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## ***The Politics of Method* and Its Agentic, Performative, and Ontological Others**

*The Politics of Method in the Human Sciences* is an extraordinary book. It is extraordinary for the sheer amount of intellectual ground covered in the individual chapters and for its significance. By historicizing and provincializing each human science in turn, *The Politics of Method* opens the door to a true reflexivity, which is the necessary condition for what Michael Burawoy (2005: 511) calls “critical” consciousness in his conclusion to the volume. But *The Politics of Method* is also extraordinary for the theoretical ambition of its editor, who seeks to subsume the entire epistemological trajectory of the postwar American human sciences under a single interpretation. This was no small task: the book is over 600 pages long, and, like many edited volumes, it is also very eclectic—full of tensions, divergent perspectives, and (sometimes) somewhat contradictory substantive claims.<sup>1</sup> Like the pieces of a kaleidoscope, each chapter has a unique character yet combines with the other chapters in multiple and intersecting ways to form different images and configurations, all of which offer a particular perspective on what is sociologically important about the epistemology of the human sciences. Ultimately, what we may call the “Steinmetz thesis”—the argument that positivism has found a fertile ground in the postwar American social sciences (with the notable exception of anthropology) for reasons that have to do with the particular mode of regulation of the American economy—is just one plausible way, among many, to articulate a very disparate set of case studies.

The strength of George Steinmetz’s thesis is to offer a unitary perspective for this volume. Its weakness, however, is to gloss over the real differ-

ences in expository and explanatory style that run across the chapters. My purpose here is to emphasize some of these differences and analyze how they intersect with Steinmetz's argument. I identify three additional ways to cut across the material presented in this book, which I call "dilemmas" because they reveal analytic choices made by the editor (and suggest that he could have gone a different way). However, rather than invalidating Steinmetz's hypothesis, I suggest that these alternative routes may help deepen it further through the incorporation of more concrete, particularly agentic, considerations (dilemma 1), performative elements (dilemma 2), and ontological questions (dilemma 3).

### The Internalism versus Externalism Dilemma

Should we approach the epistemological and methodological nature of disciplines from the point of view of their internal intellectual logic or from the point of view of the institutional context in which these ideas are entrenched? Since *The Politics of Method* is organized around disciplinary identities and histories, many chapters resort to the first explanation—that is, ideas stem chiefly from other ideas. This may happen in particular fields: for instance, the chapter by Emily Hauptman on political theory shows that intellectual change may take place through either accommodation of or opposition to competing ideas. But it often also crosses fields: several contributors, for instance, mention the role played by literary studies as purveyors of alternative epistemological models to the human sciences. To be perfectly fair, all the "internalist" authors suggest that these new ideas are themselves always connected to broader social changes—for instance, the rise of the Third World or of women's movements—but they do not seriously reflect on the how and why of such an association.

Another version of internalism may be found in the argument that a particular field is held together by a specific vision that persists over and above historical change—whether it is Boasian cultural relativism in anthropology, in the chapter by Webb Keane, or sociology's (and economics' but in a different way) fascination with "the idea of outcome" at the expense of more process-oriented conceptualizations. Thus in Andrew Abbott's chapter, the focus on one particular substantive concept—the idea of outcome—permits an interesting contrast between the epistemological and ultimately methodological worlds of economists and sociologists: sociology is backward-

looking, that is, it seeks to understand the steps that lead to a particular outcome; economics tends to be more forward-looking, that is, the outcome in the present is often the result of calculations about what the future is anticipated to be like.

The externalist story, on the other hand, is most compellingly presented by William H. Sewell Jr. and Steinmetz in their related analyses of the evolution of postwar American history and sociology. Drawing on Steinmetz's macrosociological approach, they both focus on "the impact of large-scale social structural processes and cultural discourses on sociologists' sense of the plausibility of different ways of thinking about the social" (Steinmetz 2005b: 278).

Steinmetz's argument is that the rise of "big science" sociology, and its articulation with the advent of the Fordist mode of economic and social regulation, sustained a particular epistemological doxa that he characterizes as "positivist"—in short, empiricist, causally oriented, and universalist. But how do we demonstrate the actual empirical connection between the two? Steinmetz posits that the answer must be found in the *nature* of postwar regulation—a centralized social and economic regime oriented toward standardization and mass production, organized around fairly stable social arrangements, and globally imperialistic. There is, it seems, an "elective affinity" between these characteristics of postwar regulation and the generalizing orientation of the positivist epistemology, which established itself in the American human sciences. In Steinmetz's words, the macrostructural conditions of the production of social-scientific knowledge made it more "plausible" to *imagine* the social world in the form of universal causal laws.

This is all fine and nicely argued, but is it sufficient? Incidentally, Philip Mirowski makes a somewhat similar argument for postwar economics and philosophy of science. His demonstration, however, rests on a much more concrete examination of the personal networks at the core of the supportive "Fordist" institutional nexus. Mirowski emphasizes the nontrivial consequences of economists' and philosophers' joint involvement in something called operations research, undertaken, for the most part, for the U.S. Department of Defense, the U.S. Office of Naval Research, and the RAND Corporation.

Mirowski's analysis suggests that what may be at stake is not simply the reference to Fordism but something much more fine-grained and detailed. In particular, it points to the need to reflect, perhaps, on the specificities

of *American* Fordist regulation. So rather than do away with Steinmetz's model, I would urge that we push it much further and much more systematically: in short, we will not understand the unique character of American "positivism" if we do not analyze what kind of state is this American state that emerged after World War II. In that respect, the military component appears absolutely crucial, although its influence was arguably stronger in economics than in other disciplines. Other "Fordist" states, for instance, in western Europe, understood themselves in very different ways, primarily as industrial states (e.g., France) or welfare states (e.g., Britain). We can, I think, legitimately speculate that these different understandings had substantively different effects on the forms of social-scientific knowledge produced in these other countries.<sup>2</sup>

From this point of view, even the various regulation schools are themselves "good to think," not simply as useful analytic frameworks but as objects of study in and of themselves that may be participating in the same regulatory dynamics that positivism does. To use a telling example, Alain Lipietz famously described the French "regulationists" as "the rebel sons of Louis Althusser and Pierre Massé"—that is, the rebel sons of French structural Marxism and "indicative planning" (the democratic and non-coercive version of central planning), which Massé theorized and actively implemented as planning "commissar" (Lipietz 1994).<sup>3</sup> Lipietz's seemingly innocuous phrase thus perfectly captures the subtle articulation between the two essential dimensions of the regulation school's story, the internalist one and the externalist one. But the point I want to emphasize is that both of these stories are deeply, thoroughly French—just like the trajectory of American postwar economics was shaped by very specific intellectual and institutional developments in the United States, such as the strength of a utilitarian intellectual legacy and the use of economics by military planners and strategists.

The other obvious question raised by Steinmetz's theoretical perspective is whether the Fordism–post-Fordism line of analysis applies equally well to all disciplines. Clearly, anthropology stands apart, but the account elsewhere is also mixed. Sewell, for instance, carefully qualifies the connection between postwar social history and Fordism. If anything, he attributes social history's turn to quantitative research after World War II to a different set of factors, mainly the radical political motivations of historians and a form of "social science envy." He does, however, connect the later "cultural turn" in his discipline to anti-Fordist sentiments among historians and, ultimately, the emer-

gence of a post-Fordist mode of regulation, with its associated experience of the world as flexible, changing, global, and market-oriented. But note that Steinmetz finds that the same connection between post-Fordism and current sociology is harder to establish because of, according to him, the extraordinary resilience of positivism in the field of sociology—which, once again, suggests the persistent relevance of some kind of internalist account.

### **The Context versus Performance Dilemma**

The second interesting tension I would like to comment on has to do with the causal relationship between disciplinary frameworks and the real world, or piece of the world, the various social sciences claim jurisdiction on—what I called earlier the context versus performance dilemma. Does positivism result from something happening in the world, like the shift to post-Fordist regulation, or does it *do something* to the world (e.g., participate in that shift)? As Steinmetz argues, positivism is not just a result of Fordism: it plays a role in it; it participates in the regulation itself. In other words, we find ourselves analyzing the relative place of contextual versus performative elements—a debate recently started by Michel Callon (1998) and currently carried out by the “social studies of finance” bandwagon with great brio (see, for instance, MacKenzie et al. forthcoming).

I have already discussed the contextual perspective. The performative perspective is more difficult to identify in this set of texts, but it is clearly latent in Margaret R. Somers’s fascinating study of the rise to hegemony of the concept of “social capital.” Somers analyzes social capital theory as—essentially, though she uses a different vocabulary—an intellectual and ideological vehicle for neoliberal *governmentality* and its associated technologies of self-organized social bodies. What we have, then, is a form of knowledge—a theory about the world—which is deeply intertwined with a form of government—a knowledge-power nexus à la Michel Foucault. Indeed, Somers presents compelling evidence that powerful political institutions, most notably states and international organizations, now see their role essentially as facilitating community self-help and resourcefulness. This is the case of the American and British governments—as evidenced by her remarkably fitting quotes of discourses by Jack Straw, the British Labour home secretary, or her references to George W. Bush’s faith-based initiative—and of international organizations, such as the World Bank, which is now applying

social capital theories to design social and economic development policies. In other words, the rise to power of social capital theory is tightly coupled with a redefinition of government that amounts, in large part, to encouraging people to govern themselves—which, of course, implies a corresponding weakening of the legitimacy of traditional welfare rights. In true Polanyian fashion, society is being reduced to a market, both figuratively and empirically. And of course, this ideological and material shift is further fueling the demand for . . . social capital studies, which brings us back to the systemic nature of the connection between social-scientific theories and regulation regime that Steinmetz talks about.

### The Epistemology versus Ontology Dilemma

My take on Somers's chapter differs quite a bit from Steinmetz's (and perhaps from Somers's own). In his introduction to the volume, Steinmetz (2005a: 18) indeed summarizes Somers's contribution in the following way: "The permeation of sociology by the discourse of social capital may be an example of the successful resistance to pressures from the enviroing macro-societal context that are undercutting positivism's instinctive plausibility" in the post-Fordist regime. In short, Steinmetz presents the piece as a counter-argument to his own theory about the relationship between macrosocial context and epistemology. Now I disagree with this. I do not see any reason why the theory would not continue to hold in this case. I believe, however, that it needs a very important qualification: Somers's chapter implies that ontology, perhaps even more than epistemology, is at stake. Hence my third tension—or what I would call the dilemma between epistemology and ontology. In short, Steinmetz's "conditions of plausibility" for social thought are also ontological conditions: how do we *substantively* imagine the world, not simply explain it? From that ontological point of view, social capital theory's glorified individualism is just as post-Fordist as the third wave in historical sociology and its attention to the fleeting, the flexible, the contingent, and the cultural, marvelously described by Sewell. Somers's chapter, by contrast, presents us with the possibility of a positivist yet thoroughly post-Fordist—that is, for all practical purposes, neoliberal—sociology.

In fact, to be perfectly honest, I think that ontology is a lot more relevant than epistemology to establish the connections Steinmetz identifies. One reason is that I believe that the argument for epistemology could, and per-

haps should, be a different one—not one that focuses on macrosocial regulation but one that emphasizes, in a more traditional Skocpolian fashion, the structure of political institutions in America. Strangely enough, two of my favorite books on the social sciences, Mary Furner’s classic *Advocacy and Objectivity* (1975) and Theodore Porter’s *Trust in Numbers* (1995), did not make it into the bibliography of 55 pages. Both of these books emphasize what I see as a potentially competing account with the one Steinmetz gives, namely, the importance of the way knowledge is incorporated into the political and corporate domains in America. Both books show convincingly that positivism emerged in the United States out of a unique set of political and institutional conditions. To put it bluntly, American social scientists came to retreat into scientism, particularly quantitative and empirical scientism, to shed suspicions of political arbitrariness about their work but also to be taken seriously in an institutional context marked by political and administrative fragmentation (if not outright competition). Much of this definitional movement happened early, at the turn of the twentieth century—hence *before* the advent of Fordism—and has been sustained by the relative persistence of the same institutional conditions that shaped it in the first place.<sup>4</sup> These conditions are, perhaps, where the politics in *The Politics of Methods* should lie.

## Notes

- 1 See, for instance, the chapters by Mirowski and Lawson on economics.
- 2 Following Steinmetz, see Jessop 1989 for the beginning of such an analysis.
- 3 Massé was also, incidentally, responsible for hiring the regulationists into the state economic apparatus, so that they were his sons in a material as well as an intellectual sense.
- 4 See also Fourcade forthcoming on this subject.

## References

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