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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

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 tests (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, have 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 moreso 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 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 of 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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Deciphering Goffman: Grounding Concepts in Particulars[1]

Thomas J. Scheff
University of California, Santa Barbara

Wittgenstein believed that vernacular language is the reason many problems are unsolvable. Perhaps Goffman's work can be understood similarly: as long as social science depends largely on vernacular words rather than concepts, it can only reaffirm the status quo. Goffman sought to solve this problem in two ways. First, he invented a panoply of new words and usages in a frontal attack on the assumptive world of modern societies. Secondly, and more covertly, he took initial steps to ground conceptual definitions for two vernacular terms: mutual awareness and embarrassment. Since the first direction is already widely accepted in scholarly commentaries on Goffman's work, this essay will focus on the second.

Goffman's writing is difficult to understand, even though it is brilliant, original, and entertaining. One crucial flaw is that he usually doesn't state a clear thesis. Either there is no thesis provided at all, or what is offered is misleading. The former shortcoming, lack of a clear thesis, characterizes his longest and most enigmatic book, *Frame Analysis* (1974)[2]. On the other hand, the thesis offered at the beginning and at the end of *Presentation of Self* is misleading. The whole first half, and the last chapter, deal with performances and dramaturgical staging, rituals of theatre. Behavior is scripted by the social situation; motives are not important (Goffman the Structuralist). The first and last acts lull the reader to sleep in a Durkheimian dream.

However, beginning with chapter 4 on discrepant roles, the argument begins to drift toward motives. By the sixth and most substantial chapter, on "impression management," Structural Goffman has virtually disappeared. This chapter instead concerns actors' motives, their harried attempts to stave off, or at least manage, embarrassment and related emotions.[3] Without a word of warning, the Sociological Social Psychologist has reared his head, shape-shifting. The reader has been conned.

One final example of a misleading presentation is in the essay "Where the Action Is" (1967, 149-270). At 122 pages, this chapter is almost as long as the other essays in the volume combined (6 chapters totaling 149 pages). As far as I know, it is Goffman's longest essay.

The difficulty is that there are sharp changes in topic and tone in the last quarter of the essay. The first three-quarters is mostly about gaming, but the last quarter shifts to *masculine* competitiveness, what Goffman calls the "character contest" (249). The gaming material is bland and innocuous, at least for Goffman. The last quarter is extraordinarily intense: Goffman at his inimitable best. In Chapters 1 and 10 of my book, I show that the way Goffman develops the idea of masculine competitiveness can contribute to a theory of hypermasculine violence. The two chapters also indicate how the idea of character contests can be used to interpret some aspects of Goffman's own life, particularly what Lofland (1984) has referred to as Goffman's "hazing behavior." Yet the character contest is only one of the many fruitful ideas in Goffman. What was the main thrust?

The War on Tropes

There is a substantial commentary on Goffman's work that has established that it is no help with systematic theory, method or data, at least in any conventional sense. What could he be up to? One clue is provided by Goffman's endless development of new concepts and systems of classification that lead nowhere. In his otherwise highly appreciative essay, Lofland (1980, 29) has nevertheless pointed out that the first three pages of one article of Goffman's contain:

> 3 types of face
> 4 consequences of being out of or in the wrong face
> 2 basic kinds of face work
> 5 kinds of avoidance processes
> 3 phases of the corrective process
> 5 ways an offering can be accepted (1955, 211-213).

Manning (1980, 270) notes that later, in *Frame Analysis* (1974), the following concepts "at least" are found in a 19-page span:

> 4 kinds of playful deceit
> 6 types of benign fabrications
> 3 kinds of exploitative fabrications
> 5 sorts of self-deception (1974, 87-116).

Much the same could be said about the rest of Goffman's work. Since the reader is never told the purpose of this rat's nest of classifications, and Goffman himself rarely refers to it in his subsequent work, we face a mystery.

It is possible that Goffman's main purpose was preliminary to science, to demolish ruling tropes (metaphors) in order to make room for scientific method.

> The neatly worked inner stretches of science are an open space in the tropical jungle, created by clearing tropes away. (Quine 1979, 160)

That is to say, it often necessary that an obstructive metaphor has to be overthrown. One example from astronomy: in order to chart the orbits of planets, the taken-for-granted assumption that the earth was the center of the universe had to be overcome. The methods of science are useless if one is entrapped in erroneous assumptions. A trope is a ruling metaphor in the assumptive world of a culture. Goffman's hectic and relentless invention of new concepts was a step toward clearing an open space for social science in the tropical jungle of our assumptive world.

Most social science theory and research depends on tropes, vernacular words that are metaphors rather than concepts. Seeman (1975) pointed out that one of the central ideas in social science, alienation, is not a concept, since it has at least six different meanings. It has been shown many times that the vernacular word for love, especially in English, is wildly ambiguous.[4] Yet almost all current discussion and research continues to use the vernacular words, rather than defining alienation or love as a concept.[5]

In a recent study (2004), David Fearon and I suggest that the most studied topic in all of social science, self-esteem, has never been adequately defined as a concept. As a result, all of the some two hundred self-esteem scales confound cognitive, emotional, dispositional, and relational components. Finally, in a recent (2005) essay, I proposed that emotion names are tropes, vernacular words rather than concepts, and for that reason endlessly confusing.[6]

Goffman's prolonged attacks on the trope of the self and other metaphors, such as mental illness, make credible the idea that Goffman was fundamentally a trope clearer, a giant killer. He attempted to deconstruct both Western culture and, more centrally, the vernacular foundation of modern social science.

Goffman also may have taken the next step beyond deconstruction, in the case of two basic ideas, attempting to define them clearly. Since he didn't offer theses, this idea is only an interpretation. To illustrate his second approach, I propose two examples: the way he went about defining what he calls "mutual awareness," on the one hand, and embarrassment, on the other.

Although he casually uses metaphors for mutual awareness (e.g., the phrase "mystic union")[7], he also offers a fairly elaborate and complex definition of "being in a state of talk." Since his definition requires an entire page of text, I will not repeat it all here. Suffice to know that it contains phrases that imply mutual mindreading: "An understanding will prevail [among the speakers] as to how long and how frequently each speaker is to hold the floor…" (1967, 35; a similar formulation occurs earlier, 34). The definition comes closest to explicitly describing intersubjective accord in this line:

> *A single focus of thought and attention*, and a single flow of talk, tends to be maintained and to be legitimated as officially representative of the encounter. (Goffman 1967, 34, emphasis added)

The significance of the phrase "a single focus of thought and attention" becomes more apparent if it is compared to a similar phrase, "joint attention," used by the psychologist Bruner (1983), when he is explaining how an infant learns to become *attuned* with its caretaker.[8] The mother, he says, is only trying to teach a new word. She places an object (such as a doll) in her own and the baby's line of gaze, shakes it to make sure of the baby's attention, and says, "See the pretty DOLLY." In this situation, the baby is likely to learn not only the meaning of a word, but also, since both parties are looking at the same object, how to have, jointly with the mother, "a single focus of thought and attention."

A more detailed idea of mutual awareness comes up in somewhat stronger but still indirect form in Goffman's comments on co-presence.[9]

> When in each other's presence individuals are admirably placed to share a joint focus of attention (1), perceive that they do so (2), and perceive this perceiving (3). (Goffman 1983, 3, numbers added)

This quotation points to three levels of mutual awareness. In his book on strategy (1969) Goffman at least hints that even higher orders of mutual perception might determine the winner of strategic contests, such as spying and large-scale financial transactions, if the stakes are high enough.

A conceptual definition of mutual awareness is as far as Goffman goes in attempting to explicate this idea; he

doesn't provide objective indicators. Perhaps Goffman was uncomfortable about flatly stating and following up an idea that is anathema in individualistic modern societies, that we are all "members one of another." Although church members recite this idea every Sunday, most would be loath to take its meaning literally, as both Cooley and Goffman did.

In the case of the other concept discussed here, embarrassment, he was not content to give only a conceptual definition, but also followed up, offering elements of an operational definition:

> An individual may recognize extreme embarrassment in others and even in himself by the objective signs of emotional disturbance: blushing, fumbling, stuttering, an unusually low- or high-pitched voice, quavering speech or breaking of the voice, sweating, blanching, blinking, tremor of the hand, hesitating or vacillating movement, absentmindedness, and malapropisms. As Mark Baldwin remarked about shyness, there may be "a lowering of the eyes, bowing of the head, putting of hands behind the back, nervous fingering of the clothing or twisting of the fingers together, and stammering, with some incoherence of idea as expressed in speech." There are also symptoms of a subjective kind: constriction of the diaphragm, a feeling of wobbliness, consciousness of strained and unnatural gestures, a dazed sensation, dryness of the mouth, and tenseness of the muscles. In cases of mild discomfiture, *these visible and invisible flusterings occur but in less perceptible form*. (Goffman 1967, emphasis added)

This definition links an interior emotion with surface observables. With his usual uncanny instinct, in the last sentence he even hints at the need for further elaboration of the operational definition: "these visible and invisible flusterings [that accompany embarrassment], but in less perceptible form." This clause seems to point toward the development of more elaborate coding systems for the verbal and gestural indicators of shame and embarrassment, such as the one by Retzinger (1991; 1995). Certainly in 1967, and even today, Goffman was way ahead of the curve.

Perhaps we should imitate Goffman, developing concepts grounded in the details of the reality they are supposed to represent. One way to approach this problem is to treat it as a part/whole problem: how to relate abstract concepts, the wholes, to the particulars of actual life, the parts (for my treatment of the part/whole problem, see Scheff 1997).

This issue came up in an unusual way in an interview with the novelist Muriel Spark concerning her novel *The Bachelors*. The novel describes the lives of bachelors of varying ages and stations in life in London in remarkable detail. Ms. Spark, a middle-aged unmarried woman at the time, was asked how she could possibly know so much about such men. Her answer was, "A lifetime of combing lint."

By lint, Spark seems to be referring to the detailed particulars of the lives of many people that she noted. This idea might be as useful in the early stages of science as it seems to be in the writing of novels. As William Blake put it, "Art and Science cannot exist but in minutely organized particulars." Goffman's way was to take initial steps towards organizing particulars.

In one chapter of Richard Lazarus's last book (1997), he suggests a new approach, at least for him, to the study of emotion. His initial discussion, at least, implies that with respect to emotions, some lint-combing might be in order. At the beginning of Chapter 8, he proposes that one might derive a classification of emotions by close study of narratives.[10] He gives one example, a paragraph describing an actual marital quarrel. From this one narrative, he derives four types of anger: inhibited, righteous and sullen anger, and hostility.

However, he provides only one narrative.[11] In the rest of the chapter, he goes on to derive still another theoretical taxonomy for all the major emotions out of thin air, seemingly forgetting his own suggestion about the use of narratives. He proposes many abstract concepts but only one particular, the narrative about the marital quarrel.

The idea that concepts and theories need to be closely linked to concrete particulars is the central theme of what is called "grounded theory" as proposed by Glaser and Strauss (1967). A drawback of their work is that it isn't sufficiently explicit about specific steps in this direction. They imply that ethnographic work is usually a prerequisite to theory formulation, but without specifying much about the actual methods of getting from parts to wholes.

In particular, for the development of a concept, how many *parts* as compared to how many *wholes*? It doesn't seem likely that there should be more wholes than parts, as in Lazarus's chapter. More likely, there should be many more parts than wholes, as is the case in Goffman's work. Using his approach as an example, it is possible to be explicit about the steps needed in order to ground concepts and theories in concrete particulars, what I have called here "lint-combing."

A weakness in Goffman's use of this method is that in some cases he resorts to hypothetical situations. Although they help the argument along, hypotheticals have serious drawbacks in science. The most glaring one is that they always lack the ambient details, the minute, seemingly irrelevant particulars that often provide the key. Another fault is that since hypotheticals are imaginary, the author is locked into his or her own head.

The strength of Goffman's approach to developing grounded concepts is that it avoids the thin air option. It also avoids preliminary commitment to a particular kind of theory, method or data, a commitment that hobbles much of current research. Instead it draws on diverse examples, helping him to develop concepts that have some palpable relationship to the human condition. Since Goffman's way takes a great deal of time and effort, and is hopelessly indirect and roundabout, there must be a better way. Until a better one is found, however, Goffman's might be the best available.

Notes

[1] This essay is based on and profiles my forthcoming book (Scheff 2005a).

[2] Chapter 5 of the new book proposes that Goffman's volume can be read as his attempt to define the idea of *context* as a "frame assembly."

[3] Chapter 3 suggests that the middle section of *PSEL* involves a spelling out of the implications of Cooley's idea of the looking-glass self by applying it to many examples.

[4] See, for example, Solomon (1981; 1992).

[5] Chapter 7 in my 2005 book offers a conceptual definition of genuine love.

[6] Plutchick (2003), in his review of 23 attempts to provide taxonomies of emotion names, reports very little overlap between them.

[7] Establishing a new language within the shell of an old one is a bit like lifting yourself with your own bootstraps. In *Frame Analysis*, the central theme, though unstated, is an attempt to define context as a concept. Yet Goffman uses this word 48 times in its vernacular meaning, as a residual category, like everyone else.

[8] Attunement is the term used by Stern (1977) in his studies of infant-caretaker relationships. It is difficult to choose a name for the state of mutual awareness in English, since our language establishes the individual as the fundamental unit, rather than pairs or larger groups.

[9] Luiz Baptista called this quotation to my attention. The idea of levels of mutual awareness plays a prominent role in my discussion (2005, Chapter 5) of frame assemblies and consensus.

[10] Bengt Starrin called this chapter to my attention.

[11] Although narratives are much better than thin air, they are still quite abstract, being verbal descriptions. Goffman went quite far with verbal texts. However, in developing concepts, especially emotion concepts, verbatim recordings of discourse may ultimately be needed. Such records make available the verbal and non-verbal indicators of emotion, the minute particulars.

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Chomsky vs. Mead: Albert J. Bergesen Replies

Albert J. Bergesen
University of Arizona

Puddephatt and Segaert (PS) take issue with my critique of G.H. Mead, "Chomsky vs. Mead" (Bergesen, 2004a). They still believe language emerges out of a conversation of gestures, and it is learned in a behaviorist fashion, and syntax has little effect upon sentence meaning. They pretty much accept the Meadian account of language as initially formulated. But given what we now know, this seems highly questionable. While the specifics of a universal generative grammar have yet to be fully explicated, and the initial hopes of Chomsky's program not fully realized, the critique leveled against Skinner's behaviorist account of language learning would seem to have delivered a fatal blow to Mead's behaviorist theory as well. If you think about it for a moment, it is almost inevitable. Late 19th-century ideas about the functioning of mind and brain, pre-socialized infants, and the specifics of how language works simply could not be at the same level they are today.

Yet we cling to Mead. SI, in textbooks and the potted theory of journal articles, repeats the same story line with virtually no reference to contemporary research about language development, syntactical structure, or infant cognitive capacity. From a scientific point of view it's an eerie world, as if the cognitive revolution of the second half of the 20th century never happened, or Chomsky never wrote his devastating attack on Skinner's behaviorist account of language acquisition, or there have been no discoveries about the cognitive capacities of pre-socialized infants. Just for the record, we also do the same thing with Durkheim's theory of the social origin of elementary mental categories (Bergesen, 2004b), and more generally we continue to restate the same internalization story of how society, somehow, implants primal mental architecture within an assumed blank slate mind. When confronted with this, we often deny, like PS, who said, "various parts of the brain (and thus the mind) for Mead, are innate, and cannot be socialized." But this is then followed with the statement that "the mind emerges through the mutually influencing interplay of biological impulses and socialized responses through the pragmatic problem solving process." Regardless of disclaimers, the mind remains—for sociology—a social construction. We want to understand the role of social factors on various aspects of consciousness, but we have taken such a radical position of social construction or interactive emergence, that there is now a huge gap between what we say and what is now known about the mental operations of infants, whose minds, by definition, cannot be the products of socialization. It just never seems to occur to sociology that we could actually construct a better theory of socialization if we had a more up-to-date model of mind, language, and cognitive faculties.

Why we cling to such outdated notions is a story in itself, and there simply isn't time to begin to try to unravel it here. While the origin of language in some evolutionary sense remains a hotly debated topic, the notion of humans as somehow generalizing from gestures to symbols, coming to co-agree on the meaning of these significant symbols (words, really) simply cannot account for the specifics of language. Most importantly, it has almost nothing to say about the operation of syntax. Mead's theory doesn't work for anything but what linguistics call protolanguage: a few words strung together with no syntax. It is what Koko the gorilla does when learning symbols and stringing a few of them together on command, or maybe the language of a child younger than three years old.

The absence of syntax in Mead is fatal, and following his lead, leads to assertions that just don't seem to connect to what appears fairly obvious about language. Argue PS, "there is no evidence to suggest that…syntax, if it exists at all, affects or provides for the content of meaning stored within these syntactic forms." If what they say is correct, then changing grammar (syntax) shouldn't affect meaning. Let's see. Consider the following two sentences, composed of identical words, with identical meanings, and in an identical word order. They differ only in the presence of a comma.

Now I must go and get on my lover.
Now I must go and get on, my lover.

Or another example:

A woman, without her man, is nothing.
A woman: without her, man is nothing.

These are taken from the bestseller by Lynne Truss, *Eats, Shoots & Leaves* (2003), a popular book on punctuation. Consider another example from her book: the case of two Dear Jack letters. Again all the words are the same in both letters; they differ only in punctuation marks (surface markers of syntactic order).

> *Dear Jack,*
>
> *I want a man who knows what love is all about. You are generous, kind, thoughtful. People who are not like you admit to being useless and inferior. You have ruined me for other men. I yearn for you. I have no feelings whatsoever when we're apart. I can be forever happy—will you let me be yours?*

And now again, with only grammatical changes.

> *Dear Jack,*
>
> *I want a man who knows what love is. All about you are generous, kind, thoughtful people, who are not like you. Admit to being useless and inferior. You have ruined me. For other men I yearn! For you I have no feelings whatsoever. When we're apart I can be forever happy. Will you let me be? Yours,*

It would seem grammar (syntax) does play a role in establishing meaning.

Next we can ask about the origin of such syntactic structures. While the evolutionary debate on the origin of syntax continues, the evidence suggests it isn't a cultural template, set of rules, or any form of *habitusinal* cultural object, that is taught, or in any way internalized by children being socialized. Research shows there is no effect of parental sanctioning or encouragement upon grammatical errors made by children, and PS's behaviorist hypothesis that "children are able to learn nuanced syntax rules very quickly" has virtually no empirical support. It just doesn't happen. Caretakers are not aware of the specifics of the rules they use, nor are the children who use them.

Take the case of pronoun reversal, which children unerringly do in the process of language development. One has to wonder if, in the history of human socialization, a parent has ever said the following to a child to instruct them in the use of the pronouns "I" and "You":

> *Now Johnny, here is the thing. You see I am called "I" when I am talking about myself and you are called "you" when I am talking to you about "you," but you are "I" when you are talking to me and I am not "I" anymore but now I am "you" while you are now "I," not me. Got that? Oh, not clear. OK, let's try it again…*

I raised a version of this illustration in the original article, and PS responded by saying this actually "bolsters rather than weakens Meadian (gesture-response) theory of language acquisition." And how does this happen? Well, they suggest that for language to have meaning,

> there must be a foundation of referents, built upon our perceptual interactions within the world of space/time for language to make sense. Thus, only as a result of the parent emphasizing the meaning of "I" by pointing (gesturing) to herself, does the child understand that "I" is in reference to the speaker and "you" in reference to the listener (PS).

So, let's try it again the PS way.

> *Now Johnny, here is the thing. You see I am called "I"* [pointing to herself] *when I am talking about myself and you* [pointing to Johnny] *are called "you" when I am* [switching back to herself] *talking to you* [point back to Johnny] *about "you," but you are "I" when you are talking to me and I am not "I"* [she stops: 'how do I point to reference "I am not I"'?] *anymore but now I am "you"* [now it's hard again. She thinks: 'how do I point "I am you"; that seems like me and you at the same time; maybe I use both of my hands; right one pointing to you and left one to me'] *while you are now "I," not me* [another problem for the socializer: 'how,' she now wonders, 'do I point out the phrase, "you are now I not me"?' She points to Johnny for "you," then back to herself for "I," but what will she do with "not me"? She doesn't know where to point]. *Got that? Oh, not clear. OK, let's try again…*

I am confident our caretaker will figure this out, and that Johnny will learn how and when to switch his pronouns. Still, I wonder. So, for instance, what about children where the parents only point to themselves, or to Johnny, or only point some of the time? Does this mean there will be children who refer to both self and other as "I," creating a learning disability known as "half a pronoun syndrome"? Children do make errors, of course, but as noted they appear to make then at about the same age, across socialization experiences, and they seem to get on with proper pronoun switching at

about the same time as well. And, most important, there do not appear to be "pronoun deficit" people who get left behind because they were improperly instructed in their effort to learn pronoun reversing.

Many things are, of course, learned. We memorize the names of the capital cities of countries. But we forget them too. And there are only, what, 150 or so countries, yet the adult vocabulary is around, what, 60,000 words. Imagine having to memorize 60,000 capitals. I am not saying words are innate, just that the learning or internalization process is clearly strained as a reasonable explanation when applied to our vocabulary without also identifying innate mechanisms that facilitate vocabulary acquisition. We often forget what we have learned, or don't learn our history very well, and can't, in situational necessity, recall the correct year the reign of Louis XIII ended. But we never seem to have this problem when it comes to reversing our pronouns—nor, most importantly, do very young children. The point here is that if pronoun reversal, as a syntactic device, had to be learned, like French history or the names of world capitals, we would expect over time a deterioration of memory, or some initial poor instruction, or any number of other contingencies that could come into play in real "learning" situations, resulting in a degraded output. But pronoun reversal is striking in its consistency; it is not hit and miss, as is our memory of French history or the world's capitals.

It would seem reasonable to conclude, then, that a) syntax does affect meaning, and b) that it is part of our bio-endowment, given to us through the evolution of our mind, and we can utilize it to construct the most complex of meanings in the most flexible of ways. It does not seem to be a cultural construction or a social product, into which one is socialized either intentionally or through the daily drudgery of Bourdieuian habit formation.

Yet, we continue to resist. It appears as if there is something like a Chomskyan Rubicon that sociologists won't cross. We (witness PS) continue to argue that "without language, people cannot…think beyond an animalistic level," when evidence shows an amazing capacity for complex cognitive operations by the pre-socialized. Or, we argue, as do PS, that "there is no proof that we have any particular syntax rules and combinations in the psyche prior to socialization," when we see children making plurals, or reversing pronouns, or performing any number of complex syntactic tasks without any visible evidence that they were taught or had enough experience to seriously use the notion of having been socialized. One can create, I suppose, a bullet train theory of learning, but it seems more plausible to suggest that what experience we have acts more to trigger what is already there than to be the learned origin of what syntactic knowledge the child possesses.

Finally, PS ask what difference this Chomskyan perspective would actually make for how we do sociology. I would say, first and foremost, where Mead, Durkheim, Bourdieu, or anyone has a hypothesis that isn't supported by the research evidence this needs to be pointed out. Much of the original "Chomsky vs. Mead" article was doing just that. Second, I think that if we get the process right, then we will be able to build some new, and of necessity, improved sociological theories of the individual/society interface. The Meadian/SI tradition has had the right ends in mind (explaining human plasticity, malleability, and ability to perform any number of roles, or occupy any social position). Where this tradition falls down, though, is its continued attachment to an earlier, and now largely discredited, set of hypotheses linking social learning to language acquisition.

In short, we are not flexible because we have multiple selves but have multiple selves because we are innately flexible. And this doesn't mean we are a blank slate upon which anything can be written. Remember, if you had to learn all that you know you don't have the time, memory storage space, or retrieval ability, to do what you can now do because of your innate cognitive mechanisms, including language. Cognitive flexibility, then, is an essentialist trait of the species (Bergesen, 2005). It's not a consequence of role occupancy nor learned from society, contra not only Mead, Durkheim and Bourdieu, but virtually the whole sociological tradition. We have put the horse in front of the cart. And, as such we are going nowhere. In point of fact, we interact because we have the cognitive machinery to do so; interaction doesn't create the faculties of the human mind/brain. Chomsky was right. Mead and Durkheim were wrong. It is as simple as that. And this isn't just a philosophical argument. It is a conclusion drawn from the research done on infants. As the pre-socialized, their cognitive capacities cannot be a consequence of their socialization.

On a more substantively specific topic, the generative grammar model has been applied to cultural structures besides language, such as styles of art (Bergesen, 2000, 2005), and the results are often counterintuitive. For instance,

it turns out that there is an identical underlying style structure to both High Renaissance and Abstract Expressionist/ Minimalist art, such that while sociology, from Sorokin to the present, has grouped the Renaissance and the Baroque as a common style in sharing a common religious content, in terms of the formal grammar of style, they are, in fact, quite different structures. Leonardo's *The Last Supper* (c. 1495-98) is more similar to the Minimalist painter Ellsworth Kelly's *Red Blue Green* (1963) than to Tintoretto's version of *The Last Supper* (1592-94). These cultural analogies would not have been possible without taking a generative grammar approach to art historical style structures.

Let me close with a plea that we all act like Mead. Remember, he wasn't a defender of the inherited models of self-consciousness from Cartesian notions of the soul to Platonic eternal ideas. He wanted to theorize self, language, and mind, and to do so he utilized what was the advanced science of that time—behaviorist psychology. For then, fine; for now, well, the explanatory model just doesn't hold up. So, be like Mead. Take what is the most advanced set of understandings about mind, self, and language in our time and utilize it in a new, and importantly distinctly sociological, formulation.

References

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Election Results

The results are in for the 2005 Theory Section Council elections. The winners are:

Chair-Elect: Karin Knorr Cetina
Secretary-Treasurer: Lisa Troyer
Council: Julia Adams
Lynn Spillman

Congratulations to all the new Council members.

Awards

• The *Theory Prize* goes to Noah Mark, for "Cultural Transmission, Disproportionate Prior Exposure, and the Evolution of Cooperation," ASR 2002, vol. 67 (June: 323-344).

This year, the prize committee had a large number of outstanding, prize-worthy entries, covering such topics as group interaction, theory-building, and the role of culture. Mark addresses a tough, well-established problem in sociology, political science, and economics—why do people co-operate so often, when the gains from selfish behavior seem so obvious? Mark identifies a previously unexplored mechanism—disproportionate prior exposure—behind people's choices to act in selfish or cooperative fashion. Mark then shows how this mechanism, when combined with a simple assumption about the benefits of repeated cooperation and the harm of repeated exploitation, can explain the development and persistence of cooperation in human populations, even under conditions that other social scientists have claimed make cooperation impossible to sustain. This innovation is particularly sociological—treating the cultural transmission of behavioral traits as the key to understanding their persistence allows Mark to establish a powerful result that should influence debates in many fields on the sources of cooperative behavior. For innovative, rigorous, and substantively informed analysis, the Prize committee awards Noah Mark the 2005 Theory Prize of the ASA Theory Section.

Jack Goldstone, Chair
Paul Dimaggio
Gary Alan Fine
Jonathan Turner

• Daniel E. Adkins (University of North Carolina—Chapel Hill) has been named the winner of the 2005 *Edward Shils—James Coleman Memorial Award for Best Student Paper*. Mr. Adkins' paper, "Unified Stratification Theory: Structure, Genome and Status across Human Societies," is an innovative synthesis of classic stratification research and contemporary genetics. The Shils-Coleman Committee was impressed by Adkins' creative blending of sociological theory on the social structural determinants of status attainment with geneticists' insights on genomic influences on phenotypes. Adkins' model predicts that social structural factors (e.g., levels of inequality, social closure) mediate the extent to which the genome influences ability formation as well as the extent to which ability influences status outcomes. The paper represents a notable contribution to sociological theorizing through its explicit recognition of the role of genetics in sociological processes and outcomes.

Lisa Troyer, Chair
Noah P. Mark
Gretchen Peterson
Lisa Slattery Rashotte
Brent Simpson

• The inaugural *Lewis A. Coser Award for Theoretical Agenda-Setting* was awarded to Margaret "Peggy" Somers of the University of Michigan. The committee, chaired by Andrew J. Perrin (UNC-Chapel Hill), included ASA president Troy Duster, SSSP president Gary Alan Fine, and Craig Calhoun (NYU and SSRC). Eight scholars were nominated for the award, which honors a mid-career sociologist whose work shows great promise in setting the agenda in sociology.

Professor Somers is the author of numerous articles and book chapters. Her work captures the historical, cultural, and dialogic elements of citizenship, law, and politics. Most recently, she has published a cutting-edge critical analysis of current uses of the concept of social capital, and a long-view historical analysis of debates over welfare state policies.

At next year's ASA meetings, Somers will deliver the inaugural Lewis A. Coser Memorial Lecture, and a salon will be held in her honor. The award was set up after the death of Lewis A. Coser by his friends, family, and colleagues, in order to spotlight the critical, theoretical sociological style Coser championed.

Book Announcement

Readers of PERSPECTIVES may be interested in a new book published by the University of Chicago Press. The book is authored by Arland Thornton of the University of Michigan and is titled *Reading History Sideways: The Fallacy and Enduring Impact of the Developmental Paradigm on Family Life.*

Reading History Sideways documents how European and American scholars from the eighteenth through the mid-twentieth centuries thought that all societies passed through the same developmental stages, from primitive to advanced. Implicit in this developmental paradigm—one that has affected generations of thought on societal development—was the assumption that one could "read history sideways." That is, one could see what the earlier stages of a modern Western society looked like by examining contemporaneous so-called primitive societies in other parts of the world.

In *Reading History Sideways*, Thornton demonstrates how this approach, though long since discredited, has permeated Western ideas and values about the family. Further, its domination of social science for centuries caused the misinterpretation of Western trends in family structure, marriage, fertility, and parent-child relations. Thornton also demonstrates how developmental thinking, methodology, and conclusions played a central role in changes in the Western world, from marriage to gender roles to adolescent sexuality. Through public policies, aid programs, and colonialism, these forces also continue to reshape families in non-Western societies.

Journal Announcement

Theory and Society plans to publish a special issue in 2006 on Jean-Paul Sartre in honor of the 2005 centenary of his birth. The special issue will be coedited by David Swartz and Vera Zolberg. The journal invites manuscript submissions that address in an original way aspects of Sartre's life and work that relate to his view of society in light of the specific cultural/social/political context in which he worked, and the role that writers, and intellectuals more generally, can play in modern societies. All submissions will undergo the normal review process of papers published by *Theory and Society* and will be evaluated in terms of their original contribution to scholarship. Our working deadline for manuscript submissions is December 2005.

David Swartz
Vera Zolberg
Theory and Society

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

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