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REVIEWS

Towards a Sociology of Science

Rob Hagendijk

Stephan Fuchs, *The Professional Quest for Truth: A Social Theory of Science and Knowledge* (Albany, NY: State University of New York Press, 1992), xviii + 254 pp., \$54.50, \$18.95 pbk. ISBN 0-7914-0923-6 (-0924-4 pbk).

Social studies of science has not developed a fully-fledged sociological account of science because its practitioners are too preoccupied with epistemological and philosophical issues, and because its organization privileges 'conversation' over 'solid facts'. In a nutshell, this is Stephan Fuchs's diagnosis of the current state of our field.

Constructivists are still engaged in debates about the 'true nature of science' with 'rationalists' and 'realists'. Deconstructionist reflexivity is 'yet another dubious outcome of remaining over-impressed with epistemological problems such as truth, representation and relativism' (12). To argue that science is (socially) constructed is only the starting point for a sociological theory of science, not its result. A sociological theory should account for the relations between cognitive and organizational aspects of scientific fields, and show how these fields differ from one another. The goal of this book is precisely to develop such a comparative sociological theory.

Fuchs's critical remarks do not imply, however, that he is entirely negative about what has been accomplished. He holds that the

insights of constructivism are 'indispensable for any analysis claiming to uncover the internal workings and actual contents of science' (4). He is also quite positive in his evaluation of the actor-network approach, which is, in his view, 'structural' in its core concepts. But, at the same time, Fuchs dismisses many of the core intellectual concerns which preoccupy constructivists, whom he criticizes for treating 'scientific practice' as a constant instead of dealing with it as itself variable along particular dimensions.

Fuchs's theory of scientific organization is an elaboration and reiteration of the work of Randall Collins and Richard Whitley, who have analyzed science as a particular form of work organization.¹ They proposed two basic variables – 'task uncertainty' and 'mutual dependence' – as a means of investigating the differences between scientific fields. In Whitley's case, this has led to elaborate typologies of fields. Fuchs objects to such typologies and transforms the views of Collins and Whitley into a general explanatory scheme of variables. He also holds that Collins and Whitley have failed to realize the potential of their own approach for explaining the cognitive contents of science. To correct for this, Fuchs seeks to combine their organizational approach with the neo-Durkheimian sociology of groups and group cognitions, and with the technological paradigm in organization studies. He holds that this will lead to an organizational approach to science which can explain 'why some fields produce solid facts while others engage in informal conversation . . . why some sciences look more like literature, and why certain fields are rather cumulative and mature while others are very self-critical and discursive' (7). It should perhaps be noted that Fuchs sees his own analysis as 'cognitively indifferent' (196).

The idea of expanding the organizational approach to include the analysis of differences in cognitive styles is valuable. Fuchs's argument that there are systematic relations between the ways in which work is organized and differences in cognitive and discursive styles entertained in the various fields is convincing. But his analysis also shows that there is still much historical and micro-sociological work to be done to get a firm grip on differences in 'cognitive styles', and on the mechanisms at play in the establishment and reproduction of such styles. In arguing for the plausibility of his approach, Fuchs relies too much on a common-sense understanding of the differences between sociology, physics and chemistry. As long as one compares two research areas at a time it is, of course, always possible to argue that one field is characterized by a relatively 'high' uncertainty

compared to the other, and it can easily be argued that this will be reflected in differences in style, provided one takes fields that are far enough apart to be sufficiently distinct. In the absence of concrete operational definitions of key concepts, this case can easily be made. But such crude comparisons are not enough if one wants to develop a better understanding of the relations between differences in style and the ways in which work is organized. Some of the suggestions made by Fuchs are valuable – for example, those about forms of scientific change, and about controversy in relation to work organization. Unfortunately these suggestions are rather underdeveloped, and too much of Fuchs' analysis is spent in reiterating points already made by Whitley and Collins.

It seems as if Fuchs fails to elaborate such points because of his eagerness to show that the 'task uncertainty/mutual dependence' model can be applied to almost any form of work organization, and to all sorts of other issues which somehow have to do with the organization of work. This is hardly surprising, given that both Collins and Whitley explicitly draw on organization studies in their adaptations of these concepts for the analysis of scientific work. Fuchs attributes such wide applicability to his basic scheme that one becomes suspicious of its actual explanatory power.

This stress on the 'task uncertainty/mutual dependence' scheme is also responsible for Fuchs's failure to draw out the implications of the technological paradigm approach for this theory. In one chapter, he presents fair summaries of various classical studies of the relations between production technologies employed by organizations and their structural features. But instead of drawing out the specific implications of these studies for his theory of scientific organizations, Fuchs is content with the conclusion that the common conceptual denominators of classical studies of organizational technology are reducible to . . . yes indeed, 'task uncertainty' and 'mutual dependence'. This conclusion is truly disappointing. Perrow's theory of 'normal accidents', for example, is an excellent starting-point for an analysis of how the technologies employed in various forms of research affect forms and degrees of task uncertainty and/or mutual dependence. But Fuchs merely translates Perrow's variables into his own favourite concepts. In the overall theoretical model of the theory of scientific organization presented in Chapter 7, technologies are simply left out. Fuchs correctly argues that 'task uncertainty' and 'mutual dependence' should not be treated as exogenous variables, and he relates them in his theoretical

scheme to 'size', 'reputational autonomy' and the 'concentration of resources'. He is well aware that both these variables and the dependent variables in his scheme are linked to features of the technologies that characterize work in a particular field. But he subsumes 'technology' too easily under the other variables. Fuchs is right in stressing the importance of the technological paradigm for the organization of work in general, but he fails to integrate the consequences of this into his own theory.

The book concludes with a chapter on 'Hermeneutics as Deprofessionalization'. This chapter is meant as a 'concrete empirical case-study', taking contemporary sociology as the case in point. Fuchs argues that the debates among sociologists about the methodological and epistemological foundations of their field can be elucidated by tracing their social-structural and organizational underpinnings. In Fuchs's view, such debates over 'foundations' emerge more frequently in fields that are characterized by 'high task-uncertainty' and 'low dependence'. These conditions favour the cultivation of 'conversation' and 'hermeneutics' rather than 'solid facts'. This is no doubt an interesting hypothesis, and Fuchs gives a nice ideal-typical account of how prominent 'positivists' and 'interpretivists' prefer to relate sociology to lay audiences, and to the cultures and behaviours they are investigating. But that does not make Fuchs's analysis a concrete empirical case-study. Fuchs's rating of sociology as 'high' on task uncertainty and 'low' on mutual dependence relies once more on a tacit common-sense idea of the differences between sociology and other fields of science. To pass as a concrete empirical study of structural arrangements responsible for the popularity of discursive styles in sociology, this simply will not do. The analysis lacks concrete empirical evidence or comparisons in terms of resource concentration, reputational autonomy, group size, bureaucratization, coordination problems, heterogeneity, change, competition, task uncertainty or mutual dependence. Since these variables, taken together, are constitutive elements of Fuchs's theoretical model, one wonders finally what this case-study is a case-study of.

In summary, one might say that the book contains a number of interesting ideas and provocative claims, but fails to live up to its promises and implied standards.

•NOTES

1. R. Collins, 'The Organization of the Intellectual World', in *Conflict Sociology: Toward an Explanatory Science* (New York: Academic Press, 1975), 470–523; R. D. Whitley, *The Intellectual and Social Organization of the Sciences* (Oxford: Clarendon Press, 1984).

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Reviews (continued)

Philosophy of Science and its Malcontents

Anthony Corones

Steve Fuller, *Philosophy of Science and its Discontents* (New York & London: The Guilford Press, second edition, 1993), xvi + 240pp., \$18.95/£14.95. ISBN 0-89862-020-1.

Is the philosophy of science suffering an identity crisis? It depends on who you ask. Malcontents like Steve Fuller seem bent on inventing

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