PHILOSOPHICAL INTUITIONS: THEIR TARGET, THEIR SOURCE, AND THEIR EPISTEMIC STATUS

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Summary

Intuitions play a critical role in analytical philosophical activity. But do they qualify as genuine evidence for the sorts of conclusions philosophers seek? Skeptical arguments against intuitions are reviewed, and a variety of ways of trying to legitimate them are considered. A defense is offered of their evidential status by showing how their evidential status can be embedded in a naturalistic framework.

1. *Intuitions in philosophy*

One thing that distinguishes philosophical methodology from the methodology of the sciences is its extensive and avowed reliance on intuition. Especially when philosophers are engaged in philosophical "analysis", they often get preoccupied with intuitions. To decide what is knowledge, reference, identity, or causation (or what is the concept of knowledge, reference, identity, or causation), philosophers routinely consider actual and hypothetical examples and ask whether these examples provide instances of the target category or concept. People's mental responses to these examples are often called "intuitions", and these intuitions are treated as evidence for the correct answer. At a minimum, they are evidence for the examples' being instances or non-instances of knowledge, reference, causation, etc. Thus, intuitions play a particularly critical role in a certain sector of philosophical activity.

The evidential weight accorded to intuition is often very high, in both philosophical practice and philosophical reflection. Many philosophical discoveries, or putative discoveries, are predicated on the occurrence of certain widespread intuitions. It was a landmark discovery in analytic epistemology when Edmund Gettier (1963) showed that knowledge isn't

equivalent to justified true belief. How did this "discovery" take place? It wasn't the mere publication of Gettier's two examples, or what he said about them. It was the fact that almost everybody who read Gettier's examples shared the *intuition* that these were not instances of knowing. Had their intuitions been different, there would have been no discovery. Appeals to intuition are not confined to epistemology; analytic philosophy as a whole is replete with such appeals. Saul Kripke remarks: "Of course, some philosophers think that something's having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence in favor of anything, myself. I really don't know, in a way, what more conclusive evidence one can have for anything, ultimately speaking" (1980: 42).

As a historical matter, philosophers haven't always described their methodology in the language of intuition. In fact, this seems to be a fairly recent bit of usage. Jaakko Hintikka (1999) traces the philosophical use of "intuition" to Chomsky's description of linguistics' methodology. In the history of philosophy, and even in the early years of analytic philosophy, the terminology of intuition is not to be found. Of course, historical philosophers dealt extensively with intuition in other contexts, but not in the context of appealing to particular examples and their classification. This is not to say that historical philosophers and earlier 20th-century philosophers did not make similar philosophical moves. They did make such moves, they just didn't use the term "intuition" to describe them. Consider Locke's presentation of the famous prince-cobbler case in his discussion of personal identity:

> For should the soul of a prince, carrying with it the consciousness of the prince's past life, enter and inform the body of a cobbler, as soon as deserted by his own soul, *every one sees* he would be the same person with the prince … (Locke 1694/1975: 44; emphasis added)

Locke says that every one "sees" that a certain classification—being the same as—is appropriate, and his term "sees" is readily translatable, in current terminology, as "intuits". Among ordinary-language philosophers of the mid-20th century, roughly the same idea was expressed in terms of what people would or wouldn't be inclined to *say*. One "would say" that the cobbler was the same person as the prince; one "wouldn't say" that a Gettier protagonist had knowledge. Here the propriety of saying or not saying something took the place of having an intuition; the matter was described in terms of speech inclinations rather than mental episodes. Nonetheless, the epistemological status of these inclinations or episodes

played the same role in philosophical methodology. Each was invoked as a crucial bit of evidence for the philosophical "facts" in question.

2.   *Skepticism about intuitions*

Nowadays philosophers routinely rely on intuitions to support or refute philosophical analyses, but a number of skeptics have emerged who raise challenges to this use of intuition. The skeptics include Robert Cummins (1998), Jonathan Weinberg, Shaun Nichols and Stephen Stich (2001), and Michael Devitt (1994). They dispute the evidential credentials or probity of intuitions. They deny that intuitions confer the kind of evidential support that they are widely taken to confer.

The grounds for skepticism are somewhat variable, but mostly they concern the fallibility or unreliability of intuitions, either intuitions in general or philosophical intuitions in particular. Here are three specific criticisms.

(1) Garden-variety intuitions are highly fallible. Why should philosophical intuitions be any different? If the latter are highly fallible, however, they shouldn't be trusted as evidence.

Garden-variety intuitions include premonitions about future events, intuitions about a person's character (based on his appearance, or a brief snatch of conversation), and intuitions about probabilistic relationships. These are all quite prone to error. What reason is there to think that philosophical intuitions are more reliable?

(2) People often have conflicting intuitions about philosophical cases. One person intuits that case x is an instance of property (or concept) F while another person intuits that case x isn't an instance of property (or concept) F. When such conflicts occur, one of the intuitions must be wrong. If the conflicts are frequent, the percentage of erroneous intuitions must be substantial and the percentage of correct intuitions not so high. Thus, the modest level of reliability of philosophical intuitions doesn't warrant assigning them significant evidential weight.

A third ground for skepticism doesn't appeal directly to the unreliability of

intuition but rather to our inability to (independently) *know* or *determine* its reliability.

(3) The outputs of an instrument, procedure, or method constitute data we can properly treat as evidence only when that instrument, procedure, or method has been calibrated (Cummins 1998). Calibration requires corroboration by an independent procedure. Has intuition been calibrated? Has it been shown to be reliable by a method independent of intuition itself? There is no way to do this. Suppose we have a philosophical interest in fairness, and we ask people for their intuitions about the fairness of distributions described in certain hypothetical cases. We shouldn't trust their intuitions about these cases unless we have antecedently determined that their fairness intuitor is reliable, i.e., unless it has been calibrated. But how can we perform this calibration? We don't have a "key" by which to determine which outputs of their intuitor are correct, and there is no key to be found.

3. *Initial responses to skeptical challenges*

For each of these skeptical challenges, there appear to be at least initially plausible responses. In response to challenge (1), a defender of philosophical intuition would want to distinguish between different types of intuitions. First, the intuitions we have here identified are what might be called *classification* or *application* intuitions, because they are intuitions about how cases are to be classified, or whether various categories or concepts apply to selected cases.[1] This in itself, however, provides no reason for thinking that philosophical intuitions are epistemically superior to garden-variety intuitions. Why should classification or application intuitions be superior? A supplementary response is that application intuitions are a species of *rational* intuitions, and that rational intuitions are more reliable than others. Many authors are sympathetic to this approach, but George Bealer (1998) has been most forceful in championing it. Bealer distinguishes between physical and rational intuitions, and regards only the latter as having special epistemic worth. We shall return to this below.

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1. Frank Jackson (1998) also views classification, or application, intuitions as the central type of philosophical intuition.

In response to challenge (2), a defender of philosophical intuitions might urge caution. It remains to be seen just how extensive are the conflicts in application intuitions across different individuals. Moreover, whether the conflicts are genuine depends on the precise contents of the intuitions, or what they are taken to be evidence *for*. It is possible that a state of affairs for which one person's intuition is evidence doesn't really conflict with a state of affairs for which another person's intuition is evidence, even when there is a "surface" conflict. I'll return to this point below as well.

In response to challenge (3), a defender of philosophical intuitions might reject Cummins's epistemological presuppositions. The defender might say that independent corroboration, or calibration, of an instrument, procedure, or method is too stringent a requirement on its evidence-conferring power. In particular, there must be some procedures or methods that are *basic*. In other terminology, there must be some basic "sources" of evidence. Basic sources are likely to include mental faculties such as perception, memory, introspection, deductive reasoning, and inductive reasoning. These faculties are all regarded, by many or most epistemologists, as bona fide sources of evidence. Yet all or many of these sources may be basic in precisely the sense that we have no independent faculty or method by which to establish their reliability. Yet that doesn't undercut their evidence-conferring power. Consider memory, for example. Memory may be our basic way of forming true beliefs about the past. All other ways of gaining access to the past depend on memory, so they cannot provide *independent* ways of establishing memory's reliability (see Alston 1993). If we accept Cummins's constraint on evidencehood, the outputs of memory will not constitute legitimate data or pieces of evidence. But that is unacceptable, on pain of general skepticism. It is better to accept the conclusion that basic sources of evidence don't have to satisfy the calibration, or independent corroboration, constraint. Intuition may be among the basic sources of evidence.

Although Cummins's independent corroboration condition on a source of evidence is too stringent, it seems reasonable to substitute a weaker condition as a further requirement on evidencehood. This weaker condition is a "negative" one, viz., that we *not* be justified in believing that the putative source is *un*reliable. A possible variant is the condition that we *not* be justified in *strongly doubting* that the source is reliable. The latter negative condition will sometimes be invoked in the discussion to follow.

4. *The targets of philosophical analysis*

In response to skeptical challenge (2), I said that resolution of this challenge requires a more careful inquiry into the precise targets of philosophical analysis. Philosophical analysis, of course, doesn't simply aim to answer questions about particular cases. Epistemology isn't much interested in whether this or that example is an instance of knowledge; rather, it aims to say what knowledge is in general, or something in that ballpark. Individual cases are typically introduced as test cases of one or more general accounts. Depending on how a case is classified, it might falsify a general account or corroborate it. But what, exactly, does philosophical analysis aim to give general accounts *of*? Knowledge, causation, personal identity, and so forth are typical examples of categories that absorb philosophy, but different theorists have different conceptions (often unstated) of how, exactly, these targets are to be construed. A choice among these different construals can make a big difference to the viability of intuition as a source of evidence about the targets, because many construals invite *strong doubts* that the source is reliable. Let us examine five ways of construing the targets.

(1) Platonic forms
(2) Natural kinds
(3) Concepts$_1$—concepts in the Fregean sense
(4) Concepts$_2$—concepts in the psychological sense, specifically, the individualized, personal sense
(5) Concepts$_3$—shared concepts$_2$

The first two construals invoke entities that aren't described as concepts. Each is some sort of non-conceptual entity that exists entirely "outside the mind". According to the first construal, philosophy aims to obtain insight into (e.g.) the form of the Good, and other such eternal, non-spatially-located entities. According to the second construal, knowledge, causation, personal identity, and so forth are "natural" properties or relations, which exist and have their distinctive characteristics quite independently of anybody's concepts or conception of them, like water or electricity.

There are two questions to be posed for each of these (and similar) construals. First, under this construal how could it plausibly turn out that intuitions are good evidence for the "constitution" or characteristics of the targets? Second, does this construal comport with the actual intuitional methodology used by analytic philosophers? Start with construal (1). If

the target of philosophical analysis is the constitution or composition of Platonic forms (or their ilk), the question is why an episode that occurs in somebody's mind—an episode of having an intuition—should count as evidence about the composition of a Platonic form.[2] If someone experiences an intuition that the protagonist in a selected Gettier example doesn't know the designated proposition, why should this intuitional experience be *evidence* that the form KNOWLEDGE is such that the imaginary protagonist's belief in this proposition doesn't "participate" in that form? What connection is there between the intuition episode and the properties of the form KNOWLEDGE such that the intuition episode is a reliable indicator of the properties of KNOWLEDGE? (I am assuming, for argument's sake, that this form exists.) We have reason to seriously *doubt* the existence of a reliable indicatorship relation.

Notice that it doesn't much matter how, exactly, we characterize intuitions. Whether intuitions are inclinations to believe, or a *sui generis* kind of seeming or propositional attitude (see Bealer 1998: 207), it is still a puzzle why the occurrence of such a mental event should provide evidence for the composition of a Platonic form. Compare this case with perceptual seemings and memory seemings. In these cases we know (in outline, if not in detail) the causal pathways by which the properties of an external stimulus can influence the properties of a visual or auditory experience. With this kind of dependency in place, it is highly plausible that variations in the experience reflect variations in the stimulus. So the specifics of the experience can plausibly be counted as evidence about the properties of the stimulus. Similarly in the case of memory, what is presently recalled varies (counterfactually) with what occurred earlier, so the specifics of the recall event can be a reliable indicator of the properties of the original occurrence. But is there a causal pathway or counterfactual dependence between Platonic forms and any mental "registration" of them? A causal pathway seems to be excluded, because Platonic forms are not spatio-temporal entities. A counterfactual dependence is not impossible, but there is reason to doubt that such a dependence obtains. I here register the general sorts of qualms that have long plagued traditional accounts of rational insight or "apprehension" of abstract entities. These accounts leave too many mysteries, mysteries that undercut any putative reliability needed to support a reflective acceptance of an evidential relation-

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2. For an earlier treatment of this question, and analogous questions for the other construals of the targets, see Goldman and Pust (1998).

ship between intuitional episodes and their targets construed as abstract entities.

Let us turn now to construal (2), the natural kinds construal, which has been formulated and championed by Hilary Kornblith (2002). Kornblith emphasizes that natural kinds are "in the world" phenomena, emphatically not merely concepts of ours. He rejects concepts as the objects of epistemological theorizing on the ground that by bringing concepts into an epistemological investigation, "we only succeed in changing the subject: instead of talking about knowledge, we end up talking about our concept of knowledge" (2002: 9–10). For Kornblith, the methodology of consulting intuitions (within epistemology) is part of a *scientific* inquiry into the nature of knowledge, closely akin, to use his example, to what a rock collector does when gathering samples of some interesting kind of stone for the purpose of figuring out what the samples have in common. Let us examine this approach.

Presumably, an inquiry into the composition of a natural kind is an inquiry into a *this*-world phenomenon. Even if natural kinds have the same essence or composition in every possible world in which they exist, the question for natural science is which of the conceivable natural kinds occupy *our* world. Does this feature of scientific inquiry into natural kinds mesh with the philosophical practice of consulting intuitions? No. A ubiquitous feature of philosophical practice is to consult intuitions about merely conceivable cases. Imaginary examples are treated with the same respect and importance as real examples. Cases from the actual world do not have superior evidential power as compared with hypothetical cases. How is this compatible with the notion that the target of philosophical inquiry is the composition of natural phenomena? If philosophers were really investigating what Kornblith specifies, would they treat conceivable and actual examples on a par? Scientists do nothing of the sort. They devote great time and labor into investigating actual-world objects; they construct expensive equipment to perform their investigations. If the job could be done as well by consulting intuitions about imaginary examples, why bother with all this expensive equipment and labor-intensive experiments? Evidently, unless philosophers are either grossly deluded or have a magical shortcut that has eluded scientists (neither of which is plausible), their philosophical inquiries must have a different type of target or subject-matter.

In responding to criticisms of this sort, Kornblith (2005) indicates that although he regards epistemology as an empirical discipline, it nonetheless

investigates necessary truths about knowledge. Just as it is a necessary truth that water is H_2O, so there are various necessary truths about knowledge, and it is epistemology's job to discover these truths. Might this be why it is legitimate for epistemologists to adduce merely conceivable examples, involving other possible worlds? Kornblith doesn't say this, and it seems inadequate as a potential response. While it may be a necessary truth that water is H_2O, scientists first have to discover that what water is (in the actual world) is H_2O, and Kornblith admits that this must be an empirical discovery. Intuitive reactions to merely imaginary cases are not part of such an empirical procedure. Similarly, we cannot scientifically discover what knowledge is in the actual world by consulting intuitions about imaginary cases. So why do philosophers engage in this activity?

When I raise this point (Goldman 2005) in discussing Kornblith's book, he concedes that his approach doesn't explain philosophers' preoccupation with imaginary examples. He adds: "Goldman may have underestimated the extent to which I believe that standard philosophical practice should be modified" (2005: 428). So Kornblith agrees that, so long as we are discussing existing philosophical practice, his kind of naturalism cannot do the job. But he holds that existing practice is somehow inadequate or objectionable. I shall return to these concerns of his at the end of this paper. For now I reiterate the point that as long as we are merely trying to describe or elucidate existing practice, the natural kinds approach (as Kornblith spells it out) cannot be right.

5. *Concepts in the Fregean sense*

We turn now to the third proposed construal, concepts in the Fregean sense of "concept", which we called "concepts$_1$". In this sense, concepts are abstract entities of some sort, graspable by multiple individuals. These entities are thought of as capable of becoming objects of a faculty of intuition, *rational* intuition. Moreover, philosophers like Bealer (1998) want to say that rational intuitions are sufficiently reliable to confer evidence on the appropriate classification (or "application") propositions. Indeed, rational intuition is a faculty or source that is *modally reliable* (in Bealer's terminology). Two questions arise here: What distinguishes rational intuitions from other types of intuition, and is there good reason to think that rational intuitions—specifically, the sub-category of classification intuitions—have the needed properties to qualify as an evidential source?

According to Bealer, rational intuitions are distinguishable from other (e.g., physical) intuitions in virtue of the fact that rational intuitions have a sort of modal content. "[W]hen we have a rational intuition—say, that if P then not not P—it presents itself as necessary; it does not seem to us that things could be otherwise; it must be that if P then not not P." (1998: 207) Bealer goes on to say that application intuitions, i.e., intuitions to the effect that a certain concept does or does not apply to a certain case, are a species of rational intuitions. He is not sure how to analyze what it means for an intuition to present itself as necessary (and hence to be a rational intuition), but offers the following tentative proposal: "necessarily, if x intuits that P, it seems to x that P and also that necessarily P" (1998: 207).

Does this work? How are we to understand the initial operator "necessarily"? Is it metaphysical necessity? So understood, the claim can't be right. It implies that it is metaphysically impossible for there to be any creature for whom it seems that $18 + 35 = 53$ but for whom it doesn't seem that *necessarily*, $18 + 35 = 53$. But such a creature surely is possible. For starters, there could be a creature that understands arithmetic but doesn't understand modality. Second, there could be a creature that understands both arithmetic and modality but forms intuitions about modality more slowly than intuitions about arithmetic. At some moments, it seems to this creature that the foregoing arithmetic sum is correct but it doesn't yet seem to him that it is necessary. The same point applies to application intuitions. Presented with a Gettier example, it strikes a beginning philosophy student that this is not an instance of knowing, but it doesn't strike the student as necessarily true. I suspect this is the actual condition of many beginning philosophy students. They have application intuitions without any accompanying modal intuitions.

A different approach to the explication of rational intuitions is pursued by Ernest Sosa (1998). In seeking to identify intuition in the philosophically relevant sense, Sosa places great weight on the content of an intuition being *abstract*. "To intuit is to believe an abstract proposition merely because one understands it and it is of a certain sort …" (1998: 263–264). Should rational or intellectual intuitions be restricted to ones whose contents are abstract propositions? Sosa characterizes abstract propositions as ones that "abstract away from any mention of particulars" (1998: 258). But this definition threatens to exclude our primary philosophical examples, viz., application intuitions. These often concern particulars, both particular individuals and particular situations. Thus, Sosa's account threatens to rule out the very examples that most interest us.

If we can't unify rational intuitions in terms of their *contents*, perhaps they can be unified in terms of their *phenomenology*. Perhaps a common phenomenology unites intuitions concerning logic, mathematics, and conceptual relationships. What might this common phenomenology be? A phenomenological feature they share is the feeling that they come from "I know not where". Their origins are introspectively opaque. This isn't helpful, however, to rationalists of the type under discussion. *All* intuitions have this opaqueness-of-origin phenomenology, including garden-variety intuitions like baseless hunches and conjectures, which are rightly disparaged as unreliable and lacking in evidential worth. Grouping application intuitions with this larger, "trashy" set of intuitions is likely to contaminate them, not demonstrate their evidential respectability.

This problem might be averted if we turn from phenomenology to psychological origins, including unconscious psychological origins. Hunches and baseless conjectures presumably lack a provenance comparable to that of mathematical, logical, or application intuitions. So unconscious origin looks like a promising basis for contrasting these families of intuitions. There is a serious problem here, though. It is unlikely that there is a single psychological faculty responsible for all intellectual insight. The psychological pathways that lead to mathematical, logical, and application intuitions respectively are probably quite different. Elementary arithmetic intuitions, for example, are apparently the product of a domain-specific faculty of numerical cognition, one that has been intensively studied in recent cognitive science (Dehaene 1997). There is no reason to expect logical intuitions to be products of the same faculty. Application intuitions are likely to have still different psychological sources, to be explored below. So if the suggestion is that application intuitions should be grouped with mathematical and logical intuitions because of a uniform causal process or faculty of intellectual insight, this is psychologically untenable. It is initially plausible because they are not phenomenologically distinguishable. But if causal origin runs deeper than phenomenology—as it surely does—then the sameness-of-psychological-origin thesis is unsustainable. Moreover, difference of psychological origin is important, because it undercuts the notion that rational intuitions are homogeneous in their reliability. Arithmetic intuitions might be reliable—even modally reliable—without application intuitions being comparably reliable.

If the targets of application intuitions are Fregean concepts, does this inspire confidence that such intuitions are highly reliable? Oddly, Bealer himself makes no claim to this effect; his central claim is vastly more cau-

tious. Bealer acknowledges that concepts can be possessed either weakly or strongly. Weak possession is compatible with misunderstanding or incomplete understanding. Only strong possession, which Bealer calls "determinate" concept possession, carries with it a guarantee of truth-tracking intuitions. However, Bealer offers no guarantee that either ordinary people or philosophers who possess a concept will possess it determinately. In the concluding section of his 1998 paper, Bealer summarizes his argument (in part) as follows: "With this informal characterization in view, intuitive considerations then led us to the *possibility of determinate possession*, the premise that it should be at least possible for most of the central concepts of philosophy to be possessed determinately" (1998: 231, emphasis in the original). If the determinate possession of philosophical concepts is merely *possible*, and by no means guaranteed or even probable, why should philosophers rely on ordinary people's intuitions as guides to a concept's contours? No evidence is provided that people, especially lay people, actually grasp selected philosophical concepts determinately. So Bealer's approach provides no solid underpinning for the philosophical practice of consulting ordinary people's application intuitions.

Finally, construing Fregean concepts as the targets of application intuitions doesn't safeguard against the possibility of different people having different application intuitions about the same concept and example. If there are many instances of such conflicts, these intuitions won't have even high *contingent* reliability, much less high *modal* reliability. Traditionally, philosophers haven't worried much about this prospect. But some of the intuition skeptics mentioned at the outset worry very much about it. Jonathan Weinberg, Shaun Nichols and Stephen Stich (2001) have done studies of people's intuitions, including intuitions about the applicability of the knowledge concept in Gettier-style cases. In contrast to the widespread view among epistemologists that Gettier-style cases prompt highly uniform intuitions, they found substantial divergences in intuition, surprisingly, along cultural lines. Undergraduate students at Rutgers University were used as subjects, and were divided into those with Western (i.e., European) ethnicities versus East Asian ethnicities. In one study involving a Gettier-style case, a large majority of Western subjects rendered the standard verdict that the protagonist in the example "only believes" the proposition, whereas a majority of East Asian subjects said the opposite, i.e., that the protagonist "knows" (2001: 443; see Figure 5). If cases like this are rampant (and that remains to be shown), it's a

non-trivial challenge to the reliability of application intuitions under the Fregean concept construal.

6. *Concepts in the personal psychological sense*

Suppose that the target of philosophical analysis is concepts, but concepts in the psychological rather than the Fregean sense. In this sense, a concept is literally something in the head, for example, a mental representation of a category. If there is a language of thought, a concept might be a (semantically interpreted) word or phrase in the language of thought. What I mean by a *personal* psychological sense of concept is that the concept is fixed by what's in its owner's head rather than what's in the heads of other members of the community.[3] It's an individual affair rather than a social affair. This does not prejudice the case for a separate sense of "concept" pertaining to a community (what I mean to denote by "concept$_3$").

A chief attraction of construing concepts$_2$ as the targets of philosophical analysis (though perhaps not the ultimate targets) is that it nicely handles challenges to the reliability of intuition arising from variability or conflicts of intuitions across persons. If the targets are construed as concepts in the personal psychological sense, then Bernard's intuition that F applies to x is evidence only for *his* personal concept of F, and Elke's intuition that F doesn't apply to x is evidence only for *her* personal concept of F. If Bernard intuits that a specified example is an instance of knowledge and Elke intuits otherwise, the conflict between their intuitions can be minimized, because each bears evidentially on their own personal concepts, which may differ. This may be precisely what transpires in the cases reported by Weinberg et al. Under this construal of the evidential targets, interpersonal variation in intuitions doesn't pose a problem for intuitional reliability, because each person's intuition may correctly indicate something about

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3. This is not intended as a position statement on the wide/narrow issue concerning the contents of thought. It may be that thought contents *in general* do not supervene simply on events that transpire in an individual thinker's head. Nonetheless, the specific thoughts of each person—including the specific concepts each entertains—are a special function of what goes on in that individual's head rather than anybody else's. If Jones never entertains the thought that aardvarks drive automobiles, his never entertaining it is a function of what happens in his head rather than any other person's head. And if he never entertains the concept of an aardvark, this is a function of what happens in his head rather than any other person's head—at least of what happens in his head in interaction with the environment rather than what happens in any other person's head in interaction with the environment.

his or her concept$_2$, viz., whether the concept$_2$ does or doesn't apply to the chosen example.

It must be conceded that when a person thinks the thought, or has the intuition, "The Gettier disjunction case isn't an instance of knowledge", the content of the thought is not self-referential. It isn't naturally expressed as, "The Gettier disjunction case isn't an instance of my *personal* concept of knowledge". Nonetheless, epistemologists are at liberty to take the person's intuition, or thought, as evidence for a proposition concerning that person's individualized, psychological concept. This is what I propose to do.

But why is a person's intuition *evidence* for a personal psychological concept? I assume that any evidential relationship depends, at a minimum, on a relation of reliable indicatorship. But what makes such a relation hold in the case of application intuitions and concepts$_2$? Do we have reason for thinking that it holds? And do we avoid reasons for seriously *doubting* the existence of a reliable indicatorship relation?

Distinguish two approaches to the relation between concepts and evidencehood: *constitutive* and *non-constitutive* approaches. A constitutive approach can be illustrated by reference to phenomenalism (or other assorted versions of idealism). According to phenomenalism, what it *is* to be a physical object of a certain sort is that suitably situated subjects will experience perceptual appearances of an appropriate kind. Appearances of the appropriate kind are not only evidence for a physical object of the relevant sort being present, but the evidentiary relation is *constitutively* grounded. The evidentiary status of appearances is grounded in the very constitution of physical objects. Physical objects are precisely the sorts of things that give rise to appearances of the kind in question. According to realism, by contrast, to be a physical object has nothing essentially to do with perceptual experience. True, physical objects may cause perceptual experiences, but what they *are* (intrinsically) is wholly independent of perceptual experience. This view is compatible with perceptual experiences qualifying as evidence for the presence of appropriate physical objects, but here the evidential relation would not be constitutively grounded. There are many possible theories of non-constitutive evidencehood; I won't try to survey such theories. What is important for the moment is simply the distinction between constitutive and non-constitutive groundings of evidential relations.

Although I don't support phenomenalism, I am inclined to support a parallel theory for the evidential power of application intuitions. I think that the evidential status of application intuitions is of the constitutively-

grounded variety. It's part of the nature of concepts (in the personal psychological sense) that possessing a concept tends to give rise to beliefs and intuitions that accord with the contents of the concept. If the content of someone's concept F implies that F does (doesn't) apply to example x, then that person is disposed to intuit that F applies (doesn't apply) to x when the issue is raised in his mind. Notice, I don't say that possessing a particular concept of knowledge makes one disposed to believe a correct *general* account of that knowledge concept. Correct general accounts are devilishly difficult to achieve, and few people try. All I am saying is that possessing a concept makes one disposed to have pro-intuitions toward correct applications and con-intuitions toward incorrect applications—correct, that is, relative to the contents of the concept as it exists in the subject's head. However, our description of these dispositions must be further qualified and constrained, to get matters right.

There are several ways in which application intuitions can go wrong. First, the subject may be misinformed or insufficiently informed about example x. Her intuitive judgment can go awry because of an erroneous belief about some detail of the example. Second, although she isn't misinformed about the example, she might forget or lose track of some features of the example while mentally computing the applicability of F to it. Third, the subject might have a false theory about her concept of F, and this theory may intrude when forming an application intuition. It's important here to distinguish between a theory presupposed by a concept and a theory *about* the concept, i.e., a general account of the concept's content. Here I advert only to the latter. Any of these misadventures can produce an inaccurate intuition, i.e., inaccurate relative to the user's own personal concept. For these reasons, intuitions are not infallible evidence about that personal concept.

These points go some distance toward explaining actual philosophical practice. First, philosophers are leery about trusting the intuitions of other philosophical analysts who have promoted general accounts of the analysandum, e.g., knowledge or justification. Commitment to their own favored account can distort their intuitions, even with respect to their own (pre-theoretical) concept. Second, because erroneous beliefs about an example can breed incorrect intuitions, philosophers prefer stipulated examples to live examples for purposes of hypothesis testing. In a stipulated example, the crucial characteristics of the example are highlighted for the subject, to focus attention on what is relevant to the general account currently being tested.

Although errors in application intuitions are possible, a person's application intuitions vis-à-vis their own personal concepts are highly likely to be correct if the foregoing safeguards are in place. Thus, the reliability criterion for evidence-conferring power—one very natural criterion (or partial criterion)—is met under the concepts$_2$ construal of the targets of philosophical analysis.

Another virtue of the concepts$_2$ approach is the congenial naturalistic framework it provides for the respectability of application intuitions as evidence. Unlike Platonic forms, natural kinds, or Fregean concepts, there can be a clear *causal* relationship between personal concepts and application intuitions concerning those concepts. Although psychological details remain to be filled in, there is nothing inherently mysterious in there being a causal pathway from personal psychological concepts to application intuitions pertaining to those concepts. Personal psychological concepts can be expected to produce accurate intuitions concerning their applicability. So as long as the various threats of error of the kinds enumerated above aren't too serious, high reliability among application intuitions is unperplexing and unremarkable under the concepts$_2$ approach. Although naturalistically-minded philosophers are understandably suspicious and skeptical about intuitions and their evidential *bona fides*, here we have a satisfying resolution to the challenge from naturalistic quarters, a resolution that copes straightforwardly with existing evidence of interpersonal variation in intuitions. Thus, I share with Kornblith the aim of obtaining an epistemology of philosophical method that sits comfortably within a naturalistic perspective. Whereas Kornblith's naturalism leads him to extra-psychological objects as the targets of philosophical theory and to very limited acceptance of intuitional methodology, my psychologistic brand of naturalism leads to personal psychological concepts as the initial targets of philosophical analysis and to a greater acceptance of standard intuitional methodology.

7. *Shared and socially fixed concepts*

A predictable response to our proposal is that even if intuitions constitute evidence for personal psychological concepts, that's not a very interesting fact. Personal concepts can't be all—or even very much—of what philosophy is after. Fair enough. I am not saying that the analysis of personal concepts is the be-all and end-all of philosophy, even the analytical part

of philosophy. But perhaps we can move from concepts$_2$ to concepts$_3$, i.e., shared (psychological) concepts. This can be done if a substantial agreement is found across many individuals' concepts$_2$. Such sharing cannot be assumed at the outset, however; it must be established. Philosophers often presume that if their own and their colleagues' intuitions point to a certain conclusion about a concept, that's all the evidence needed. If discerning judges agree in matters of concept application, then other judges would make the same assessment. The empirical work of Weinberg, Nichols and Stich (2001), however, raises doubts about this. And we all know from even casual philosophical discussion that philosophers don't always share one another's intuitions. Moreover, intuitive disagreement is probably underreported in the literature, because when philosophers publish their work they typically avoid examples they know have elicited conflicting intuitions among their colleagues. So the extent of disputed intuitions may be greater than philosophers officially acknowledge, and this may challenge the hope of identifying unique, socially shared concepts.

To safeguard some sort of supra-individual conception of concepts, there are other ways to proceed. One possibility is not to place the personal concepts of all individuals on a par, but to privilege some of them. How might this be done? There are several possibilities, some appealing to metaphysics and some to language. An appeal to metaphysics might return us to the natural kinds approach. Concepts that correspond to natural kinds should be privileged, those that don't, shouldn't. The problem here is that it's doubtful that every target of philosophical analysis has a corresponding natural kind. Take knowledge again as an example. A popular view in contemporary epistemology (with which I have much sympathy) is that knowledge has an important context-sensitive dimension. The exact standard for knowledge varies from context to context. Since it seems unlikely that natural kinds have contextually variable dimensions, this renders it dubious that any natural kind corresponds to one of our ordinary concepts of knowledge.

A more promising approach is to recast the entire discussion in terms of language. Concepts are the meanings of (predicative) words or phrases (Jackson 1998: 33–34). The correct public concept of knowledge is the meaning of "know". Many people who use the word "know" and its cognates may not have a full or accurate grasp of its meaning. Their intuitions should be ignored or marginalized when we try to fix the extension and intension of the term. Only *expert* intuitions should be consulted. This is

a natural line of development of Putnam's (1975) theme that meanings are determined by a division of linguistic labor in which experts play a central role.[4]

I hesitate to go down this road for two reasons. First, the idea of a division of linguistic labor, in which deference to linguistic experts holds sway, makes most sense for technical terms that aren't mastered by ordinary users of the language. Clearly, it would be a mistake for philosophical theorists to rely on the classification intuitions of users with inadequate mastery of the meanings of the words. However, concepts expressed by technical terms are not the chief concern of philosophical analysis. Philosophical analysis is mainly interested in common concepts, ones that underpin our folk metaphysics, our folk epistemology, our folk ethics, and so forth. I don't say this is *all* that philosophy is or should be concerned with. But when philosophers engage in analysis, folk concepts are what preoccupy them (Jackson, 1998). In this terrain, there isn't any significant expert/novice divide. Thus, if there are still differences in personal concepts associated with a single word, the differences cannot be resolved by appeal to (semantic) experts.

Second, there is a general problem with any attempt to configure the conceptual analysis enterprise in purely linguistic terms. Many of our most important folk-ontological concepts, I submit, are prior to and below the level of natural language. For instance, our unity criteria for physical objects fix the contours of single whole objects without recourse to predicates of natural language. They are independent of particular linguistic sortals, as illustrated by our ability to visually pick out a unitary physical object without yet deciding what *kind* of object it is. ("It's a bird, it's a plane, no, it's Superman!") Indeed, deployment of such criteria is a prerequisite for children to acquire mastery of verbal sortals. Children must already pick out unified physical objects in order to learn (at least with approximate accuracy) what adults refer to by such sortals as "rabbit", "cup", "tree", "toy", and so on (Bloom 2000). Evidently, the concept of a whole physical object is an important one for folk metaphysics to analyze. Thus, it would be a mistake to equate the domain of *conceptual* analysis with the domain of *linguistic* analysis.

I conclude that there is no satisfactory way to promote a public or community-wide conception of concepts to the primary, or central, position in

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4. Terence Horgan and colleagues develop a semantic approach to application intuitions in which semantic competence plays a prominent role (Graham and Horgan 1998; Henderson and Horgan 2001).

the project of conceptual analysis. From an epistemic standpoint, certainly, it is best to focus on the personal psychological conception of concepts as the basic starting point, and view the public conception of concepts as derivative from that one in the indicated fashion.

8. *Are intuition-based beliefs justified a priori?*

Defenders of intuition-driven methodology hold that intuitions provide evidence, or warrant, for classification propositions of interest to philosophers. What kind of warrant is this? The warrant in question is commonly held to be of the a priori variety. Intuition, after all, is a traditional hallmark of rationalism, an oft-mentioned source of a priori warrant. Is this something I am prepared to accept? Isn't my purpose, in this and related papers, to show how the evidence-conferring power of intuitions fits within a naturalistic perspective in epistemology? How can a priori warrant be reconciled with epistemological naturalism?

A first reply is that, in my view, there is no incompatibility between naturalism and a priori warrant. True, many contemporary naturalists, following Quine, wholly reject the a priori. But I see no necessity for this position. My favored kind of epistemological naturalism holds that warrant, or justification, arises from, or supervenes on, psychological processes that are causally responsible for belief (Goldman 1986, 1994). The question, then, is whether there are kinds of psychological processes that merit the label "a priori" and are capable of conferring justification. It seems plausible that there are such processes. The processes of mathematical and logical reasoning are salient candidates for such processes. They are processes of pure ratiocination, which is the hallmark of the a priori. So I see no reason why epistemic naturalism cannot cheerfully countenance a priori warrant (Goldman 1999).[5]

It is an additional question, however, whether arriving at classification intuitions is a species of a priori process, and whether it gives rise to belief that is warranted a priori. This must be examined carefully. We must first distinguish between first- and third-person uses of application intuitions to draw conclusions about concepts. Start with the third-person perspective on application intuitions.

5. A main theme of naturalistic epistemology is that the project of *epistemology* is not a (purely) a priori project. But it doesn't follow from this that there is no a priori warrant at all.

Concept-analyzing philosophers seek the intuitions of others as well their own. Third-person conceptual investigation can readily be interpreted as a proto-scientific, quasi-experimental enterprise, the aim of which is to reveal the contents of category-representing states. Under this quasi-experimental construal, each act of soliciting and receiving an application judgment from a respondent may be considered a complex experimental procedure. The experimenter presents a subject with two verbal stimuli: a description of an example and an instruction to classify the example as either an instance or a non-instance of a specified concept or predicate. The subject then makes a verbal response to these stimuli, which is taken to express an application intuition. This intuition is taken as a datum—analogous to a meter reading—for use in testing hypotheses about the content of the concept in the subject's head. From the point of view of the experimenter, the philosopher engaged in conceptual analysis directed at another person, the evidence is distinctly observational, and hence empirical. The warrant he acquires for any belief about the subject's concept is empirical warrant.

What about first-person cases, where a philosopher consults his own intuitions? This is where a priori warrant looks most promising. In consulting one's own intuition, one makes no observation, at least no perceptual observation. Does this suffice to establish that any warrant based on the intuition is a priori warrant? No. Although the inference from non-observational warrant to a priori warrant is often made, I think it's a mistake. Some sources of warrant are neither perceptual nor a priori. One example is introspection; a second is memory. Introspection-based warrant about one's current mental states is not a priori warrant; and memory-based warrant about episodes in one's past is not a priori warrant. Since some sources of warrant are neither perceptual nor a priori, application intuition might be another such source.

Indeed, the process of generating classification intuitions has more in common with memory retrieval than with purely intellectual thought or ratiocination, the core of the a priori. The generation of classification intuitions involves the accessing of a cognitive structure that somehow encodes a representation of a category. Of the various sources mentioned above, this most resembles memory, which is the accessing of a cognitive structure that somehow encodes a representation of a past episode. Thus, although I am perfectly willing to allow that application intuitions confer warrant, I don't agree that the type of warrant they confer is a priori warrant.

9. *Kornblith's critique of "détente"*

In this final section I briefly respond to Hilary Kornblith's critique of my approach as presented in earlier papers. Kornblith (this volume) argues that the "détente" I offer between methodological naturalism and the method of appeals to intuition just won't work. There are three strands to his argument. The first concerns the question of whose concepts philosophers should analyze, and whether intuitions should be uncontaminated by theory (i.e., as Kornblith interprets it, whether the preferred concepts should be pre-theoretical). The second concerns the question of whether there is any point to the project of studying commonsense epistemic concepts as a precursor to the study of scientific epistemology. I have defended the value of studying commonsense concepts, as a first stage of philosophizing. Kornblith disputes its importance. Third, Kornblith claims that standard philosophical analysis is committed to the thesis that concepts are mentally represented as necessary and sufficient conditions, the so-called "classical" view of concepts. This view, Kornblith tells us, has been refuted by empirical psychology. So here is a sharp conflict between empirical findings and traditional philosophical methodology. How can I hope to achieve a détente between empirical psychology and traditional philosophical methodology when the two approaches conflict so sharply?

On the first point, Kornblith argues against the view that we should study just the intuitions and concepts of the folk. On the contrary, he urges, the theory-informed intuitions of thoughtful philosophers should count for more than the intuitions of the folk (who have given no systematic thought to a philosophical topic). Furthermore, in contrast to the methodological precept that urges suspicion of theory-contaminated intuitions, Kornblith says that theory-informedness is a good thing.

The problem with this argument is that two entirely different relationships are being conflated between theories and concepts (or theories and intuitions). A theory can be related to a concept either by being embedded *in* the concept or by being a theory *of* the concept. A theory *of* a concept says that the concept has such-and-such content. A theory embedded *in* a concept isn't about the concept at all; it's about some other set of phenomena. The intuitional methodology I preach only says that one should avoid intuitions that are influenced by a theory *of* the target concept. Influence by such a theory can prevent the target from issuing a "normal" response to an example, a response that expresses the real content of the concept.

21

The methodologist's desire to avoid theory-contaminated intuitions should not be confused with a desire to avoid intuitions concerning theory-embedded concepts. There is nothing undesirable about theory-embedded concepts. I part company with Kornblith when he suggests that theory-embedded concepts are *superior* to theory-free concepts, because there are all sorts of theories. A concept that embeds a bad theory is of dubious worth. So I don't share Kornblith's preference for consulting philosophers' intuitions simply because their concepts embed theories more than folk concepts do. The crucial point, however, is the distinction between a methodological stricture against theory-contaminated intuitions and a possible stricture against theory-embedded concepts. I endorse only the former.

Kornblith's second criticism takes issue with my endorsing the study of folk epistemic concepts as a helpful precursor to the study of scientific epistemology. This endorsement was predicated on the idea that we must first identify the features of folk epistemology in order to figure out how it might be transcended by scientific epistemology, while ensuring that the latter project is continuous with the former. Here is an illustration of what I had in mind. Examining folk epistemic concepts should reveal how truth (true belief) is a primary basis of epistemic evaluation and epistemic achievement. This is indicated, for example, by the truth-condition on knowledge and the reliability desideratum associated with justifiedness. When moving from folk epistemology to scientific epistemology, we should retain the concern with truth-related properties of methods and practices. We should try to make them more reliable than our existing practices. If we never studied folk epistemic concepts, or studied them without proper understanding, this desideratum might elude us. It has indeed eluded postmodernists and (many) sociologists of science, who spurn the activity of conceptual analysis applied to concepts like knowledge or justification. They preach a kind of reformed or purified epistemic regime that ignores truth altogether. This radical and unfortunate detour from traditional epistemological concerns could be avoided by not abandoning folk epistemic notions and not neglecting the important features they highlight, such as truth.

Kornblith's third criticism is that a serious respect for the findings of cognitive science is incompatible with traditional conceptual analysis. I cannot advocate both, as I appear to do. Traditional analysis assumes that concepts are represented in terms of necessary and sufficient conditions, whereas cognitive science tells us that concepts take quite a different form

from this classical one. Kornblith urges us to heed the teaching of cognitive science and abandon traditional conceptual analysis.

I deny that traditional analysis is committed to the thesis that concepts (in the psychological sense) are mentally represented by features that are individually necessary and jointly sufficient. In fact, in two previous papers (Goldman 1992; Goldman and Pust 1998: 193–194) I have specifically recommended the exemplar-based approach that Kornblith also calls to our attention. The method of consulting intuitions about cases places no constraint on the psychological format of concept representations. *Any* hypothesis about concept representations that correctly predicts "observed" classification intuitions is tenable and welcome. Intuition-driven methodology imposes no requirement that hypotheses must posit a classical format for concept representation. True, in formulating the content of a concept representation, philosophers have customarily adopted the format of necessary and sufficient conditions, but I see nothing essential about that practice. For example, a recursive format could be adopted instead, using base clauses, recursive clauses, and a closure clause. In any case, exemplar based data-structures, paired with a set of similarity operations, might well yield classification judgments that can be captured in terms of necessary and sufficient conditions. (The conditions might involve a rather tedious set of disjunctions of conjunctions.) So the necessary-and-sufficient-conditions format for expressing a concept's content is neutral with respect to the psychological "syntax" by means of which the concept is psychologically represented (and processed).

Finally, I disagree with Kornblith's claim that commitment to a necessary and sufficient condition style of analysis biases philosophers toward unrealistically elegant or "pretty" analyses and toward dismissal of intuitions that shouldn't be dismissed. He criticizes philosophers, for example, for trying to explain away data that seem to show that knowledge can be false, by appeal to examples like "Most of what the experts know turns out not to be true". Admittedly, epistemologists commonly seek an alternative explanation of such intuitively acceptable utterances, an explanation that explains away the implication of false knowledge. But I see nothing wrong with this. It is plausible to explain such cases by saying that our speech often describes direct or indirect discourse, or propositions that are objects of propositional attitudes, while omitting overt quotation marks or attitudinal operators. In the present case, the utterance probably means something like this: "Most of what so-called experts credit themselves

with knowing, or are credited by others with knowing, turns out to be false". Here's another case (due to Richard Feldman, 2003: 13) of a (true) sentence that apparently implies the existence of false knowledge. You are reading a mystery story in which all the clues, until the last chapter, point toward the butler. Only at the end do you learn that the accountant did it. After finishing the book you say, "I knew all along that the butler did it, but then it turned out that he didn't". Pursuing the explanatory scheme suggested above, one might paraphrase the sentence as follows: "All along I was prepared to say, 'I know that the butler did it', but then it turned out that he didn't". This is a good explanation of how the sentence is understood, and it doesn't imply the falsity of what was known. This simple explanation of an apparent departure from the rule that knowledge is true looks like a perfectly good maneuver. It offers a general principle of language use that has considerable appeal and makes sense of the indicated utterances. It doesn't look implausibly ad hoc, and certainly not driven by an *unreasonable* commitment to necessary-and-sufficient-conditions-style analyses.

So, to summarize this last section, Kornblith hasn't given us good reason to think that taking cognitive science seriously forces us to abandon the intuitional methodology of conceptual analysis, at least if this methodology is understood in the liberal way I have sketched.

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