in mind—he does mention us in the preface—I suspect that Latour's intended audience is in fact scientists themselves. He refers to a rather confusing and paradoxical "we" throughout the book. With a nod to Tonto and Lone Ranger, I ask "What you mean 'we,' Kemosabe?" The section on the science wars seems to be very much about showing the intersections among science and its critics, or the ways in which "we" are "really" all on the same side. After all, don't "we" want better versions of reality? Don't "we" want to know things about the world? Yet while this is a marvelous book for explaining how science works, Pandora's Hope is entirely inadequate for explaining how to make sense of the consequences of science on bodies and lives, both human and nonhuman. I want a science studies—and a sociology—that do more than offer benign understandings of what scientists do or help them do it better. I do not want to leave worldly interventions to scientists alone. But the "how tos" of enhancing democratization and civic participation in increasingly complicated technoscientifically hybrid situations are not to be found in Pandora's Hope, although Latour's book goes a long way in clarifying why these are necessary and important.


J. Milton Yinger Oberlin College JMY@pocketmail.com

Sociology is fortunate in having a steady supply of critics from its own ranks, pointing out weaknesses and urging changes in basic theory, methods of research, or topics of study. Since the critics are in rather wide disagreement with each other, however, and tend to speak in quite severe—one might even say apocalyptic—terms, it is not easy to agree with their messages. I have found it helpful to fall back on Durkheim's theory of deviance—it's inevitability and its value. Most of us are content with doing sociology, adapting our theories as seems wise and enriching our methods with new technologies. That is not good enough, say Lopreato and Crippen. After a "glorious" beginning "sociology soon went astray and, as is widely noted both within and without the profession, it has long been a discipline adrift. . . . To survive it must rediscover the brain, the circumstances of its evolution, and the evolution of its products." (p. 4)

I share with the authors of Crisis in Sociology the wish that sociologists might reduce some of their errors and strengthen their interpretations by paying careful attention to evolutionary biology. Every human behavior has to be biologically possible. I would quickly add, however, that very little human behavior is the direct and exclusive product of biological factors. Scarcely a week passes without one's reading about a new demonstration of how the brain works or how a newly discovered gene or the secretions of a gland influence behavior. In their first three chapters, Lopreato and Crippen trace "the early promise" of sociology and then "argue that at present sociology offers a shallow and distorted view of human nature that prevents it from understanding the real world and thus from the likelihood of demonstrating its utility to society" (p. 43).

In examining the crisis, as they see it, Lopreato and Crippen give scant attention to the shifting environments within which human behavior occurs. I believe it correct to say that most biologists are students of the environments within which the body and the brain work, agreeing with Charles Child (1924) that the concept of a fixed hereditary program built into cell and tissue inadequately explains the observed facts. More recently Steven Jay Gould has emphasized that heretability does not mean inevitability. Is it not the task of sociologists to analyze the range of behaviors that occur within the biological framework, as a result of variation in social structures and social interaction?

In the early 1950s it was widely believed that men had reached the outer limit in the ability to run a mile, having, through several decades, reduced the record to only a few seconds over 4 minutes. Then, in 1954, Roger Bannister ran a mile in 3 minutes 59.4 seconds. By 1964 more than 70 runners had broken the 4-minute barrier. Imagine the chagrin of the several that broke the barrier in some of the races only to come in fifth or sixth. This is a kind of metaphor for a field theoretical principle in which the several levels of influence on human behavior (biological, psychological, cultural, and social) converge, sometimes with one, then another being the dominant force.

Interpretations of the "crisis in sociology" in this book and elsewhere often involve the ques-
tion, at least by implication: What is sociology? There is no Supreme Court to answer the question, but probably most of us answer it by the way we do our work. Although Lopreato and Crippen offer no formal definition, they might be comfortable with something like this: Sociology is the scientific study of the influence of social structures, cultures, and social interaction on human behavior, the powerful and limiting influence of biological imperatives and needs—as developed in the process of evolution—having been taken fully into account. In their eagerness to bring biological forces into sociological study, they tend to exaggerate, or so it seems to me, the rigidity of the genetic influence. They also exaggerate the alleged failure of sociologists to recognize the importance of biological forces. "Sociology is still addicted to the increasingly implausible assumption that human behavior is solely the result of socialization" (p. 21). All sociologists are thus addicted? Some? A few?

In addition to a more nuanced use of the place of biological forces in the definition of sociology, I missed reference to another aspect of the question: Does sociology incorporate an activist dimension—the Auguste Comte of Système de politique positive as well as the Comte of Cours de philosophie positive? In its origins and in its current activities, sociology is deeply involved in "social problems." I believe that many sociologists will agree with the current president of the ASA, Joe Feagin, saying (1999) that "the strength of sociology has long resided in its intellectual diversity." Which includes a strong interest in "urgent moral and practical concerns."

An interesting chapter is devoted to "fundamentals of social stratification." The authors start with a valuable discussion of the strengths and weaknesses, as they see them, of the views of Marx, Weber, and the team of Kingsley Davis and Wilbert Moore. They shift then to a review of "Dominance Orders among Primates," drawing from this evolutionary background the view that stratification systems are much more deeply rooted in our inheritance, which most sociologists—"obsessed" with interest in complex societies, as they put it—overlook almost entirely.

"Sexual selection, the competition for mates," Lopreato and Crippen affirm, "has played a crucial role in the evolution of dominance orders" (p. 229). The persisting power of these inherited influences explains "the failure of destratification efforts." Perhaps feeling a bit uncomfortable with their emphasis on the failures of destratification efforts, the authors note that "we have deliberately stressed the prehistorical period of our hunting and fathering past" (p. 241). They discuss briefly a few changes that may have modified the rigidity of stratification systems. One would welcome a fuller discussion of the influence of the domestication of plants and animals, bringing a more stable food supply, the invention of writing, the appearance of cultural norms emphasizing more open and egalitarian systems, and other social and cultural developments that have created quite different environments within which the nature of stratification systems is worked out.

A chapter on "The Clannish Brain" examines the extent to which ethnic group boundaries are most strongly marked by kinship—by an inherited sense of the division between "us" and "them." Lopreato and Crippen cite some of the numerous sociological uses of "ethnic group" that emphasize the family aspects: Weber's "status groups," Sumner's "ethnocentrism" concept, Pareto's picture of the family with all of its appendages.

The terms used to describe and analyze human groups, however, cannot readily draw sharp lines of distinction. We are not separating lions and tigers. "Ethnic group" is now a concept widely used to refer to populations that share one or more of several characteristics—language, national background, historical memories, religion, and a label from the "others." In the United States we often refer to "Hispanics" as an ethnic group. Some Hispanics are primarily of European background and speak Spanish as their first language. Others are partly Native American or African American, speak a variety of languages, and bring to their place as Hispanics a wide variety of historical memories.

Thus we have a rather sharp distinction drawn by those who see ethnicity in "primordial" terms and others, looking at current situations, in "instrumental" terms, to use the distinction drawn by Pierre van den Berghe. One can understand why Lopreato and Crippen, given their goal of raising the evolutionary consciousness of sociologists, state that their aim is to argue the hypothesis that ethnicity, ethnocentrism, and ethnic conflict hark back to prehistoric, perhaps even prehuman times" (p. 248).

We cannot stop with that hypothesis however. The term ethnic is now used so widely to refer
to large and heterogeneous groups that our efforts to study the causes of their “unity” and the effects of their labels will be seriously wide of the mark if we have only a “prehistoric” concept.

We need more than a distinction between primordial and instrumental. Won’t someone reading this review invent a scale of ethnicity, designating the several measures that determine a group’s placement along the range from “purely ethnic” (perhaps a tribe in the headwaters of the Amazon River or in the mountains of New Guinea) to “barely ethnic,” a convenient label for a population with a few shared characteristics and facing similar problems?

References


PETER BEILHARZ
Harvard University
beilharz@wjh.harvard.edu

Why read this book, if you aren’t already in the circle? Jürgen Habermas is brilliant, always has been, and well received if not canonical in American sociological theory. Judging by good bookshop yardage, Habermas, together with Bourdieu, counts. But this book would probably not be the place to start. It is dense and often difficult. As Maeve Cooke aptly explains, Habermas’s formal pragmatics serves as the theoretical underpinning of his theory of communicative action, those two big books that somehow belong alongside Parsons’s Structure of Social Action. Here the content is quite specifically linguistic, oriented to ongoing philosophical discussion of truth, rationality, action, meaning. Of course, utterance or exchange counts mainly as example. Habermas’s interest in the life world is systemic rather than experiential, so those more interested, say, in nonverbal action or power may find themselves disappointed by the levels of abstraction involved. None of this is without irony, or paradox, for language is at once the most opaque and yet the most accessible of Habermas’s interests. Whatever happens to language after Theory of Communicative Action, language is the pivot and turn of Habermas’s theory out of Marxism, for it takes over where the earlier idea of praxis left off. Where the later Marx substitutes labor for praxis, the earlier Marx is moved by the idea of activity, where in principle, to read the story backward, speech-act also counts as activity. Enter Habermas.

Maeve Cooke gathers together papers that inform this research project, which itself grew to metatheoretical dimensions as Habermas shifted away from rethinking critical theory to reconceiving social science. In answer to the question What difference would such an approach make, for example, to the conduct of conversational analysis? Habermas can only reply as expected, indirectly. Abstraction, like theory in general, might sensitize us to matters that are not imminent to analysis. In this regard, Habermas’s enthusiasm for formal pragmatics is entirely consistent with the older project of critical theory. So what’s new? For sociologists who read Structural Transformation of the Public Sphere or Legitimation Crisis, the most tangible essays here are probably those that tangle with locals, like Charles Taylor or Richard Rorty. Habermas is a stubborn interlocutor, though it is also fascinating to watch him think, show his wealth of intellectual culture, revise his views in response to criticism. Certainly the Teutonic inflection is clear, but who else among our leading thinkers do we see using formulations such as “my mistake . . .” or “as a sociologist I ought to have known . . .”—and even if these aside are tongue in cheek, they at least show some sense of conversational detachment from our own occupational language games. There is a generosity of spirit in these conversations with Habermas, even if the density of the subject matter overshadows it, and even as that density is broken by the incidental hilarity of Habermas’s propositional examples (“9. Please bring me a glass of water. 9.1. No. You can’t treat me like one of your employees, etc.).

These essays by Habermas are worth reading, perhaps especially by those whose interests so indicate. Some larger, contextual issues occur to me, as I am reading them in Cambridge, visiting for a year from Melbourne. Let me close on a broader note, to do with culture and reception. The reception of thinkers such as Habermas in the United States, to my mind, is part of a larger phenomenon that looks from the outside like a mirror effect. At least since Tocqueville,