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## KNOWING SCIENCE

Alexander Bird

Reviewed by Don Ross

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### *Knowing Science*

Alexander Bird

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In this transparently organized and argued book, Bird defends two main theses: that the aim of science is production of (scientific) knowledge, and that even moderate empiricism is an incorrect account of the epistemology of science. The two theses are directly logically related by his account of evidence. Evidence, he maintains, is whatever can be used as a sound inferential basis for knowledge; and, at least in contemporary science that relies on sophisticated instruments, automated analysis, and distributed processing across specialist authors, this basis seldom if ever includes reports of anyone's sense perceptions.

Bird's defence of the first core claim is based on an analysis of the function of knowledge that in turn draws on analogy with the claimed function of individual belief. I don't find this argument persuasive, because it depends on regarding belief as a natural psychological kind, whereas I think that Dennett

([1987]) is correct to argue that belief is a (culturally obligatory) social construct. Bird says that his argument can accommodate this view, because it still allows for specification of the functionality of belief on which the analogy depends (p. 78, note 13). I'm doubtful about this. On the Dennettian account, the functions that sustain the usefulness of belief ascriptions are cultural–evolutionary and therefore variable and open-ended with respect to epistemological ambitions of ascribers.

Despite the space and care that Bird devotes to his argument from analogy, I think that this is a side issue. His contention that science aims at knowledge is compellingly supported by his detailed accounts of representative cases from the history of science, and by his direct general reflections. A scientific enterprise that aimed at piling up justified beliefs would be too easy, and a scientific enterprise that confined itself to solving problems would fail to satisfy the persistent human interest in generality. 'Knowledge', regardless of what a specific philosophical analysis pronounces it to be, is the only natural label for what scientists generally seek: a cumulative, shared, secure record of the general structure of reality. Naturalists (of which I am one) are often uneasy about appealing to the concept of knowledge, especially for a philosophically portentous purpose such as identifying the general target of science. The basis of this uneasiness is the apparently transcendental character of knowledge on analyses according to which we must be able to verify its truth and objective justification. But Bird rejects the claim that for something to be known the knower must know that she knows it; and he furthermore is comfortable with regarding knowledge as unanalysable. These attitudes should assuage the naturalist's hesitation. Knowledge can obviously be described and ostended, and scientists achieve recurrent agreement about when and where they have it. This accounts, *contra* van Fraassen ([1980]; and elsewhere), for why scientists make assertions with modal force, and it furnishes a much less complicated way of being a realist than depending on analyses that rely on analytically distinguishing true from untrue beliefs.

Bird mainly associates scientific knowledge with its propositional content. He does not explicitly discuss the idea that much scientific knowledge is also embodied in its craft, a concept that does not feature in his book. ('Craft' does not simply mean 'practice'.) My sense, derived mainly from his examples (but see also p. 109, note 2), is that his account is open to this extension. I would defend the extension mainly by reference to how, as a practicing experimental economist, I understand what it is that we know in our lab, a very substantive aspect of which is how our measurements and estimations should be done, precisely conditional on our aim to make permanent and unified contributions to the stock of... what can only reasonably be called, as Bird says, 'knowledge'. I am doubtful that propositional knowledge can be analytically pried apart from the craft knowledge. Crucially, both go into the scientific record and both are carefully scrutinized by conscientious reviewers.

A very valuable part of Bird's book is his concise and compelling account of science as an institutional structure. Scientific knowledge, he argues by citing well-curated examples, does not reduce to, or even supervene on, individuals' knowledge. It is, at the end of the day, science that accumulates knowledge, not scientists. It seems to me that this is in fact a major part of what makes Bird's thesis about knowledge as the aim of science so persuasive. It makes little sense to say that science believes things (or, anticipating attention to Bird's other main claim, that science perceives things). This leads me to wonder whether we should really want to say non-metaphorically that individual scientists even have scientific knowledge. Rather, we might say that they use, cultivate, and (crucially!) protect the knowledge that science has. Bird's argument about the function of knowledge by analogy with a putative general function of belief suggests

that he would not want to endorse this possible radical dissociation of the two concepts. However, as I said above, the argument by analogy is a part of the book that did not carry me along.

I now consider Bird's other main topic, evidence. I found this part of the book a bit more contentious. First, he defends Williamson's ([1997]) thesis that evidence must itself be knowledge ( $E = K$ ). This supports a functional account of evidence as what can be input to inferences that yield (further) knowledge. So any kind of proposition (including, I would add, rigorous description of properly executed scientific craft) can in principle provide evidential content, and this is important for understanding accumulation of scientific knowledge. Thus no special class of propositions, particularly propositions about sense experiences, are specially privileged. This is part of Bird's argument against empiricism, the other, more aggressively critical, part being that propositions about sense experiences aren't typical parts of interesting evidence at all.

Bird of course recognizes that observation (as all scientists call it) is important in obtaining scientific knowledge. But observation, he argues, isn't perception. This is partly supported by a sound argument from the biological history of sense organs against regarding them as reliable tribunals (p. 144, note 13). He defines observation functionally, as whatever can supply evidence for knowledge-generating inferences. So, the content of observations must be known:  $E = K$  is extended by  $O = K$ . Empiricism is then held to be 'false' because, he maintains, empiricism mis-identifies observation with perception.

Is all empiricism guilty of this charge? I think that Bird should be worried that most scientists like to call themselves empiricists. Does anyone—even van Fraassen—still believe in the usefulness of a Viennese notion of 'sense-data'? What scientists typically mean when they say they're empiricists is that their inferences are driven by data. Now, Bird allows that data contribute to evidence. What he denies is that they're evidence about the objects or processes that are the subjects of the knowledge at which science aims—he says that what makes data count as data are contingent measurement practices, so they have no general, special, functional status, *qua* being data, with respect to knowledge.

I am doubtful about the wisdom of driving such a wedge between data and observation. In our lab, Bayes rules. For estimation and other inferences, we put all data into our priors, whether we intended to measure them or not (thus we can enjoy immunity from allegations of p-hacking and avoid the foolishness of pre-registering hypotheses and observational stopping points), and it seems most natural to describe the underlying epistemology here as follows: We treat data as our core evidence. In terms of semantic conventions, we teach our graduate students to use 'observation' to refer to the interesting aggregations of data that directly help to identify structural relations and parameters in well-identified models. This does align with Bird's functionalist account of observation. But I suggest that it undermines his amplified anti-empiricist rhetoric.

The reason we insist that our apprentices follow a strict house practice on use of 'observation' is because we want them to rigorously distinguish between observations and estimates. This is in turn because sound inference algorithms must multiply standard errors on the latter but not on the former. (There is, alas, no shortage of published papers that neglect this. 'They are not knowledge!', I now get to enjoy saying thanks to Bird.) Bird is right that these observations aren't 'sense registrations', or anything like those mythical constructs. But I can't see my way to stop describing this as 'how to be a good empiricist'.

By this I don't mean a philosophical empiricist in the sense of someone who quarrels with realists. But I think that philosophers of science should choose rhetoric that doesn't invite scientists to think that they're being accused of errant epistemology. The best scientists are empiricists and they are realists who aim to produce knowledge. Philosophers should be figuring out how to make that a consistent and coherent thing to say.

Bird's book is efficiently written, by which I mean that it packs in a wealth of themes without getting too long. I lack space to do justice to them all. There's a nice argument that scientists don't go from 'certain' data points to uncertain summaries of data (p. 154). This certainly describes practice accurately. We can tolerate errors in data records—we had better, because they usually have them!—as long as they are accidents and not too large a share of the record in question, and we typically have more confidence in the summary, as long as we're careful with the statistics, than in any individual data point. We get the best confidence of all when we can triangulate on sets of data by multiple measurement techniques—craft knowledge.

Bird argues that inference to the best explanation (IBE) can lead to rational preference between hypotheses, but that only IOE ('inference to the only explanation') can lead to knowledge. IBE is truth-conducive, Bird argues, because explanatory loveliness is correlated with truth. The mechanism for this correlation is held to be scientists' use of Kuhnian exemplars (so loveliness is discipline-relative or field-relative). The high bar on knowledge gains established by this principle will make it likely that, as he later says, some whole disciplines or fields don't produce knowledge. I fear that there really are a few such disciplines in which people earn livings.

However, if we're to be persuaded that knowledge is the aim of science, then we must be careful not to let our view of knowledge become so stringent that it won't bear the weight of practical scientific reality. Bird sometimes seems to insist on precisely parameterized (as opposed to structural) knowledge of non-abstract systems as the aim of science. Some non-wayward sciences, which must wrestle with very complex causal networks, establish only highly general structural knowledge and then most of their busy activity consists in extending capacities to apply this structural knowledge to wider suites of cases, with respect to each of which IBE is the best they can do. My field of experimental economics is an example. Economists know only a few deep things, but then generate lots of rationally preferable beliefs (that we can all agree to rationally prefer).

Where we can't achieve IOE and must fall back on IBE, Bird argues, the rationally preferred hypothesis,  $h$ , must be accorded a subjective probability of less than one. Then a Bayesian can't treat this  $h$  as evidence by Bird's standard. Bird defends a 'super-objectivist', as opposed to subjectivist, Bayesianism, according to which probabilities represent degrees of plausibility as judged by the scientific community (by reference to loveliness and exemplars). This construction feels, to me, weird. In Bayesian estimation we often include other estimates in our priors—with, as noted earlier, proper error coefficients on them. Since error terms multiply, this puts strong filters in place against basing inferences on weak evidence. What we don't include in our evidence are explicit causal interpretations of model structure (let alone explanatory loveliness)—each conclusion of each inference is causally interpreted *ex post*. That's arguably the empiricist spirit shining through—without any equation of observation with perception. (And: failure to keep causal interpretations local is what encourages scepticism about causation by some over-zealous empiricists. But lots of specific causal identifications are plausibly knowledge.)

Bird necessarily, for the sake of his argument about super-objectivist Bayesianism, denies that scientific beliefs are credences. In one sense this is right: they aren't individual scientists' credences. But I don't see why we can't hold that they're credences at the scale of the expert community. Bird's argument against beliefs as credences is based on avoidance of extreme bets. This argument focuses on credences of individuals. But it fails for individuals as soon as we allow for risk aversion—that is, risk-averse individuals avoid extreme bets even if their beliefs are credences. Do we know that expert communities (of scientists) aren't risk averse? I know of no rigorous investigation of this in the literature. And I don't think we should reject the idea on the basis of an *a priori* argument about what 'credence' has to mean.

Bird concludes by rejecting global meta-scientific theories. This discussion is very nice. No special epistemological arguments seriously threaten science in general. The only alternative to believing that science is the basis of most human knowledge is nihilistic scepticism. We can and should identify the structural knowledge cores produced by specific disciplines or fields by looking closely to see where arguments such as the pessimistic meta-induction might apply locally. What isn't threatened by this is the general success of science as knowledge producing. Then we get the intuitively right kinds of results: someone could argue (though I'm not doing so here) that there is no structure to speak of in social psychology, and social psychology knows nothing; there is an effulgence of structure in every part of (empirical) physics, and physics knows a lot; economics falls between these cases, knowing just a few things that are a very useful basis for rational assessment of cases.

On balance, then, Bird's case for (collective) knowledge as the aim of science is convincing. Better: it's empowering! His account of observation is largely persuasive, certainly with respect to his claim that it isn't sensory perception. He over-generalizes, and too hastily, about the role of data, and the force of his anti-empiricist rhetoric can be read as a bit of philosophical acting-out. I learned a lot from his book, but I'm still going to call myself an empiricist.

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