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Experimental Philosophy 2.0

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I recommend three revisions to experimental philosophy's 'self-image' which I suggest will enable experimentalist critics of intuition to evade several important objections to the 'negative' strand of the experimental philosophy research project. First, experimentalists should avoid broad criticisms of 'intuition' as a whole, instead drawing a variety of conclusions about a variety of much narrower categories of mental state. Second, experimentalists should state said conclusions in terms of epistemic norms particular to philosophical inquiry, rather than attempting to, for example, deny that intuitions produce justified belief. Third, experimentalists should acknowledge the limitations of the 'method of cases' model of philosophical inquiry, and expand their experimental work accordingly.

Keywords experimental philosophy; intuition; methodology; centrality; metaphilosophy

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In 2001, Jonathan Weinberg, Shaun Nichols and Stephen Stich published a now-famous study indicating that East Asians were more likely than Westerners to attribute knowledge in Gettier cases.[1] Within a few short years, experimental philosophy burst onto the academic scene like a grass-roots revolution. When early studies indicated a wide variety of unexpected sensitivities in intuitive judgment, X-phi's proponents called on philosophers to burn their armchairs like so many brassieres. As the discipline matured, however, the dramatic battle-cries gradually became difficult to maintain. The argumentative move from variation findings to a condemnation of intuition has turned out to be much less straightforward than early proponents of the 'negative' strand of the experimental movement suggested.

Some authors have noted that a wholesale rejection of intuition threatens to result in global skepticism (Williamson 2007). Others have claimed that experimentalist findings aren't obviously generalizable to the 'expert' intuitions of philosophers (Devitt 2011; Hales 2006; Ludwig 2007). Worse, some have questioned the assumption that philosophers treat intuitions as evidence in the first place (Cappelen 2012; Deutsch 2010). And finally, many of the more dramatic negative findings have failed to replicate, with quite a few recent studies indicating substantial *stability* in intuition (Adleberg et al. 2015; Nagel et al. 2013; Sarkissian et al. 2010; Seyedsayamdost 2015) — thus drawing even the initial premises of experimentalists' critiques into question.

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Meanwhile, much of the actual work in experimental philosophy has moved away from the 'armchair burning' model—according to a recent survey by Sytsma and Livengood (2015), only 3.9% of experimental philosophers identify solely with the 'negative' project. The remainder are interested in issues beyond criticism of intuition, including the use of experimental data to understand the psychology of philosophical cognition and/or to generate positive contributions to philosophical debates. It may seem to some that the heyday of the negative project has passed.

The issues facing the negative strand of experimental philosophy are genuine challenges, and opponents have been right to press them. They do not, however, imply that the negative project is doomed. A critical examination of philosophy's methods is still, to my eyes, sorely needed—and experimental data are still centrally relevant to such a task. What the challenges mentioned above do indicate, I'll argue, is that experimentalist critics of intuition are operating with a set of assumptions which oversimplifies the methodological questions to which their empirical findings give rise. I'll recommend a few simple revisions to experimental philosophy's self-conception—an 'experimental philosophy 2.0', if you will—which will enable experimentalist critics to largely evade the worries mentioned above. Insofar as these changes lead to a new perspective on philosophical inquiry, they can, I think, enhance the less negative strands of experimental work as well.

Consider the following statements describing the ambitions of the 'negative program' in experimental philosophy.

> Experimental philosophy challenges the usefulness of [appealing to intuition] in achieving justified beliefs. (Alexander, Mallon, and Weinberg 2010, p. 298)

> Experimental evidence seems to point to the unsuitability of intuitions to serve as evidence at all. (Alexander and Weinberg 2007, p. 63)

> Sensitivity to irrelevant factors undermines intuitions' status as evidence. (Swain, Alexander, and Weinberg, 2008, p. 141)

> [The conclusion of experimentalist argument is that] the judgments elicited by thought experiments do not provide warrant for assuming their content. (Machery and O'Neill 2014, p. xvi)

These characterizations display two features which are typical of negative experimentalists' portrayal of their sub-discipline. First, the methodological conclusions they describe are broad—commonly, they are stated in terms of the epistemological failings of 'intuition' as a whole. Second, they employ the concepts of standard analytic epistemology—conclusions are couched in terms of the justification, evidential status, or warrant of intuition-based beliefs.

Consider now some of the assertions made by experimentalists about philosophy's methods.

> We propose that to find the correct theory of reference, philosophers of language are committed to using what is sometimes called 'the method of cases'. (Mallon et al. 2009, p. 338)

> We advance philosophical theories on the basis of their ability to explain our philosophical intuitions, defend their truth on the basis of their overall agreement with our philosophical intuitions, and justify our philosophical beliefs on the basis of their accordance with our philosophical intuitions. (Alexander 2012, p. 1)

> It is difficult to overestimate the importance of the method of cases … while it is not the only method available to philosophers, it plays a very large role. (Machery and O'Neill 2014, p. xiv)

These reflect a third common feature of the dialectic of negative experimental philosophy—a model of philosophical practice in which the 'method of cases' looms large.

Experimentalist criticisms of philosophical methodology, then, have largely assumed (1) that the appropriate target of criticism is the class of mental states that fall under 'intuition'; (2) that the relevant question regarding such states is whether they produce some familiar epistemic status such as 'justification' or 'knowledge'; and (3) that philosophical method largely consists of the testing of theories against the 'intuition data' produced by consideration of cases. Other participants in the 'intuition debates' have typically shared this broad metaphilosophical picture.

None of these three assumptions is essential to the overall experimental philosophy research project. What's more, there is a fair bit of inherent plausibility to the rejection of each. Rejecting all three assumptions results, I'll argue, in a picture of experimental philosophy which is more or less untouched by several of the most troublesome objections the negative project currently faces.

One central feature of the metaphilosophical literature is its nigh-exclusive focus on 'intuition'. Outside of the experimentalist camp, one finds a cornucopia of theories analyzing intuition and its epistemological virtues; debates rage over whether intuition is best defined by its phenomenological character (Bealer 1998; Chudnoff 2013), its etiology (Ludwig 2007), and so on. It is in part this very profusion of accounts that has led Herman Cappelen to suggest that 'intuition' is a defective theoretical term (Cappelen 2012). Elsewhere, Cappelen (2014) suggests that generating a definition of intuition that captures (even roughly) all-and-only the states that experimentalists study is likely impossible, leaving experimentalists without a characterization of their subject matter and hence without a workable meta-theory of their own practice. Timothy Williamson (2007), meanwhile, has suggested that the term 'intuition' is really nearly as broad as 'belief' or 'judgment', covering processes that underlie both philosophical and everyday cognition. This appears to pose a 'skepticism' problem for critics of intuition—reject intuition, and you reject a great deal of ordinary cognition too.

One might wonder why experimental philosophers *need* a characterization of their target in the first place. A discipline can possess legitimacy despite being wholly unable to meticulously define their subject matter, or even the overall nature of their field — philosophers should know this more than anyone. It might, however, be argued that proponents of experimental philosophy's 'negative project' *do* need a definition of intuition. These critics of traditional methodology urge us to abandon intuition, or at least dramatically restrict its use. Plausibly, anyone who urges us to reject or restrict some category of judgments x owes us an account of the boundaries of x.

But note that a small change in experimental philosophy's background theoretical framework would utterly avoid the problem. Simply put, experimental philosophy should be viewed as investigating a multitude of judgment types, rather than a single category of judgment called 'intuition'. Many of the quotes surveyed in section 1 express the wrong type of conclusion — namely, that empirical work casts doubt on the evidential status of *intuition*. Here is an example of a much better sort of conclusion: 'empirical work indicates that spontaneous moral judgments in response to such-and-so type of case exhibit sensitivity to emotional state'. This is a much less exciting conclusion, to be sure. But it clearly still has (at least potential) philosophical import.

Regardless, broader conclusions aren't likely warranted in the first place. The states or processes that fall under the term 'intuition' are presumably quite psychologically heterogeneous, which may in turn render different sorts of 'intuition' susceptible to different types and degrees of bias — logical intuitions, for instance, may be much less problematic than moral ones. These are empirical claims, of course, to be assessed through empirical studies. But the hypothesis that 'intuition' is heterogeneous strikes me as infinitely more plausible than the hypothesis that any given finding on any sort of intuitive judgment can be legitimately generalized to 'intuition' as a whole.[2]

Studies indicating that a given judgment displays bias, then, should cause us concern only about other judgments that are produced by *similar psychological processes*. Crucially, we may not immediately be in a position to determine exactly which processes are relevantly similar. We may not yet be in a position, then, to delineate the 'types' of judgment which are susceptible to bias.[3] Consequently, we may not yet be in a position to determine which of the judgments actually used by philosophers are problematic — if any. But that clearly does not show the findings to be irrelevant; it simply shows that more work needs to be done to determine their metaphilosophical implications. That further work is also largely empirical, and within the scope of the overall experimental philosophy research agenda. Abandoning focus on 'intuition' doesn't, then, threaten to neuter the experimental philosophy enterprise; it merely requires critics to give up on 'burning the armchair' in favor of a strategic cauterization of localized bits of upholstery.

Abandoning the broad-target approach has certain immediate benefits — it defuses certain worries that might arise from stability findings mentioned in the introduction, for instance. The fact that subjects across cultures judge moral responsibility to be incompatible with determinism (Sarkissian et al. 2010) doesn't show that 'intuition' is stable, any more than findings on emotional biases in moral judgment show that 'intuition' is *un*stable.[4] It also plausibly enables experimentalists to largely avoid the

accusations of self-defeat that have occasionally appeared in the intuition literature; even if we concede to folks like George Bealer (1992) and Joel Pust (2000) that anti-intuition arguments inevitably rely on intuitions in their premises, this only raises problems for conclusions which call for the rejection of the very same psychological processes that were invoked by those premises. No self-defeat arises from relying on epistemological intuitions in a critique of moral intuitions, or intuitions about reference, or intuitions about consciousness.

The narrowing of our targets does not, unfortunately, immediately suffice to answer the worries raised by Cappelen and Williamson. Consider the 'skepticism problem' mentioned in the previous section. Williamson (2004, 2007) argues that 'philosophical' intuitions likely rely on the same kinds of psychological processes as 'everyday' judgments about knowledge, morality, and so forth. Now, suppose an experimentalist were to draw on Swain, Alexander, and Weinberg (2008) findings on order effects in Truetemp judgments to criticize use of (say) 'judgments of knowledge attribution that proceed via the same psychological processes as the judgments examined in the study'. That class may well include lots of cases of 'everyday' knowledge attribution. A rejection of this class of judgments, then, might still be rather 'skeptical'.

There is, however, another small change to experimental philosophy's self-conception that will avoid this issue. As noted in section 1, experimentalist critics (and their opponents) standardly employ the concepts of mainstream analytic epistemology in debate; it's common to cast the experimentalist conclusion as a claim that intuitions are 'not evidence' or 'not justified'. But note that this is very much *not* how an analogous methodological criticism would proceed in the sciences. Scientists clearly find unaided, uncontrolled observation to be insufficiently rigorous for the purposes of experimental data-gathering; hence the use of measuring instruments, double-blinding, and so forth. It is appropriate to criticize a researcher for neglecting the use of any such procedure. What's more, if we imagine ourselves at a time prior to the invention of (say) double-blinding, but posterior to the recognition that experimenter expectations frequently bias research, it would be wholly appropriate to express concern about the legitimacy of then-current scientific practice. It's obvious that this can be done without, for example, claiming that observation fails to provide justified belief.

No one expects the layman to employ the stringent procedures of science during ordinary instances of perceptual observation; the layman need not, say, run statistical analyses before drawing a generalization from experience. One potential explanation for this asymmetry is as follows: scientific inquiry is simply subject to ultrastringent epistemological standards that far exceed what we expect from everyday cognition.[5] Such elevated standards make sense—individual cognizers in everyday contexts are subject to constraints on time, available resources, and so forth which simply do not apply to the activities of the scientific community as a whole. Scientific rigor is overdemanding with respect to an individual layman's everyday cognitive activities; not so for the inquiries of the scientific community. If this is right, then it's at least potentially arguable that similar considerations hold for philosophy. Philosophers may well

be epistemically obligated to meet higher standards than those required for ordinary justification.

Suppose, then, that experimental philosophers separate methodology from epistemology; instead of claiming that intuition fails to yield justification or evidence, experimentalist critics might instead claim that intuition (at least as currently used) fails to meet the rigorous methodological standards to which philosophers ought to ascribe. When experimentalist conclusions are restated in this way, there is no 'skepticism problem'. Skepticism involves claims about whether or not we possess knowledge. Critics of intuition aren't in that line of business, any more than early proponents of double-blind methods were. Critics of intuition are in the business of evaluating our adherence to the standards appropriate to philosophical inquiry—which may be quite different from the standards that must be met to qualify as knowing. Note too that this move avoids the complaint by Ernest Sosa (1998, 2007) that the sorts of epistemic deficiencies observed in intuition are no more severe than those observed in perception; Sosa notes that such deficiencies do not prompt us to abandon perception. Just so—but they do present a challenge to the use of unaided and uncontrolled perception in science, and *mutatis mutandis* for intuitions in philosophy.

Thus far, I've recommended: (1) that experimentalist critics shift their focus from 'intuition' to a variety of much narrower categories of mental state; and (2) that critics state their conclusions in terms of methodological norms appropriate to philosophical inquiry, rather than in terms of the standard categories of analytic epistemology. But again, these two changes turn out to be insufficient to address certain criticisms of the negative project.

One worry that has been insufficiently addressed is the 'anti-Centrality' argument pressed by Deutsch and Cappelen. A move away from 'intuition-talk' doesn't really address the underlying issue—after such a move critics would no longer *describe* themselves as studying 'intuition', but this is irrelevant if the states they *are* studying are not treated as evidence in philosophy. Both Deutsch and Cappelen make just such a claim. Philosophers, they note, do not generally rely on brute appeals to intuition; they provide *arguments*.[6] Deutsch (2010), for instance, points out that Gettier argues that the 'accidental' or 'lucky' nature of the justified true beliefs in his counterexamples disqualify them as knowledge.

As noted earlier, experimentalist critics have largely operated with a 'method of cases' picture of philosophical methodology—according to which, roughly, a theory's implications about some phenomenon p are tested against the 'intuition-data' which results from consideration of the presence or absence of p over a number of 'cases'. Regardless of whether one finds Cappelen's and Deutsch's examples convincing, the method of cases model clearly fits quite poorly with a large proportion of what philosophers actually *do*. Many (possibly most) philosophical arguments are not even remotely plausibly characterized as 'checking a theory against intuitions about cases'. Consider, for instance, the generality problem for reliabilism (Conee and Feldman 1998), or Fodor's arguments for the Language of Thought hypothesis (Fodor 1975).

Experimentalists should, then, concede that the method of cases model is overly simplistic and that the current experimental focus on thought experiments is too narrow. This may lessen the impact of certain existing studies, but it clearly does not invalidate negative experimental philosophy as a research project. First, there is nothing preventing experimentalists from applying empirical methods to other forms of philosophical cognition. An experimentalist might study, for instance, the cognitive processes that lead us to endorse the premises of the arguments we employ in our philosophical reasoning. Or, she might investigate whether the persuasiveness of the arguments themselves is sensitive to factors such as cultural background or emotional state.

Second, even if argument is what justifies our acceptance of philosophical conclusions, 'intuition' may play a problematic role in the 'context of discovery'. For any given philosophical problem, there tend to be good arguments for multiple different solutions. The intuitiveness of a proposition P may well lead philosophers to develop arguments in support of P rather than its alternatives; in other words, philosophers may be subject to confirmation bias. If the relevant 'intuitions' are, for instance, culturally local, then it seems we still have cause for concern — cultural bias may persist even in the presence of argument. As argued above, justified belief is plausibly compatible with a failure to meet certain stringent epistemic standards to which philosophers ought to hold themselves — even possession of a justification-granting argument, then, does not suffice to show that a philosopher has successfully eliminated potential sources of bias nor that she has fulfilled her epistemic obligations with regard to her philosophical theorizing.

Finally, accepting that the method of cases model oversimplifies philosophical practice need not commit one to denying that philosophers ever baldly appeal to bare, unadorned intuition. Cappelen and Deutsch provide some indication that a few of the most famous and successful examples of philosophical work employ somewhat more subtle methods than brute intuition-mongering. But my suspicion is that a selection of papers from slightly less lofty sources would be much more likely to display the sort of 'dig-in-your-heels' appeal to intuition that experimentalist critics often target. This is of course an empirical hypothesis — one whose investigation, I'll note, clearly fits within the scope of experimental philosophy as I've characterized it in this paper.

Note that embracing a more nuanced model of philosophical method additionally gives the experimentalist critic leverage against the 'expertise defense' — that is, the claim that philosophers possess special expertise which inoculates them against the biases and deficiencies that 'folk' intuition displays. The existence of said expertise is never demonstrated empirically; instead, proponents claim that we are entitled to assume its existence by default, with the burden of proof resting on the experimentalist to demonstrate the *in*existence of expertise. The most common characterization of this expertise is in terms of improved intuition (see e.g. Hales 2006; Devitt 2011); and to some degree, then, this move is already rendered problematic by the discussion thus far. A workable 'expertise defense' would require motivating a myriad of different expert capacities corresponding to the differing sorts of judgments involved in 'intuitive' cognition.

Once we have abandoned the oversimplistic 'method of cases' model of philosophical activity, a generic one-size-fits-all claim to 'expertise' becomes even more implausible.

The 'default assumption' must now be held to licence not only multiple different expert intuitive capacities, but also a number of different expert skills corresponding to the nonintuition-based aspects of our methods. Surely, we are entitled to a default assumption of *some* form of expertise among professional philosophers; but we are certainly not entitled to assume *every* specific form of expertise that would be needed to address the bevy of epistemological deficits that experimental work might uncover. Such permissiveness in our attributions of expertise would license, for instance, the assumption that scientists have some special expertise that obviates the need for double blinding. Once we recognize the complexity of the cognitive processes that underlie philosophical reasoning, the mere fact that we do philosophy seems woefully insufficient reassurance that all our various methods pass muster.

I have, admittedly, only gestured toward the sorts of responses one might give to the various extant critiques of the 'negative project'. What's more, the alterations I have suggested to experimental philosophy's background assumptions have been hinted at, but not fully developed — one would need extended argument to do justice to the claims that intuition is heterogeneous, that philosophy is governed by extra-stringent epistemological norms, or that the method of cases captures at best a minority of philosophical work. Yet, I hope the following general moral has been sufficiently motivated: the effectiveness of many existing critiques of 'negative' experimental philosophy is dependent on certain features of the metaphilosophical framework to which participants on both sides of the debate have explicitly or implicitly ascribed. By and large, experimentalist critics have not sufficiently considered the available alternatives to that framework; yet relatively small changes in perspective may well have the potential to wholly transform their aims, methods, and conclusions for the better.[7]

The changes I've recommended are ones I would urge not only on 'negative' experimentalists, but on anyone interested in the structure of philosophical inquiry — including more positive experimentalists[8] as well as nonexperimentalist participants in the metaphilosophical literature. The assumptions I have targeted appear, after all, on all sides of the debates. These assumptions not only obscure the potential importance of a critical, empirical investigation of our own methods; they also perpetuate a picture of philosophical methodology that I find to be oversimplistic at best. Changing our perspective on that methodology would do more than just enable methodological critiques. It would, I think, clarify all types of investigation into philosophical cognition.

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Notes

1 See Weinberg, Nichols and Stich (2001).

2 Note that the category of intuition might still be relevant for a variety of other epistemological projects. Epistemologists do focus on the heterogeneous category of 'perception', even though the processes falling under it are dissimilar in their susceptibility to various truth-irrelevant factors (e.g., poor lighting conditions impair vision more than taste). Note too that for a critique of, for example, particular observational methodologies within the sciences, narrower categories are clearly more useful.
3 I do think we can comfortably predict that they will be fairly narrow, perhaps along the lines of 'knowledge attributions on cases involving epistemic luck'.
4 Many of these studies are problematic to proponents of the negative project not only because they indicate stability, but also because they serve as failures of replication for the core findings upon which the empirical critique was built. It's an open empirical question whether any substantive findings of bias will prove robust in the long run—one that can't be settled here. In the meantime, the conceptual and methodological changes under discussion here can, at least, go some way towards clearing the philosophical tangles that currently clutter the laboratory.
5 At least, *certain* claims made in scientific contexts are plausibly subject to said ultra-stringent standards. For some claims, the relevant standards are plausibly the same as those governing everyday cognition—consider for instance mundane claims about the gender breakdown of one's experimental participants, or descriptive claims about one's experimental procedure. Plausibly, the norms regulating scientific activity are varied and complex; the same is likely true for the norms that do (or should) govern philosophical inquiry.
6 A further aspect of anti-Centrality arguments is opposition to 'psychologization'—the idea that philosophical evidence consists of (e.g.) intuitions about knowledge rather than *facts* about knowledge. This is separable from the point about the philosophical practice of using arguments. It is also fairly innocuous—scientific evidence consists of facts about observed phenomena rather than observations, but this does not show that the biases of perception are irrelevant to scientific methodology.
7 An anonymous reviewer wonders if the research project resulting from these changes would still warrant the name 'experimental philosophy'. And perhaps it would not. My primary interest is in formulating a conceptual framework that would enable a critical empirical examination of philosophical methods; if said examination is more of a 'successor project' than an incarnation of experimental philosophy, then so be it.
8 In fact, many positive experimental studies already draw the fairly narrow conclusions I have recommended—targeting, for example, the psychology underlying judgments of free will, or of attributions of consciousness, rather than the psychology of 'intuition'.

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