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Mark Solovey, "Shaky Foundations. The Politics-Patronage-Social Science Nexus in Cold War America". New Brunswick-London: Rutgers University Press, 2013, 314 pp.

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Book Review

Mark Solovey, *Shaky Foundations. The Politics-Patronage-Social Science Nexus in Cold War America*. New Brunswick-London: Rutgers University Press, 2013, 314 pp.

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Expanding his previous work in the history of the social sciences in the United States during the Cold War era, the historian Mark Solovey has shed some new light on the system of research funding arising from the intricate relationships between a host of collective actors, among which Washington politicians, the military, the National Science Foundation, the Ford Foundation, and, obviously, the social scientists themselves emerge as the main players. Solovey advances three points: 1) public and private patrons created an informal but working coordination, so that they should be treated as a loose system of sort; 2) these actors clearly promoted a conception of social science based on the twin principles of naturalism/scientism and the practical value of social research against other conceptions of social science; 3) the process was not without obstacles, for this conception of social science was constantly attacked for both political and scientific reasons from both sides of the political spectrum. *Shaky Foundations* includes four chapters: two dedicated to the creation and the development of the NSF, one devoted to the analysis of the relationship between the military and the social sciences, and one focusing on the development of behavioral science programs at the Ford Foundation.

Solovey's third point is his strongest: he demonstrates how the allegedly hegemonic scientific paradigm [see p. 11] always was the object of critiques and attacks – i.e., the landscape of Postwar social sciences was more complex and conflict-ridden than conventional histories of “Cold War science” have been ready to admit. Social scientists struggled hard to advance their cause, and in the process got tangled in a web of interests and challenges that somehow changed their outlooks and their ways of “doing social science”; more generally, the book is relevant because it shows how the autonomy of scientific fields always is a fragile and provisional accomplishment which depends on the relational and political skills of many different players and can never be taken for granted. In fact, it is funny to see how clumsy social scientists were in advancing their cause in Washington – something akin to a performative contradiction, so to say, for people who based their requests on, among other things, their professional ability to study *and* influence other people's behavior.

This said, an analysis of the distinctions used by Solovey to cast his description of the politics-patronage-social science nexus during the Cold War era shows that the book should be read more as a history of the politics of the social sciences than a history of the social sciences and their contexts. In other words, it seems to me that the main distinctions — liberal vs. conservative; applied vs. basic science; politically sensitive vs. “neutral” objects; neutral vs. value-laden social science – all pertain to a semantics of politics, with scant attention to the more ideational, content-wise aspects of social science. For example, a deeper discussion of what “scientism,” “unity of science,” “social engineering,” and the like meant *in practice* for Cold War social scientists and their patrons would have helped in understanding the stakes and the problems they had to face. In fact, these

terms are often treated as if they were self-explanatory, and maybe something more could have been done in presenting hard data on the projects who got grants from the various patrons described in the book and those which did not get them. An analysis of research practices would have also helped in solving one of the biggest puzzles emerging from the book: did social scientists advancing the cause of the behavioral sciences *really believed* in their scientific/practically-oriented approach? Solovey hints at a positive answer here and there, but the reader remains in doubt and a description of research practices and their practical translation into political or administrative programs would be crucial in showing that the whole discourse of naturalism was not just a strategy of adapting to non-scientific interests but a genuine epistemological point of view.

The impression of a certain detachment from the ideational content of the social sciences is reinforced by the fact that the scientific/interactional networks between Postwar social scientists are never reconstructed or described, so that a reader would learn, say, about the ways of Parsons, Merton, Stouffer, Lazarsfeld, Alpert, Sutton, and Rostow without any hint to their teacher/student relationships or to wider “schools” or groups such as “structural-functionalism,” “systems theory,” or “modernization theory.” In other words, in Solovey’s reconstruction, the meso, informal level of theory groups and intellectual movements is lost between the micro-level of individual social scientists and the macro-level of formal organizations and institutions. This becomes a serious problem when it comes to map the many fields of Postwar social science, for Solovey seems uninterested in understanding how funding strategies and processes heavily influenced the structuring of the fields themselves – for example, Solovey speaks “naively” of the strategy of “lifting the peaks,” that is of funding the best social scientists in order to advance the whole field, but never stops to reflect on the circularity of any hegemonic definition of what counts as “excellence.”

It is my conviction that a closer look at some key works by sociologists on the patronage nexus would have helped Solovey in solving this kind of problems and rounding up his theses. Speaking of the history of sociology alone, I am thinking of Jonathan Turner and Stephen Turner’s *The Impossible Science* and Randall Collins’ work on the very structure of the social science as compared to consensual, rapid-discovery sciences, which advanced bold theses on the relationship between the absence of a single paradigm, the plurality of funding agencies, and different social and societal interests; to George Steinmetz’s many works on the politics of method in sociology; to Christian Fleck’s collective biography of emigré social scientists who populated the landscape of American social science during the Postwar period; and to an old, but still interesting book like *Theory and Theory Groups in Contemporary American Sociology* by Nicholas C. Mullins, a map of different networks of social scientists working during the 1950s and the 1960s. Distinguishing between different intellectual approaches and reconstructing how they got funding to advance their theoretical or methodological views, for example, would have helped in explaining why so many different paradigms and groups of social scientists were “ready” to attack the scientific consensus during the mid- and late 1960s, and why they almost succeeded – I write “almost” because a scientific, naturalistic approach to the social sciences has never been canceled by other views.

Histories of social science written by social scientists are often plagued by an excess of detail in distinguishing schools of thought, groups, and approaches, *as if* all changes

and were to be explain by internal – in fact, ideational – factors only. On the other hand, it seems that histories of social science written by professional historians only focus on the external relationships between social scientists and their environment *as if* they were originally two independent and autonomous worlds or fields. Solovey's book partially corrects this last problem in presenting a complex and heterogeneous picture of the interests and the political positions surrounding the the great advancement of the social sciences during the Postwar era. Maybe an effort should be made to craft more balanced and diverse accounts of the development of the social sciences.

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