

Critical Reply to David Hess' "Neoliberalism and the History of STS Theory: Toward a Reflexive Sociology"

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Introduction

Hess' article "Neoliberalism and the History of STS Theory: Toward a Reflexive Sociology" makes a strong bid "for a more integrated approach to the structure-agency-meaning triangle in STS via the use of field sociology." The paper uses the conceptual development of STS as a case study to exemplify this approach. As such, its aim is twofold, first to exemplify the application of field sociology and secondly to address a historical problem, namely, how did more structural and institutional approaches to the sociology of science come to be so marginal within STS.

As a starting point, let me indicate my full-hearted embrace of the overall project. The call to (re-)introduce structural and institutional approaches into STS has been voiced at numerous times in the past decades from various corners. Recently the cudgel has been adopted by a group of scholars around the Political Sociology of Science; key contributors include Hess, as well as Daniel Kleinman, Scott Frickel, Abby Kinchy and Kelly Moore, to name but a few. As structural approaches go, Bourdieu's field theory is one of the most coherent and promising in terms of its explanatory potential. As a version of structuration theory, it integrates macro, meso and micro analysis in a way which takes into account dominant STS concerns with meaning, interpretation and practice, without neglecting issues of power and institutions. It also offers a distinctive epistemology of science, which, at its best, can be applied both to its own practice and to that of the intellectuals and scientists being studied. For readers unfamiliar with the approach, *The Craft of Sociology* offers one of the most persuasive guides to reflexive sociology that I know.

My comments in this brief note focus on Hess' application of reflexive sociology to the history of STS. My aim is not to detract from the article and its argument, but to use my own concerns to map out a picture of what a more systematic field analysis of STS might include. In my own idealized version, such an account would do a number of things. It would offer a reflexive account of omissions (misrecognitions) as well as accomplishments. It would map out the shifting contours of the intellectual field (of which STS was a part or which it constituted, depending on the level of analysis). It would engage with both the semiotic and more sociological (social spatial) aspects of Bourdieu's field theory and it would use this field analysis to explain the hold of particular key concepts. A few observations help to illustrate how Hess' article contributes to this agenda and where we might take it next.

Hess' begins from the argument that "some of the dominant conceptual frameworks of the field are inflected by decades of neoliberal thought" (178). As this suggests, his focus is on concepts and the fit between concepts, rather than an analysis of homologies between the social structure of intellectual and political fields. More specifically, Hess

argues for a 'fit' between functionalist sociology of science, a la Merton and Cole, and social liberalism and between agent based variants of STS and neo-liberalism. At the most general level, the argument is persuasive. Both Mertonian sociology of science and meritocratic versions of social liberalism argued against the introduction of particularistic characteristics, be it class, race or gender, into the distribution of scientific and political opportunities. Similarly, both agent-based views of science and neo-liberalism privilege individual actors and share Margaret Thatcher's famous dictum from 1987 that "there is no such thing as society."

Linking concepts to problems

Beginning with the discussion of Merton, I admit to not having been convinced by Hess' argument regarding the central place of 'fairness' as a core scientific value in Functionalist Sociology of Science. More specifically, I kept wondering: why focus on 'fairness' (which I don't remember Merton using) rather than on Merton's own concepts of communism, universalism, disinterestedness and organized skepticism, especially as Merton himself makes such a cogent argument about the fit of these values with a particular version of democracy. According to Hess, this omission rests on his argument that Functionalist Sociology of Science quickly divorced itself from epistemological concerns and focused its attention on the attribution of recognition. But did it really, or is this an imposition of contemporary STS' disavowal of the specificity of scientific knowledge on the Functionalist program for science? At the expense of stating the obvious, my point here is not about what science is, but on the image of science that drove their research agenda.

For Bourdieu fields are made up of problems such that individual positions within a field correspond to different problems. Any field account of concepts, and especially taken for granted core concepts, would need to demonstrate a relation with this core problem. For example, for Functionalist Sociologists of Science such as Merton and Hagstrom, recognition was important not for its own sake or the pursuit of self-interest, but for its functional contribution to this particular image of science. This point is clearly illustrated by the story of Darwin and Wallace that Hess recounts. The message of the story, it seems to me, was that Darwin ended up acting in the interests of science.

In the article, Hess rests his argument about the fit between Functionalist Sociology of Science and social liberalism on an elision of explicitly political values of 'fairness' and 'egalitarianism' with Mertonian values of universalism and disinterestedness. But are they really the same? As the passing reference to affirmative action suggests, the former are about distributive outcomes, whereas the latter are about the perceived 'needs' of science (or democracy). By the end of the discussion Hess does arrive at a definition of social liberalism in terms of equality of opportunity and meritocracy that resonates more with the Functionalist program and conceptual framework, but the discussion is murky, trying to hit too many bases, instead of identifying the precise meaning of terms. My point here is not to critique Hess' analysis (the argument of homologies remains

persuasive), but to suggest the need to situate core concepts, even those characterized by misrecognition, in the core constitutive problems of the relevant field.

Liberal Ontologies and Criteria of Value

Whereas Hess' discussion of Functionalist Sociology of Science focuses on an 'elective affinity' (189) between political and scientific concepts of 'fairness', his analysis of agent based theories focuses on a homology in core ontologies. More specifically, Hess' argument rests on a shared individualistic ontology and value of entrepreneurship in both agent based versions of STS and neo-liberalism. Before commenting on this, it is worth noting that the previous discussion of Merton and company would have been considerably enriched had Hess analyzed homologies in the ontological models informing Functionalist Sociology of Science and Social Liberalism. Turning to agent based theories in STS, here I find Hess' analysis to be far more insightful (in its contribution to our understanding of contemporary STS), but again, I would have liked to have seen a more nuanced discussion linking core concepts to the set of problems constituting the intellectual field and to differences as well as similarities in the political and intellectual deployment of seemingly similar agent based models.

One of Hess' main points seems to be that both STS and neoliberalism share a highly individualistic model of scientific and political activity. The point is well taken, but are they really that similar in the type of activity which they set out to model? One of the striking characteristics of agent based theories of science — be they ethnomethodological studies of discourse, laboratory studies, actor network analyses or social world analysis — is a principled indifference to the content and specificity of the knowledge. But whereas STS refuses to privilege any particular criteria or 'interest' in the establishment of scientific claim, neo-liberalism is explicitly and proudly biased towards business and their interests. The point is not to deny the central place of individualism in their dominant ontological models, but to invite further reflection on the way in which that individualism fits with other components of their ontological model, core problematic and criterion of value.

Bourdieu and Structural Analyses

My last point involves a plea for field theory to move beyond the observation of elective affinities to a more systematic analysis of the structural homologies linking the fields and the basis of those homologies. Bourdieu's work on fields can be divided between his semiotic analyses and more sociological work. Curiously, while Hess calls for a return or renewal of interest in structural and institutional analyses, his own discussion remains focused on concepts and their 'fit'. A return to one of Hess' examples helps to illustrate how the inclusion of a more systematic analysis of fields as social spaces might enrich the analysis.

I take as an example a key moment in Hess' historical narrative, namely the 'missed opportunity' for STS to embrace Bourdieusian field theory. Hess writes that Bourdieu's

theory of scientific fields was available since the mid-seventies. He also suggests that Bourdieu's failure to defend his own theory in the face of criticisms from Latour and Callon (and Knorr-Cetina, who was based in Germany at the time) is partly to be blamed for its marginalization. But this account ignores the dramatic differences in the structuring of intellectual fields in France and the Anglo world. If one looks at the structural position of Bourdieu, Latour and Callon in Paris at the time, a different account of these developments emerges. Within the Parisian intellectual world, Bourdieu and his followers (a number of whom such as Eric Brian were engaged in Bourdieusian history of science) were located at the *École des Hautes Etudes*. In other words they were the orthodoxy, at least in the field of sociology or rather of Parisian social science (my own difficulty in deciding which points to the challenge of applying the concept of 'field' to empirical cases). Latour and Callon developed their distinctive approach from the *École des Mines*. While they gained considerable recognition and following in the Anglophone world, when it came to science studies in Paris they were part of the heterodoxy.¹

Far from weakening Hess' argument, attention to the social structure of relevant fields supports his general argument, while nuancing some of the individual claims. The far greater reception of Latour's theory in the UK and US than in France fits with Hess' argument for an elective affinity between ANT and neo-liberalism (which dominated and dominates Anglo political discourse since the 1980s but has had far less traction on the Continent). Similarly, Bourdieu's orthodox position and Latour's heterodox one helps to explain why Bourdieu did not actively defend his analysis of scientific fields in the face of criticisms — after all, why should he from a position of orthodoxy?

In Sum

Moving beyond specific claims to the broader argument, while Hess' conceptual analysis highlights logical resonances or elective affinities, it does not in its current form 'explain' how and why particular authors came up with particular theories and how they came to be adopted by particular broader intellectual communities and marginalized by others. The limitation does not, to my mind, stem from limits of Bourdieusian field theory per se, which offers many analytic tools for that kind of explanatory sociology of knowledge, but rather from the need to integrate the semiotic and social spatial aspects of his theory and to apply them more systematically. In short, while Hess' article makes a number of provocative claims and offers a promising path to follow, it also leaves lots of work for the rest of us.

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¹ This structural positioning was visibly played out in the early 90s in a series of public lectures in which the members of the *Ecole des Hautes Etudes* invited representatives of different strains in the history and sociology of science to speak, including Latour. At stake was the possible election of one or another to the institution (the *École des Hautes Etudes* elects its academic staff). Latour, needless to say, was not elected, but went on over 20 years later to be appointed Vice President of Research at Science Po, positioning him at the center of orthodoxy in the Parisian social science intellectual field.

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