

## Who needs intuitions?

Jonathan Ichikawa

ichikawa@gmail.com

Draft in Progress (7 September, 2008). Please do not cite.

**Abstract.** “Intuitions play critical evidential roles in philosophy.” This statement has air of truism about it; it is widely assumed, and occasionally argued, to be correct. It forms the basis of an influential series of challenges to traditional philosophical methodology; among these are a reliability challenge—‘how can we explain how our intuitions are reliable guides to philosophical truth?’—and the experimentalist critique—‘it is illegitimate to assume from the armchair that people really have the intuitions traditional methodology says they do; in fact, survey data indicates that they often don’t.’

I argue that this widespread assumption about the evidential role of intuitions is importantly ambiguous, and, in the sense that is relied upon for many such critiques, it is false. Philosophical evidence is not primarily psychological; traditional methodology does not require introspected premises of the form ‘I have the intuition that  $p$ ’. In this matter, I agree with recent work by Timothy Williamson.

I examine three attempts to recast the experimentalist critique in terms that do not rely upon this false assumption about traditional methodology: the concepts we happen to have are arbitrary in a way undermining traditional methodology; empirical research undermines our confidence in our abilities to discern philosophical truths; general worries about the epistemology of disagreement, combined with experimental results, undermine traditional methodology. I suggest that all of these reformulated challenges can be met.

Here is a familiar situation: some philosophers are considering whether some philosophical theory  $X$  is true. One of them argues, by invoking a thought experiment, that it is not. “Here is an imaginary case  $S$ ,” she says. “Intuitively, it is a case in which  $p$ . But according to theory  $X$ ,  $S$  is a case in which not- $p$ . So  $X$  is false.” In many cases, arguments of this form have been accepted, nearly universally, as sound, and have provided the basis for the mainstream rejection of the relevant theories. A particularly influential example is Gettier’s famous refutation of the theory that knowledge was identical to justified true belief; another is Putnam’s refutation of semantic internalism; a third is Kripke’s refutation of the descriptivist theory of names.

In the past decade, arguments of this sort of form have come under a particular sort of philosophical scrutiny: insofar as they rely on *philosophical intuitions*, it is natural to

wonder whether such intuitions are reliable guides to truth. The *experimentalist critique* of traditional methodology comprises the extensive recent literature challenging the invocation of intuitions in philosophy on empirical grounds. The challenge (one challenge, anyway—the movement is not at all homogenous) comes in the form of a body of empirical work designed to demonstrate that the folk do not always, or often enough, have the intuitions that philosophers say they do. Insofar as traditional philosophy relies on the alleged evidence that the folk have particular intuitions, this traditional methodology is put open to empirical question—and, in some cases, the experimentalist critique presses, it is severely undermined. It is my present project to examine some of these empirical challenges.

### **1. Pressing the Experimentalist Critique**

In their influential (2001), Jonathan Weinberg, Shaun Nichols, and Stephen Stich articulated the worries that characterized early versions of the experimentalist critique. This paper, which has important roots in Stich (1990), has inspired a broad new movement of experimentalist philosophy. I cannot, in this brief discussion, address the movement comprehensively; instead, I will focus primarily on the worries pressed in this original paper, and highlight a few connections to later work along the way. My goal, as in previous work (particularly Ichikawa and Jarvis forthcoming) is to vindicate something that is at least in the neighborhood of traditional methodology. I will focus, with Weinberg, Nichols and Stich—hereafter WNS—on questions about the methodology of epistemology; again, much of what I say, and what they say, should generalize.

A central challenge of WNS's worry about projects in the neighborhood of conceptual analysis is that they are ill-equipped to answer normative questions. Traditional epistemology, it is thought, is in the normative business: epistemology concerns what belief-forming faculties we *should* have, or what kinds of beliefs are *good* beliefs, or what kind of cognitive agent is a *rational* one. These are evaluative notions. But, the worry goes, the methodology of traditional epistemology has no resources to provide a satisfactory answer to such normative questions. Consider the thought experiment: we consider some imaginary scenario, then form an intuitive response: *the subject knows*, or *the subject is unjustified in believing*, or *that cognitive state is more epistemically valuable than this one*.

We start getting worried when we realize that intuitions like these are the products of minds that are heavily influenced by idiosyncratic features of our languages and societies. There are possible individuals and societies who employ different standards of evaluations and have systematically different epistemic intuitions than we do; indeed, tentative evidence is cited that suggests that that there are actual such individuals and societies. Such possible or actual standards are thought to pose a number of challenges to the viability of the traditional epistemic project. In this passage, WNS, citing Stich (1990), lay out the central challenge. 'Intuition Driven Romanticism' is their name for the family of epistemic methodologies that are the target for their critique.

There might be a group of people who reason and form beliefs in ways

that are significantly different from the way we do. Moreover, these people might also have epistemic intuitions that are significantly different from ours. More specifically, they might have epistemic intuitions which, when plugged into your favorite Intuition Driven Romantic black box yield the conclusion that *their* strategies of reasoning and belief formation lead to epistemic states that are rational (or justified, or of the sort that yield genuine knowledge—pick your favorite normative epistemic notion here). If this is right, then it looks like the IDR strategy for answering normative epistemic questions might sanction any of a wide variety of regulative and valuational norms. And that sounds like bad news for an advocate of the IDR strategy, since the strategy doesn't tell us what we really want to know. It doesn't tell us how we should go about the business of forming and revising our beliefs. One might, of course, insist that the normative principles that should be followed are the ones that are generated when we put *our* intuitions into the IDR black box. But it is less than obvious (to put it mildly) how this move could be defended. Why should we privilege our intuitions rather than the intuitions of some other group? (435)

They go on to present evidence suggesting that the hypothetical people they discuss are actual. I will have a bit to say about this data below. First, it is worthwhile to examine an assumption of this experimentalist critique.

## **2. Intuitions as Evidence?**

### *2.1. The target of the critique is methodology that relies on psychological inputs.*

A premise of the experimentalist critique is that traditional epistemology relies centrally on intuitions; that intuitions are, in some sense, the final arbiters in questions about epistemic properties, norms, and values. It is clear that this premise is intended—for without it, the specter of alternate intuitions is not obviously relevant. The proponents of the critique are explicit about this assumption. WNS write:

The family of strategies that we want to focus on all accord a central role to what we will call *epistemic intuitions*. Thus we will call this family of strategies *Intuition Driven Romanticism* (or IDR). As we use the notion, an epistemic intuition is simply a spontaneous judgment about the epistemic properties of some specific case—a judgment for which the person making the judgment may be able to offer no plausible justification. To count as an Intuition Driven Romantic strategy for discovering or testing epistemic norms, the following three conditions must be satisfied: (i) The strategy must take epistemic intuitions as data or input. (It can also exploit various other sorts of data.)... (432)

It is clear that WNS intend IDR to include a very broad part of traditional epistemology—including, for example, Gettier's famous argument that knowledge is not

justified true belief (452).

The assumption that intuitions serve as important evidence in traditional epistemology appears to be an innocuous one; this, presumably, is why it has received so little explicit defense. However, it is worth examining. The most explicit extended defense of the claim that intuitions are used as important evidence in epistemology of which I am aware is chapter 1 of Joel Pust's (2000) book, *Intuitions as Evidence*. Pust argues for the apparently-obvious claim in the obvious way: by pointing to myriad apparent examples of uses of intuitions as evidence in philosophical matters. He cites thought experiments about knowledge, justification, reference, moral rightness, justice, rationality, personal identity, and explanation—in each case, he points to an 'intuitive' judgment that plays a central role in the argument.

## 2.2. *Do philosophers rely on intuitions as evidence?*

There is a gap, however, between the observation that many philosophical arguments rely on premises that are 'intuitive' and the WNS assumption that standard philosophical methodology takes facts about intuitions—psychological facts—as evidence. I believe that once this distinction is articulated, we may understand the suggestion that intuitions are important evidence in traditional epistemology, or in philosophy at large, as ambiguous. Although it is plausibly true in one sense, this is not the one that is invoked in the WNS argument. I will state the two senses in question presently; first, by way of illustration, consider Pust's first example intended to show that intuitions are used as crucial evidence:

Here is a case (derived from Lehrer ...) from that massive literature:

[I] Nogot's Ford. Suppose your friend Nogot comes over to your house to show you the new Ford automobile he has just purchased. [standard Gettier story omitted] ... Do you *know* that a friend of yours owns a Ford?

Most philosophers take the fact that they have the intuition that S does not know that *p* in this case to show that S does not know that *p*. (5)

Pust closes here with a sociological claim: most philosophers take *the fact that they have a particular intuition* to demonstrate a philosophical thesis. He offers no defense of this empirical claim, apparently considering it obvious. It is certainly true that most philosophers take S not to know that *p* in this case; and it is also certainly true that most philosophers have the intuition that S does not know that *p* in this case. But I am inclined to doubt Pust's claim that most philosophers take the fact about their own mental states to *show* that the fact about S is true. Suppose they were asked to defend the judgment about S. The appropriate response would be to cite, for instance, the fact that S's belief that *p* was derived from a falsehood, or that he was lucky to have gotten his belief right. It is probably true that, upon sufficient questioning, they might exclaim, 'I just have an intuition!' But this is a way of ending the dialectical train of inquiry, not a serious attempt

to explicate the evidence.

Very shortly after the passage I quoted above is another bit from Pust that can provide insight into the mainstream diagnosis of intuitions as evidence:

The analysis of justified belief proceeds in exactly the same fashion. A theory is proposed ... and *it is tested by its ability to account for intuitive judgments* regarding the justifiedness or unjustifiedness of particular actual and hypothetical beliefs. That this is so is recognized by many philosophers who reflect on their practice. For example, the epistemologist John Pollock claims that in epistemological analysis:

[O]ur basic data concerns what inferences we would or would not be permitted to make under various circumstances, real or imaginary. This data concerns individual cases and our task as epistemologists is to construct a general theory that accommodates it.

(5, emphasis in original)

This strikes me as a remarkable passage. Pust cites Pollock as an example of an epistemologist who reflects on his own practice, and recognizes the crucial role that intuitions play in it; yet the quotation Pust selects does not include the word ‘intuition’ or any of its cognates. Indeed, Pollock is explicit: the basic data is the *acceptability of inferences*. Somehow, from this, Pust takes away the moral that Pollock recognizes that the basic data are intuitive judgments. What has happened?

### 2.3. ‘Intuitions are evidence’ is ambiguous.

The answer, I think, is that there is a crucial ambiguity in the suggestion that ‘intuitions are evidence’, and that Pust has allowed himself to slide between uses. I’ll suggest shortly that WNS have done the same.

Take a prototypical case in which intuitions are allegedly important evidence in traditional epistemology: a Gettier argument. We consider a thought experiment and have the intuition that the protagonist does not know, even though he does have a justified true belief; we conclude, therefore, that knowledge is not identical to justified true belief. Pust will say, ‘this intuition is a critical piece of evidence for the conclusion of non-identity.’ This statement sounds overwhelmingly plausible.

However, it is worthwhile to look more closely at the structure of the Gettier argument. Benjamin Jarvis and I argued in our (Ichikawa and Jarvis, forthcoming) that the proper formalization of the Gettier argument is of this form:

(1)  $\diamond g$

- (2)  $\square (g \supset \text{Someone has non-knowledge justified true belief})$

Therefore,

- (3)  $\diamond \text{Someone has non-knowledge justified true belief}$

In our formalization,  $g$  was the proposition that the members of a particular set, *STORY*, whose members are propositions about the protagonist of the thought experiment, are all true. Timothy Williamson (2004, 2007) has defended a similar (for present purposes) formalization:

- (1')  $\diamond \text{The (interpreted) sentences making up the thought experiment are true.}$
- (2')  $\text{The (interpreted) sentences making up the thought experiment are true} \square \rightarrow \text{Someone has non-knowledge justified true belief}$

Therefore,

- (3)  $\diamond \text{Someone has non-knowledge justified true belief}$

In neither formalization does either premise invoke psychological facts about the folk or the person running the argument; there is no mention of intuitions at all. Assuming that something along these lines is right, is there any space for intuitions to play the role that Pust and WNS assume they do?

Let's back up. What do we mean when we say that 'this intuition is evidence' for a philosophical claim—say, the claim that knowledge isn't identical to justified true belief? We might distinguish between a strong and a weak reading of 'the intuition that  $p$  is important evidence for  $q$ .'

Evidence is propositional. According to a strong reading, the intuition that  $p$  is evidence for  $q$  just in case the proposition that *I have the intuition that  $p$*  is important evidence for  $q$ . Pust clearly has in mind—at least sometimes—the strong reading. He is sometimes explicit, as when he says, as quoted above, that 'most philosophers take the *fact that they have the intuition that  $[q]$*  to show that  $[q]$ '. The strong reading is also explicitly endorsed by WNS, who characterize intuition-driven romanticism as a methodology that takes facts about intuitions as inputs, and generates philosophical theories on the basis of these psychological data.

But there is a weaker reading available for this sentence. On the weaker reading, 'the intuition that  $p$  is evidence for  $q$ ' means something like the claim that the *intuited proposition*—namely,  $p$ —is evidence for  $q$ . It is on the weaker reading that the Pollock quote given by Pust plausibly lends credibility to the claim that Pollock treats intuitions as evidence in his epistemology. *That such-and-such is permissible*—an intuited proposition—is the starting point for Pollock's theorizing.

The weak reading is not an abuse of English. When Holmes investigates a crime scene, there is a perfectly acceptable sense in which he is relying crucially on his beliefs. But this is not to say that facts about his belief states play crucial evidential roles in his theorizing. In typical cases, he reasons with propositions about the crime scene, not his own psychology. He may run an entire investigation with no explicit introspection at all. In a parallel way, we can express a truth by saying that ‘all we have to go on is our knowledge’; but ‘our knowledge’ here refers to something like the set of known propositions—not any collection of our own mental states of knowing, or facts about what knowledge we have. (If, as most epistemologists think, KK fails, then the former set outstrips the latter.) It is on this model that we may understand the weak reading of ‘the intuition that  $p$  is important evidence for  $q$ ’.

Only this weak reading, I think, is plausibly true. There is no obvious place for psychologistic facts to play evidential roles in traditional epistemology. Consider a standard Gettier argument as formalized above:

- (1) Such-and-such is a possible story.
- (2) Necessarily, such-and-such involves a case of justified true belief that is not knowledge.

Therefore,

- (3) It is possible for there to be justified true belief without knowledge.

If I came to know (3) via an argument like this one—and like many epistemologists, I did—then, if someone asks me to cite my evidence for (3), then I will cite (1) and (2). I won’t mention any facts of the form, *I have the intuition that...* Indeed, it’s hard to see how facts of that sort could play an important role in an argument like this one. To insist upon the psychologization of evidence in philosophy is unduly skeptical, just as it is to insist upon the psychologization of evidence in science. In this I fully agree with Williamson (2007, 211).

Traditional philosophical methodology, then, is not an instance of intuition-driven romanticism, as defined by WNS.

### **3. Objections and Replies**

#### *3.1. Objection: Premise (2) just is the Gettier intuition.*

Yes, I’m happy to speak in those terms; that’s why I take it to be true, in the weak sense, that an intuition plays an evidential role in this argument. The Gettier intuition is a proposition about Jones, or Fords, or fake barns, or whatever; it’s not a proposition about my intuitions, or those prevailing in my society. *That I have* the Gettier intuition is irrelevant to the argument. (This is not to deny the obvious truth that, if I didn’t have it, I might well fail to be persuaded by the argument.)

### 3.2. *Objection: Intuitions are, in the strong sense, essential evidence for (1) and (2).*

Although this approach seems to be orthodoxy, its central claim is a substantive one. In my view, it is neither well-defended, nor plausible. That a story is possible, or involves justified true belief without knowledge, are not claims about anyone's intuitions—indeed, further, they seem to be entirely independent of facts about anyone's intuitions. So it's difficult to see why citing someone's intuitions should play any central evidential role in establishing either one. (This is not to deny that it is likely that our intuitions are by and large reliable in such matters, and that the fact of the intuition *can* be evidence for the claim—just as the fact that I have the belief that *p* (or that someone else generally reliable does) can provide *some* evidence that *p*. But the intuition is no more *essential* evidence than the belief is. The intuition, like the belief, is a *response* to the appropriate evidence.)

Why are so many philosophers tempted by the view that intuitions must be our evidence for claims like (1) and (2)? My speculative diagnosis is that they have difficulty seeing how else we could know such claims, other than by basing them on our own intuitions. The prescription, then, is to articulate alternative accounts.

On my own view, we can come to know premise (1) by invoking something in the neighborhood of a conceivability argument.<sup>1</sup> There is no premise here of the form *I am coherently imagining that g*; rather, we know that *g* is coherent simply by competently recognizing it to be coherent (something we do most easily when coherently imagining it)—not by introspecting and discovering that we coherently imagine it, or competently recognize it as coherent. Similarly, premise (2) is known via the application of a quite general competence for concept application, as articulated in Ichikawa and Jarvis, (2007). Similar stories are given in Devitt (2006), Williamson (2003), and Williamson (2007, 188-90).

### 3.3. *Objection: Traditional epistemologists themselves describe themselves as using intuitions as evidence.*

As I've emphasized above, I do think that there is a sense in which it is true that we use intuitions as evidence in cases like the Gettier argument. In much the same way, I think, we describe ourselves as using our knowledge as evidence in general—but the evidence is the propositional content of the knowledge, not the mental state itself. I therefore take myself to be vindicating some philosophical work—including some of my own—that describes itself as centrally involving intuition. If judgments about hypothetical thought experiments are identified with intuitions, then there is an important sense in which intuitions are centrally important to standard methodology. But it is the content of the intuition, not the fact of the intuition itself, that plays a key evidentiary role—this is the entity that is input into the relevant strategy.

---

<sup>1</sup> The challenge, of course, is the necessary *a posteriori*: some (apparently) conceivable propositions are impossible. Benjamin Jarvis and I hope, in the future, to publish an approach to modal epistemology that is sensitive to such complications.

I don't mean to deny that some traditional epistemologists have explicitly signed on to the strong interpretation of intuitions as evidence. Nelson Goodman is an oft-cited example—see Stich (1990, 77). Although I'm less than fully convinced that this interpretation of Goodman is correct, I have no desire to press the point. These are subtle matters, and are easily confused. I'm happy to admit that some or even many traditional epistemologists may be on the record as endorsing a conception of traditional analytic epistemology as taking psychological facts about intuitions as evidence. George Bealer (1999, 2002) is certainly an example. I am arguing that such philosophers are incorrect about the nature of their shared project. One does not centrally rely upon intuitions merely by virtue of defending the view that philosophers centrally rely upon intuitions, any more than one fails to know truths about the external world merely by virtue of being an indirect realist. (Let us assume, for the purpose of illustration, that indirect realism implies skepticism.) It is best to look to the methodology itself—not to any particular group of epistemologists' views about the methodology. The experimentalist critique purports to discredit the role that thought experiments play in arguments in traditional epistemology. It is this methodology I mean to be defending; part of my defense involves the observation that at no point do facts about intuitions play a critical role in these arguments.

(Another category of philosophical approaches should be mentioned, if only to be set aside. Some epistemologists explicitly sign on to a project in which facts about intuitions are important evidence, and do so, I think, without error. I have in mind epistemologists whose project is not the traditional one gestured at above—one about, roughly, the nature and grounds of human knowledge. Rather, they are concerned with questions about epistemic concepts. Alvin Goldman (2001, 2007) is an exemplar of this approach.<sup>2</sup> The project of articulating the nature and application of epistemic concepts, which has a perfectly obvious and respectable use for psychological facts about intuitions, is not the project that presently concerns me.)

#### **4. Recasting the Experimentalist Critique**

If the strong conception of intuitions as evidence were relied upon by traditional methodology, then the experimentalist critique would appear quite forceful: on a traditional approach, facts about what intuitions you have play a crucial role in determining the correct normative epistemic view. So if other people had importantly different intuitions, then their applications of the standard methodology—even proper applications of it—would lead to divergent views. The challenge of why we should prefer the use of facts about our intuitions to ones about theirs looks daunting.

But I've argued that the strong conception is not true; intuitions are only important evidence in the weaker sense articulated above. How does the experimentalist critique

---

<sup>2</sup> An interesting historical note: WNS (2001) was part of a special issue on the Philosophy of Alvin Goldman; Goldman's (2001) reply to WNS consists largely in the clarification that his work is to be understood as engaging with the mentalistic project discussed here.

fare with only the weak understanding of intuitions as evidence? On the conception of traditional epistemology that I've argued for, facts about intuitions are not typically plugged into WNS's 'IDR black box'. The kinds of propositions that are plugged into the IDR box are propositions like *such-and-such is not a case of knowledge*. Little of traditional epistemology, it seems to me, falls under WNS's heading of intuition driven romanticism.

How much of the experimentalist worry, then, is dispelled, and how much can be recast in terms that do not rely on the premise that traditional epistemology makes critical use of psychological facts about intuitions as evidence? Certainly, many of the experimentalist critics, when faced with a challenge like the one I've just made, are not much impressed; it is thought that it is not particularly difficult to re-cast their skeptical worries in terms that do not invoke the false assumption. If this is right, then the argument I've attempted to distill from WNS doesn't get to the heart of the experimentalist critique. Three attempts to re-cast the critique without invoking the problematic assumption of traditional reliance of psychological facts as evidence suggest themselves; I discuss them now in turn.

*4.1. First attempt: It is arbitrary and xenophobic to privilege our own concepts and judgments.*

The original WNS emphasis was on normativity: how can traditional methodology reliably yield normative conclusions? Perhaps we should understand the experimentalist critique as emphasizing this feature. Sure, we (well-educated, white Westerners who have studied philosophy) value knowledge; but there are *possible* people who value some other, non-knowledge state instead. Here is a challenge: Suppose we successfully articulated what rules we must follow in order for our beliefs to fall under our concept KNOWLEDGE; why should we *care* about following those rules? What value is there in complying with a standard that happens to be reflected in our language and society? It is xenophobic to privilege our own standards merely because they're ours. Stich (1990) writes:

[U]nless one is inclined toward chauvinism or xenophobia in matters epistemic, it is hard to see why one would much care that a cognitive process one was thinking of invoking (or renouncing) accords with the set of evaluative notions that prevail in the society into which one happened to be born. (94)

And:

Since our notion of justification is just one member of a large and varied family of concepts of epistemic evaluation, it strikes most people as simply capricious or perverse to have an intrinsic preference for justified beliefs. (95)

The critic pressing this 'isn't that nice' challenge to normative epistemology may well

concede that, for example, armchair resources are sufficient for knowledge that the traditional judgment about a Gettier case—that the subject’s state does not fall under the everyday concept KNOWLEDGE—is correct. In so doing, of course, he admits that we know that the Gettier subject does not know. (All parties must agree that S knows that  $p$  if and only if S’s relation to  $p$  falls under the concept KNOWLEDGE, and that this fact is easily known by those of us who have the concept.) But the critic isn’t much impressed by this admission. As Stich likes to say: now we know what knowledge is, and *isn’t that nice?* He (1990) writes:

The analytic epistemologist proposes that our choice between alternative cognitive processes should be guided by the concepts of epistemic evaluation that are “embedded in everyday thought and language.” But this proposal is quite pointless unless we *value* having cognitive states or invoking cognitive processes that accord with these commonsense concepts. And it is my contention that when they view the matter clearly, most people will not find it intrinsically valuable to have cognitive states or to invoke cognitive processes that are sanctioned by the evaluative notions embedded in ordinary language. Nor is there any plausible case to be made in favor of the instrumental value of beliefs or cognitive processes that are justified or rational. (93)

The idea, I take it, is that the interesting questions of epistemology are normative—they’re supposed to help us to know what sorts of beliefs to pursue. Knowing what beliefs fall under our ordinary concept KNOWLEDGE is no help in this normative enterprise unless we have some reason to value having beliefs that fall under that concept; this, Stich says, is implausible.

We must tread carefully here. Here is a fact that I know: the referent of the everyday concept KNOWLEDGE is knowledge; this follows straightforwardly from the fact that I am employing the everyday notion in thinking that thought and writing that sentence. If we keep this in mind, I think it should be clear that the value Stich attributes to the analytic epistemologist—*according with the standards of everyday thought and language*—is optional as the epistemologist’s object of value.

Here is a traditional view: knowledge is valuable. The attempt to explain the value of knowledge has occupied considerable attention from epistemologists since Plato. Among the candidate explanations are suggestions like: knowledge is the norm of assertion; knowledge is the norm of action; knowledge helps the subject achieve his interests; knowledge is a more stable kind of true belief; knowledge is part of the Platonic Good; knowledge is a successful achievement of a characteristically human performance.

It is no part of my present project to contribute to this vast literature. The question that concerns me is whether Stich’s argument casts doubt on the cogency of the project of treating knowledge as valuable, and seeking the explanation for that value. I agree with Stich that it would be an odd creature indeed who placed great value in the state of *having beliefs that fall under the extension of the everyday concept of knowledge*. Call

this state S. Such a valuation is probably not incoherent, but it does appear ill-motivated. It is no great defense of traditional epistemology if it leaves the value of knowledge like that.

Must one value S in order to value knowledge? Plausibly not. Although in fact, in the actual world, all and only people with S have knowledge, S and knowledge are not the same property. Depending on how we individuate concepts, the biconditional that one has knowledge iff one has S may well be only contingently true—there may be worlds where the concept KNOWLEDGE does not refer to knowledge. And there are certainly worlds where the word ‘knowledge’ does not refer to knowledge.

Etiquette norms (the ones around here) dictate that the fork be set to the left of the plate. Many of us value acting in accordance with those norms. There is, plausibly, at least instrumental value in complying with the rules of etiquette in one’s society. The way in which we value setting the fork on the left is contingent on the rule being as it is. The way we value epistemic norms are different. Knowledge is valuable, regardless of what epistemic ideals happen to be coded into our language. The disanalogy is especially apparent in divergent counterfactuals:

If our social norm were to put the knife on the right, instead of on the left, there would be no etiquette value to putting it on the left.

If our social norm were to have beliefs that are schknowledge, instead of knowledge, there would be no epistemic value to knowing.

Many of us will accept the first but not the second. That second has some of the feel of:

If our social norm were to kill all the old people, there would be no moral value to refraining from killing them.

I take it just about everybody who thinks there is actual moral value in refraining from killing old people rejects this one.

Another way to see this point is to observe how far Stich’s argument, if sound, would generalize. Take whatever candidate for value that you like—desire-satisfaction, or pleasure, or eudaimonia, or true belief, or whatever you find most plausible. Stich’s counterpart can argue:

You propose that our choice between alternate courses of action should be guided by what falls under our concept PLEASURE. But this proposal is quite useless unless we *value* having states that accord with this commonsense concept. It is my contention that when they view the matter clearly, most people will not find it intrinsically valuable to have states that are sanctioned by the PLEASURE concept that happens to be embedded in ordinary language. Nor is there any plausible case to be made in favor of the instrumental value of states that are pleasurable.

It is a mistake to argue, from the premise that it is implausible to value *matching the ordinary concept of XNESS*, to the conclusion that it is similarly implausible to value xness.

The challenge from arbitrariness and xenophobia takes a similar starting point. Our epistemic evaluations are informed by our epistemic concepts, which are a product of contingent features of our upbringing. WNS write, in Nichols, Stich, & Weinberg (2003), that:

Without some reason to think that what white, western, high [socioeconomic status] philosophers call ‘knowledge’ is any more valuable, desirable or useful than any of the other commodities that other groups call ‘knowledge’ it is hard to see why we should care if we can’t have it. (245)

Suppose we learned that in some society, the word best translated as ‘knowledge’ carried a different meaning from the ordinary English ‘knowledge’—perhaps their word means justified true belief. (On one interpretation of WNS, their data suggests that certain idiolects of English are like this.) What reason, WNS ask, do we have to prefer our criterion of epistemic evaluation (knowledge) to theirs (JTB)?

Even if it turns out that there are no such societies, a version of this challenge may still be pressed: surely we *could have* used a concept of evaluation like that. So it is in some sense *arbitrary* that we don’t. What reason have we to prefer the notion we happened to end up with to any other? A very plausible response to this challenge seems to me to be the pluralist one suggested by Ernest Sosa (forthcoming): what’s to stop you from valuing JTB? Nothing at all. We value all sorts of things; value whatever you want. This is consistent with continuing to value knowledge. Sosa writes:

The fact that we value one commodity, called ‘knowledge’ or ‘justification’ among us, is no obstacle to our also valuing a different commodity, valued by some other community under that same label. And it is also compatible with our learning to value that second commodity once we are brought to understand it, even if we previously had no opinion on the matter. (15-16, manuscript)

This response strikes me as entirely correct. But Stich identifies two challenges for this pluralistic line: first, that a satisfactory epistemology should supply norms of permission, not merely norms of valuation—something for which pluralism is less plausible, and second, that even if we limit ourselves to concerns with value, the pluralist has no resources to weigh tradeoffs in value in the inevitable case when one is forced to choose between alternate alleged epistemic goods. On norms of permission, Stich (forthcoming) writes:

Norms of valuing do play a role in traditional epistemological debates, but they are not the only sorts of norms that epistemologists have considered.

As we noted earlier, Goldman insists, quite correctly, that justification rules (or “J-rules”) play a central role in both classical and contemporary epistemology, and J-rules specify *norms of permissibility*, not norms of valuing. They “permit or prohibit beliefs, directly or indirectly, as a function of some states, relations, or processes of the cognizer” (Goldman 1986: 60). When we focus on these rules, the sort of pluralism that Sosa suggests is much harder to sustain. If a rule, like the one cited a few paragraphs back, says that *ceteris paribus* we ought to hold a belief if it is an instance of knowledge, and if ‘knowledge’ is interpreted in different ways by members of different groups, then Sosa’s pluralism leads to inconsistency. There will be some beliefs which we ought to believe on one interpretation of ‘knowledge’ but not on the other. (9, manuscript)

And on trade-offs:

Moreover, even in the case of norms of valuing Sosa’s pluralism can lead to problems. Sosa is surely right to claim that someone who values owning money banks can also value owning river banks. But if there is one of each on offer and the person’s resources are limited, she will have to make a choice. Which one does she value more? (9, manuscript)

With regard to norms of permission, it is not at all clear that the fact that some *ceteris paribus* rules will come into conflict with one another is decisive against the relevant sort of pluralism. That they’re *ceteris paribus* rules, instead of absolute ones, is just what is required to tolerate this sort of conflict. So if my neighbors use ‘knowledge’ to pick out JTB, which they value, they and I can all endorse and share the relevant *ceteris paribus* permissibility rules:

*Ceteris paribus*, believe that  $p$  if and only if doing so will result in knowledge that  $p$ .

*Ceteris paribus*, believe that  $p$  if and only if doing so will result in JTB that  $p$ .

(Incidentally, it is worth pointing out that it is not clear that there are many actual practical circumstances in which these rules advise an agent in divergent ways; the person who tries to maximize the one will look quite a lot like the person who tries to maximize the other. This suggests that the extent to which genuine conflict among plausible epistemic norms of permissibility may not be as great as Stich assumes. But I will not pursue this line of thought further.)

Of course, this defense of pluralism only holds if we treat the norms as *ceteris paribus* rules. Could a version of the critique insist on absolute rules? Not very effectively, for absolute rules of this sort are highly implausible. It is *all things considered* permissible to fail to maximize knowledge in some circumstances, even though doing so will violate some *ceteris paribus* epistemic rules. An all-things-

considered knowledge maximization principle would prohibit building houses in favor of counting bricks. As Sosa (forthcoming) points out, this is so even if we limit the relevant domain to the epistemic:

Silly beliefs about trivial matters can attain the very highest levels of epistemic justification and certain knowledge even if these are not beliefs that one should be bothering with, not even if one's concerns are purely epistemic. (17, manuscript)

So there is no particular difficulty with a pluralist response to the xenophobia challenge that goes along with Stich's preference for *ceteris paribus* norms of permission. And stronger rules are implausible, independent of considerations of alternative norms or values. Stich may well insist: *but which rule should we follow?* This is in effect to assimilate the challenge in terms of rules to the challenge in terms of values formulated above; I will therefore turn to values and consider the challenges together. Stich's second argument is that there is a problem for the pluralist even at the level of value. I say, with Sosa, that there's nothing stopping me from valuing the other societies' epistemic goods in addition to my own, if I can learn to think about them. If some people or societies value true belief, or justified belief, or justified true belief, or belief derived from a generally reliable source, or certainty, but don't even have a word for knowledge, we can all get along just fine, and even learn to value one another's preferred states too.

Stich replies, but *which do you value more?* Since we are finite creatures with finite resources, we must choose among the things we value; the pluralist hasn't told us how to adjudicate between valuable things—and traditional armchair epistemic methodology doesn't obviously have the resources to identify the appropriate criterion. It should be clear that this is exactly analogous to the question just raised about *ceteris paribus* rules.

My answer here is simple: I agree with Stich that traditional armchair epistemology does not obviously provide the resources to adjudicate between different valuable states, or conflicting *ceteris paribus* rules. But I very much doubt it ever pretended to. Suppose we set aside questions about differing epistemic concepts; even if knowledge is the only epistemic game in town, we still have to decide whether to read the encyclopedia or walk the dog. If Stich thinks it is a great scandal that traditional epistemology provides no clear advice on this matter, he has broader expectations for epistemology than I.

I have so far been assuming that the rival epistemic goods, though not identical to our epistemic goods, were not antithetical to them. If we value knowledge, and our neighbors value truth, JTB, or certainty, then there seems to me to be no obstacle to our sharing their values, as explained above. This case to me seems analogous to this one: I like opera. I can get along just fine with Emily who likes Puccini operas, Andrew who likes theatrical performances in general, and even Martin who likes basketball. Not only are we peaceful neighbors, but we can even learn to appreciate one another's particular preferences, and share them to a large extent.

But there could be a person or society with vastly different epistemic values—values

that are not only non-identical to ours, but in direct tension with them. Maybe they value false beliefs, or unjustified ones. Or maybe they're Pyrrhonians, who value the complete absence of belief. This is more like the case where I like opera and my neighbor demands total silence—our values just plain conflict. (Of course, there is no evidence on the table that there are people or societies like this.) In this extreme case, I reject the values of my alien neighbors. But this is no xenophobia—I avoid them, not because they are different from me, but because their values are inconsistent with the things I value.

The arbitrariness reformulation of the experimentalist critique, then, need not convince us to abandon traditional methodology.

#### *4.2. Second attempt: Recast the critique by swapping 'judgment' for 'intuition'.*

Joshua Alexander and Jonathan Weinberg (2007) provide a response to Timothy Williamson's (2003, 2007) invocation of the view I have been defending: that psychological facts about intuitions are not the primary evidence in philosophy. It is worth quoting their response at some length. Alexander and Weinberg write:

Timothy Williamson has also developed a more radical response to the restrictionist threat: rejecting the picture of philosophical practice as depending on intuitions at all! He argues that our evidence, in considering the cases like those listed in section 1, is not any sort of mental seeming, but the facts in the world. He compares philosophical practice to scientific practice, where we do not take the perceptual seemings of the scientists as our evidence, but the facts about what they observed. Similarly, then, we should construe Gettier's evidence to be not his intellectual seeming that his case is not an instance of knowledge, but rather the modal fact itself that such a case is not an instance of knowledge. We retreat from talk of the world to talk of percepts when we (mistakenly) attempt to accommodate the skeptic; so, too, do we retreat to talk of intuitions only under the pressure of skeptical arguments. And since Williamson is himself antiskeptical, emphasizing the continuity between ordinary modal cognition and philosophical cognition, he concludes we should give up thinking of our philosophical evidence in the thinly psychological terms of intuitions.

But we do not think that Williamson's arguments can provide much solace for traditional analytic philosophers. For the results of experimental philosophers are not themselves framed in terms of intuitions, but in terms of the counterfactual judgments of various subjects under various circumstances. Although the results are often glossed in terms of intuitions to follow standard philosophical usage, inspection of the experimental materials reveals little talk of intuitions and mostly the direct evaluation of claims. The restrictionist challenge does not need to turn on a (potentially mistaken) psychologization of philosophers' evidence; that it does not turn on that skeptical move hopefully helps make clear that it is not itself a

skeptical challenge. In terms that Williamson should be happy with, the challenge reveals that at the present time philosophers may just not know what their evidence really is. And the true extent of their evidence is not, we think, something that they will be able to learn from their armchairs.  
(72)

There is much to appreciate in this thoughtful passage. We must take care, however, not to draw more lessons than are warranted. One might be tempted from this passage to think that the experimentalist critique can be reformulated from a critique of the invocation of ‘intuitions’ to a critique of the invocation of ‘judgments,’ and thereby circumvent the response with a simple invocation of the find-and-replace button. But this is in at least some cases (such as the original WNS critique) false. The case I’ve made, that facts about intuitions are not used as fundamental evidence in philosophy, generalizes against the claim that facts about *judgments* are used as fundamental evidence in philosophy. Williamson’s (2003) controversial claim that ‘intuition’ talk is unhelpful, and that philosophical ‘intuitions’ are really only particular sorts of judgments, is far from the centrally important observation. The centrally important observation is that philosophical evidence is not primarily psychological at all.

Alexander and Weinberg do have in mind, however, experimentalist arguments beyond the WNS one I’ve been focusing on. Some of these do point to important reasons for humility in our engagement with sensitive philosophical questions. For example, Swain, Alexander, and Weinberg (2008) have found that when subjects who are presented with thought experiments and asked to judge whether their protagonists have knowledge, their responses seem susceptible to a pernicious order effect: subjects are more likely to attribute knowledge in a tricky case—a fake barn case or a TrueTemp case—if they’ve recently been asked about an obvious case of non-knowledge than they are if they’ve just been asked about an obvious case of knowledge. That casual judgment is susceptible to such irrelevant features should not be particularly surprising; there is strong independent reason to believe that humans are susceptible to many such kinds of performance errors (for a nice summary, see Stich (1990), pp. 4-9, citing Nisbett and Borgita (1975), Wason and Johnson-Laird (1970), Tversky and Kahneman (1983), and others). Nevertheless, the point that philosophers must not blindly stick to whatever philosophical intuitions they find themselves attracted to is well-taken. We should be circumspect in our philosophical judgments, especially in situations where we are particularly prone to error. Here, empirical data can surely help us to improve our epistemic positions, by helping us to identify the fallacies and biases to which we are prone. There is important and valuable empirical work to be done in this part of experimental philosophy; in my view, the movement’s most impressive achievements occur in this area. For instance, Horowitz (1998) impressively employs Kahneman and Tversky’s Prospect Theory, an empirical theory about how humans reason with risks and probability, to discredit certain deontological intuitions in normative ethics. Her project strikes me as entirely compelling. Gendler (2007) offers a catalogue of similar projects, and Gendler (2002) is her own attempt to discredit, on empirical grounds, a particular sort of intuition about personal identity. All of these projects are worthwhile and philosophically relevant; none, I think, undermine traditional methodology, properly

understood.

Weinberg (2007) expresses pessimism that philosophical judgments are capable of sufficiently careful treatment to figure into a respectable methodology. At present I will say only that this pessimism does not seem to me well-founded; it relies on unfriendly assumptions about the homogeneity of philosophical judgment, and our inability to distinguish shakier intuitions from more solid ones. I see no reason to think that careful philosophers cannot take care to avoid these sorts of errors, the same way that careful perceivers do.

What of the traditional view that much of philosophical methodology is *a priori*? Does my concession here undermine this claim? Not necessarily. A full defense here would involve a criterion for apriority, which is beyond my present scope, but we may go some way toward establishing the plausibility of the apriority of the methodology by examining proposed criteria for apriority that have been influential.

Here are two. On one influential approach to apriority, a proposition *p* is known *a priori* just in case, roughly, *p* is known, and the subject's warrant for *p* does not derive from sensory or perceptual appearance. (Kitcher, 1980; 2000). On another widespread approach, one has *a priori* warrant for *p* if, roughly, a full understanding of *p*, or an ability clearly to entertain the proposition that *p*, is sufficient for warrant, or knowledge, that *p*. (Bealer, 1999; 2002, Sosa, 2002). Both of these approaches are controversial, but many philosophers believe that something in the ballpark of at least one of them is correct.

Both of these formulations are consistent with claim that *a posteriori* human limitations sometimes lead us to systematic errors, and that we philosophers are well served to attend to such limitations in order to avoid such errors whenever possible. A simple model illustrating this compatibility is one in which the interference of a systematic human error is a defeater for *a priori* knowledge and of full understanding; in the absence of such errors, we are able to achieve *a priori* knowledge. In ordinary cases, when we are not making such errors, we have warrant sufficient for knowledge—we are not here forced to reject the attractive ideas that this warrant does not derive from sensory experience, or that it does derive from understanding of the relevant propositions. On this approach, an empirical understanding of our susceptibility to such errors can help put us in a position to achieve more *a priori* knowledge.

On a similar theme, compare Williamson (2007):

[P]sychological experiments might in principle reveal levels of human unreliability in proof-checking that would undermine current mathematical practice. To conclude on that basis alone that mathematics should become an experimental discipline would be hopelessly naïve. (7)

I should add that the dialectic is here complicated by the fact that Williamson himself does not consider philosophy to be *a priori*, as he rejects the dichotomy between apriority

and aposteriority, primarily on the basis of pessimism about providing an adequate characterization of the alleged distinction (2007, 165-69). So I count Williamson as an ally against Weinberg here only insofar as he agrees with me that much philosophy is legitimately pursued from the armchair. Providing, in response to Williamson's critique, a rigorous characterization of the distinction between apriority and aposteriority is an important project beyond my present scope.

*4.3. Third attempt: The experimentalist challenge derives from general challenges from disagreement.*

Finally, I am also inclined to be moderately concessive with respect to a third reformulation of the experimentalist critique—one in which the posited divergent intuitions are treated as epistemic challenges to methodology just insofar as our confidence in any particular belief should in general be shaken upon encountering disagreement. That is to say, discovering people who think that Gettier cases are knowledge undermines traditional philosophical practice because in general, discovering people who believe that not- $p$  undermines the belief that  $p$ .

This strikes me as right so far as it goes; however, I doubt it will go very far. A combination of two lines of defense will, I think, serve to vindicate the traditional approach with respect to the clear cases, like the Gettier intuition. First, as Ernest Sosa (2007) has emphasized, one plausible explanation for apparent divergences in opinion about Gettier cases could easily be that the relevant subjects are not judging with respect to the intended proposition. There are two ways this could be so: first, if they 'filled out' the story of the thought experiment in a nonstandard way. Thought experiments are inevitably underspecified, and we bring much to bear on the contents of the thought experiment stories that is not entailed by the literal text of those stories. So one explanation for apparent divergent intuitions may be that some people are engaging with different scenarios. A second way in which the subjects may not really be disagreeing with the traditional philosophical view is if they interpreted key terms, such as 'knowledge', in a nonstandard way; *i.e.*, if they mean something other than (but presumably similar to) knowledge by 'knowledge'. Divergent judgments about the cases in question are exactly what would be predicted by such possibilities; indeed, it's hard to imagine a better way to recognize that a person means something other than knowledge by 'knowledge' than by observing that he uses it in to describe nonknowledge cases, does not defer to the authority of experts or his community, is unwilling to infer that a belief is not a case of 'knowledge' from the fact that it was derived from a falsehood, etc.

A second mitigating factor against the undermining of apparently-firm philosophical beliefs by disagreement is the possibility that the deviant subjects are ignorant, confused, inattentive, or otherwise cognitively inferior with respect to the judgment that is being asked of them. Of critical importance in how much our beliefs are undermined by disagreement is the authority of our disagreeing interlocutors; if I learn that someone thinks that I am dead, this does not much—if at all—undermine my belief that I am alive. In this matter, I am the relevant expert. (See Elga 2007 on how we should respond different to disagreement from different kinds of people.) Likewise, the physicist who

knows that tables comprise mostly empty space and the astronomer who knows that the universe is billions of years old retain their knowledge, even in the face of the onslaught of folk who disagree with them. Why not so for the epistemologist who knows that Jones doesn't know someone in his office owns a Ford?

There is one obvious difference, of course: the epistemologist's judgment is *a priori*; the physicist's judgment about tables is based on scientific theory and observation, and my judgment about my own life is on the basis of direct experience. People who believe that tables have no empty space inside them, or that I am dead, are lacking important evidence; the folk who think Jones knows are not. So goes one argument.

This disanalogy cannot stand up as presented. As philosophers well know, not all *a priori* investigations are easy; there's no guarantee that just anyone will get these questions right. (Compare the analogous situation when the folk disagree with a mathematician about an *a priori* mathematical fact.) To recognize Gettier cases as cases of non-knowledge is a cognitive achievement; it is entirely possible that, without philosophical training, some people might fail to achieve it. Happily, there is evidence that philosophical training *does* help—some of WNS's data suggests that people who had studied philosophy were significantly more likely to have the Gettier intuition. (Nichols, Stich & Weinberg 2003, 242.) Just as people who have studied chemistry and physics are more likely to make correct judgments about the constitution of tables and chairs, so too are people who have studied epistemology more likely to make correct judgments about knowledge. This is just as it should be.

It is plausible that a combination of these two responses can defend traditional methodology in these cases to a very considerable extent. A challenge from general disagreement along these lines that was not susceptible to this dual response would have to be one in which there was strong evidence that the subjects in question were thinking clearly, sufficiently intelligent, and relevantly informed, and *also* that, when they uttered sentences like 'in this story, Smith knows that someone in his office owns a Ford,' their word 'knowledge' means knowledge, and that they were engaging with the same story we were. This set of circumstances is not obviously possible; certainly, the experimentalists have not presented evidence that it actually obtains.

I conclude that the case for extreme pessimism is weak. The experimentalist challenges can be met.

### **References**

- Alexander, J., & Weinberg, J. (2007). Analytic epistemology and experimental philosophy. *Philosophy Compass* 2(1): 56-80.
- Bealer, G. (1999). A theory of the a priori. *Philosophical Perspectives*, 13:29-29-55.
- Bealer, G. (2002). Modal epistemology and the rationalist renaissance. In T. Gendler, & J. Hawthorne (Eds.), *Conceivability and possibility* Oxford: Clarendon. (Pp. 71-

126).

- Devitt, M. (2006). Intuitions. *Ontology Studies Cuadernos de Ontologia: Proceedings of VI International Ontology Congress* (San Sebastian, 2004), Victor Gomez Pin, Jose Ignacio Galparaso, and Gotzon Arrizabalaga, eds. (2006): 169-76.
- Elga, A. (2007). Reflection and disagreement. *Noûs*, 41(3): 478-502.
- Gendler, T. (2002). Personal identity and thought experiments. *The Philosophical Quarterly*, 52(206): 34-54.
- Gendler, T. (2007). Philosophical thought experiments, intuitions, and cognitive equilibrium. *Midwest Studies in Philosophy*, 31: 68-89.
- Goldman, A. I. (2001). Replies to contributors. *Philosophical Topics*, 29: 261-511.
- Goldman, A. I. (2007). Philosophical intuitions: Their target, their source, and their epistemic status. *Grazer Philosophische Studien*, 74: 1-25.
- Horowitz, T. (1998). Philosophical intuitions and psychological theory. *Ethics*, 108: 367-85.
- Ichikawa, J. & Jarvis, B. (forthcoming). Thought-experiment intuitions and truth in fiction. *Philosophical Studies*, forthcoming.
- Kitcher, P. (1980). A priori knowledge. *The Philosophical Review*, 89(1): 3-23.
- Kitcher, P. (2000). A priori knowledge revisited. In P. Boghossian, & P. Benacerraf (Eds.), *New essays on the A priori*. New York: Clarendon Press. (Pp. 65-91).
- Nichols, S., Stich, S., and Weinberg, J. (2003). Meta-skepticism: meditations in ethno-epistemology. In S. Luper, ed., *The Sceptics*. Aldershot, UK: Ashgate Publishing. (Pp. 227-247.)
- Nisbett, R. & Borgita, E. (1975). Attribution and the psychology of prediction. *Journal of Personality and Social Psychology*, 32.
- Pust, J. (2000). *Intuitions as evidence*. New York and London: Garland Publishing, 2000.
- Sosa, E. (2002). Reliability and the a priori. In T. Gendler, & J. Hawthorne (Eds.), *Conceivability and possibility*. New York: Oxford University Press. (Pp. 369-384).
- Sosa, E. (2007). Experimental philosophy and philosophical intuition. *Philosophical Studies*, 132(1): 99-107.

Sosa, E. (forthcoming). A defense of the use of intuitions in philosophy, in D. Murphy, ed., *Stich and his critics*, Malden, MA: Blackwell.

Stich, S. (1990). *The fragmentation of reason*. Cambridge, MA: The MIT Press.

Stich, S. (forthcoming). Reply to Sosa, in D. Murphy, ed., *Stich and his critics*, Malden, MA: Blackwell.

Swain, S., Alexander, J., & Weinberg, J. (2008). The instability of philosophical intuitions, *Philosophy and Phenomenological Research*, 76(1): 138-55.

Tversky, A., & Kahneman, D. (1983). Extensional versus intuitive reasoning: The conjunction fallacy in probability judgment. *Psychological Review* 90(4).

Wason, P., and Johnson-Laird, P. (1970). A conflict between selecting and evaluating information in an inferential task. *British Journal of Philosophy*, 61: 509-15.

Weinberg, J. (2007). How to challenge intuitions empirically without risking skepticism. *Midwest Studies in Philosophy*, 31(1): 318-43.

Weinberg, J., Nichols, S. and Stich, S. (2001). Normativity and epistemic intuitions. *Philosophical Topics*, 29: 429-460.

Williamson, T. (2004). Philosophical 'intuitions' and scepticism about judgement. *Dialectica*, 58(1), 109-153.

Williamson, T. (2007). *The Philosophy of philosophy*. Malden, MA: Blackwell Publishing.