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Whatever happened to knowledge?

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Fifty years on, Kuhn's *Structure of Scientific Revolutions* (1996 [1962]) remains a landmark, but understanding its consequences for science studies is not simple. In many ways it used ideas from Conant and Polanyi, but it reinserted claims about ungroundable premises from neo-Kantian philosophy, with the predictable result of problems over relativism and scientific progress. These elements remained in social constructionism, leading to problems over reflexivity and reality. Latour, in *Science in Action* and elsewhere, determinedly turned away from cognitive explanations in favor of networks. But networks do not explain on their own. So the task of socializing the epistemic and espistemologizing the social returns.

Kuhn's *Structure of Scientific Revolutions* was, at the time, a dramatic break with something, though in retrospect it is more difficult to see what the break was with than it seemed at the time it was published. 'Positivism' is the usual candidate for the thing that it broke with, but the book was published in the most 'positivist' of book series, and was seen as unproblematic by that most positivist of readers, Rudolph Carnap. The basic framework of the book, the idea of conceptual revolutions, was taken over from Kuhn's mentor and one of the crucial figures in the pre-history of science studies, James Bryant Conant. Conant had employed Kuhn in the Harvard course he had designed to teach non-science undergraduates what science was, from the point of view of the working scientist struggling with scientific problems, rather than teach them chemical and physical formulas. Much of the descriptive account of the experience of science in Kuhn's book was taken over from Michael Polanyi (1958), whose notion of tacit knowledge, among other things, made its way into the concept of paradigm.

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Before Kuhn

What made Kuhn revolutionary, and at the same time retrograde, was something not to be found explicitly in these writers, except in a very attenuated way in Carnap's internal–external distinction and conventionalism: the kind of radical relativism about the foundations of science that Kuhn built into his notion of paradigm. This too had a source, and it was a source threaded through the 20th century in various guises, including one that had a special influence at Harvard itself through Alfred North Whitehead. Also at Harvard, the physiologist/chemist/philosopher/sociologist Lawrence J. Henderson promoted the idea of 'conceptual schemes' (1970 [1941–42]); and Carnap, who spent time at Harvard in the late 1930s, developed an influential analysis of the internal–external distinction that served to translate these issues into linguistic terms, but also to conceal the problem of relativism by treating 'external' questions as problems insoluble by the kind of formal analysis that could be done of the internal logic of a scientific theory (Carnap, 1950).

Conant, like Polanyi, was a physical chemist who had lived through the quantum revolution: for him the transformation of conceptual schemes was an existential fact. But it was a fact that had implications for the nature of scientific knowledge, and this was what transfixed them. Both Conant and Polanyi were concerned with the specificity of scientific knowledge, and with how scientists thought, both in relation to their subject matter and other scientists. Polanyi in particular was fascinated with the actual psychology of scientific thinking, awareness, and the ways in which discovery happened.

This was very much in tension with both the philosophy and sociology of science of the time. As Stephen Cole (2004: 837–842; esp. 839–840) has shrewdly observed, Robert K. Merton's interest in science was with science as a whole, not with the specifics of this or that discovery. The philosophy of science had inherited a collection of strongly held taboos against 'psychologism', a bogeymen invented by philosophers such as Husserl, Schlick, and Frege (cf. Kusch, 1995), and so concerned itself with the logical structure of theories and the rational reconstruction of this structure, and of confirmation, rather than what was dismissed as 'the context of discovery'. One may think of this as a step in the de-naturalization of epistemology. But it was to get worse.

Positivism of the Vienna variety was created as a response to a certain kind of anti-relativistic neo-Kantianism, which it replaced with the idea that parts of the framework of physics, namely the mathematical structure, were relative (Howard, 1990: 369–373; Friedman, 1999: 71–86, esp. 81). The basic thought that positivism disposed of was the idea that there was a conceptual structure uniquely valid for and presupposed by any field of science. The innovation of logical positivism was to recognize that one could get the same 'scientific' results, the same predictions, with different 'presuppositions', which implied that these presuppositions were not properly part of science, or even part of the realm of the factually true. They were at best conventions or something like it: 'necessary' in the sense that you couldn't do the prediction without some machinery like this, but not uniquely necessary, because one could do the same predictions with different conventions.

This reasoning, however, inadvertently transformed the way one could think about the history of science. The old neo-Kantian view was that physics had a (unique) conceptual order that it presupposed and that could be reconstructed by philosophical analysis. The new view was that the history of science was the history of successive presuppositional schemes. The fact that these schemes could be further reconstructed in different ways to produce the same predictions was a curiosity beside the historical point: that different thinkers in fact operated with different presuppositions.

The return of Neo-Kantianism

The idea of shared presuppositions as real, operative, historical facts that explained differences in periods of science had a startling but problematic implication. If presuppositions were, as they were by definition, the end of a logical regress, they could not be founded on anything more basic. They could not be refuted by the facts, for they were presupposed by the description of the facts themselves. Taken seriously, this meant that there was no real 'outside' for a world-constituting scheme of shared presuppositions. If there was no outside, there was no outside standard of progress, either. And if the history of science was, at least in a significant part, a history of the succession of such schemes, it could not be a history of progress, much less truth. Indeed, the reality of which science was supposed to provide knowledge recedes into the mist of the unknowable and indescribable.

The history of philosophical neo-Kantianism ended in the 1920s with the 'ontological turn', the moment when it was recognized that this idea of presupposition had pushed philosophy into a relativistic dead end. The only way out was to somehow rethink the problem of the nature of the reality being constituted. The history of science studies repeated this. Conant and Polanyi had avoided abandoning the notion of progress, because Conant took a more or less instrumental view of conceptual schemes and saw scientific progress in terms of the increasing ratio of theoretical to empirical content; Polanyi saw science as a tradition, which grew in the stepwise manner of other traditions, with a goal that was rethought and respecified within the tradition, not a goal that served as a permanent yardstick. Kuhn's notion of paradigm, to Conant's dismay, inserted the element that reproduced the insoluble problem of relativism that had flummoxed the neo-Kantianism from whence the notion had derived: the idea that particular scientific 'paradigms' were based on ungroundable premises which supplied their own standards of success.

This element, together with the 'sociological' idea that premises were shared by a community of some kind, transformed, and was in an odd sense foundational for, subsequent science studies. 'Relativism' was said by Barnes and Bloor (1982: 21) to be 'required' for 'the scientific understanding of forms of knowledge'; it was made into a research program by Harry Collins and subjected to endless debate, leading to the infamous 'epistemological chicken' dispute, a lengthy obsession with regress issues in the form of the problem of reflexivity, and so forth. The positive side of all this was excitement: philosophers are congenitally averse to 'relativism', so this permitted a lengthy war of words that elevated 'social constructionism' into a major bogeyman denounced for its relativistic implications; these flowed from constructionism's incorporation of the aspects of Kuhnianism that carried the relativistic virus – specifically in the term 'social', which was taken to imply 'not determined by physical reality', and hence over-the-top 'irrational'.

It would take a much longer essay to reconstruct the ways in which this ballooning comedy of errors evolved. Its apotheosis was the *Social Text* affair, and the defense by

Andrew Ross (1996) of the acceptance of an absurd paper on 'postmodernist science'. The paper had virtually nothing to do with anything to be found in science studies as it was actually practiced, but it enabled a number of people to wrap themselves in the mantle of 'defender of science and reason'. For the philosophers in this discussion, 'reason' rather than science was the issue. For them, at stake was the whole project of naturalizing, and naturalizing epistemology and the discussion of science. They wanted 'reason' left in, acknowledged as necessary in the description of science, and acknowledged as a form of necessary normativity.

But this issue had some rather *recherché* elements. The sides had colluded in the introduction of the same problematic anti-naturalistic Kantian language. They had simply introduced opposed variants: the absolutistic language of 'reason' as a presupposition for thought and the neo-Kantian language of shared – and hence 'social' – ultimate and ungroundable presuppositions for particular forms of belief, including particular scientific views. The problem with these two inheritances were not so much their determined anti-naturalism, but the fact that there was nothing in the realm of fact that corresponded to the explainers that they invoked. There were neither presuppositions nor sharings in the natural or social world. Nor did it even make sense to describe the process in natural terms. Even Harry Collins – whose recent *Tacit & Explicit Knowledge* (2010) is a perfect example of the recycling, in the language of collective tacit knowledge, of these same clichés – acknowledges that it is a complete mystery as to how things like the supposedly 'shared' presuppositions that are needed to make this reasoning work get into people's heads. But it is not a mystery, so much as an unwarranted appeal to a mystical non-natural process (Turner, 2011). And it is evidence of our chronic inability to move beyond the toxic inheritance of neo-Kantianism.

So science studies found itself in a situation in which two mysticisms – the mysticism of overarching unnatural Kantian normative reason and the mysticism of unnatural neo-Kantian shared presuppositions – were making a good intellectual living by taking in each other's washing. But this was a phony war. Despite the ritual denunciation of science studies by philosophers and historians of science, the ways in which they explained particular developments in science came to largely converge with the way science studies people did. This was especially evident with the appearance of Pickering's *Constructing Quarks* (1984). For historians and philosophers of science, the way to make this into the best history of the subject was to razor out the pages in the introductory chapter about social construction.

Actor-network theory

Science in Action (Latour, 1987) was a self-conscious goodbye to all that. Latour explicitly rejected as bogus the conflict between the competing explanations of science. This was his response to the science wars: to be 'freed from all these debates about "rationality", "relativism", "culture"', and so on (p. 213). Consequently he called for a 'moratorium on cognitive explanations of science and technology' (p. 91) at the same time as he rejected social constructionism. Actor-network theory (ANT) was the radical substitute. The actors were not actors, however, but actants. Latour eliminated the knower, even the intending agent, by flattening agency to the point that everything had it, including physical objects, all of which were actants and potential network members.

So instead of naturalizing epistemology, ANT wound up de-epistemologizing the social, and in the end getting rid of the social as a distinctive category as well. At the same time, Latour (1988) hinted that once his kind of description had its day, there might be no need for these explanations. Indeed, he explicitly rejected the 'demand' for explanation in favor of description, though he redescribed 'description' to include what others might call explanation. Cognitive explanation was, however, excluded. 'If, by some extraordinary chance, there is something still unaccounted for, then, and only then, look for special cognitive abilities' (Latour, 1987: 246). This was bravado. The toxic neo-Kantian element that produced the trouble, and the de-naturalization, of science studies explanation was groundless presuppositions, or a priori truths. Could these be replaced by network language? Faced with this challenge of making sense of a priori truth, Latour tried to reduce the problem to one of the status of formalisms, but was compelled to concede that there were no accounts of the power of these a priori items in network language. He could merely gesture in the direction of how such an account should be done.

But the basic problem with network explanations is that they aren't explanations at all, cognitive or otherwise. At best they are descriptions. This was a lesson learned by Durkheim, when he proposed to explain things with concepts like population density, but was forced to acknowledge that what counted as density was what people took to be density. Similarly for networks. If a network does anything, it does so through the action of its elements. If the elements are intentional agents, it is their beliefs about the facts summarized by the description of the network that do the explaining: not the anthropologists' kinship map, for example, but actual beliefs about kin and their relations. A standard example is this: people report that it is easier to find babysitters in the suburbs. But the number of potential babysitters is lower than in the city. What differs is the information, trust, and so forth that permits typical parents to be comfortable with them. The epistemic, in short, creeps back in, and the network aspect fades.

The use of terms like 'actant' and the application of action terms, such as 'mobilization', gave the illusion that something more profound was happening with actor network explanations. It was not. But the language served as a means of justifying and guiding a huge number of research projects in which the networks were described and the actual explanations, the things that made people within the network believe and act, were smuggled in without much scrutiny. Once the parts of the network are animated, the animation does the explanatory work, and talk about networks drops out.

Philip Kitcher attempted to provide some explanatory 'go' for network thinking by arguing that one could give some normative and perhaps explanatory force to the Latourian system. He reasoned that the networks connected not just to actants, but to values, cognitive and social ends. The apparent fact that widely connected webs were scientific and technological winners was explained by the fact that they connected to valued ends (Kitcher, 2003). As for Latour, he has recently published *The Science of Passionate Interests: An Introduction to Gabriel Tarde's Economic Anthropology*, with Vincent Antonin Lépinay (2009). This text in a sense is an answer to the problem of what sort of causal force or reality underlies the networks and what they do, and also an admission that something is needed.

Knowledge, in short, or epistemic considerations, are inescapable. David Bloor had it right with his exchange with Larry Laudan 30 years ago, in which he asked, as someone trained as a psychologist, why science had to stop cold when it came to explaining true scientific belief (Bloor, 1981; Laudan, 1981). The battle over the limits of naturalism and the legitimacy of normativism as an element of explanation – of whether invoking a normative notion of reason or truth actually explains a scientific belief – was the right battle, but it was subverted and confused by the presence of unreal normative transcendental notions, such as shared presuppositions.

The hard work, from which these lengthy episodes have been a diversion, is to epistemologize the social, to understand the elements of belief and belief formation, which inevitably depend on our knowledge of others and of institutional routines, and to do so in a naturalistic way, avoiding explanatory short-cuts, such as the appeal to transcendental non-facts of the sort that Kuhn added to Conant and infest social constructionism. And this will force us to ask the politically uncomfortable questions we find so difficult, questions about when to believe experts, about whether and when the consensus formation processes of science can be relied on, and to face our prejudices with hard questions.

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