

Who Needs Intuitions? Two Experimentalist Critiques

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Abstract. A number of philosophers have recently suggested that the role of intuitions in the epistemology of armchair philosophy has been exaggerated. This suggestion is rehearsed and endorsed. What bearing does the rejection of the centrality of intuition in armchair philosophy have on experimentalist critiques of the latter? I distinguish two very different kinds of experimentalist critique: one critique requires the centrality of intuition; the other does not.

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 with this sort of shape have been wide accepted, and have provided the basis for the mainstream rejection of the relevant theories. Examples of apparent arguments of this form are easy to come by: Edmund Gettier’s (1963) famous refutation of the identity of knowledge with justified true belief is one; another is Hilary Putnam’s (1973) refutation of semantic internalism; a third is Saul Kripke’s (1980) refutation of the descriptivist theory of names. These instances of the argument form are generally, if not universally, accepted among analytic philosophers engaging in broadly armchair methods. Other arguments of the same general form are more controversial: consider David Chalmers’s (1996) argument from the intuitive possibility of phenomenal zombies to the denial of physicalism about the mind, or Judith Jarvis Thompson’s (1971) argument from an intuitive verdict about an unwelcome violin player to the permissibility of abortion. Appeal to intuition appears ubiquitous in armchair philosophy.¹

In the last decade, arguments of this sort of form came under a particular sort of philosophical scrutiny.² Insofar as philosophical argument relies on philosophical intuition, 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. So-called ‘experimental philosophers’ have produced data that purport to call the trustworthiness of philosophical intuition into question.³

¹ What is ‘armchair philosophy’ exactly? I can here do little beyond pointing to exemplars, like those in this paragraph. I am not at all convinced that all instances of ‘armchair philosophy’ have anything interesting in common that is not so general as to include all investigation. Since my ultimate project in this paper is to rebut a critique that is targeted at armchair philosophy, I take it the imprecision in the latter notion is the critique’s problem, not mine.

² The experimental critique represents an intensified form of a critique that was already present in the literature; some important precursors can be found in (Harman, 1977), (Stich, 1990), (Cummins, 1998), and (Hintikka, 1999).

³ Not all experimental philosophers press the experimentalist critique; some do empirical work simply because they are interested in what are uncontroversially empirical questions. See e.g. (Knobe, 2007), emphasizing

Given the apparent importance of philosophical intuition in traditional philosophical methodology, these studies are sometimes thought to provide serious worries for the latter.

However, the intuition-emphasizing treatment of traditional methodology can and has been resisted; I believe it is correct so to resist it. My present project is to evaluate the significance of the experimentalist critique, once naïve ideas about the importance of intuition have been rejected. In §1, I review and endorse some recent suggestions that the role of intuition in philosophical methodology has been exaggerated. In §2, I consider whether this constitutes a quick objection to the central tenets of the experimentalist critique; I agree with recent experimentalists that there is an important sense in which it does not. This motivates the central project of §3: drawing an underappreciated distinction between two very different sorts of experimentalist worries. One experimentalist worry—the one emphasized in §2, and typified by projects such as (Weinberg, 2007) is indeed orthogonal to questions about the role of intuition in philosophical methodology. In §4, I argue that the other experimentalist worry—more prevalent in experimentalist papers like (Weinberg, Nichols, & Stich, 2008), and emphasizing important connections to earlier projects like (Stich, 1990)—looks, at least on its surface, to depend on an important role for intuitions in standard philosophical method. I will consider alternate recastings of this critique in §§5-8; I conclude that none are cause for serious pessimism about armchair philosophy.

§1. Does Armchair Philosophy Need Intuition?

It is a commonplace that armchair philosophy relies on intuition, but is it true?

‘Intuition,’ like attitude names generally, is polysemous. For example, the word ‘belief’ can pick out a particular mental state or episode—‘her belief is a belief that everything happens for a reason’—or it can refer to the propositional content of such a state—‘her belief is that everything happens for a reason’. Likewise, when Dave Chalmers has the intuition that zombies are possible, we can use ‘intuition’ to refer to a state of Chalmers’s mind, as in ‘the intuition explains his attraction to dualism’—or we can use it to refer to the content of this state, as in ‘the intuition entails the falsity of physicalism’. (And so, some among us might go on to say, ‘the intuition must be false’.) William Lycan (1998) calls ‘intuitions’ in the former sense ‘*intuitings*’, and ‘intuitions’ in the latter sense ‘*intuiteds*’. How should we interpret the claim that armchair philosophy relies on intuitions? Is it a claim of reliance on attitudes, or reliance on contents?

A claim of reliance on intuitive contents would be hard to deny. If we admit that there are such things as intuitions, and that often, we have intuitions with contents that are used as premises in armchair philosophical arguments, then we’ve already admitted that armchair philosophy relies on ‘intuition’ in this sense. For instance, it is overwhelmingly plausible that some armchair philosophy relies on the fact that ‘Gödel’ refers to Gödel, even if it turns out he didn’t really prove the incompleteness of arithmetic.⁴ It is also overwhelmingly plausible that many of us have the intuition that ‘Gödel’ refers to Gödel, even if it turns out he didn’t really prove the incompleteness of arithmetic.⁵ So we often rely on intuitive contents. This

questions about the human mind. This paper is concerned only with the experimentalist critique—what (Alexander, Mallon, & Weinberg, 2010) call the ‘negative program’ of experimental philosophy.

⁴ (Kripke, 1980)

⁵ It is uncontroversial that many of us have this intuition, but (Machery, Mallon, Nichols, & Stich, 2004) argues that our intuitions may be more esoteric than has generally been assumed; (Mallon, Machery, Nichols, & Stich,

verdict is as uninteresting as it is obvious; philosophers rely on intuitions in this sense the same way philosophers (along with everybody else) rely on beliefs—this shows us nothing interesting about the epistemology or the appropriate methodology of philosophy.

The more interesting claim is the claim that philosophers rely on those mental states that are intuitions. At least some defenders and critics of armchair philosophy alike have understood it as so relying. For example, Weinberg et al. 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.)...’ (Weinberg, et al., 2008, p. 19, my emphasis)

I take this passage as committing their target to reliance on ‘intuition’ in the mental state sense. Intuition is here identified with judgment. (Of course, ‘judgment’ is susceptible to the same readings as ‘intuition’ is, but the psychological qualifier ‘spontaneous’ forces the state reading.)

Weinberg et al. are concerned to cast doubt on armchair philosophy. But some defenders of the armchair, too, have committed to the significance of intuition in this more interesting sense. Here, for instance, is Joel Pust (2000):

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

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 should we accept Pust’s claim that most philosophers take the fact about their own mental states to *show* that the fact about S is true? I suggest not. Suppose a philosopher is 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, many philosophers might ultimately exclaim, ‘I just have an intuition!’ But this is a way of ending the dialectical train of inquiry, not a serious attempt to

2009) argues that this undermines many philosophical arguments. Against the former, see (Martí, 2009); against the latter, see (Ichikawa, Maitra, & Weatherson, forthcoming).

explicate the evidence.

Very shortly after the passage quoted above is another, more telling, passage from Pust that can provide insight into the mainstream diagnosis of ascribing importance to the mental state, intuitions:

‘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)

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—nor, indeed, does Pollock describe any psychological states as data. According to Pollock, the basic data are the *acceptability of inferences*. But Pust takes away the moral that Pollock recognizes that the basic data are intuitive judgments. I can see this only as a kind of equivocation on the two senses of ‘intuition’ outlined above.

According to a psychologistic reading, the intuition that *p* is evidence for *q* (or data in favor of *q*, or something that shows that *q*, etc.) just in case the proposition that *I have the intuition that p* is important evidence (etc.) for *q*. Pust clearly has in mind—at least sometimes—this psychologistic 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 reading is also explicitly endorsed by Weinberg et al., 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 we have seen there is a more modest reading available for ‘the intuition that *p* is evidence for *q*.’ It may mean merely 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.

It’s undeniable that philosophers rely on many propositions that are intuitive; but it does not follow that philosophers rely on psychological states, intuitions, and allow them to play evidential roles. Neither does it follow that intuitive propositions are either available or relied upon *because* they are intuitive. So let’s distinguish three kinds of metaphilosophical claims:

1. Intuited contents are (often) taken as important evidence/reasons/data/input in armchair philosophy.

2. Intuited contents are (often) taken as important evidence/reasons/data/input in armchair philosophy because they are intuited.
3. Intuition states, or facts about intuition states, are (often) taken as important evidence/reasons/data/input in armchair philosophy.

Once these three claims about intuitions are distinguished, it becomes much less obvious whether there is any generally widespread commitment to the stronger claims about the roles of intuition in philosophy. Even many of those who have done the most to emphasize the role of intuitions are not obviously committed to ‘intuition’ in the state sense as playing such a role; George Bealer (1998), for instance, takes care to distinguish his central position from (3):

‘When I say that intuitions are used as evidence, I of course mean that the *contents* of the intuitions count as evidence. When one has an intuition, however, often one is introspectively aware that one is having that intuition. On such an occasion, one would then have a bit of introspective evidence as well, namely, that one is having that intuition. Consider an example. I am presently intuiting that if P then not not P. Accordingly, the content of this intuition—that if P then not not P—counts as a bit of my evidence; I may use this logical proposition as evidence (as a reason) for various other things. In addition to having the indicated intuition, I am also introspectively aware of having the intuition. Accordingly, the content of this introspection—that I am having the intuition that if P then not not P—also counts as a bit of my evidence; I may use this proposition about my intellectual state as evidence (as a reason) for various other things.’ (205)

It is not entirely obvious from this passage what Bealer’s considered attitude toward (3) is; it is clear that (3) does not capture his central view about intuition, but it is not clear whether he thinks it is true; whether he does depends on which ‘various other things’ he has in mind.⁶

Explicit commitments to (3) by armchair philosophers are hard to come by, setting aside one notable class of exceptions. Some philosophers explicitly sign on to a project in which facts about intuitions are important evidence, and are right to do so. I have in mind philosophers who study psychological matters like concepts (or intuitions themselves). So rather than, for instance, studying the nature and grounds of human knowledge, one might be more interested in questions about epistemic concepts. (Goldman, 2007) is an exemplar of this approach.⁷ 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 in this paper.

When the subject matter is nonpsychological, emphasis on psychological intuitions does appear misplaced. And it’s not at all clear that much actual practice does include such an

⁶ Does Bealer endorse (2)? It’s not entirely clear. In his (2002) he glosses his view as the claim that ‘by virtue of having an intuition that p, one has a prima-facie reason or prima-facie evidence for p.’ (74) This is not a statement of (2), which would say that in the relevant circumstances, one would have p *as* evidence, not merely have evidence for p. Perhaps (2) is the best view to attribute, given this quotation and the one cited in the main text.

⁷ An interesting historical note: (Weinberg, et al., 2008) was originally published (in 2001) as 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.

emphasis. (Williamson, 2007) and (Deutsch, 2010) defend both these claims; their arguments seem to me entirely correct. I see no reason to accept (3). Contrary to the suggestion of Weinberg et al., then, very little contemporary analytic philosophy take psychological intuitions as central inputs.

A thorough consideration of (2) is beyond my present scope,⁸ but my own view is that (2) and (3)—the only principles on the table that provide an important role to intuition states—are both false, at least when used to describe the best examples of armchair philosophy in the literature. (I have no doubt that some philosophers have been led astray by questionable methodology.) I shall call any view that endorses (2) or (3) one in which intuitions play a *central* role in armchair philosophy; it will be working assumption for much of this paper that intuitions do not play a central role.

So it is at best less than obvious that those psychological states that are intuitions have an important role to play in the epistemology of armchair philosophy. What bearing has this on the experimentalist critique?

§2. Does the Experimentalist Critique Need Armchair Philosophy to Need Intuition?

Prima facie, the experimentalist critique looks to be one that challenges psychological intuitions. So it is easy to think that it constitutes a significant challenge to armchair methodology only if the latter relies on such intuitions.

Weinberg et al. (2008), in the passage quoted above, clearly presses a version of the experimentalist critique that commits to the use of psychological data as inputs into the mechanism of theory construction. Much a lot of the ensuing literature in the critical experimentalist spirit has followed suit in emphasizing intuitions. So it is not surprising that, for instance, Max Deutsch (2009, 2010) takes the rejection of the centrality of intuition⁹ to neutralize the experimentalist critique.¹⁰

‘Philosophers need not assume that their own intuitions about cases are universal. So surveys showing them that they are not universal are irrelevant. Majority opinion does not determine the truth, or constitute the primary source of evidence in philosophy, and despite appeals to ‘what we would say’ about cases, majority opinion has never been thought to play these roles in philosophical argument.’ (Deutsch, 2009, p. 465)

Experimentalist critics are not, generally speaking, much impressed by this sort of move;¹¹ a central aim of the present paper is to explain why not. Although I’m in quite a lot of agreement with the line pressed by Deutsch, there is, I think, something to be said for limiting its significance against the experimentalist critique.

⁸ Benjamin Jarvis and I offer extensive criticisms of (2) in ongoing work in progress.

⁹ Deutsch most explicitly considers the version of the centrality of intuition on which philosophers proceed from psychologistic premises—what I called (3) in the previous section. It’s not obvious how his arguments bear on (2), the claim that intuited propositions are evidence by virtue of being intuited.

¹⁰ (Cappelen, forthcoming) also argues in this way.

¹¹ See e.g. (Alexander, forthcoming), (Alexander & Weinberg, 2007, p. 72), (Weinberg, 2009).

Alexander and Weinberg (2007) consider Williamson's rejection of the idea that psychologistic facts about intuitions are given strong evidential significance in armchair philosophy. They offer two responses in succession. The first, I think, largely misses the point:

'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 ...

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.' (72)

It is true that part of Williamson's project consists in an ontological claim about intuitions: they're mere judgments. But this is not closely related to his strategy outlined in the first quoted paragraph: that of denying that philosophical evidence is ultimately psychological. As such, the choice of psychologistic terms—intuitions, counterfactual judgments, or whatever you like—is irrelevant to the question of the sources of our philosophical knowledge. Alexander and Weinberg say that the experimentalist results can be described as measuring 'counterfactual judgments' just as well as they measure 'intuitions'—but each is equally psychological, and each is, on the Williamsonian approach, equally denied status as the philosopher's ultimate evidence. Gettier's evidence, on Williamson's view, is neither his intellectual seeming as though there is no knowledge in the relevant case, *nor* his state of counterfactual judgment that there would be no knowledge in the relevant case; it is the fact itself: that there *is* no knowledge in the relevant case.

Once this is clear, the last quoted sentence, in its relevant interpretation, is clearly false. Experimental philosophers do *not* typically make direct evaluations of the relevant claims; their data is behavioral and psychological: how do subjects react—what intuitions do they have, or what judgments do they make, or which option do they circle, and maybe: how confident are they about their answers? In theory, one could devise an experimental philosophy study that directly investigated the relevant claims. For instance, one might take subjects from various ethnicities and subject them to Gettier cases. They'd be given misleading evidence, such that they formed false beliefs, then prompted in such a way so as to induce them to infer truths from those falsehood. One might then observe whether members of one ethnicity were more likely to have knowledge than the other. Such would be a cross-cultural study of Gettier cases—are Western subjects more or less likely than East Asian subjects to have knowledge in Gettier cases?—that involved direct evaluation of philosophically relevant claims. But experiments like this one are, of course, far from the norm in experimental philosophy.

Alexander and Weinberg go on immediately from the previous passage to make a second argument; this one, I think, is much more germane.

‘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)

The idea here seems to be that the experimentalist critique provides philosophers with reasons to doubt the epistemic status of the premises on which they rely, and that these challenges demand a fairly significant shift toward a more empirical methodology. This is a recognizable and respectable form of critique, and one worth taking seriously. And Alexander and Weinberg are correct to say that it does not depend on whether philosophy proceeds from psychologistic evidence or sources of evidence. The form of this sort of critique is perfectly general.

Is it too general? There are two sorts of reasons one might be worried. One is that the skeptical implications of the concern extend too far, and will undermine all of our methods—not just within armchair philosophy. I won’t go into this important question here.¹² A second worry about generality is that it doesn’t make sufficient sense and contact with the original presentations of the experimentalist critique. Why do so many papers, like the ones cited above, seem to commit to a psychologistic conception of philosophical evidence, if such is irrelevant to the critique itself?

The answer to this question is that ‘the experimentalist critique’ has shifted over time. There are now two importantly distinct families of critique in the experimentalist tradition. Although they have not been clearly distinguished, even by their practitioners, they seem to me to have little to do with one another. We can make philosophical progress by distinguishing them rather sharply.

§3. Which Experimentalist Critique?

The passage from Alexander and Weinberg suggests a general form of the experimentalist critique: experimental data provides us with reason not to trust our armchair judgments about philosophical thought experiments—whether you call them ‘intuitions’ or not, and whether you think they proceed from psychologistic premises or not. Empirical data about human reliability can provide undercutting defeaters for judgments of any kind. Call this the *defeater critique*. The defeater critique sees traditional armchair methods as insufficiently self-reflective; armchair philosophers proceed without enough regard for their abilities to discern relevant facts. Twentieth-century analytic philosophy, on this view, proceeded in something of a dogmatic slumber. When epistemologists first encountered Gettier cases, it seemed obvious to them that they were cases of ignorance rather than knowledge; but maybe they should have slowed down a bit before just assuming they were right.

Empirical data can certainly bear on this sort of claim, and it’s not too hard to imagine uncovering rather strong reasons for worry. It will come as no surprise to anyone that our

¹² See e.g. (Weinberg, 2007), (Williamson, 2004), (Sosa, 2007).

philosophical capacities are fallible—sometimes we make mistakes—but many specific ways in which we are fallible could well turn out to be surprises. And at least in theory, these surprises could easily bring with them radical methodological consequences for armchair philosophy. Consider the various sorts of fallacies to which we humans are sometimes susceptible. I here mean ‘fallacies’ in a broad sense—gross logical errors like affirming the consequent are certainly examples, but so are biases towards or against the arguments of people of particular races or genders, or giving extra evidential weight to a certain view just because it is novel, etc. The extent to which we are subject to these fallacies is an empirical question; so too is the question under what circumstances are we better and worse at avoiding them.

To take a rather extreme example for the sake of argument, it might turn out, through empirical investigation, that armchairs like the one I’m now sitting in bring out the worst in our cognitive abilities. Something about the experience of sitting in an armchair affects our brains in a way that causes us to be worse at philosophy than we’d be if we were standing, or sitting on a bench. This would be a very strong vindication of the defeater critique. Empirical evidence to this effect would certainly undermine our rational confidence in much philosophy, and it would motivate a change in methodology. It would enjoin us quite literally to leave the armchair.

The actual data, of course, are not nearly so clean as in this hypothetical case—which is why the soundness of the defeater critique is controversial—but the model is the same. Experimentalists uncover data about a certain kind of unreliability, and suggest that it is infecting philosophical judgment in a way that should undermine our confidence in our own judgments. For example, Swain, Alexander, and Weinberg (2008) uncover data that philosophical intuitions are unduly influenced by the order in which cases are considered: 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.¹³ 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. Insofar as the relevant discoveries are distinctively empirical, and insofar as they constitutes a significant reason to worry about armchair methodology, we have empirical data that rationally undermines confidence in our philosophical judgments. Many other experimentalist arguments also fit this mold.¹⁴

The more modest form of the defeater critique runs on the basis of particular fallacies discovered by particular experiments: we’ve discovered that factors X, Y, and Z negatively

¹³ For a nice summary, see (Stich, 1990), pp. 4-9, citing (Wason & Johnson-Laird, 1970), (Tversky & Kahneman, 1983), and others).

¹⁴ To offer just a couple of examples: (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. 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.

influence philosophers' moral judgments, so philosophers ought to avoid relying on moral judgments in the presence of X, Y, and Z. Depending on the particular factors, doing so may itself require 'leaving the armchair' and using empirical methods; even if it does not, we've already left the armchair in a broader, methodological sense, since it was empirical research that taught us to be careful around X, Y, and Z.

A less modest version of the defeater critique makes an additional inductive step: since we've discovered, empirically, so many surprising factors—X, Y, and Z—that interfere with our philosophical abilities, there's good reason to suspect there could be lots more similar factors that we haven't found yet. We therefore should not rest with our armchair judgments at all, until we've done quite a bit more empirical work identifying what does and what does not impair us.

Many words have been written about the defeater critique; I'll not give it too many more just now. I am interested in distinguishing it from another, rather different, experimentalist critique of armchair philosophy.

§4. The Arbitrariness Critique

I have agreed with the suggestion of Alexander & Weinberg that there is a recognizable empirical critique of armchair methodology that does not depend on the latter's granting of a substantive role for intuitions. I do not think, however, that this shows that those who have attempted to defend armchair philosophy by resisting the psychologization of philosophical evidence have been spinning idly. For there is another experimentalist critique in the literature that operates quite differently, and independently, from the defeater critique; this, which I call the *arbitrariness critique*, may well depend on problematic views about the role of intuitions in philosophy.

The arbitrariness critique is emphasized most prominently in the work of Stephen Stich, although other experimental philosophers have also pressed versions of it—sometimes combining it, without perhaps quite realizing it, with the defeater critique. Given the influential formative role Stich played in the experimental philosophy movement, it is not surprising that his version of the critique should have held such influence. However, I believe that the critical experimentalist literature, on the whole, is helpfully thought of in broad generalist terms as having shifted its focus from the arbitrariness critique to the defeater critique.

What is the arbitrariness critique?

It is worthwhile to return to the earlier presentations of the experimentalist critique; let us focus for a time on (Weinberg, et al., 2008).¹⁵ I've already remarked on one odd feature of their argument, with respect to understanding them as offering the defeat critique: the focus on psychologistic intuitions does not seem relevant. Empirical studies about biases and errors can cast doubt on all kinds of judgments, not merely those that are products of processes that take intuitions as inputs. Another surprising feature of Weinberg et al.'s project, from this point of view, is the focus on distinctively normative questions. Their paper, "Normativity and Epistemic Intuitions," would have reason to focus on neither normativity nor intuition, were it a presentation of the defeat critique. Their project is different.

¹⁵ This paper was originally published in 2001, and played a substantial role in advancing the whole experimental philosophy movement.

Weinberg et al. focus in particular on armchair epistemology. Because of the emphasis on the normative, it is not obvious to what extent the arguments might generalize to other areas in philosophy. Other normative domains, like ethics, may be treatable in the obvious way, but it is not clear how or whether the arbitrariness critique, as I understand it, could be extended to, for instance, metaphysics or the philosophy of language. In the rest of this paper, I will follow Weinberg et al. in their focus on normative epistemology. They explicitly concern themselves only with that branch of epistemology that “attempts to establish norms to guide our epistemic efforts,” (p. 18) answering the question, “How ought we to go about the business of belief formation and revision?” (p. 19)

The worry is particular to these normative questions. Weinberg et al. charge that an intuition-based methodology has insufficient resources to answer them. Sure, we could rely on our intuitions about whether a subject is believing appropriately, or whether a state is knowledge. But intuitions like these are the products of minds that are heavily influenced by idiosyncratic features of our languages and societies. We could have grown up with any number of different sets of codes and norms; why think that the ones we happen to have provide any guidance about what we ought to do? They write:

‘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?’ (22)

It’s worth noticing that the arbitrariness critique, as given here, is pressed in a broadly a priori manner in (Stich, 1990). Weinberg et al. add to Stich’s earlier critique by going on to present evidence suggesting that the hypothetical people they discuss are actual, thus neutralizing, in their minds, the objection that the possibility of such individuals is too far-fetched to treat seriously.¹⁶ The results are well-known, even if the context is not: they suggest that there is evidence that East Asian subjects have systematically different intuitions about central cases in epistemology.

So this, at its core, is the arbitrariness critique. We could have had any number of different intuitions. And if we had different ones then our actual ones, then proper exercise of the methods we use to make normative epistemic judgments would produce different

¹⁶ They attribute such an objection to (Pollock & Cruz, 1999).

judgments—perhaps even conflicting ones. It is in an important sense arbitrary which intuitions we happen to have, and so it is arbitrary which normative systems we endorse. But this is a situation that should make us deeply uncomfortable; epistemic norms are too important to be so susceptible to matters of chance. So we'd better use some other method to decide which norms to accept.¹⁷

We could compare the situation to that in other domains in which we are led by arbitrary factors to subscribe to particular principles, such as the acquisition of language, or norms of etiquette. We come to general norms of etiquette by systematizing the behaviors and judgments we happen to be exposed to; I know to mail paper wedding invitations, rather than emailing or just inviting people orally—this knowledge comes from my observation of how such things are usually arranged, what is praised, what is censured, etc. These features are to a significant degree arbitrary; if I happened to live in a culture that did things differently, I'd have internalized different norms. This verdict is comfortable, in the case of etiquette, in a way that it is not in the case of epistemology. A culture of people who think unjustified beliefs are great is, we think, making a mistake, in a way that a culture of people who think oral invitations to weddings are great is not.

As I have presented it—and as Weinberg et al have presented it—the arbitrariness critique does depend critically on the psychologistic role of intuitions in normative theory construction.¹⁸ If something other than intuition—something less arbitrary—does the relevant work in our choosing of a particular set of norms, then the set of norms we have arrived on is not arbitrary: it is the one countenanced by that nonarbitrary factor.

So it appears as if the arbitrariness critique is susceptible to the denial of the centrality of intuition, in a way that the defeater critique is not. The moves of (Alexander & Weinberg, 2007), cited in §2, do not defend the arbitrariness critique from the denial of a central role for intuitions.

Is there another move available for the arbitrariness critique? Is there a way to press it without relying on a central role for intuitions? In the final sections, I will consider several strategies. None, I think, should be particularly troubling for the practitioner of armchair philosophy.

§5. Xenophobia and Conflicting Values

We value knowledge; that's part of the reason we have a word for it. Suppose there was another culture, rather different from ours, who did not value knowledge. Their epistemic practices are different from ours. Now it looks like an individual's own emphasis on knowledge is a quirk of where he happened to grow up. Our epistemic evaluations are informed by our epistemic concepts, which are a product of contingent features of our upbringing. Nichols, Stich, and Weinberg (2003) write that:

‘Without some reason to think that what white, western, high [socioeconomic status] philosophers call “knowledge” is any more valuable, desirable or

¹⁷ What other method? (Weinberg, et al., 2008) don't tell us; their project is negative. (Stich, 1990) endorses a kind of pragmatism that is meant to fill this role.

¹⁸ (Weinberg, et al., 2008) are very explicit in admitting this—see, for instance, the list of conditions on their p. 20.

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)

In a similar vein, 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)

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 Nichols et al., their data suggests that certain idiolects of English are like this.) What reason, they ask, do we have to prefer our criterion of epistemic evaluation (knowledge) to theirs (JTB)? To prefer in the absence of any such reason, we’re told, would be unwarranted xenophobia. Once again, we’re presented with some evidence that such individuals are not merely possible but actual; but even if this is mistaken, surely we *could have* used a concept of evaluation like that. So isn’t it 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?

The first thing to say in response is that it’s a rather substantive assumption that our thinking about knowledge is in the relevant sense arbitrary. If it is an objective fact that knowledge is an important feature of anyone’s intellectual life—if knowledge turns out to be, in the Lewisian sense, a very natural property—then the possibility of a society getting by without thinking of it may become a bit more dubious. For example, if one is sympathetic to Timothy Williamson’s project, emphasizing the centrality of knowledge for many diverse properties—evidence, belief, assertion, reference, justification, etc.¹⁹—then one will be more puzzled by the very idea of a thriving society that doesn’t care about knowledge. For my part, I am rather attracted by these Williamsonian views, although I won’t go into them further now.

A second plausible response to this challenge seems to me to be the pluralist one suggested by Ernest Sosa: 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.’ (Sosa,

¹⁹ Williamson (Williamson, 2000). For the view about reference, see (Williamson, 2007), ch. 8.

2009, p. 109)

This response strikes me as entirely correct. But Stich challenges this pluralistic line, saying first that a satisfactory epistemology should supply norms of permission, not merely norms of valuation, and second, that pluralism about norms of valuation is implausible. Stich 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. (Stich, 2009, p. 235)

There are several avenues of possible resistance available to the armchair philosopher.

First, Sosa’s pluralism—broadly the view that all parties speak truly in their normative epistemic claims—does not, contrary to Stich’s assertion, lead to inconsistency on the assumption that ‘knowledge’ refers to different properties in different cultures, and there is a rule that we ought to believe what we know. Stich writes that a rule ‘says that *ceteris paribus* we ought to hold a belief if it is an instance of knowledge.’ Stich is part of our culture, and speaks our language, so his word ‘knowledge’ means knowledge. So there is a rule about knowledge. How is the presence of other cultures in which speakers mean other things by ‘knowledge’ supposed to lead us to inconsistency?

I suspect that Stich is imagining that our alien friends also avow sentences like ‘there is a rule that *ceteris paribus* we ought to hold a belief if it is an instance of knowledge.’ This is *not* stipulated to hold in the hypothetical case; and perhaps more significantly, we have, so far as I can tell, no data whatsoever that points to anything like this actually being the case. But if we suppose that members of alien cultures who mean other than knowledge by ‘knowledge’ do utter sentences like that one, then a pluralistic line that tries to make everybody right does face some *prima facie* tension. Even still, there are a number of options available.

One option is to suggest that other words in the alien language differ from the corresponding words in English, just as ‘knowledge’ does. Perhaps their ‘rule’ or ‘ought’ or ‘believe’ means something that renders their sentence in no tension with ours. A related option is to exploit the context-sensitivity of the shared modal ‘ought’—giving each the English stable character, but letting it express a different modal relation in its own conversational context.²⁰

²⁰ (Jenkins & Nolan, 2010) argue that some apparent moral disagreements within English are resolvable in this way.

Even if we granted that our aliens utter the same norm-sentences we do, and that only ‘knowledge’ takes a different meaning between our two languages, *and* that no contextual variation is active to help them play nicely together, the pluralist line would *still* not be inconsistent. For the norm sentences take the form of *ceteris paribus* rules; it is of course a hallmark of *ceteris paribus* rules that they tolerate exception. 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 (once we work out how to talk to each other):

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*.

(Here, as in the rest of this paper, I am writing in English.)

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 live by the one will look quite a lot like the person who tries to live by the other. (*Ceteris paribus*, JTB is knowledge!) This suggests that the extent to which genuine conflict among plausible epistemic norms of permissibility may not be as great as Stich assumes. 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 believe unknowledgably, or to withhold belief that could have been knowledge, in some circumstances, even though doing so will violate some *ceteris paribus* epistemic rules. Demanding that one believe everything knowable would place implausible demands of attention; it would prohibit building houses in favor of counting bricks. As Sosa 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. (Sosa, 2009, p. 110)

We face no new challenges by being pluralists about epistemic value.

Stich gives a second objection to Sosa’s pluralist suggestion:

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? (Stich, 2009, p. 235)

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. Is it even obvious that the sorts of value in play here are commensurable? 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 could there 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 must reject the values of my alien neighbors. But this need be no xenophobia—societies like the ones imagined, if they are possible, will be dramatically unsuccessful ones. There are clear, objective, non-arbitrary grounds on which we can confidently judge our practices superior to theirs.

§7. Arbitrariness and the Value of Knowledge

Here is another argument relating arbitrariness and value that can be found in Stich's work.

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? Call this the 'so what' objection: now we know what knowledge is. So what?

The critic pressing this 'so what' 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 says: so what? Stich 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. (Stich, 1990, p. 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 are sanctioned by our commonsense epistemic evaluative concepts is no help in this normative enterprise unless we have some reason to value having beliefs that are so sanctioned; this, Stich says, is implausible.

The referent of the everyday concept KNOWLEDGE is knowledge. This is easy for me to know; it 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.

Does Stich’s argument cast 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 *according with the standards of everyday thought and language*. Call this state Φ . Such a valuation is 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.

But one needn’t value Φ to value knowledge. Although in fact, in the actual world, all and only people with Φ have knowledge, Φ and knowledge are, of course, not the same property. They have different modal profiles; the biconditional that one has knowledge iff one has Φ is only contingently true—there are possible worlds where knowledge is not what it takes to accord with the standards of everyday thought and language.

Etiquette norms (the ones around here) dictate that wedding invitations be sent by post. Many of us value acting in accordance with those norms. There is at least instrumental value in complying with the rules of etiquette in one’s society; perhaps there is intrinsic social value as well. But the way in which we value mailing invitations 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 send wedding invitations by singing telegram, instead of by post, there would be no etiquette value to sending them by post.

If our social norm were to have beliefs that are justified true belief, 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 torture animals whenever we were bored, there would be no moral value in refraining from torturing animals when we were bored.

I take it just about everybody who thinks there is actual moral value in refraining from torturing animals 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.

It is a mistake to argue, from the premise that it is implausible to value *matching the ordinary concept of X-NESS*, to the conclusion that it is similarly implausible to value x-ness.

§8. Arbitrariness and the Interest of Knowledge

So hypothetical or actual diversity with respect to which mental states are valued, or feature into rules, does not undermine the value of knowledge, or the plausibility of epistemic rules that feature knowledge. But there is another way in which one might be led from considerations of such diversity to an arbitrariness-based concern about normative epistemology: we might worry, not whether knowledge is a valuable state to be in, but whether knowledge is a theoretically interesting state worthy of philosophical study. (And so, *mutatis mutandis*, for various other epistemic, or more broadly philosophical, notions.)

I have not found an explicit presentation of this kind of line in the extant literature pressing the arbitrariness critique, but it is perhaps a natural fallback to the concerns about the value of knowledge itself, refuted in the previous section. If knowledge is but one of many similar epistemic properties that have epistemic value and figure into epistemic rules, does it warrant the philosophical energy armchair philosophers have spent upon it? Are we being too narrow-minded—perhaps even xenophobic—by studying the epistemic state we happen to talk about, at the expense of others?

We do have limited cognitive resources, and so when we choose study something, it is at least often at the expense of studying something else. And the discovery of people who seem to do pretty well, focusing on X instead of Y, might well give us reason to shift some of our

Y-attention to X: maybe they're onto something. But this does not constitute a serious threat to armchair philosophy, for three reasons.

First, pluralism in philosophical subject matters is just as plausible as is pluralism about epistemic value. The issues here largely parallel those discussed in the discussion of pluralism in §5. Although epistemologists often are particularly interested in knowledge, they need not be, and are not, exclusively so interested.²¹ The study of knowledge just doesn't preclude the study of anything else.

Second, armchair philosophy includes the resources for adjudicating between the study of various subject matters. Indeed, as in the case of the value of knowledge itself, we face the question of how to weigh the value of studying knowledge against other values—including the value of studying other philosophical topics—regardless of experimentalist concerns about cognitive diversity. It is interesting and worthwhile to study knowledge; it's also interesting and worthwhile to study happiness (and to study earthquakes, and to read novels...). How do we decide where to focus our energy? As far as how to choose between intellectual concerns goes, we each begin with our best guess, or with the topic we're first introduced to, or where we have our first original idea, and attend to what lines of inquiry are promising, and what are not. Often, we take cues in this matter from our colleagues—which of our ideas play interesting roles in the work of others? What is received with interest, and what with boredom? What tends to get published? Armchair philosophy, as practiced by actual armchair philosophers, does exhibit some sensitivity to these questions. (I do not mean to suggest that some of us ought not to exhibit more!)

In the particular case of knowledge, I consider (Williamson, 2000) definitive in favor of the theoretical interest of knowledge, as opposed to other epistemic states, although of course this can be contested. The central point for present purposes is that it can be, and is, contested and evaluated within armchair philosophy.²²

Third, the methodological objection is specific to its particular subject matter; diversity with respect knowledge challenges the value of studying knowledge; this objection should be evaluated entirely independently from one that challenges the value of studying true belief, or justified belief, or safe belief (or, for that matter, virtuous action, causation, reference, intentionality, parthood, etc.). So even if, contrary to what I have just suggested, cognitive diversity implies an arbitrariness that undermines the importance of studying knowledge, this is no objection to armchair philosophy on the whole—nor even to that subset of it which is normative, nor even to that which is both normative and epistemic. One is not challenged *qua* analytic philosopher by engaging with an argument that a particular subject matter is uninteresting.

I see no threat from arbitrariness against the value of studying knowledge. Furthermore, there is no generalized threat from arbitrariness about the ability of the armchair philosopher to focus her work in interesting areas. And, to summarize the conclusion of §§5-8, there does not seem to me to be any obvious way to press the arbitrariness critique in way troublesome for the armchair philosopher without reliance, as in Stich's original presentations of it, on the

²¹ Two quick examples of epistemological projects which do not focus on knowledge are (Wright, 1991, p. 88) and (BonJour & Sosa, 2003). In the latter (pp. 22-23), Bonjour even offers an argument (to my mind, a dubious one) that knowledge is not worthy of study.

²² See e.g. (Kaplan, 2003).

centrality of intuition. So rejecting centrality is an effective way of resisting the arbitrariness critique.

Conclusion

There is more than one ‘experimentalist critique’. At least two have developed in the literature over recent years—I have called these the defeater critique, and the arbitrariness critique. They ought to be clearly distinguished, indeed, other than making use of at least some of the same kinds of data, and targeting features of the same practice, they have little in common with one another.

In particular, questions about the epistemic role of intuitions in the methodology of armchair philosophy are irrelevant to the defeater critique. Experiments that demonstrate limits to human philosophical abilities have relevance to human philosophical practice, regardless of what roles intuitions do or do not play in that practice.²³

However, when it comes to the arbitrariness critique, the centrality of intuitions seems to play a much more important role. I considered several ways of attempting to advance the arbitrariness critique without assuming intuition centrality; none were found troublesome. So if, as suggested in §1, intuition centrality is false, then the armchair philosopher needn’t be worried by the arbitrariness critique.²⁴

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²³ My own view—not argued in this paper—is that the defeater critique can be resisted on other grounds: the data do not show that our limitations undermine our methodology.

²⁴ Thanks to Derek Ball, Yuri Cath, Juan Comesaña, Benjamin Jarvis, Carrie Jenkins, Aidan McGlynn, Ernest Sosa, Stephen Stich, and Brian Weatherson for helpful conversations, comments on earlier drafts, and pointers to relevant literature.

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