

What lies over the horizon? Is all that antihistorical thunder just so much white noise? How do we know if we are mediating, moderating, holding, or feigning the center? Heading into the millennium, what counts as a wider humanistic discussion? What nothingness borders history?

Two popular positions shape current debates. On one side, postcolonialists dismiss careful engagements with the European canon on the grounds that careful engagements with the European canon are careful engagements with the European canon. Elsewhere, philosophers of history simply ignore non-European traditions. Among the diminishing contingent of academics who really specialize in historicity, some of us concentrate on France, some Germany, some Anglo-America, and a few Africa or southern Asia, but no one bridges all of these subfields. Who could speak all the languages? In an age of global historicities, we should be grateful that David Roberts has given us a book that tackles a nice sweep of the western hemisphere. Show me the center.

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James B. Rule, *Theory and Progress in Social Science*. New York: Cambridge University Press, 1997.

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There can be few sociologists of any theoretical stripe who have not, at some time, had to defend themselves against the charge that their subject is a jargon-ridden litany of the commonplace. It seems to be something of an occupational hazard. But the accusation cannot easily be dismissed by anyone who has ever read a book or journal article and, to their annoyance or embarrassment, found the author to be stating the numbingly obvious with a sense of profound discovery. Although ephemeral empirical issues and trendy theoretical concerns come and go in every discipline, sociology seems unhealthily prone to them. Why doesn't it seem able to accumulate theoretical and empirical knowledge?

In *Theory and Progress in Social Science*, James B. Rule addresses this problem with uncommon clarity and directness. The book has three parts. In Part I, Rule outlines the sorry state of the discipline as he sees it. In social theory, conceptual revolutions are a paradigm a dozen.

Over and over, theoretical programs arise that claim to outline the core ideas necessary to understand the basic processes of social life. And, just as surely, within a generation or so these programs ignominiously fail. Their insights are suddenly banal, their concepts vacuous, their concerns sadly misguided. Taking three examples from many contenders, Rule describes the career paths of Parsonsian functionalism, Baesian process analysis, and ethnomethodology. He contends the pattern is the same in each case: grand claims, early success, widespread popularity, and then a big crash.

Having outlined these disaster stories, Rule attempts to establish a proper metric of progress. He pitches his analysis in terms of debate in the sociology of science. In one camp, Robert Merton and his followers measure progress by the appearance of “important discoveries and inventions” (p. 50) and theoretical success by counting citations. In the other, Bruno Latour and Steve Woolgar see scientists inventing truth in their laboratories. Unsurprisingly, Rule finds something of value in both approaches but rejects their more extreme implications. Setting up dichotomies of this kind and attempting to reconcile them is a dangerous business: very often the “solution” is simply a rhetorical call for the best of both worlds.

Rule’s solution is to distinguish between “formal” and “substantive” varieties of progress. The former is defined by the internal aims and standards of a theory and is therefore inadequate for his purposes. All theories make progress by their own lights. (This is his concession to constructivism.) The latter is the acid test: substantive progress is measured by the capacity of a social theory “to generate insights with the potential to outlive the context of their origins” (p. 9). (This is his appeal to realism.) He rejects claims that one needs to learn theory before one can appreciate its wonders. Natural science has given us indispensable empirical and theoretical knowledge about the world, knowledge that one has to have no special love for physics or chemistry to need or rely upon.

In order to sharpen his criteria for substantive progress, Rule makes a second distinction between first- and second-order questions. Good social science answers first-order questions (making substantive progress as it goes). These are “perennial” problems that are “implicit in the existential tensions predictably generated by the exigencies of social life itself.” He argues that “no program of social inquiry . . . will long claim attention if it fails to address a core of historically enduring

questions” (pp. 44–45). Rule cites the causes of deviance, the conditions of economic growth, and the origins of civil violence as examples of first-order problems. New approaches start out with a fresh perspective on these problems, thereby attracting attention and popularity. But, for reasons which remain unclear, they quickly run out of steam. Part of the explanation is that theories tend to turn in upon themselves, concentrating on second-order questions of interest only to the initiated. Nothing kills a school of thought faster than obsessive attention to its own second-order problems, according to Rule.

In Part II of the book, Rule casts a cold eye on four currently popular approaches with large ambitions – Rational Choice, Neofunctionalism, Network Analysis, and Feminism – and asks if there is any reason why they should not go the way of the fashions of the past. His treatment of each theory is generally fair and often incisive, although his focus varies somewhat. The chapters on Rational Choice and Network Theory are the best. In each case, Rule compares studies to show the difference between a creative theoretical explanation of an empirical problem and a self-contained exercise that takes the importance of the theory for granted. His distinction between first- and second-order questions (and thus substantive and formal progress) is at its most convincing here. The chapter on Neofunctionalism is really a critique of Jeffrey Alexander’s work. It is certainly interesting, but it is not the promised survey of a general approach. The chapter on Feminism (co-authored by Leslie Irvine) has the opposite problem. There is too much to cover. Feminism, as Rule and Irvine acknowledge, is more of a social movement than a subfield. Their promising discussion loses its way as they carry out an ill-advised search for a single concept uniting all feminist analyses. In the end they settle on “relatedness.” Having ascribed this principle to feminist theory, they then find it rather easy to criticize: Isn’t a sensitivity to relatedness “simply good advice for any and all inquiry?” (p. 160). Indeed it is; it being so general as to be altogether vague. But this is the fault of the authors, not the feminists.

Part III returns to the concerns left in the air at the end of Part I. Rule explores a third distinction between “expressive” and “coping” aspects of social theory. The expressive virtues of a theory lie in its purely intellectual or aesthetic features: some theories capture the mood of the times better than others. Work of this kind – Rule mentions Sennet and Cobb’s *Hidden Injuries of Class* and Elton Mayo’s organizational sociology – is important because it taps some vital experience or phenomenon, even though it may be an inadequate guide to practical

action, or “coping.” Theoretical programs may shift along this axis. Freud began his work attempting to cope with the neuroses of Viennese women but ended up with an expressive theory of civilization. Rule notes that this seems to be a general tendency: at the end of structural-functionalism’s life-cycle, there was no phenomenon that could not be explained within its framework. Paradoxically, the ability to explain everything marked the death of the research program: as with psychoanalysis, it was difficult “to imagine any form of empirical observation that might conceivably count as evidence against the theory” (p. 194).

So, theoretical programs fail for two reasons. First, they lose touch with first-order questions and instead get wrapped up in problems defined by their own terms of reference. Second, theorists overplay their hands. They are prone to develop their work along expressive lines: good answers to specific problems are transformed into big metaphors for the human condition. These metaphors are powerful for a while, but then the *zeitgeist* moves on and we are left with nothing. This is why a reading of Parsons tells us more about the 1950s than it does about society in general. For Rule, theory must instead be judged by its ability to give robust, useful, non-trivial answers to long-standing questions about social life. Rule stakes this claim in the face of constructivist critics of science who, he thinks, suggest that theories can only be evaluated on their own terms. He accepts that the findings of social science are made as much as discovered, but insists that, in the long run, theories must address first-order questions if anyone is going to stay interested. “Coping” with first-order problems is the bedrock of progress. Its results can easily be shown to outsiders and they survive after the initial expressive appeal of a new approach has worn off. Most large-scale programs have some substantive, coping insight as their kernel. Rule concludes that the most useful theoretical ideas relinquish their *a priori* claims to definitive supremacy and enter a “toolbox of analytical options” (p. 220). The discouraging track record of social theory suggests that we should keep it at arm’s length, however seductive its claims. In the end, “eclecticism is the only prudent strategy” (p. 224).

Rule takes a difficult problem and deals with it in a balanced and even-handed way. He writes clear, vigorous, and accessible prose. His diagnosis is acute, and his case histories are sharp and observant. He writes as someone who is sympathetic to and knowledgeable about the project of social theory, whilst remaining unconvinced by its claims. This perspective is rarer than it should be, and all the more valuable for it. External critiques of social theory often end with hard-headed calls for

it to come down from the clouds. But it is easy to sound very sensible by advocating a down-to-earth, common-sense approach. It is more difficult to see that there is more to the problem than this. Rule knows that today's common sense is tomorrow's common nonsense. In an interesting section (pp. 32–34) he examines the claims of C. Wright Mills and Barrington Moore who, in the 1950s, made strong demands for a return to the real issues. Rule notes that, in 1958, Moore dismissed a paper based on women's experiences of sexual aggression as "an enormous diversion from more important problems" (p. 33). This small irony of intellectual history makes Rule unwilling to join those calling for more straightforward attention to "real issues" or "important problems," as though they were obvious.

However, he remains very worried about the threat of constructivism, represented by Latour and Woolgar in sociology and Feyerabend in philosophy. They point the way to a hopeless relativism. His treatment of this complex issue is the most unsatisfactory part of the book. In order to avoid relativism, Rule thinks it necessary to insist that there are such things as first-order questions. This is a strange move, especially seeing as he himself presents a very good argument that the important questions are not necessarily obvious. This dissonance is further amplified by his discussion of feminism, a paradigm case of how theory can generate new "first-order questions" rather than simply addressing existing ones. Rule is therefore uneasy about his own position: even as he offers a list of these allegedly perennial, first-order concerns, it sounds as though he does not really believe in them. He immediately reins in his claims, saying his list is "not exhaustive, nor even a representative sample." Neither could "such questions be answered in any definitive ... fashion" (p. 46). This is a long way from the "historically enduring" problems arising from "endemic tensions in social life" that we are promised on the previous page. If first-order questions are so anemic, it is hard to see why we should bother with them.

Theories should conform to certain standards, and it is important to be clear about their relationship to empirical evidence. But this does not imply that there is a list of perennial questions that theory in general must address. Rule's first-order questions are red herrings, too general to be of any real guidance. Natural science has no first-order questions – other than the empty "to find out how the world really works" – and social science does not need them either. What both require are criteria for good theory. In places, Rule suggests what these might be: clarity,

empirical accuracy, parsimony, the capacity to be constrained by the evidence. These are good standards (and open to argument: this is what the philosophy of science is for), but they do not depend on the existence of first-order questions.

Rule paints himself into a corner because the philosophical alternatives he offers are both stark and outdated: it's either Feyerabend's (and Latour's) relativism or Hempel's (and Homans's) covering laws. Neither of these will do. But rather than looking for alternatives, Rule insists that common sense tells us that there is a perennial set of questions that need answering, a strategy he has already criticized others for pursuing. His dilemma is a false one, and it becomes more oppressive as the book goes on.

How could Rule have avoided this problem? He might have moved closer to contemporary analytic philosophy of science and taken the opportunity properly to introduce sociologists to recent debates in this area. He would then have been in a position to make a more refined argument for some measure of theoretical quality and disciplinary progress. Alternatively, he might have drawn more on sociological sources. A discussion of the institutionalized practices, hierarchies, and standards of social science would have strengthened his argument about the life-cycles of grand theories. The role of the academic *habitus* could have been considered independently of philosophical issues. For example: in sociology, counter-intuitive theories and results are highly valued. In analytic philosophy, they are to be avoided. Why is this so? How does it relate to differences in the claims, style, organization, and standards of each discipline?

Rule leaves the conceptual question of progress in social science unsettled, and neglects the institutional bases of success and failure. This is because the book's analytic framework is refractory to its otherwise valuable cases and observations. However, he does make an insistent and entirely compelling case for changing the way that the claims of social theory are presented. His critique of current programs implies that "elaborating a theory" should *not* mean finding a functionalist, rational choice or network explanation for every available phenomenon. This sort of elaboration has the effect of rendering the theory useless by draining the blood from its central insights and turning them into tautologies. Rule is at his most effective when attacking hubris. By lining up the paradigm shifts, unified frameworks, and ultimate insights, he deflates them. It is difficult not to agree that the claims of

theory exceed its achievements to an embarrassing degree. Efforts at “theoretical seduction” (p.196) do far more harm than good, leading theorists toward expressive rather than practical rewards. In short, social theory needs to be more modest. Although unsatisfactory in some respects, Rule is well worth reading – not least because he takes his own medicine. The book’s pleasing candor and lucid style recommend it as an antidote to the often beguiling but usually misleading claims of social theory.

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