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Perspectives

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Please submit materials for future editions of
Perspectives to:

Neil Gross
Department of Sociology
Harvard University
William James Hall
33 Kirkland Street
Cambridge, MA 02138
Email: ngross@wjh.harvard.edu

Adam Kissel, Publishing Assistant
Email: akissel@wjh.harvard.edu

www.asatheory.org

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:

	<i>Normative</i>	<i>Explanatory</i>
<i>Formal</i>		
<i>Informal</i>		

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 scientificity 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 19th 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 to

handle 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 Brief Reply to My Critics

Stephen K. Sanderson
Indiana University of Pennsylvania

[This is a much abbreviated version of a much longer response. Readers interested in the detailed response, along with numerous references, may go to the ASA Theory Section website at <http://www.asatheory.org>. My original essay and the four responses to it will also appear there.]

The many interesting points raised by the critics of my essay “Reforming Theoretical Work in Sociology” are most welcome because they give me an opportunity both to clarify some of my arguments and to expand on others. I shall concentrate here on four main issues.

1. *The Nature and Practice of Theoretical Pluralism*. 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. I do not disapprove of exegeses of the classics, and in fact I have done such work myself and continue to do it. 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. What I am opposed to is the kind of social theory that falls under such headings as cultural studies, “whiteness studies,” or “queer theory.” To me, when you put political adjectives like “whiteness” or “queer” in front of “theory,” what you end up with is a complete oxymoron.

Astonishingly, Adams also interprets me as being against theoretical pluralism. Not only am I in favor of it, *but I 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 by a ratio of about 2:1. And contrary to Adams and Perrin, I do not wish to “quarantine” or “cordon off” one type of theorizing from another, or send social theorists and exegetes of the classics into exile. In fact, I would read some of their work. Nor am I “asking for my ball back” because “I want to go home,” as Perrin rather insensitively remarks. I am just asking to be allowed to play with the ball as much as the other types of theorists are playing with it.

I have no doubt that Adams is sincere when she says that the pages of *ST* are open to all types of theoretical work. 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. However, it is common knowledge that editors have biases that have consequences for what gets published, and it is hard to imagine that the Yale Group will be bending over backwards to publish my kind of theory rather than their kind.

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 merely 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. Science deserves its privileged status because it has produced real results – real knowledge – and the reason is that it is the most self-correcting form of knowledge that we possess.

Perrin also accuses me of being a naïve Popperian. In fact, I subscribe to the much more sophisticated falsification of Imre Lakatos, who has shown that scientific research programs often thrive despite many anomalous empirical findings. For this reason, research programs have to be judged comparatively, that is, in terms of 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. Perrin apparently thinks, however, that the criterion of testability is of little or no use in evaluating theories, since “the

distinction between testable and non-testable theories is, at best, blurred.” 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 non-testable 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, although some have seemed to be on the right track. And I fail to see why we would need to know the classical roots of modern theories in order to evaluate them. Modern world-system and dependency theories of underdevelopment have their roots in Marxism, and state-centered theories of revolution in Weber, but these theories can be and have been evaluated without reference to either classical thinker.

3. *The Practice of Science in Sociology.* Wilkes correctly notes that much of what passes for science and empirical testing in sociology is the kind of narrow, hyperpositivistic work published in the three leading journals, but he is far wide of the mark to assume that I share this view of science and scientific theory testing. Truth to tell, I have long been a critic of this kind of sociology, which represents 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 in itself and is often impossible to understand and evaluate for those of us who are not highly quantitatively oriented or trained.

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. To mention just a few of my favorite examples of the very best in theory-driven scientific sociology, I would list the following: Gerhard Lenski’s evolutionary theory of stratification using data from the Human Relations

Area Files and the Ethnographic Atlas; Timothy Wickham-Crowley's use of Boolean algebra to understand Latin American revolutions; James Mahoney's use of fuzzy-set methods to understand the effects of colonial penetration on economic development in Spanish America; Theda Skocpol's and Jack Goldstone's use of comparative-historical methods to develop state-centered theories of revolutions; Edgar Kiser's use of rational choice theory and historical case studies to understand numerous features of early modern European states; Donald Black's use of a range of comparative and historical data to understand the behavior of law; and Randall Collins's use of Weberian theory and historical analysis to understand such social phenomena as rise and fall of agrarian states and the long-term expansion of American education. This research and much else like it is a far cry from the kind of narrow empirical sociology published in the top three journals.

4. *"Open" versus "Closed" Styles of Sociological Theory.* In his critique, Dustin Kidd 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 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.

I must confess that 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: 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?

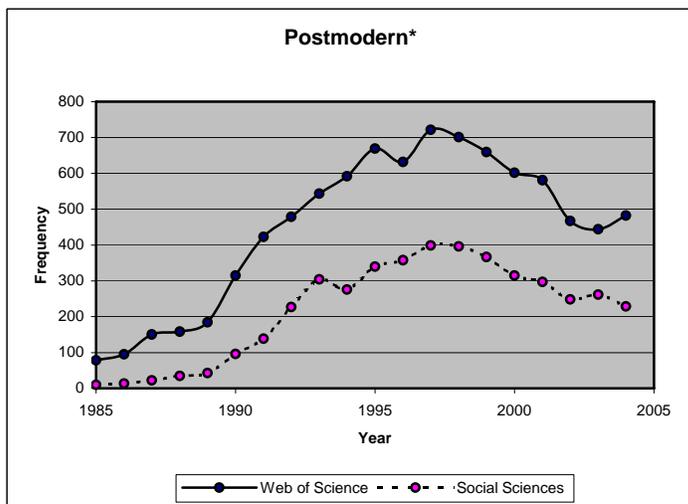
Conclusion. Wilkes worries about what will happen if "we leave sociology to the Sandersons of the world." But what he thinks would happen would not. If I became "Czar of American Sociology" for a day, I would seek to return the discipline to its original mission a century or more ago: a very broad comparative and historical science of society that would ask bold questions and seek bold answers through the use of a wide range of research methods. Not only would sociological theory be radically transformed from its current state, but so would the leading research journals.

SOME EMPIRICAL TRENDS IN THEORY

Saleema Saioud (sal.saioud@utoronto.ca) and Marion Blute (marion.blute@utoronto.ca)

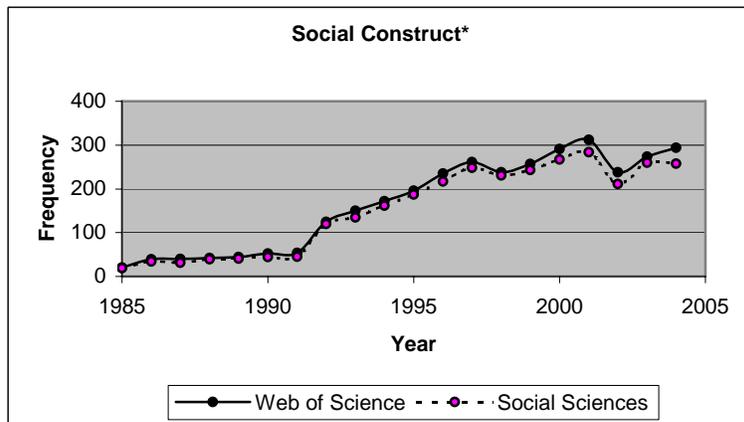
It is always difficult to choose a final topic for a graduate course in contemporary theory. Whatever is chosen suggests what you think is most important and the direction in which the field is moving. Being vaguely dissatisfied with the 'postmodernism' topic of recent years (not least of all because the previously required reading, Charles Lemert's *Postmodernism Is Not What You Think* went out of print), we decided to check some empirical trends on the Web of Science. The results we think were interesting enough to share with readers of *Perspectives*.

After some initial exploration we took a closer look at postmodernism, social constructionism, and globalization (including variants). For each term we performed two "general searches" from 1945 to 2005 (a general search pulls up articles with the term in any of the title, abstract, or keywords). One search was of the Science Citation Index (Expanded Version which includes the natural and social sciences as well as the humanities) while the other was restricted to the Social Sciences Citation Index (the social sciences subset of the former). (Only some of the many journals sociologists publish in contain "Sociology" in their title and this modest project was not planned to be elaborate enough to search many journals individually by title and then cumulate the data. Given that the patterns from the two searches performed were very similar to each other for the three cases, we expect the results cumulated from such a "sociology" list would be quite similar as well.) The three graphs display the resulting frequencies by year from 1985 to 2004.

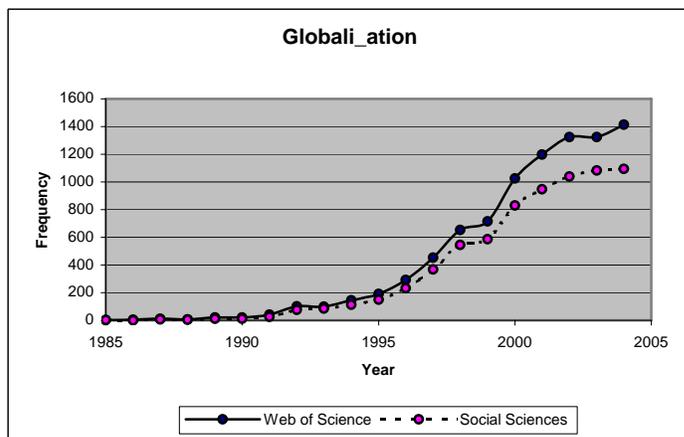


Our intuition that it was time to retire postmodernism was confirmed. Not surprisingly, about half of its usage is outside the social sciences, undoubtedly in the humanities. After a fairly steady increase from the mid 1980's, significant interest peaked in 1997 and has declined thereafter by a third. Experience suggests that in the absence of an exogenous shock, such a large decline is unlikely to be reversed. The data on social constructionism are different but similarly interesting. First, it is almost exclusively used in the social sciences (3377 of a total of 3643 articles). Secondly, it appears to

have reached a limit in 2000 and thereafter entered into a period of instability. This is a fairly common pattern - overshooting, falling back, overshooting again and so on. While such a pattern can



continue, it does suggest there is not a lot more room for expansion. For the third concept, globalization, there is some suggestion in the increasing spread of the two curves, that its usage has been spreading from the social sciences to academia more widely. In any event, it is the only one of the three that so far, seems to be still on a growth trajectory (confirming the data of Guillen in the *Annual Review of Sociology*, 2001 from other sources).



These are raw data, not standardized for the total number of articles published. However, this does not affect the comparisons among the three. Did the data determine the decision in favor of globalization? Actually, no. Not being slavish positivists, and keeping in mind the criteria of what is judged to be important as well as where things seem to be headed, the decision was made to conclude the course with Jared Diamond's *Collapse: How Societies Choose to Fail or Succeed* anyway!

**American Sociological Association
1307 New York Avenue NW, Suite 700
Washington, DC 20005-4701**

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