

Intuitions, Counter-Examples, and Experimental Philosophy

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Abstract Practitioners of the new ‘experimental philosophy’ have collected data that appear to show that some philosophical intuitions are culturally variable. Many experimental philosophers take this to pose a problem for a more traditional, ‘armchair’ style of philosophizing. It is argued that this is a mistake that derives from a false assumption about the character of philosophical methods; neither philosophy nor its methods have anything to fear from cultural variability in philosophical intuitions.

Some of the most striking results from the new ‘experimental philosophy’ movement are those that appear to reveal significant variability, along cultural and other dimensions, in peoples’ philosophical intuitions. For example, the experimental philosophers, Weinberg et al. (2001), conducted cross-cultural empirical studies on the Gettier intuition and claim to have found that significant majorities of East Asian and Indian subjects will intuit that an agent in a Gettier case ‘really knows’, as opposed to ‘only believes’, the relevant proposition. A different group of experimental philosophers, Machery et al. (2004), undertook cross-cultural experiments on intuitions about reference, and claim to have discovered that East Asians’ intuitions strongly favor a descriptivist theory of reference for proper names, while Westerners’ intuitions equally strongly support Kripke’s (1980) ‘causal-historical’ alternative.

This variability, if it really exists, is interesting in itself, and cries out for some explanation. But what, if anything, does it tell us about philosophical method? Is cross-cultural variability in intuitions something that Gettier and Kripke, for example, should worry about? Some experimental philosophers say that it is. In fact, both groups of experimental philosophers mentioned above use their experimental results to challenge the more traditional, ‘armchair’ method of

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philosophizing practiced by Gettier and Kripke. I argue here for the opposing view: Variability in philosophical intuitions poses no threat to philosophy.

My argument begins with the observation that it is common, even outside of experimental philosophy circles, to misrepresent arguments in philosophy as *depending on* the intuitiveness, or counterintuitiveness, of some proposition or other. For example, many philosophers would accept (G) as an accurate characterization of the way in which Gettier is supposed to have refuted the justified true belief (JTB) theory of knowledge:

(G): Gettier refuted the JTB theory by presenting cases in which a subject has a justified true belief that p , but it is *intuitive* (or *intuitively true*) that the subject does not know that p .

And many philosophers would accept (K) as an accurate characterization of the way in which Kripke is supposed to have refuted the descriptivist theory of reference:

(K) Kripke refuted the descriptivist theory by presenting cases in which it is *intuitive* (or *intuitively true*) that some speaker uses a proper name, N , to refer to x even though the definite descriptions the speaker associates with N do not denote x .

To the extent that (G) suggests that Gettier's anti-JTB argument *requires* that we find it intuitive that the agents in his cases fail to know despite justifiably and truly believing, the characterization is a misrepresentation. Similarly, to the extent that (K) suggests that Kripke has refuted descriptivism *only if* his cases are intuitive counterexamples to descriptivism, it too is a misrepresentation. Gettier refuted the JTB theory, if he did, and Kripke refuted descriptivism, if he did, by presenting counterexamples, full stop. Whether these counterexamples are intuitive for anyone is a separate, and purely psychological, matter.

Talk of theories and their alleged counterexamples allows a useful framing of the issue, for it is precisely by failing to keep questions about whether an alleged counterexample is *genuine* separate from questions about whether an alleged counterexample is *intuitive* that allows experimental philosophy to gain a foothold. The experimental philosophers have solid seeming evidence that what may be intuitive to Westerners is not intuitive to other cultural groups. They therefore suppose themselves to have evidence that, even by philosophy's own standards, Gettier's alleged counterexamples to the JTB theory of knowledge, or Kripke's alleged counterexamples to the descriptivist theory of reference, are not genuine. But this mistakes intuitiveness for genuineness.

The mistake may be an innocent one, since misleading characterizations of philosophical arguments such as (G) and (K) abound, even within traditional philosophy itself. And appeals to intuition, or 'what we would say' about hypothetical cases and thought experiments, appear with great frequency directly in various philosophical arguments, without an especially clear indication of the argumentative role these appeals are meant to play. In fact, there are plenty of cases in which it looks for all the world as though a philosopher is inferring p (not- p), from the fact that p is intuitive (counterintuitive) or represents (fails to represent) 'what we would say' about a case. How many times have we heard that a philosophical theory must be rejected because it has 'counterintuitive consequences'? Too many, it seems to me; theories must be rejected if they have

false consequences, but true theories might have consequences that strike us as false without actually being false.

So, experimental philosophers should not be blamed for supposing that many philosophical arguments depend on empirical speculation about peoples' intuitions. How can they be blamed when as eminent a philosopher as Frank Jackson advocates 'doing serious opinion polls on people's responses to various [philosophical] cases' in order to confirm or disconfirm this or that 'conceptual analysis' (Jackson 2000, 36-7)? Whether they are to blame for it or not, however, it is a mistake. Opinion polls won't settle any philosophical questions for us and, Jackson aside, it is doubtful that many philosophers would, on carefully considering the matter, say that they could.

Earlier, I said that (G) and (K) are misrepresentations because they suggest that the arguments they are meant to characterize depend on the intuitiveness of certain counterexamples. I claimed that the arguments instead depend only on whether the counterexamples are genuine, not on whether they are intuitive. These points deserve some elaboration.

Both the JTB theory of knowledge and the descriptivist theory of reference for proper names entail certain generalizations. The JTB theory entails the generalization that, for every subject, *S*, and every proposition, *p*, if *S* justifiably and truly believes that *p*, then *S* knows that *p*. The descriptivist theory of reference implies the generalization that, for every proper name, *N*, *N* refers to the denotation of the definite descriptions users of *N* associate with *N*. Gettier and Kripke argue against these generalizations by describing counterexamples, by which I mean possible cases that falsify the generalizations.¹ The counterexamples are quite well known, but since I am concerned to show that they do not depend on what is or is not intuitive, it will be useful to rehearse a pair of them here. Here is Gettier's presentation of his famous *10 coins-case* counterexample to the JTB theory.

10 coins-case

Suppose that Smith and Jones have applied for a certain job. And suppose that Smith has strong evidence for the following conjunctive proposition:

d. Jones is the man who will get the job, and Jones has ten coins in his pocket.

Smith's evidence for (d) might be that the president of the company assured him that Jones would in the end be selected, and that he, Smith, had counted the coins in Jones's pocket 10 min ago. Proposition (d) entails:

e. The man who will get the job has ten coins in his pocket.

¹ I take it that this is a standard understanding of 'counterexample'. However, in a recent paper defending the JTB theory, Brian Weatherson (2003) defines 'counterexample' in the following way: 'Let us say that a counterexample to the theory that all *F*s are *G*s is a possible situation such that *most people have an intuition* that some particular thing in the story is an *F* but not a *G*' (Weatherson 2003, 2; Italics added). Weatherson may use words as he pleases, of course, but his definition runs together precisely the two things I think need separating, namely (a) the issue of whether a possible case genuinely falsifies a theory (I call such cases 'counterexamples,' or 'genuine counterexamples'.) and (b) the issue of whether 'most people have an intuition' that a case falsifies a theory. Note that on Weatherson's definition there may be counterexamples to a theory even if the theory is true. This makes Weatherson's initially surprising claim that the JTB theory may be true even in the face of Gettier's counterexamples rather less surprising.

Let us suppose that Smith sees the entailment from (d) to (e), and accepts (e) on the grounds of (d), for which he has strong evidence. In this case, Smith is clearly justified in believing that (e) is true.

But imagine, further, that unknown to Smith, he himself, not Jones, will get the job. And, also, unknown to Smith, he himself has ten coins in his pocket. Proposition (e) is then true, though proposition (d), from which Smith inferred (e), is false. In our example, then, all of the following are true: (i) (e) is true, (ii) Smith believes that (e) is true, and (iii) Smith is justified in believing that (e) is true. But it is equally clear that Smith does not *know* that (e) is true; for (e) is true in virtue of the number of coins in Smith's pocket, while Smith does not know how many coins are in Smith's pocket, and bases his belief in (e) on a count of the coins in Jones's pocket, whom he falsely believes to be the man who will get the job (Gettier 1963, 122).

And here is Kripke's presentation of his equally famous *Gödel-case* counterexample to descriptivism:

Gödel-case

Let's take a simple case. In the case of Gödel that's practically the only thing many people have heard about him—that he discovered the incompleteness of arithmetic. Does it follow that whoever discovered the incompleteness of arithmetic is the referent of 'Gödel'?

Imagine the following blatantly fictional situation. (I hope Professor Gödel is not present.) Suppose that Gödel was not in fact the author of this theorem. A man named 'Schmidt', whose body was found in Vienna under mysterious circumstances many years ago, actually did the work in question. His friend Gödel somehow got hold of the manuscript and it was thereafter attributed to Gödel. On the view in question, then, when our ordinary man uses the name 'Gödel', he really means to refer to Schmidt, because Schmidt is the unique person satisfying the description, 'the man who discovered the incompleteness of arithmetic'. Of course you might try changing it to 'the man who *published* the discovery of the incompleteness of arithmetic'. By changing the story a little further one can make even this formulation false. Anyway, most people might not even know whether the thing was published or got around by word of mouth. Let's stick to 'the man who discovered the incompleteness of arithmetic'. So, since the man who discovered the incompleteness of arithmetic is in fact Schmidt, we, when we talk about 'Gödel', are in fact always referring to Schmidt. But it seems to me that we are not. We simply are not (Kripke 1980, 83-84).

Notice that *there is no mention of intuitions or the intuitiveness of any proposition in either presentation*. Gettier, on the question of whether Smith knows (e), says that, although it is clear that Smith justifiably and truly believes (e), 'it is equally clear that Smith does not *know* that (e) is true' (Gettier 1963, 122; Italics in original). He does not say that it is *intuitive* that Smith does not know; he says straight out, and emphatically, that Smith does not know. Likewise, Kripke insists that, in the circumstances he imagines in his story about Gödel and Schmidt, we 'simply are not' referring to Schmidt when we talk about 'Gödel' (Kripke 1980, 83). It is not that

it is *intuitive* that we are not talking about Schmidt, it is that we are not talking about Schmidt, period.² Of course, Gettier's and Kripke's counterexamples are intuitive counterexamples for at least some readers, but this is a logically inessential feature. The *10 coins-case* qualifies as a refutation of the JTB theory just in case it is true that Smith does not know (e) despite justifiably and truly believing it. Similarly, the *Gödel-case* refutes descriptivism just in case it is true that, in the imagined circumstances, the uses of 'Gödel' refer to Gödel instead of Schmidt. If these things are true, the counterexamples are genuine, regardless of whether anyone finds them intuitive.

Abstracting away from the details of Gettier's and Kripke's arguments, the form of each can be represented as the conclusion that a generalization is false via a premise asserting that there is a counterexample:

The Form of the Arguments:

- (1) There is an *F* that is not a *G*.
- (2) Hence, not all *F*s are *G*s.

The experimental philosopher critics, and perhaps a number of more traditional philosophers as well, think there is a missing premise in this representation of the arguments' form. According to them, (1) is supposed to follow from a premise asserting that the counterexample is intuitive. So, according to them, the arguments would be better represented by the following:

The Alleged Form of the Arguments:

- (0) It is intuitive that there is an *F* that is not a *G*.
- (1) So, there is an *F* that is not a *G*.
- (2) Hence, not all *F*s are *G*s.

² An anonymous referee (this journal) reminds me that, in *Naming and Necessity*, Kripke writes, '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 about anything, ultimately speaking' (Kripke 1980, 42). So, am I wrong to insist that, even by Kripke's own lights, the *Gödel-case* argument against descriptivism does not depend on treating intuitions as evidence? Firstly, even if I am wrong to insist on this, the important issue, as I make clear later in the main text, is not whether *Kripke thinks* that intuitions about the *Gödel-case* argument matter crucially as evidence, but instead whether they really do matter in that way. Secondly, the surrounding context of the quote makes it clear, I think, that Kripke is *not* endorsing an intuitions-as-evidence method for philosophy in general, or for evaluating counterexamples in particular. In the surrounding text, Kripke is responding to a skeptic who doubts that the *philosophical* distinction between *necessary* and *contingent* properties overlaps with any distinction possessed by *non-philosophers*. Kripke maintains that it does, and then goes on, in the quote above, to respond to a different sort of skeptic, one who doubts that that kind of overlap is evidence for, specifically, the *meaningfulness* of assertions involving the philosophical notions. So, the sorts of things that have 'intuitive content' are not judgments about hypothetical scenarios, they are, instead, philosophical distinctions and notions. And their having 'intuitive content' is a matter of how closely these match up with non-philosophical distinctions and notions. Lastly, closeness of match, Kripke says, is evidence only that the philosophical notions may be used to make meaningful assertions. In fact, Kripke explicitly puts aside the question of whether 'there are any nontrivial necessary properties', and asks us to focus on just 'the meaningfulness of the notion' (Kripke 1980, 42-43). Clearly, we cannot infer from any of this that Kripke would accept that intuitions about hypothetical cases are essential evidence about what is true concerning them. The quote with which I began may seem to suggest that there is at least an implicit appeal to intuitions in Kripke's presentation of the *Gödel-case*. Closer inspection of its surrounding context reveals that this is not so.

Representing the arguments this way makes it appear imperative that we find out whether the counterexamples really are intuitive, and that, in turn, provides the experimental philosophers' empirical studies with their point. But what justifies the view that (0) is required by the best representation of the arguments' form? From the way Gettier and Kripke actually express themselves, it appears that the simpler '(1), therefore (2)' representation (*The Form of the Arguments*, as opposed to *The Alleged Form of the Arguments*) is the more accurate one.

Taking (1) to follow somehow from (0) is to treat the intuition that there is an *F* that is not a *G* as *evidence* that there is an *F* that is not a *G*. Some metaphilosophies do treat intuitions as evidence.³ However, whether or not intuitions are evidence, it is fairly clear that the cogency of various traditional philosophical arguments does not *require* them to be. Gettier's and Kripke's arguments—paradigmatic traditional philosophical arguments, I take it—do not implicitly assume that intuitions are evidence. Intuitions play no evidential role in these arguments. Since a reasonable conclusion to draw from the variability in intuitions uncovered by the experimental philosophers is that at least some philosophical intuitions are not evidence for their contents⁴, it should come as a relief to philosophers that their arguments do not take them to be and do not *need* to take them to be. Gettier and Kripke may have refuted their target philosophical theories even if, as the data collected by the experimental philosophers perhaps shows, the *10 coins-case* and the *Gödel-case* are not intuitive counterexamples to, respectively, the JTB theory of knowledge and the descriptivist theory of reference.

It is time to respond to the most obvious objection to my proposal that Gettier's and Kripke's arguments don't depend on the intuitiveness of their counterexamples, namely that, if it is not the intuitiveness of the counterexamples that gives us reason to accept that the counterexamples are genuine, what *does* gives us that reason?

The objection appears to assume that the genuineness of the counterexamples must be *inferred* from something else. It is not clear that this is a fair assumption, however. Is there something obviously wrong with the idea that one might know *directly* that, for example, Smith does not know (e): *that the man who will get the job has 10 coins in his pocket?* Clearly, some people, the majority of East Asians in Weinberg et al.'s poll, for example, don't know it directly, since they don't even believe it.⁵ It doesn't follow, however, that those of us who *do* believe it fail to know it directly.

As far as I can see, there is nothing to prevent philosophers from maintaining that, on at least some occasions, the belief that a counterexample is genuine qualifies as direct, noninferential knowledge that the counterexample is genuine. The intuition that *p* would not be *evidence* for *p*, on this metaphilosophical picture, but would instead be a *manifestation* of one's *direct knowledge* that *p*. Implicit allegiance to this sort of picture perhaps partly explains the frequency with which

³ See Bealer 1998, Goldman and Pust 1998, and Pust 2000.

⁴ I say 'reasonable' not 'clearly correct,' since it is possible for those who accept the view that intuitions are evidence to argue that variability in intuitions of the kind uncovered by the experimental philosophers does not pose an insurmountable difficulty. Perhaps only *considered* or *reflective* intuitions are evidence, or perhaps it is only the intuitions of the experts (i.e. the philosophers) that matter.

⁵ This is somewhat inaccurate, since Weinberg et al. (2001) did not use the *10 coins-case* as a test case in their studies. (Instead, they used a variation on Lehrer's (1965) 'Nogot/Ford' case.)

philosophers appeal to intuitions in their theorizing, without any attempt to determine whether the relevant propositions are intuitive for anyone else. When a philosopher claims that p is intuitive, we should not understand this as, 'Most people would pretheoretically judge that p .' After all, if it were understood in that way, how could anyone fail to recognize the relevance opinion polls might have to the claim? Instead, the claim is best interpreted as, 'I know p directly, without inference.'

The point may be less than perfectly clear; am I trying to distinguish 'knowing directly' from 'intuiting'? What could this difference consist in when the judgment concerns a hypothetical philosophical case? But I do not mean to be distinguishing 'knowing directly' from 'intuiting.' Knowing directly that, say, 'Gödel' does not refer to Schmidt in the *Gödel-case* is just intuiting that that it doesn't. Indeed, I take it as given that, if we know that 'Gödel' does not refer to Schmidt in the *Gödel-case*, we know this via intuition and, hence, directly. However, in asserting this, I mean to assert only that the *causal source* of the judgment is intuition. Intuition may be the causal source of the judgment without being its *justificatory source*, and without the fact that the judgment is intuitive serving as a premise in an inference to the judgment's truth. This is an important distinction for understanding the role of intuition in philosophical methodology. Various arguments in philosophy—presentations of counterexamples to general philosophical theories, for example—may 'rely', or 'depend,' on intuition, but only causally, not inferentially or evidentially.

However, drawing this distinction is not quite enough to answer a skeptic who demands evidence for taking various counterexamples to be genuine. Compare perceptual knowledge. Perhaps we sometimes have direct perceptual knowledge, but against a skeptic who wants to know how we have any perceptual knowledge at all, it seems evasive to answer that some perceptual knowledge is direct. That is not an answer to the relevant how-question. The relevant how-question is not: How is perceptual knowledge *arrived at*? Instead it is: How is it that perceptual beliefs ever 'add up' to perceptual knowledge? And to answer this question we must find some set of considerations from which it follows that perceptual beliefs sometimes qualify as knowledge, even if we do not typically arrive at this knowledge via inference from those considerations.

Once the relevant how-question is on the table, however, it is fairly clear that there need be no *single*, fully general answer to it. The relevant how-question concerning counterexamples to philosophical theories is: How can we be sure, in any given case, that a counterexample is genuine? The answer foisted on philosophy by its experimental philosopher critics is that we know whether a counterexample is genuine by checking to see whether it is intuitive. I have already argued that this answer does not accurately reflect actual philosophical practice, since, in many cases (Gettier's and Kripke's, e.g.), a counterexample is presented with no explicit or implicit appeal to its intuitiveness. But the answer has a deeper flaw: It presupposes that there is some *one* way according to which we know whether a counterexample is genuine. There is no one way. Consider just the two examples we have looked at here. Surely, if we know that Gettier's counterexample is genuine, we know this on quite different grounds from the grounds on which we know, if we do, that Kripke's counterexample is genuine. In the former case, the grounds concern various

epistemic issues, while in the latter case they concern semantic ones. And in neither case do they concern who intuits what.

What *are* the differing grounds on which it has been argued that Gettier's and Kripke's counterexamples are genuine? Perhaps reminding ourselves of these differing grounds will help dispel the impression that the case for the genuineness of the counterexamples bottoms out in an appeal to what is or is not intuitive. Here, under the heading *K-Grounds*, are some of the reasons that have been offered for taking the *Gödel-case* to be a genuine counterexample to the descriptivist theory of reference. The second list, labeled *G-Grounds*, lists reasons that have been given for supposing the *10 coins-case* to be a genuine refutation of the JTB theory of knowledge. The striking fact about both lists is that neither cites any claim about what is or is not intuitive.

K-Grounds:

- (a) Kripke points out that the imaginary *Gödel-case* has real-life analogues. All that many of us 'know' about Peano is that he was the discoverer of certain axioms concerning the natural numbers. But it turns out that Dedekind discovered those axioms. If descriptivism is true, many of us have been referring all along to Dedekind with our uses of 'Peano'. But we have not been referring to Dedekind with those uses. We have been referring instead to Peano, *misattributing* to him the discovery of the axioms. This is not simply a further putative counterexample; it strengthens the claim that the *Gödel-case* is a counterexample by showing us that the way in which we *ought* to judge, with respect to the imaginary *Gödel-case*, should line up with the way in which we *correctly* judge about the real-life Peano case. (See Kripke 1980, 84–85.)
- (b) Kripke argues that the view that 'Gödel' refers to Schmidt—the prediction made by descriptivism concerning the *Gödel-case*—suggests a more general view to the effect that *one can never be mistaken* in uttering a sentence of the form '*N* is the *F*', when 'the *F*' denotes, and is a definite description one associates with '*N*', a proper name. But one *can* be mistaken in uttering 'Peano is the discoverer of the axioms', even if one associates 'the discoverer of the axioms' with 'Peano'. The falsity of this general view is evidence that Kripke is right in claiming that 'Gödel' refers to Gödel, not Schmidt, in the *Gödel-case*. (See Kripke 1980, 85n, 87.)
- (c) Kripke argues for an alternative account of the way in which 'Gödel' refers (the causal-historical account) which explains, Kripke thinks, why 'Gödel' refers to Gödel in the *Gödel-case*. The existence of a satisfying general theory of reference that predicts that 'Gödel' refers to Gödel in the *Gödel-case* counts in favor of the view that 'Gödel' refers to Gödel in the case. (See Kripke 1980, 91–93.)

G-Grounds:

- (a) As Gettier himself says in his original presentation of the *10 coins-case*, it is clear that Smith does not *know* that (e) is true; for (e) is true in virtue of the number of coins in Smith's pocket, while Smith does not know how many coins are in Smith's pocket, and bases his belief in (e) on a count of the coins in Jones's pocket, whom he falsely believes to be the man who will get the job (1963, 122).

The thought here, shared by many later commentators on Gettier's cases (See especially Goldman 1967.), is the thought that S 's justified true belief that p might fail to be knowledge if there is a disconnection between: (i) what causes S to believe p , and (ii) what makes S 's belief that p true. In the *10 coins-case*, it is the number of coins in *Jones's* pocket that is (partly) causally responsible for Smith's belief that the man who will get the job has 10 coins in his pocket. But that belief is true "in virtue of" the number of coins in *Smith's* pocket. According to Gettier, this disconnection between what causes Smith to believe and what makes Smith's belief true justifies the judgment that Smith does not know.

- (b) Some epistemologists (e.g. Unger (1968), Engel (1992), Pritchard (2005)) argue for the related idea that agents in (at least some) Gettier cases are only *luckily correct* in believing what they do, and that the presence of this sort of 'epistemic luck' explains why such agents lack knowledge. In the *10-coins case*, it is an accident (luck) that Smith is right in believing that the man who will get the job has 10 coins in his pocket, given that this belief is based partly on a count of the coins in the pocket of a man who, as it happens, is *not* the man who will get the job. This appears to be compelling grounds for concluding that Smith's belief does not add up to knowledge.
- (c) Other epistemologists (e.g. Lehrer and Paxson (1969)) claim that S 's justified true belief that p may fail to qualify as knowledge if there are "epistemic defeaters" to S 's justification. Roughly, q is defeater to S 's justification for p , if q is compelling evidence against p about which S is unaware. The presence of defeaters might explain Smith's lack of knowledge in the *10 coins-case*. If Smith were apprised of the fact that it was he who would get the job, not Jones, then Smith would no longer justifiably believe that the man who will get the job has 10 coins in his pocket. Smith's justification for his belief is thus defeated and, for this reason, arguably fails to count as knowledge.

The existence of the *K-Grounds* and *G-Grounds* ought to remove the suspicion that the case for taking Kripke's and Gettier's counterexamples to be genuine rests solely on an appeal to the counterexamples' intuitiveness. Neither list cites any appeal to intuitions or the intuitiveness of any principle or proposition. Still, some will say that the *K-Grounds* and *G-Grounds* don't do much to dispel the impression that, in building a case for the counterexamples' genuineness, there is going to be an appeal, *at some stage*, to the brute intuitiveness of *something*. For example, part (b) of the *K-Grounds* appeals to the falsity of a general principle apparently implied by the descriptivist theory of reference. But how do we know that this general principle is false? Perhaps we infer its falsity from the fact that its falsity is intuitive. If so, my attempt to show that there are grounds, which have nothing to do with the counterexample's intuitiveness, for taking the *Gödel-case* to be genuine only succeeds in *relocating* the appeal to intuition. Perhaps it is not a straight out appeal to intuition that grounds the view that the *Gödel-case* is a genuine counterexample, but, ultimately, the justification for some principle or proposition cited as part of those grounds is going to bottom out in such an appeal.

There is a whiff of something right in this reaction. It is true that the *K-Grounds* and *G-Grounds* appeal to various principles and propositions for which we may

demand further grounds. This is a general point about justification, however, not one that holds only of arguments for the genuineness of philosophical counterexamples. In general, if one argues for p on the basis of q , it may be fairly asked why q should be accepted. However, it is implausible to suppose that the last link of every justificatory chain must be a premise asserting the intuitiveness of some proposition or other. This too is a general point about justification: More often than not, justifications come to an end with premises that assert something other than that some proposition is intuitive for someone or some group of people. There is no obvious reason this general point should fail to hold of arguments for the genuineness of philosophical counterexamples. So, while the principles and propositions cited in the *K-Grounds* and *G-Grounds* are perhaps not justificatory rock bottom, we need not accept the idea that the rock bottom (if there is such) must consist of claims about what is and is not intuitive.

I can imagine experimental philosophers complaining that, although there *need be* no rock bottom appeal to intuitions in arguments for philosophical counterexamples, the fact is that there often *is* such an appeal. I have already conceded that there is a good deal of misleading talk about intuitions and their evidential role in philosophy. On the other hand, I take myself to have shown that Kripke's argument against descriptivism and Gettier's argument against the JTB theory do not depend at any stage on whether anything is intuitive for any person or group—and Kripke and Gettier do not even talk as if their arguments depended on this. Maybe there are clear cases in which a philosophical argument does bottom out in an appeal to what is or is not intuitive. We should ask, in such a case, whether the appeal to intuition clearly assumes that intuitions are evidence, or whether, instead, the philosopher presenting the argument merely means to signal that she has reached a premise she feels requires no justification.

In any case, the important point is that philosophers *need not* appeal to intuitions as evidence, regardless of whether they sometimes do. What difference would it have made if, contrary to fact, Kripke and Gettier had argued as experimental philosophy's caricature of them takes them to have argued? If Gettier, for example, had argued that the *10 coins-case* is a counterexample to the JTB theory simply and solely because it is an *intuitive* counterexample to that theory, then his premise would have been open to refutation-by-opinion-poll. Those of us convinced by the *10 coins-case*, however, presumably would have shrugged our shoulders at this. For, even if Gettier had offered bad reasons for supposing his counterexample to be genuine, this would not prevent us from casting about for better reasons, such as those mentioned in the *G-Grounds*.

The methodological conclusions drawn above apply to 'armchair' arguments quite generally, I think. I have been focusing on Gettier's and Kripke's arguments, but, as I have said, I take these arguments to be paradigmatic examples of the type. There is surely no obvious reason why the method of offering counterexamples to semantic or epistemological theories should be different in some crucial respect from the method of offering counterexamples to metaphysical or ethical theories. It would be peculiar if, while Kripke's counterexamples to descriptivism require no backing via an appeal to their intuitiveness, counterexamples to, say, utilitarianism do require it. No, if I am right, and the generalization-vs.-counterexample method we see in the work of Kripke and Gettier does not evidentially depend at any stage on what is

intuitive, then it is safe to assume that the method, as it appears in the work of others in different areas, does not evidentially depend on what is intuitive either.⁶

The role of intuitions in philosophical arguments has thus been greatly exaggerated, by experimental philosophers, certainly, but also, and perhaps equally, by more traditional philosophers themselves. Kripke and Gettier are innocent, I think, but there are others who seem to accept the inaccurate and strangely psychologistic view that, in philosophy, ‘it all comes down to intuitions.’ It almost never comes down to intuitions, if what this means is that philosophical arguments either do or must terminate with premises asserting that some person or group is in (or would be in, given the right prompts) the psychological state we would describe as *intuiting that something is so*. Instead, in philosophy, it all comes down to *arguments*, not intuitions, and very often these arguments terminate with premises asserting that some not-purely-psychological fact obtains. Gettier’s and Kripke’s arguments are cases in point.⁷

Sometimes, experimental philosophers present their empirical studies as a challenge not to philosophical theories of x (where x is knowledge, reference, or etc.) but instead as a challenge to theories of the *concept* of x . They claim that a good portion of philosophy involves ‘conceptual analysis,’ and that their empirical studies provide evidence for or against a variety of received conceptual analyses.⁸ Is there an important distinction here that I have overlooked? Perhaps experimental philosophers take more traditional philosophers to take intuitions as evidence, not for claims about the (nonconceptual) *world*, but instead for claims about the *application conditions of concepts*. Earlier, I mentioned in passing that Jackson advocates conducting opinion polls in order to test conceptual analyses. Is testing conceptual analyses an importantly different sort of activity from the activity of offering counterexamples to theories, and, if so, do experiments on philosophical intuitions have some evidential bearing on accounts of a concept’s application conditions?

The answer to both questions is, I think, ‘no.’ Consider the claim that Smith from the *10 coins-case* does not know (e): *that the man who will get the job has 10 coins in his pocket*. This is not a claim directly about concepts and their conditions of application. On the other hand, the claim is true if and only if the concept *knowledge* fails to apply to Smith and his relation to the proposition expressed by (e). Given this

⁶ Two anonymous referees (this journal) urge that Rawls’s (1971, 1993) account of how to select justified ‘principles of justice’ is an account that depends on taking moral intuitions about particular cases as evidence for moral theory. The picture is that, on the Rawlsian account, we start with our moral intuitions about particular cases on one side and we try to bring these into ‘reflective equilibrium’ with a variety of moral principles on the other. Though I am off my home turf here, and though Rawls himself seems to endorse a somewhat different picture, I will say that the account just described strikes me as very strange indeed. What we want, surely, are moral principles that don’t conflict with particular moral *truths*; conflicts with intuition will matter only if what is intuited is true. And how do we know whether our moral intuitions about particular cases are true? We know this by assessing arguments for or against their truth. In other words, in ethics, just as in epistemology and philosophy of language, philosophers need not appeal to intuitions as evidence, even if they, misleadingly, sometimes do.

⁷ Keep in mind that to deny that philosophy must treat intuitions as evidence is not to deny that intuition is the causal source of some philosophical knowledge.

⁸ Joshua Knobe’s widely read papers in experimental action theory (Knobe 2003a, b) concern, Knobe says, the *concept* of intentional action. Similarly, Shaun Nichols’s papers in experimental metaphysics (Nichols 2004 and Nichols 2006) are supposed to concern the *concept* of free action.

equivalence, the evidence, whatever it may be, for thinking that Smith does not know the proposition is going to be evidence for the claim about the concept *knowledge* failing to apply, and whatever fails to count as evidence for the former will fail to count as evidence for the latter. I have already argued that intuitions are not, and need not be, treated as evidence for the claim that Smith does not know (e). This argument applies equally to the view that intuitions are needed as evidence for the claim that the concept *knowledge* fails to apply to the relation between Smith and the proposition in question.

Of course, the existence of a counterexample, such as the *10 coins-case*, allows us to generalize. We can say, based on the existence of the case, that there are, possibly, instances of justified true belief that are not instances of knowledge, and hence that the concept *knowledge* does not coextend with the concept *justified true belief*. So reflection on hypothetical (and sometimes real) cases can reveal the contours of various concepts we find philosophically interesting and important. But it is a mistake to think that there is some separate stage of conceptual analysis, different in some significant way from theory construction and the consideration of putative counterexamples, at which intuitions might play some crucial evidential role. It is therefore also a mistake to think that intuition surveys and their results might pose a threat to this or that conceptual analysis. An intuition survey will not settle the question of whether Smith knows that the man who will get the job has 10 coins in his pocket. For identical reasons, such a survey will not settle the question of whether the concept *knowledge* applies to the relation between Smith and the proposition expressed by (e).

Since intuitions need not be regarded as evidence, either for theories of philosophically interesting phenomena, such as knowledge and reference themselves, or for analyses of the concepts of such phenomena, philosophers should not fret, if it turns out that different cultural groups have differing philosophical intuitions. However, in the circumstance in which the philosophical intuitions of two or more cultures clash, how do we decide who is right? Is it not imperious (and perhaps imperialistic) to assume that the intuitions of our own culture are correct?

If all we had to rely on in justifying our judgments about philosophical cases were the fact that those judgments are intuitive, cultural variability would put us in a real bind. We would have to admit that cultural groups with differing intuitions have just as much right to their judgments about cases as we do to ours.⁹ The intuitiveness of the judgments is not all we have to rely on, however. We can try to convince those who disagree with us by providing *justifications* for our intuitions. If our

⁹ Some philosophers, including Ernest Sosa (2007), William Lycan (2006), and an anonymous referee (this journal), have suggested that persistent cultural variation in philosophical intuitions may indicate mere ‘verbal disagreement’. (Nichols and Ulatowski (2007) take a similar line on the ‘Knobe Effect’, first reported in Knobe 2003a.) Perhaps, for example, ‘knowledge’ in an East Asian English speaker’s mouth expresses a different, though presumably related, concept than that expressed by ‘knowledge’ in a Western English speaker’s mouth. If so, then there may be no real conflict—no ‘substantive disagreement’—between East Asian English speakers who judge that agents in Gettier cases ‘really know’ and Western English speakers who judge that they don’t. I find this line implausible. It is of course *possible* that subtle conceptual differences are producing the apparent disagreement. But there is no evidence at all that this is in fact what is going on. People can and do have persistent disagreements about even putative necessary truths without this implying that they are operating with different concepts. Philosophers have such disagreements with one another all the time. Presumably, they are not, most of the time, ‘speaking past each other’.

justifications are good enough, we can say, without feeling guilty of cultural insensitivity, that we are right and they are wrong. My own view is that the *K-Grounds* and the *G-Grounds* provide a very strong case for thinking that the *Gödel* and *10 coins* cases are genuine counterexamples to the semantic and epistemological theories they are meant to challenge. If East Asians and Indians intuit otherwise, they ought, on reflection, and after being exposed to the *K-Grounds* and *G-Grounds*, to capitulate.¹⁰

What explains cross-cultural variability in philosophical intuitions, even if it is a mistake to think such variability poses a severe problem for traditional philosophizing? That is a good question—for a social scientist.¹¹

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¹⁰ The criticism of experimental philosophy offered in the main text differs from most other published criticisms. Most such criticisms *concede* that philosophy treats intuitions as evidence. Ernest Sosa (2007) and William Lycan (2006) concede this, and, in commentaries on Machery et al. 2004, Michael Devitt and Henry Jackman do so as well. These philosophers accept that philosophers treat intuitions as evidence, but they deny that cultural variation of the sort uncovered by Machery et al. (2004), or Weinberg et al. (2001), impugns philosophical method. Sosa and Lycan argue that cultural variation in philosophical intuitions (if it really exists—both Sosa and Lycan express doubts about whether the studies conducted to date really establish that there is such variation) may indicate only that different cultures have different *concepts of knowledge, reference*, or what have you. (See note 9, above.) Devitt (ms) and Jackman (2009) argue, among other things, that, in the case of semantic theories at least, it is the intuitions of semanticists, not ‘the folk’, which should matter. If I am right, these concessive replies concede something untrue; traditional philosophical method does not depend on treating intuitions as evidence, and experimental philosophers, along with a fair number of misguided traditionalists, are therefore operating with a flawed conception of philosophy. An exception is Timothy Williamson (2004, 2007), who argues, as I do, that philosophy does not depend evidentially on intuitions.

¹¹ *Philosophers* are surely not obligated to explain cultural variability in philosophical intuitions; they lack the proper training. Cultural anthropologists or social psychologists are best equipped to explain that sort of variability. Of course, philosophers are obligated to explain why certain philosophical intuitions are *false*, if they believe them to be. But explaining why a given philosophical intuition is false is different from explaining why it is held by some person or group, and only the former is a properly philosophical project.

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