ASA Theory Section
Debate on Theoretical Work, Pluralism, and *Sociological Theory*

Below are the original essay by Stephen Sanderson in *Perspectives*, the Newsletter of the ASA Theory section (August 2005), and the responses it received from Julia Adams, Andrew Perrin, Dustin Kidd, and Christopher Wilkes (February 2006). Also included is a lengthier version of Sanderson's reply than the one published in the print edition of the newsletter.

REFORMING THEORETICAL WORK IN SOCIOLOGY: A MODEST PROPOSAL

Stephen K. Sanderson

Indiana University of Pennsylvania

Thirty-five years ago, Alvin Gouldner (1970) predicted a coming crisis of Western sociology. Not only did he turn out to be right, but if anything he underestimated the severity of the crisis. This crisis has been particularly severe in the subfield of sociology generally known as "theory." At least that is my view, as well as that of many other sociologists who are either theorists or who pay close attention to theory. Along with many of the most trenchant critics of contemporary theory (e.g., Jonathan Turner), I take the view that sociology in general, and sociological theory in particular, should be thoroughly scientific in outlook. Working from this perspective, I would list the following as the major dimensions of the crisis currently afflicting theory (cf. Chafetz, 1993).

1. *An excessive concern with the classical theorists*. Despite Jeffrey Alexander's (1987) strong argument for "the centrality of the classics," mature sciences do not show the kind of continual concern with the "founding fathers" that we find in sociological theory. It is all well and good to have a sense of our history, but in the mature sciences that is all it amounts to – history. Let's face it, we have probably extracted all of the value contained in the work of the masters; it's time to take what is of value, discard the rest, and move on to build new theories that can be tested empirically.

2. *An excessive concern with "chic" European theorists*. In a 1994 survey of Theory Section members conducted by Jane Lord and me (Lord and Sanderson, 1999), we asked members to give their impressions of who were the most important current theorists. The top three were Jürgen Habermas, Pierre Bourdieu, and Anthony Giddens, with scores of, respectively, 246, 200, and 190 (based on 3 for a first-place vote, 2 for a second, and 1 for a third). All of these thinkers are Europeans and none is known for an especially scientific outlook or for conducting anything resembling rigorous scientific research (Bourdieu, however, did a lot of ethnographic work in his early years). Of the list of theorists that respondents were given, two sociologists who have, in my judgment, done excellent theorizing combined with serious empirical work, Theda Skocpol and Gerhard Lenski, scored only 21 and 16 points, respectively. These results suggest that what now passes for "theory" in Western sociology is largely the musings of highly abstract, largely nonempirical thinkers – not how one should properly go about generating a real understanding of how societies work.
3. *The construction of highly abstract models that explain everything but then nothing*. Of course, the leader in this regard was Talcott Parsons, who had no peer when it came to building extremely abstract theoretical systems that bore precious little relation to empirical reality or to explaining concrete social phenomena. But this is also true, although perhaps to a lesser extent, of Habermas, Bourdieu, and Giddens. Genuine sociological knowledge is much more likely to be produced through the formulation of more modest sets of propositions that are focused on specific substantive phenomena and that can be subjected to empirical test (e.g., Black, 1976; Stark and Finke, 2000; Turner, 2003).
4. *A shift to a nonscientific or even antiscientific mode of sociological theorizing*. Many theorists have come to reject the time-honored notion that science represents a privileged road to knowledge, embracing a strong epistemological relativism. Theorists of this persuasion wish to connect sociology more to philosophy or even to literary criticism than to the other social sciences, and certainly not to such natural sciences as neurobiology or cognitive science. However, this can only be a regressive move. The postmodernists notwithstanding, science has achieved enormous understanding of the natural world, and neurobiology and cognitive science are making enormous

contributions to understanding human behavior. The challenge is to do sociological science better, not to abandon it.

5. *Extreme politicization*. Sociology in general, and theory in particular, has become increasingly politicized in the last two decades, and for many theorists the purpose of theory is radical social transformation (neo-Marxism, feminist theory, "whiteness" studies, and "queer theory," in particular, come to mind, but there are many other examples; cf. Seidman, 1994). Objectivity is decried as an impossibility. But my response would be that, while complete objectivity is indeed impossible, one can still hold to it as a goal to be approximated. It seems to me that it is those who decry the possibility of objectivity who are most incapable of it. Since they know they cannot be objective – honest in face of the facts, unpleasant though these facts may sometimes be – they overgeneralize from themselves and consider every scholar incapable of objectivity. But it just isn't so.
6. *Incorporating nontheorists into "theory*." This has become something of an industry of its own in recent years, and many examples can be cited. But to take just one: In Charles Lemert's celebrated *Social Theory: The Multicultural and Classic Readings* (1993), along with many classical theorists and a few contemporary ones he lists Virginia Woolf, M.K. Ghandi, Mao Zedong, Martin Luther King, Jr., Betty Friedan, Gloria Anzaldúa, and Vaclav Havel. These people are either political or literary figures. Sociologists they are not, and certainly not "theorists" in anything more than an exceptionally loose and casual sense of the term. The thinkers seem to be treated as "theorists" because they have politically relevant thoughts that those regarding them as "theorists" like. Little else.
7. *Hermetic isolation from the rest of sociology*. It has been said many times, but it bears repeating, that theorists today talk mostly to each other. Many of them write abstruse and arcane books and articles that only they can understand; they disengage not only from the real world of actual social life but from what the vast majority of sociologists are doing. This kind of academic inbreeding can only be a prescription for disaster, and indeed that is exactly what it has been.

What, then, is the solution to these problems? Most scholars today identifying themselves as both sociologists and theorists are, I suspect, quite unlikely to be persuaded that the problems I have identified are, in fact, problematic. I have little or no hope that I can persuade them. They will want to go about their business in the way that they have been. Most of these people are what I would identify as *social theorists*, and they often so identify themselves (e.g., Lemert, 1993). Social theorists see themselves as social commentators and critics and as formulating theoretical critiques of modern society as much as, or more than, explaining social life. They are usually not committed to a scientific sociology and are often strongly opposed to it. Their goals are primarily or even exclusively political. Of the list formulated by Lord and Sanderson for Theory Section members to choose from, the social theorists would be Habermas, Bourdieu, Giddens, Erving Goffman, Michel Foucault, Dorothy Smith, Alfred Schutz, Jeffrey Alexander, and Jacques Derrida.

In contrast to social theorists, there are what are probably best termed *sociological theorists*. Sociological theorists are less concerned with criticizing and rebuilding society than with understanding it. They tend to be committed to a scientific sociology, at least in the broadest sense of the term. They may do general theory, or concentrate on formulating specific theories of particular substantive phenomena, and in some cases combine the two. Sociological theorists on the Lord and Sanderson list were Talcott Parsons, Robert Merton, Randall Collins, James Coleman, Peter Blau, Immanuel Wallerstein, George Homans, Harrison White, Theda Skocpol, Gerhard Lenski, Pierre van den Berghe, and Janet Chafetz. Someone who should have been on the list but was inexplicably omitted, Jonathan Turner, would clearly qualify as a sociological rather than a social theorist, and perhaps more so than anyone else. Three scholars on the list – Harold Garfinkel, Herbert Blumer, and Claude Lévi-Strauss – are somewhat difficult to place clearly into one category or the other.

What is the proportional representation of these three forms of theory in the pages of the leading theory journal, *Sociological Theory*? I surveyed every issue of the journal from the time it became a regular paper journal in 1986 to the close of Jonathan Turner's editorship at the end of 2004. The results are most interesting. Norbert Wiley edited the journal from 1986 through 1989. Under his editorship, the three forms of theory were fairly evenly represented, with 35 percent of the articles falling under the heading of sociological theory, 33 percent under theorizing about the classics (including such "late classical" theorists as Parsons), and 27 percent under social theory, with about 6 percent of the articles either difficult to classify or overlapping into both social and sociological theory. Things changed dramatically under the editorships of Alan Sica and Craig Calhoun (1990 through 1999), with a very strong bias toward social theory

(51 percent of the articles), and, secondarily, theorizing about the classics (25 percent). Only 19 percent of the articles published during the ten years these two sociologists were heading the journal could realistically be called sociological theory (about 5 percent were hard to classify or overlapping). Then, when Jonathan Turner came on the scene in 2000, things shifted dramatically toward sociological theory, with 55 percent of the articles falling within that category compared to only 26 percent falling within social theory and only 13 percent within the category of theorizing about the classics (8 percent were difficult to classify or overlapping). This will not be surprising to knowledgeable Theory Section members, since Sica and Calhoun are well known for favoring social theory and classical theorizing, and Turner is without doubt the most vigorous advocate of scientific theorizing among today's leading theorists. If we take a grand total, we get the following results: 39 percent of the articles published fell within social theory, 34 percent within sociological theory, and 21 percent under theorizing about the classics (6 percent were ambiguous or overlapping). This means that, throughout the lifetime of *Sociological Theory*, scientific sociological theorizing represents only about a third of all articles published. This confirms my own impressions over the years as to the kinds of articles that tend to make their way into this journal.

I stopped the survey just before Jeffrey Alexander and his colleagues at Yale assumed the editorship with the latest volume, but if the past is any indication of the present and the future, then we can expect a strong shift back toward social theory and theorizing about the classics. For those of us advocating scientific sociological theorizing, Turner's years were a sort of "golden age," but now we likely face several years' worth of issues filled with articles that do not interest us in the least.

What can we scientific sociological theorists do about this state of affairs? My solution is the following. I propose that the distinction between social and sociological theory become institutionalized within sociology, and that this institutionalization be reflected in the structure of the journals (which, of course, is a fundamental part of what institutionalization means in academia). Divide *Sociological Theory* into two journals, one to be called *Social Theory* and the other to be named *Theoretical Sociology*. Sociologists wishing to continue to do social theory would submit their work to *Social Theory*, whereas the more scientifically minded sociological theorists would publish in *Theoretical Sociology*. As for those who continue to insist on the centrality of the classics and who do not want to abandon their exegeses and elaborations of them, create yet a third journal, called the *Journal of Classical Sociology* or *Journal of the History of Social Theory*, that is to be exclusively devoted to such work. Since it is likely to attract fewer contributions, publish it only twice a year, in contrast to three or four times a year for the other two journals.

In closing, let me not keep my agenda hidden. I am a sociological theorist strongly committed to building general theory, but a general theory that has many subtheories that can be used to develop specific theoretical propositions for empirical testing (cf. Sanderson, 2001). I am a Theory Section member and I regularly read *Sociological Theory*, but I am, quite frankly, tired of encountering in its pages abstruse and arcane articles, often filled with pretentious Gallicisms, that seem to go nowhere and that have little or no relevance to explaining social life. To my social theory colleagues, and my colleagues who cannot get the classical theorists out of their system, I say, may the force be with you, but please, no offense intended, could you just go somewhere else to ply your trade and leave the rest of us alone to ply ours. I am a strong believer in letting all voices be heard and in maximizing discourse and debate, but that doesn't mean I want to listen to all of these voices or to treat them as the same thing I think I am doing (although I still might check in on them occasionally to see what they are up to).

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WHY PLURALISM STILL MAKES SENSE FOR *SOCIOLOGICAL THEORY:* REPLY TO STEPHEN SANDERSON

Julia Adams

Yale University

In his recent *Perspectives* article, “Reforming Theoretical Work in Sociology: A Modest Proposal,” Stephen Sanderson calls for two things: founding a new theory journal to give “scientific” sociological theory a more hospitable home outside of “the leading theory journal, *Sociological Theory*,” and the general reformation of theorizing in the discipline. I’ve my thoughts about the latter – who does not? – but it’s really the former that needs to be addressed here. As one of the current editors of *Sociological Theory*, I worry that Sanderson’s article might lead people to believe that the journal is in some way preemptively closed to their submissions. Nothing could be further from the truth.

There are two basic reasons why Sanderson has such a low opinion of and blighted hopes for *Sociological Theory*. The first is that his preferred style of theorizing – which he dubs “a general theory that has many subtheories that can be used to develop specific theoretical propositions for empirical testing” (2005, p. 4) – is represented in, by his count, 34% of the journal’s articles published between 1986 and the end of Jonathan Turner's editorship in 2004. The other approximately two thirds consist of two categories of which he disapproves, so-called "social theory" and classical theorizing. Second, he assumes that the current editors – that is, the Yale contingent Phil Gorski, Ron Eyerman, Jeff Alexander and myself – will necessarily make matters even worse. Thus Sanderson predicts that "we likely face several years' worth of issues filled with articles that do not interest us in the least" (ibid., p. 3). These, one must assume, are the "abstruse and arcane articles, often filled with pretentious Gallicisms, that seem to go nowhere and that have little or no relevance to explaining social life" (ibid., p. 4).

Here readers should imagine a deleted paragraph, decorated with highly Frenchified exclamations and asides, not to mention Gallic epithets hurled at Stephen Sanderson for his old-fashioned patriarchal description of the current editorial collective as "Jeffrey Alexander and his colleagues at Yale" (ibid., p. 3) – mon dieu! (or, rather, ma déesse!), Now, back to sober Anglo-American argument…

Even on the basis of the eight already-published issues in volumes 22 and 23 (not to mention the forthcoming issues that are already in the hopper), it will be obvious to any serious reader of *Sociological Theory* that Sanderson's gloomy predictions have not been borne out. The journal is publishing all forms of theorizing current in our discipline. Substantively-embedded and formal theories; decomposition equations and constructive textual exegeses; the gamut of stances on the desirability of arriving at general laws versus specifying the particularities of processes in time and place – to name just a few dimensions of differentiation – all are welcome. We, the editors, are bending over backwards to make this pluralism possible both on principle and because we think that this best suits this period of ongoing theoretical deconstruction, reconfiguration and experimentation. One reading of Sanderson's intervention, however, is that such pluralism is undesirable. (His "could you just go somewhere else to ply your trade and leave the rest of us alone to ply ours" (p. 4) can't be readily understood in any other way.) ASA journal editors don't really have the professional-ethical luxury of simply declaring substantial parts of the field a no-go zone, however, so *Sociological Theory* won't be following his injunctions to purify the collective theoretical territory anytime soon.

I also believe that there are positive scientific reasons to prefer theoretical pluralism and cross-cutting theoretical conversations right now. Quite a few of the articles that come across *ST*'s desk are *internally* plural, even in the limited sense that they could be simultaneously located in two or even all three of Sanderson's idiosyncratic categorical slots. Clearly the arguments that many sociologists are now pursuing and their capacities to explain and interpret what they see are nourished by these very intersections. Conversely, it can take several linked forms or families of theory to pose or solve a given theoretical problem. This holds not only in our disciplinary theoretical space but even within the span of single articles – as readers who consult the last couple of issues of *Sociological Theory* will see. I think that the attempt to quarantine or cordon off one sector of contemporary sociological theory will fail on these practically generative bases alone. If we take submissions to *Sociological Theory* as one barometer, today's sociologists are drawing some of their best theoretical energies from the intersections among paradigms, fields, and heretofore distinctive modes of theory.

That said, it is probably only certain kinds of cross-cutting conversations and forms of theory that bedevil Sanderson. He is all for connecting sociology to "such natural sciences as neurobiology or cognitive science" (p. 2). The problem is rather the potential association with the humanities, with "chic" European theorists, and with neo-Marxism, feminist theory, "whiteness" studies, "queer theory" and their ilk. His antipathy to these wildly various modes of thought goes beyond not wanting to read about them (which would have been the reasonable response) and extends to wanting them out of the journal and out of his sight. One sees an analogous fearfulness of culturalist contamination and pollution in a few sectors of comparative historical sociology these days, as Elisabeth Clemens, Ann Shola Orloff and I show in our introduction to our edited volume *Remaking Modernity* (2005: 44-45). It's as regrettably shortsighted – and as unscientific – as would be the refusal to consider the intersection among the social, intrapsychic, and neurobiological aspects of human life.

It may be, someday, that sociology transcends or relinquishes its status as what Mayer Zald called "quasi-science quasi-humanities" (Zald 1991-2). It may also be that sociological theorizing comes to center around a single scientific paradigm. We can all expect *Sociological Theory* to reflect that new disciplinary consensus if that day comes. Meanwhile, in my and my fellow editors' view, the journal should be open and pluralistic in its orientation.

Of course what *Sociological Theory* publishes is only as good as what people send in, from all corners of the discipline. So far that has been superb. Please keep it up!

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INTEGRATING THEORETICAL SOCIOLOGY: A REPLY TO SANDERSON

Andrew J. Perrin

University of North Carolina, Chapel Hill

It is a staple of deconstruction—presumably one of the "obscure Gallicisms" Professor Sanderson dismisses in his polemic (Sanderson 2005, henceforth *RTW*)—that "the text deconstructs itself" (Derrida 1985). A great irony therefore arises: the principle's veracity is demonstrated by the very critique Sanderson levels. It is, essentially, the fundamental irony of a polemic against polemicism: an argument for pure science that cannot be sustained without recourse to the very type of normative injunction against which it inveighs. In what follows, I will demonstrate this breakdown in *RTW's* argument and suggest, instead, a substantive reason for the coherence of what now counts as theory in sociology.

RTW begins by citing Alvin Gouldner (a self-identified "social theorist") as predicting a crisis in western sociology. The prediction was apt, we are told, as Gouldner "turn[ed] out to be right," if insufficiently pessimistic. But Gouldner's critique shared little substance with the argument in *RTW*; rather, he called for "a Reflexive Sociology [that] is and would need to be a radical sociology. Radical, because it would recognize that knowledge of the world cannot be advanced apart from the sociologist's knowledge of himself and his position in the social world, or apart from his efforts to change these" (Gouldner 1970: 489).

Professor Sanderson begins with the admission that the basis for his argument is simply personal preference: "I take the view that sociology in general, and sociological theory in particular, should be thoroughly scientific in outlook." That view, though, remains undefended, and certainly does not reflect either the historical practice or the contemporary consensus of the field. Hence the remainder of *RTW*'s argument fails unless the reader simply accepts the principle as given.

The article is full of similar tropes and blanket statements. "Mature sciences," we are admonished, do not hearken back to their "founding fathers." This claim is based on self-identity, not content: whom we want to be like, not what research advances our knowledge. What we need instead are "new theories that can be tested empirically"—again, an undefended preference for a rather naïve Popperian approach to science. Indeed, contemporary theories of knowledge suggest a rather more skeptical view of empirical testability! Of course explanatory theories may be evaluated as "better" or "worse" than one another, at least in contingent ways, but we know too much about the ways measurement, communication, and categorization inject uncertainty into the observation process that the distinction between testable and non-testable theories is, at best, blurred.

I am tempted to make a "big-tent" argument, akin to the one that has worked remarkably well for the Republican Party in the field of American politics. In this calculus, interpreters of classical social theory, contemporary social theorists, and formal sociological theorists agree to coexist under the institutional banner of the Theory Section (and the journal *Sociological Theory*) because they recognize that each can expect greater institutional resources that way than if they go their separate ways. Without seeking to draw specific comparisons, this is essentially what economic libertarians, right-wing Christians, and big-business hacks have done in the Republican Party: none has much patience with the others' general approaches, but they coexist because each expects to gain from the institutional success of the Party.

According to such an argument, we would agree to hammer out factional disagreements within the fora of the Section in the service of securing common gains. Theory is, to paraphrase Forrest Gump, as theory does. But while it may be instrumentally appealing, that argument is intellectually unsatisfying. I contend, instead, that there is a reason for us to understand these three practices as three branches of a common theoretical enterprise.

The categories *RTW* presents (social theorists as opposed to sociological theorists) are quite unstable. Among the social theorists, every example given has presented at least one theoretical discussion of a social phenomenon, *distinct from social critique or commentary.* Goffman, for example, offers a dramaturgical metaphor and its accompanying theory of audience, front stage, backstage, and performance as claims (albeit not formalized ones) about the expected behavior of individuals in social settings. Ditto each of the others, with the possible exception of Derrida, whose relationship to sociology is tenuous.

Similarly, among the listed sociological theorists, many have "formulat[ed] critiques of modern society." Certainly Coleman, Blau, Wallerstein, Skocpol, and Lenski, at various times, have written important works that blend sociological analysis with political partisanship. Rather than the one-way distinction suggested in *RTW,* I suggest sociology's trademark two-way table, which would categorize theoretical work (not, incidentally, theorists) according to two axes: one identifying the work as normative as opposed to explanatory, the other as formal vs. informal:

	Normative	***Explanatory***
Formal		
Informal		

Readers are left to their own devices to slot exegeses into cells.

Most importantly, though, I submit that each of our three branches contributes to a critical, dialogical process by which sociology's three big elements—theory, method, and substance—inform one another. Without grand, informal theory, explanatory, formal theory is left without a basis for generating hypotheses. Without normative theory, formal and informal explanatory theories alike are irrelevant conceits. Without understanding the classical roots of contemporary theory, we are left unable to evaluate new claims and concepts in the context of those that have succeeded (and failed) in the past. Without the careful implementations of formal theory, we are left guessing as to the relative merits of informal grand or explanatory theories. And without the "pretentious Gallicisms" that have taught us to own up to uncertainty and be suspicious of claims to total knowledge, we might accept the products of formal theory as providing more direct access to social reality than is warranted.

Since Durkheim and Mauss (Gauls, yes, but I hope not pretentious ones), we have known that categorization is an immensely powerful cultural process, and that the drawing of conceptual boundaries carries enormous cognitive weight (Durkheim and Mauss 1963; for a very different version of the same claim, see Allen 2004: 56). What can we observe? How do mediating influences distort our view of what we claim to observe? How can we account for contingency, uncertainty, and inconsistency in observations? These are questions that, by their very epistemological nature, cannot be formalized. Yet they constitute profound problematics for sociological theory: problematics that have been addressed, contingently and imperfectly, by generations of social theorists past.

The conclusion of *RTW* suggests that, as Professor Sanderson is “tired of... [the] abstruse and arcane articles” he encounters in *Sociological Theory,* scholars whose work he dislikes should depart the subfield for exile in ever more specialized sections and journals. “Can I have my ball back?” he effectively says; “I want to go home.” But the need to keep talking is not simply motivated by “letting all voices be heard,” but rather by the fact that, regardless of the fatigue it may cause some formal theorists, each of these branches really does constitute an integral piece of sociological theory.

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CLOSED AND OPEN-FORM APPROACHES TO SOCIAL THEORY: A PEDAGOGICAL REJOINDER TO SANDERSON

Dustin Kidd
Temple University

According to Stephen Sanderson, a crisis is upon us—a crisis felt most acutely in the area of social theory. Sanderson's article in the last issue of *Perspectives* insists that sociological theory must be "thoroughly scientific in outlook" and that its failure to retain rigid scienticity has brought us to the brink of disaster; indeed, we may be over the edge already. I assume that all members of the Theory Section found Sanderson's article to be provocative and that it encouraged all of us to think about our own work and to wonder whether we think Sanderson is right, and why.

As someone who is currently teaching a graduate course on social theory, and who also frequently teaches undergraduate theory, I found myself thinking especially in terms of what I *teach* about theory. From my own theory training, I am aware that some approaches to theory are quite closed—limited to strictly scientific approaches in most cases—while others are very open. The open approaches are often interdisciplinary, recognizing that social theory is produced across the social sciences, and can also be found in any of the humanities. The most open approaches even go so far as to incorporate non-academics into the discussion, including the theoretical discussions of such folks as political leaders and producers of culture.

I have certainly embraced this open approach, and that choice is heavily reflected in the syllabus for my graduate seminar on contemporary theory (which can be found on the resources page of the Theory Section website). Below, I respond to each of Sanderson's critiques of contemporary theory, with a particular eye to this open-form approach in the theory classroom.

Critique 1: An excessive concern with the classical theorists.

My course being focused on contemporary theory, this isn't exactly true for us, but we do make several nods to classical theorists such as Marx, Weber, Tocqueville, and Durkheim. This is especially true at our first meeting, when we discuss a "Neo" for each of those four classical theorists. If society were static, then perhaps we could argue that these theorists were no longer necessary (though I doubt it). But in an ever-changing society, the ideas of theorists past are always finding new applications. Commitment to their work saves us from re-inventing the proverbial wheel.

Critique 2: An excessive concern with "chic" European theorists.

Sanderson lists in particular Habermas and Bourdieu, both of whom are included in my syllabus, and Giddens, who is not separately included, but whose work is heavily discussed. I don't think these folks particularly chic, and I doubt you could say they are given excessive attention in my syllabus, but I do agree that they get perhaps too much attention in current curricula at the expense of other important theorists. In turning away from this short list of European theorists, I doubt Sanderson and I would turn in the same direction, but I do feel that many excellent theorists are given short-shrift as a result of our fascination with a few.

Critique 3: The construction of highly abstract models that explain everything but then nothing.

It is unclear what is referred to here, but I suspect he is especially suspicious of conceptual work. This would include discussions of what exactly we mean by such terms as structure, agency, and culture, for instance, which are areas of concern that my syllabus gives a lot of attention to. This work does not really advance sociological knowledge but it does improve the ways that we make sense of such knowledge. I would argue that it is an important version of theory formation that improves communication across the social sciences and makes for more careful empirical research.

Critique 4: A shift to a nonscientific or even antiscientific mode of sociological theorizing.

To the limited, but real, extent that this is true, it is actually a result of the sociological observation that the association of science with knowledge and truth is a social construction (but still real) that is worth comparing to other ways that humans can determine some information to be valid knowledge. Sanderson's claim is based on an assumption that the physical sciences are the standard bearers of determining what counts as science. I hasten to point out that biology and physics, unlike social science, are not studying subjects that are infused with meanings. In other words, the interpretive tradition that we trace to Weber demands a different set of scientific standards than that which we could import from our colleagues in the physical sciences.

Critique 5: Extreme politicization.

Some politicization has certainly taken place, though whether we can call it extreme is arguable. I could certainly imagine taking it a lot further. Sanderson's insistence that we hold objectivity as a goal is noble and appropriate, but politicization is not the problem here. The barrier to objectivity is actually derived from a lack of engagement with real people and real social situations. Political engagement with the world, I would argue, actually increases our objectivity, simply for making us more connected to and identified with the social worlds we study. In other words, objectivity is not a matter of being apolitical. Rather, objectivity is determined by the extent to which we have a legitimate relationship with the material we study that allows us to make careful and nuanced observations.

Critique 6: Incorporating nontheorists into "theory."

It is significant that Sanderson targets Charles Lemert's book *Social Theory: The Multicultural and Classical Readings*. This critique has implications for issues of race, gender, class, and nationality that deserve to be addressed head-on, and not through the backdoor assault on "nontheorists." My own curricular approach is derived from the work of Patricia Hill Collins, bell hooks, Henry Giroux, and Antonio Gramsci, and begins with the assumption that all human actors are intellectuals who engage society within a socio-theoretical framework. The goal of social theory as a field is to make these many theories explicit and place them alongside each other for purposes of analysis and assessment. Many so-called nontheorists are simply folks like the Black feminists in Patricia Hill Collins's work who record knowledge in ways that are very different from the institutionalized traditions that are canonized as "sociological theory"—whether because they are excluded from social institutions or because these other forms are simply preferred over articles in *Sociological Theory* and other journals.

Critique 7: Hermetic isolation from the rest of sociology.

Here, I am in complete agreement with Sanderson. Social theorists need to be engaged in research themselves, and they need to be in conversation with scholars across the varieties of sociology, across the varieties of social science, across the university, and well beyond the university. In fact, I do not think theory can be classified as a sub-field any more than methods could be classified as such. Every sociologist proceeds with theory and methods in hand; the sub-fields are those areas to which we apply our theories and methods as we address interesting questions of social research.

Sanderson never specifies what this looming crisis looks like or what the consequences are. As far as I can tell from what he does say, the only problem he has is the annoying experience of reading journal articles that are not directly relevant to his work. That annoyance extends well beyond the realm of theory, and is really a minor occupational hazard faced by every academic.

For a closing point, I also would say that Sanderson's finding that theory journals are equally split between scientific theory, social theory (his term for non-scientific approaches to theory), and new translations and applications of classical theory—and the finding that they toggle between privileging scientific and social theory—strike me as reasonable, pluralistic, and democratic. In the absence of an authentic crisis, I would suggest that we take the opportunity afforded by this pluralism to get to know an approach to theory that is different from our own training.

* * *

A MODEST PROPOSAL INDEED

Christopher Wilkes

Pacific University

I want to comment on Sanderson's article (Reforming Theoretical Work in Sociology: A Modest Proposal, *Perspectives,* vol. 28, no. 2, August 2005), which speaks to the nature of science in sociology, and the scope of social and sociological theorizing. It seems to me that much is at stake in what he says, and that the topic is worthy of very serious reflection. And while what I say may have no effect at all on the practices of our discipline, it is nonetheless valuable to spend at least a moment considering the impact that a certain view of science will have on our discipline.

It is a commonplace to say that American sociology in its most respected and iconic form (*ASR*, *AJS*, *Social Forces*) is deeply positivistic, and closer examination shows it to be strongly wedded to various forms of regression analysis as the "purest" and most clearly rewarded form of science. An informal review of five years of these journals' issues (1995-2000) reveals that over 90% of the articles in these journals during this period adhere to this model of sociological science. These publications count most for tenure, promotion, prestige and other forms of reward. Publication here assures the author of the highest levels of recognition, delivers the most valued form of capital which is transportable, and can be cashed in for the usual kinds of benefits. And while there are brilliant examples of sociological work to be found here, I want to argue that our presently narrow vision of our science has serious dangers. My claim in what follows is that the structuring of our science in this way robs sociology of much of what is valuable, limits its scope to what is either interesting or useful, and denies important changes in epistemology and philosophy during the last forty years which could suggest to us new ways forward.

Sanderson begins (Claim 1) by asserting that we spend too much time with the history of the discipline. I think we spend too little. Implicit in his argument is that there is an agreed form of science, that a certain kind of empirical testing is what's most important, and the rest can be thrown away. The dialectic between theory and evidence is certainly important, but the error, in my view, is made when one form of "testing" is invoked contra all the other possibilities, which earlier generations of sociological workers manifested in their studies, and which brought the discipline to life. To discard all these ideas for the sake of a narrow positivism robs us of much of what is interesting and creative in sociology, and reduces our work to a branch of social statistics.

Then, a mild form of anti-Europeanism creeps in. In Claim 2, we sociologists are charged with being too fashionable and being in love with "chic" European sociology. This is hardly a full-blown prejudice, but it is subtly ethnocentric. Again, it is claimed that non-empiricism is the problem. This form of thinking again mis-specifies the nature of science itself, locking our scientific self-conception into 19^{th} century visions of a hypothetico-deductive paradise (or an inductive logical positivism—take your pick) which eschews the hard-to-measure, the ill-structured, and the non-statistical. Thus culture, emotions, attitudes and all forms of the subjective are jettisoned, and the remainder of what passes for human life is shoe-horned into poorly fitting measurements, and forced to do duty as the raw product in the relentless machine of empiricism. Sanderson then examines theories which explain everything and nothing, notably in Parsons' work (Claim 3), and instead proposes a science which explains more and more about less and less. The logic of the science he advocates means we will need not to talk to too many people, and be able instead to concentrate on smaller and smaller regions of the social world, about which we can be absolutely sure of our measurements and statistics, until at the limit we will know almost everything about almost nothing.

It seems entirely reasonable, however, to agree with him that "science" might be a privileged road to knowledge (Claim 4), but a better question should examine what science itself might mean. Here Sanderson reveals his love for the natural sciences, and his hostility to the philosophical and literary traditions. But from Kuhn (1962), who demonstrated how *social* science is, to Bhaskar, whose critical realism developed our most sophisticated view of the multi-layered nature of the social world, to Jameson's emphasis on the "Cultural Turn," to Butler's reinvention of the social subject, to Foucault's history of the knowledge claims of science, we are provided with new avenues for our empirical work based on thoroughly reasoned, though newly-formed, scientific principles. A larger vision of sociological science is opened up to us. And Sanderson is good enough to acknowledge that Bourdieu, at once an impressive philosopher and ethnographer, and an analyst of the literary and artistic fields, is an empirical researcher. Perhaps here something can be learned—a sociological worker who invoked philosophy, literature, aesthetics, ethnography and quantitative reasoning to develop deeply empirical accounts of the social world. But this is not the science that Sanderson is proposing.

Claim 5 bemoans "extreme politicization" and makes disappointingly naive arguments about objectivity. But as Bourdieu and others have argued in compelling fashion for some time, to speak of objectivity in this way is also to claim for science a view of the social world that is beyond society, beyond debate, beyond subjectivity, beyond dispute. It is, in short, the science of the perfectly pure, the "cultural" made "natural," as Barthes put it 40 years ago, a view that most sociology has been at pains to question in the first lecture of Sociology 101. Then (Claim 6), it seems that others have been talking about the social. In another step in the exclusionary process, we are asked to throw out all novelists, politicians and others who are not fully-paid-up members of the ASA. Are we really so proud of our science that we can afford to ignore most of the great thinkers of our era, simply because they don't adhere to the hypothesis testing model which we teach in our graduate schools? In Claim 7, Dr. Sanderson reverts back to the old sore of separating out the social theorists (critics, rabble-rousers, non-empiricists) from the "true believers." In this final act of purification, Sanderson has now cleansed the discipline of everyone who doesn't think like him, and urges us to go find our own journal and to stop bothering him with "pretentious Gallicisms." "Could you," he asks plaintively "... go somewhere else to ply your trade and leave the rest of us alone?"

Here's what I think will happen if we leave sociology to the Sandersons of this world. We'll be stuck with a science which can only deal with a small amount of the social material that it needs tohandle and explain. Our best students flock to cultural studies, postmodernism and revisionist history (not to mention business, journalism, social activism, social work and global back-packing) because they're bored with the desiccated form of scientism that we insist upon. Does anyone but a statistician really enjoy the major journals any more? Do we really leap to them full of anticipation and enthusiasm, expecting fabulous insights and understandings? Or have we limited our expectations to being bored for the right reasons, because science is "hard," but science tells the truth, and a certain amount of boredom is acceptable in the rigorous world of high analysis?

Instead, it's essential that sociology rethinks its science, rediscovers philosophy, and rebuilds its understandings, not around the vagueness and uncertainty of postmodernism, but around realist philosophy, and the enduring questions of inequality, power, money and culture, which have always been our central concerns. We need to be open to much broader influences than in the past. For example, are we really prepared to suggest that Jane Austen has nothing to say about power, wealth and property in 18th-century England because she lacked training in regression analysis? Or that Dickens provides no useful observations about the rise of capitalism? There are many similar examples. But we do this all the time—we laugh openly at the twittering of the literary contingent, sneer at philosophy, ignore politicians with a wave of the hand as biased observers, and dismiss ethnography as soft science. This is a damaging and arrogant position to take, and it harms our possibilities. Only by expanding our view of science, by extending what counts for rigorous analysis, rather than narrowing our scope of inquiry, can we hope to fulfill the promises of the discipline to explain, to understand, and to ameliorate the social conditions that surround us every day.

Of course, it's likely we won't do any of this. I am pretty certain that the Sanderson view will prevail. I don't see the editors of the major journals falling over themselves to change direction. The reward system which undergirds the structure of the discipline will see to that. Sociology in the Sanderson view is a cult based on rigid theories of exclusion. So it will be no surprise when some of our best thinkers, our most promising researchers, and our hopeful graduate students slip away to do something else. Because if the doors remain closed to new ways of thinking, to the possibility of reinventing our science, then I am afraid we are consigned to becoming really very good at something nobody will much care about.

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REFORMING THEORETICAL WORK IN SOCIOLOGY: A DETAILED REPLY TO MY CRITICS

Stephen K. Sanderson
Indiana University of Pennsylvania

I am delighted that my article on reforming theoretical work in sociology elicited such interest and so many responses. The many interesting points raised by the critics give me an opportunity both to clarify some of my arguments and to expand on others. Because of space limitations, I shall concentrate on those I consider of greatest significance and likely to be of greatest interest to the members of the Theory Section. (I am not going to respond to silliness, such as Julia Adams's claim about my alleged "patriarchalism," or Andrew Perrin's cute little point about how "the text deconstructs itself." If true, then this applies to his text as well as to mine and is, like so many postmodernist arguments, self-refuting.) Let me focus on four main themes in the essays of the critics.

1. The Nature and Practice of Theoretical Pluralism (Or, Will the Real Pluralists Please Stand Up). Let me say at the outset that Adams is quite mistaken when she asserts that I disapprove of exegeses of the classics or of any form of theorizing that is not scientific. I do not. I simply think that we will get a lot further a lot faster in terms of real sociological knowledge if we concentrate on building propositional theories and testing them empirically. To prove beyond any doubt that I do not disapprove of exegeses of the classics, in fact I have done such work myself and continue to do it. In 1990 I published a book on the history of evolutionary theorizing in sociology and anthropology (Sanderson, 1990). Much of that book was exegetical and it included ample discussions of such classic thinkers as Herbert Spencer, L. H. Morgan, E. B. Tylor, and Marx and Engels. I have just completed a major revision of that book (Sanderson, forthcoming) and have added an entire chapter on classical evolutionism, this time treating classical thinkers – L. T. Hobhouse, William Graham Sumner and Albert Galloway Keller, and Edward Westermarck – who did not get any discussion the first time around. One of the things I discovered upon reading the works of these other classical evolutionists is that they have anticipated theories now being developed in the social sciences – in Keller's case, natural selectionist theories of social evolution, and in Westermarck's sociobiology and evolutionary psychology. Reading and writing about their work was a thrill, and I think it has added immeasurably to the revised book.

As for social theory, I am not opposed to it per se, and I can easily look back over previous issues of *ST* and identify a number of articles that represent social theory that I have found both interesting and valuable. For example, Neil Gross's piece in the September 2005 issue on the transformation of intimacy is an excellent conceptual contribution, and one could easily go through it and pull out specific hypotheses that could later be empirically tested. What I am opposed to is the kind of social theory that falls under such headings as cultural studies, "whiteness studies," "queer theory," and "their ilk." ("Ilk" is Adams's word, but it is exactly the word I myself would have chosen to characterize such work.) To me, when you put political adjectives like "whiteness" or "queer" in front of the word "theory" what you end up with is a complete oxymoron, mostly highly politicized rhetorical nonsense.

Adams also interprets me as being against theoretical pluralism. This claim is nothing short of astonishing, inasmuch as I am not only in favor of it, *but offer an explicit plan for its achievement*!! My plan would, in fact achieve far greater pluralism – or perhaps I should say a more truly *egalitarian* pluralism – than we have seen throughout most of the history of *ST*, given that nonscientific theoretical articles have outnumbered scientific theoretical articles at a ratio of about 2:1. I would think that the only good reason for opposing my plan would be that it stretches resources too thin, and therefore might encounter difficulties of a practical sort. But that is not Adams's argument. She is content with the current system and obviously threatened by any attempt to change it. This is quite unsurprising, given that the current system clearly favors the kind of theorizing that she and her three co-editors advocate and practice! Based on my knowledge of her writings, I can only conclude that Adams is firmly against structures of domination. But do the rules somehow change when one belongs to the hegemonic group?

Contrary to Adams, I do not wish to "quarantine" or "cordon off" one type of theorizing from another, or, as Perrin suggests, send social theorists and exegetes of the classics into exile. Nor am I "asking for my ball back" because "I want to go home," as Perrin remarks (or as Cartman might say on *South Park*, "Screw you guys, I'm outta here"). I am just asking to be allowed to play with the ball as much as the other types of theorists are playing with it. In the National Football League, after a team with the ball scores, the rules require them to give the ball back to the other team so they can have a chance to score. But that is not the way things have most often operated in the editorial offices of the ASA's flagship theory journal. The social theorists have the ball, and they want to hang onto it as much as they can. When they "score," they don't want to give it up every time to the sociological theorists so they might have an equal chance to "score." The social theorists want to be given at least twice as many chances to score. Adams's and Perrin's arguments seem to confirm a basic argument of conflict theory: The values and stated preferences of dominant groups will conform to their interests. And, since I belong in this case to a minority group, it is unsurprising that I want to create a much more genuine form of pluralism that lets all voices be heard in a much more equal manner. (And, by the way, were my plan somehow to come into effect, I would still pay at least some attention to the journals devoted to social theory and to classical theorizing, as I fully acknowledge that there can be useful cross-fertilization between these forms of theory work and scientific theorizing.)

I have no doubt that Adams is sincere when she says that the pages of *ST* are open to all types of theoretical work. However, it is common knowledge that editors have their biases, and that these biases reflect what gets published. Often this is conscious, but just as often it is unconscious.[1] This is why I took the trouble to do those tabulations for my original essay, tabulations that show the consequences of the biases of past editors. Is Adams telling us that she and her three coeditors are somehow unlike other human animals in being free from such biases? Of course *ST* is open to all types of contributions, and of course it will publish all types. It has been doing so throughout its history. But not all types equally. When it comes to *ST*, all modes of theoretical work may be equal, but some appear to be more equal than others.

But I am going to give Adams and the Yale editorial team a chance to prove me wrong. I have in progress two articles that I think are suitable for *ST*. The first, tentatively entitled "The Evolutionary Forms of the Religious Life," is an attempt at a Darwinian interpretation of the origins of religion and a social evolutionary interpretation of religious transformations throughout history – interpretations that will undoubtedly cause the Yale team to raise all eight of its eyebrows. Then, I want to follow that up with an article on two discarded and forgotten classical sociologists, Edward Westermarck, the first Darwinian sociologist (actually, the first sociobiologist), and Albert Galloway Keller, a student of William Graham Sumner and to the best of my knowledge the first person to develop a natural selectionist theory of social evolution. I can imagine already the winces on the faces of the Yale team as they contemplate the awful and oh-so-untrendy things that are likely to be contained in these papers. I will submit them to *ST* when they are finished and keep my fingers crossed that they will get the same fair editorial shake as articles on the "cultural turn," the "public sphere," "feminist epistemology," and suchlike.

2. The Nature of Science. Both Perrin and Wilkes raise questions concerning what they seem to think is my naïve and narrow understanding of science. Perrin contends that I admit that my preference for scientific theorizing is nothing but a personal preference. I make no such admission whatsoever. My preference for scientific theorizing is an *epistemological preference*, and such a preference is justified a thousand times over by the fact that science works far better than nonscience at producing real, cumulative knowledge. As the anthropologist Marvin Harris has put it so well, "In the entire course of prehistory and history only one way of knowing has encouraged its own practitioners to doubt their own premises and to systematically expose their own conclusions to the hostile scrutiny of nonbelievers" (1979:27). That way of knowing is what we call science. A short time after Einstein developed his general theory of relativity in 1915, physicists found a way to test one of its predictions, the bending of light rays from the sun as they passed by Mercury. If I remember correctly, Einstein's theory predicted a degree of bending on the order of about eight decimal places. When the test was able to be made during a solar eclipse, Einstein's prediction turned out to be correct to the very last decimal. This is knowledge by normative injunction or personal preference? I don't think so.

Perrin also accuses me of being a naïve Popperian. Certainly not. I have read my Popper, but I have also read my Lakatos and my Laudan, two philosophers of science who are ardent defenders of science but in a more nuanced way than Popper. Popper's early thinking was encumbered by a kind of naïve falsificationism in which a single disconfirming instance could be allowed to overturn a hypothesis, but he later came to see that this was unrealistic and not how science actually proceeds. His later sophisticated falsificationism admitted degrees of falsification. His former student and later critic, Imre Lakatos (1970, 1999), went even further by showing that scientific research programs often thrive despite many anomalous empirical findings. For this reason, research programs have to be judged comparatively, i. e., in terms of how they stack up against rival programs. Scientific progress occurs in the form of *theoretically progressive problemshifts*, which are research programs that can explain everything their rivals can and at least something more – even if there are still many anomalies. These progressive problemshifts may eventually turn into degenerating problemshifts, in which case they will be replaced by rivals that are theoretically progressive. So, in other words, science is a matter of setting forth hypotheses that can be evaluated against both bodies of evidence and rival hypotheses, and it proceeds ratchet-like in (at least over the long term) largely an upward spiral.

Larry Laudan (1977), a former student of Thomas Kuhn, agrees with Lakatos about the importance of comparative theory evaluation, but adds two important points. First, when new research programs replace older ones, there will be losses as well as gains, and thus scientific progress is much messier than some sort of steady growth by accretion. Second, scientists are guided in their theoretical judgments by more than just empirical evidence. They often fall back on such things as philosophical and political worldviews. For example, Darwin's concept of natural selection was greatly resisted by even those biologists who accepted the reality of evolution because it removed purpose from nature, which for most nineteenth- and early-twentieth-century thinkers was anathema (Mayr, 1991). And in sociology there is great resistance to sociobiology, evolutionary psychology, and kindred ideas because these perspectives fly in the face of the Durkheimian dictum that social facts can only be explained by other social facts, and also because such approaches are thought to have unacceptable political implications. Of course, eventually worldviews tend to be worn down by the sheer accumulation of evidence. In the case of natural selection, the new population genetics of the 1930s showed how this process could actually work, and as a result the resistance of evolutionary biologists to this proposed mechanism of species change was rather rapidly overcome. Likewise, I suspect that the resistance of sociologists to biological forms of explanation will eventually be overcome as the empirical evidence for the importance of biology becomes massive, and as sociology becomes even more marginalized within the academy than it is already.

But I am not going to hold out false hope that any of this will especially impress Perrin, inasmuch as he seems to think that the criterion of testability is of little or no use in evaluating theories. "The distinction between testable and non-testable theories is, at best, blurred," he avers. I guess I fail to see the point, or at least the problem. Of course there will always be cases in which hypotheses may be difficult to test, cases in which it may take real intellectual ingenuity to devise good tests. But in most cases the boundary between the testable and the nontestable is not all that fuzzy. Marxian theory, for example, has made numerous testable predictions. Many of these have now been fairly decisively disconfirmed by history (such as the prediction that socialist revolution would occur in the most advanced industrial societies), but some have seemed to be on the right track (such as the claim that as capitalism unfolded capital would becoming ever more concentrated and centralized). And contemporary dependency and world-system theories have made clear-cut predictions that have been subjected to numerous highly sophisticated empirical tests, some of which support the predictions (Bornschier, Chase-Dunn, and Rubinson, 1978), some of which contradict them (Firebaugh, 1992), and some of which suggest that the theories must be substantially reformulated (Kentor and Boswell, 2003).

Perrin also claims that we need to know the classical roots of modern theories in order to know how to evaluate them, but I fail to see the logic of this. Dependency theory can be evaluated straightforwardly without knowing anything about its ancestry. Similarly, modern state-centered theories of revolution (e. g., Skocpol, 1979; Goldstone, 1991; Wickham-Crowley, 1992) can be evaluated in terms of their predictions; knowing that these theories may be traceable to, say, Weber, does not assist us in evaluating them. And as for insisting that normative commitments are necessary to make our theories other than irrelevant conceits, this seems faintly ridiculous. Normative commitments constitute *serious obstacles* to evaluating theories, because in their presence "wish often replaces thought" (Goldberg, 1991).[2]

3. The Practice of Science in Sociology. Christopher Wilkes correctly notes that most of what passes for science and empirical testing in sociology is the kind of quantitative work published in the three leading journals, and that publication of such work in these journals is critical to career success at the highest levels. In my view he is also right on target in noting that much of this work represents a very narrow view of science. However, for the life of me I cannot understand why Wilkes thinks that I am advocating this kind of narrow positivistic theoretical and empirical work. Truth to tell, I have long been a critic of this kind of sociology, which is based on a very poor grasp of how "real science" is actually conducted. Much of the research published in the top three journals seems to be devoted to quantification as an end itself and is often impossible to understand and evaluate for those of us who are not highly quantitatively oriented or trained.

Moreover, sociological researchers often misunderstand science in at least two other fundamental ways: They usually study a single society (in American sociology, most often the United States), and they are so devoted to multivariate modes of explanation that they throw as many variables into their regression equations as they can dream up. With respect to the first problem, science seeks the most *general* theories possible, and such theories can only be developed by studying a very large number of cases. Biologists do not define their field as the study of elephants, or bees, or woodpeckers. At the very least, sociologists need cross-national samples and, even better, the much larger samples of the full range of human societies, such as the Ethnographic Atlas (Murdock, 1967), which includes 1,267 societies of every conceivable type, or the Standard Cross-Cultural Sample (Murdock and White, 1969), a subsample of 186 of these societies. In terms of the second problem, the big goal seems to be to maximize the amount of variance explained in one's dependent variable. This is a quite appropriate goal, but many sociologists seek to achieve it by maximizing the number of independent variables. The problem with this is that it severely violates the *principle of parsimony* – trying to explain the most with the least – that is such a fundamental part of the natural sciences, but apparently little understood by most sociologists.

Methodologically I am about as eclectic as it gets. I have little patience for methodological debates that pit one approach against the others as the only viable methodological strategy. I believe we get much further by using the entire range of approaches – survey research, cross-national and cross-cultural studies, comparative-historical analysis of selected cases, ethnographic work, and the rest of them – and the approach to be used should be based on the kind of problem being studied, the availability of data, and so on. If Wilkes were to read my own published empirical research, he would see that I practice what I preach. Here is a sampling of research I have done: a close historical case study of the origins of apartheid in South Africa using split labor market theory (Ndabezitha and Sanderson, 1988); a historical analysis of early modern European family patterns using a world-system perspective (Alderson and Sanderson, 1991); a comparative-historical study of the origins of capitalism in Western Europe and Tokugawa Japan (Sanderson, 1994); a regression analysis testing three theories of fertility decline using a large cross-national sample (Sanderson and Dubrow, 2000); and a regression analysis testing three theories of gender inequality using the Standard Cross-Cultural Sample of 186 societies (Sanderson, Heckert, and Dubrow, 2005). I currently have in progress a regression analysis of the evolution of religion using the Standard Cross-Cultural Sample (Roberts and Sanderson, 2005), and a regression analysis of world democratization between 1850 and 2000 using a large cross-national sample (Sanderson, 2004). In every single one of these research studies, I am either testing one or more theories with empirical data, or using a specific theory as a guide to interpret social and historical events. Anyone reading these publications will quickly see that my conception of science is much broader than the conception being taught by most methods professors and being published in the three leading journals.

And here are some examples of what I consider the best scientifically oriented theoretical sociology: Lenski's (1966) evolutionary theory of stratification using the Human Relations Area Files and the Ethnographic Atlas; Wickham-Crowley's (1992) brilliant use of Boolean algebra to understand Latin American revolutions; James Mahoney's (2003) use of fuzzy-set methods to understand the effects of colonial penetration on economic development in Spanish America; Theda Skocpol's (1979) and Jack Goldstone's (1991) use of comparative-historical methods to develop state-centered theories of revolutions; Edgar Kiser's (1987, 1994) use of rational choice theory in conjunction with the historical analysis of selected cases to understand numerous features of early modern European states; Donald Black's (1976) use of a wide range of comparative and historical data to understand the behavior of law; and Randall Collins's use of Weberian-oriented historical analysis to understand such social phenomena as the rise and fall of states and empires (Collins, 1986, 1995) or the long-term expansion of American education (Collins, 1979). This is all theory-driven research using a wide range of methods and data sources, and it is precisely the sort of thing that I think theorists should be engaged in.

4. Social Science, the Humanities, and Everyday "Theorists." In Dustin Kidd's critique, he makes a distinction between "open" and "closed" approaches to theorizing. Open approaches are those that are amenable to insights from all of the social sciences and from the humanities, whereas closed ones are those that are limited to science. This reminds me of a colleague I had years ago who commonly used the phrase "he's very open." I became puzzled when I actually encountered the people he was referring to, because I found them to be very closed-minded and dogmatic. The puzzle was quickly solved when I realized that what my former colleague meant by "open" was "open to those views *that I like*." Much the same seems to be true for Kidd. We could translate Kidd's argument into a kind of chain of Lévi-Straussian binary oppositions: Open:Closed::Social Sciences & Humanities:Natural Sciences::Good:Bad.

I must admit, I find it more than just a little odd that being committed to a scientific approach to knowledge should be identified as closed. This seems to me to be a merely personal and highly subjective preference. If anything, scientific approaches are the most open, in the very sense of the quote from Harris given earlier: Science is always done in a wider community, and scientists have a widely understood obligation to make their findings, methods, data sources, and so on public so that they can be evaluated by others. Scientific arguments, being testable and falsifiable, create the maximum likelihood that bad ideas will be driven out over the long run by good ones. What could be more open than that?

However, despite what Wilkes seems to think, I am not at all against the humanities. They have a vital role to play in our modern system of higher education. Indeed, I have read extensively in such humanities fields as philosophy and history. But the humanities operate with different epistemological modes, which are not scientific, and thus they are not the fields to which scientific fields should be looking in order to make advances. The humanities – or at least humanities like art, music, and literary criticism – are notoriously soft and subjective. What counts most heavily in these fields is *taste*, and taste is highly (although perhaps not entirely) subjective. I cannot see how we are we going to advance our understanding of the world by giving as much emphasis to subjective fields lacking widely agreed upon objective standards equal time with science. This does not mean that the novels of a Dickens or a Jane Austen, for example, cannot produce insights into human behavior. Indeed, the very finest novels do precisely that. But these insights cannot be compared with the knowledge gleaned from systematic social-scientific research.

In moving from a scientific mode of understanding to a humanistic one, we step toward the edge of the cliff. But in taking yet another step to people who are not even professional scholars, as Kidd recommends and as so many social theorists are doing these days, we step *off that cliff*. The fashionable idea among the postmodern crowd that people like Mao Zedong, Gloria Anzaldúa, Betty Friedan, Gandhi, and Vaclav Havel (Lemert, 1993) – who are politicians or political activists with ideological rather than intellectual goals – seems to me an insult to those of us who spent so many years sweating blood and tears earning our Ph.D.'s and many more years reading and writing books and articles to advance our discipline and our careers. It is true that all humans are natural theorizers in the sense that they are curious about at least some things and want to explain why they are the way they are, and indeed I have been making this very point for over thirty years to students in my theory classes. The approach known as *evolutionary epistemology* (Campbell, 1987; Wuketits, 1990) can explain why, in fact, humans are natural theorizers (the short answer: it is highly adaptive in a Darwinian sense). But let us not confuse *theorizers* with *theorists*. Everyone of normal mental capacity is a theorizer, but not everyone is or even can be a theorist. Being a theorist requires something much more rigorous and demanding, and, presumably, many years of training and hard work at thinking. The idea that ordinary people are theorists just like professional intellectuals seems to me just another one of those currently fashionable ideas that is driven more by egalitarian ideology than by anything else.[3] If this leads to my being charged with elitism, then I can only say, "*Vive élitisme*!" (Sorry for the pretentious Gallicism.)

Conclusion. Wilkes worries about what will happen if "we leave sociology to the Sandersons of the world." The tone I sense in his statement is sort of like the question, "What will happen to politics it we leave it to the George Bushes of the world." Wilkes thinks the outcome will be some sort of narrow, tedious, trivialized, and boring positivism because he falsely believes, without any evidentiary basis whatsoever, that I favor ever greater amounts of the hyperpositivistic trivia that too often adorns the pages of the leading journals. No, what would happen would be a return to the original mission of the great sociological thinkers of yesteryear: a very broad comparative and historical science of society that would ask bold questions and seek bold answers through the use of a wide range of research methodologies. That is what would happen. Not only would sociological theory be radically transformed from its current state, but so would the leading research journals.

Notes

1. In the mid-1980s I wrote an article entitled "Eclecticism and Its Alternatives" and submitted it to *ST*. It was rejected. I then submitted it to *Current Perspectives in Social Theory*, where it was quickly accepted and published (Sanderson, 1987). When I wrote back to the editor of *ST* to inform him of this, he seemed surprised and rather defensive, once again justifying his decision. His main point was that the article "had too much of a natural science feel to it." He went on to explain that he was currently working on Lacan and some French semiotician, apparently implying that that was the sort of work that was most suitable for his journal. Had *ST* at that time been under the editorship of a person who actually thought a "natural science feel" was a positive rather than a negative attribute of theoretical work, I suspect that my article would have been accepted for publication in *ST*.
2. Perrin insists that the three major forms of theoretical work I identify are often blended together in the same theorist, citing Coleman, Blau, Wallerstein, Skocpol, and Lenski as

examples. This is often true, but normally one of the theory modes will stand out clearly apart from the others. For example, what strikes me about the work of Coleman, Blau, and Lenski is that all three have been strongly committed to scientific theorizing and have been remarkably free of strong political biases. Skocpol certainly has her political commitments, but she is an excellent scientific theorist in the comparative-historical sense. Of the five, Wallerstein has the strongest and most explicit political commitments, but his most important work is excellent theory-driven sociology based on the accumulation of mountains of historical data. And he did generate an extremely interesting paradigm that can be and has been empirically evaluated (Sanderson, 2005).

3. Mao Zedong would have to go down in history as one of the biggest intellectual disasters ever. His ideas were driven entirely by his own perverse politics and an egomaniacal desire for power, and the policies he put into place in China during his short rule proved to be totally out of touch with reality and resulted in social catastrophes on an unprecedented scale (Courtois et al., 1999). And we would look to someone like that for advances in social theory?

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