statistical mechanics concerns itself struck me as odd. But it does have a pedigree precedent: Boltzmann himself wrote that a 'wide perspective' opens up 'if we think of applying this science to the statistics of living beings, human society, sociology and so on, instead of only to mechanical bodies'.

In sum, I heartily recommend this book. It challenges the foundations of a field that sorely need challenging. I am confident that the book will take its place alongside Reichenbach's *Direction of Time* as one of the most stimulating on this topic.

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With Friends Like This, Who Needs Enemies?

By Steve Fuller

Alexander Bird, *Thomas Kuhn*. Princeton: Princeton University Press, 2000. Pp. xii + 308. US$16.95 PB.

Anyone interested in reading Bird's book will already know that I am no friend of Thomas Kuhn (see my *Thomas Kuhn: A Philosophical History for Our Times*, Chicago, 2000). But with friends like Bird, Kuhn has no need for people like me! Bird's is the sort of book that is published only once the subject is no longer around to object to the way his project has been dragooned into someone else's holy war. Here we find Bird customising Kuhn as the standard-bearer, albeit imperfect, for the currently fashionable naturalistic approach to epistemology and the philosophy of science. Bird does this by inventing a new field for Kuhn's work—'theoretical history' (p. viii)—that is sufficiently close to philosophy for Bird to draw from it as he pleases, yet sufficiently distant for Bird not to have to deal with all, or even most, of it. Too bad Kuhn himself thought he was always doing philosophy, albeit in a

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historicist vein not normally recognised as philosophy in the anglophone world.

More generally, analytic philosophers like Bird commonly employ a version of the *ad hominem* argument that draws attention to someone's intellectual weaknesses, only to excuse that person in a way that enables the arguer to do whatever he wants with the person's work. Thus, we are treated to this patronising account of Kuhn's intellectual practice:

> [Kuhn] rarely engaged in the stock-and-trade of modern philosophers, the careful and precise analysis of other philosophers' views, and when he did so the results were not encouraging (for example in his discussion of Kripke and Putnam on the causal theory of reference). This is not to say that Kuhn was a bad philosopher . . . we might expect the grand, synoptic view to be characteristic of an important revolutionary thinker while the analysis of individual arguments might be cast as philosophy's parallel to 'normal science.' Even so, for a philosopher whose main achievement in the eyes of many is to have undermined a whole philosophical tradition, it is perhaps surprising that he makes little direct reference to the claims of that tradition; even less does he give chapter and verse that such-and-such is what the logical positivists did indeed think. This fact I think is further evidence that *The Structure of Scientific Revolutions* is primarily something other than philosophy—of the 150 footnotes in the first edition of that book only 13 include references to philosophers, and almost all of these are to philosophers whose views are in tune with Kuhn's. (pp. ix–x)

There is much here that is annoying. 'Philosophy' here clearly means analytic philosophy, and even then the more significant philosophers do not conform to what Bird considers normatively desirable practice. Is Kripke or Putnam, for example, known for 'the careful and precise analysis of other philosophers' views'? And what would a look at their footnotes reveal? Indeed, Bird is better understood in this quote as justifying his own 'normal science' status in analytic philosophy than excusing Kuhn for not conforming to philosophical standards as such. It never crosses Bird's mind that perhaps Kuhn did not discuss the logical positivists because he had no particular interest in undermining them. Certainly, Rudolf Carnap, who commissioned Kuhn to produce *The Structure of Scientific Revolutions* as the final installment of the *International Encyclopedia of Unified Science*, saw Kuhn's work as complementing, not contradicting, his own. Of course, this point has yet to be registered

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in textbook philosophy of science, which may explain Bird's own failure to register it.

Bird's book succeeds best when it sticks to the exposition of Kuhn's views, though Bird betrays the all-too-familiar tendency to interweave his lecture notes for Philosophy 101 into his lecture notes for Philosophy 301. Thus, in the midst of competent Kuhn exegesis, we are given potted discussions of the various textbook '-isms', in terms of which Kuhn cannot be fitted comfortably, which of course turns out to reflect the 'fact' that Kuhn is not a 'proper philosopher'. The net result is that Bird's argument is focused on the bits of Kuhn that correspond to bits of the textbook '-isms'. Let me dub this strategy the bits-of-bits fallacy, on the basis of which Bird attempts to make both an overall judgement about Kuhn's project and the relative merits of various '-isms'. If anything, Bird's bits-of-bitsiness reinforces my suspicion that no sophisticated philosophical position can be fruitfully analysed in terms of (say) empiricism, relativism, realism, or naturalism—a point that looms large in what follows. At the end, I shall return to the deeper import of this fallacy, which raises questions about how philosophers attempt to extract 'philosophical content' from what they read.

Not surprisingly, Bird founders whenever he attempts to pose a deep philosophical puzzle about Kuhn's project. Because Kuhn is supposed to have overturned the positivists, and the naturalists have been the long-term beneficiaries of Kuhn's purported intellectual revolution, Bird continually puzzles at why Kuhn himself never embraced naturalism. To be sure, 'naturalism' has stood for many different positions in the last 150 years. Bird himself holds the fashionable Darwin-inflected version that 'selective advantage' is conferred on those who possess reliable belief-forming mechanisms (p. 249). As for Kuhn, while he helped popularise the idea that knowledge growth is akin to speciation in evolutionary biology, he also scrupulously refused to interpret the metaphor literally. Here Bird deserves some credit for realising that in this respect Kuhn remained much closer to his (alleged) positivist opponents than his (alleged) naturalist followers. Unfortunately, Bird treats it as a matter of Kuhn being more Moses than Joshua, unable to make it all the way to the Promised Land of Naturalism. Here, against my better judgement, I find myself on Kuhn's side, defending his reluctance to take what I (but clearly not Bird) regard as the recent reactionary turn that is Bird's version of naturalised epistemology.

As I discussed in *Thomas Kuhn*, one of Kuhn's few virtues as a philosopher of science was his insistence that historical and other empirical research mattered to his project only insofar as it illuminated 'science' as a normative ideal of organised inquiry, a.k.a. the search for knowledge.

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Like his positivist forebears, Kuhn regarded the search for knowledge as a rather unique human activity that required continual monitoring by a normatively recognised community. That Kuhn's later work on paradigm acquisition was partly inspired by child development studies of concept acquisition did not imply—contra the naturalist reading—that he saw science as simply an amplified version of how we naturally come to know the world. Rather, what interested Kuhn was how people could orient their behaviour around a norm in a constrained learning environment. At most the latest theories of brain science (which pop up intermittently in Bird's text) explain the raw potential from which a scientific intelligence may be actualised under the right constraints. The question that interested Kuhn here was whether anything general could be said about those constraints at a finer-grained level than his various accounts of 'paradigm'. I believe that Kuhn posed a good question in a bad way, since his clear bias toward cognitivist-cum-behaviourist solutions prevented him from taking the moral and political character of scientific institutions sufficiently seriously. However, Kuhn and I would be on the same side, against Bird, that one should not reduce the issue simply to the biological basis of our capacity for reliable beliefs.

Bird believes that Kuhn refused to take the naturalistic turn because he was skeptical about the prospect of evaluating theories by an external standard of validity. Thus, Kuhn continued the positivist practice of speaking of truth as relative to theoretical languages, albeit now presented as somewhat more socially embedded 'paradigms'. Bird's naturalism is supposed to reopen contact with an 'external world' by recognising that we would not be in a position to evaluate theories in the first place, had we not already developed reliable belief-forming processes, presumably as part of our evolutionary heritage. Bird regards his position as an improvement over Kuhn's mainly because it helps solve the problem of skepticism. But it is not clear that skepticism is the main epistemological problem that bothered either Kuhn or the positivists. In contrast, what most bothered all these philosophers of science is actually characteristic of Bird's own position, namely, a failure to distinguish between science and technology, or more precisely, science as a means of *understanding* and *controlling* the world. Admittedly, the italicised distinctions may sound old-fashioned in our era of 'technoscience', but they are worth rediscovering in order to interrogate Bird's intuitions about what counts as 'progress' in the philosophy of science.

Like most Kuhn commentators, Bird ignores the fact that Kuhn draws his examples from a rather restricted subset of the practices we normally call 'science', namely, the physical sciences from roughly 1620 to 1920. Kuhn explicitly excluded not only the social sciences, but also the biomedical

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sciences, engineering and indeed any disciplinary practice that has been driven primarily by instrumental considerations. In terms of the historical and empirical bases of their respective theories of science, the logical positivists and Popperians, though not identical, are surprisingly similar to Kuhn. Moreover, all of these types of philosophers were either silent (*à la* Kuhn and the Cold War positivists) or critical (*à la* Popper and Feyerabend) of the scientific practices of their day. These facts culminate in an embarassing puzzle for Bird's naturalism, namely, that these philosophers might not have counted the naturalist's favourite sciences—evolutionary biology and neurophysiology—as proper sciences. Kuhn *et al.* would be bothered, in a way Bird presumably is not, that the same reliable belief-forming processes are involved, whether one is splitting atoms to fathom the ultimate nature of matter or to build the ultimate weapon of destruction—or, for that matter, that the same reliable belief-forming processes have enabled most of the world's peoples to survive quite reasonably on cosmologies that more closely resemble Aristotle's than Newton's. In short, once the naturalistic turn is taken, knowers have no particular reason radically to alter their epistemic practices, if their current practices enable them to control their environment to their satisfaction. While this result may constitute 'progress' in a philosophical paradigm that takes skepticism to be the most important problem, its effect may be rather conservative and even backward from the standpoint of overall social progress, which seems to require that epistemic practices combine expansionist ambitions with self-checking mechanisms, which together keep those practices from falling into the naturalist's rut of adaptationism.

Let me end with a methodological remark about the reading practices of analytic philosophers. By every other standard but their own, analytic philosophers are poor readers. Even when they agree with the texts they discuss, they generally leave them less interesting than when they started. This is because the analyst's goal in reading is a so-called 'argument', which, once identified, invariably turns out to be an instance of some combination of familiar dialectical moves. The text is then reduced to this argument, and the philosopher proceeds from there. Everything else is lost. I say this because one might wonder how exactly does the analytic philosopher know that his reading has enabled him to successfully abstract the philosophical content of a text. In particular, Bird is confident that he can distinguish the content of Kuhn's thought from the social conditions of its production and reception. Consequently, while Bird can grant at least in principle that Fuller's is a reasonable sociology of knowledge account of Kuhn, he is quite sure that it does not affect the validity of Kuhn's arguments, which require the sort of philosophical scrutiny that he provides (p. 278).

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I understand Bird's confidence here as implicitly underwritten by a belief in the difference between the contexts of discovery and justification—which, perhaps surprisingly, I also happen to accept. What I do not accept, however, is that Bird's approach to reading enables him to distinguish these contexts correctly. Just because the distinction in contexts has a philosophical, and perhaps even logical, origin, it does not follow that training in philosophy or logic best equips one to draw the distinction in practice. Analytic philosophers like Bird, who believe otherwise, thus commit what I mischievously like to call the 'genetic fallacy' fallacy. To put the point bluntly but briefly, if one is truly interested in dis-embedding some ideas from a thinker's text, then one needs to be steeped in the history and sociology surrounding the text. Otherwise, one will only succeed in providing, as I believe Bird does, an account that arbitrarily abstracts from the text's context, carrying over objectionable features that are then represented as part of its content. The overall effect in this case is to make both Bird and Kuhn less open to critical scrutiny.

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Constructing Exactness

By Katherine Neal

Henk J. M. Bos, *Redefining Geometrical Exactness: Descartes' Transformation of the Early Modern Concept of Construction.* New York: Springer-Verlag, 2001. Pp. xvii + 470. US$99 HB.

The twenty-two years of labour devoted to the writing of *Redefining Geometrical Exactness* were well worthwhile in that Bos has produced the definitive work on construction and geometrical exactness in the period between 1588 and 1650. This is of interest because 'exactness'—so closely associated with such terms as 'acceptable',

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