

The Paradox of Positivism

The essays in *The Politics of Method in the Human Sciences* contribute to a historical and comparative sociology of social science by systematically comparing the rises, falls, and absences of “methodological positivism” across the human sciences. Although all of the essays are of extremely high quality, three contributions develop the argument most fully: George Steinmetz’s introduction and William H. Sewell Jr.’s and Steinmetz’s contributions to the volume. My remarks focus on these three pieces, drawing on the other contributions to illustrate aspects of the argument or to suggest tensions that need exploration.

What Is Positivism?

What are the authors trying to explain? The term *positivism* has at least three meanings. It can be a commitment to social evolution in the sense of Auguste Comte and Emile Durkheim. It can refer to an articulated philosophical tradition: logical positivism. Or it can refer to a set of scientific research practices: methodological positivism. It is the last meaning that is most relevant for Steinmetz (2005c: 109).

Methodological positivism refers to a concept of knowledge, a concept of social reality, and a concept of science. First, it is an epistemology that identifies scientific knowledge with covering laws—that is, statements of the type “if A occurs, then B will follow.” Second, it is an ontology that equates existence with objects that are observable. Third, it is associated with a self-understanding of scientific activity in which social science is independent of the reality it describes (Steinmetz 2005a: 32; 2005b: 281–83).

Establishing the Phenomenon

If this is methodological positivism, the second question is: In what disciplines and during what period were this conception of knowledge, this ontology, and this self-understanding of social science dominant? A central claim, running through both Steinmetz's and Sewell's articles, is that positivism had a period of dominance from 1945 up until the early 1970s in history and sociology but not, as Webb Keane (2005) shows, in anthropology. From about 1970 this domination has slowly receded. Still, positivism in some form retains a surprising resilience in the face of sustained attacks in many disciplines. Indeed, for Steinmetz (2005a: 17) positivism in sociology has had an "unnaturally" long life.

What is the evidence for this periodization? Steinmetz examines journal articles and major works of sociology from the 1930s and draws two conclusions. First, the *American Journal of Sociology* and the *American Sociological Review* were more cosmopolitan in the geographic scope of their topics, and much more methodologically eclectic, than they were after the 1960s. Second, at the end of the 1930s "nonpositivists," above all Talcott Parsons himself, "had an important place in U.S. sociology" (Steinmetz 2005b: 293). By the 1960s, Steinmetz (*ibid.*) argues, this situation had changed dramatically (see also Steinmetz 2005c: 113–17). It was not that positivism eliminated non-positivist alternatives. Rather, a positivist conception of science became the dominant pole of the social scientific field in sociology (*ibid.*: 112–13). Sewell (2005: 176–82) documents a similar but slightly later shift in which sociological techniques were imported systematically into history in the 1960s.

By the mid-1970s, however, the situation seems to have changed. Sewell documents this turn most completely. Postpositivism had two manifestations in history: one was the cultural turn, and the other was the emergence of neo-positivist rational choice theory, network models, and so on (*ibid.*: 198–99). In sociology and other disciplines positivism had a longer afterlife, however. Indeed, Margaret R. Somers (2005: 241–42) argues that the rise of social capital theory represents a retreat from the programmatically antipositivist stance of classical sociology.

The volume also documents disciplines in which positivism did not win out in the 1960s and 1970s. The most striking case is anthropology, where, as Keane points out, methodological positivism has never been dominant. The brief high positivist moment in the 1950s was swamped in a wave of criticism in the 1960s and 1970s (Keane 2005: 67–69). Instead, this intellectual field has

been polarized around a debate between “particularistic” and “theoretical” accounts that does not fit neatly into the issue of positivism at all.

This produces three empirical problems. First, what explains the rise of positivism in sociology and history in the postwar United States? Second, what explains the rise of nonpositivist forms of explanation, together with the survival of certain forms of positivism, after about 1975? Third, why was positivism stronger in some disciplines, such as sociology and history, than in others, such as anthropology? The remainder of my remarks focus on the first of these questions, because it seems to me that the argument is most developed here.

Explaining the Shift from Prepositivism to High Positivism

Steinmetz’s article, developing previous work, is key. He considers four explanations, two internalist and two externalist, for the rise of methodological positivism in the postwar period. Finding none of these explanations satisfactory, he develops an alternative Gramscian account based on the idea of Fordism.

Internalist explanations account for the development of social science in terms of the “‘immanent’ development within the body of social theory and knowledge of empirical fact itself” (Parsons 1949: 5). The first of these kinds of explanations suggests that sociology tried to imitate the most advanced science in the field; in the case of the postwar United States, this field was physics. But the problem here is that there were important disciplinary differences in the degree to which disciplines pursued positivism. Further, the science considered most advanced is in any case sociohistorically determined. So this cannot explain why some disciplines became positivist and others did not.

The second “internalist” explanation, drawn from Pierre Bourdieu, is that positivism is a strategy of distinction, defined as a way of displaying “distance from necessity.” But it is not obvious that positivism is “more” disdainful of necessity than other nonpositivist stances (Steinmetz 2005b: 290). As Steinmetz (*ibid.*) argues, “Bourdieu cannot explain why certain definitions of distinction will be more successful than others.” Thus, concludes Steinmetz, neither *internalist* account can explain the rise of positivism in postwar U.S. sociology.

What about externalist accounts? One simply argues that the rise of positivism was determined by “funding constraints.” The basic problem here is that funding streams both preexisted and outlasted high positivism. Thus Steinmetz (*ibid.*) writes, “Taken alone, funding cannot account for the waxing and waning of methodological positivism.” Steinmetz then considers what he calls “macrosocial” externalist accounts. Arguments of this type attempt to identify a shared “deep structure,” like Georg Lukács’s reification or like Michel Foucault’s episteme, common to “all of the actors in a settled scientific field” (*ibid.*: 291). In its classical version this argument, growing out of *Capital*, suggests that capitalism’s social laws appear to all social agents as natural laws. Thus it is natural that capitalism produces a social science that attempts to ape natural science. Like all of the other accounts, Steinmetz suggests, this argument cannot capture the historical specificity of the rise of positivism. It accounts neither for the fact that positivism arose after 1945 but not before nor for the fact that positivism appears to be in retreat as capitalism strengthens. Thus these social epochal approaches do not explain the temporal patterning of the rise and putative decline of positivism.

Despite their criticisms of the macrosocial approach, Steinmetz and Sewell (2005: 173–74) both want to preserve a variety of this type of argument. Drawing on the work of Antonio Gramsci and his followers, they seek to provide an account of the rise of positivism in terms of changes in the “mode of regulation” of capitalism. Specifically, they suggest that high positivism resonated with “Fordism,” that is, with a mode of capitalist regulation characterized by “Keynesian management of aggregate demand, full employment strategies, welfare state institutions, and highly bureaucratized forms of both public and private management” (Sewell 2005: 180). The category of Fordism comes from the regulation school and ultimately from Gramsci (1971: 279–80). It is a set of techniques that ensures a balance between capital accumulation and consumption (Steinmetz 2005b: 294). Fordism partially counteracts the fundamental problem of capitalism: the inability of individual capitalists to predict future demand and invest accordingly. It does this by combining Taylorist methods of mass production in which the work process is organized according to methods of scientific management, a policy of relatively high wages, and an effort to control the consumption patterns of the working class through cultural mechanisms. Fordism is thus a “mode of regulation” aiming to counteract the tendency of the rate of profit to fall by establishing conditions for stable effective demand.

It is important to distinguish between the existence of elements of Fordism and Fordism as a society-wide mode of regulation. For example, Henry Ford introduced the combination of mass production, high wages, and cultural penetration from 1914 to 1919, but he sharply opposed the New Deal, which, argues Steinmetz, was largely a projection of Fordist methods onto society as a whole. Fordism, then, is a society-wide system of capitalist regulation. The elements of this system appeared at a local level during and after World War I, but they were extended to whole societies mostly in the post-World War II era.

With the basic terms of the analysis now clear, we can ask what the connection might be between Fordism and methodological positivism. Steinmetz and Sewell provide a basically Marxist analysis of this connection. Further, like Marx they provide two formulations of their argument, although they do not always clearly distinguish them. In one formulation, methodological positivism is said to resonate with the underlying reality of Fordism. The Fordist mode of regulation makes positivism plausible. As Steinmetz (2005a: 16) writes, "My essay offers a historical sociology of postwar U.S. sociology that reconstructs the macrosocial conditions that led sociologists to find it increasingly *plausible* to describe the social world in a positivist-scientistic manner." Or as he puts it in his article, "Orderly postwar Fordist societalization resonated with positivist notions of repetition" (Steinmetz 2005b: 296). Sewell (2005: 197) seems in places to have a similar view, tracing the other side of the turn when he writes that "systemic changes in the mode of economic regulation have had profound effects on people's daily social experiences." In this version of the argument, methodological positivism is a conceptual reproduction of the basic forms social experience associated with Fordism. This seems to be an argument not unlike that of the Marx (1994: 211) of the 1859 preface, who suggests that the "economic structure of society [constitutes] the real foundation . . . to which correspond definite forms of social consciousness." But this argument is combined with a second, very different account.

In the second formulation, methodological positivism is a form of "misrecognition" or conceptual inversion of Fordism's underlying reality. Thus at times both Steinmetz and Sewell emphasize the paradoxical, rather than the direct, connection between Fordism and positivism, on the one hand, and post-Fordism and postpositivism, on the other. For example, Steinmetz (2005b: 298) writes that "the integration of sociology into the Fordist domes-

tic and foreign policy scientific infrastructure seemed paradoxically to validate the claim that science was ‘value-free.’” Sewell (2005: 190), again writing about the other side of the connection, states, “Here . . . is a vexing paradox: during the very period when historians have gleefully cast aside the notion of structural determination, the shape of our own social world has been fundamentally transformed by changes in the structures of world capitalism.” This account has its lineage in *The German Ideology*, in which Marx and Friedrich Engels (1994 [1845]: 111) suggest that the characteristic feature of all ideology is that “men and their circumstances appear upside down . . . as in a camera obscura.” Thus, as in Marx himself, two quite different theories of ideology underlie the connection that both Sewell and Steinmetz seek to establish between the economic structure and forms of social consciousness. Examined more closely, it becomes clear that the “reflection” and “inversion” arguments apply to different aspects of the overall syndrome of methodological positivism.

This is most obvious in Steinmetz, who argues for five connections between Fordism and positivism: a political one (the military-industrial state), an economic one (Keynesianism), a cultural one (the rise of mass consumer culture), a spatial one (the dominance of the nation-state form), and a geopolitical one (U.S.-style imperialism). It is important to examine each. First, the main effect of the integration of social science into the domestic and foreign policy establishments was that it “buttressed the value/fact dichotomy” (Steinmetz 2005b: 298). Government support made social scientists “conceive of themselves as a separate and autarkic scientific community” (ibid.) despite the fact that they were integrated into social reproduction in a way that they had not been in the pre-Fordist period. Second, Fordism stabilized capitalism through its countercyclical policies and attempts to increase effective demand. Thus, as Steinmetz (ibid.) writes, “The steady improvement of the standard of living and the thickening of the welfare statist safety net lent credence to the idea of general social laws.” Third, Fordism flattened cultural differences lending credence to positivism’s claim to be a “context-free” form of knowledge. Fourth, the “state-space” of Fordism reinforced cultural homogenization. The “nation-state” was regarded as an “obvious,” “taken-for-granted” category of analysis and understanding. Fifth, Fordist imperialism justified itself through a convergence model of social development “modernization theory.” This differed dramatically from what had gone on from 1880 to 1945. As Steinmetz (ibid.: 302) puts the point:

"The organization of core-periphery relations in the period from the 1880s to 1945 had encouraged the development of separate theories and even sciences for the colonized. . . . By contrast, the new U.S.-dominated postwar global regime reinforced the positivist program of using a single model to analyze both 'other nations' and 'one's own country.'" This set of arguments is arrestingly bold and highly sophisticated. As such, it deserves careful scrutiny.

Importantly, the mechanisms that Steinmetz proposes linking Fordism and methodological positivism differ according to the various aspects of Fordism he discusses. While the connection between positivist social science and the Fordist state is one of misrecognition, all of the other connections suggest a reflection theory, in which methodological positivism is a kind of conceptual reflux (to put it crudely) of the underlying experience of Fordism. Further, while all of the other connections between Fordism and methodological positivism refer to the practice of social science, the first one refers to scientists' conceptions of themselves and their activities.

Of course, there is nothing inherently wrong with arguing that methodological positivism both resonated with and misrecognized Fordism. Still, a question remains. Why did the American Fordist state produce positivist misrecognition along some dimensions but not others? Specifically, does this misrecognition have to do with Fordism, or is it connected more particularly to its postwar American form? It is to this question that I now turn.

Positivism and the Intellectuals

A comparative examination suggests that the self-conception of intellectuals has varied widely among different Fordist regimes. The particular, and paradoxical, self-presentation of social science as disinterested that consolidated in the postwar United States seems to have more to do with features particular to that historical context than with Fordism per se. In any case, this is what I would like to propose for the sake of argument. Let me begin with a brief set of comparative remarks.

The Steinmetz-Sewell thesis insists on arguing that Fordism arose in the "postwar" period. The "interwar" period in contrast is treated as "pre-Fordist." This conceptualization seems to me to depend very much on the United States and to be somewhat less applicable to other contexts.

What, in particular, are we to make of fascism in this scheme? This is a particularly important question for Steinmetz given the centrality of

Gramsci to his thinking. Gramsci argued that at least three key aspects of Fordism—the increasing importance of industry in relation to finance, a policy of high wages and rationalized work routines, and attempts to counteract the tendency of the rate of profit to fall—were all established in the 1920s and 1930s (Gramsci 1971: 279). Further, it seems relatively clear that one of the regimes that Gramsci had in mind in his discussion of Fordism was fascist Italy. It is worth emphasizing that Italy in the fascist period developed a rather successful form of Fordist regulation based on mixed public private enterprises organized from the early 1930s (De Cecco and Pedone 1996: 262). It is also widely argued that National Socialist Germany was the first country to introduce Keynesian fiscal policies (Roseman 1996: 210). The question then is, is there evidence of the same connection between methodological positivism and Fordism for these cases of early Fordism, as there was in the United States in the postwar period?

This would require real research, but there may be some reason to think that things in these cases were a bit different, especially on the issue of the self-conception of social science intellectuals. Indeed, the notion of the intellectual as a disinterested expert seems foreign to both classic fascist regimes, despite their Fordism. Let me speak briefly about Italian fascism. As much recent research shows, Italian intellectual life under fascism was surprisingly complex and pluralistic. There was clearly a positivist and scientific group of social science intellectuals, especially those gathered around Corrado Gini (inventor of the Gini coefficient) at the National Statistical Institute (ISTAT). This group produced a highly specific kind of racial theory, very different from its northern European and German counterpart. Although this theory had a positivist flavor, the ISTAT intellectuals had a surprisingly antiempiricist, and thus antipositivist, conception of race and nation. In his fascinating article “The Scientific Basis of Fascism,” Gini (1927: 103) insisted that the true interests of the nation could not be confused with the empirical interests of the underlying population. These true interests could be established only by balancing the interests of present and future generations. The reasoning here is not so different from that of Lukács (1971: 51), who in *History and Class Consciousness* insisted on “the distance that separates class consciousness from the empirically given, and from the psychologically describable and explicable ideas which men form about their situation in life.” In other words, Gini’s nationalism, like Lukács’s Marxism, refused to be trapped at the level of empirically describable interests.

Moving out of the strictly positivist milieu around ISTAT, the point becomes even clearer. Arguably, the dominant Italian intellectual of the fascist period in Italy was Giovanni Gentile, a man radically hostile to positivism, particularly to the idea of the politically disengaged intellectual. Gentile became Mussolini's first minister of public instruction, and his school reforms still constitute the basis for much of Italy's system of public education. He also played a prominent role in what was arguably the most important cultural enterprise of the fascist period, the *Treccani Italian Encyclopedia* (Turi 1980). Gentile's philosophy of actualism developed as a critical reaction to Marx's *Theses on Feuerbach*, a text that the Sicilian philosopher was the first to translate into Italian. Gentile argued that the valuable element in Marx's philosophy was his notion of praxis, the self-making of human beings. But, he argued, Marx obscured this element by wedding it to an outdated "materialist" and positivist philosophy that insisted on the primacy of the economy (Gentile 1974: 54–55; Turi 1995: 65–66).

Thus, much as Lukács and also Gramsci did, Gentile insisted on the importance of the Hegelian and nonpositivist character of Marx's philosophy of history, though he turned this analysis in the service of fascism rather than bolshevism. By the 1920s Gentile had developed this viewpoint into a full-scale attack on positivism in all its forms, which he sought to wed to the regime. "Positivism," he wrote, "was defeated in its major and minor representatives; persecuted, hunted, satirized in all its forms" (Gentile 1929: 21). For Gentile, fascism was fully part of this reaction against positivism. Fascism was "thought that is action" (*ibid.*: 38). Thus for him, fascism was the supersession of Hegelian Marxism rather than its antithesis.

Gentilean actualism was premised on a radical rejection of one of the central elements of methodological positivism, the separation of the act of knowledge from the object known. Of course, this may be simply an Italian peculiarity, or more systematic research may reveal that positivism had more importance in Italian interwar culture than Gentile's dominant role would suggest.

However, if my analysis here is broadly correct, it suggests that antipositivism, at least in terms of the self-conception of social science intellectuals, is quite compatible with Fordism. Indeed, given the active role that intellectuals play in Fordist regimes, one might argue that an antipositivist conception of the role of the expert is more compatible with Fordism than a positivist one.

All of this, I think, suggests that the connection between positivism and

particularly scientism and Fordism has to be viewed in a broader temporal and comparative perspective than the Steinmetz-Sewell focus on the postwar United States. The points that I have made for Italian fascism could clearly be extended. How does methodological positivism in the social sciences relate to social democratic, National Socialist, and state socialist forms of Fordism? It would seem, on Steinmetz's argument, that the most highly developed Fordist regimes, particularly Scandinavia and postwar Germany, would produce the most positivist social sciences. As Steinmetz (1994: 189) himself writes, "Though several elements of Fordism were pioneered in the United States . . . Fordism was less complete in the United States than in many other countries." Would one not, then, expect methodological positivism to have been strongest in postwar Germany and Scandinavia and not in the United States? This seems unlikely on the face of it, although, again, research is needed here.

Conclusion

It may turn out, then, that a more differentiated theory of the connection between positivism and Fordism is needed. Value neutrality and concept independence may not grow in any natural or direct way out of Fordism, though other elements of methodological positivism may be more directly rooted in Fordist experience. Rather, one of the puzzles may be to explain the anomalous coincidence of Fordism and social scientists' self-conception in the postwar period in the United States. Precisely this self-conception would seem violated, not confirmed, by the experience of a societalized capitalism, as Steinmetz himself acknowledges. There are, of course, a number of ways that one might account for this anomaly. Let me try to develop briefly a hypothesis that remains within the spirit of the Steinmetz-Sewell argument but alters it in some respects.

A weakness of Steinmetz and Sewell's theoretical model is its treatment of interstate competition. Surely part of the reason that scientism and Fordism went together in the United States is that scientism resonated with the American self-presentation as an anti-ideological power both before and after 1945. As Steinmetz (2005b: 307) himself points out, the debate about positivism was closely connected to the struggle against fascism and to the Cold War. Philip Mirowski (2005: 160) reinforces this point, showing that positivist accounts of the autonomy of science from society were in part

constructed against “Marxism . . . and fascism.” Thus the dynamics of inter-state competition may go some way toward explaining the paradoxical coincidence of Fordism and social scientist positivist self-conception in the postwar United States. Thus I would suggest two ways forward. What is needed first is research focused on establishing the specific mechanisms linking elements of the Fordist mode of regulation to elements of methodological positivism. As I have argued, in Steinmetz’s own account these mechanisms are rather complex and differentiated. Second, it would be worth establishing how these connections vary across Fordist modes of regulation. Again as I have argued, Fordism in fascist Italy was probably associated with a radically different conception of the social scientist than that dominant in the postwar United States. I have also suggested that we might expect similar differences in social democratic, National Socialist, and state socialist forms of Fordism as well.

Whatever one ultimately thinks about the Steinmetz-Sewell theses, or about my roughly sketched countermodel, there is no doubt that both authors have bestowed renewed energy on the sociology of knowledge by breaking with cramped internalist accounts and developing a bold argument about the connection between modes of capitalist regulation and knowledge. This is a research program very much worth pursuing in Lakatosian fashion. We now need both to develop the core and to incorporate the anomalies.

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