of attaching this policy research to policy solutions are particularly glaring, in a period in which studies of "who the poor are" have led to calls for policies as diverse as the denial of benefits to unmarried mothers and the government provision of minimum levels of child support. Better understanding of the potential and the limitations of different kinds of policy research is unlikely to calm ideological disputes over poverty, but hopefully it can lead to better accounts of how the facts of poverty should be understood.

Of Relationships and Mechanisms

The difference between a causal relationship and a causal mechanism is hard to describe, in part because it is like looking at the same picture in two different ways. Causal relationships are about the systematic conjunction of two factors, one of which, all things being equal, is argued to follow logically from the other. As an example, consider the fact that people who have prior work experience usually find it easier to get work than people who don't have prior work experience. Obviously, not everyone who is currently employed also was employed previously in a different job, but the two conditions commonly and plausibly are found together, across a wide variety of occupations and education levels. Persons similar in most other respects to other applicants usually will find it harder to get a job if they never have been employed and the other applicants

have been. The more consistently this is true, the more evidence there is for the existence of a causal relationship.

A causal mechanism, by contrast, explains why previous work experience and present employment should be related. Perhaps employers look for previous work experience because that serves as a sign that this applicant has training in the specific task required, or perhaps previous work experience signals work habits—people who have worked are more likely to know they need to come to work on time and to be polite to customers and supervisors. Sometimes a mechanism can be broken down into finer gradations of causal relationships. One could look at whether previous work experience, from a job totally unrelated to one's present employment, was related systematically to employability, but sometimes mechanisms are not divisible. Employers may use previous work experience as a sort of folk wisdom, as something they believe without much reason. When interviewed, they may justify this habit in several different ways; but the researcher observes that even when those justifications break down, employers still use previous work experience as their rule of thumb.

From this description, it should be evident that causal relationships and causal mechanisms do not exist independently of each other. If causality exists, it must include both a "what" and a "how"—a relationship and a mechanism.⁵ One must be able to identify discrete, even if interdependent, factors that are causes or effects; one also must be able to explain why these factors are more than simple correlation. For the purposes of policymaking, the necessity of identifying both is even more compelling. Without establishing a causal relationship, one does not know which factors should be addressed by policy; without establishing the mechanism, one will not understand how to address those factors.

Expanding upon the example of previous work experience and current employment can serve to show the importance of identifying both relationships and mechanisms. Recent quantitative work on welfare receipt has emphasized the importance of understanding welfare spells—a pattern of welfare use in which recipients cycle on and off between welfare and self-sustaining low-income work. Along with race, education, marital status, and disability status, the relationship of previous work experience to the end of a welfare spell is particularly significant (Bane & Ellwood, 1994, pp. 40–43). Work experience matters because of its relationship to employability; increased earnings through employment account for anything from 25% to 40% of all exits from welfare (Bane & Ellwood, 1994, pp. 44–60).

Such a finding is significant. Although it confirms something intrinsically plausible, finding that the relationship persists over time and within a large sample lends substance to plausibility. Yet it also is possible to see where qualitative work could add important information to the bare-bones finding of that quantitative work. Everyone can agree that previous work experience plausibly is linked to employability, but there are many plausible stories embedded in that conclusion. As we have seen, previous work experience may be a sign to employers of training or work habits. It also may suggest something about the employee's motivation and self-confidence. Perhaps people who have worked before want to work, whereas people who have not worked are lazy; or perhaps people who never have worked fear they are unqualified, and thus fail to apply for work they could get. Work experience also might not matter at all in and of itself, but instead because it is correlated with something else that employers use for hiring: recommendations from current employees. People who have worked

before are more likely to have friends who currently are working, and thus are more likely to be hired.

Some of these possibilities might be reasonably easy to assess through survey work. One could add a question on a quantitative survey asking how workers found their current employment, but many others would be difficult to determine without using qualitative methods—intensive, open-ended interviewing, participant observation, and document analysis. Without conversations with recipients about the way they look for work, the jobs they apply for, and the work they eventually find—and conversations with employers about the people they hire—it is difficult to understand what is meant by a single variable like work experience. At times, the complexities of these relationships simply are too difficult to disentangle in a survey questionnaire. In others, one might not even be able to identify what some of the possible relationships are (and thus what survey questions to ask) without help from the people one is studying. Finally, without observing behavior as well as asking questions, one could not uncover either conscious or unconscious deceit.

Once the qualitative data come, however, they can be used in many different ways. The first and most obvious way to use the data is positivist—taking the data themselves as observations, trying to figure out which pieces of information are associated, and then evaluating the strength of the association by thinking through counterfactuals and problems of reliability and representativeness. For instance, interviews with recipients about how and where they look for work might uncover the fact that recipients without previous experience looked in the classified ads, while recipients who had previous experience found jobs through former coworkers. The researcher would take these associations and see if they held in different subgroups of workers. What about those welfare recipients with previous experience who nevertheless used ads rather than contacts? Do they still find jobs? Do they find them faster than never-employed recipients who use the ads? The researcher also would wonder whether the answers that people gave were reliable. Is there something about the particular economic situation at the time, the rapport the researcher had with respondents, or anything else that might make the findings out of the ordinary? Finally, the researcher would want to know whether the recipients questioned were likely to be representative of all welfare recipients. For instance, perhaps all interviewing was done in a one-factory town; in that case, it would not be surprising that networks of coworkers were important, and it would be hard to argue that similar networks would matter as much in places where the supply of jobs was more diverse.⁶

This method of finding causal relationships has several advantages. One is that it leads the researcher to think in terms of plausible causes. By evaluating any particular hypothesis in the context of the universe of possible causes, the researcher is kept from settling on one alternative too quickly. As a result, this approach also leads to a better understanding of general phenomena. By thinking about the many alternative explanations, one can get a better sense of the factors likely to cause a certain outcome, even if that outcome did not occur or was caused by something else on one particular occasion. A third advantage is that it gives some method for evaluating the strength of observed associations. By trying to figure out whether a particular factor is a likely or unlikely cause, one gains a sense of whether the particular observed process is commonplace or unique.

For those who seek to study, design, or evaluate policy, all of these characteristics are important. By the constant comparison of hypotheses and alternative explanations, the policymaker or analyst is prevented from jumping to

the conclusion that the first and most obvious policy solution is best. Since policy alternatives often must be general solutions rather than case-by-case determinations, learning about similarities across cases is a crucial part of deciding what will work, if imperfectly, in a multitude of contexts rather than perfectly in one.⁷ This kind of approach to the discovery of causal relationships also helps to prevent policy made in response to one extraordinary event that in fact may be atypical of the general problem.⁸

What the positivist approach does not give, however, is an understanding of causal mechanisms. While it allows the researcher to discover whether two or more phenomena are linked consistently, it does not explain why the link exists. To return to the example of previous work experience and employability, it certainly is important to know that recipients who have worked use networks, while recipients who never have worked use ads, but establishing this relationship does not explain the reasons behind this relationship. True, there is one obvious and plausible reason why this might be so—previously employed recipients made friends during their previous spells of work, and those friends often are willing to recommend recipients to their own employers; but, is this in fact the case? Perhaps the real reason behind this difference is that recipients who never have worked don't realize that they could ask their friends to help them find work, or perhaps they are embarrassed, or perhaps they give up more easily while the previously employed recipients have more faith that they eventually will work again, or perhaps employers don't even place an advertisement unless their employee networks don't produce job candidates or refuse to hire welfare recipients unless they come with a recommendation.

As these examples show, correctly explaining the mechanism that connects two (or more) causally related variables does not merely enrich the causal story. Instead, it changes the causal story. If the causal mechanism is simply that recipients with previous experience have access to more sources of jobs, the policy problem can be solved by hiring job developers to help make job contacts for recipients who don't have their own networks.⁹ On the other hand, if the problem is that employers want references from current employees—perhaps because they don't trust welfare recipients, perhaps because they think that employing the friends of current workers ensures that the recommenders will keep their friends in line lest they get in trouble with the employer for recommending a bad prospect—then job developers can do little good.¹⁰ Of course, the policy problem is different still if the unwillingness to use networks stems from recipients' own embarrassment or lack of initiative.

Discovering and delineating the difference between these causal mechanisms is work most suited to an interpretivist approach. Interpretivist questions remind the researcher to look not only for the presence or absence of a relationship, but also the specific ways in which it is manifested and the context in which it occurs. Thus, the researcher is able to go beyond "what" has occurred to see "how" it has happened. The researcher does this by paying attention to the actors' stated reasons for their behavior, trying to figure out how behavior and belief match, and looking for ways in which those beliefs and behaviors are echoed in other specific practices. In the case of previous work experience, for example, the researcher would talk to employers about how they evaluate applicants' qualifications, would look to see how employers actually selected among applicants, and would ask why those rationales "fit" the behavior, but the researcher also would go beyond the particular problem of employment to discover where beliefs about good and bad workers parallel other kinds of beliefs and

behaviors. For instance, the researcher would try to discover what the need for qualifications reflects: perhaps the preferences of customers, perhaps the fear that welfare recipients are unreliable, perhaps the belief that evaluation is what supervisors should do. The researcher might discover this in a set of economic constraints, cultural categories, or social roles, or might find echoes in language, in decisionmaking, in other seemingly unrelated practices. All these, in turn, enrich the reader's understanding of the causal mechanism. The details make an otherwise ad hoc or merely plausible connection between work experience and employment one that is embedded in a world with consistent logic. That logic may not match the researcher's; but it will be coherent from within the actors' frame of reference, even if incoherent outside of it.

This last point is particularly important when taken to the world of policy analysis, design, or evaluation. Policy researchers must move, beyond the recognition of causal patterns, toward the discovery of solutions. Knowing that a relationship exists between a causal factor and a problem does not dictate the solution, because that relationship may not be subject to manipulation. For instance, knowing that never-employed welfare recipients do not have the networks that lead to jobs does not make it possible to create a network of employed friends and family to aid them; but if researchers know what a network means to a welfare recipient—how it serves as a source of jobs—they then can suggest replacement strategies: making contacts for recipients, increasing employer confidence, using the welfare office to monitor recipient performance on the job.

Going beyond the identification of causal relationships observed for a particular problem is also crucial if policy solutions are to avoid unintended consequences and to solve implementation dilemmas. Those who propose policy solutions must be attentive to the ancillary problems they can cause. Solutions can merely displace problems from one venue to another, exacerbate other, different policy problems by affecting other policy areas, or become entrenched despite having outlived their usefulness (Wildavsky, 1979, chaps. 2–4). For instance, if the supply of jobs in an area is allocated primarily through networks, and is completely satisfied by the demand uncovered through networks, adding job developers to find out about jobs that formerly were unadvertised will result only in displacing jobs from people with networks to people with help from job developers. The characteristics of the pool of unemployed people would change, but the overall problem of unemployment would remain the same.¹¹

Issues of implementation matter as well. No matter how technically efficient or scientifically brilliant a policy may be, it needs the support and cooperation of stakeholders to be implemented (Elmore, 1979–80; Mazmanian & Sabatier, 1989; Peterson, Rabe, & Wong, 1986). Since even the material benefits or costs of a policy are dependent upon frames of reference that identify them as benefits or costs, one never simply can “see” incentives or disincentives by noting their presence in the data.¹² For instance, it would be easy to see the availability of subsidies for employers to hire welfare recipients as an incentive, but it might not win the cooperation of employers because subsidies would not help them establish the informal control they have over employees whose family or friends take responsibility for monitoring their work.

The combination of positivist and interpretivist approaches in policy studies thus provides both the causal “what” and the causal “how”—something neither approach can provide alone. It allows the policy researcher not only to add qualitative data to a problem, but to train different kinds of questions on it. The generalizing power of the positivist model gives the researcher a sense of the

important variables and the scope of a problem; the intensity of the interpretivist model provides the explanations necessary to conclude that a set of relationships is significant theoretically and substantively. Each approach also reminds the other of the assumptions that it cannot make: an understanding of positivist logic keeps the interpretivist from treating the case as a representative microcosm of a general problem, while an understanding of interpretivist logic keeps the positivist from assuming that plausible stories about mechanisms can remain assumptions.

Despite the ability of the two approaches to complement each other, though, conflicts over their understandings of validity separate proponents of the two persuasions. Positivists believe that generalization is both the measure and the goal of causal research. In other words, if the causal relationship cannot be replicated in another set of data, the researcher must conclude that there was something wrong with either the sets of observations or with the understanding of the general relationship and its workings.¹³ Interpretivists, however, do not define validity by generalizability. Interpretivist work draws upon notions of credibility and accuracy of description to establish validity, not upon the evaluation of how often the variables are repeated and in what combinations. Interpretivists also have a different understanding of generalization, seeing it as the creation of taxonomies rather than as the discovery of causal relationships that operate across different times and spaces (see, for example, Geertz, 1973, chap. 1; Lincoln & Guba, 1985).

Assessing Validity

These differences in definition and practice do not make it impossible for one approach to take insights or data from another. As the quotations in the beginning of this paper suggest, positivist researchers believe that they can take information from thick description or case studies about variables and hypotheses that they then test more rigorously, while a favorite interpretivist trope is to take a simple causal relationship and show how it is more complicated in its actual operation. A true combination of positivist and interpretivist work requires more than a simple borrowing of data; it also requires the intent to use each approach as the check on the biases of the other.

To accomplish this, one must understand the standards necessary for evaluating the validity of positivist and interpretivist work. Analyzing this problem is complicated by the fact that interpretivist work often has been condemned by positivist researchers as anecdotal and ill-measured, while defenders sometimes have insisted that the details of interpretivist work somehow make the researchers' work more solid or reliable, by definition. Such byplays can lead the frustrated observer to suspect that there are no attempts to validate qualitative research, but only a fight between detractors and supporters over whether such validation is even possible or necessary.

In fact, however, both positivist and interpretivist researchers who use qualitative methods have developed sophisticated ways of evaluating and analyzing data that address issues of validity. Qualitative researchers in both traditions require the research *process* to consist of documented procedures, which can be described to outsiders and justified in terms of the knowledge one hopes to obtain. Thus, ethnographers keep field notes, interviewers produce transcripts, and document analyzers reference passages and the context that surrounds them. All qualitative researchers also keep tabs on how they enter a research site, who their

informants and introducers are, and how people react to their presence. In addition, qualitative researchers assume that complete knowledge is impossible, and indeed that all conclusions must be uncertain; an important corollary to this is that it must be possible to conceive of and evaluate alternative explanations (King et al., 1994, especially chap. 1; Kirk & Miller, 1986; Lincoln & Guba, 1985, chaps. 11 & 13; Miles & Huberman, 1984).

Qualitative researchers part ways when confronted with questions of how to evaluate research *content*. Positivists are more likely to use an approach that adapts the logic of statistical inference to qualitative work, while interpretivists employ "thick description" of the research site (Geertz, 1973). For those who use a modified statistical logic, the standard to which one should compare one's actual research results is the "average" effect, or the effect that one might imagine a variable having if the world allowed for a series of replications of the observation, collected in such a way as to avoid known biases and then averaged so as to wipe out random variations. For those who use thick description, by contrast, the standard of comparison is the accuracy of the details invoked and the completeness and specificity of the description; although the researcher's purpose is to distill general patterns or salient features from a set of observations, what is distinctive about a particular place or time must not be lost in doing so. In the first case, the researcher's aim is to be representative, in the sense of "typical." In the second case, the researcher's aim is to create a representation, in the sense of "faithful." Both are legitimate standards against which to evaluate the validity, or truth, of a research project.

Comparing two studies here might be helpful. In his book, *Living on the Edge: The Realities of Welfare in America*, Mark Rank (1994) demonstrates the validity of his interviews by explaining the sampling technique he used to get a group of respondents who would be able to provide a picture of the average welfare recipient. First, he generated a random sample from a county chosen to reflect "the overall state population in terms of urban and rural areas, occupational diversity, and so on" (Rank, 1994, p. 209, and Appendix A). He also thinks about the limitations of his sample: It is not a perfect match to a random sample of welfare recipients in the country, and the interviews occurred sometime after the period in which he collected his quantitative data. He "selected quotes based on whether they were representative and characteristic of major themes, generalities, or categories found within the sample ... across a broad range of the respondents ... [and] particularly revealing of overall categories and generalities" (Rank, 1994, pp. 218-219). He reports the response rate, and explains how he contacted respondents and how the interview was explained to respondents. He assesses the quality of their information by evaluating the consistency between oral responses and the information in the respondents' welfare files, as well as by subjective measures of "rapport" between interviewer and interviewee. Finally, throughout the book there also is implicit mutual validation of his qualitative data with his quantitative results.

By contrast, *Streetwise: Race, Class, and Change in an Urban Community*, by Elijah Anderson (1990), is an ethnographic account of street life and public culture in two neighboring communities—one black and very poor, one increasingly white and middle-to-upper-income. Instead of reporting his sample or asserting that the "Village-Northton" (his study area) was typical of the larger city or of the United States, Anderson explains how he immersed himself in the lives of people in the area and why he believes that this particular community is important: "To gain an effective point of view, I spent many hours on the streets,

talking and listening to the people of the neighborhood.... I photographed the setting, videotaped street corner scenes, recorded interviews, and got to know all kinds of people.... I frequented neighborhood bars, laundromats, and carryouts and attended brunches, parties, and community gatherings" (Anderson, 1990, pp. ix–x). He then goes on to explain that the Village-Northton is an example of two types of social change: the return of young wealthier Whites and some middle-class Blacks to certain city neighborhoods, displacing the lower-income, mostly Black residents in the process, and the growth of unemployment, crime, drug use, family disorganization, and antisocial behavior, which creates its own kind of disorder in low-income Black areas (Anderson, 1990, pp. 1–4). The fact that each community serves as a foil for the other's assumptions makes it possible for him to understand what is taken for granted in each, while his outsider status forces him to connect the perspectives he sees to general categories that they resemble.

These two examples show how the two approaches deal with uncertainty. In the positivist case, uncertainty can be reduced through a research design that compares as many observations as possible on what the hypothesis predicts will be important dimensions (see King et al., 1994, chap. 4). Careful consideration of implications of the analysis is also necessary to see that alternative explanations, and thus important omitted dimensions or aggregated concepts, are ruled out as causes (see King et al., 1994, chap. 5). Thus, for instance, one of Rank's arguments is that welfare recipients are as interested in getting off welfare as the average taxpayer is in keeping them off. Rank looks at married couples as well as unmarried mothers, those who were working while receiving aid and those who were not, at behavior and also at attitudes, to make his case (Rank, 1994, chaps. 5, 7). He also takes on an implication of his analysis—that since welfare recipients buy into the mainstream values that most Americans share, they would be as derisive of "people on welfare" as are nonrecipients—and finds that to be true (Rank, 1994, chap. 8).

By contrast, the accuracy of interpretive work depends on the faithfulness of the interpretation to the circumstances being studied and the thickness of the description offered.¹⁴ Faithfulness can be determined by checking one's interpretations against the criticisms of peers, against the reactions of the members of the group being studied, and against as many different kinds of observations as possible. Thus, Anderson's text includes long passages from his field notes, descriptive passages of the area, long quotes from interviews, and accounts of events; the field notes both show the range of sources he used and allow others to challenge his interpretations.¹⁵ In addition, the thickness of the description provided is crucial to an evaluation of the researcher's conclusions. There must be enough information for a reader to feel convinced that the writer is depicting a world rather than finding information for a specific hypothesis. Anderson does this by analyzing a variety of subjects—everything from the area's history to drugs, sex codes and family life, police activity, and community improvement efforts—which might seem at first to have little to do with his discussion of how people of different races and social classes interact in public space that they share. He does so because his descriptions of street behavior and social codes mean little unless they are rooted in the experiences and beliefs that create those particular actions. From the research tradition in which Anderson works, his analysis of street interactions would be too shallow if he could not show the context from which they arose.

From this description, it should be clear that the standards used by positivists and interpretivists are incommensurable—not better or worse, but

different because they are used for different reasons. Such differences lie at the heart of the refusal to consider combining positivist and interpretivist approaches. This refusal can be disguised as tolerance. Positivists often agree that more detail and specification would clarify their understanding of a problem,¹⁶ and interpretivists often express interest in knowing how similar their research site is to others,¹⁷ but when this tolerance is accompanied by insistence on maintaining one common standard of validity, it is akin to saying that research from a different tradition must be held to my standard of validity while my own work need not satisfy any standard but my own. This is why, even apart from arguments about whether "context" is important or "generalization" is possible, positivists and interpretivists often attack the credibility of each other's work. Positivists suggest that interpretivist work is mere anecdote, implicitly suggesting that the specific incidents that interpretivists describe must be found in many other places before they can be taken seriously. Interpretivists suggest that positivist work is a mindless multiplication of superficially similar facts, which do not explain anything because they are shorn of the specific details that interpretivists value.

Epistemological imperialism aside, this refusal closes off the ways in which combining positivist and interpretivist approaches can help to reduce biases inherent in either alone. An interpretivist complement to positivist research keeps results from being technically correct but too detached from the context to produce an accurate picture; it can be imagined as preventing the prediction that because babies cry, and adults are annoyed by crying, adults should be annoyed by babies. Similarly, a positivist component to interpretivist research keeps particular results from being considered microcosmic; it prevents one parent's reactions to a colicky baby from being considered the set of reactions common to all parents.¹⁸

This combination is especially important when research is used in the service of policy. The positivist interest in general rules and the average case easily can lead to the assumption that because one can isolate a relationship between a set of causes and their effect, one also can expect to manipulate those causes in isolation. Policy solutions made in this way are likely to fail, because the world seldom moves through controlled experiments where all things but one are equal. A second problem arises when outsiders or the researchers themselves cannot identify with the communities who are the target of a policy. In this case, the researcher is apt to assume that any effort to solve a problem is good. For that community, however, the causes of a problem almost always are related to other important values and practices, even when they recognize and dislike the by-product problems. For the community in question, positivist solutions therefore can be worse than the cause.

Interpretivists often pride themselves on knowing more about communities they study than positivists do, and thus on being less likely to suggest solutions the community would not approve, but the limitations of their approach lead to biases of their own. The interpretivist interest in finding general principles manifested in a specific case can lead to the assumption that the specific case is a microcosm of the whole. Policy based on such cases often is not general or multidimensional enough to fit all the sites for which it is intended. The problem is exacerbated when the stories reported in the analysis are the most colorful or the most memorable ones; those traits often spring from the fact that the circumstances of the case are atypical. A complementary problem is that the interpretivist often shows so many intertwining factors at the root of a particular policy problem that, absent massive social or economic change, no single intervention seems like it could improve the situation. In the mirror image of the

positivist, the interpretivist sees too many things that could go wrong with a particular solution, a condition that leads to paralysis.

A final example may help to clarify this point. A central focus of recent policy debates in welfare has been the enforcement of child support payments. It is an idea that can fit into many ideological agendas; some reformers see it as reinforcing parental responsibility for children and bringing the father back to a place of honor in the family (Blankenhorn, 1995), while others find it a lever for changing the welfare system into a system based on parental obligations, but with a level of support ensured by the government (Ellwood, 1988; Garfinkel & McLanahan, 1986). Positivist quantitative and qualitative work has documented the failure of courts to order fathers to pay child support, or for payment to be enforced once it is ordered, with severe economic consequences for mothers and children. Researchers hail the increased enforcement of child support as the economic margin that would allow single mothers to support their children with additional part-time work (standard for mothers in two-parent homes), without increased government expenditures (since most of the money would come from absent fathers) (Bane & Ellwood, 1994, pp. 154–156).

As pressing as is the need for paternal responsibility, however, some interpretivist work is beginning to suggest that the consequences may not be good for low-income African-American families—presumably the families for whom child support would become a substitute for welfare. This work suggests that in the inner city, unmarried fathers often support their children covertly through in-kind gifts of formula and diapers, babysitting, or financial contributions from their own funds or from their parents (the baby's paternal grandparents) and other relatives. At the same time, these fathers cannot formalize such contributions through the welfare system, because chronic unemployment means that their wages and contributions are sporadic. Many also fear any involvement with the judicial system, an institution that many African-American men believe is biased against them. Recent attempts to reintroduce statutory rape laws only increase fathers' unwillingness to be named officially the father of their children (Johnson, 1993, 1996; McLaughlin, 1996).

All of these findings suggest the inappropriateness of considering the problem of paternal support outside of the context of poverty and chronic unemployment and of a history of discrimination against African-Americans by the legal system. They also predict implementation problems were child support collection from low-income African-American men to be more strictly enforced. Without an interpretivist perspective, most of these issues would be (and so far have been) invisible to policymakers. With an interpretivist perspective, policy alternatives to blanket, court-ordered enforcement—such as programs that involve the families of young parents in support agreements—are easier to design.¹⁹

On the other hand, suggesting that such alternatives would be suitable for all fathers, or that child support enforcement cannot work absent the eradication of racism or poverty, would be inappropriate generalizations from interpretivist work. Here positivist work, suggesting the widespread nature of the problem (fathers of all incomes do not pay child support, and single mothers have economic difficulty raising children in most circumstances), provides a useful corrective. It suggests that additional work is necessary, to see if the problems that these low-income African-American fathers face are generalizable to all African-American fathers, to all low-income fathers, or to fathers in general. It also suggests that more interpretivist work on middle-class employed fathers needs to be done if

policymakers are to understand the context within which child support goes unpaid.

Calling for more, and more varied, research is, of course, the perennial complaint of the researcher. For it to happen, something more than exhortation has to occur; it is necessary to have a realistic assessment of how researchers, trained and for the most part preoccupied with one approach, can take on the demands of another. I examine this problem in the conclusion.

Research in Conversation

Each kind of research—positivist or interpretivist—imposes its own way of viewing the world, and calls for the triangulation²⁰ of approaches often falter on exactly this point. Often, taking a positivist approach can direct one away from learning about context (because contextual factors, however important substantively, do not bias one's estimates of the causal effect of any variable that is uncorrelated with those factors). Taking an interpretivist approach similarly discourages positivist work (because learning about one case thoroughly means narrowing one's focus away from the general). As researchers plan or stumble into a research program, familiarity with one approach inevitably puts blinders on the questions one asks, leads one to forget to collect data that would be useful in answering questions from a different approach, and causes one to modify even carefully triangulated research designs in ways that make it difficult to apply a different approach later. Learning to do research within the confines of a positivist or interpretivist approach also means that one is unfamiliar with the other, in ways that impede intuition and make inferences less surefooted.

But the difficulty of designing complete studies with varied approaches should not blind researchers to their ability to do studies that at least are aware of different approaches. Here I offer four options, each of which requires progressively more immersion in both qualitative traditions. Through informed discussion of other literatures, outlines of complementary studies, exploratory interpretive studies, and comparative case studies, it is possible to take insights from one of the two traditions even when one's primary focus is in the other. Perhaps more importantly, these options make it possible for those who come from a different qualitative tradition to appreciate and use different research, thus contributing to cross-fertilization within an intellectual community, if not in any one particular project.

Many research projects use a search of the literature to frame and situate their contribution, but such literature reviews often narrow down quickly into only question- and method-specific research. By default, this tends to confine literature discussions to either positivist or interpretivist approaches. A stronger approach would be to track down literatures that have the same substantive focus as one's own question but use a different approach. The researcher then should discuss which ways of recasting her question or probing new sources of information could take place as a consequence. For Kathryn Edin (1991), for instance, this approach would have suggested a closer reading of Carol Stack's (1974) book about family exchange networks for food, clothing, and other necessities that help poor families make it in cash-poor times. Questions about in-kind donations received and gifts given by mothers, from and to friends and family, not only would have tested Stack's argument outside of her "neighborhood," they also potentially would have led to insights about whether tight family networks facilitate or impede the move

off welfare rolls. Similarly, a new ethnographic study of welfare recipients might take into account Rank's (1994) observation that welfare recipients, faithful to sociological understandings of stigma, often express prejudice against other welfare recipients. Such a study might try to fit observations of prejudice into a context: When do such expressions occur, and in response to what events or questions?

A second, similar but more complicated, technique is for researchers to outline studies from a different approach that would complement their own work. In other words, rather than forming questions that are based partly on literature from a different approach, researchers take their own discoveries and explore how they could be enriched by different studies. The important point here is actually to outline studies that could be done, because this process allows others to reconstruct the researcher's train of thought and thus permits future studies and future critique to be more productive.

To complement his ethnographic work in *Streetwise*, for instance, an interpretivist like Elijah Anderson (1990) might suggest a positivist survey that would ask teenagers from a variety of different economic and community backgrounds about premarital sex, abortions, childbirth, and childraising. This kind of study could show whether the ghetto-specific attitudes he describes are shared widely among teens, though described in terms of their specific environment and ambitions, or whether the young people in his ghetto actually develop attitudes and behaviors substantively different from those of young people of other backgrounds. A survey informed by ethnographic work might help illuminate just which problems should be attributed to the ghetto, which problems are related to the reasons people are found in ghettos (poverty, racial segregation, and the like), and which problems seem most pressing in the ghetto but in fact are shared widely in the country at large. Such a survey also would have a better chance of asking the right questions than would a survey designed in the absence of specific knowledge about the phenomenon in question.

This type of approach suggests a third possibility: When researchers have a chance to combine the two types of research in their own work, they should start with an exploratory interpretivist study. The reason for this lies in the ability of interpretivist work to suggest hypotheses and mechanisms that then can serve as the basis for positivist research. Through interpretivist exploration of a causal mechanism, for instance, the many causal steps needed to make it work can be identified. Each of those steps then can serve as the basis for a positivist study. By contrast, the reverse order forces researchers to impose a "plausible" theoretical structure on the data before they examine it, a structure that easily could leave out some important variable or include some nonessential one. While a positivist study, in its focus on a few causal relationships, still may leave out contextual information that the interpretivist will find important, at least those relationships are likely to be specified correctly and the contextual information to be justified as nonessential to the establishment of the specific relationships under study. Researchers thus are enabled to give a substantive answer to their critics, rather than ducking questions with the suggestion that those who disagree should do their own study.

An example of such a study might include immersing oneself in the workings of a particular welfare-to-work office, its staff practices, and client incentives, before creating a study of the implementation of welfare reform in or across states. The interpretivist work would allow the researcher to discover which practices seem to matter, which incentives are ignored or unappealing in practice, and which extra-program factors (the availability of jobs, or public attitudes in the

community) affect the operation of the program. The positivist work then would try to see if the findings from the interpretivist work were distributed widely across a sample of welfare-to-work offices or efforts. If not, future researchers then could examine why the program originally studied was *sui generis*, which conditions seem to create one reform process as opposed to another, or what explains the absence of relationships between variables that plausibly are linked. By contrast, if one were to do a positivist study first, say of the relationship between incentives and client participation, one would not be able to tell whether the summary conclusions produced by one's analysis reflected the same mechanism, different mechanisms operating in different locations, or different mechanisms operating for different people. One might be able to uncover the answers eventually by better parsing of the data, but even this would depend on having the knowledge or intuition to know which categories to parse.

Finally, if one's resources permit, the comparative case study makes it possible to incorporate some of the best features of positivist and interpretivist work in the same study. The case study allows the researcher to see the phenomenon of interest within its context—to trace out and recreate the mechanisms that connect events or relationships—but the inclusion of several case studies in one project also forces the researcher to be more rigorous about defining specific relationships, provides the researcher with a ready-made collection of alternative explanations, and keeps the definition of terms from being so situation-specific that parallels to other situations are lost. This is not to say that all comparative case studies combine interpretivist and positivist perspectives. It is completely possible to look for the same sets of observations in several different locations without examining how those observations are connected in each location, and it is possible to do several case studies, each an interpretation of specifics within a situation, none of which bridges the specific details in order to speak to each other, but if one chooses to take advantage of the opportunity, the space for a combination of positivist and interpretivist work is available in this research design.

Examples of this kind of work are few in the area of poverty studies, but they are growing in both number and prominence. One notable example is Katherine Newman's (1988) study, *Falling From Grace: The Experience of Downward Mobility in the American Middle Class*. Newman's book looks at four examples of downward mobility—"downsized" managers and their families, air traffic controllers who were fired collectively after a strike, the members of a community dependent on a closed factory, and divorced women. Specific context is particularly important in two of her cases: air traffic controllers, whose job loss occurred as part of a particular set of historical events, and the residents of the small city whose life had been structured largely by the presence of its largest employer. In the other two cases, similarities in middle-class lifestyle and in expectations of permanence, which are experienced by managers before layoffs and ex-wives before divorce, are used to explain their particular adaptations to downward mobility. Yet combining the four disparate cases also allows the similarities in the experience of downward mobility—the nontransferability of skills, delayed or dashed expectations, family stress, and individual guilt—to be illuminated. Newman also identifies a common method of coping with the psychic loss embodied in downward mobility, through the ability to derive a sense of honor and commitment to something larger than oneself. While the form this takes is very different in each group she examines, seeing all four groups together

allows her to identify a phenomenon that otherwise easily would be attributed to the distinctive circumstances of the members in each group.

Newman (1988, p. 19) states explicitly that her purpose is to provide a look at the way American culture provides "an architecture of interpretation" that people use to explain their downward mobility. The positivist elements in her work—the discovery of common elements across disparate situations—thus primarily are an artifact of her technique and not of intent. An interpretivist researcher who was committed more explicitly to a set of positivist concerns, however, could have made the combination of approaches even more explicit by collecting some additional data during the interviews—for instance, information on prejob loss and postjob loss of income, on the methods used to look for work or on the changes in friendship patterns—and examining them systematically across the categories. The great advantage of the comparative case study is that, either with planning or with the willingness to revise one's research design in midstream, it provides a structure through which both positivist and interpretivist ends easily can be reconciled. Indeed, the structure makes this combination hard to avoid.

Each of these options requires the ability to think in different research patterns, in part to shape the scope of data collection, in part to see the different uses that data can have, and in part to make a conscious choice between the questions one hopes to answer and the questions one will leave unanswered. The mental gymnastics this requires indeed do ask more work of the researcher, but the payoff is great, and is not limited to the improvement of the individual researcher's work. Instead, as we learn more about the limitations of our studies and encourage others to correct us, we create a community of scholars who build on each others' work across traditions. For an interdisciplinary area such as public policy, this seems like an inherently useful goal.

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Notes

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¹ My decision to discuss positivist and interpretivist as "approaches" is deliberate and deserves some explanation. This paper brackets disputes over the existence of causality and as such does not deal with positivism and interpretivism as full-blown epistemologies. Instead, I prefer to think of positivist and interpretivist questions, and thus approaches to research. While I often counterpose the two in this paper, I believe that one of the virtues of considering positivist and interpretivist approaches is that it allows for the combination of research questions in different traditions. A similar combination of epistemologies requires much more careful work to delineate, and as such is beyond the scope of this paper.

² Note that this is a causal argument; she is not just describing a state of affairs but linking that state of affairs to precedents and antecedents. As I describe below, interpretivists deal with causality just as positivists do, but they have different notions of what a causal argument is and how it is evaluated.

³ I thank one of the reviewers for emphasizing this point and suggesting this language.

⁴ I do not mean to imply here that interpretivists care about only causal mechanisms, or that noncausal research from either a positivist or interpretivist tradition is unimportant. Good descriptive work has an important place in both traditions, and causality aside, many researchers see their job as the discovery of insights and perspectives, not as the discovery of causes. My analysis in this paper does not include this kind of work, but it should not be interpreted as excluding these other kinds of work from attention.

⁵ This is an important dispute, especially to many interpretivists, who take issue with mechanistic notions of causality. I bracket this question in the paper with the understanding that my discussion therefore can address only researchers who are willing to take causality as an assumption, and I hope to offer a better defense in future work.

⁶ For a more technical and more complete explanation, see King, Keohane, and Verba (1994, pp. 82–85).

⁷ Robert Behn (1981, especially pp. 206–211) makes this argument, though for a different purpose.

⁸ The argument here parallels the one in Donald Horowitz's (1977, especially pp. 38–45) classic study of the implementation of court decisions. He suggests that one reason courts are ill-suited to make public policy is that the cases that come to them are so often distinctive: "Because courts see only the cases that come their way, they make general law from what may be very special situations" (Horowitz, 1977, p. 44).

⁹ For information on the role that job developers can play, see: Bardach (1993); Friedlander, Riccio, and Freedman (1993); Cave, Bos, Doolittle, and Toussaint (1993); and Hollister, Kemper, and Maynard (1984).

¹⁰ Katherine Newman made this point at a seminar in the Woodrow Wilson Public Affairs Summer Program, University of Michigan School of Public Policy, July 1995.

¹¹ This actually happens; in Oklahoma, welfare agency staff comment that job developers from the various state agencies actually compete to get jobs for their clients (Lurie & Hagen, 1993, p. 79). A more pernicious interpretation also could be placed on this phenomenon: Opponents of job development no doubt would suggest that recipients with previous job experience got lazy once the job developers came in and no longer looked as hard for work on their own.

¹² For instance, imagine a solution that seeks to attack student misbehavior by keeping offenders after school. This is a disincentive only if the primary frame of reference is dislike of time spent in school; it becomes an incentive, however, if being required to stay after school gives students a reputation as "tough" among their friends. Research on the effect of such sanctions easily could be misguided if only the disincentive effects of a sanction were assumed to be plausible. In that case, the researchers simply might look for a relationship between the presence or absence of a sanction and truancy and conclude that the sanction had no effect, rather than two significant but contradictory ones (see Lin, 1994).

¹³ Readers of King et al.'s (1994) book will recognize the last two sentences as a rephrasing of points 1 and 4 in Section 1.1.2, "Defining Scientific Research in the Social Sciences," and of their further elaboration elsewhere in the book. These two points—"the goal is inference" and "the content is the method"—as they are described by King et al. (1994), seem to me to be the embodiment of good positivist science. Their other two points in this section—"the procedures are public" and "the conclusions are uncertain"—are accepted by good interpretivists as well, though most interpretivists would not agree with the implications that King et al. (1994) draw from these propositions.

¹⁴ This paragraph is based heavily on Lincoln and Guba (1985), especially their chapter 11, "Establishing Trustworthiness." Other useful sources are Kirk and Miller (1986) and Spradley (1979).

¹⁵ Again, there is an analogy here to King et al.'s (1994) concern with uncertainty, though again the goal is not a measure of uncertainty but the consideration of alternatives.

¹⁶ "Too much" information does raise a problem of efficiency in the positivist model; if "irrelevant" variables are included in an analysis, the observations in the data set are asked to tell too many stories, and the confidence one can place in any one story is reduced (see King et al., 1994, pp. 182–185). However, this is a problem primarily for pure positivists, whose interest in the causal relationship is paramount. If one thinks of the goal of research as creating and extending categories, the additional information becomes useful for convincing one that the original categories were justified and allows new categories to be tested. The aim, in other words, is bigger than any one causal relationship. Note also that interpretivists do not believe that all information is relevant; the weather might be relevant to an interpretation of planting ceremonies, but it might not be relevant to decisions about increases in social welfare spending.

¹⁷ Note that identifying something as atypical is an important first step in then thinking about why the typical is as it is. A discussion of how to think about atypical instances is found in Martha Feldman (1995). For an example of how one learns about the typical by examining the atypical in great detail, see Geertz (1973), "Religion and social change: A Javanese example," pp. 142-169.

¹⁸ In *Reliability and Validity in Qualitative Research*, Jerome Kirk and Marc Miller (1986, p. 21) argue that quantitative research "has relied almost entirely on techniques for assuring reliability, in part because 'perfect validity' is not even theoretically attainable. Most nonqualitative research methodologies come complete with a variety of checks on reliability, and none on validity." Although the analysis in this section has been informed by their argument, this formulation seems too imperialistic to me. I prefer allowing both epistemologies to hold their understanding of validity in peace, as long as it is made clear that very different concepts are meant by the word.

¹⁹ The fact that unanticipated consequences exist, of course, is well known to positivists. It is no news to an economist that imposing regulations subverts informal markets, in this case a more efficient collection mechanism for child support in low-income African-American communities, but predicting that regulation has these effects is very different from being able to describe the specific form that an unanticipated consequence will take or the issues to which it is connected. Knowing the specific mechanism allows policymakers to devise alternatives; knowing only that unanticipated consequences will be produced gives policymakers only two choices, regulation or its absence.

²⁰ This term is borrowed from Todd Jick (1979).

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