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If not positivism, then why is sociology important?

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Positivism basking in the "new" social theory

The critics of positivism are now more prevalent than the theorists they scorn. Indeed, contrary to Bryant's comments, there is no clear revival of positivism; rather, theory today is now a mixture of commentaries on: (1) the faults of positivism and scientific sociology; (2) the ontological and epistemological problems of theorizing about human interaction and organization; (3) the offering of alternatives which (pick your favorite combination) take into account human agency, indeterminacy, history, context, or contingency; (4) the advocacy of critique of technology, capitalism, and assorted evils or the offering of a program and plan for doing criticism when all the philosophical issues are worked out; (5) the worship of the masters through history of ideas, name dropping and quoting, or scholarship on particular theorists; and (6) the fine-tuning of the lost art of discourse (on just about anything non-empirical). The result is that much sociological "theory" does not seek to explain how the social world operates. Bryant's critique of positivism performs at least four of these tasks: it faults the work of positivists; it tells positivists that they have not seriously considered the epistemological and ontological issues; it proposes an alternative that encompasses (3) above; and it invokes the great masters — Marx and Weber — to substantiate the critique against positivists. And, it does not seek to explain anything.

To say that we have heard all this before is an understatement, but Bryant is to be commended for writing well, thereby making his argument accessible to readers. Yet, like most diatribes, it is rather selective in its argumentation and

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vague in what explanatory alternative to positivism is being espoused, save for some unspecified appeal to historical analysis. Moreover, the starting point of the paper — that positivism has failed to produce explanatory theory — is patently false. Thus, we have a polemic that is not very original and, I think, wrong in its starting point and conclusions.

Theoretical vs. historical explanation

Since the author cites my book, *Societal Stratification* (Turner, 1984), he is aware that I make a distinction between historical and theoretical explanations. I do not assert that one type of explanation is superior to the other, at least not in recent years, but only that historical and theoretical approaches are very different ways of explaining phenomena. Historical explanations address contexts and the causal sequences of events that produced a particular phenomenon of interest and, as such, they are filled with accounts of agency, particulars, and contingency. Bryant clearly prefers this kind of explanation, and so be it; I certainly have no quarrel if he and others wish to develop such explanations. I just wish that a larger group of theorists would actually develop historical explanations rather than talk about how they will, someday, develop them. For me, the work of such scholars as Mann (1986), Skocpol (1979), Moore (1966), Goldstone (1990), Tilly (1978), Braudel (1977), Abu-Lughod (1989), and others array data for theoretical explanations, although in each case one can also observe a theory and an ambivalence over whether or not to develop its principles beyond the historical cases under consideration.

A theoretical explanation is one that seeks to explain empirical phenomena in terms of abstract principles and models; and this is what theory is in the natural sciences. The goal of scientific theory is to account for some set of empirical processes in terms of the dynamics delineated in abstract, context-free, and non-contingent (save for scope conditions) principles and models. Theoretical explanations yield a different kind of understanding and knowledge than historical explanations; and if Bryant and others find such explanations too constraining, this is their prerogative. But, as I will discuss shortly, there are serious consequences for sociology when one rejects scientific theory as "pretentious" or some other epithet.

To the extent that Bryant tells us about his alternative, it emanates from approving sounds about Marx and Weber. Yet, these two scholars were highly ambivalent about the relationship between historical and theoretical explanation. Weber thought that the use of ideal types could serve as the general and abstract portions of historical explanations, whereas Marx saw abstract laws as relevant for particular historical epochs. Neither of these efforts at reconciling historical with theoretical explanation has been very effective, however. Weber's strategy produces category systems for pigeon-holing data, although his emphasis on causal processes makes his work more dynamic; Marx's approach created a great deal of talk about the evils and contradictions of capitalism as well as a spate of flawed (though interesting) interpretations of history in terms of Marxian principles (e.g., Anderson, 1974), but such interpretations tended to be "deductive" and rather insensitive to data which does not confirm the "first principles." Indeed, Bryant's criticisms could be easily leveled against Marx and Marxists.

My point here is that Bryant has not resolved the issue with simple assertions in his "straw-man" portrayal, for positivism is neither categorically "bad" nor is historical-contextual explanation absolutely "good." They are both useful depending on one's purposes. Yet, scholars doing one often try to perform the other in some fashion, and here is where problems often emerge because theoretical and historical explanations are very different activities: one abstracts and pulls away from particulars, the other does the reverse. To indict those of us who do scientific theory for not being historians, ethnographers, and otherwise immersed in data, context, and acts of agency is, well, irrelevant to me, and other positivists.

The critique of Turner's work

Bryant's critique of my work is cavalier and distorted. He quotes me in ways that imply my disregard for the "prototheorists" or early masters; and surely he must know that this is nonsense. I have spent a career engaged with the early masters, trying to take them from prototheorists to scientific theorists. My point in the mid-1980s was this: Spencer, Marx, Weber, Durkheim, Simmel, Mead, and others understood many of the basic dynamics of the social universe; this is why we find their work so interesting and keep rereading it; and thus why we should extract their theoretical insights, state them more formally, build upon them, and then move on. But most theorists continue praying to St. Marx, Weber, and Durkheim as opposed to using their ideas to explain something (there are clear exceptions here, but much "theory" in sociology is *about* our canonized masters rather than about the empirical world).

Bryant also quotes me as relegating "all opposition" to the "anti-science fringe," which is rather amusing in light of the fact that Bryant wants to consign me and others to the "positivist" fringe (a fact which, without Bryant's efforts, has been done by many others). All that I assert is this: science seeks to understand the fundamental processes of a universe in terms of abstract principles and models; this is what science is, period; and those who do not share this ultimate goal in their research and conceptual efforts are not practicing science. This does *not* mean, however, that the works of others who are uncommitted to scientific explanation are irrelevant to science; I use the works of many who did not, and who do not now, believe that sociology can be a science, and I would hope that the reverse is true. But Bryant and others cannot simply change the definition of science because they want to do something else, whether history,

ethnography, or social criticism, nor can they redefine science in order to seek the respectability that it has brought to other disciplines. If sociologists do not want to be scientists, they will have to accept the consequence of this decision — a point to which I will return.

Bryant cites some of the principles that I sought to develop in *Societal Stratification* and, then, simply dismisses them. We are given such well-worn phrases as "history furnishes numerous examples," but I do not see much history in his comments, just an ad hoc listing of places which, in the author's eyes, refute what I was attempting to do. For Bryant, work such as mine is "so limited and elementary that any sociologist should feel embarrassed," and then quotes my colleague, Randall Collins, to really nail me in my coffin and plant me forever in the ground. However, quoting someone else does not constitute very powerful criticism, nor does it inform us why my theory is wrong. There are so many more distortions in Bryant's representation that I hardly know where to begin. Let me simply make a few caveats.

First, Bryant does not provide *any* real empirical evidence except vague phrases like "history furnishes." Moreover, he claims that "virtually every study" refutes a portion of an equation, but he does not cite one such study; and he conveniently ignores the rest of the equation which would obviate his criticism. Second, he accuses me of adding textual qualifications, which is true because my focus was on stratification at the societal level, but then accuses me of doing so for every equation, which is not true. Third, he cites a number of scholars whose historical work would, when coming into contact with my principles, reveal how "brittle" those principles really are. My reading of these works would suggest just the opposite, as have my personal conversations with historical sociologists like Michael Mann. Fourth, no effort is made by Bryant to discuss the data that I do array to assess the principles; apparently, only "thick descriptions" are permissible in the world according to Bryant, although he provides no such descriptions, or even references to them.

I could go on here for a while, but my conclusion is this: Bryant does not muster one single piece of data to refute the equations. The few elliptical references are not enlightening and glib assertions about what the world reveals (with no citations, or engagement with data) are just that, glib. Bryant thus musters verbiage, but little else. I can only conclude that Bryant is a young scholar who simply needs to grow up intellectually and learn more; he does not have to become what he preaches against, but he had better learn how to make critical points in a more precise and less insulting way. I will wait anxiously (well, not that anxiously) for an important contribution, as opposed to shallow critique, on some topic that he considers important. Criticism is very easy, when you are hell bent on making one, but doing what one's criticisms dictate is another matter. We will just have to wait to see if Bryant can back up his big talk with scholarship.

If not science, what is sociology?

Within theory circles, there has been a decided shift away from positivism — an empirical fact which seems to have escaped Bryant. There are, of course, many who still seek to make sociology scientific, but they now constitute the minority of sociological theorists. This situation so puzzled me that I wrote, along with Stephen Turner, a book about it: *The Impossible Science* (1990). There can be no disputing that sociology is not a very mature science: much theory is endless discourse with itself; most promising theoretical principles are rejected as too abstract by individuals such as Bryant; research is ad hoc and theoretically uniformed, and often uninforming; and many sociologists work within some narrow camp and ignore everyone else. But contrary to Bryant's assertions, there are very creative and cumulative efforts at building theory, as I have tried to demonstrate in such works as *The Structure of Sociological Theory* (1991), *A Theory of Social Interaction* (1988), *Societal Stratification* (1984) and many others (to name a few, see Turner, 1975a, 1975b, 1987, 1990a, 1990b, 1990c) where theories are brought together and formalized. Each of these theories explain a great deal; together, they explain more. But critics like Bryant will never be satisfied with these explanations, because they do not seem to understand the difference between description and explanation, between empirical generalizations and abstract principles, and between history and theory. It is true that much of the creative theorizing in sociology is divided into camps, and as a result, most of my theoretical efforts have been devoted to bringing these camps together, thereby providing better theory. Those who want to describe, to create time-bound generalizations, and to interpret history are often performing intellectually interesting work, but it is not scientific theory. My sense is that these efforts are data that can be made more interesting by explanation in terms of abstract models and principles, although as Bryant's diatribe signals, many would disagree.

And if many are doubtful about what I say, this raises the question: If sociology does not see its goal as the production of theoretical explanations, then what kind of discipline are we? For some the answer is to be social critics, but this is hardly a very unique calling (when everyone is mad about something) and is not likely to make sociology more viable within or outside academia. Others like Bryant appear to want us to be historians, but they will have to compete with a more established discipline, history, which will guard its turf. Still others want to describe and comment on current events, but they will have to demonstrate that they are better than the hordes of journalists and commentators in the media. Others want to have discourse on people, places, and times, but they will come in second to philosophers, journalists, and commentators. A good many others want to engage in the quantitative analysis of data, any data, but they will find many non-academic competitors for their cherished research grants as well as many within and outside sociology who do not see mounds of data as amounting to very much.

Thus, if our goal as a discipline is not to develop theories, to test them, and to cumulate knowledge about the social world, we will have to accept the consequences: we will play second fiddle to other disciplines and occupations which can make better claims to legitimacy. We will thus be second-rate, and vulnerable to well-justified attacks by those who do not see the necessity for another history and philosophy department, for more criticism, for more ideological commentary, for more journalism, for more reflective and self-sustaining discourse, for more data, and so on.

Without scientific theory, I believe, sociology has little that is unique or interesting to offer. It will become a dreary little discipline that can sustain itself by appeals to certain kinds of students, but it will not be a big player in either the world of academia or practice. We must accept this, because Bryant and others want it to be so; and they are the majority.

A final note on formalisms

Much of Bryant's critique seems to be inspired by a distaste of formalisms. He appears to prefer the clever turn of phrase and innuendo as the proper way to express arguments. Many others also feel this way, and so let me address them.

In a soon-to-be-published book (Hage, 1992), a number of advocates and critics wrote essays on why the formal theory movement of the late 1960s and 1970s failed (e.g., Blalock, 1969; Dubin, 1969; Gibbs, 1972; Hage, 1972; Reynolds, 1971). My own view on the reasons for this failure are that the effort was misguided from the beginning, in several senses. First, theory is not "constructed" and "built" like a house; if it is, it will be a house of cards. Yet, most formal theory texts had cookbook formulas for building theory, and as such, they advocated a mechanical and sterile set of procedures. Second, many of those advocating "theory construction" were quantitative methodologists in theoretical drag. This view of theory was like a research design: first you do this, then something else, and then another thing, and the end product will be a theory. Moreover, when methodologists do theory, they keep adding variables in order to "explain more variance," whereas good theory limits the dynamics denoted by concepts. Third, many of the formal theories actually produced by those adhering to proper "theory construction" protocols (which varied, I might add) were, in fact, empirical generalizations dressed up to look formal; and their inability to explain very much soon became evident. And fourth, theory construction books had a very idealized and unrealistic view of how theories are tested by data. They tended to ignore the fact that, without the ability to perform experiments, the "fit" between theory and data is often far from perfect (this situation is not, however, unique to sociology). Moreover, these books paid no attention to the reality that organizational politics are frequently involved in assessing a theory with data (and this is true in all sciences).

In contrast to "theory building" advocates, I see theorizing as a creative set of insight into the properties and dynamics of a basic social process. Just how one expresses this insight — words, diagrams, equations, telepathy — is less important than the insight. Such insights are almost always a mixture of induction and deduction, but the goal is to see if an insight can be expressed abstractly so that its implications for diverse empirical cases can be fully drawn out. Again, just how the abstractions are expressed is less significant than the effort to move above the particulars of empirical cases and to see these cases as illustrative of more fundamental processes. In stating a theory abstractly, one should seek to be clear and precise, but one does not have to get bogged down in the concept formation dictates of philosophers (e.g., Hempel, 1965). And in trying to assess the power of the theory, one makes deductions from the theory to the empirical world, but these are often "folk deductions" in that no precise calculus is used. Rather, we simply try to show that empirical processes are an instance of a more general and basic process specified in our abstract theory.

My model for theory is not physics (although I once flirted with this ideal), because no science is like physics, except physics. Biology is a better template for what sociology should be — lots of description and experimental work, ultimately unified by some generally theoretical principles, such as the synthetic theory of evolution. This theory is partly formal (the genetics portion), but mostly verbal or pictorial; and deductions are typically "folk deductions" to particular empirical cases. But it is nonetheless powerful in its ability to explain much of the universe of interest to biologists. I think that sociology can do the same, with perhaps a short inventory of basic principles, most of which have already been crudely articulated but which have not been formalized and pulled together adequately, especially in a hostile climate created by critics such as Bryant.

Bryant and others like him would call this the effort to pull together general theoretical principles the pursuit of an illusion. If more sociologists come to believe as Bryant does, most of them may be looking for a job, because the resource base for sociologists is weak enough without throwing away our best chance to make an impact on the intellectual and real worlds. We do not need to be "anal retentive" in developing theory, but we must make the effort. Without this effort, sociology has little claim on the resources of government, academia, students, lay publics, and sponsors in search of knowledge. And it is a discipline that few will care about, or consider important.

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