KNOWLEDGE AND ITS PLACE IN NATURE

Hilary Kornblith

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Investigating Knowledge Itself

Not so long ago, philosophy was widely understood to consist in an investigation of our concepts. There were books with titles such as *The Concept of Mind*;[1] *The Concept of a Person*;[2] *The Concept of Law*;[3] *The Concept of Evidence*;[4] and *The Concept of Knowledge*.[5] The idea that philosophy consists in, or, at a minimum, must begin with an understanding and investigation of our concepts is, I believe, both natural and very attractive. It is also, I believe, deeply mistaken. On my view, the subject matter of ethics is the right and the good, not our concepts of them. The subject matter of philosophy of mind is the mind itself, not our concept of it. And the subject matter of epistemology is knowledge itself, not our concept of knowledge. In this book, I attempt to explain what knowledge is.

My insistence that epistemology should not concern itself with our concept of knowledge requires that I depart, in important ways,

[1] Gilbert Ryle, *The Concept of Mind* (Barnes & Noble, 1949).

[2] A. J. Ayer, *The Concept of a Person and Other Essays* (Macmillan, 1964).

[3] H. L. A. Hart, *The Concept of Law* (Oxford University Press, 1961).

[4] Peter Achinstein, *The Concept of Evidence* (Oxford University Press, 1983).

[5] Panayot Butchvarov, *The Concept of Knowledge* (Northwestern University Press, 1970).

from some common practices. I will not, for the most part, be comparing my account of knowledge with my intuitions about various imaginary cases; I will not be considering whether we would be inclined to say that someone does or does not have knowledge in various circumstances. I do not believe that our intuitions, or our inclinations to say various things, should carry a great deal of weight in philosophical matters. But if we abandon these traditional philosophical tools, then how are we to proceed? How are we to go about investigating knowledge itself, rather than our concept of knowledge? Indeed, what could it even mean to suggest that there is such a thing as knowledge itself apart from our concept of it?

In this chapter, I focus on issues of method. Conceptual analysis, the use of imaginary examples and counterexamples, and appeals to intuition are the stock-in-trade of many philosophers. Indeed, George Bealer[6] has described the appeal to intuitions as part of 'the standard justificatory procedure' in philosophy, and, as a simple sociological matter, I believe that Bealer is right; appeals to intuition are standard procedure. More than this, Bealer detects the use of this standard procedure in philosophers who otherwise differ on a wide range of issues; even philosophers who favor a naturalistic epistemology, Bealer argues, make use of appeals to intuition.[7] This is a special problem for naturalists, as Bealer sees it, because naturalists are committed to an epistemology that makes no room for appeals to intuition. So much the worse, Bealer argues, for naturalism. The very practice of philosophy is incompatible with a naturalistic epistemology.

[6] See George Bealer, 'The Philosophical Limits of Scientific Essentialism', *Philosophical Perspectives*, 1 (1987), 289–365; 'The Incoherence of Empiricism', in S. Wagner and R. Warner (eds.), *Naturalism: A Critical Appraisal* (Notre Dame University Press, 1993), 163–96; and 'Intuition and the Autonomy of Philosophy', in M. DePaul and W. Ramsey (eds.), *Rethinking Intuition: The Psychology of Intuition and Its Role in Philosophical Inquiry* (Rowman & Littlefield, 1998), 201–39. Page numbers in parentheses refer to 'The Incoherence of Empiricism'.

[7] See also Frank Jackson, *From Metaphysics to Ethics: A Defense of Conceptual Analysis* (Oxford University Press, 1998), p. vii: 'And, as you might expect, if I am right about our need for it, conceptual analysis is very widely practiced—though not under the name of conceptual analysis. There is a lot of "closet" conceptual analysis going on.'

Now the kind of epistemology I favor, and the kind I will argue for here, is a form of naturalism, and Bealer's argument is thus directly relevant to the conduct of this enquiry. If Bealer is right, a naturalistic epistemology is self-undermining. Bealer is not the only one to have made this sort of argument. Similar arguments have been made by Laurence BonJour,[8] Frank Jackson,[9] Mark Kaplan,[10] and Harvey Siegel.[11] Naturalistic epistemology, on this view, proclaims allegiance to a theory that is fundamentally at odds with the philosophical practice of its adherents. A naturalistic epistemology is thereby shown to be untenable.

The clarity and force with which Bealer and others have presented this argument requires that it be given a fair hearing. A naturalistic epistemology has far greater resources, I will argue, than these philosophers have given it credit for. In the course of responding to this argument, I hope to explain how it is that philosophical theorizing may flourish while assigning a significantly smaller role to appeals to intuition than do the critics of naturalism. And in providing an account of philosophical theory construction from a naturalistic point of view, I hope to explain how it is that one may reasonably hope to give an account of knowledge itself, and not just the concept of knowledge.

1.1 *Appeals to intuition: The phenomenon*

First, let us get clear about the phenomenon. Although any characterization of the phenomenon will be highly contentious, there is no difficulty in giving examples of the practice at issue. We will thus do

[8] Laurence BonJour, 'Against Naturalistic Epistemology', *Midwest Studies in Philosophy*, 19 (1994), 283–300, and *In Defense of Pure Reason* (Cambridge University Press, 1998).

[9] Frank Jackson, *From Metaphysics to Ethics*.

[10] Mark Kaplan, 'Epistemology Denatured', *Midwest Studies in Philosophy*, 19 (1994), 350–65.

[11] Harvey Siegel, 'Empirical Psychology, Naturalized Epistemology and First Philosophy', *Philosophy of Science*, 51 (1984), 667–76.

best to pin down the practice by way of examples, examples of what we hereby dub 'appeals to intuition'; later we may address the question of what it is these examples are examples of.

There are substantial bodies of literature in philosophy that are driven in large part by frankly acknowledged appeals to intuition and are motivated by a desire to formulate accounts that square with those intuitions. Thus, in epistemology, there is the literature on the analyses of knowledge and justification, and especially would-be solutions to the Gettier problem. Imaginary cases are described, involving Brown and his travels in Spain; Nogot, Havit, and their vehicles; Tom Grabit and his kleptomaniacal proclivities at the library; gypsy lawyers; Norman the clairvoyant; barn façades in the countryside; and a host of others. In each of the cases described, there is a good deal of agreement about whether, under the described conditions, a subject knows, or is justified in believing, something to be the case. Intuitions about these cases are then used to clarify the conditions under which various epistemic notions rightly apply. No empirical investigation is called for, it seems. Each of us can just tell, immediately and without investigation of any kind, whether the case described involves knowledge, or justified belief, or neither.

But epistemologists are not the only ones to use this method. In philosophy of language, there is the literature on the Gricean account of meaning, replete with subjects and their self-referential intentions, including the American soldier who hopes to convince his Italian captors that he is German by uttering the one German sentence he knows, 'Kennst du das Land wo die Zitronen bluhen?'; not to mention a character who intends to clear a room with his rendition of 'Moon Over Miami', at least in part, of course, in virtue of his audience recognizing that very intention. Here we have quite clear intuitions about when it is that a subject means something by an utterance, and when a subject merely means to achieve a certain effect without meaning anything by the utterance at all. There is also the literature on the causal or historical theory of

reference, with the cases of Gödel, Schmidt, and the goings-on on Twin Earth.

There is the literature on personal identity, with its cases of brain transplantation, memory loss, and duplication. And there is the literature in moral philosophy involving children who amuse themselves by pouring gasoline on cats and igniting them; the woman who wakes up one morning to find herself an essential part of the life-support system for an ailing violinist; and a very large number of people unaccountably loitering on trolley tracks.

This method of appeal to intuitions about cases has been used in every area of philosophy, and it has often been used with subtlety and sophistication. There are those—and I count myself among them—who believe that there are substantial limitations to this method, and that some of these bodies of literature have diverted attention from more important issues. Even we, however, must acknowledge not only that the method of appeal to intuitions plays an important role in actual philosophical practice, but also that it has been used to achieve some substantial insights in a wide range of fields. We need an account of how it is that this method may achieve such results.

1.2 Bealer's account of intuition and the standard justificatory procedure

Now George Bealer offers us precisely such an account, and he uses this account to argue that naturalism[12] is self-defeating. In order to see how Bealer's argument proceeds, we must begin with his characterization of the phenomenon.

[12] Bealer has argued against a number of different targets, including naturalism, scientific essentialism, and empiricism. There are, of course, numerous versions of each of these views, and one may consistently subscribe to more than one of them. Indeed, the view I favor, and that I defend here, may rightly be described as falling under each of these three headings. It is for this reason that I feel called upon to answer Bealer's attacks on all these positions.

Bealer describes what he calls 'the standard justificatory procedure' (pp. 164–7). As Bealer notes, 'we standardly use various items—for example, experiences, observations, testimony—as *prima facie* evidence for things, such as beliefs and theories' (p. 164). After describing a typical Gettier example, Bealer notes that intuitions as well count as prima facie evidence. But what are intuitions? According to Bealer, 'When we speak of intuition, we mean *"a priori* intuition" ' (p. 165).

Although use of the term 'intuition' varies widely among philosophers, Bealer is careful to make his use of the term clear. 'Intuition', Bealer tells us, 'must . . . be distinguished from common sense . . . common sense is an amalgamation of various widely shared, more or less useful empirical beliefs, practical wisdom, *a priori* intuitions, and physical intuitions. Common sense certainly cannot be *identified* with *a priori* intuition' (p. 167). This distinction, Bealer tells us, is 'obvious once [it] is pointed out' (ibid.).

Once this account of the standard justificatory procedure is in place, with its reliance on intuition in Bealer's sense,[13] the route to an indictment of naturalism is clear. Naturalists subscribe to a principle of empiricism: 'A person's experiences and/or observations comprise the person's *prima facie* evidence' (p. 163). This rules out intuition as a legitimate source of evidence, and thus flies in the face of the standard justificatory procedure. Naturalists themselves make use of intuitions; they too subscribe, in practice, to the standard justificatory procedure. So naturalistic theory is belied by naturalistic practice. Indeed, if consistently followed in practice, Bealer argues, naturalistic theory would not only rule out philosophy generally as illegitimate, but, given the role intuition plays in 'following rules and procedures—for example, rules of inference' (p. 167), a

[13] I don't mean to suggest that Bealer's usage here is idiosyncratic; it isn't. Compare, for example, BonJour's notion of intuition: 'judgments and convictions that, though considered and reflective, are not arrived at via an explicit discursive process and thus are (hopefully) uncontaminated by theoretical or dialectical considerations' (*In Defense of Pure Reason*, 102).

consistent naturalist would have little room left for legitimate belief of any sort at all.

Bealer argues that some naturalists face an additional problem as well. Those who wish to make use of, rather than eliminate, epistemic terminology will find, Bealer argues, that their theory is at odds with their practice in yet another way. What Bealer calls 'the principle of naturalism' holds that 'the natural sciences . . . constitute the simplest comprehensive theory that explains all, or most, of a person's experiences and/or observations' (p. 163). Naturalists also endorse what Bealer calls 'the principle of holism': 'A theory is justified . . . for a person if and only if it is, or belongs to, the simplest comprehensive theory that explains all, or most, of the person's prima facie evidence' (ibid.). When these two principles are added to the principle of empiricism, which limits our source of prima-facie evidence to observation, naturalists are forced to eschew all epistemic terminology, because 'the familiar terms "justified," "simplest," "theory," "explain," and "*prima facie* evidence" . . . do not belong to the primitive vocabulary of the simplest regimented formulation of the natural sciences' (p. 180). Not only is naturalistic theory at odds with naturalistic practice, but the very terms in which naturalistic theory is formulated, Bealer argues, are disallowed as illegitimate by that very theory. Naturalism is thus found to be self-defeating twice over.

There is more to naturalism, I believe, than is to be found in Bealer's account of it. There is room within a naturalistic epistemology for the practice of appeals to intuition, suitably understood, and also for the use of epistemic terminology. What I wish to do is explain how the naturalist may accommodate these phenomena. Much of what I say will be familiar; the story I have to tell, I believe, is at least implicit in the work of a number of investigators working within the naturalistic tradition.[14] But in squarely addressing these

[14] For example, Michael Devitt remarks, in a footnote to his 'The Methodology of Naturalistic Semantics,' *Journal of Philosophy*, 91 (1994), 545–72: 'The naturalist does not deny "armchair" intuitions a role in philosophy but denies that their role has to be seen

charges against naturalistic epistemology, we may not only put them to rest, but we may also lay the foundation for a deeper understanding of proper method in philosophical theorizing.

1.3 *A naturalistic account of appeals to intuition*

Naturalists and their opponents have divergent views about how philosophy ought to be practiced. At the same time, however, there is a great deal more agreement in actual practice than there is in theory about that practice. I do not believe that these differences are insignificant, and later in this chapter I will want to say something about what those differences are and why they matter. But for now, I want to focus on the areas of agreement in practice between naturalists and anti-naturalists, and I will assume that the characterization Bealer gives of the standard justificatory procedure accurately characterizes that common practice. That is, I will assume, with Bealer, that philosophers of all sorts assign prima-facie weight to experience, observation, testimony, *and intuition*, although I will not assume, with Bealer, that intuition here comes down to 'a priori intuition'. Instead, I will take intuition to be pinned down by the paradigmatic examples of it given above in sect. 1.1.

How should naturalists regard the standard justificatory procedure? The first thing to say about the intuitions to which philosophers appeal is that they are not idiosyncratic; they are widely shared, and—to a first approximation—must be so, if they are to do any philosophical work. Some philosophers will say, 'I'm just trying

as a priori: the intuitions reflect an empirically based expertise at identification' (564 n. 27). Devitt refers there to Bealer's work. The present chapter may be seen as an attempt to expand on this remark of Devitt's. After completing a draft of this chapter, my attention was drawn to the work of Terry Horgan which has many points of contact with the views expressed here. See Horgan's 'The Austere Ideology of Folk Psychology', *Mind and Language*, 8 (1993), 282–97, and Terence Horgan and George Graham, 'Southern Fundamentalism and the End of Philosophy', *Philosophical Issues*, 5 (1994), 219–47. Finally, see also Richard Boyd's 'How to Be a Moral Realist', in Geoffrey Sayre-McCord (ed.), *Essays on Moral Realism*, esp. pp. 192–3.

to figure out what *I* should believe; I'm just trying to get my own intuitions into reflective equilibrium.' But even philosophers who say this sort of thing must recognize that wholly idiosyncratic intuitions should play no role even in figuring out what they themselves ought to believe. If I attempt to offer a philosophical account of knowledge by drawing on my intuitions, and it should turn out that crucial intuitions upon which my account relies are had by no one but me, then this will not only dramatically reduce the interest of my account for others; it ought, as well, reduce the interest of my account for me. If my intuitions are wildly idiosyncratic, then most likely the project of accommodating them is no longer one that is engaged with the phenomenon others are attempting to characterize. Unless I can show that others have been somehow misled, what I ought to conclude is that I am probably the one who has been misled, and I ought to focus my attention on correcting my own errors, rather than taking my intuitive judgments at face value. The intuitions of the majority are not definitive, but they do carry substantial epistemic weight, at least in comparison with the intuitions of any single individual, even oneself.

Why is it that the intuitions of the majority carry such weight? It is not, of course, that we merely wish to be engaged in the project, whatever it may be, that other philosophers are engaged in. This would make philosophy into a shallow enterprise, a kind of intellectual imitation game in which the participants seek to engage one another in what they are doing, without any regard for what that might be. Instead, we must be assuming that disagreement with the majority is some evidence of error, and now the question is how that error should be characterized.

Now it is at this point that many philosophers will be tempted to bring in talk of concepts and conceptual analysis: in appealing to our intuitions, it will be said, we come to understand the boundaries of our shared concepts. But I don't think this way of seeing things is illuminating. By bringing in talk of concepts at this point in an epistemological investigation, we only succeed in changing the subject:

instead of talking about knowledge, we end up talking about our concept of knowledge.

As I see it, epistemologists should be trying to understand what knowledge is. There is a robust phenomenon of human knowledge, and a presupposition of the field of epistemology is that cases of knowledge have a good deal of theoretical unity to them; they are not merely some gerrymandered kind, united by nothing more than our willingness to regard them as a kind. More than this, if epistemology is to be as worthy of our attention as most epistemologists believe, and if knowledge is to be as worthy of our pursuit, then certain deflationary accounts of knowledge had better turn out to be mistaken. What I have in mind here is those social constructivist accounts which, while granting a substantial theoretical unity to cases of knowledge, see that unity as residing in the social role that knowledge plays. Knowledge, on this kind of view, is merely a vehicle of power. Knowledge may well play some such social role, but its ability to play such a role, if I am right, is explained by a deeper fact, and it is this deeper fact about knowledge that gives it its theoretical unity.[15]

Now one of the jobs of epistemology, as I see it, is to come to an understanding of this natural phenomenon, human knowledge. Understanding what knowledge is, if the project turns out as I expect it will, will also, simultaneously, help to explain why knowledge is worthy of pursuit. When we appeal to our intuitions about knowledge, we make salient certain instances of the phenomenon that need to be accounted for, and that these are genuine instances of knowledge is simply obvious, at least if our examples are well chosen. What we are doing, as I see it, is much like the rock collector who gathers samples of some interesting kind of stone for the purpose of figuring out what it is that the samples have in common.

[15] I have discussed this point at greater length in 'A Conservative Approach to Social Epistemology', in Fred Schmitt (ed.), *Socializing Epistemology* (Rowman & Littlefield, 1994), 93–110, and in 'Naturalistic Epistemology and Its Critics', *Philosophical Topics*, 23 (1995), 237–55.

We begin, often enough, with obvious cases, even if we do not yet understand what it is that provides the theoretical unity to the kind we wish to examine. Understanding what that theoretical unity is is the object of our study, and it is to be found by careful examination of the phenomenon, that is, something outside of us, not our concept of the phenomenon, something inside of us. In short, I see the investigation of knowledge, and philosophical investigation generally, on the model of investigations of natural kinds.

This point is quite important, for what it means is that a good deal of the work involved in defining the subject matter under investigation is actually done by the world itself rather than the investigator. The subject matter of the rock collector's investigation is the natural kind, whatever it may be, which (most of) the samples picked out are members of; but the investigator need not be in a position to characterize the essential features of that kind. The investigator's concept of that kind, therefore, because it may be quite incomplete or inaccurate, need not itself do very much of the work of defining the subject matter under study.

This is contrary, of course, to what defenders of conceptual analysis claim. Frank Jackson, for example, nicely lays out the traditional view about the importance of concepts in defining subject matter.

> The role of intuitions about possible cases so distinctive of conceptual analysis is precisely to make explicit our implicit folk theory and, in particular, to make explicit which properties are really central to [the subject matter under study]. For surely it *is* possible to change the subject, and how else could one do it other than by abandoning what is most central to defining one's subject? Would a better way of changing the subject be to abandon what is *less* central?[16]

But surely a central point in favor of the causal or historical theory of reference is the observation that reference may remain stable even in the face of substantial changes in belief. It is not that subject matter is changed, as Jackson rhetorically suggests, by changing less

[16] Jackson, *From Metaphysics to Ethics*, 38.

central rather than more central defining features, for what is central or peripheral to our concept plays little role in defining subject matter in the first place. Rather, subject matter is defined by way of connections with real kinds in the world, and what we regard as central or defining features does not determine the reference of our terms.

When philosophical investigation is viewed on the model of the investigation of natural kinds, the method of appeal to intuitions is, I believe, easily accommodated within a naturalistic framework. The examples that prompt our intuitions are merely obvious cases of the phenomenon under study. That they are obvious, and thus uncontroversial, is shown by the wide agreement that these examples command. This may give the resulting judgments the appearance of a priority, especially in light of the hypothetical manner in which the examples are typically presented. But on the account I favor, these judgments are no more a priori than the rock collector's judgment that if he were to find a rock meeting certain conditions, it would (or would not) count as a sample of a given kind.[17] All such judgments, however obvious, are a posteriori, and we may view the appeal to intuition in philosophical cases in a similar manner.[18]

[17] What of the claim that something is a natural kind? Is that too known empirically? I believe that it is. Consider, again, the case of gold. What caught our attention and resulted in the introduction of the term, let us suppose, was a collection of salient features such as the color, reflectance pattern, malleability, and so on of a number of samples of rock. It was an empirical discovery that these constituted a natural kind, i.e. that these samples were not, from a theoretical perspective, merely heterogeneous, but rather that they shared some deep, underlying properties responsible for the more superficial properties that initially attracted our attention. The same is true of natural-kind terms generally. While a person introducing a term may well believe, at the time the term is introduced, that the referent of the term is a natural kind, subsequent investigation may reveal that this belief is false. By the same token, a term may be introduced for a property that is, in fact, a natural kind, without the person introducing the term recognizing that fact.

[18] Here I simply take for granted a causal or historical account of the reference of natural-kind terms. While the details of such a theory remain to be established, the general outline is, I believe, perfectly clear in the foundational work of Kripke and Putnam. The following points should be agreed upon by all sides: (1) The initial

What should we say about the rock collector's judgments at early stages of investigation, i.e. prior to any deep theoretical understanding of the features that make his samples samples of a given kind? Such judgments are, of course, corrigible, and they will change with the progress of theory. What seemed to be a clear case of a given kind in the absence of theoretical understanding may come to be a paradigm case of some different kind once the phenomena are better understood. At the same time, it would be a mistake to see these initial naïve judgments as wholly independent of background theory. Our rock collector is naïve, but he is not a tabula rasa. Background knowledge will play a substantial role in determining a first-pass categorization of samples. Judgments about which features of the rocks are even deemed relevant in classification—hardness, for example, but not size perhaps—are themselves theory-mediated, although the operation of theory here is unselfconscious and is better revealed by patterns of salience than it is by overt appeal to principle. The extent to which naïve investigators agree in their classifications is not evidence that these judgments somehow bypass background empirical belief, but rather that background theory may be widely shared.

So too, I want to say, with appeals to intuition in philosophy. These judgments are corrigible and theory-mediated. The extent of agreement among subjects on intuitive judgments is to be explained by common knowledge, or at least common belief, and the ways in which such background belief will inevitably influence intuitive judgment, although unavailable to introspection, are none the less quite real.

samples that prompt the introduction of a natural-kind term need not all be members of the kind in order for reference to occur; (2) The most central beliefs about a kind held by early investigators may turn out to be false without thereby undermining reference. These points are not so much argued in the text as taken for granted. What I do argue is that reference to knowledge may work in precisely the same way as reference to, for example, gold, and that there are no problems specific to the case of knowledge beyond those already addressed in the work on more paradigmatic natural kinds.

Indeed, I want to push this analogy considerably further. The judgments of rock collectors at early stages of investigation are substantially inferior, epistemically speaking, to those at later stages, when theoretical understanding is further advanced. We should not say that initial judgments are of no evidential value, for were this the case progress in theory would be impossible. Our untutored judgment must have some purchase on the phenomenon under investigation; but, that said, it must also be acknowledged that judgment guided by accurate background theory is far superior to the intuitions of the naïve. Intuition must be taken seriously in the absence of substantial theoretical understanding, but once such theoretical understanding begins to take shape, prior intuitive judgments carry little weight unless they have been endorsed by the progress of theory. The greater one's theoretical understanding, the less weight one may assign to untutored judgment.

All this applies equally well to the case of appeals to intuition in philosophy. We sometimes hear philosophers speak of some intuitions as 'merely' driven by theory, and thus to be ignored. While it is certainly true that judgments driven by bad theories are not to be taken seriously, the solution is not to try to return to some pure state of theory-independent judgment, before the fall, as it were; rather the solution is to get a better theory. Intuition in the absence of theory does not count for nothing, especially if no credible theory is available. But this is not to award high marks to intuitive judgment before the arrival of successful theory, let alone after, when the initially low value of such judgment drops still lower.

Now if this account is correct, why do philosophers spend so much time scrutinizing their intuitions, that is, looking inward, if, on my view, what they are really interested in is external phenomena? I have two things to say about this. First, if I am asked a question about rocks, for example, one way to answer the question is to ask myself what I believe the answer is. Although I am asked a question about rocks, I answer it by enquiring into what I believe. This is a perfectly reasonable thing to do if I have good reason to think that my current

beliefs are accurate, or if I do not have access to a better source of information. By looking inward, I answer a question about an external phenomenon. This, to my mind, is what we do when we consult our intuitions.

At the same time, however, I do not think that this can be the whole story here, and this is where the difference between the practice of naturalists and that of anti-naturalists comes into play. If my account is correct, then what we ought to be doing is not just consulting the beliefs we already have, but more directly examining the external phenomena; only then would appeals to intuition be given what, on my view, is their proper weight. Thus, appeal to intuition early on in philosophical investigation should give way to more straightforwardly empirical investigations of external phenomena. This is, to my mind, just what we see in the practice of naturalistically minded philosophers. Just a few decades ago, the philosophical practice of naturalistically minded epistemologists, for example, was almost indistinguishable from that of their more traditionally minded colleagues. Examples and counterexamples were used to motivate various accounts of knowledge and justification, and the progress of these accounts was shepherded along by a succession of appeals to intuition. This was, by my lights, a good thing to do at that stage of the investigation. Important insights were gained, which, given the absence of available explicitly articulated theory, could not have been gained by any other means. But now, as theory has progressed, more straightforwardly empirical investigation should be called upon; and this, of course, is just what we see. There is work on the psychology of inference, concept formation, cognitive development, and so on. Similarly, at the social level there is work on the distribution of cognitive effort, and, more generally, the social structures of science that underwrite and make scientific knowledge possible. As theory has advanced here, raw appeals to intuition have declined. Just look at the difference between early papers by Fred Dretske and Alvin Goldman, for example, and their more recent work. Similar results may be found by looking at naturalistically

minded work in philosophy of mind, and even in ethics, where work in cognitive science and anthropology have been shaping the work of contemporary naturalists. The difference in methodology between naturalists and their more traditional colleagues has, to my mind, been paying substantial dividends for those willing to draw on empirical work. But even those who disagree with me here will have to agree that naturalistic methodology is now importantly different from that of other philosophers, even if not very long ago it would have been difficult to separate the naturalists from the non-naturalists by looking at their methods.

From a naturalistic perspective, there are substantial advantages to looking outward at the phenomena under investigation rather than inward at our intuitions about them. Most obviously, since it is some external phenomenon that we are interested in, we should approach it by the most direct means possible, rather than the more indirect approach of looking at what we currently believe about it. Aside from being indirect, the approach of examining our intuitions clearly robs us of the best available source of correctives for current mistakes. Moreover, the appeal to imaginable cases and what we are inclined to say about them is both overly narrow and overly broad in its focus. It is overly narrow because serious empirical investigation of a phenomenon will often reveal possibilities which we would not, and sometimes could not, have imagined before. It is overly broad because many imaginable cases are not genuine possibilities and need not be accounted for by our theories. We might be able to imagine a rock with a certain combination of color, hardness, malleability, and so on, and such a rock, were it to exist, might be difficult or impossible to fit into our current taxonomy. But this raises no problem at all for our taxonomic principles if the imagined combination of properties is nomologically impossible. On the naturalistic view, the same may be said for testing our philosophical views against merely imaginable cases.

The suggestion that our intuitions about knowledge might actually be mistaken in the ways in which our intuitions about, say, gold

can be will strike some philosophers as implausible. It is important, however, to realize that our intuitions about knowledge are, in important ways, historically conditioned. Descartes's idea that knowledge required certainty was surely a product of his view that things firm and lasting in the sciences could only be achieved if scientific claims could be given an absolute guarantee. As it turned out, Descartes was wrong about this. In ways Descartes could never have anticipated, the sciences have gone on to achieve levels of ever-increasing explanatory and predictive success coupled with technological applications crucially dependent upon the approximate truth of their theoretical claims. These successes, producing things 'firm and lasting in the sciences', of just the sort Descartes hoped to achieve, did not depend in any way on the sort of certainty Descartes took to be a prerequisite for knowledge. The view that knowledge requires certainty is no longer widely held; it is an intuition that very few people have any more. In retrospect, this change in people's intuitions about the relationship between knowledge and certainty can be seen as a byproduct of the ways in which scientific success has actually been achieved. It is now just obvious to almost everyone that knowledge is possible without certainty. But it would be a mistake to see this realization as a matter of a priori insight into the nature of knowledge. Properly understood, it is, however indirectly, a claim empirically justified by the manner in which knowledge has in fact been gained.

Now all of this would make little difference if our intuitions responded to relevant empirical evidence in a timely fashion. Consulting our intuitions would be just as accurate as looking directly at the relevant phenomena if only our intuitions were suitably responsive to appropriate evidence. But there is no reason to think that our intuitions are suitably responsive to available evidence. Changing a society's intuitions about a particular subject matter takes a good deal of time. It is one thing for the scientific community to make an important discovery; quite another for that discovery to become common knowledge. It takes still longer before our whole

conception of a phenomenon comes to seem so obvious that we can no longer even remember what it was like to conceive of it in another way. But if we take the long historical view, this is exactly what happens with our intuitions when important discoveries are made. If we wish to understand a phenomenon accurately, we thus cannot merely seek to elucidate our current intuitive conception of it; we must examine the phenomenon itself. And this applies as much to understanding the nature of knowledge as it does to understanding the nature of gold.

One might seek a middle ground here. Alvin Goldman has suggested that there is room both for conceptual analysis of our folk epistemological concepts as well as a more scientific epistemology that would develop epistemological concepts that depart in important ways from our folk notions. But why, on this view, do the folk notions take on any epistemological import at all, especially in light of the fact that they are bound to build in, as Goldman himself points out, false presuppositions? On Goldman's view, there must be substantial continuity between the folk notions and the more scientific ones if there is to be such a thing as epistemology at all. Without such continuity, Goldman argues, we are just changing the subject. Now I have already argued that this account of what is involved in changing the subject, an account that Goldman shares with Jackson, is not correct. Continuity of concept is merely one way to mark commonality of subject matter; causal theorists of reference have another (and to my mind, better) account. Goldman does not consider the possibility of such an account in the case of knowledge, since he regards it as simply obvious that knowledge is not a natural kind. 'Whatever one thinks about justice or consciousness as possible natural kinds, it is dubious that knowledge or justificational status are natural kinds.'[19] It is the burden of Ch. 2 to show that such a possibility cannot be so easily dismissed.

[19] Goldman, 'Psychology and Philosophical Analysis', in *Liaisons: Philosophy Meets the Cognitive and Social Sciences* (MIT Press, 1992), 144.

Although I do not agree with Goldman that there must inevitably be the degree of continuity he requires between our folk concepts and those of a properly scientific epistemology, I do not wish to exaggerate my disagreement with him either. After all, the account of knowledge I endorse is, in the end, a reliability account very similar to the one Goldman himself offers. Even if, however, we should discover that there is a tremendous amount of continuity between our folk epistemological notions and those of a proper scientific epistemology, it remains to be shown why our folk epistemological notions are of *epistemological* interest in their own right. Why should our folk epistemological notions be of any more interest to epistemologists than our folk chemical notions are to chemists?

Goldman responds to this challenge: 'even if one rejects the plea for continuity, a description of our epistemic folkways is in order. How would one know what to criticize, or what needs to be transcended, in the absence of such a description? So a first mission of epistemology is to describe our folkways.'[20] But if knowledge truly is a natural kind, then this sort of response is inadequate. We would hardly think that the chemist's first job is an elucidation of folk chemical notions (especially if this required extraordinary effort by the entire community of chemists over a period of millennia) so that we would know what chemical views need to be transcended. In the case of chemistry, we can simply skip straight to the project of understanding the real chemical kinds as they exist in nature. My suggestion here is that we should take seriously the possibility that a similar strategy might be equally fruitful in epistemology.

I do not mean to suggest that on the naturalistic view we will ever be able wholly to avoid appealing to our intuitions. I do think that appeals to intuition will continue to play a role in the development of philosophical views, even as theory progresses. I noted earlier that the actual practice of naturalistically minded philosophers has changed with the progress of theory so that now there is a good deal

[20] Goldman, 'Epistemic Folkways and Scientific Epistemology', ibid. 156.

more empirical examination of various phenomena rather than an exclusive reliance on appeals to intuition. But this does not mean that appeals to intuition simply drop out of the picture. Thus, for example, in philosophy of mind not so many years ago there was a good deal of discussion about whether creatures who failed to exhibit certain sorts of characteristic behavior might nevertheless be in pain. These discussions did not involve much of a look at the empirical literature on pain; instead, they relied exclusively on appeals to intuition.[21] Now, although work in philosophy of mind involves a great deal of examination of the empirical literature, we still see appeals to intuition playing a role, although the intuitions are about more esoteric matters. For example, there is discussion of what magnetosomes represent, whether it be the presence of certain sorts of magnetic fields or, instead, the presence of anaerobic conditions.

The intuitions that naturalists currently appeal to, intuitions about matters far more esoteric than what is known about Brown in Barcelona, present clear cases of theory-mediated judgment, judgment which is rightly influenced by a large body of background belief. At the same time, these judgments are phenomenologically basic; their inferential heritage is not introspectively available. More than this, these judgments are typically far less well integrated with our best available theories, and thus not nearly so well justified,[22] as our more explicitly theory-guided judgments. As the scope of our theories expands, the use of such weakly founded judgments is a necessary stepping-stone to better theory. The use of intuitive judgment does not disappear at any stage of theorizing. Instead, old intuitions give way to well-integrated theoretical judgments, and, in addition, to new intuitions about matters not yet fully captured in explicit theory.

[21] For a valuable discussion of the state of the art on this issue, see Colin Allen, 'Animal Pain', manuscript.

[22] I do not mean to be diverging here from a reliabilist account of justification. Rather, I mean only to be pointing out that allowing one's judgments to be influenced by accurate theory tends to be a source of increased reliability.

We may thus respond to the first of Bealer's objections to naturalism by pointing out that appeals to intuition do not require some non-natural faculty or a priori judgment of any sort. Bealer's argument gets off on the wrong foot by assuming that intuitions are a priori; more than this, Bealer says, the distinction between common-sense empirical judgment and intuition is 'obvious' (p. 165). Obvious it may be to those opposed to naturalism, but the appeal to a priority is, of course, contentious in this context. Bealer is right to think that naturalists owe us an explanation of their practice of appealing to intuition, especially in light of their rejection of the a priori. At the same time, I hope I have shown that this explanation is one that naturalists may easily provide. The practice of appealing to intuition has no non-natural ingredients.

1.4 *Naturalism and rules of inference*

Bealer argues that naturalists are not only unable to account for their own philosophical practice, but that naturalistic scruples leave little room for legitimate belief about any subject, since 'following rules and procedures—for example, rules of inference' (p. 167) requires an acknowledgement of the force of a priori intuition. And Laurence BonJour comments, 'the practice of even those who most explicitly reject the idea of substantive *a priori* justification inevitably involves tacit appeal to insights and modes of reasoning that can only be understood as *a priori* in character, if they are justified at all.'[23] As BonJour points out, this leaves naturalists in an unenviable position: 'we see that the repudiation of all *a priori* justification is apparently tantamount to the repudiation of argument or reasoning generally, thus amounting in effect to intellectual suicide.'[24] But naturalists do not see the following of rules and procedures, in particular, the role of rules of inference, in the way in which Bealer and BonJour do.

[23] BonJour, *In Defense of Pure Reason*, p. ix. [24] Ibid. 5.

Naturalists, of course, make inferences, and they need to account for the legitimacy of this practice, at least in those cases in which it is legitimate. The legitimacy of an inference, on the naturalist view, is dependent upon its reliability: reliable inferential practices are epistemically legitimate; those which are unreliable are not. We must thus engage in a project of self-examination, in which we scrutinize our own epistemic practice. We wish to examine the inferential rules that underlie our practice of belief acquisition, and to the extent that we find unreliable inference patterns at work, we need to re-examine and modify our own practice. The empirical work involved in understanding our inferential habits is well underway, as is the assessment of its epistemological importance.

Reliability is the naturalist's standard here.[25] Meeting a priori standards is simply irrelevant. Rules of inference that tend to produce true beliefs in the kinds of environments that human beings occupy may fail to live up to a priori standards of cogency, but they are none the worse for that. By the same token, rules of inference that do meet a priori standards may be unworkable in practice or hopelessly mired in problems of computational complexity. These kinds of problems are not in any way ameliorated if the rules do meet a priori standards of cogency. A priori standards thus drop out of the picture entirely as simply irrelevant to proper epistemic practice. They fail to bear on the conduct of enquiry.[26]

A naturalistic account of proper belief acquisition thus does not need to appeal to a priori intuition of appropriate principles of inference. Recognition of appropriate inferential patterns is an empirical affair for the naturalist. More than this, justified belief, on at least one widely held naturalistic account, is a matter of reliable belief production and does not itself require recognition of that reliability.

[25] Why this should be so will be discussed in Ch. 2.

[26] It is thus particularly ironic that Mark Kaplan both criticizes naturalistic epistemology as irrelevant to the conduct of enquiry and simultaneously defends the importance of a priori standards of cogent argument. See Kaplan, 'Epistemology Denatured', 350–65.

Naturalistic scruples about appropriate belief production thus leave room for a great deal of knowledge.

1.5 *Naturalism and epistemic terminology*

Let me turn then to the last of Bealer's charges against naturalism, that in eschewing a priori intuition, naturalism leaves no room for epistemic terminology. This charge too, I believe, falls short of its mark. While the account naturalists give of epistemic terminology is anything but uncontroversial, it should not be controversial that naturalists have an account of that terminology that satisfies their own epistemic standards. The suggestion that naturalism is self-defeating is thus turned aside.

Epistemology, according to naturalism, investigates a certain natural phenomenon, namely, knowledge, and the term 'knowledge' and other epistemic idioms gain their reference in much the same way that natural-kind terms do. Now supposing that terms like 'knowledge' gain their reference in this way is not without its presuppositions, as I pointed out earlier. The phenomenon we call knowledge must have a certain degree of theoretical unity if reference is to be secured. Were we to discover that there is no more theoretical unity to the various items we call knowledge than there is to the set consisting of ships and shoes and sealing wax, then a presupposition of the introduction of the term would be undermined, and the view that there is no such thing as knowledge would be sustained. But naturalists, and indeed, most non-naturalists, do not think that such a possibility is at all likely.[27] Indeed, almost all epistemologists believe that there is a great deal of unity to the phenomenon of human knowledge. If there is indeed such a unity, one goal of epistemology is to say what it consists in. And of course if it should

[27] But see Michael Williams, *Unnatural Doubts: Epistemological Realism and the Basis of Skepticism* (Princeton University Press, 1996).

turn out there is no such unity, then one goal of epistemology would be to make that fact plain.

The investigation of the phenomenon of knowledge, on the naturalist's view, is an empirical investigation, and the legitimacy of epistemic terminology depends on its properly latching on to genuine, theoretically unified kinds. That is all that naturalistic scruples require. Because epistemology thus conceived is a wholly empirical investigation, naturalists have nothing here to apologize for. Their terminology earns its keep in just the way that chemical or biological or physical terminology earns its keep: it must be part of a successful empirical theory. The fact that terms such as 'knowledge' are not part of physics or chemistry does not show that they are not naturalistically acceptable. Rather, the question for naturalists is whether knowledge turns out to be a theoretically unified phenomenon, and this gives every appearance of being a legitimate and tractable empirical question.

Some will say that this enterprise, because it is descriptive, fails to engage the normative dimension of epistemological theorizing. They will argue that it is only by removing knowledge from the empirical realm, and making it the object of a priori investigation, that its normative character may emerge. This is an issue I want to discuss in detail later (in Ch. 5), but for now it is possible to say something brief that will go some distance toward responding to this challenge. The empirical investigation of knowledge may well reveal a phenomenon worthy of our pursuit. Surely such a suggestion does not require a degree of optimism ungrounded in fact. But this is all the normativity that our epistemic notions require. We should not suppose that the investigation of knowledge must be non-empirical if we are to be able to explain why knowledge is worth having.[28]

[28] Nor is it clear why a non-empirical investigation, if such were possible, would go any distance at all toward addressing the normativity issue. Much of the criticism of naturalism on this score has been, I believe, entirely beside the point. While it is correctly pointed out that it is hard to see how an empirical investigation could address

Epistemic terminology, and, indeed, philosophical terminology in general, must be grounded in the world if it is to be naturalistically legitimate. This does not require that such terminology appear in our physical theories, for naturalists need not accept any sort of reductionism. Once we regard epistemology as the investigation of a certain natural phenomenon, we clear the way for distinctively epistemic terminology. Naturalism would only threaten to eliminate epistemic terminology as illegitimate if there were no prospect of discovering theoretically unified epistemic phenomena. But there is little reason, I believe, to think that this is currently a genuine possibility. Moreover, if there were reason to worry on this account, it would spell the demise not only of naturalistic epistemology, but epistemology generally. Bealer's final objection, that naturalists are not entitled to the very epistemic terminology they make use of, thus ultimately fails.

1.6 *The autonomy of philosophy*

I want to close this chapter by addressing one further issue, a concern that, I believe, motivates Bealer's attack on naturalism, and this has to do with the autonomy of philosophy. In a lengthy attack[29] on the kind of scientific essentialism I favor, Bealer begins by suggesting that naturalism raises the possibility that science will somehow 'eclipse' philosophy. Naturalism threatens the autonomy of philosophical enquiry, Bealer argues, and it is thus only by rejecting naturalism that we may make room for a distinctively philosophical enterprise.[30] BonJour has a similar conception of philosophical

our normative concerns, critics of naturalism take this to count in favor of some sort of a priori investigation. But neither is it clear how an a priori investigation could address those concerns. The appropriate conclusion, I believe, is that we simply don't currently see how normative concerns can be addressed at all. This does not count for or against any particular view.

[29] Bealer, 'The Philosophical Limits of Scientific Essentialism'.

[30] For a related concern, see the objection due to Fumerton discussed in sect. 6.2 below.

endeavors: 'philosophy is *a priori* if it is anything (or at least if it is anything intellectually respectable)'.[31] But then the naturalistic rejection of the a priori results immediately in a repudiation of philosophy itself. As Bealer and BonJour see it, if naturalism is right, we should all give up doing philosophy and take up science instead.

Now I myself have a high regard for philosophy. Although I am a naturalist, I do not believe that philosophy must be eliminated. Naturalists do not regard philosophy as illegitimate, nor do they see it as in any way threatened by the progress of science. At the same time, we do not wish to grant philosophy the degree of autonomy that Bealer, BonJour, and other opponents of naturalism would favor. There are important issues here, and it is worth making clear just where naturalism stands on them.

Questions about knowledge and justification, questions about theory and evidence, are, to my mind, legitimate questions, and they are ones in which philosophy has a special stake. The questions that philosophers wish to ask about these topics are different from those addressed by historians, sociologists, and psychologists, but no less important or intellectually respectable. If the autonomy of a discipline consists in its dealing with a distinctive set of questions, or in approaching certain phenomena with a distinctive set of concerns, then philosophy is surely an autonomous discipline. There is no danger that these questions and concerns will somehow be co-opted by other disciplines.

When Bealer raises the issue of philosophy's autonomy, however, he has in mind something quite different from this. For Bealer, the autonomy of philosophy is identified with the claim that philosophical knowledge is a priori, entirely independent of anything the empirical sciences have to offer. Now this, of course, is a claim any naturalist will want to reject. On the naturalist's view, philosophical questions are continuous with the empirical sciences. Work in the empirical sciences is deeply relevant to philosophical questions, and

[31] BonJour, *In Defense of Pure Reason*, p. ix.

our philosophical theories are constrained and guided by results in other disciplines. It is worth noting that the special sciences are not autonomous in anything like Bealer's sense, i.e. they are not wholly independent of work in other disciplines. Work in biology is not wholly independent of chemistry; sociology is not wholly independent of psychology; and so on. But the loss of this sort of autonomy does not rob these disciplines of their legitimacy, nor does it threaten the special sciences with the loss of their distinctive subject matter. Biologists need not fear that their field will be taken away from them by chemists once it is recognized that chemistry is relevant to biological concerns.

So too with philosophy. In recognizing that philosophy is continuous with the sciences, we need not fear that philosophy will thereby be 'eclipsed' by science. The constraints that science presents for philosophical theorizing should be welcomed, for philosophical theorizing unconstrained by empirical fact loses its connection with the very phenomena which we, as philosophers, seek to understand. Philosophy is an autonomous discipline, in the sense that it addresses a distinctive set of questions and concerns, and in this respect it is no more nor less autonomous than physics or chemistry or biology. This is surely all the autonomy we should want. It is, in any case, all the autonomy that we may have.

1.7 *Conclusion*

Bealer's multi-count indictment of naturalism is not supported by the facts. We can make perfectly good sense of a thoroughly empirical philosophy, one that does not assign either the weight or the provenance to intuition that Bealer and others do. Epistemology is fully naturalized when it regards knowledge as a natural phenomenon, an object of study with a substantial degree of theoretical unity to it. And with the burden of this indictment now discharged, we may turn to an investigation of knowledge itself.