

***Reply to Libby Schweber’s Comments on “Neoliberalism and the History of STS Theory”***

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I want to thank Schweber for developing a generous and thoughtful [reply](#) to what is, after all, only the beginning of a reflexive cultural analysis and field sociology of STS (science and technology studies). On rereading the essay, I can see that I might not have been as clear as I should have been, especially in interdisciplinary venue, about methods and disciplines, so I will begin with a few comments on these issues, perhaps less in direct reply to Schweber, who probably understands well these issues, and more for other readers of this exchange.

In an interdisciplinary field such as STS, there is always the possibility of “evidences invisibles”— or, as the English translation of a [book](#) of that title has it, “cultural misunderstandings”— that can occur across the disciplines. Perhaps one of the least visible and most misunderstood differences is that between anthropology and sociology, two fields that shared common points of reference in social theory and sometimes also live together in the same academic department. Having spent much of my career betwixt and between those disciplines, I am especially attuned to what each has to offer the other, and the tension in Bourdieu’s work between his anthropological and sociological voices is one of the great attractions that his work holds for me.

Anthropology and sociology are both very diverse fields, and there are forms of cultural analysis and sociological explanation in both, so my comments here, as in my essay on neoliberalism and STS, rest on typologies that are used for the purpose of drawing out a contrast. A particular type of anthropological approach, which I was employing in this essay, is less about developing explanatory generalizations and more about deepening the understanding of a particular historical and cultural site. Good work in this vein provides a new interpretation that makes us think differently about ourselves, our subject matter, and sometimes both. This is essentially a humanistic project, one that has more in common with history, literary criticism, and philosophy than with the social sciences and one that is arguably an appropriate genre for an essay in a journal such as *Social Epistemology*.

Within this approach, I generally draw on a tradition of semiotic or cultural analysis that runs from De Saussure and Jakobson through Lévi-Strauss, Leach, and Dumont to post-structuralist analysts such as Boon (and Bourdieu-the-anthropologist is also deeply influenced by parts of this tradition). In the simplest form of this method, a distinction within a particular field, such as among animals in a body of myths or among research frameworks in a research field, is interpreted through the translation of homologous differences, which in turn is elaborated with more translations. At its best, the method provides a fresh look at a new or already “known” world, such as the meaning of the bear in northern hemisphere New World mythology and the jaguar in Amazonian mythology,

and the approach can result in new understandings of existing assumptions about societies and their histories and cultures. In Bourdieu's work on the modern intellectual world, this method is most closely approximated in *The Political Ontology of Martin Heidegger*. Much more than his other work on modern knowledge, such as *The State Nobility*, there is room in this short book for an anthropological voice that approximates in many places cultural analysis (a term that he would not use).

As Sahlins explains in [Culture and Practical Reason](#), cultural analysis has different goals and distinctive contributions from those of a social science project of explaining cultural systems. The method is the analysis of semiotic homologies, A:B::C:D::E:F..., rather than the explanation of those homologies by some set of sociological or material variables, such as segmental domination or individual gain, in the form  $A:B::C:D... = f(X_1, X_2...X_n)$ . (I discuss the difference in approaches in *Science and Technology in a Multicultural World* in the comparison of "cultural constructivism" and "social constructivism.") In the limited space of the essay in question, I was not attempting to develop a field sociology or any other explanatory sociology of the STS field as a changing system of intellectual and cultural distinctions, and I do not see cultural analysis or an anthropology of science as only complete if encompassed by (to use Dumont's term) a sociology of science. However, I do think the sociology of STS as nomothetic inquiry is important and that both cultural analysis and sociological analysis (again, to use a shorthand) have valuable roles to play, especially in the reflexive study of scientific knowledge. In the essay I suggested some ideas for a starting point for a field sociology of STS, such as an analysis of the position of the Bureau of Applied Social Research in American sociology and of [the Center for the Sociology of Innovation in France](#). I also flagged [an article of mine](#) that has made, I believe, some progress toward a field sociology of STS (in the journal *Minerva*), where I relate intellectual distinctions in the STS field such as the relative priority of race, class, gender, the environment, and democracy to the prestige hierarchy of the field and the relations among disciplines.

Schweber offered another suggestion for a starting point for a field sociology of STS by arguing that the positions of Bourdieu and Latour in the French intellectual field may explain his non-engagement with actor-network theory. I agree that there is potential in the approach, and I think the strategy is consistent with my suggestion and also with Bourdieu's analysis of the neoliberalization of the French housing policy field in [The Social Structures of the Economy](#). Answering the broader question about the extent to which the focus on Bourdieu's position within the French academic world could also explain his non-engagement with the sociology of scientific knowledge in the international field (that is, his attitude not only to French actor-network theory but also to a more diverse set of post-strong-program schools in the Anglophone world) would require more research. Likewise, answering the even broader question of the transition to agent-centered frameworks across a range of disciplines and subfields would require a broader scope of analysis than the social and intellectual positions internal to the STS

field. But I think that we agree that there is value in a field sociology of STS in addition to a cultural analysis of it and about some starting points for such a project.

With respect to the connections between political ideology and social theory, the elective affinity between actor-network theory and neoliberalism has been recognized by other scholars, and on this issue I am standing on their shoulders. I suggest the value of a more comprehensive analysis that includes agent-based theories in STS in general (and even in the social sciences) and the contrast with functionalist approaches and the “radical flank” of social theory, such as Marxism and feminism. When broadening the analysis to the heightened role of agency with respect to social structure that appeared in actor-network theory and other agent-oriented theories during the 1980s, it is important to avoid simple syllogisms such as the claim that structure is to agency as social liberalism is to neoliberalism. Instead, the question should be what type of agency, and even what type of associated political subject, is particularly appropriate for this discussion.

With respect to neoliberalism, I am focusing on a world-making, even entrepreneurial agency in a framework that places social structure in the background or even reduces it to an epiphenomenon of microsocial relations. In Thatcher’s terms, [there is no society](#), and in the terms of theories in the STS field, social structural concepts in social theory (race, class, gender, world systems, etc.) are superseded by agents, hybrids, and their heterogeneous networks. This absence of structural analysis is well-known and commented on by various critics, notably Kleinman. My point is to draw out a cultural implication: the absence of a bidirectional or dialectical relationship between agent and social structure is the basis of the underlying homology between theories of heroic actors who build their networks and the neoliberal, responsibilized individuals who thrive in a world in which there is no society.

The parallel analysis of the political subject of social liberalism could benefit from elaboration of my brief comparison with Martin Luther King, Jr. For the social liberal subject, society shapes the possibilities of action (especially in the form of unequal opportunities) in ways that raise questions of particularism, discrimination, and fairness. To understand this subject as traversing political ideology and social theory, the term “fairness” is helpful rather than distracting. I chose the word because of its use not only in the political debates on ideology (where debates over social fairness are ubiquitous in disagreements between American social liberals and neoliberals) but also in the intellectual debates that developed with respect to universalism and particularism in science. I was thinking of the book [Fair Science](#) and the ensuing exchanges, described in my book [Science Studies](#), between the Mertonian network (especially the Coles and Zuckerman) and the opposing network of American sociologists of science (Allison, Long, McGinness, Reskin, etc.) over the extent to which career attainment in science is universalistic or particularistic, with unequal career attainment for women often the central empirical problem.

The term “fair science” was a pun that related questions of particularism and universalism to gender disparities, with all of the associations with the term “fair sex” waiting to be unpacked in a yet unwritten dissertation. The issues of universalism, particularism, and fairness underlay a deep debate and substantial empirical literature that ran from that book through the later book *Making Science* (which also has a title worth pondering). A central question was, in Cole’s terms in *Making Science*, “Is the correlation between prestige of department and measures of output entirely a contextual effect, or a result of ‘unfair’ cumulative advantage as opposed to ‘fair’ cumulative advantage” (p. 169)? From a perspective critical of Merton on this issue, Collins and Restivo wrote, “The functionalists have consistently defended science against charges of unfairness and inefficiency” (*Sociological Quarterly*, 1983, p. 188), and in the book review from the more critical (or, to use a shorthand, science-as-particularism) position (titled “[Fair Science: A Fair Test?](#)”), Reskin commented, “I suspect that people with strong feelings about discrimination in science or affirmative-action programs will react strongly to portions of this book. Feminists may sometimes find the tone troubling” (in *Contemporary Sociology* 1980, 9/6, 794).

The terms of the “fair science” debate were linked not only to universalism and particularism but also to the evaluation of gender discrimination in science (much less so to racial discrimination, but with some exceptions, such as the work of Pearson). Moreover, as Reskin suggests, the debate was also linked to the need to remedy discrimination. In other words, this was a debate over the degree to which the playing fields of science were even or uneven, and as I suggested the debate was part of the “Kuhn loss” in the paradigm shift to the sociology of scientific knowledge, at least in the form in which it was originally configured around the microsociology of world-making agents. To reduce the argument, if there is no society, there is no need for a fair science debate. Whether the Merton network came down on the “wrong side” of the debate, as Collins, Restivo, and Reskin were suggesting, is a historical and evaluative question, but my point is that the questions raised by functionalist sociologies of science made such a debate not only possible but central.

For the social liberal subject, the relationship with the conditions that shape the life trajectory, not to mention the extent to which society is particularistic and needs to be changed, is central. The world of the fair science debate is characterized by generally unnamed scientists, the non-epynomous rank and file, whose lower career attainment is a result of the contradictions of universalistic and particularistic institutional processes. The cultural message is not the Panglossian argument that the structures of the reward system are functional for science and/or society (this is the debatable and debated topic of the fair science exchanges); rather, from the perspective of the contrastive analysis that I am suggesting, the cultural message is that a type of subject is constituted that is capable of identifying and even remediating particularism. This is the agency behind the “universalizing” social movements that call for reforms that address discrimination and that focus on equality of opportunity (hence my allusion to the social liberal forms of the

civil rights movement, although the reference is complicated due to the diversity and changing contours of that movement). But the social liberal subject is an ameliorating agency that is, from the perspective of both the radical flanks and the neoliberal conservatives, highly limited politically. As an ideal type, the universalizing movements can be distinguished from the more radical forms of political subjectivity that would call into question the fundamental structures of society such as the raced, gendered, and postcolonial structures of the ownership of the means of production. But they can also be distinguished from the conservative forms of subjectivity that trend toward market fundamentalism and images of a society as level playing field. These are, of course, typologies that can be broken down and recombined in more detailed analyses, but they represent a framework for the beginning of such work.

With respect to STS as a research field, I see the “highly individualistic model” of scientific and political activity as only one current within the field. As Bourdieu has taught, the boundaries of the field are part of what is at stake in the struggles of the field, and I have always advocated for a wide-tent approach to the boundaries of STS. The field should be conceptualized not as a network of agent-oriented constructivist frameworks but as a babel of constructivism as well as functionalism, culturalism, feminism, political sociology, and so on. This view of STS should not be interpreted as degrading or undervaluing the contributions of the sociology of scientific knowledge, from which I have learned a great deal, and especially its later, more “public” developments in the study of expertise and politics; to the contrary, I agree that the shift from functionalism to various post-strong-program research programs involved an important and widely recognized reopening of the problem of the sociology of knowledge (a topic for which Merton also made contributions, especially in his early work on science in the seventeenth century).

However, I also suggest that the new or renewed attention to the sociology of the “content” of science (its methods, concepts, problem areas, and controversies) was not necessarily as apolitical as might first appear, at least in the versions articulated during the 1980s. The sociology of scientific knowledge could have left the space of laboratories, actor-networks, and internalist controversies early on and followed feminist multicultural scholars such as Harding toward a different kind of sociology of scientific knowledge, one less focused on the nitty-gritty details of struggles over fact-making, which should not be ignored, and more on the broader issue of “whose science” and “whose knowledge” is highlighted or marginalized. In my terms and perhaps those of a few of my colleagues, the sociology of scientific knowledge could have attended more to, and could still pay more attention to, the construction of agendas of research fields and the contours of done and undone science within and across those fields. If one envisions the sociology of scientific knowledge in this way, then the political neutrality of a type of sociological explanation (especially one focused on intrafield dynamics the structures as the outcomes of performances rather than as also their shaping forces) is less politically neutral than it may first appear to be. Of course, that view may also be better adapted to

an academy and society undergoing neoliberalization than those of the radical flanks, which, if concentrated organizationally, risk suffering [the fate of Berkeley's criminology school](#). Although I am not speaking from the center of the STS field, I will continue to issue an invitation to a reflexive, critical project in addition to a reflexive, explanatory one, and I am happy to find that a colleague of Schweber's caliber agrees that such an invitation is to be welcomed and discussed.

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