Erkenn (2011) 75:505–524
DOI 10.1007/s10670-011-9334-7

Epistemology Without History is Blind

Philip Kitcher

Received: 27 September 2011 / Accepted: 27 September 2011 / Published online: 14 October 2011

Abstract In the spirit of James and Dewey, I ask what one might want from a theory of knowledge. Much Anglophone epistemology is centered on questions that were once highly pertinent, but are no longer central to broader human and scientific concerns. The first sense in which epistemology without history is blind lies in the tendency of philosophers to ignore the history of philosophical problems. A second sense consists in the perennial attraction of approaches to knowledge that divorce knowing subjects from their societies and from the tradition of socially assembling a body of transmitted knowledge. When epistemology fails to use the history of inquiry as a laboratory in which methodological claims can be tested, there is a third way in which it becomes blind. Finally, lack of attention to the growth of knowledge in various domains leaves us with puzzles about the character of the knowledge we have. I illustrate this last theme by showing how reflections on the history of mathematics can expand our options for understanding mathematical knowledge.

1 Scrutinizing the Traditional Agenda of Epistemology

In a famous (possibly the most famous) passage in *Pragmatism*, William James declares his commitment to the "pragmatic principle of Peirce":

Many thanks to Uljana Feest and Thomas Sturm for conceiving the conference at which this essay was originally presented, and for inviting me to take part in it. I learned much from the reactions of the audience to an earlier (and more strident) version of this essay. I am particularly indebted to Wolfgang Carl, Hannah Ginsborg, and especially Barry Stroud (who deserves commendation for his extraordinary patience). Two anonymous referees for *Erkenntnis* offered further constructive suggestions.

P. Kitcher (✉)
Columbia University, New York, NY, USA
e-mail: psk16@columbia.edu

> There can *be* no difference anywhere that doesn't *make* a difference elsewhere – no difference in abstract truth that doesn't express itself in a difference in concrete fact and in conduct consequent upon that fact, imposed on somebody, somehow, somewhere and somewhen. (James 1907/1987, p. 508)

Many commentators, especially those least impressed with classical pragmatism, interpret this and kindred passages as intimations of a verificationist approach to linguistic meaning, concluding that James had a sloppy and inexact version of what they take to be a Very Bad Answer to the Central Question in Philosophy. The immediate context, however, makes it clear that any such interpretation would distort James' intentions. Issues about linguistic meaning hold no great interest for him. Instead his goal is to scrutinize the significance attached to the questions philosophers pose and attempt to answer. Philosophical disputes, he tells us, often collapse into triviality, once we consider what difference it might make, to anyone anywhere, however they were resolved. *Pragmatism* is a call to rethink the traditional philosophical agenda.[1]

James' even greater successor, John Dewey, articulates this revisionary approach to philosophy in more detail. Dewey starts from the suggestion that the task of philosophy is to help people make sense of their lives. In different contexts, this task is specified in distinct ways. The changing circumstances of human life make it appropriate to pose different philosophical questions, but a common error in the history of philosophy is to suppose that certain issues are timeless, that the "core problems" of some special philosophical discipline—metaphysics, epistemology, philosophy of language, say—are on the agenda for philosophical inquiry in each generation. Philosophical traditions fossilize, doggedly pursuing the questions they have inherited from earlier generations even when their inquiries would fail the test James proposed, that philosophy, like other inquiries, should make a difference to someone, somewhere and somewhen.[2]

Whether or not you are sympathetic to the pragmatist call for the renewal of philosophy,[3] it is surely healthy to ask, periodically, why a particular field of philosophy endures and why it poses the questions it does. In this spirit, I want to begin by asking why we might want *any* theory of knowledge, and *what* we might demand from any such theory if we had one. Human resources, including intellectual capital, are plainly limited, and yet there are infinitely many questions we might pose, infinitely many potential topics for theory. Why does knowledge deserve our attention?

The obvious answer to philistines who raise questions like this is that the concept of knowledge is important, and therefore one for which a theory ought to be constructed. Because there are so many ways in which concepts can be "important"

[1] For a sustained defense of this reading, see (Kitcher 2011a).

[2] Dewey articulates this perspective in three major works: Dewey (1920/1988), (1925/1981), and (1930/1988).

[3] My formulation indicates both agreement and disagreement with Richard Rorty. Rorty sees correctly that the classical pragmatists wanted to change the way philosophy is done, but he is more concerned with their judgments about old ways of proceeding than with their thoughts about the way in which the subject might continue. For further discussion of these themes, see (Kitcher 2011b, c).

and, consequently, so many different potential targets for theorizing, this is a flabby answer, but I shall let that pass for the moment. It does not follow from the fact that some attention might be paid to issues about knowledge that we require the kinds of minute specification at which so much philosophical effort has been directed during recent decades. Extremely talented people have spent hours (months, years) trying to say precisely when a person knows a proposition, and their efforts have spawned all sorts of derivative cottage industries: Can your knowledge be undermined by evidence you do not possess? Must you always be in a position to specify your justification? When you know must you have reasons for believing that you know?—and so on and on. Even granting the importance of the concept of knowledge, both the general project and the subsidiary ventures need further motivation. Concepts can be central to worthwhile inquiries, even though investigators do not attempt to provide complete definitions for them: the sciences use numerous central concepts (*gene*, *transcription* to cite two examples) that nobody knows how to define precisely, and, as history reveals, when definitions do come, they arrive relatively late in inquiry. Furthermore, conceptual clarification arises in response to confusions and unclarities that block progress; we do not require specifications that would enable us to classify outré possibilities. Finally, it is far from obvious that the crucial issues about knowledge that arise for us concern the conditions under which individual subjects succeed in knowing, rather than in understanding when a putative item of information has the status of deserving to be recognized as "something we now know", something to be "put on the books".

Contemporary philosophical journals contain many articles that decisively fail the Jamesian test. Erudite, ingenious, and sophisticated as they may be, they make no real difference to anyone beyond an inbred group of more-or-less-obsessed puzzle-solvers. Nor will it do to respond to this charge by claiming that today's epistemologists are the counterparts of scientists who investigate the minute properties of particular molecules in particular cells of particular organisms. Those scientists are indeed pursuing technical problems, problems whose significance is not evident to the outsider. Yet, in their case, the technical questions arise from larger inquiries, whose importance is obvious—questions about the course of embryonic development, the internal economy of cells, the causes of pathology, and so forth. Breaking those large questions into tractable parts enables the community of biologists to resolve the individual issues, to combine the solutions into larger pictures, and so to work collectively towards settling the straightforwardly significant issues. Philosophical inquiry fails on three counts: there is no connection to broad issues of clear significance, there is no cooperative construction of larger insights from smaller results, and no genuine resolutions at the allegedly technical level. The disputes, and the articles, proliferate until disenchanted lassitude terminates the enterprise.

Dewey's complaint about the fossilized agenda of earlier philosophy is pertinent to epistemology today. Large parts of the professional discipline pay no thought to the origins of the problems hailed as the focus of study: tradition is not scrutinized, even though what it has bequeathed lacks any obvious broader relevance. That is the first—and least interesting—sense in which epistemology without history is blind.

2 A New Agenda: Collective Human Knowledge as a Historical Process

Fortunately, other large parts of contemporary epistemology do much better. But instead of trying to list the virtuous ventures, I want to use the James-Dewey test to demarcate their place in the intellectual economy. Once again: what might we want from a theory of knowledge?

There's an obvious answer. A theory of knowledge should enable us to get more of it. That might be done through identifying methods for formulating new hypotheses, or through setting standards for acceptance, thereby assisting in the resolution of disputes when rival doctrines are at odds or leading us to remove from the books putative items of information that have been prematurely accepted. Since the mid-twentieth century, the majority view has been skeptical about possibilities for methods of discovery.[4] But identifying conditions under which propositions are appropriately accepted, or perhaps specifying the probabilities that should be assigned to them, has been viewed as a major task of epistemology. Without supposing that it is the whole of the subject, or even that the search for methods of discovery is impossibly ambitious, I'll focus in the next phase of the discussion on epistemology as the theory of confirmation.

Seeking clear criteria for when a proposition is supported by evidence, or for the extent to which it is supported by evidence, makes good sense in circumstances where there are genuine worries about premature acceptance. Imagine living in an age when it has very recently become apparent that the received wisdom that has dominated many areas of inquiry for two millennia is radically mistaken in quite fundamental respects. You might well draw the moral that this dismal record should never be repeated, that the course of inquiry *from now on* should be especially attentive to the credentials of the candidates for knowledge. You might even take a further step, seeking ways of identifying propositions for which there would be absolutely no danger of future revision, so that what was inscribed on the books could be written in indelible ink. Perhaps you might retreat, alone, into a stove-heated room, to ascertain which propositions can survive all possible doubt.

Enlightened fallibilists can easily smile at the erection of a conception of knowledge that requires absolute certainty. It is, however, valuable to understand that the demise of Aristotelianism in the seventeenth century made this conception extremely attractive, for it expressed the resolve that a 2000-year debacle should never occur again. Once this well-motivated conception was in place, some epistemological questions lurched into prominence, while others vanished from the scene. From the 1640s to the present, generations of thinkers have struggled to show that belief in the existence of an external world can be justified (or, sometimes, to argue that the belief cannot be justified), yet the issue arises only because regaining the external world was once an important step in a serious project. To establish the kind of systematic knowledge that would be a proper successor to the Aristotelian world-view, the philosopher-scientists of the seventeenth century needed to demonstrate, at a minimum, that the belief in external objects would accord with

[4] Although see the work of Clark Glymour and his associates on the generation of statistical hypotheses in the social sciences (Spirtes et al. 2001).

their high standards for knowledge. If we continue to wrestle with this form of skepticism today it should not be for the reasons that originally led Descartes to tackle the topic. The grounds will lie elsewhere—perhaps in the enduring difficulties of making sense of perceptual experience.[5] Some of the Cartesian questions may legitimately endure; others may have become no longer pertinent.

The more important point for present purposes concerns the disappearance, within the Cartesian framework, of some questions about knowledge that should be live for us. If you aim at knowledge that can be certified once and for all, then history and society cease to matter. Unless tradition and the deliverances of others can be independently vindicated—subjected to the test of possible doubt and shown to pass with flying colors—then they cannot count as knowledge. The inquirers of the past and the informants of the present are, at best, extensions of the individual knowing subject, who can and must calibrate them and independently check the information they supply. Knowledge is built up *hic et nunc* from the faculties of a single isolated individual. There are, then, no serious issues about the identification of legitimate authorities, or the conditions under which one can rely on testimony, or on the division of cognitive labor (Kitcher 1990). Social epistemology becomes a non-subject.

It is now a commonplace that the ambitious program of rebuilding a comprehensive system of knowledge to satisfy the demand for certainty was doomed—we are all fallibilists nowadays. Yet many contemporary epistemologists ignore, or only concede grudgingly, the social and historical dimensions of knowledge, continuing to be attracted by the vision of solitary knowers whose body of beliefs is justified (in some sense that no longer requires certainty) by a combination of reason and experience. Again, there can be legitimate ventures here, as when one tries to fashion a convincing picture of the preconditions for types of individual knowledge, investigating perception, memory, and so forth.[6] Yet an exclusive fascination with the individual embodies a neglect of history, a failure to see how the particular Cartesian vision emerged from a seventeenth-century project that was once well-motivated, but has now been abandoned.

Here again we encounter the fossilization of philosophical tradition of which Dewey complained. Dewey's predecessor, Peirce, saw the fundamental point in a sequence of essays that challenged the seventeenth-century perspective (Peirce 1992, Essays 2, 3, 7, 8). Once you appreciate the impossibility of allaying all possible doubt, there is no motivation to discard *everything* you have inherited from the inquiries of the past, for that will simply deprive you of the resources you need to *improve* your system of beliefs. Instead, your task is to identify the points at which genuine doubt arises, to scratch where it really itches, and to replace what is problematic with something better. The *static* picture of human knowledge, in which each of us has a body of belief that is, we hope, justified in terms of evidence available to the individual, gives way to a *dynamic* picture, one that sees us as dependent on one another and on those who have preceded us and that asks not for the justification of belief but for the justification of *change* of belief.

[5] Here I am indebted to the thoughtful essay by Barry Stroud, and to conversations with him.

[6] As rightly emphasized in the essays of Barry Stroud and Wolfgang Carl.

Once you adopt the dynamic picture, the agenda for epistemology changes. The methods to be sought for the extension of knowledge are no longer intended to inform us about the conditions under which an individual—in splendid isolation—is justified in believing some hypothesis on the basis of some presumptive class of "evidence statements", but rather how subjects, individually and collectively, are justified in modifying a heterogeneous corpus of statements they have inherited from their predecessors. Insofar as subsequent epistemology has taken this problem seriously, it has focused on finding abstract accounts of belief revision.[7] It is not, I think, entirely clear how much useful advice for the extension of human knowledge can be derived from formal attempts to model change of individual belief, but the more evident lack in contemporary epistemology concerns the social dimensions of knowledge. The actual practice of inquiry since the seventeenth century has fashioned methods of collective investigation that make profound differences to what we currently regard as established knowledge, and yet, despite the attention lavished on individual belief, there has been little serious scrutiny of the reliability of these methods. How much cognitive diversity is good for a community? How should the authority of different contributors to collective knowledge be appraised? These are basic questions whose correct answers might have large implications for human inquiry, and yet, because of the failure to think through the commitments of the dynamic picture, they have rarely been addressed.[8]

To sum up: the seventeenth century project for epistemology was motivated by a perfectly plausible diagnosis of the then contemporary situation; almost four centuries on, we have learned to settle for a less ambitious conception of knowledge; as Peirce clearly saw, our recognition should lead us to a dynamic picture, one that sees collective human knowledge as a historical process, and that, while preserving some classic epistemological questions, discards others and raises new ones. The failure to appreciate that is a second sense in which epistemology without history is blind.

3 History as the Methodologist's Laboratory

How, then, can philosophers (or anyone else, for that matter) identify good methods for changing belief? If we turn back the clock 50 years, two distinct answers, based on rival conceptions of the enterprise of philosophy, present themselves. One, more traditional, supposes that there are privileged ways of proceeding, available to thinkers in their desk-chairs, faculties of reason or logic or conceptual analysis that can deliver methodological counsel, provided only that the thinker works hard enough. The alternative, relatively new in 1960 and inspired by then-recent work in the history of the sciences, suggests that the history of inquiry can serve as a laboratory or a field-station for the exploration and testing of proposals about the advancement of knowledge. In the past 50 years, the excitement of that second

[7] Including, most notably, Bayesian approaches, as well as the belief-revision theories of Isaac Levi and Peter Gardenfors (Levi 1982; Gärdenfors 1988).

[8] For a pioneering exception, see (Goldman 1999).

option has faded, and epistemology has largely returned to the desk-chair and to the search for *a priori* principles. That, I maintain, is a third form of blindness.

One conception of the *a priori* is evidently a legacy of the seventeenth-century notion of knowledge. Although it turns out to be impossible to generate a systematic body of belief that will be immune to future refutation, there are, it is alleged, special types of belief—logic, mathematics, general principles about nature or about inquiry into nature—that can achieve this special status. Without recourse to experience, they can be justified, through processes of thought that are always available, that always generate truth, and that justify no matter what the subject's body of experiences turns out to be.[9] Knowledge of this sort will be *strongly a priori*. But there are excellent reasons to believe that none of our knowledge meets these demanding conditions. Aware, as we ought to be, of our own failures in reasoning and thinking, as well as in seeing and feeling, we are never in a position to override the deliverances of experiences that suggest either that we have made a mistake or that the concepts in terms of which our reasonings are framed are subtly flawed. If particular kinds of propositions appear evident to us, even so evident that we cannot see how they could be wrong, that is simply a result of the power of the tradition in which we stand to give them this appearance. Given the concepts we have acquired and the models for applying them that we have absorbed, we find ourselves with strong convictions, but that is an artefact of education and the evolving lore that stands behind it. For all we can determine, there might have been alternatives. The strong notion of the *a priori* yearns for an independence from tradition that is unattainable.

When the commitments of the strong notion of apriority are brought into clear view, many philosophers want to retain the label but opt for a weaker conception. *A priori* knowledge is then taken to be knowledge that the subject can justify on the basis of some process of thought that is available independently of his current experience—knowledge you can generate at your desk. There's no question that people (and not just philosophers) do generate beliefs in this way. Relying on things that their traditions and education have made available to them, things they're allowed to take for granted, they draw conclusions. Some of this, possibly quite a lot of it, counts as knowledge. The body of knowledge thus generated is, however, quite disparate, for there are many kinds of beliefs for which no itch of doubt arises. *Weak a priori* knowledge is obtained by using approved forms of inference to draw conclusions from approved premises.

Perhaps, then, epistemology could proceed, from the desk, by seeking principles for the improvement of belief as traditionally-sanctioned consequences of traditionally-sanctioned propositions? Possibly. Equally, natural science might operate in similar fashion, and perhaps, in the tradition of thought-experimentation, it has frequently done so. Yet, however plausible the thought-experiment may be, we expect the inquiry to go further, for the investigator to take a look, if it is possible to do so, and ascertain whether what has been apprehended at the desk is actually found in nature. By the same token, the aspiring epistemologist should do more than simply pronounce *ex cathedra*. Reflection on aspects of what tradition

[9] I scrutinize conceptions of this sort in (Kitcher 1980, 2006).

has allowed to be taken for granted inspires a hitherto-unrecognized idea about the revision of belief. Since it is conceded that the conclusion fails to be *a priori* in the strong sense, there is no guarantee that amending belief in the envisaged way will lead to improvement. So why not check? Has any such procedure actually been tried in the history of inquiry, and what effects has it had? Those are questions that are worth asking. Hence history re-emerges as the methodologist's laboratory.

As I have already admitted, the enterprise of turning to history as a test—or an inspiration—for methodological innovations has lost its appeal in the past decades. In part that is simply a consequence of the difficulty of probing historical episodes with enough precision and attention to detail to yield convincing implications. Laziness, however, is not the only motive for philosophical migration from the library and the archive back to the desk-chair. It is reinforced by the accusation, now almost a commonplace, that the kind of history aspiring epistemologists hoped to write is entirely misconceived. Imre Lakatos, Larry Laudan, and their successors (including me) have been told again and again that "philosophers' history" is no legitimate history at all.

There is, I think, something entirely comprehensible about this charge. The suggestion that a text will tell the story "as it ought to have happened", while the actual course of events is consigned to the footnotes, was bound to cause offense—if only because of the clear implication that industrious historians were unimaginative under-laborers.[10] It would have been unconscionably arrogant for philosophers to suggest (as, perhaps, some did) that the "rational reconstructions" at which they aimed were the only legitimate form of history. Yet any such assertion of privilege or uniqueness was evidently unnecessary. To make use of historical episodes as inspiration for or tests of putative methodological principles, all that was required was the presupposition that tracing lines of evidence and argument in important instances of scientific change could be *one* form of legitimate history.

It is worth reflecting on the many ways—and our existing paradigms will surely be extended in the future—in which history has been written. We are fortunate to have many different perspectives on fascinating events in the human past, studies that concentrate on various sorts of social, or cultural, or economic, or technological causes of change. Equally, there are histories that do not aspire to delve into causes, whose aim is to construct a picture of a society or local community at a particular time and place, to introduce us to a different way in which people have lived. *The Machiavellian Moment*, *The Body and Society*, *Montaillou*, *The Face of Battle*, and *The Great Devonian Controversy*, are five different historical studies, focusing on different aspects of human life, using different narrative and expository techniques, and answering different kinds of questions. It would be foolish to hail any of them as uniquely doing "real" history, and churlish to dismiss any of them as failing as "real" history. In each instance, because of the questions addressed, there is, inevitably, a selection of the facets of the situations and episodes considered: history is "written to a purpose". The selection is entirely justified because the omissions are irrelevant. Were someone to introduce details previously omitted from the

[10] The famous proposal about texts and footnotes comes from (Lakatos 1976).

account, that would not affect the conclusions that are drawn and the answers that are delivered.

Historical charges that philosophers who aim to reconstruct the methodological features of past scientific changes are not practicing "real" history rest on the thought that, in these epistemological studies, the criterion I have just formulated is violated. To put the point crudely, many historians suspect that, if the history paid more attention to biographical details about the actors, or to broad features of the societies in which those actors find themselves, or to specific social pressures that impinge upon them, the inferences reconstructed in the "case study" would appear rather different from the way in which they are actually presented. Philosophers *sanitize* past debates, sometimes by ignoring disagreements or the sources of disagreement, sometimes by failing to see how, in a particular context, there are social or cultural needs to be met that cannot be accommodated by the proposals for change that are singled out as justified. In principle, the same worry could arise for any form of history: selection always runs the risk of sanitization. Nevertheless, just as the historians who select can sometimes defend themselves, through showing that their work honors the fundamental criterion of invariance of conclusions under insertion of more detail, so too for philosophical histories. Perhaps there are extant examples that do satisfy the criterion. If there are not, the appropriate conclusion is not that "philosophers' history" is misguided, but that the enterprise requires more engagement with the many-sided episodes than philosophers have so far managed.

My third form of blindness, then, is embodied in the avoidance of the historical laboratory in favor of the desk-chair, and in the dismissal of the possibility of a genuine history that is epistemologically revealing.

4 Dealing with Skepticism About the Progress of Science

It is time to acknowledge the incompleteness of the conception of epistemology I have so far offered. No pragmatist should maintain that the only differences inquiries can make are matters of intervention. Some sciences rightly aim at clarification and explanation rather than at reshaping the world (think of attempts to sort out the hominid family tree). By the same token, epistemology has functions beyond those of delivering new methods for changing our beliefs. On occasion, what we want from a theory of knowledge is an improvement in our understanding, the clearing up of puzzles that arise for us. Among the challenges to our comprehension are the live forms of skepticism. Those crop up in a number of contexts—as I have already noted, one of them arises from the challenge to develop an adequate account (psychologically and epistemologically) of perception.

Here is another. The criterion for a successful "philosophers' history" of some major episode of scientific change demands that the addition of further details should not subvert the narrative that has been given. During the past decades, many scholars have believed that the criterion could only be met if another tacit demand, one favored by philosophers, were simultaneously violated. Taking the historical details seriously, it is suggested, would show that the methods deployed by the champions of the ultimately victorious view were no better—no more rational, no more able to confer justification, no more likely to yield truth—than those used by those who lost the debate. Properly understood, scientific controversies are always symmetrical, and the consequence is that our image of the growth of scientific knowledge needs radical revision. The skeptical conclusion holds that, with equal title to truth, justification, and knowledge, there could have been rival sequences of claims about nature that would have offered incompatible pictures of our world.

Whether or not he intended to promote it, this skeptical challenge is often traced to Kuhn's influential *Structure of Scientific Revolutions*, and taken to have been further strengthened by subsequent work in the history, sociology, and anthropology of science. Some contemporary scholars believe, on the basis of rather general arguments—sweeping appeals to underdetermination, for example—that the skeptical conclusion is warranted (or, presumably more exactly, as warranted as the more optimistic view it opposes). Equally, many scientists and philosophical allies, impressed with the success of contemporary sciences in intervening in nature, contend that the challenge is absurd, and that those who make it are guilty of bad faith ("Show me a relativist at 30,000 feet!" as the gibe goes). A more reasonable response would not rest satisfied with reliance on general features, supposed to support one or the other conclusion. Rather, it would undertake the historical work I have recommended, exploring whether a convincing account of the rationality of major transitions in the history of science can be given, in a way that satisfies the requirement not to omit perturbing details.

The frustrations of the recent "Science Wars" are the product largely of a failure to pursue this project with the rigor it demands. I have no wish to recall, or rehearse, those frustrations here, and shall simply be content with two observations. First, the skeptical challenge deserves to be taken seriously, for, even if the rationality of major scientific changes can be vindicated, it would be valuable to see clearly how it was achieved. Second, attention to the kinds of episodes to which skeptics often point—the Chemical Revolution or the Darwinian Revolution, say—would not only allay the genuine doubts historians and sociologists have raised, but would contribute to an enhanced appreciation of the methods through which scientific knowledge has grown.[11]

Serious historical epistemology is not needed merely to free us from the nagging of annoying skeptics—it can also play a powerful elucidatory role in domains of knowledge where our understanding of what the practitioners say and do is cloudy or incomplete. Even when no serious doubt arises about the truth or justification of the claims those practitioners make, it is often hard to explain just *how* those claims come to be true or *how* they are justified. Few people think that there are serious alternatives that might displace contemporary mathematics, and yet Bertrand Russell's quip is apt: this is a subject in which we don't know what we are talking about. Philosophical reconstructions of mathematics introduce strange entities and

[11] In (Kitcher 1993) Chapter 7, I have examined some facets of both these episodes in a way that achieves both goals. That is not to say, however, that the accounts I offer there would not benefit from extension and deepening.

mysterious processes, an abstract realm whose properties are fathomed by the community of mathematicians, faculties of intuition through whose use the axioms of mathematics are justified. It is hard to connect the entities and processes with what we know of ourselves and of the world in which we live, and yet it appears that they must be admitted. How else could we understand our mathematical knowledge? Thus ontologies and epistemologies of desperation are born.

This predicament, too, is an expression of the blindness of epistemology without history. Inspired by the post-seventeenth-century picture of the individual knower, the philosophical would-be reconstructors want to rebuild mathematics in the here-and-now, showing the "foundations" on which the entire discipline rests. They offer us implausible pictures, whose oddities must, it appears, be accepted, on pain of relinquishing the truth and justification of one of our best-established areas of knowledge. The failures of understanding, however, rest on the neglect of history. Epistemology without history proves blind in this instance, because it deprives us of any satisfying explanation for the knowledge we have.

5 Pragmatic Naturalism: Accounting for Mathematical Knowledge

I want to close by developing this theme in more detail, by illustrating a stance I shall call *Pragmatic Naturalism*. Naturalism generally is worried by the philosophical proclivity for introducing entities and processes that are difficult to integrate with the prevalent scientific picture of the world—ghostly selves, abstract mathematical objects, apprehensions in the light of pure reason, and the like. In some versions, naturalism supposes not merely that the entities and processes admitted be concordant with the findings of disciplined inquiry (which extend beyond the natural sciences to investigations of human life and culture), but that some particular part of natural science—neuroscience or evolutionary biology, say—suffices for answering all philosophical questions; *Pragmatic* Naturalism makes no such further commitment. Its naturalist emphasis is simply guided by the cautionary thought that there should not be more things dreamt of in philosophy than there are in heaven and earth (Goodman 1956/1983 34). In that vein, it supposes that philosophical positing must accord with methodological standards akin to those deployed in assessing innovations in other areas of inquiry: judged in this way, much contemporary philosophy looks like fantasy. Pragmatic Naturalism hopes to discover better solutions to the problems that spawn the fantasies. To this end, it suggests that areas of human practice can often be illuminated by considering the historical route through which they have emerged.

The illustration I offer concerns the example I used to show how failure to consider history can engender mysteries and unclarities. Mathematics is problematic because we don't know what it is *about*: mathematicians do not appear to be describing *physical* reality. Indeed, the world they conjure, with its indefinitely extending hierarchies of numbers, sets, and functions, seems far larger and less contingent than the physical universe. The central task of philosophical reflection on mathematics is to provide an account of what mathematics is about that will simultaneously explain the successful enterprise of mathematical knowledge.[12]

Many thinkers have responded by assimilating mathematical objectivity to the objectivity of the natural sciences, supposing that the task of mathematics is to describe a universe—not the concrete, physical universe, but a realm of timeless, necessary, abstract objects. Axioms and theorems of mathematics are true in virtue of referring to the constituents of this abstract universe and ascribing to them properties and relations that actually obtain: truth in mathematics, like truth elsewhere, is a kind of correspondence.[13] "$2 + 3 = 5$" is true in virtue of the existence of abstract objects, 2, 3, and 5, and because when the function of addition is applied to 2 and 3 the result is 5. This proposal is appealing because it gives a straightforward reading of mathematical statements that exhibits their objectivity. Its disadvantage, however, lies in the difficulty of understanding how the minds of mathematicians make contact with the alleged abstract realm. How, given this view of the content of mathematics, is mathematical knowledge possible?

"Yet surely everyone knows how mathematicians know the things they do! They prove theorems." So much is banal—but it cannot be the end of the story. Proofs have to begin somewhere, and, in research practice, they start with results already established, previously proved theorems. Ultimately, those proofs can be traced back to mathematical axioms—mathematical statements that are not themselves proved—and if the proofs are to generate knowledge, then the axioms too must be known. Here proof can no longer be invoked as a source of knowledge. How then are the axioms known?

Answers to this question exemplify one of two strategies. Most common is evasion. Many thinkers declare that the axioms of mathematics (of particular parts of mathematics like arithmetic, or of an envisaged complete system "mathematics-as-a-whole") are "evident", and leave it at that. Evidently, however, to say that the axioms are evident is to say no more than everyone already knew, that these axioms are known and that they do not need to be proved. The entire mystery was to explain *how* they are known. Nor should the request for explanation be dismissed with the charge that this is to indulge in "psychologism": for the question does not suppose that the *content* of mathematics is subjective or psychological (indeed, it begins from the assumption that the picture of mathematics as describing a realm of abstract objects is correct), but rather that there is some process through which basic truths about mathematical entities come to be apprehended by us, an assumption that seems hard to resist. If someone were to demonstrate extraordinary abilities to announce what is happening in distant regions of the globe, without having any discernible connection to the places at which the pertinent events were occurring, we would not be content to leave matters with the judgment that these things were

[12] A lucid twentieth-century formulation of the problem is provided by Paul Benacerraf in "Mathematical Truth" (Benacerraf 1973). Benacerraf is more specific than I have been, posing the question as that of combining an adequate theory of mathematical truth with an adequate account of mathematical knowledge. This leads him to pose the important, and seminal, dilemma, which I discuss in the text.

[13] Or, more exactly, truth is that kind of correspondence delineated by Tarski in his celebrated reconstruction of the concept of truth. Benacerraf (1973) presents this idea very clearly.

simply "evident" to the lucky person but would seek an explanation for the ability. So too in the mathematical case.[14]

A small minority of thinkers are courageous enough not to evade the question. Prominent among them is Kurt Gödel, who articulates the picture of mathematics as describing a realm of abstract entities, and then, in a famous passage, reflects on human access to that realm:

> But, despite their remoteness from sense experience, we do have something like a perception of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e. in mathematical intuition, than in sense perception, … (Gödel 1948/1983, pp. 483–4)

Yet although Gödel takes the question seriously, he, too, substitutes a label for any detailed explanation. If there is "something like a perception" of mathematical objects, how does it work? How do we do it well, or avoid doing it badly? What is the analogue, if any, of the transmission of light or sound, in visual or aural perception? At best, we have only the starting point for a satisfactory theory of mathematical knowledge.

I offer a harsher judgment: Gödel's proposal is an epistemology of desperation. Convinced of a picture of the objectivity of mathematics, he is led to introduce a type of psychological process for which there is no shred of evidence, and whose workings are utterly mysterious. Pragmatic Naturalism counsels caution here, for the brave strategy of actually addressing the question has led quickly to that inflation so notable in the history of philosophy, the introduction of nebulous entities and processes that resist integration with our current picture of the natural world. Yet Pragmatic Naturalism is not merely negative. Inspired by the recognition that human practices have long histories, it can offer a more satisfactory way of approaching the issues.

We are only led to evasion or epistemological desperation if we suppose that mathematical knowledge can be completely reconstructed for individuals, here and now, without reliance on historical tradition. The easy evasion that mathematical axioms are "evident" and the bolder proposal that they are accepted on the basis of perception are both radically undermined by the briefest immersion in the history of mathematics, for it then becomes clear that many of these axioms have emerged only with difficulty. If they are "evident" now, they were not always so, and sometimes our predecessors struggled with variants of them that we reject.[15] To understand how mathematicians (including Gödel) now know the principles they

[14] It is striking how frequently *labels* are adopted as a substitute for any theory of knowledge in the twentieth-century Anglo-Saxon tradition of discussing mathematics. Many eminent philosophers have been entirely satisfied to declare that basic truths of mathematics are "certain", "*a priori*", "evident", "analytic", "logical truths" and so forth, without feeling any need to say how they have this status or how they are known. Despite the forceful challenge in Benacerraf (1973), and despite the influence of that essay, the strategy of evasion continues happily into the present.

[15] See, for example, the history of basic claims about the existence of different types of numbers—zero, negative numbers, "imaginary" numbers, and so on. I discuss cases from the history of analysis—principles about continuity and infinite sums—in Chapter 10 of (Kitcher 1983).

take as basic, and even how those principles “force themselves upon them”, it is necessary to uncover the route through which those principles emerged, to show how their initial acceptance depended on their ability to extend or systematize a previously available body of mathematical practice, to show how subsequent practices of teaching mathematics made the principles appear constitutive of the concepts acquired by the aspiring mathematician.

It is also worth noting that any foundationalist account, of the sort philosophers typically propose, offers a curious vision of those who preceded the time at which the alleged foundations were exposed. The axioms of set theory may force themselves on Gödel and his successors, but they did not force themselves on Euclid or Archimedes, on Leibniz or Euler, on Gauss, Cauchy, Galois or Weierstrass. To condemn these forerunners to mathematical ignorance is surely harsh—and also an ungrateful return for their efforts in making post-Gödelian knowledge possible. Far better, I submit, to suppose that the epistemological order of mathematics broadly recapitulates the historical order.

This historically unfolding process of justification must have a beginning, and indeed it does. The start lies in the practices of ancient civilizations, those of Mesopotamia, Egypt, India and China, in which the language of arithmetic and geometry was developed for tallying, counting, measuring, and dividing physical objects. Instead of conceiving this language as picking out previously undetected abstract objects that (somehow) prove valuable in mundane efforts to collect and match bits and pieces of the ambient environment, we do far better to focus on the activities of collecting and matching themselves. The statements of arithmetic and geometry from which mathematics begins were justifiably accepted by our remote ancestors, because they could deploy those sentences with successful results in concrete transactions: the utility of “$2 + 3 = 5$” shows up in a complex sequence of collecting and matching actions; (take an object and another object; set them aside; now take a new object, another new object, and yet another new object; set them aside; pool all the objects you have set aside; recite the number words, pointing to a distinct object as you say each one; you will stop at ‘5’).[16] Reflection on the history not only helps with the understanding of mathematical knowledge but also guides the construction of a more adequate picture of mathematical objectivity.

As the mathematical languages developed in the ancient world were applied to the everyday transactions of business and the measurement of land, they generated new questions. Simple problems about how to tether animals so that they can (or cannot) reach particular places may well have been the ultimate sources of geometrical locus problems, just as difficulties in dividing inheritances according to particular rules of proportion gave rise to the practice of solving algebraic equations (or, more exactly, what post-Renaissance mathematicians would recognize as this

[16] Here I offer only the briefest sketch of a way of endowing elementary—historically primary—parts of mathematics with content, that is of relating them to our interactions with physical nature. In Chapter 6 of (Kitcher 1983), I tried to provide a systematic reconstruction of mathematics along these lines; but, although there are some points of kinship with the approach I adopt here, that systematic reconstruction was insufficiently attuned to the historical processes through which mathematics has evolved. My present views are elaborated in (Kitcher, ms.) where I develop the connection with the ideas of the later Wittgenstein (1953, §1), which is already implicit in the sketch offered in the text.

practice). Some of these questions were pursued for their practical significance, but a concentration on problems of a particular type can yield techniques that are applicable generally. Thus, in antiquity, mathematicians effectively discovered the formula for solving quadratic equations, that schoolchildren learn today, usually with much less effort.[17]

Pursuit of general mathematical questions—the solution of cubic or Diophantine equations, the determination of complicated geometrical loci—could easily appear the expression of idle curiosity. We do well to remember that the status of mathematicians in the Middle Ages and early Renaissance was relatively low, and that brilliant and original thinkers served as court entertainers.[18] Only when the new languages developed in the sixteenth century—with symbols for complex numbers, a general algebraic notation, and, later, terms for infinite sums and the operations of differentiation and integration—had shown their worth, both in resolving the general mathematical questions inherited from antiquity and in allowing for application in studies of the physical world (for example, in understanding problems about motion), was there a significant change in the mathematician's role. Mathematicians were given a license to extend their languages in ways that enabled them to solve problems of no obvious practical concern—matters that are "subtile and useless", in a famous phrase[19]—in the expectation that the languages so articulated could be deployed by others in physical inquiry.[20] Mathematics can be seen as an increasingly extensive collection of games, played by elaborating ever more complex languages, that address problems generated at earlier stages of mathematical practice, and that stem ultimately from the mundane questions tackled by the pioneers of the ancient world. Innovations are justified through their resolution of prior questions, and through the later development of new filiations between mathematical language and aspects of nature studied by the sciences

[17] Without algebraic notation, the procedure for solving quadratic equations is extremely hard to formulate, and the recognition of it a great intellectual achievement. Similarly, problems in multiplying large numbers are difficult to solve without a good notation (Roman numerals do not work well in this regard!). It is a mistake to dismiss particular historical developments (the contributions of the Arabic scholars who gave us a workable numerical system, for example) as "merely" matters of introducing new language.

[18] Niccoló Tartaglia and Girolamo Cardano both made their living through public displays of mathematical prowess. Hence Tartaglia's fury when Cardano published the method for solving cubic equations that he had elaborated (and had been independently found by Scipione del Ferro).

[19] The language stems from the Italian of Rafaello Bombelli, who recognized a purely mathematical point in introducing terms for square roots of negative numbers. That language only became firmly established significantly later, when Euler showed how mathematically valuable it was, specifically by forging a connection between the exponential and trigonometric functions. (Nagel 1935/1979 provides a valuable account of these developments).

[20] Interestingly, the great mathematicians of the seventeenth and eighteenth centuries typically worked both on the articulation of language to solve mathematical puzzles and on the application of those languages in the study of nature. Even as late as the nineteenth century, apparently "pure" mathematical researches are bound up with "applied" concerns—witness the work of Cauchy and Hamilton—as if the legitimacy of the proposed extensions must still be established by revealing pragmatic benefits. Gradually, however, confidence develops that at least some of the linguistic novelties will prove beneficial in broader inquiry, and pure mathematics obtains its full license.

(typically through the extension of practices of measurement).[21] Sometimes they are also prized for their aesthetic appeal (witness the "beautiful identity", $e^{i\pi} = -1$), or simply because the games are enjoyable to play.

Pragmatic Naturalism thus rejects the picture of mathematics as descriptive of some realm of abstract objects, to which human access is only possible through occult means. It recognizes that, throughout history, mathematicians have extended their languages, not through episodes in which they voyaged into new parts of the abstract universe, but through symbolic manipulations, related to and inspired by previously posed problems. Imaginary numbers, functions everywhere continuous but nowhere differentiable, Noetherian rings, and the like, were never discovered through taking an unprecedented sort of Gödelian peek. Viewing mathematics as a series of increasingly involved games dissolves the epistemological mysteries that the standard picture of mathematical objectivity brings in its train.

An approach of this sort is best motivated by thinking seriously about examples of mathematical discovery in which there is some record of how the discoverer(s) proceeded. For the late eighteenth century introduction of non-Euclidean geometry, or for Hamilton's efforts to develop the theory of quaternions, there is ample evidence of what happened. The mathematicians involved spent considerable time in symbolic manipulation—attempts to show that replacing the Euclidean axiom of parallels led to contradiction (in the one instance), repeatedly frustrated efforts to construct a multiplication table in the other. Processes of this sort, not mysterious ways of apprehending some ideal realm, are the stuff out of which mathematical transitions that introduce new "entities" are born.

Yet mathematicians routinely talk of their subject-matter as if they were describing a world of abstract entities, to which they have intuitive access, and it seems that Pragmatic Naturalism (or any sort of naturalism) ought to take that talk seriously.[22] Their *theorizing* about the processes guiding their innovations is typically vague, if not peculiar, but the possibility of an important psychological difference between the creative mathematician and the mathematically-informed (but pedestrian) outsider is worth taking seriously. Hardy was surprised when the seriously ill Ramanujan responded to his remark that his bus ticket had an uninteresting number—1729—by pointing out that it is the smallest number that can be written as the sum of two cubes in two different ways. Ramanujan would probably have declared that this fact was immediately obvious to him, and if asked to account for his "vision" of recondite truths about numbers, would probably have referred (as he so often did) to the visitations of the goddess who was the supposed source of his creativity. Without acquiescing in any such theory about the nature of his "intuition", it seems reasonable to suppose that Ramanujan had a skill, some splendid psychological capacity, that even a mathematician as creative and talented as Hardy lacked.

[21] The connections between mathematical language and nature become increasingly complex through the centuries, but one can see the beginnings in the simple practices of collecting, matching, and tallying, as well as in land measurement. Later modifications are often interwoven with the development of scales of measurement.

[22] Here I am indebted to a referee.

To see how Pragmatic Naturalism can respond to this point, it helps to begin by contrasting the example of Ramanujan with that of Hamilton. In his search for quaternions, Hamilton covered reams of paper with flawed attempts to construct a multiplication table—he succeeded only when he abandoned the requirement of commutativity. Ramanujan did not require any such experimentation on paper. *The difference is readily explicable in terms of the capacities with which their mathematical training had equipped them.* Even those of us whose mathematical abilities are at a far remove from the creative geniuses have some limited abilities to juggle symbols in our heads, *when we are working with the mathematical languages we have used to solve many problems*. Hamilton, struggling to find a three-, and then four-, dimensional analog to the complex numbers, was not able simply to draw on capacities that had been put to work on numerous previous occasions. Ramanujan, by contrast, became so adept at symbolic manipulation that he could process extremely complex calculations in his head. We can view his fine-tuned abilities as rooted in extant mathematical practice—and thus do without the visitations of the goddess, or even the abstract realm to which she is supposed to offer access.

It may reasonably be objected, however, that the sacrifice required by taking the history seriously is too high: what becomes of mathematical truth if one abandons the realm of abstract objects? The obvious answer declares that a mathematical statement is true if it can be derived according to the rules of a mathematical game, or, more exactly, of a mathematical game that is worth playing. So blunt an answer provokes an equally blunt objection.[23] This is to change the subject, it is no genuine account of *truth* at all.

There are, in fact, two different ways of conceiving truth. One approach is *structural*, focusing on characterizing truth, on showing how the truth of statements arises. The alternative is *functional*, seeking to understand what we *aim at* in our linguistic practices. With respect to a large class of statements, descriptive statements about the physical world, from the talk of common sense to the most refined claims of the sciences, a correspondence account of truth (more exactly a Tarskian theory of correspondence truth) can be viewed as delivering both structure and function, and that is why it is so compelling. For, given the Tarskian account of how the truth of statements arises, we can see why we aim at true descriptions: we want to see how these objects are and how they stand with respect to one another. But there are other language-games we play for which it's not so obvious that we should expect a structural account to shed light on the function of seeking truth. Mathematics is a case in point.

[23] There are other, more technical worries, for example the concern that identifying truth with derivability founders on Gödel's first Incompleteness Theorem. On my view, the sequence of worthwhile systems (the languages of mathematical interest) proceeds indefinitely. One of the directions in which it can extend consists in the addition to any formal system adequate for arithmetic of the pertinent Gödel sentence, to yield a new formal system for which the same extension can again be carried out. Once one has seen this, and understood why this is the preferred way of going on, these further articulations are of no particular further interest. We learn from Gödel that there will be no first-order theory adequate for the whole of mathematics. That lesson is perfectly compatible with the thesis I espouse, to wit that, for any mathematical truth, there is a worthwhile system within which that truth can be reached by licensed transitions.

Mathematicians, and commentators on mathematics, use ‘true’ to mark out the statements at which mathematics aims. On many occasions, what the mathematician is seeking is a licensed transition within a well-established system: he or she wants to produce a certain kind of statement by using the transitions that are allowed. There are other times—and they’re prominent at the major turning points in history, at which mathematicians look for modifications of those systems that will accord with broader methodological rules—when, to put it more colloquially, they are trying to find new games that are worth playing. If they are successful, then new language will be adopted and their successors will hail some sentences in these new languages as worth inscribing on the books, as legitimate starting points for further transitions, in short as “true”.

This is a functional use of “truth” one that takes “true” statements to be understood as those statements you’re trying to produce in a particular language-game—and it is applicable to a broader class of linguistic practices than those that center on description. The functional use need not be combined with the structural conception as it is in the Tarskian account of descriptive statements. In the case of mathematics, there is good reason to avoid the combination, for it is out of that easy connection that the idea of an abstract mathematical reality arises, with its consequent epistemological mysteries and the complete disconnection from the historical processes through which mathematics has actually been extended. We do better to treat mathematical truth from the functional perspective, and to declare that the language-games mathematicians play just aren’t in the description business.[24]

But what about “mixed statements”, statements containing both mathematical and physical (commonsense vocabulary)? Here, I appeal to an idea once influential in thinking about scientific theories. If you conceive mathematical language as initially uninterpreted, you can suppose it is *given* an interpretation in particular contexts: arithmetical language is deployed in everyday contexts by linking the arithmetical terms to operations of collecting, matching, combining, and tallying. I sketched this possibility above, and developed it more extensively in Chapter 6 of (Kitcher 1983). Similarly, geometrical language is given an interpretation by connecting its terms to operations of moving rods and chains around pieces of land. Yet this is only the beginning of an account, for it must be explained how the more complex parts of mathematics are linked to more refined procedures for interacting with bits of physical nature. The task is to reconstruct the history of measurement practices, from the most elementary cases of tallying and laying down rods to the applications of complex analysis in electrodynamics. Until the task has been completed, the approach to mathematics I have proposed will remain vulnerable to skepticism. Once again, however, historical study proves valuable in turning back challenges: Newton’s specifications of how his novel language bears on phenomena of motion and Fourier’s connections between his mathematical innovations and heat diffusion serve as exemplars of the extension of mathematical interpretation.

[24] This paragraph owes an obvious debt to Wittgenstein’s *Philosophical Investigations*, in particular to the opening sections. For more detail about the Wittgensteinian connections, see (Kitcher, ms.).

Pragmatic Naturalism views the objectivity of mathematics as grounded in the discharging of various functions by mathematical language. The truths of mathematics, we might say, are those that figure in linguistic practices that enduringly and stably achieve particular goals.[25] The goals in question are those to which I have already referred: first the mundane practices to which the earliest ventures in mathematics responded, secondarily the resolution of questions generated in facilitating those mundane practices, as well as the further uses of mathematical language in inquiries into the natural world.[26] In a subsidiary fashion, the goals of aesthetic satisfaction, and even of enjoyable play, may also figure in the story.

Pragmatic Naturalism, I suggest, offers a vision of mathematical truth and of mathematical objectivity, without invoking special mathematical objects. We may say, if we like, that "there are numbers such that …", but this is not to gesture towards mysterious entities, only to declare that an articulation of mathematics to meet its goals would continue to endorse the statement we assert. Pragmatic Naturalism might be equally useful in other areas where our understanding of what we are about is deficient. It might, for example, help us in the case of ethics [although that is another story, told in (Kitcher 2011d)]. For the moment, however, the promise of the pragmatic naturalistic approach to philosophical questions is a last example of my major theme: epistemology without history is blind.

References

Benacerraf, P. (1973). Mathematical truth. *Journal of Philosophy, 70*, 661–679.

Dewey, J. (1920/1988). Reconstruction in philosophy. In J. Dewey (1988) (Ed.), *The middle works* (Vol. 12). Carbondale: University of Southern Illinois.

Dewey, J. (1925/1981). *Experience and nature*. In J. Dewey (1981) (Ed.), *The later works* (Vol. 1). Carbondale: University of Southern Illinois.

Dewey, J. (1930/1988). *The quest for certainty*. In J. Dewey (1988) (Ed.), *The later works* (Vol. 4). Carbondale: University of Southern Illinois.

Gärdenfors, P. (1988). *Knowledge in flux*. Cambridge, MA: MIT Press.

Gödel, K. (1948/1983). What is Cantor's continuum problem? reprinted in P. Benacerraf & H. Putnam (Eds.) (1983), *Philosophy of mathematics: Selected readings* (2nd ed.). Cambridge: Cambridge University Press.

Goldman, A. (1999). *Knowledge in a social world*. New York: Oxford University Press.

Goodman, N. (1956/1983). *Fact, fiction, and forecast*. Cambridge MA: Harvard University Press.

James, W. (1907/1987). Pragmatism. In W. James (Ed.), *Writings 1902–1910*. New York: Library of America.

Kitcher, P. (1980). A *priori* knowledge. *Philosophical Review, 89*, 3–23.

Kitcher, P. (1983). *The nature of mathematical knowledge*. New York: Oxford University Press.

[25] Here, the conception of truth recapitulates ideas of classical pragmatism, specifically the proposals of Peirce and James.

[26] Since, as my discussion of natural science emphasized, the goals of inquiry evolve, so too the legitimacy of extensions of mathematical language will be partly shaped by contingent ideas about what scientific issues are worth pursuing. Even if mathematicians justifiably extend their languages in response to previously-posed mathematical questions, similar points about evolving interests apply. This means that there may be no ideally complete system that is the inevitable result of inquiry, even though any justifiable development of mathematics must contain certain mathematical languages at its core.

Kitcher, P. (1990). The division of cognitive labor. *Journal of Philosophy, 87*, 5–22.
Kitcher, P. (1993). *The advancement of science*. New York: Oxford University Press.
Kitcher, P. (2006). A priori. In P. Guyer (Ed.), *The Cambridge companion to Kant and modern philosophy* (pp. 28–60). Cambridge: Cambridge University Press.
Kitcher, P. (2011a). The road not taken. (to appear, in German, in a volume celebrating the centenary of James' *Pragmatism* edited by Martin Hartmann and Marcus Willaschek, Frankfurt a.M.: Suhrkamp).
Kitcher, P. (2011b). The importance of Dewey for philosophy (and much else besides). (to appear in J. Shook (Ed.), *Dewey's enduring impact*. Amherst NY: Prometheus Books).
Kitcher, P. (2011c). Philosophy inside out. *Metaphilosophy*, *42*(3), 248–260.
Kitcher, P. (2011d). *The ethical project*. Cambridge, MA: Harvard University Press.
Kitcher, P. (ms.). Mathematical *truth*? (draft presented at conference honoring the retirement of Paul Benacerraf).
Lakatos, I. (1976). *Proofs and refutations*. Cambridge: Cambridge University Press.
Levi, I. (1982). *The enterprise of knowledge*. Cambridge/MA: MIT Press.
Nagel, E. (1935/1979). Impossible numbers. (reprinted in *Teleology revisited* (pp. 166–194). New York: Columbia University Press, 1979).
Peirce, C. S. (1992). In N. Houser & C. Kloesel (Ed.), The essential Peirce (Vol. 1). Bloomington: Indiana University Press.
Spirtes, P., Glymour, C., & Scheines, R. (2001). *Causation, predication, and search*. Cambridge, MA: MIT Press.
Wittgenstein, L. (1953). *Philosophical investigations*. Oxford: Blackwell.