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ESSAY REVIEW

Preaching to the Choir? Robert Klee and the Latest Face of Scientific Realism

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Robert Klee, *Introduction to the Philosophy of Science: Cutting Nature at its Seams* (New York and Oxford: Oxford University Press, 1997), xi + 258 pp., ISBN 0-19-5106113, paperback.

Perhaps the most important point to make about Robert Klee's recent *Introduction to the Philosophy of Science: Cutting Nature at its Seams* is that we need many more books like it: it is an engaging, accessible and comprehensive introductory text in the philosophy of science which manages to avoid sailing over the head of the beginner philosophy student without talking down to the working professional. I suspect that many teachers of the philosophy of science will, along with their students, find something in this book to stimulate their own thinking about the subject. Despite its many strengths, however, the book's central strategy of argument is, as we shall see, compromised by a fundamental weakness.

One particularly impressive feature of the text is its effective use of immunology as a case study. Klee's first chapter presents the basics of immunological science, and it serves him well as a constant source of illustration throughout the text. As Klee notes, this pedagogical strategy avoids the traditional exclusive focus on the (sometimes idiosyncratic) features of physics. Far more importantly, however, it avoids the classic philosopher's mistake of testing accounts of science against a high-school textbook reconstruction of scientific activity, rather than the sophisticated, complex, richly detailed and messy business in which real science consists. Klee writes with the confident authority of a philosopher who knows immunology well and knows how to apply its realistic features, history and examples to philosophical argument.

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Also of distinctive merit are Chapters 2 and 3, in which Klee efficiently describes the positivist account of science and some of its most important difficulties. Klee avoids the standard introductory portrayal of logical positivism: a mere articulation of scientific common sense—what a bright undergraduate science student might come up with herself if she bothered to spend a long afternoon thinking about it—which turned out to be overly simple and therefore vulnerable to counterexamples. Instead, Klee does an impressive job of showing how logical positivism constituted an attempt to solve distinctively *philosophical* problems about science: the positivists' deployment of mathematical logic, for example, is motivated by the promise it seemed to hold for elucidating metaphysically troubling notions like causation, natural law, and physical necessity. Klee's helpful discussion of the vexed distinction between observational and theoretical terms might benefit, however, from a greater emphasis on the important antifoundationalist implications of replacing a principled distinction of this kind (like that of the positivists) with one that is a matter of degree, like Klee's own intriguing suggestion that the distinction is grounded in the 'distance' of events from our unaided sensory apparatus.

The pre-Socratic Milesian philosophers are also beneficiaries of Klee's talent for sympathetic history: Chapter 5 begins with an uncommonly thoughtful portrait of their important (but often ridiculed) nascent efforts to explain the world in terms of natural causes. The chapter continues with an interesting discussion of reductionism, the Unity of Science program, and supervenience. Here Klee seems to endorse Jaegwon Kim's view that supervenience (asymmetric property covariance) relations are logically independent of asymmetric determinative dependence relations, and are thus insufficient to ground any interesting version of the Unity of Science program (which requires, he suggests, asymmetric determinative dependence relations between the various branches of science). Klee's subsequent discussion, however, seems to illustrate simply that the existence of a supervenience relation between two families of properties is insufficient to determine the unique *kind* (for example, causal, mereological, semantic) of asymmetric determinative dependence relation holding between them. The much stronger claim that supervenience relations can obtain in the *absence* of any relations of determinative dependence would seem to be the one relevant to Kim's conclusion concerning the Unity of Science program.

The historical centerpiece of the text is Chapter 7. Here Klee provides a subtle, nuanced portrait of Thomas Kuhn which is nonetheless accessible to the undergraduate student of science or philosophy. Klee's Kuhn is caught between his realist intuitions about scientific activity and his deep knowledge of the history of science, and he is ultimately ambivalent and unsure about what that history implies for realism, truth, progress and theory change in science, or if these traditional categories are even the appropriate tools of analysis.

Another positive feature of the book is Klee's tendency to announce and defend his own opinions about these central issues. Eschewing the impersonal, omniscient

voice of 'the textbook' serves to remind his students that we must each sort through the mess ourselves and give reasoned arguments for our views. This is a deliberate pedagogical decision: in the Preface, Klee tells us that he has made no attempt to hide his sympathies for scientific realism from the reader and that he doubts any effective text could affect a false neutrality about such a crucial issue.

Unfortunately, Klee's partisanship concerning scientific realism turns out to be the book's fundamental weakness. The Introduction honestly advertises Klee's opinion that science tells us 'the actual way things really are' and assures us that the text will constitute an extended defense of this claim. But Chapter 1 proceeds to prejudge the issue, telling us that 'Many theories are composed of statements so well confirmed and confidently held that only an extremely neurotic person could doubt their truth' (p. 11), that a particular account of the causal mechanism for allergies is 'basically correct in fundamentals if a bit oversimplified' (p. 14), and that the fact that patients who have taken hyposensitization shots for many years have elevated levels of G-class antibodies in the absence of any other infectious state 'does go together with the other forms of successful intervention and manipulation to produce a high plausibility for the current theory of allergic disease' (p. 22). The chapter's conclusion notes the historical emergence of a structurally intelligible theory of allergic phenomena which fit into a broader immunological picture and asks:

> But how could that be, if our theory of allergic disease was in error about the basic causal mechanism at work in allergic phenomena. How could our intervention and manipulation succeed unless our explanatory scenarios were largely correct, and how could those same explanatory scenarios be largely correct unless the causal structures the theory postulates were largely correct? The answer is, neither could be correct unless the determinative structures postulated by the theory were basically right... there are always minor errors of minute detail—those are the constant source of challenge for practitioners—but the probability of global error, massive error, is so remote as to be negligible. (p. 25)

Why would we bother to read the rest of the 'extended defense of realism' presented in the text? Klee warns us that he is flying in the face of 'the intellectual fashion among trendy thinkers and cultural critics' who 'pooh-pooh science's claim to tell us "the ways things actually are"' and that we will consider their arguments later. But hasn't the issue already been decided? After all, who wants to be a 'trendy thinker' or an 'extremely neurotic person' who could doubt the (approximate) truth of our scientific theories? Not me, and not, I'll bet, the undergraduate reader of Klee's text.

Perhaps Klee's remarks are themselves intended to be a version of the abductive argument that holds the truth of our scientific beliefs to be the best explanation of their success; a version of this argument is indeed the support ultimately offered for scientific realism. If so, we should insist that they be presented *as* an argument, and not simply as the obvious and unavoidable dictates of every thinking person's common sense. Our first real glimpse of this abductive argument comes in Chapter 4, but is muddled in a crucial and unfortunate way.

Chapter 4 opens with a lucid presentation of the Quine–Duhem Thesis and intro-

duces an absolutely brilliant specific illustration from immunology—the ^{51}chromium release assay—of why scientific claims must face the tribunal of experience in collections, and of the problems this generates for naive falsificationism. The beauty of this example is that it uses the gritty and complex detail of scientific practice itself to undermine a naive element of scientific self-understanding. Whatever charge might be fairly leveled at Klee's argument here, it could *not* be that he is a mere philosopher who does not understand the real details of scientific work; knowing the fine-grained details is what fuels his attack on falsificationism.

Trouble arises when Klee sets out to describe and endorse Quine's own analysis of the implications of the Quine–Duhem Thesis. Quine did not mean, Klee assures us, to suggest that there are no rational grounds for choosing between alternative ways of modifying a set of beliefs to accommodate the observational evidence, but simply that such adjustment is a highly 'pragmatic' affair; that is to say, for Klee, that we adjust our theories

> so as to maximize fertility of the resulting theory in generating new predictions, to maximize consistency with as much of our former system of beliefs as possible, to maximize the overall simplicity of the resulting theory, and to maximize the theory's modesty so that its claims are as narrowly drawn as possible. As a pragmatist, Quine believes that such pragmatic features 'track the truth' in the sense that a maximally fertile, familiar, simple, and modest theory is, in virtue of possessing those traits, that much more likely to be a true theory. (pp. 65–66)

Two distinct views must be considered here. Traditional Peircian Pragmatism (as Klee later notes) holds that truths are simply the beliefs that emerge in the limit of inquiry: thus, fertility, simplicity and so on 'track the truth' simply because they are the virtues that our theories will maximize to the greatest extent allowed by the data in the limit. But Klee neither endorses this view nor ascribes it to Quine: instead he ascribes and endorses the view that fertility, familiarity, simplicity and modesty track truth in the ordinary, 'correspondence' sense of the term—a completely different account of the matter. Most importantly, the latter account requires serious argumentative support to be at all compelling: in particular, we will want to know why theoretical virtues like fertility, familiarity and simplicity are more likely to pick out the theories that correspond with reality from a set of alternatives which are all perfectly consistent with the observational evidence.

This muddle persists into the Chapter's concluding discussion, which promises to tell us 'why Quine thinks that those who believe that science truthfully represents an inquiry-independent reality have good cause to be encouraged even in the face of the Quine–Duhem Thesis' (p. 77). The section starts off describing the traditional Pragmatist position that the truth is what 'works' (that is, the set of beliefs consistent with experience which best maximizes pragmatic virtues). But this account offers no encouragement at all to those who believe that 'science truthfully represents an *inquiry-independent* reality'. Thus, Klee proceeds to thoroughly conflate the defense of this Pragmatist view with that of the realist alternative attributed to Quine:

> Why should it be the case that what works is what's true, that what works reveals what is real? Well, argues the pragmatist, what other explanation for a theory's working well over a long period of time could there be? What other explanation for why practitioners achieve their goals using the theory could there be? What else could being real consist in but that we can successfully work with it? (p. 79)

This passage entertains two completely different answers to the question it poses. 'What other explanation could there be...' is a version of the abductive argument for realism Klee will offer in Chapter 10, while 'What else could truth consist in...' is the genuinely Pragmatist insistence that the truth about the world is *constituted by* whatever it is maximally useful to believe in the limit of experience (and is therefore *not* inquiry-independent).

Getting Quine right is not the issue here. The point is, Klee tries to argue for realism by muddling together two completely distinct responses to the Quine–Duhem Thesis. They need to be disentangled, but much more importantly, the Pragmatist view requires articulation, explanation and consideration on its own merits, while Klee's Quinean-realist alternative—that pragmatic virtues like fertility, familiarity, simplicity and modesty track correspondence to an inquiry-independent reality—*requires some defense*. Without such a defense, Klee has given scientific realists no reason at all to be encouraged in the face of the Quine–Duhem Thesis.[1]

The oversight is critical, for Klee repeatedly appeals to the antirealist implications of an account of science as grounds for suspicion or outright dismissal. Philip Kitcher's Unificationist account of explanation is abandoned, for instance, because, in holding that facts about the world are partly determined by the nature of our inquiry, 'it comes perilously close to being a form of antirealism', a position which may apply to the 'soft' sciences but which 'seems, frankly, downright bizarre when applied to something like physics or chemistry' (p. 123). We are forced to move on and cling to Klee's assurances that realism will be defended in the final chapter.

The same litmus test is applied when Klee argues that David Bloor's Strong Programme cannot escape antirealism,[2] and that Bruno Latour and Steve Woolgar are guilty of such sins as 'antirealism by the case method with a vengeance' (p. 171) and 'nuclear strength antirealism' which they must ultimately 'fess up to' (p. 173). Let us be clear: social constructivists have sometimes offered outlandish claims supported by weak argumentation, many of these arguments have been influential, and Klee effectively demolishes the worst of them. But it is not enough to show that social constructivists are committed to antirealism after all. Some social constructivists hold their positions for serious reasons, having to do with

[1]Klee does later defend scientific realism, but never takes up this crucial task of showing that pragmatic virtues track correspondence truth.

[2]Incidentally, Klee is right that Bloor cannot escape what philosophers call antirealism or 'anything (in principle) goes' relativism, and for the reasons he suggests. Of course, Bloor resists these labels because he thinks that traditional philosophical conceptions of things like 'knowledge' and 'the real world' (and the classifications of positions that they imply) are indefensible, but (as he is aware) such redefinitions do not change the substantive features of his account.

worries about the coherence of the notion of an objective world independent of our particular ways of experiencing or conceiving of it, and those worries are not disarmed simply by pointing out their antirealist implications.

Klee's single-mindedness in combating 'the fashionable rage among trendy humanists and other intellectuals' (p. 124) sometimes leads him to miss the real point at issue. He criticizes the symmetry principle of the Strong Programme in the Sociology of Scientific Knowledge (SSK), whose point is simply that the truth of a belief never reaches out and makes us endorse it, so the adoption of any belief, true or false, must be explained by appeal to whatever was taken to be *evidence* for its truth (never simply that it is true).[3] This is simply sound methodology for the history and sociology of science. SSK goes awry, I think, in holding that the constraints on our beliefs provided by nature (and by epistemology or methodology, which are themselves taken to be up for social negotiation) are so weak, and so many options remain open to us in light of the experimental evidence, that most of the real work of choosing our beliefs is done by 'social and/or political factors'. With Klee, I think that this seriously underestimates the constraints on our beliefs provided by nature, and by non-negotiable, universal principles of good epistemology and methodology. But the proof must lie in the detailed study of particular scientific episodes: if sociologists of scientific knowledge can make a plausible empirical case for the weakness and negotiability of these empirical and methodological constraints, then Klee and I are wrong about what determines theory choice in science.[4] But this is the real issue worth our attention, not the commitment of social constructivists to antirealism or to the incoherence or inaccessibility of an inquiry-independent world.

Related troubles afflict Klee's critique of Steven Shapin and Simon Schaffer's account of the Boyle–Hobbes dispute. He allows that theory-choice was underdetermined by data in this instance, but he faults Shapin and Schaffer for failing to recognize that the mere existence of alternative theoretical explanations consistent with the data does not entail that these alternatives are all equally good ones (and thus, presumably, that the work of choosing among them need not be done by social and political factors). He refers us back to Quine's 'pragmatic' grounds for choosing among theories and insists that

> Arguments can be given for why theories that maximize simplicity, consistency with our established beliefs (conservatism), modesty, and fertility at generating novel predictions are more likely to be self-correcting toward the truth over the long run than theories that do not maximize those pragmatic virtues. (p. 178)

At this point, the reader would sure love to hear some of those arguments, for they appear to be the linchpin of Klee's case against social constructivism, as

[3]Klee seems (pp. 158–160) to confuse this with a different issue: whether the *success* of a belief should be explained by appeal to its truth. SSK denies this too, but not because of symmetry.

[4]I cannot resist concurring with Klee that, despite the best efforts of SSKers to find cases in which the constraints I note are as weak and subject to social negotiation as possible, the strength of the evidence thus far assembled is underwhelming.

well as just what he needs to establish a case for scientific realism in light of the underdetermination of theory by evidence. Alas, this is the last we hear in defense of the truth-tracking pragmatic virtues.

When Klee moves on to consider feminist critiques of science, he offers a more diverse array of criticisms, some quite convincing. He is particularly concerned to argue (and does so effectively) against feminists' claims to have special 'epistemological privileges'—to know important things, simply in virtue of being feminists or marginalized persons, that others don't. Even here, though, Klee seems to miss the most important philosophical issue: *if* our political and moral commitments do indeed play some role in choosing among alternative theoretical possibilities, then science *would be* oppressive if it took only the political and moral commitments and/or experiences of a privileged class into account. Like Klee, I think that feminists greatly exaggerate the role of such political and moral commitments in directing theory choice, but once again, Klee's devotion to resisting antirealism leads him to neglect what seems like the central issue.

Furthermore, the antirealist inquisition continues. Sandra Harding is convicted of 'a form of extreme antirealism' either unnoticed by her or 'cavalierly embraced in passing as though it did not have the evidentially implausible and politically Orwellian implications it does' (pp. 186–187). We have received repeated assurances of, but no case for, antirealism's evidential implausibility.

One wonders if Klee's discussion of Helen Longino is even fair. Longino clearly believes that political and moral considerations play some important role in our choosing from among equally evidentially adequate theories, and while she accepts the existence of a 'nature' independent of our ways of investigating it, she insists that the object of scientific inquiry is instead 'nature under some description', which is not inquiry-independent after all. Klee parlays these two commitments into an account on which we are free (indeed, encouraged) to make up whatever world we like, constrained only by our moral and political preferences. This surely exaggerates Longino's view: even in the passages Klee cites, she seems concerned to insist simply that there is room for our political and moral aims to play some role in choosing what theory we endorse, and thus to play some role in constituting the world as conceived by science ('nature under some description'). Interestingly, Lynn Hankinson Nelson's 'Feminist Empiricism' is, despite some serious criticism, praised as the most promising feminist alternative, purely on the strength of her commitment to a distinction between how things are and our theories about them and to the view that not all evidentially adequate theoretical alternatives are equally likely to be true (p. 204).

By the time we reach the heavily advertised final chapter, Klee's promissory notes have whetted the reader's appetite for some truly dispositive arguments for scientific realism.[5] What we find, however, is simply the abductive argument hinted

[5]Somewhat unfortunately, Klee uses Peircian Pragmatism to illustrate the antirealist position. This will surely be confusing in an introductory text celebrating Quine's brand of pragmatism as showing us how to blunt any antirealist implications of the Quine–Duhem Thesis.

at in Chapter 4: wouldn't the convergent success of our scientific theories, under so many independent tests, be a miraculous, cosmic coincidence if they weren't (at least approximately) true? In fact, Klee chooses a curious example to illustrate this argument—the convergent success of our immunological theory of AIDS: 'Now surely, argues the realist, it could not be a coincidence that the immune systems of AIDS patients behave experimentally, time and again, under so many different conditions, *as if* the immunological theory of AIDS was correct, even though it is completely off the mark'. (p. 213) Until very recently, however, our immunological theory of AIDS was the subject of extremely heated controversy precisely because so many of its empirical implications (particularly those concerning therapeutic interventions) had *failed* (see Epstein, 1996).

This is not to say that we do not get a good discussion of the abductive argument: Klee offers interesting criticisms of van Fraassen's reply, and a convincing case against Hacking's 'manipulability' version of the argument itself. He also makes worthwhile, although not unanswerable, points (along with some question-begging ones) criticizing arguments put forward for antirealism by Steven Fuller, Harry Collins, Bas van Fraassen, Larry Laudan, and Arthur Fine. But Klee does not ultimately take seriously enough the traditional problem for the abductive argument: the apparent historical record of successful, false theories. In response to this problem, he takes issue with a few of the many theories Larry Laudan characterizes as false-but-successful, suggesting that the humoral theory of medicine, vitalist biology, and spontaneous generation were either not as successful as Laudan thinks or were not successful in the right way. These criticisms may be well taken, but it is dangerous for Klee to try to make his case by attacking specific examples, for this defends realism only if we are ultimately convinced that there are no (or very few) historical examples of false-but-successful (in the relevant sense) theories.[6] Some of the usual suspects, like the caloric theory of heat and the phlogiston theory of combustion and calcination, seem to have enjoyed substantial amounts of the kind of genuine convergent success that Klee seems to think can only be explained by truth: one can easily imagine Priestley reasonably saying 'it could not be a coincidence that so many phenomena behave experimentally, time and time again, in so many different conditions, *as if* the phlogiston theory was correct, even though it is completely off the mark'.[7] In any case, the extensive use that Klee makes of consistency with realism as a litmus test for accounts of science would seem to require more support than holding out the bare possibility that all or almost all of Laudan's examples are spurious.

Curiously, despite the fervor with which Klee advocates realism throughout the

[6]Klee rightly notes that his holism precludes him from adopting Philip Kitcher's fascinating alternative strategy: arguing that those *parts* of past theories which were successful are in fact true (Kitcher, 1993).

[7]I have argued elsewhere (Stanford, 1995) that Georges Cuvier's creationist biology was also successful in the relevant way.

text, he ultimately concludes that the truth of the matter is largely unimportant. After suggesting that each side in the realism dispute is so well entrenched that arguments amount to 'preaching to the choir' (p. 218) and that 'we are left drawing our mutually inconsistent lines in the sand' (p. 231), Klee concludes his 'extended defense of realism' by asking,

> So, who is correct? Which side wins? I'm not sure that it matters nearly as much as the parties to the dispute might like to think it does, as long as it is not science itself that suffers harm in the course of the disputation. All sides to the realism/antirealism dispute admit that most quarters of science seem to progress quite nicely in blissful ignorance of the fact that there is a raging debate within philosophy of science about what science is, what it is doing, and where it is going. (p. 238)

I can hardly imagine a more disappointing conclusion to a course spent puzzling over the realism dispute and the intricacies of philosophical accounts of science than to be told that the truth of it all doesn't matter much, because it doesn't impact scientific practice anyway. Faced with such a ringing testament to the impotence and insignificance of philosophical disputation, the reader might reasonably wonder why she has just devoted a semester to the philosophy of science when she might just as well have stuck with her prephilosophical intuitions about what science is and what it is doing, especially since those (realist) intuitions turned out to be right all along. In the end, she learns, everyone just believes what they believed at the beginning, arguments are only convincing to the converted, and even the philosophical disputants don't think it matters who's right.

This pedagogical weakness is important, but such weaknesses are the exception to the rule in Klee's text. Despite his own pronouncements, Klee himself is not just preaching to the choir, and I would suggest that much of what he says deserves to be taken much more seriously than it has been by those who casually dismiss scientific realism as naive or reactionary ('trendy humanists', you know who you are). In the end, I think that there is much work left to do before any side can claim victory in the dispute over scientific realism, that this work is worth doing because it matters a great deal who is right, and that Klee's book makes some genuine contributions to the debate.

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