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On Philosophical Intuitions

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Graduate Program in Philosophy

A thesis submitted in partial fulfillment of the requirements for the degree in Doctor of Philosophy

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(Thesis format: Monograph)

by

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Abstract

I will argue that the scientific investigation of philosophical intuition ('experimental philosophy') is of philosophical interest. I will defend the significance of experimental philosophy against two important types of objection. I will term the first objection 'eliminativism' about intuitions: roughly, it is the claim that philosophical methodology does not in fact rely on intuition, and thus experimental philosophy's investigation is ill-conceived—in the words of one such opponent, 'a big mistake.' I will then consider a second objection, the 'expertise' defense. The expertise defense argues that the expert intuitions of professional philosophers are distinct, and to be preferred to those of the 'folk.' Against the eliminativists, I will argue that an ineliminable mental component remains that can be subject of fruitful empirical investigation. Against the expertise defense I will argue that, at least in the context of philosophy of language, expertise is itself a potential source of bias. Since expertise is domain-specific, however, a general rebuttal will not be given. I will conclude that experimental philosophy has much to contribute.

Keywords

Acknowledgments

for jenn and wren

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Chapter 1

1   Introduction

1.1   Overview

In this dissertation I will argue that the scientific investigation of philosophical intuition ('experimental philosophy') is of philosophical interest. I will defend the significance of experimental philosophy against two important types of objection. I will term the first objection 'eliminativism' about intuitions: roughly, it is the claim that philosophical methodology does not in fact rely on intuition, and thus experimental philosophy's investigation is ill-conceived—in the words of one such opponent, 'a big mistake.' I will then consider a second objection, the 'expertise' defense. The expertise defense argues that the expert intuitions of professional philosophers are distinct, and to be preferred to those of the 'folk.' While I will respond to eliminativism in general, my brief against the expertise defense will remain confined to the philosophy of language; this is because the objection must be considered contextually, as I will show.

I will first provide an overview of experimental philosophy and the use of intuitions in philosophy, and then present a *prima facie* case for experimental philosophy's importance (chapter 2). I will then consider the eliminativist objection and argue that the objection fails (chapter 3). I will then turn my attention to the expertise defense in the context of the philosophy of language and argue that it, too, does not succeed (chapter 4). I will conclude that experimental philosophy is relevant to philosophy (chapter 5).

In some more detail: In chapter 2, I will present a *prima facie* case for the significance of experimental philosophy. If intuitions are used in the course of philosophical theorizing, then we should wish for some account of this source of evidence in the specific form of an explanatorily adequate theory of the cognitive processes underlying intuition. Such a theory, I will claim, is interesting in its own right, separate from philosophical reliance on intuition as evidence: there has been a long-standing disciplinary interest in descriptively adequate psychology, which in turn affects philosophical theorizing in several fields. Given that philosophers *do* rely on intuitions, our interest is compound: experimental

investigation can help us assess the reliability, diversity, stability, and sources of intuition, allowing for better and more confident theorizing.

Nothing turns on whether or not 'philosophical intuitions' are a distinct kind of judgment, or constitute a motley. Rather than defining 'intuition,' I will point to examples in the literature, saying only that *this* is what we wish to study, whatever it turns out to be. There will be a common thread across the examples presented, however: an agent has intuitions, and nothing can replace the agent's having them (say, observation or experiment). This ineliminable mental component is enough to anchor the experimental project, even if uses of intuitions across philosophical subfields (language, logic, value theory, epistemology, and so on) turn out to be radically different from each other.

Experimental philosophers have approached their investigation in two distinct ways. First, there is a metaphilosophical / methodological project that is concerned with the reliability of 'intuition' and intuitive evidence in the context of philosophical theorizing. The claim is that contemporary philosophical methodology relies on intuitive response to imagined cases, and some principled account of this source of evidence is required. Some experimentalists draw a sharply negative conclusion and contend that reliance on 'armchair intuition' is to be abandoned on the basis of the variability and instability of intuitive response. Others contend that the patterns of intuitive response are themselves important to theorizing.

This leads us to the second aspect, the 'positive program,' essentially a form of philosophically-inspired cognitive science. Experimental philosophers working in this vein should be understood as providing an explanatory account of the cognitive processes underlying intuition and judgments of conceptual application. The focus here is distinctly not metaphilosophical, but simply philosophical: it is claimed that fine-grained characterization of intuitive response is not only of intrinsic interest, but will lead to philosophical insight by making more explicit the factors driving intuitive judgment.

Thus, as appeal to intuition is an important component of contemporary analytic methodology, a scientific understanding of intuitive faculties is in part motivated by a desire to understand and justify the methodology. But the experimental project goes

beyond this: far from being solely a metaphilosophical concern, experimental philosophy also directly contributes to positive theorizing in several areas of philosophy. This dual aspect—one methodological, one positive—is essential to understanding experimental philosophy's philosophical ambitions.

Two broad types of objections are pressed to experimental philosophy. The first type of objection, what I termed eliminativism about intuitions, seeks to minimize any putative evidential role of intuitions in philosophy: if intuitions are not in fact part of philosophical methodology, there is nothing for experimental philosophers to do that is philosophically significant. The second type of objection acknowledges reliance on intuitions as evidence, but affirms the reliability of expert intuitions in philosophy. In this case, too, there is no need for the investigations of experimental philosophers.

In chapter 3 I will critically discuss Herman Cappelen's *Philosophy Without Intuitions,* Timothy Williamson's *The Philosophy of Philosophy,* and a pair of papers by Max Deutsch that press distinct eliminativist arguments.[1] I will argue that 'eliminativism' about intuitions typically defines 'intuition' out of existence, only to surreptitiously replace it with some equivalent notion that itself is sufficient to motivate experimental philosophy's methodological and positive projects. To the best of my knowledge, no one has of yet identified 'eliminativism' as a distinct challenge to experimental philosophy, or grouped together these critiques under one label.

In particular, both Cappelen and Deutsch argue that experimentalists have assumed that intuitions play a foundational role in philosophical argument: concordance with intuition, they claim, has been put forward by the experimentalists as a necessary criterion for philosophical adequacy, the only way argument can proceed. The eliminativists find in practice no such foundational reliance. Indeed, conclusions putatively supported by

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[1] Herman Cappelen, *Philosophy Without Intuitions* (Oxford: Oxford University Press, 2012); Timothy Williamson, *The Philosophy of Philosophy* (Malden: Blackwell Publishing, 2007); Max Deutsch, "Intuitions, counter-examples, and experimental philosophy." *Review of Philosophy and Psychology* 1, no. 3 (2010): 447-460; Max Deutsch, "Experimental philosophy and the theory of reference." *Mind & Language* 24, no. 4 (2009): 445-466. I will use shortened citations from here on.

intuition can in fact be supported by argument. The urgency and importance of experimental philosophy, however, turned on this foundational reliance on intuition. Without it, experimental philosophy appears to be wasted effort.

I will dispute this picture. Experimentalists themselves typically view the methodological role of intuitions as substantially weaker than the eliminativists contend. Concordance with intuition and the deliverances of intuitive evidence are more often understood as a (defeasible) sufficient condition for adequacy. In such cases intuitions are deployed in the service of establishing a conclusion (by figuring as a premise in an argument). But intuitions are also pressed into service in other, more indirect, ways. They frame issues; raise a puzzle or apparent conflict; give us insight into what is conceivable and not; or simply provide data points that, once interpreted, guide and constrain theorizing. These uses of intuition do not fit the model proposed by eliminativists. Yet, as I will argue, all these uses are more than enough to justify the investigations of experimentalists.

Williamson's metaphilosophical claims will also come under scrutiny. Cappelen adumbrates several key points of the former's *The Philosophy of Philosophy* during the course of his argument, and I will challenge these. Williamson rejects conceptual analysis, arguing instead that philosophical questions are about 'things themselves.' By sidestepping the issue of conceptual analysis entirely, Williamson can further argue that what we have termed 'intuition' is in fact ordinary counterfactual evaluation, no less reliable in the case of philosophy than its applications in our daily lives.

In response, I will argue against both these claims: first, it is not clear that the kind of philosophical questions Williamson points to are only about things themselves; some conceptual mediation is inescapable. Second, even if there were no distinct faculty of 'intuition,' the reliability of garden-variety counterfactual evaluation is not sufficient to ground such evaluation in the context of philosophical theorizing, where the epistemic standards are higher and the cases more complex, obscure, and fraught. A systematic account of counterfactual evaluation is more than warranted, and does not generalize to a form of global skepticism, as Williamson warns.

I will conclude, then, that intuitions can be scientifically investigated in virtue of the methodological role they play, despite the eliminativist objection. But suppose the eliminativists are right—or, for that matter, the negative wing of the methodological program—and that we do not (or ought not) rely on intuition as evidence. Does this leave experimental philosophy with nothing to do?

No. To the contrary, the 'positive program' is of importance to philosophy independently of any methodological reliance on intuitions by philosophers. Thus, even if the eliminativist argument succeeded in showing that intuitions are not part of the methodological toolkit of philosophers, achieving 'explanatory depth' in characterizing patterns of pre-theoretic judgment is of intrinsic philosophical interest. It is crucial that this theorizing is not a form of 'conceptual analysis' by other means. The goal, rather, is to gain an understanding of what underlies 'intuition' by studying specific effects. To this extent the significance of experimental philosophy is not solely linked to philosophical use of intuitions-as-evidence, and the eliminativist challenge can be met even if we grant their own conclusion.

In chapter 4, I will turn to a rather different objection: the expertise defense. Some experimental philosophers concerned with the methodology of intuitions worry about variation and instability in intuitive response. Absent a principled means of selecting the 'correct' intuition among intuitive diversity, it is claimed that philosophical theories which have relied on variable and unstable intuition are undermined: why should we prefer the intuitions of professional philosophers?

The expertise defense answers this by affirming the basic reliability of the appeals to intuition when conducted by relevantly-trained experts, that is, philosophers. Experimental philosophy's methodological program misses the mark in this respect: by conducting experiments on non-experts, the data generated, however interesting, does not speak to the question of the reliability of intuitions in philosophy. Nor does it pose a challenge to appeals to intuition by philosophers. It is, rather, data about folk psychology.

The expertise defense is primarily directed at experimental philosophy's methodological program, and more particularly those within that program who are pessimistic about

appeal to intuition. However, it is also the case that if acquired expertise does provide a reason to privilege the intuitions of trained philosophers, there is every reason to suspect that the theories of the positive program will have marginal value to philosophers: the explanatory depth achieved will be about folk patterns of response, not expert response. In this way the expertise defense is a challenge to experimental philosophy's methodological and positive projects.

Responding to the expertise defense in a comprehensive way is difficult, and probably impossible. The reason is that 'expertise' is a tightly domain-specific notion. Experts in one area are not more reliable, in general, in areas outside their expertise. As a result, I will argue, expertise (and expertise defenses, plural) must be assessed on a case-by-case basis. Another complicating factor is that appeals to intuition in philosophy have few, and sometimes no, independent checks. This makes the specific context of argument all the more important.

Keeping these problems in mind, I will focus on cases drawn from the philosophy of language and discuss expertise in this context, drawing some general morals at the end. I will begin by describing experiments conducted on intuitions of reference based on a case due to Kripke taken to disprove descriptivist theories. Experimental philosophers have argued that variability in intuitions of reference undermines not just the anti-descriptivist case, but all theorizing about reference that appeals to intuition.[2]

The expertise defense in the context of referential intuitions is well-developed and serves as an ideal test-case. Friends of the expertise defense make two general moves: first, they offer an account of reliable intuition in terms of domain-specific acquired expertise.[3] Second, they explain the divergent, or 'incorrect' intuitions, in virtue of some aspects of the vignette or experiment design. In the case of reference, for instance, it is sometimes argued that the experimental set-up was ambiguous between speaker's and semantic

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[2] Machery et al., "Semantics, Cross-Cultural Style"; Mallon et al., "Against Arguments from Reference."

[3] I will most prominently discuss Devitt, "Experimental Semantics" and Ludwig, "The Epistemology of Thought-Experiments."

reference, or that the question posed capture folk theories of reference ('metalinguistic intuition'), not usage.[4]

Experimental philosophers have criticized both aspects of the expertise defense. In the case of the latter, criticism of experimental design has been met with attempts to replicate the results while controlling for possible ambiguity.[5] In the case of the former, experimental philosophers have argued that the notion of domain-specific acquired expertise is itself subject to empirical investigation. It has been suggested that expert intuitions are 'reinforced,' not 'reflective,' and that philosophical expertise is not analogous to other forms of expertise due to lack of clear, reliable feedback on which to train one's intuitions.[6]

While the responses are on the right track, in the specific case of semantic intuitions in the philosophy of language a better rejoinder can be made. I will argue that the expertise of philosophers is real, but that this expertise consists in the application of specific epistemic, logical and metaphysical commitments to semantics, yielding intuitions that are not descriptively adequate. Indeed, these commitments are normative: they are about the language we ought to have, not the languages we in fact have. For instance, the proponents of 'expertise' I discuss explicitly claim that predicates ought to be defined in terms of mind-independent theoretical significance, a view that has deep roots in the philosophy of language (via the seminal work of David Lewis).[7] It is also claimed that thought experiments in philosophy should operate under specific "idealizations about language" such that natural language predicates which appear vague are assumed,

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[4] Ichikawa et al., "In Defense of a Kripkean Dogma"; Martí, "Against Semantic Multi-Culturalism."

[5] Beebe, Undercoffer, "Individual and Cross-Cultural Differences in Semantic Intuitions"; Machery, Olivola, "Linguistic and Metalinguistic Intuitions."

[6] Machery et al., "Semantics, Cross-Cultural Style"; Weinberg, "Are Philosophers Expert Intuiters?"

[7] David Lewis, "Putnam's Paradox"; Weatherson, "What Good are Counter-Examples?"; Devitt, *Coming to Our Senses.*

instead, to be "semantically complete and consistent."[8] Likewise for theories of reference, which should be constrained by modal desiderata.

Yet philosophers of language critical of experimental philosophy also agree that the target phenomena of semantic theory are the norms and conventions of public language as practiced by other competent speakers.[9] I will argue that critics of experimental philosophy who endorse the expertise defense can't have it both ways: they cannot both import philosophically-inspired semantic norms and claim to describe the conventions of actual speakers. The norms imported reflect theoretical utility for various scientific and metaphysical purposes, I will argue, while the norms of competent speakers are (or, more precisely, *can be*) indifferent to these.

This latter claim is empirical. In support of it I will discuss work by James Flynn, who argues that 'kind-categorization' is just one of the things we do with language, a practice which requires training and education.[10] Evidence from IQ testing shows an increase in average test scores over the past century that can only be explained by a gradual change in cognitive norms that reflects the desiderata of such tests: abstract classification, causal relationships, and analytical thinking most prominently. Through training, our experience of what is intuitive has shifted. This, in turn, has a dramatic effect on the intuitions of philosophers of language, who receive more such training than the average speaker.

The notion of a 'natural kind' is tightly linked to causal theories of reference. Building on the work of experimentalists on reference I will extend my criticism of the expertise defense from natural kind predicates to the specific case of proper-name reference, which launched the discussion. I will argue that the philosophical intuitions supporting the causal theory are also normatively-governed, and that further empirical work is required to ascertain the actual referential practices of speakers. We need experiment, I will

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[8] Ludwig, "The Epistemology of Thought-Experiments," 134.

[9] Devitt, "Whither Experimental Semantics"; Ludwig, "The Epistemology of Thought-Experiments."

[10] Flynn, *What is Intelligence?*

conclude, to determine what conventions are actually in play and, more broadly, what is possible in language. I will suggest that an important direction of investigation in this regard will be semantic internalism, for the gap between philosophical norms and speaker conventions could plausibly be a gap between ideal theory and natural language / psycholinguistics. This would also give traditional philosophers of language and experimental philosophers distinct projects, which would resolve the dispute.

In the conclusion I will argue that experimental philosophy is likely to remain an important subfield of research in the years to come. The two objections I have considered ultimately do not succeed; and experimental philosophy's dual aspect, while intuition-focused, offers fecund prospects for further research. I will close with some general remarks on the investigation of intuition. There is no genuine consensus on what intuitions are. Despite this, competing accounts share a certain 'family resemblance.' I will argue that the features that make up the intuition 'family' allow for scientific investigation in almost all extant permutations in the literature across a wide variety of topics. The idea is not to abandon or replace intuitions, but to understand them, and integrate this understanding into philosophical research inasmuch as our intuitive judgments figure as an aspect of our methodology.

Chapter 2

2   The Prima Facie Case for Experimental Philosophy

Experimental philosophers, like most philosophers, believe that empirical facts can be relevant to philosophical theorizing in some non-trivial sense. What is distinct is three-fold: first, that experimental philosophers believe that science delineates a class of empirical facts of particular importance; second, that experimental philosophers carry out their own experiments rather than waiting on their colleagues in other disciplines; third, these experiments are (usually) directed at understanding "the internal psychological processes that underlie" conceptual application.[11] Thus, at first gloss, 'experimental philosophy' is philosophy that is not only guided by scientific data, but also *produces* said data in both a philosophically-inspired project of cognitive science and a metaphilosophical project.

In contemporary terms, this picks out a group of philosophers who began by critically examining reliance on intuition: this first generation, largely methodological and metaphilosophical in orientation, was spear-headed by Stephen Stich, Edouard Machery, Joshua Knobe, Shaun Nichols, Jonathan Weinberg and others, has now led to a remarkable proliferation of 'second-generation' work building on earlier success.[12] This second-generation work is often closer to philosophically-inspired cognitive science than 'metaphilosophy.' I will distinguish, then, between the 'methodological' and 'positive' projects:

The Methodological Project: Given philosophy's reliance on intuition, scientific assessment of the reliability of intuition is crucial to producing a defensible

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[11] Josh Knobe, Shaun Nichols. "An Experimental Philosophy Manifesto," 8.

[12] See, e.g., the collection of papers in Joshua Knobe, Shaun Nicholas, *Experimental Philosophy* and *Experimental Philosophy, Vol. II*; or the bibliography of Thomas Nadelhoffer, Eddy Nahmias, "The Past and Future of Experimental Philosophy" for 'first-generation' work. More recent work can be accessed via the Philpapers 'experimental philosophy' topic page.

metaphilosophy: "the ultimate hope," experimentalists write, is to "determine whether the psychological sources of [intuitive] beliefs undercut the warrant for the beliefs."[13]

The Positive Project: To provide an empirically adequate explanatory account of 'intuition' in terms of the "internal psychological processes" that "influence applications of a concept." The goal is not to "characterise the actual pattern of people's intuitions" but, rather, "achieve explanatory depth."[14]

What unites both projects is an ongoing scientific focus on 'intuition', broadly defined. The methodological project examines the use commonly made by philosophers of intuitions. The positive project examines patterns of intuitions with a deeper explanatory goal.

2.1   Some Examples of Intuitions in Philosophy

The roots of experimental philosophy can be found in a dawning suspicion of reliance on intuition in philosophy, viz., what I've termed the 'methodological project.' Appeals to intuition, the story goes, figure prominently in philosophical argument: a case or scenario is described and considered, generally asking "what would we say *if*...?" and a spontaneous 'intuitive' judgment is issued, typically (but not necessarily) a judgment of 'conceptual application.' We might intuitively judge that a hypothetical case is not an instance of 'knowledge' or of genuine 'intentionality.' But what is intuition? And how is it used in philosophical argument?

It is difficult to define 'intuition' in an uncontroversial way. Accounts vary and conflict: some claim intuition is a *sui generis* faculty that plays a central and indispensable role in *a priori* philosophical methodology; others deny that 'intuition' is anything distinct from ordinary judgment; some deny that philosophy has ever relied on 'intuition.' I will look at some of these controversies below. For now, rather than wade into complex

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[13] Joshua Knobe, Shaun Nichols. "An Experimental Philosophy Manifesto," 7.

[14] Ibid, 5.

definitional disputes, I will provide some examples of the use of intuition in philosophical argument.

The underlying methodological assumption made by experimental philosophers is that many philosophical theories are grounded in philosophers' response to imagined cases. Most often, a situation or scenario is described with stipulated features meant to elicit an 'intuition' or judgment in the reader about the applicability of a concept in that situation. In this way intuitions can both serve as counter-example to, and evidence for, philosophical theories. It is commonly held that a something's *being intuitive* carries evidential weight for a theory. If this is correct, then investigation into the psychological processes that underlie intuitive response is of significant interest.

Intuitions can be deployed in several different ways in the course of philosophical argument. They can demonstrate some truth. They can provide a counter-example to a theory. Finally, they can provide evidence for a theory. I will provide an example of each.

First, intuitions might demonstrate some truth: intuitions about 'Twin-Earth' cases are taken to show that externalism is true of at least some mental content. Considering the case *shows* us that there is more to meaning than 'what's in the head.'

We are to imagine a planet just like ours in every respect---a 'Twin Earth'---with the exception that the liquid found in lakes, oceans, bottled and drank by inhabitants, etc., is not H_2O but some other chemical compound ('XYZ'). Further, imagine an inhabitant of our planet and his perfectly identical counter-part on Twin Earth. Both are using the term 'water' to talk about their respective liquids. Yet they do not mean the same thing: intuitively, an inhabitant of Earth refers to H_2O, and an denizen of Twin-Earth refers to XYZ.[15] There is nothing 'in the head' of either agent to distinguish between their concepts (suppose the year is 1750, prior to modern chemistry): yet their concepts *are* intuitively different, showing that 'meaning' is individuated by more than mental content. Causal

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[15]Hilary Putnam, "The Meaning of 'Meaning'" 139-141.

relations to the external environment matter, as reflection on the imagined case demonstrates.

Crucial to this case is an appeal to intuitive facts about 'use.' While the Twin Earth case purports to show that "the meaning of 'water' *as used by Mary* is externally individuated in terms of environmental features," the claim about use is at least as important as the claim of external individuation.[16] It is possible that a speaker could use 'water' as a functional term that covers both H_2O and XYZ; that we in fact do not is a claim that requires an intuition about the status of 'water' as a natural kind term while reflecting on Twin Earth.

This bears repeating. There surely are possible worlds where a competent speaker could use 'water' (or any term!) to refer to *any* stuff with water-like properties. For such speakers, 'water' is not a kind term with a chemical essence, but a functional or cluster term, like 'paint' or 'rubber.' This possibility is admitted by all parties to the discussion: it just so happens that this possibility is not actual.[17] *We* use 'water' as a natural kind term; and because we do, the meaning of 'water' is individuated in virtue of external, environmental features. How do we know that 'water' is so used? Well, reflection on Putnam's case brings this fact to the fore: in other words, we have an intuition (about what we intend with the term 'water'). This aspect simply *cannot* be removed: reflections on linguistic intentions are necessarily psychological. In particular they are not *social*, even if there is social coordination of public meaning. The *methodology* of a case like Twin-Earth requires individual reflection on what convention has been adopted, and consideration of what this implies in an imagined case.

Second, intuitions can accentuate the negative: Gettier cases tell us that accounts of knowledge as 'justified true belief' are wrong or incomplete. The scenario presented in a

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[16] Jesper Kallestrup, *Semantic Externalism*, 60. The emphasis is mine.

[17] The counterfactual's possibility is indeed explicitly endorsed: "If it should turn out that only philosophers baulk at classifying XYZ as water, I am ready to defer in my usage to the non-philosophical majority and say that 'water', like 'glue', is not the name of a kind with a chemical essence." (Christopher Hughes, *Kripke: Names, Necessity and Identity*, 63.)

Gettier case serves as a counter-example to an analysis, but it does not demonstrate some positive fact (as in the Twin Earth example above). Again, reliance on intuition is crucial. The scenario asks us to imagine an entailment of a justified false belief. The entailment, by chance, happens to be true. Most philosophers agree that belief in the entailment is not a case of 'knowledge,' even though the imagined subject's belief seems to satisfy the conditions of the justified true belief account.

Suppose, for instance, that Jane acquired an autographed copy of David Foster Wallace's *Infinite Jest* from a reputable bookseller that vouchsafed its authenticity. As it happens, the signature is a clever forgery; nevertheless, Jane is justified in believing "I own an autographed copy of *Infinite Jest.*" She explicitly believes, too, the further proposition "I own at least one autographed book" on this basis. As it happens, her copy of Cormac McCarthy's *The Crossing* is autographed unbeknownst to her. Jane's belief that she owns one autographed book is both justified and true; but, most philosophers agree, she doesn't *know* she owns at least one autographed book. Therefore, justified true belief is not sufficient for knowledge, proved by counter-example.

One interesting feature of the Gettier scenario is that it is not 'far-fetched' in the way sometimes derided by critics of intuitions.[18] Timothy Williamson is fond of remarking that the Gettier scenario can be brought about trivially in real-life situations:

> The modal element in Gettier cases is not even essential. For Gettier cases have actually occurred. Just to make sure of that, I recently brought one about when giving a visiting lecture on intuitions.[19]

But there is no advantage to bringing about a 'live' Gettier case during a visiting lecture, or in any other circumstance. It is not as if, by making a Gettier scenario actually occur, one could then independently verify the presence or absence of 'knowledge.' No apparatus or instrument could detect 'knowledge': as in the Twin Earth case, a agent must judge. As Williamson notes, "a real-life Gettier case makes almost no difference to the

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[18]Dennett has been particularly forceful on this, e.g., Daniel Dennett, *Intuition Pumps…*

[19]Timothy Williamson, "Armchair Philosophy, Metaphysical Modality, and Counterfactual Thinking," 12.

epistemic status of the result that justified true belief is insufficient for knowledge," result which, for Williamson, rides on a general capacity for agents to handle counterfactual conditionals. Williamson denies that such judgments are 'intuitions' inasmuch as intuitions are understood as special or *sui generis*; there is nothing distinct about 'intuition' in "any sense that would distinguish it from other judgements."[20]

Williamson's particular brand of eliminativism can be contrasted with the view that philosophical intuitions do indeed form a distinct *kind* of judgment, which, for epistemic and methodological purposes, can be treated together. The Stanford Encyclopedia of Philosophy's entry on 'intuition' claims with no further ado that "it is plausible (and will be assumed here) that the intuitions of interest in philosophy constitute a single epistemic and psychological kind," which would then bring together moral, logical, epistemological, semantic, modal, and metaphysical intuition under one tent.[21]

Yet, for experimental philosophy nothing rides on whether or not intuition is *sui* generis and a 'kind' or not. What *is* important for our present purposes is that *some* mental phenomenon underlies evaluation of the case, and nothing else (no possible independent instrumentalization, and no interesting epistemic distinction between imagined and real cases). It is the characteristically *mental* aspect of 'intuition' that allows experimental philosophy some purchase: it takes an agent to intuit, and the agent cannot be substituted. This is why, in the end, Williamson, Cappelen and Deutsch's attempts at eliminativism will fail (chapter 3).

Often, this process of mental evaluation produces a seemingly *deductive result* that takes the form of a straight counter-example. By 'deductive result' I only mean the intuitive judgment issued is used as a premise appearing in reasoning of the 'deductive' form. So 'negative' use of intuitions is, roughly, a *modus tollens*: a theory *P* implies *Q*; intuitively, not-*Q*; therefore, not-*P*. We see this form of argument in the Gettier scenario: justified true belief (JTB) accounts of knowledge predict that Gettier cases are instances of

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[20]Ibid.

[21]Joel Pust, "Intuitions."

knowledge; but they are not. Therefore, the JTB account is false.[22] But this 'deductive form' is not an essential feature of the use of intuition, either.

Indeed, a third way intuitions have been used in philosophy is by providing a form of 'inductive' evidence for a theory's correctness: a theory's concordance with our intuitions is evidence that the theory is correct, a compelling-but-defeasible theoretical virtue. Such views are often expressed in philosophy of language and value theory; less often in analytic epistemology or metaphysics.

For instance, a recent discussion of the pragmatics of counterfactuals by Sarah Moss takes the reader through several cases of intuitively 'felicitous' and 'infelicitous' sequences of counterfactual sentences ('reverse Sobel sequences'): our intuitions balk, to varying degrees, at certain formulations but not so much others. It is particularly interesting that the problem discussed by the paper has to do with reversing the ordering of counterfactuals, resulting in an unexpected intuitive clash. Consider the pair of sentences

> (2a) If Sophie had gone to the parade, she would have seen Pedro.
>
> (2b) But if Sophie had gone to the parade and been stuck behind a tall agent, she would not have seen Pedro.

"Intuition," Moss affirms, "says that the counterfactuals in (2) can be true together."[23] However, if we reverse the order, intuitions change:

> (3a) If Sophie had gone to the parade and been stuck behind a tall agent, she would not have seen Pedro.
>
> (3b) #But if she had gone to the parade, she would have seen Pedro.

Intuitively, if (3a) is true, then (3b) can't be: we have what an experimental philosopher might call an 'ordering effect.' But extant theories of counterfactuals do not take order into account: it shouldn't matter which member of the pair we consider first. Intuitively,

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[22] Or, to take another example, descriptivist theories of reference predict that in the Gödel-Schmidt case, we refer to Schmidt; but, as Kripke writes, "it seems to me we are not. We simply are not." Saul Kripke, *Naming and Necessity*, 84. The logical form is another *modus tollens*.

[23] Sarah Moss, "On the Pragmatics of Counterfactuals" 562.

however, it does. For Moss, and indeed the literature she adroitly discusses, these sorts of intuitions are simply taken to constrain an adequate theory of counterfactuals: "It is a virtue of my theory that it fits the contours of our judgments," she writes, later summarizing that "my theory accounts for our intuitions about felicious and infelicitous reverse Sobel sequences."[24] A philosophical theory should account for our judgments; and our judgments, or 'intuitions', about cases, are the data points the theory must 'fit.' The theories Moss criticizes fail to account for our intuitions about reverse Sobels; hers does so account; and so her theory is the better. Importantly, when considering sets of complex cases, there are 'degrees' of naturalness, and we can always doubt whether judgments accurately 'signal' truth and falsity.[25]

Approaches such as Moss' are not unusual at all. In a similar methodological vein, Soames says of a clash between intuition and his semantic theory that

> The conflict between this intuitive response to examples ... and the semantic theses I have adopted is not to be taken lightly. On the one hand, the intuitions are persistent and widespread. On the other hand, the semantic theses I have adopted are highly motivated; it is not at all easy to see how they could be wrong...[26]

Soames' methodological proposal is to carefully investigate the 'sources' of the conflicting intuitions, and weigh them against the arguments presented for his semantic claims; we may find that the intuitions were ill-motivated in comparison to the conclusions they conflict with. Intuitions serve as an important source of information to the theorist: intuitive conflict or concord is evidence that a theory is, or isn't, on the right track. Crucially, the evidence is 'defeasible': a strong theory might lead us to 'revise' our intuitions, or to make a serious attempt to diagnose their source. Yet this 'defeasibility' is in turn very weak, as intuitions are taken as 'strong' evidence: perhaps not on the order of direct counter-example, but, as Soames claims, "not to be taken lightly," either.[27]

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[24]Ibid, 578.

[25]Ibid, 584.

[26]Scott Soames, *Beyond Rigidity*, 142. I've elided the details of the actual cases, which are complex.

[27]Soames is similar to Kripke, unsurprisingly: the latter famously opined that "something's having intuitive content" is a "very heavy *evidence*." (Saul Kripke, *Naming and Necessity*, 42, my emphasis). I will discuss

We've seen three ways intuitions are deployed in the course of philosophical theorizing: directly establishing a result; directly invalidating a result (by counter-example); and indirectly providing support for a result. In practice the distinctions are blurry. Twin-Earth cases could be interpreted as primarily showing that internalism is false (and the externalist's being right requires the enthymeme premise that *either* internalism or externalism is true, exclusively). The negative mode is certainly more prevalent, as intuitions lend themselves more readily to the elaboration of counter-examples than theories. Typically an intuition is elicited to show than an existing theory is incorrect, and following this a new or modified theory is elaborated that *is* intuitively acceptable. Mature subdisciplines tend to generate indirect modes of intuitive argument, as theoretical complexity rises and cases multiply. Often the transition from 'direct' to 'indirect' use of intuition in a subdiscipline is imperceptible. The classic Gettier case is a direct 'counter-example'; yet the current literature features a bevy of 'Gettier-like cases' that require fine-grained sifting of multiple intuitions in the indirect 'data-point' vein.[28]

The point, however, is that intuitions, whatever they are, *are* used in philosophical argument; and if we are not sure how to define them, we can always point to these instances as the thing we want to study, whatever it turns out to be in the end. Following, one might think, the 'causal-historical' mode of reference typical of scientific investigation: we wish to know about *that*. The examples serve as reference-fixing descriptions that may be ultimately dispensed with.

Further, as should be clear now, intuitions are evidently a *mental* phenomenon, however they are deployed. An agent has intuitions, and nothing can substitute for the agent

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later a paper due to Cumming, *Variabilism*, which establishes its conclusion in a similar way. I submit that this 'indirect mode' is by far the most common use of intuition, but it is also ignored by proponents of eliminativism.

[28] See John Turri, "Knowledge Judgments in 'Gettier' Cases," for overview and experimentally-oriented discussion.

(except, perhaps, many agents). For the purposes of philosophical *methodology*, it makes no difference whether or not the case is 'far-fetched' or trivially realizable: evaluation of the case requires an agent making a judgment in order to generate knowledge (or a knowledge-claim). Epistemically speaking we might legitimately worry that 'far-fetched' cases might tax our judgment, resulting in a higher failure rate—yet the *procedure* in both 'realizable' and 'far-fetched' cases is essentially the same.

It is important to remember that not *all* thought-experiments are such that the scenario described *requires* an agent's judgment to generate knowledge: an intuition might be sufficient in such cases, but not necessary. In many instances of 'physical' intuition we can substitute a real experiment for the 'thought-experiment' and *observe* to determine whether or not our intuitions 'match up' with the physics. A famous example is Galileo's 'falling objects' thought-experiment, which was conducted on the moon by Apollo 15 astronauts—confirming, once again, Galileo's 'intuition.'

It is sometimes claimed that philosophical intuition is really no different that the sort of intuition routinely used in the sciences. Putative similarities between thought-experiments in science and philosophy are belied by the fact that in many of the most famous thought experiments in philosophy no observable physical facts discriminate between the possible states of affairs the intuitions are speaking to, and this so even if the imagined scenario can be made to occur in 'real life.'

An example of a thought-experiment with no physical correlates is the logical possibility of phenomenal zombies: a zombie world is by stipulation not physically different from ours. Alternatively we might not be able to observe instead of intuit when the scenario is not feasibly realizable, such as Swamp-Man or Searle's Chinese Room.[29] Though in all these cases, as so with the Gettier or Twin-Earth, even if we could meet Swamp-Man, build a Chinese Room, or visit a supposed zombie world, there are no experiments that could determine whether or not the 'zombie' has conscious experience; whether Swamp-

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[29]David Chalmers, *The conscious mind*; Donald Davidson, "Knowing one's own mind"; John Searle, "Minds, Brains, and Programs."

Man refers to anything; whether the Chinese Room understands Cantonese. In such cases the 'intuition' is *necessary* for the generation of a knowledge-claim. Again, no position on the exact nature of intuition need be taken. It is enough that there are imagined cases in philosophy such that there is no substitute for an agent evaluating the case in order to generate a knowledge-claim.

In light of this, experimental philosophers have argued that, given the important role intuitions have played in philosophical theorizing, 'intuitions' (or whatever label we assign the mental phenomenon underlying judgment of this sort, *sui generis* or not) should be scientifically investigated to determine whether they are widely shared, whether they are reliable, what factors influence them, and what cognitive processes give rise to them. "This sort of question becomes especially pressing," experimentalists write, "in cases where the intuitions are actually serving as evidence for a particular philosophical view."[30] This is so because, should certain intuitions prove variable, unstable, or produced by unreliable processes, they cannot well support philosophical theories in the ways described above.

Recall that 'intuition' is the only method of generating a knowledge-claim in many of the cases considered by philosophers. Widespread variability and instability of intuitive response concerning some case should make us cautious about the epistemic status of intuitions about that case, and, in turn, theorizing on this basis. On the other hand, lack of variation and robustness is some evidence that the intuition is reliable. Note that the 'truth-maker' of the intuition isn't the fact that majorities have the intuition under experimental conditions. Rather, experiment would provide evidence for something's being genuinely intuitive. Hopefully, what makes an intuition correct is that it describes what is the case, and that intuitiveness tracks this somehow. Widespread variation and instability in intuition and intuitive response makes it harder to discern which intuitions are 'tracking' what is, in fact, the case.

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[30]   John Knobe, Shaun Nichols, "An Experimental Philosophy Manifesto," 8.

The major concern of the first phase of experimental philosophy was to conduct this sort of research, and we will look at some examples shortly. But, as I've emphasized, there is also the intrinsic interest of an explanatory account of intuition, independent of any metaphilosophical scruple; I will outline an example of the positive project in (*iv*), below. First I will make the *prima facie* case for the significance of experimental philosophy.

2.2   The Prima Facie Case

All this gets us the *prima facie* argument for the relevance of experimental philosophy. Earlier I divided up experimental philosophy in two related projects: one methodological, one positive. They are linked: the methodological project requires the input of the positive program. Conversely, the methodological project creates positive data. In light of this the *prima facie* case is an argument with a methodological conclusion, with the positive program appearing as a pair of premises (4 & 5). It goes like this:

> *Premise 1.* Philosophers use 'intuitions' as evidence to guide their theorizing.[31]
>
> *Premise 2.* These 'intuitions,' however we define them, are at the least a *mental* phenomenon: an *agent* has intuitions.
>
> *Premise 3.* Good theories include an account of their sources of evidence.
>
> *Premise 4.* Mental phenomena, including 'intuitions,' can (in principle) be accounted for.
>
> *Premise 5.* The best way to do so is by using normal methods of science.
>
> *Conclusion:* Good philosophical theories should include some scientific account of the source(s) of intuitive evidence they use.

We've already discussed why the first two premises might be true. It is easy to find examples of 'intuition' guiding philosophical theorizing; and, further, it is clear from the

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[31] While it may seem that only 'indirect' uses of intuitions are 'evidence,' we can adopt a wider notion of 'evidence' that also captures the notion of the 'intuitive counter-example.' Opponents of experimental philosophy certainly balk at this sort of formulation. It might be strange to say of a premise in a deductive argument that it is 'evidence' for the conclusion derived. But of course we must distinguish between validity and soundness and ask after the latter: the soundness of an 'intuitive' premise *is* an evidential matter.

examples discussed above that nothing non-mental could 'substitute' for an intuition. It takes an agent.[32]

The third premise I take to be an unassailable truism of the philosophy of science, one whose relevance to philosophical intuition has already been noted: "every scientific subdiscipline spends a good deal of effort identifying and correcting errors and artifacts" in their sources of evidence, as Robert Cummins notes, going on to suggest that there is no way to do so in the case of philosophical intuition.[33] A telescope's heavenly deliverances are likely reliable if, when pointed at local mountains we see familiar peaks, if we have a sound theory of optics, and so on. There being no equivalent procedure for intuition, Cummins argues that we must forgo the theoretical virtue of understanding this source of evidence, a significant drawback.

Yet perhaps, on this, he is incorrect: even if we cannot have "nonintuitive access to its [intuition's] targets," as he puts it, investigation into the mental phenomena of intuitive judgement could bear some fruit, as premise 4 asserts. Here the idea is to test for the robustness, universality, and sources of intuitive judgment, as an indirect means of ascertaining their reliability. To some extent this reflects existing disciplinary commitment: when professional philosophers are themselves split over a case, appeal to intuition falters.[34] Experimentalists propose to go further and not just catalog intuitive variation, but, in the context of the positive project, craft hypotheses that explain patterns of intuitive response.

The indisputable success of science in such areas, I think, justifies the fifth premise; ultimately, the proof is in the doing, and hopefully extant positive research will show that it has been worthwhile. These two premises, (4) and (5) are at the heart of the positive

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[32]Which is not to say that a philosophical conclusion that happens to be supported by an intuition cannot *also* be supported by argument.

[33]Robert Cummins, "Reflections on Reflective Equilibrium," 116.

[34]The Philpapers survey results highlights interesting cases of intuitive disagreement such as Newcomb's paradox or the possibility of p-zombies.

program, for they affirm the possibility of a study of "the internal psychological factors" that "influence applications of a concept."[35]

Thus I wish to affirm the *prima facie* case for experimental philosophy. It merely states that experimental philosophy's two projects are of *some* philosophical importance. Opponents have resisted, typically, premises 1 or 5.

Recall the *eliminativist* objection, which denies that philosophers rely on intuitions as evidence in the course of theorizing. It is then claimed that experimental philosophy is 'a mistake', because the significance of experimental *philosophy* was conditional on philosophical reliance on intuition. Some eliminativists claim that philosophical theories are not theories of intuition, or even of something's being intuitive, but of *a priori* truths. Intuition is *one* route to the *a priori*, but we should not mistake evidence with a source of evidence.[36] Thus there is no sense in studying 'intuition,' which could only give us *a posteriori* psychological facts about 'intuitiveness,' but would not tell us anything of philosophical significance.

Deutsch's argument, for instance, is that philosophy does not rely on intuition *because* its conclusions are *a priori*. Intuition qua mental phenomena is *a posteriori* psychology; but there is such a thing as *a priori* knowledge; Deutsch therefore concludes that experimental philosophy is attempting the impossible task of studying the latter via the former, resulting in an irrelevant folk psychology of concepts, poorly conducted to boot. Both aspects of experimental philosophy are wasted efforts.[37]

In defence of the *prima facie* case for experimental philosophy, I will argue that philosophy *does* rely on a scientifically tractable notion of 'intuition,' the study of which *is* relevant to philosophy both methodologically and positively. Note that this *prima facie* case can be stated independently of any 'sceptical' result about the use of intuition: should

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[35] Josh Knobe, Shaun Nichols. "An Experimental Philosophy Manifesto," 5.

[36] Max Deutsch, "Experimental Philosophy and the Theory of Reference."

[37] Others reject the methodological project but cautiously accept the positive program in a limited way: e.g., Timothy Williamson, "Philosophical Criticisms of Experimental Philosophy."

intuitions 'hold up' under empirical scrutiny, friends of intuitions would have occasion to celebrate, for this would increase confidence in philosophical theorizing—an obvious good. The *prima facie* case *only* states that it would count as a metaphilosophical virtue to have a scientific account of intuitive evidence.

Second, I will argue that the philosophical significance of the positive project is independent of any *philosophical* reliance on intuitions as evidence in theorizing: thus, even if 'eliminativism' was right about the metaphilosophical issue, there remains an interesting, substantive question about the nature and structure of pre-theoretic judgment. The positive project *contributes* to the methodological one to the extent that philosophy does use 'intuition,' broadly defined; but the interest of the former does not ride solely on that of the latter.

Before considering the objections outlined above, however, I will explain in some further detail what it is that 'experimental philosophers' *do* in practice. This context is necessary to understand the critiques to come.

2.3   What Experimental Philosophers Do

The immediate precursor to contemporary 'experimental philosophy' is Stich's 1988 *Reflective Equilibrium, Analytic Epistemology and the Problem of Cognitive Diversity.* In the paper, Stich argues that the set of cognitive practices privileged in analytic epistemology amount to an undefended ethnocentrism, for the relevant practices may well be different, and equally justified, in other cultures.[38] The deliverances of intuition, then, reflect contingent, culturally acquired practices, threatening the method of intuitive armchair analysis. In a footnote, Stich writes that

> Evidence on this point, like evidence about cross-cultural differences in cognitive processes, is hard to come by and hard to interpret. But there are some intriguing hints in the literature.[39]

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[38] Stephen Stich, "Reflective Equilibrium…"

[39] Ibid., fn. 13 p. 111.

Stich points to the Yoruba, a West African people, as a possible example: lacking a distinction between knowledge and true belief (viz., our concept of 'justification'), the Yoruba nevertheless distinguish between beliefs acquired by immediate experience and beliefs not so acquired: "the Yoruba categories of epistemic evaluation," Stich concludes, "are significantly different from our own."[40] The worry, then, is that intuition will simply reflect local categories, not a genuine 'kind.'[41] This argument contains the seeds of the 'methodological project' as I've defined it, for Stich's paper raises the specter of 'intuitive diversity' as a potential problem for philosophical reliance on intuition.

This worry, in turn, ultimately derives from psychological work by Rosch and others suggesting that we do not represent concepts and categories in terms of sets of necessary and sufficient conditions.[42] If this is so, 'intuition' may reflect local epistemic concepts that could very well differ in their specific 'genetic make-up' (metaphorically speaking) across the broad family of *possible* concepts that could be had. But, Stich acknowledges, more empirical evidence is required that this postulated 'genetic diversity' of epistemic concepts in fact exists. It is this reliance on 'scientific' data that distinguishes Stich's argument as an early form of 'experimental philosophy,' and the thought that philosophers could themselves produce the desired data was not far behind.

Stich re-wrote and expanded on this argument in his book T*he Fragmentation of Reason.* In a review by Alvin Goldman (a central target of Stich's) the following year, Goldman echoes Stich's concern over the relative paucity of evidence, writing that

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[40]Ibid., fn. 13 p.111.

[41]There is a great deal of research in linguistics about 'evidentials,' grammatical elements that indicate the nature of evidence for a given statement: one-quarter of the world's languages use some form of grammatical evidentiality, some quite complex, with five terms or more (Alexandra Aikhenvald, *Evidentiality*).

[42]Eleanor Rosch, Carolyn Mervis, "Family Resemblances." The classic 1975 article was re-printed in 1998's *Re-Thinking Intuition*. Experimental philosophers have conducted studies on 'psychological essentialism,' arguing that essentialist intuitions are "are culturally dependent, demographically variable, and easily malleable." Edouard Machery, et al., "Is Folk Essentialism a Fundamental Feature of Human Cognition?"

>...one wishes there were more definite evidence that there are cross-cultural differences in epistemic concepts. The one example Stich discusses ... is pretty inconclusive, a mere 'hint' as he himself puts it.[43]

Searching for this evidence was seen as the first task of experimental philosophy, in the context of a 'negative' metaphilosophical / methodological project that criticized appeals to intuition in philosophy. Cross-cultural studies in epistemology followed in due course, with a sharply negative metaphilosophical bent.

For example, 2001's *Normativity and Epistemic Intuitions* argues that there is no reason to think that intuitions have "real (as opposed to putative) normative force" in epistemology when it is not only possible, but actual, that epistemic intuitions differ widely between groups of people.[44] The article presented evidence of systematic differences in epistemic intuitions between samples drawn from Western and East Asian populations on several variations of the 'Truetemp,' 'Gettier' and 'Fake Barn' cases. The experimental philosophers predicted such variations would be present based on work from Nisbett demonstrating systematic differences in various cognitive tasks between these populations. This is what the experimentalists found: populations drawn from East Asia (EA) exhibited systematically different intuitive responses from Westerners (W). A smaller sample size drawn from the Indian subcontinent differed from both EAs and Ws.[45]

If epistemic intuitions differ, so will epistemic theories, for—as we've seen— intuitions are taken to constrain acceptable theories. So the experimentalists ask "why should we privilege our intuitions rather than the intuitions of some other group?"[46] What makes a theory we've intuitively derived 'correct' other than we've derived it from *our* intuitions? The authors argue that the 'intuition-driven romanticism' of analytic epistemology

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[43] Alvin Goldman, "Review: The Fragmentation of Reason," 189.

[44] Jonathan Weinberg et al., "Normativity and Epistemic Intuitions," 434.

[45] Ibid, 439-441.

[46] Ibid, 435.

amounts to a "culturally local endeavour ... what we might think of as ethno-epistemology."[47] Conclude the authors,

> Intuition-Driven Romanticism (IDR) seems a rather bizarre way to determine the correct epistemic norms. For it is difficult to see why a process that relies heavily on epistemic intuitions that are local to one's own cultural and socioeconomic group would lead to genuinely normative conclusions.[48]

Such conclusions are typical of the first phase of experimental philosophy, which focused primarily on intuitive variation as a challenge to philosophical methodology and its reliance on the intuitions of members of the profession.

Variation was not solely demographic, moreover: while intuitive variation between individuals and groups might be one reason to remain wary of reliance on intuition, experimentalists have also found that many intuitions are not 'robust.' By this it is simply meant that an individual's intuitions about cases can be affected by apparently extraneous factors: the order cases were presented in, the specific wording of the case, features of the environment (smell, disorder, noise, font) and so on. For example, one study demonstrated significant 'framing' and 'ordering' effects on responses to Trolley-type scenarios.[49] Even with identical outcomes, participant responses differed dramatically whether the words 'save' or 'kill' were used in the presented vignettes. It is difficult to maintain that one or the other wording is the 'correct' way to present the case: any 'neutral' formulation is just another way of biasing the outcome (if we believe that affective content matters in moral judgment, say). These kinds of difficulties are pervasive and there is every reason to believe that we have only begun to discover unanticipated cross-dependencies of the sort above.[50]

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[47] Ibid, 454.

[48] Ibid, 454.

[49] Lewis Petrinovich, Patricia O'Neill, "Influence of Wording and Framing…"

[50] S. Matthew Liao et al., "Putting the Trolley in Order" presents original research and summarizes existing literature on the robustness of trolley-intuitions, which are susceptible to ordering effects, framing effects, etc., concluding that "undermines the supposed evidential status of intuitions about the loop case." (661)

Not that such conclusions went unchallenged: opponents of this first phase either affirmed the reliability of reflective or 'expert' intuitions, or denied that intuitions as understood by the experimentalists played any significant role in contemporary philosophy. But also an interesting thing happened: put in practice, experimental philosophy turned out to be properly philosophical, and not just metaphilosophical. The results gathered in the methodological project were soon understood to be significant in their own right. This is the second, 'cognitive science' phase of experimental philosophy.

The contrast between first and second generation work can be found in the table of contents of two anthologies published a decade apart. The widely-discussed 1998 volume *Rethinking Intuition* is wholly focused on the metaphilophical, methodological critique. It might be somewhat anachronistic to consider the book 'experimental philosophy' at all: no studies are conducted by philosophers, and the papers were all published well before the term 'experimental philosophy' was coined. Yet the basic approach is there: the relevance of psychology and cognitive science to the methods of analytic philosophy are discussed in detail.[51] Several contributors are pessimistic about intuition-based methods (Stich, Cummins, Horowitz); others are more sanguine (Bealer, Sosa, DePaul). All would go on to debate the relevance of 'experimental philosophy' proper.

Ten years later, with the publication of the collection simply titled *Experimental Philosophy*, only four of the twelve compiled papers are concerned with the 'negative' metaphilosophical project.[52] More strikingly, in the period 2009-2013, of the 453 studies conducted by experimental philosophers compiled by Joshua Knobe and his student Ike Silver, very few at all were engaged in criticizing intuition:

> A number of experimental philosophers present their work as contributing to a 'negative' program, which aims to show that people's intuitions are fundamentally unreliable. These experimental philosophers have always described

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[51] Michael DePaul, William Ramsey, *Rethinking Intuition*. The anthology's subtitle: 'The Psychology of Intuition and Its Role in Philosophical Inquiry.'

[52] Josh Knobe, Shaun Nichols (eds.), *Experimental Philosophy*. The 2013 follow-up, *Experimental Philosophy Volume II*, devotes only one of its five sections to 'metaphilosophy': three papers, one of them critical.

their approach, extremely accurately, as just one among the various different projects within the broad field of experimental philosophy. However, some metaphilosophical work by non-experimental philosophers seems to proceed on the assumption that this negative program is the main focus of contemporary experimental work. This assumption turns out to be wildly incorrect. In actual fact, contributions to this negative program account for only 1.1% of empirical studies in experimental philosophy in the past five years. The remaining 98.9% of studies aim to make some sort of positive contribution.[53]

There is, of course, a reason that the 'negative methodological program' garnered so much attention: the claim was bold, the alleged consequences profound. Stich in particular was forceful, arguing that "the venerable tradition that views philosophy as a largely *a priori* discipline that can be pursued from the armchair is untenable" without intuition.[54] This 'first-phase' work is certainly still around.[55] It should not, however, be identified with the whole of experimental philosophy. As Knobe and Silver note, 'positive theorizing' accounts for the overwhelming majority of research.

What do experimental philosophers working on the 'positive project' *do*? A typical paper by experimental philosophers will take up a philosophical vignette or thought-experiment and pose the vignette to a number of subjects under several different conditions in an attempt to understand what cognitive processes underlie the intuitive responses generated. The guiding assumption is that the intuition probes will yield measurable and replicable context-sensitivity that, in turn, will help experimenters gain insight into aspects of human cognition. Hitherto unsuspected and robust 'interaction effects' between what appeared to be conceptually distinct items, experimentalists claim, tell us not only about how our minds work, but what constraints might be placed on philosophically adequate theories.

An example is in order to show how this is supposed to work. In their 2010 paper *Investigating the Neural and Cognitive Basis of Moral Luck*, Young, Nichols and Saxe

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[53] Josh Knobe, "What Experimental Philosophers Actually Do."

[54] Stich, "Experimental Philosophy and the Bankruptcy…"

[55] Edouard Machery's upcoming book-length work entitled *Metaphilosophy* promises to extend first-phase arguments in light of a decade's worth of research in experimental philosophy.

produce experimental evidence in favour of what they term a 'rationalist account of moral luck.' Moral luck refers to a phenomenon whereby an agent can (rightly) be held as an object of moral judgment even though much of the judgment turns on factors that were outside of the agent's control. Intuitively, we tend to find agents more blameworthy in situations with unlucky bad outcomes than with lucky good outcomes even if they performed the same action. This appears to violate a metaethical principle that we should only be held responsible for things that are in fact under our control (the 'control principle'), and therefore that blameworthiness should not be influenced by luck, good or bad. Even worse, since in any given situation there are any number of factors that we cannot control, the problem of moral luck threatens to generalize: "it is *impossible* to morally assess anyone for anything if we adhere to the Control Principle" rigorously.[56]

The experimentalists argue that the problems raised by the Control Principle require an empirical premise about the nature of moral judgment: that our moral judgments indeed track factors outside of an agent's control, creating Control-violating asymmetries in moral judgment between lucky and unlucky outcomes. This premise is disputed. Instead, the experimentalists advance the 'rationalist hypothesis,' which is that moral luck asymmetries are not driven by outcome evaluation, but rather by mental state assessment, i.e., whether the agent had a justified belief that their actions wouldn't have a harmful consequence.[57] This move does not deny the existence of moral luck, but argues instead that moral evaluation proceeds on the basis of what we think an agent would reasonably believe in the circumstances presented: if one could reasonably anticipate the unlucky outcome but proceeds anyway, one is blameworthy, regardless of whether the unlucky outcome occurs. The hypothesis is 'rationalist' because the explanation is about beliefs, not outcomes. If this is correct, our moral intuitions do not directly violate Control, resolving the apparent paradox.

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[56] Dana Nelkin, "Moral Luck."

[57] Young et al., "Investigating the Neural and…" 333.

The hypothesis is then put to the test. We should keep in mind that such tests are not meant to be the final word in the issue; it is part of the implicit methodology of analytic philosophy to present conclusive arguments in an attempt to settle the matter definitely. Empirical research is different, and the authors of this paper are aware of this difference (as experimental philosophers are generally). The open-ended nature of empirical conclusions is not a drawback, but just a general feature of such research.

That being said, the paper has all the hallmarks of good experimental philosophy: an intuition-driven philosophical literature, a clever experimental set-up, a discovered 'interaction effect' assumed to be psychologically real, and a philosophically-relevant conclusion. Let's look at these in turn. Notice the absence of any 'methodological skepticism' about intuition, showing the positive program to be indeed distinct.

First, let's take a deeper look at the intuitive problem. As Nagel sets it out in *Mortal Questions*:

> Prior to reflection it is intuitively plausible that people cannot be morally assessed for what is not their fault, or for what is due to factors beyond their control ... Without being able to explain exactly why, we feel that the appropriateness of moral assessment is easily undermined by the discovery that the act or attribute, no matter how good or bad, is not under the agent's control ... So a clear absence of control, produced by involuntary movement, physical force, or ignorance of the circumstances, excuses what is done from moral judgment.
>
> But what we do depends in many more ways than these on what is not under our control ... and external influences in this broader range are not usually thought to excuse what is done from moral judgment, positive or negative ... The things for which people are morally judged are determined in more ways than we at first realize by what is beyond their control. And when the seemingly natural requirement of fault or responsibility is applied in light of these facts, it leaves few pre-reflective moral judgments intact. Ultimately, nothing or almost nothing about what an agent does seems to be under his control.[58]

Paradox. Reflection on moral luck in light of the Control Principle seemingly removes the very possibility of moral judgment. The issue of moral luck does "appear to grow

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[58]Thomas Nagel, *Mortal* Questions, 25, 26.

inevitably from the consistent application of ordinary standards," as Nagel has it.[59] The principle that moral judgments ought not to depend on luck appears to imperil all moral judgment, since 'moral luck' is a ubiquitous feature of life. Nagel says the principle itself is 'intuitive'; and a similar appeal to what we "usually think" is deployed when he claims that a broad range of external factors do not "excuse what is done from moral judgment."[60] The entire discussion, and the very idea of 'ordinary standards,' is couched in the extended vocabulary of intuitive plausibility.

As such, the literature seemed a fruitful target for experimental investigation. Recall that the main goal of the 'positive project' was to achieve explanatory depth in accounting for the actual patterns of people's intuitions. This was precisely what Young et al. set out to do. They note that the traditional accounts of moral luck "suggest that moral luck reflects the direct influence of the outcome on moral judgments," such that bad outcomes independently entail more moral blame.[61] The experimentalists, however, offer a different hypothesis:

> ...moral luck depends primarily on observers' assessment of the beliefs and intentions of the unlucky agent. That is, people's different judgments of lucky and unlucky agents are due primarily to the difference between true and false beliefs, rather than neutral and bad outcomes.[62]

The experimentalists suggest that the *actual* pattern of intuitive response to cases of moral luck is better explained by such a 'rationalist' account over an outcome-based account. Our moral judgments, the hypothesis goes, are primarily driven by the fact that we view the (false) beliefs of unlucky agents as less justified in virtue of the bad outcome (and not driven by the bad outcome directly). Young et al. tested this hypothesis in a clever way: they presented experimental subjects with a vignette in which an agent was 'extra-lucky' (as opposed to unlucky) in a way that undermined the justification for his

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[59] Ibid, 27. I take all talk of 'ordinary standards,' of what we 'usually think,' etc., to be part of the broad vocabulary of intuition.

[60] Ibid, 25, 27.

[61] Liane Young et al., "Investigating the Neural and Cognitive Basis of Moral Luck," 334.

[62] Ibid.

belief, though the outcome was 'neutral' and not bad. Subjects responding to the vignette did indeed hold the agent morally blameworthy in the absence of a bad outcome, suggesting that the lack of justification drives the judgment.

Actually, the experimental set-up was more complicated: 54 different vignettes were tested, testing multiple combinations of background, belief, and outcome in the context of two separate behavioral experiments (one asking a question about justification, and another about blameworthiness) and one fMRI experiment. In essence, the experimentalists wanted fine-grained data about intuitive response to 'moral luck cases' controlling for the agent's reasons for belief, the truth of the belief, and the outcome of the action based on the belief.

This is exactly what we should want if we are to use 'intuition' as a constraint on theorizing as a point of good methodology: instead of focusing on one 'crucial' case, multiple variants are tested to check for unexpected cross-dependencies and results. This is especially important if we reflect on the fact that surprising counter-examples have structured philosophical argument in the past: the 'right' case can steer research and guide discussion for, it seems, decades. By exploring more variant cases and searching the 'possibility space' more systematically, experimental philosophers are increasing our chances of finding the sorts of cases and arguments that result in better theories.

The vignettes here were all variations on a 'bathtub' case where a parent ('Mitch') is about to give a bath to their two-year old. The phone rings, and Mitch tells the child to stay put and leaves to answer the phone. The child could listen (good outcome); could climb into the bath (neutral); or could climb into the bath and subsequently drown (bad). The parent could also have varying degrees of justification in their belief that the child would listen and stay put. Importantly the vignette sought to control for any contributions to moral judgment made by false belief and by poor outcome. As the authors put it,

> To compare the contributions of false beliefs and bad outcomes to moral luck, we therefore introduced an "extra lucky" condition, in which the agent's belief was

false but the outcome was neutral (e.g., Mitch's son climbs into the tub but ends up, luckily, fine).[63]

Again, in line with the intentions of the positive project, the experimentalists working on moral luck sought further 'explanatory depth' in the pattern of intuitive response and found that beliefs, not outcomes, account for judgment in cases of 'moral luck':

> As predicted, we found that false beliefs contribute more to moral luck than bad outcomes: the difference in moral blame for false versus true beliefs was greater than the difference in moral blame for bad versus neutral outcomes. Agents with false beliefs were judged to be more blameworthy than agents with true beliefs, even when no bad outcome occurred. For example, Mitch was blamed more when his belief was false (e.g., his son gets in the tub) than when his belief was true (e.g., his son stays put), even when no harm came to his son in either case.[64]

Interestingly, the authors suggest that in the subset of cases where differences in outcomes *do* lead to differences in moral judgments, the order of explanation is reversed: moral judgments seem to influence mental state judgments, so that harsh moral judgments leads to a critical view of the agent's justification, perhaps because we wish to find a rational justification for our 'gut' moral judgment.[65] But such cases are typically already those of clear-cut negligence or recklessness involving agents "who are already unjustified to think they won't cause harm."[66]

The experimentalists conclude that bad outcomes are not the primary drivers of judgment. Indeed, it turns out that

> When assigning moral blame, we care mostly about whether agents are justified in thinking that they won't cause harm. To the extent that moral luck asymmetries are driven by such mental state assessments, we may be able to defend a rational approach to morality.[67]

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[63] Ibid, 345.

[64] Ibid.

[65] Ibid, 347.

[66] Ibid.

[67] Ibid.

This is a philosophically significant conclusion, if correct. The paradox raised by Nagel was that 'external factors' outside of agent control always impinge on outcome. Outcome, in turn, influences moral judgment; yet we shouldn't be held accountable for factors outside of our control. What Young et al. show is that outcome is secondary to evaluation of an agent's belief-states, defusing the paradox: there is such a thing as 'moral luck,' of course, but moral *evaluation* proceeds via evaluation of an agent's justification in a context, not luck.

This paper is a perfect example of what experimental philosophers engaged in the positive project are doing. First we begin with a genuinely philosophical issue that has primarily been discussed in terms of broad intuition-vocabulary (including, e.g., 'ordinary' evaluations and principles, what 'we usually think / say,' what 'we would think if...' and so on). Then we have an alternative hypothesis derived from extant literature in cognitive science, which is put to the test in a fine-grained way. As I've emphasized, of particular importance is the move away from single examples towards multiple variations of vignettes to control for all possible factors (away from deployment of intuition as a kind of 'crucial thought-experiment').[68] In this case the data, it is claimed, lends credence to a 'rationalist' hypothesis whereby moral evaluation tracks the beliefs of agents, not lucky or unlucky outcomes.

But the conclusion is not merely a conclusion of cognitive science: it is of philosophical significance. In showing that mental-state assessments are more important than outcomes in moral judgment, the 'problem' of moral luck appears potentially soluble rather than paradoxical. The issue, recall, was that the pervasive phenomenon of moral luck, together with the Control Principle, seemed to conflict with our ordinary judgments of blameworthiness. If asymmetries in judgment are driven by outcome bias, adherence to the Control Principle tells us such judgments are wrong; the point generalizes, for all outcomes are subject to moral luck, and all moral judgments suspect. But what if the

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[68]Intuitive thought-experiment seems to follow the model of the *experimentum crucis*; experimentalists prefer a more Duhemian form of confirmation holism, as should be evident by now. As I argued more systematic exploration of cases is an obvious good for philosophical theorizing.

antecedent is false? What if intuitive moral judgment is not driven by outcome bias, but by consideration of the mental states of agents and what they could reasonably be held to believe (and how justified they are in believing so)? While the paper does not definitely establish this is so—and, moreover, does not pretend to sort out normative issues which cannot be settled by experiment—the problem of moral luck can be re-interpreted in a different light. It might have been a mistake to assume outcome bias generates a deep puzzle.

Experimental philosophy asks us to re-consider the crucial 'agential' aspect of the issue, just as I argued in the *prima facie* case. Consider, again, the basic issue:

> Moral luck occurs when an agent can be *correctly* treated as an object of moral judgment, despite the fact that a significant aspect of what he is assessed for depends on factors beyond his control.[69]

The critical assumption examined by the experimentalists was precisely '*what he is assessed for*,' which turned out to be, plausibly, not the outcome, but relevant beliefs and their justification. This is the specific point in the question where our judgments figure (it takes an agent to morally evaluate; but what it is that we are evaluating, precisely?). And, just as the *prima facie* case argued, a better understanding of the sources of our judgment led to a certain measure of additional theoretical insight that is applicable to philosophy.

In this way 'experimental philosophy' is, indeed, both. Let us turn, then, to some detractors.

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[69] Dana Nelkin, "Moral Luck."

Chapter 3

3 Eliminativism About Intuitions

3.1 The Eliminativist Objection (in brief)

The *prima facie* case in favor of experimental philosophy relies on premises that may be disputed. The first premise states that philosophers routinely use intuitions, without, however, specifying the exact nature of this use, or indeed what intuitions are. The eliminativist objection, meanwhile, denies that philosophers rely on intuitions as evidence. This is taken to show that experimental philosophy is a confused project (a 'big mistake') as the significance of experimental *philosophy* was conditional on philosophical reliance on intuition.

We distinguish two related versions of eliminativism about intuitions in philosophy. The first, is a straight-forward rejection of premise 1 of the *prima facie* case: that philosophers *do* rely on intuitions as evidence. If we can eliminate intuition-talk entirely, and show that philosophical argument nowhere appeals to intuition, then the entire case for experimental philosophy fails to get off the ground. This is so for the 'positive' but especially the 'methodological' projects: in the case of the latter, if there are no appeals to intuition in philosophy, we don't need to worry about the reliability of intuitive evidence or indeed are obligated to provide any account of intuition. In the case of the former, it is claimed that empirical study of intuitiveness is not properly philosophical, but a part of another discipline (sociology, psychology, etc).

This is the position of Cappelen, who has forcefully argued that, because philosophy does not rely on intuition at all, there is nothing for experimental philosophers to study; it has all been wasted effort. As Cappelen writes, "if philosophers don't rely on intuitions, then the project of checking people's intuitions is philosophically pointless."[70]

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[70]Cappelen, *Philosophy Without Intuitions*, 222.

An interesting variant is Williamson's eliminativism, who assimilates 'intuition' into a more general capacity to 'handle counterfactuals.' Philosophy does *not* rely on a special sort of rational insight ('intuition') but *does* often leverage a very prosaic sort of ability: to imagine counterfactual circumstances and evaluate features of them, which is not exclusively *a priori*. The reliability of this sort of evaluation is generally taken for granted in our everyday life. Why reserve a special scepticism for philosophy's counterfactuals? Absent a principled reason, experimental philosophy's methodological scepticism over-generates scepticism about counterfactual evaluation across the board, surely an intolerable result. The upshot is the same for both Williamson and Cappelen: philosophy does not rely on 'intuition,' even a little, though they disagree with each other on what philosophical methodology *is*.[71]

Deutsch's brand of eliminativism is somewhat different. He does not directly reject premise (1) of the *prima facie* case, but argues that what *really* supports philosophical theorizing is *a priori* knowledge, of which intuition might be a 'causal source.' He denies that something's *being intuitive* is evidence for its truth; in this sense, philosophy does not rely on intuitive *evidence*. It relies on *a priori* knowledge. Crucially, however, no amount of empirical work could shed light on the *a priori* via investigation of intuition; this is simply a category mistake. Therefore, the entire project of experimental philosophy rests on a simple conceptual confusion.[72]

I will respond to Cappelen, Williamson and Deutsch in what follows. Against Cappelen, I will argue that intuition, understood as an irreducibly mental phenomenon, is an aspect that cannot be eliminated from philosophical methodology's use of cases. Cappelen's argument for eliminativism effectively defines intuitions out of existence by placing implausible constraints on that intuitions are and how they support theories; but close examination of cases vindicates the view that intuitions do play a role. Williamson's argument that experimental philosophers 'over-generate' skepticism I will dispute by marking a distinction between theoretical contexts and ordinary evaluation. Finally,

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[71] Williamson, "Armchair Philosophy, Metaphysical Modality…"

[72] Deutsch, "Experimental Philosophy and the Theory of Reference."

against Deutsch, I will (roughly) argue that *apriority* plays a methodologically equivalent role, and is as such is vulnerable to my rebuttal to Cappelen. Along the way, in each case I will have more to say about the arguments and frameworks presented by these authors.

3.2   Philosophy Does Not Rely On Intuitions

3.2.1   A Preliminary: The Thesis of 'Centrality'

Cappelen's version of the eliminativist objection denies that philosophers rely on 'intuition' in any form. Above, we've seen a few examples of the deployment of intuitions in philosophy: two famous cases (Twin Earth, Gettier) and a less-known case (Sobel sequences). I've already argued that, in all three cases, an ineliminable mental component remains: an agent has to judge the case, and nothing can substitute for the agent. I did not provide a definition of intuition; instead I looked to these examples, in effect saying that *this* is the target of investigation, whatever it should turn out to be.

In the case of Twin Earth, the argument requires an intuition about linguistic intent: whether 'water' was indeed used as a natural kind term. The Gettier scenario requires us to intuit whether or not it constitutes a case of knowledge. The seemingly innocuous fact that the Gettier scenario is easily made to occur in reality leads us to realize the important role intuitive judgment indeed plays: as Williamson noted, actualized Gettier instances do not offer independent access to the presence or absence of 'knowledge' in a way purely imagined scenarios do not. Finally, Moss' discussion of Reverse Sobel Sequences explicitly and self-consciously relied on the relative intuitive acceptability of sets of ordered sentences in order to develop an adequate pragmatic theory. Perhaps all these judgments form a distinct kind; perhaps they are a motley; nothing rides on this. The important thing is that 'intuition' and its extended vocabulary ('judgment,' 'acceptability,' 'ordinary talk,' 'what we'd say,' etc.; I identified a similar feature in the case of moral luck) features at least *sometimes* in philosophical argument, and it not obviously replaceable by something else.

For some philosophers, appeal to intuition is *the* distinctive method of philosophy: as Bealer puts it,

it is truistic that intuitions are used as evidence (or reasons) in our standard justificatory practices … it is safe to say that these intuitions—and conclusions based on them—determine the structure of contemporary debates in epistemology, metaphysics, and philosophy of logic, language, and mind. Clearly, it is our standard justificatory procedure to use intuitions as evidence (or as reasons).[73]

Bealer's conception of philosophy is as a distinctly *a priori* field that is, at least in principle, autonomous and authoritative in virtue of intuition as a "basic source of evidence" with a reliable modal tie to truth.[74]

My argument in the *prime facie* case, however, does not rest on intuitions being distinctive of philosophical methodology (any more than it rests on philosophical intuitions constituting "a single epistemic and psychological kind."[75]) Nor on intuitions being especially prevalent or on any particular substantive view of intuition. The only crucial fact is that examples *can* be offered of the practice, and that *that* is the thing we wish to study (scientifically, if possible). I will not focus on views such a Bealer's, then, since they are not crucial to the argument.

The multiple competing and conflicting accounts of what intuition consists of provides some evidence that the practice exists: it is hard to imagine that not only Bealer, but Goldman, Jackson, Sosa, Kornblith, BonJour, Papineau, Nagel, and Chalmers (to name a few!), in offering distinct positive accounts of intuition, are really theorizing about an illusion, something that does not really exist.[76] Appealing to consensus is not definitive, of course, but given how widespread the view that intutions *do* figure in argument is, the

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[73] George Bealer, "A Theory of the A Priori," 2-3.

[74] George Bealer, "Intuition and the Autonomy of Philosophy," 217.

[75] Joel Pust, "Intuitions." Bealer is also of the view that philosophical intuitions are *sui generis*, to be distinguished from mere 'physical' intuitions and other mental states ("Intuition and the Autonomy of Philosophy").

[76] Alvin Goldman "Philosophical Intuitions"; Frank Jackson, *From Metaphysics to Ethics*; Ernesr Sosa, "A Defence of Intuitions"; Jaegwon Kim, "Explanatory Knowledge and Metaphysical Dependence"; Hilary Kornblith, "The Role of Intuition…"; BonJour *In Defense of Pure Reason*; Jennifer Nagel, "Epistemic Intuitions"; Chalmers, "Intuitions: A Minimal Defense." I am indebted to Jenkins "'Intuition,' Intuition, Concepts, and the A Priori" for some of this bibliography. See also Bruce Russell, "A Priori Justification and Knowledge" for more sources.

burden of proof is thereby increased. Of particular importance to the game of shifting burdens is the fact that 'intuition' is not posited to exist in order to serve some explanatory burden: friends of intuition do not infer that there are intuitions (as one might infer a planet from observation of gravitational anomaly); they (claim to) experience the having of intuition, which then plays an important role in argument.

Faced with such apparently clear-cut cases of appeal to intuition, and the seeming disciplinary consensus about their existence, it might be difficult to imagine anyone endorsing straight eliminativism. Yet Cappelen's *Philosophy Without Intuitions* does just this:

> Our alleged reliance on the intuitive makes many philosophers who don't work in metaphilosophy concerned about their own discipline: they are unsure what intuitions are and whether they can carry the evidential weight we allegedly assign to them. The goal of this book is to argue that this concern is unwarranted since the claim is false: it is not true that philosophers rely extensively (or even a little bit) on intuitions as evidence. At worst, analytic philosophers are guilty of engaging in somewhat irresponsible use of 'intuition'-vocabulary.[77]

Cappelen's basic argument takes aim at a thesis he calls *centrality:*

> Centrality (of Intuitions in Contemporary Philosophy): Contemporary analytic philosophers rely on intuitions as evidence (or as a source of evidence) for philosophical theories.[78]

There are several points of ambiguity in Cappelen's articulation of 'centrality,' to which he admits. This a reflection of the methodological situation in analytic philosophy: there is too little agreement to generate a target that is both clear and wide-ranging. For instance, 'centrality' does not tell us whether philosophical reliance on intuition is distinctive of philosophy; whether it is the sole source of evidence for philosophical theorizing; whether it is a normative or a descriptive thesis.[79] He does provide a

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[77] Herman Cappelen, *Philosophy Without Intuitions*, 1.

[78] Ibid, 3.

[79] Cappelen raises these issues in section 1.4., 12-16. For what it is worth I do not think that intuitions are distinctive of philosophy, that they are the only way to motivate philosophical theories, or that appeal to intuition is even especially prevalent.

'diagnostic' for the presence of intuition: that they have a special phenomenology and foundational epistemic status deriving solely from conceptual competence. Cappelen argues that when we attend to apparent appeals to intuitions more carefully, we see that these 'diagnostics' are in fact absent: there are no intuitions to be found. Thus philosophers do not appeal to intuitions.

The importance of experimental philosophy, Cappelen argues, depends on the assumption of centrality. Cappelen grants that, if centrality were the case, "we should find out whether the intuitions philosophers appeal to are representative and reliable."[80] He terms this 'the conditional insight of experimental philosophy.' His argument against centrality thus turns into a *modus tollens* against experimental philosophy.

My own strategy in the *prima facie* case was to point to concrete examples of appeal to intuition and forgo any attempt at diagnostic or definition, with one exception: what we found in common across the Twin Earth, Gettier and Sobel cases was an ineliminable mental aspect. An agent has intuitions, and nothing substitutes for the intuition. Cappelen, for his part, considers several concrete cases of his own, and comes to the opposite conclusion: no appeal to intuition can be found.[81]

I will proceed as follows. First I will argue that Cappelen's argument against intuitions, and experimental philosophy, is far more radical than he lets on. This is because he not only denies that intuitions plays any role in philosophy, but also endorses Williamson's view that there are no conceptual / analytic truths. This view is congenial to many experimentalists who also deny the possibility of conceptual analysis, though on different grounds. The problem with Cappelen's argument is that it requires, implausibly, that philosophers were doubly mistaken about what it is they do: about intuitions, and about analysis.

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[80]Ibid, 220.

[81]He considers, among others, Burges' arthritis case, Thomson's violinist, the Trolley case, Keith Lehrer's 'true temp' case in epistemology, fake barns, and Chalmers on zombies.

I will then argue that Cappelen's case studies are mistaken in this regard: there *is* an ineliminable mental component present in many such cases, plausibly labeled 'intuition'; that this component plays an important dialectical role, which Cappelen unhappily mischaracterizes; and, finally, that this role can be investigated, using the methods of normal science, in the context of both the methodological and positive projects.

3.2.2   Cappelen's Reliance on Williamson

First, then, we should note a striking aspect of Cappelen's view: he does not only deny that philosophy relies on intuition, but also militates against the practice of conceptual analysis. Cappelen endorses arguments due to Williamson to the effect that "there is no philosophically interesting phenomenon plausibly labeled as a conceptual truth or analyticity … the search for conceptual justification is like the search for unicorns."[82] Cappelen denies that successful analyses of concepts such as "truth, freedom, knowledge, reference, causation, consciousness, forgiveness, agenthood, etc." could be given.[83] I wish to make two points about Cappelen's position on conceptual analysis in relation to experimental philosophy.

One, it is true that many experimental philosophers engaged in the 'methodological' project believe that intuitions are not reliable, and should not be used as evidence for philosophical theories. Often these philosophical theories take the form of proposed analyses of concepts. For some, such as Stich, the ultimate upshot is that the practice of conceptual analysis in analytic epistemology is without foundation; as such it is to be abandoned.[84] The same basic argument has been extended to other areas of philosophy, such as reference in the philosophy of language.[85]

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[82] Herman Cappelen, *Philosophy Without Intuitions*, 211.

[83] Ibid, 214.

[84] Stephen Stich, "Reply to Sosa," 230.

[85] E.g., Machery, "it remains mysterious how theories of reference are to be supported or undermined." Edouard Machery, "What is the significance of the demographic…" 17.

In effect, many of the experimental philosophers who see in intuitions no evidential value agree with Cappelen that conceptual analysis is to be abandoned in many areas. Superficially it seems that while they agree on the issue of conceptual analysis, they do so on different grounds. But, point two, in fact the respective positions are surprisingly close. Cappelen endorses Williamson's argument against 'analyticity' which relies on a premise about possible variation in the evaluation of cases. The only difference is that Williamson, and Cappelen, do not think the variation in question is variation in 'intuition' or 'intuitive response.' But I think that both are working with a too restrictive account of intuition. Williamson's argument against analyticity, as we will see, holds that an agent can always understand a candidate analytic proposition in such a way as to hold it to be false. I've been describing 'intuition' broadly: chiefly by example, while stressing that agents have intuitions. Williamson's case against analyticity is, in effect, arguing that we should expect variability across agent judgments about cases, and draws a negative conclusion from this: there are no analytic truths. This is quite compatible with experimental philosophy; to show this I will explain Williamson's argument in a little further detail.

Cappelen agrees with Williamson that "there is no such thing as conceptual possibility or conceptual necessity," and therefore there are no *conceptual* truths relevant to philosophy.[86] This is because 'nothing at all' is epistemically available solely on the basis of linguistic or conceptual competence, and so there is nothing to ground analyticity. Williamson observes, correctly in my view, that "metaphysical conceptions of analyticity do not themselves imply that linguistic or conceptual competence constrains one's attitudes to analytic sentences or thoughts."[87] My point is that these notions (linguistic and conceptual competence) are the kind of agent-centric mental phenomena experimental philosophy aims to investigate. There is a rapprochement in the negative arguments of Williamson's that Cappelen endorses; one's interest in the empirical investigation of these factors as they relate to analyticity, or conceptual truth, may vary,

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[86] Cappelen *Philosophy Without Intuitions,* 212.

[87] Timothy Williamson, *The Philosophy of Philosophy*, 73.

of course, but they surely fall within the domain of experimental philosophy. What I want to draw out here is that Cappelen endorsing Williamson on analyticity means he also endorses Williamson's premise that empirical facts about linguistic and conceptual competence impinge on the evaluation of propositions (in this case, candidate analytic truths). This is probably enough to get the *prima facie* case off the ground.

More specifically, how does Williamson make his case? To begin, friends of analyticity hold that *understanding leading to assent* is a marker of something's being analytic:

> …if someone is unwilling to assent to the sentence "Every vixen is a female fox," the obvious hypothesis is that they do not understand it, perhaps because they do not understand the word "vixen." The central idea behind epistemological conceptions of analyticity is that, in such cases, failure to assent is not merely good evidence of failure to understand; it is constitutive of such failure.[88]

Consider an even simpler example, the sentence **S**: "Every Vixen is a Vixen." Anyone who understands it, assents to it. Williamson argues that it is perfectly possible to understand **S** but withhold assent. Suppose Peter believes that universal quantification is existentially committing, and does not believe foxes exist (perhaps he is a very strange sort of conspiracy theorist). Peter understands, and rejects, **S**.[89] Or perhaps Stephen has a truth-gap theory of vague predicates and believes there are borderline cases of foxhood--- evolutionary ancestors who are not clearly foxes or non-foxes—such that for some x, 'x is vixen' is neither true nor false. This entails that 'if x is a vixen, then x is a vixen' is neither true nor false.[90] This leads Stephen to reject **S,** on the basis of a sophisticated (though controversial) theory of vagueness; but more controversial still, Williamson claims, is a construal of 'understanding' such that every gap-theorist of the sort does not understand the predicates and logical constants they fluently deploy, and argue over, in their language: in effect the epistemological conception of analyticity implies that only those who have a fully complete and correct logic understand much of the language they speak. This is too strong.

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[88]Ibid, 73.

[89]Ibid, 86.

[90]Ibid, 87.

Therefore, concludes Williamson, we cannot ground analyticity in understanding and assent, for failure to assent to a sentence is *not* constitutive of not understanding. This is because, roughly, other (logical, semantic, conceptual, etc.) commitments may lead to a failure of assent that is not indicative of misunderstanding. Writes Williamson,

> The underlying style of argument against links from understanding to assent or dispositions to assent is quite general. For each candidate one must still find appropriate counterexamples … nevertheless, with a little ingenuity one always succeeds.[91]

In essence, what Williamson is arguing here is that, for any putative analytic truth, there is a way of 'understanding' the terms involved that denies the candidate analytic truth. This is, again, because linguistic and conceptual competence do not constrain judgments or attitudes and thus cannot ground analyticity.

But then, one might object, how are we to account for meaning at all? What gives the multiple uses and understandings of a term a common meaning, if not some invariant feature(s)? Williamson claims that "the complex interrelations of the constituents, above all, their causal interrelations" is enough to make a "unity out of diversity" of linguistic practice.[92]

I claimed above that linguistic and conceptual competence's relation to understanding and assent was itself a subject of empirical investigation. Here the resemblance was to the methodological program, and the conclusion akin to the negative wing: since a variety of judgments could be made about candidate analytic sentences in virtue of agent-centric facts about competence, we should abandon the notion of analytic truth, and, further, the project of giving conceptual analyses. Now this might be right or wrong; but it doesn't show that experimental philosophers have nothing to do.

In this instance Williamson's positive account of linguistic meaning (the 'complex interrelations') lends itself to empirical scrutiny, now more akin to the 'positive'

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[91]Ibid, 120.
[92]Ibid, 123.

program. Experimental philosophers, in the terms of Williamson's metaphilosophy, are investigating aspects of these 'complex interactions' as they play out cognitively, in a systematic effort to understand the conceptual evaluations that are actually made. As I argued in the case of 'moral luck,' sometimes the patterns uncovered are philosophically interesting. In fact the prevailing pattern of research findings in the positive program is finding surprising causal interrelations in response. Most famously, perhaps, in the case of the 'Knobe effect', which postulates that attribution of intentional action shows "a systematic sensitivity to moral considerations."[93] More specifically, it seems there is an important asymmetry: we are more likely to say some side-effect was done intentionally if it brings about harm than if it helped. It's a good example of these 'complex interactions' at play.

I do not mean to endorse the specific claims and conclusions of Williamson's metaphilosophy. Rather I am arguing the key claims Cappelen relies on in the course of his anti-experimental argument are compatible with experimental philosophy, and do not undermine the explanatory ambitions practitioners have given themselves. Instead the key premises of the *prima facie* case can be easily translated into the metaphilosophical picture provided by Williamson, provided we keep to the broad use of intuition I have recommended.

Cappelen downplays the significance of this apparent mutual concord by claiming that philosophy has never relied on 'intuition,' and that conceptual analysis is (and always was) a marginal, at best, activity within analytic philosophy—so that the concord on the negative thesis is of minimal significance.

Again, this seems at odds with philosophy's self-conception, but Cappelen is unmoved by appeal to self-conception:

> The case studies showed no evidence of conceptual analysis being an activity philosophers actively try to engage in. Just as philosophers have false beliefs

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93 Joshua Knobe, "Theory of Mind and Moral Cognition," 357.

about the role of intuitions in philosophy they have false beliefs about the role of conceptual analysis in philosophy.[94]

At this point we might suspect a sleight of hand on the part of Cappelen: his argument that philosophy does not rely on intuitions, nor on conceptual analysis, is largely based on arguments to the effect that there are no such things as 'intuitions' (only irresponsible uses of intuition-vocabulary) and no such thing as conceptual analysis (only false beliefs about concepts and analyses). But of course it is possible to rely, mistakenly, on fictions: there never were such a thing as phlogiston, but early modern scientists routinely relied on phlogiston to explain combustion. It would be a bizarre argument to claim that 17th-century theories of combustion did not rely, even a bit, on phlogistons, since phlogistons do not in fact exist. Cappelen wishes to avoid commitment to this argument and seeks to *explain away* intuition-*talk*, redescribing what appears and is described as 'appeal to intuition' as normal argument.

In effect, he is arguing that philosophy *ought not* adopt the metaphilosophical view that appeal to intuition is a significant part of the analytic toolkit, since there are no such things, and we do not *in fact* use them even when we think we do (there has only been loose, careless talk of intuition, then mistakenly reified). At the level of descriptive practice there are no genuine 'appeals to intuition.' Philosophy is in order (and has always been); metaphilosophy has been led astray.

But this move—and this is my second complaint about Cappelen's radicalism—is in tension with his position on conceptual analysis. This means that it is not just metaphilosophy that has been led astray. Cappelen's argument against intuition targeted a metaphilosophical misconception; but Williamson's argument against analyticity is directed at the level of philosophical practice, not metaphilosophy. And so Cappelen wishes, too, to re-interpret philosophical practice as not *really* being about conceptual analysis. This requires two sets of false beliefs on the part of philosophers: one, a erroneous metaphilosophical theory about the methods employed in the course of argumentation ('intuition'); second, a dual mistake about analyticity and what the

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[94] Herman Cappelen, *Philosophy Without Intuitions*, 217.

philosophical arguments themselves are attempting to establish (an 'analysis'). It is rather different to say "that's not an intuition, but really a (disguised) argument," than "here is a philosophical argument showing there are no analytic truths." The first re-describes practice. The second exhorts us to switch practices.[95]

The problem is that the absence of analytic truths does not show that philosophy does not at least attempt to provide analyses, the way that the non-existence of intuitions might show that philosophy does not in fact rely on intuition (provided an appropriate re-description can be provided). Yet if philosophers are attempting to provide analyses, they *are* issuing judgments of conceptual applicability—e.g., phlogiston-like 'intuitions.' Now, as it turns out, these intuitions are not reliable for the project of conceptual analysis, because there are no relevant analyticities to ground (or justify) them—perhaps that makes them merely *ersatz* intuitions, not 'genuine' ones, since there are none. But reliance even on *ersatz* intuitions goes against Cappelen's argument, which is that philosophy does not rely (and has never relied) on intuitions, 'even a little.' And reliance on *ersatz* intuitions is an inevitable by-product of conceptual analysis, misguided or not. Cappelen's eliminativism is not that philosophers should abandon intuitions, but that intuitions were never really appealed to; they were an illusory metaphilosophical construct all along.

For consistency's sake, Cappelen's attempt to re-describe methodological reliance on intuition-talk thus requires that conceptual analysis, too, can be relevantly re-described, and further that many philosophers held 'false beliefs' about what they were doing: not conceptual analysis, but something else. Cappelen's case against intuitions in philosophy rests not only on being able to describe intuition-talk in other terms, but on showing that purported instances of (attempts at) conceptual analysis were, all along, something else, despite philosophers taking themselves to be doing conceptual analysis. Sometimes quite

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[95] The PhilPapers survey results has 71% of faculty respondents accepting *a priori* knowledge, and 65% accepting the analytic-synthetic distinction: see http://philpapers.org/surveys/results.pl or David Bourget, David J. Chalmers, "What do Philosophers Believe?"

vociferously so![96]   In sum, it is far more plausible than intuition-*talk* was misdescribed, and misleading, than the entire *project* of conceptual analysis really being something else all along. But Cappelen really does claim that philosophers have had *false beliefs* about analysis—and not just engaged in a fruitless endeavour, but mistaken about the nature of the endeavour itself. On this point he writes that "just as philosophers have false beliefs about the role of intuitions in philosophy they have false beliefs about the role of conceptual analysis in philosophy."[97] As I've been stressing, however, there is an important difference between eliminating intuitions by claiming that intuition-talk can be relevantly re-described (so that we have never, in fact, relied on intuition) and arguing that conceptual analysis is wrong-headed—which is not an eliminativist argument.

To recapitulate, Cappelen's rejection of analytic truths does the following three things: (1) it agrees with some conclusions of the *negative* methodological program, viz., that armchair conceptual analysis should be abandoned (offering an argument from competence that is, itself, subject to experimental philosophy's investigations); (2) it does so by endorsing Williamson's positive picture of meaning as sets of complex, causal inter-relations, which can be relevantly re-described in terms familiar to the experimentalist of the positive program; (3) it implausibly suggests that the abandonment of intuition-talk *and* conceptual analysis is not radical, but an uncontroversial (re-) description of contemporary philosophical methodology, though the former and the latter are importantly different.

Of these three things, the last is most important. Even if there are no 'real' intuitions, the practice of conceptual analysis appeals to 'ersatz' intuitions as a matter of course: this is why Cappelen must marginalize the practice. If Cappelen is right that philosophers do

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[96]Jackson's much-cited *From Metaphysics to Ethics: A Defence of Conceptual Analysis* is perhaps a case in point. Cappelen cites *From Metaphysics to Ethics* exactly twice, once to show that his version of 'centrality' is widely-held and again while discussing Jackson's famous thought-experiment in his *Epiphenomenal Qualia*—only to say that "I will, however, not take those views [in *From Metaphysics to* Ethics] to be authoritative for how to interpret this passage," viz., Cappelen won't take Jackson's own metaphilosophical views into consideration while discussing Jackson's work. Herman Cappelen, *Philosophy Without Intuitions*, 76.

[97]Herman Cappelen, *Philosophy Without Intuitions,* 217.

not, and have never, relied on intuitions, and do not, or have rarely, sought to give conceptual analyses, then experimental philosophy's methodological project does lose some purchase. Aspects of the positive program may yet remain, perhaps as adjunct to philosophy of mind or cognitive science.

Nevertheless I think that Cappelen is wrong here: there *are* intuitions, and philosophers *do,* often enough, seek to give conceptual analyses. Moreover, these two are separable: intuitions can (and do) play a role outside of conceptual analysis; and, further, it is *not* a marginal (or marginalizable) practice. (Below I argue that Cappelen's case studies fail to show that intuitions do not figure.)

Williamson's own methodological proposal is worth discussing in this latter regard since he does not definitely establish that his 'non-conceptual' understanding of philosophy eschews any appeal to conceptual content that might render experimental philosophy otiose. The reason he wishes to banish conceptual analysis and analyticity stems from a dissatisfaction with 'the linguistic turn,' when the proper focus of philosophical activity is theorizing about the modal and logical features of mind-independent reality:

> On a natural view, concepts constitute only a small fraction of a largely mind-independent reality. That the goal of philosophy is in some sense to analyze that small fraction is no platitude … Most thoughts are not about thoughts. To make philosophy the study of thought is to insist that philosophers' thoughts should be about thoughts. It is not obvious why philosophers should accept that restriction … Much contemporary metaphysics is not primarily concerned with thought or language at all. Its goal is to discover what fundamental kinds of things there are and what properties and relations they have, not to study the structure of our thought about them – perhaps we have no thought about them until it is initiated by metaphysicians.[98]

The reason why Williamson dismisses *conceptual* analysis is because philosophical activity is not primarily directed at the analysis of our own concepts, our thoughts, or our language: we are interested in the nature of what there is, really. We might investigate this by reflecting on cases, Williamson admits. Such counterfactual evaluation is not about the conceptual, it is about what there is: "philosophy is in no deep sense a linguistic

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[98] Williamson, *The Philosophy of Philosophy,* 14; 18-19.

or conceptual inquiry, any more than physics is," and when we reflect on, say, the Gettier case, we are reflecting on Knowledge, not our concept 'knowledge.'[99]

If this is indeed the self-image of many contemporary philosophers, the earlier problem I posed *might* appear less urgent: abandoning both intuition and analyticity is just part and parcel of abandoning the linguistic/conceptual turn in favor of 'reality,' requiring less 're-reading' and revision that one might initially suspect.

This is not necessarily the case. The shift from concepts to things does not immediately mean that intuitions are not appealed to. Williamson's admonition is to replace 'intuition' with counterfactual evaluation, a move which does not affect the *prima facie* case. Williamson argues that

> We have a general cognitive ability to handle counterfactual conditionals […] we have no good reason to expect that the evaluation of 'philosophical' counterfactuals uses radically different cognitive capacities from the evaluation of ordinary 'unphilosophical' counterfactuals. [100]

He also denies that there is any 'special' faculty of intuition. Williamson concludes that "a reasonable speculation is that their applications in philosophy have roughly the same degree of reliability as their applications elsewhere," which is to say good: we use counterfactual evaluation constantly.[101]

Since there is no distinction between philosophical uses of 'intuition' and ordinary counterfactual evaluation (and even non-counterfactual, but actual, evaluation), Williamson claims that

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[99] Ibid, 19; this could be the basic thesis of the book. Or, again, "the subject matter of much philosophy is not conceptual in any distinctive sense. Many epistemologists study knowledge, not just the ordinary concept of knowledge. Metaphysicians who study the nature of identity over time ask how things persist, not how we think or say they persist." ibid, p. 211.

[100] Timothy Williamson, "Armchair Philosophy, Metaphysical Modality…" 13.

[101] Ibid, 21.

experimental philosophers' critique of reliance on philosophical intuitions will become a global scepticism, at odds with their conception of their general enterprise as a positive contribution to naturalistic inquiry. [102]

I disagree. We do not over-generate 'skepticism' when giving an account of our sources of evidence in the context of theorizing. This last qualifier is important. No one is suggesting that we suspend ordinary judgment or perception. Rather, given that 'intuition,' 'evaluation,' 'judgment' (however one wishes to describe it!) is used to theorize about the fundamental structure of reality—in metaphysics, epistemology, language, logic, etc.—a higher-than-ordinary standard of evidence is required. If, as Williamson suggests, the 'gap' between philosophy and science is not so wide, then this aspect of the *prima facie* case can't be neglected. Williamson argues that because intuition can't be defined in contrast to 'ordinary' modes of evaluation, global skepticism threatens. But we don't require an unassailable definition of philosophical intuition, we just need a common-sense understanding of philosophical uses of intuition / evaluation. The context of theory is held to a higher epistemic standard as a matter of course.

It is as if we were arguing against double-blind experiments because this would threaten a 'global skepticism' about perception, since there is no 'principled' way of defining or distinguishing scientific observation from ordinary perception. But such an argument is without force. Ordinary evaluation is distinct from theorizing. This should be obvious, yet the argument 'from skepticism' is popular. Sosa makes the same case, and it is wrong for the same reasons. [103] Or, to make another analogy, to probe at the weak spots in vision does not undermine visual evidence (rather, it strengthens it).

Williamson's argument against the linguistic turn is also suspect. His argument that questions such as "Was Mars always either dry or not dry?" are about *Mars* and *Dryness*

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[102]Timothy Williamson, "Philosophical Criticisms of Experimental Philosophy,"

[103] "Why require, for the defense of intuition or introspection or perception that one specify in general terms the conditions within the respective faculty is sufficiently reliable? ... even granting that intuition is systematically unreliable with respect to specifiable sorts of questions, what follows even so? Introspection and perception are also in that way and to that extent unreliable. If that sort of consideration is a serious indictment of intuition, therefore, it seems no less serious when applied to introspection or perception." Ernest Sosa, "Minimal Intuition," 267.

itself, and not about vague predicates in natural language contexts, is not wholly satisfying. While Williamson offers a thorough discussion of the truth-conditions of vague predicates in the context of classical, intuitionistic, three-valued, and fuzzy logics, the claim that the question is *about* the referents of 'Mars' and 'Dry' is tersely defended:

> What is the original question about? "About" is not a precise term. On the most straightforward interpretation, a sentence in a context is about whatever its constituents refer to in that context … Since the original question contains no metalinguistic expressions, it is not about the name "Mars" or the adjective "dry." Evidently, the original question is not explicitly about words. [104]

Williamson takes the 'context' to be the sentence itself, not its appearance in a book of philosophy.[105] This is evident in his argument that many sentential equivalents are not taken to be about language, nor are translations (into German, or French) of the sentence; but the point is not that there are translational equivalents, but that Williamson is explicitly discussing how vague predicates *in general* operate, with dryness on Mars as example.

In ordinary every-day language, questions that take the form of 'always x or not-x,' typically are querying whether there is a third intermediate value that is relevant (or one *is* cognizant of a third value, and inquiring whether it has ever held).[106] One could ask whether Mars has always been dry, given that it is dry; or always not-dry, given that it is not dry. The question Williamson raises arises *only* in a philosophical context, so we cannot easily avail ourselves of a 'straightforward' referential interpretation. (How do we refer to the law of excluded middle?)

Williamson might claim that metaphysical facts about mind-independent reality impinge on the content of natural language predicates such that their meaning is constrained by

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[104] Timothy Williamson, *the Philosophy of Philosophy*, 26.

[105] "I am sitting with a philosopher in the garden; he says again and again "I know that that's a tree", pointing to a tree that is near us. Someone else arrives and hears this, and I tell them: "This fellow isn't insane. We are only doing philosophy."" Ludwig Wittgenstein, *On Certainty* §467.

[106] Example: I am watching *Buffy the Vampire Slayer* and wondering if a character has even been undead. I ask: Has so-and-so always been only either dead or not dead?

the correct logic (classical logic, for instance). This seems to be what he is arguing in the following passage:

> Some philosophers, often under the influence of the later Wittgenstein, deny the relevance of formal semantic theories to vague natural languages. This attitude suggests a premature and slightly facile pessimism. No doubt formal semantics has not described any natural language with perfect accuracy; what has not been made plausible is that it provides no deep insights into natural languages. [107]

This seems to get the direction of explanation backwards. As Emmon Bach noted in his seminal *Natural Language Metaphysics,* the ontological question raised by formal semantics is:

> What do people talk *as if* there is? What kinds of things and relations among them does one need in order to exhibit the structure of meanings that natural languages seem to have?[108]

The way you get 'at' ontology via formal semantics is by looking at how speakers use the language, and see what this implies about what exists: for instance, one might look at tensed statements to see what needs to be the case for tensed statements to come out as true (and false).

Williamson says that questions such as "Was Mars always dry or not dry?" aren't about language, but the *things themselves*, and motivates this by appeal to formal semantic theory. Yet formal semantic theory takes as its starting point a natural language such as English and asks: what needs to exist *given* sundry semantic aspects, not the other way around.[109] Bach says, after all, "what do people talk *as if there is*" thus constraining the possible metaphysical answers. Meanwhile, Williamson's analysis of vague predicates

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[107] Williamson, *The Philosophy of Philosophy*, 37.

[108] Bach, "Natural Language Metaphysics,"*, 573*

[109] The other way around being: given what exists, what should be the semantic aspects of English? As I will argue in the next chapter, this latter question is a source of some confusion between descriptive and normative aspects of theorizing in language.

says at the outset that thinking of "the question as implicitly about thought or language turns out to be a mistake."[110]

In effect, Williamson can't motivate his view by appeal to formal semantics. When Williamson says suspicion of his project is suspicion of formal semantics generally—a very successful theoretical framework, and not something we should wish to besmirch—he glosses over a crucial difference between his project and that of formal semantics. He claims philosophical inquiry is about thing-in-themselves first, and then arguing that language must be that way. But Bach is more cautious:

> I've now said a little (but perhaps more than enough) about some of the kinds of things we seem to need in our ontology for English and a little bit (not near enough) about how we might get them into a semantics for English. It would be immoral of me as a linguist (I'm stealing a phrase from Montague) to make claims one way or the other about whether or not these sorts of things correspond to real things in the world, perceptual or conceptual categories that are independent of language, or nothing at all. [111]

Despite his caution, Bach *does* think there is a connection between language and reality:

> Do the fundamental distinctions that are reflected in the overt and covert categories of natural language correspond in any way to the structure of the world? How could they not? [112]

This is not the place to answer the rhetorical question; there have been many answers. The point is that Williamson can't help himself to formal semantics to *show* that questions like "Was Mars always either dry or not dry?" aren't about language. From the point of view of formal semantics, such questions are most assuredly about language. The more difficult question is about the relation between language and 'the (real) structure of the world,' which Bach carefully raises—through his question 'what do people talk *as if there is.*' And since we all agree language is (in part or in whole) a psychological phenomenon, we do not escape the *mental* aspect that in turn motivates the *prima facie*

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[110]Timothy Williamson, *The Philosophy of Philosophy, 23.*

[111]Bach, "Natural Language Metaphysics*,*" 592.

[112]Ibid,, 593.

case; so we return to experimental philosophy. Williamson's appeal to formal semantics fails.

This has obvious repercussions for Cappelen's appropration of Williamson's metaphilosophy. Even when we don't do conceptual analysis, linguistic *mediation* is inescapable. And if this sort of mediation occurs, we will expect the mental to figure in a way that motivates the *prima facie* case. This is exactly what we will find turning to Cappelen's case studies. So we turn away from Williamson and back to Cappelen.

3.2.3   Cappelen, Cases and Centrality

How *does* Cappelen support his argument against 'centrality'? Recall he defined centrality as the thesis that "contemporary analytic philosophers rely on intuitions as evidence (or as a source of evidence) for philosophical theories," which does not in fact claim that intuitions *are* 'central' to philosophy, just that reliance occurs.[113] Later on Cappelen is more explicit about the 'central' role intuitions are supposed to play when he articulates the 'method of cases' (MOC):

> MOC: A theory of some philosophical topic X, T, is an adequate theory only if it can account for (or explain or predict) intuitions about X in actual and possible cases.[114]

This characterization of the 'method of cases' is too strong. In section (*1.ii*) I looked at some examples of intuitions: Twin-Earth, Gettier, and Sobel Sequences. It is not required that a theory would be adequate *only* if it could account, predict, or explain intuitions about possible cases. In the Sobel case, Moss was explicit that it was a *theoretical virtue* that her account fit the countours of our intuitive judgment.[115] Theoretical virtues of the sort lend strong credence to a theory, but are not requirements of their adequacy. Cappelen's 'only-if' definition of the method of cases makes it a requirement of adequacy that (**RA**) intuitions are accounted for, or explained, or predicted (**IA**):

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[113] Cappelen, *Philosophy Without Intuitions*, 3.

[114] Ibid, 96.

[115] Sarah Moss, "On the Pragmatics of Counterfactuals," 578.

a. Cappelen's 'strong' MOC: **RA → IA**

'If a theory of X is adequate, then it will account for intuitions about X.'

b. Plausible 'weak' MOC: **IA → RA**

'If intuitions about X are accounted for, then the theory of X is *prima* facie and defeasibly adequate.'

'Weak' MOC is, I think, a better characterization of the actual MOC employed by philosophers and investigated by experimentalists. This is especially so in cases where intuitions are used as 'evidence' in the indirect mode, though I will argue that the 'direct' mode does not endorse strong MOC either. The 'indirect' mode solicits many intuitions and attempts a theoretical 'best fit.' The 'direct' mode uses an intuition as a premise in a deductive argument.

We should also note it is left unspecified in both the weak and strong MOC which intuitions are the target. It seems implausible to saddle the method of cases with the responsibility of handling or accounting for everything we can label 'intuition.' We might wish to separate out reflective from unreflective intuition, or expert from non-expert intuition. This is fair enough, and does not affect the basic distinction between the weak and strong construals of the method of cases: on the stronger version, a theory is adequate *only if* it accounts for some subset of intuition (reflective intuition, for instance); on the weak version, accounting for that subset is a sufficient and defeasible condition for theoretical adequacy. I emphasize defeasibility here because a theory's concordance with intuition is 'sufficient' only absent defeaters. In many cases, intuitions are all we have to go by: as I discussed earlier (2.1), many thought-experiments are stipulated such that no observable physical facts can impinge on the scenario. The conceivability of 'philosophical zombies' is one such scenario. In these cases the 'intuition' of conceivability is assumed to be sufficient to establish the metaphysical possibility of

philosophical zombies, though the possibility of a defeater is acknowledged.[116] This all in line with what I term the 'weak' method of cases.

To begin with indirect uses and the 'weak' method. Moss and Soames, as I discussed in chapter 2, are explicit on this point: a well-motivated theory can lead us to discard (some) intuitions. Under Cappelen's 'strong' MOC philosophical theories can never discard intuitions on pain of *modus tollens*: a theory is adequate *only* if it accounts for intuitions. No other theoretical virtues or evidence can trump accounting for intuition. This is surely too strong. This version makes it necessary for a theory's adequacy that it accounts for intuitions: it is *the* method for establishing a philosophical conclusion.

The weak version makes accounting for intuitions defeasibly sufficient, which is surely correct in the case of indirect uses of intuition. Indeed it is difficult to justify, as a methodological principle, the view that *one* source of evidence (intuition) *must* be always accounted for as a necessary condition for adequacy. This baldly denies the possibility of confirmation holism in philosophy; of any countervailing input to intuition. Cappelen's view on the 'strong' MOC is in line with his view that intuitions are treated by experimental philosophers as a sort of 'bedrock', a court from which there is no appeal or alternative. We should not be surprised he fails to find such a thing.

What about 'direct' uses, when an intuition figures as a premise in a deductive argument? In the 'Gettier' case, for instance, the JTB analysis is shown to have an intuitive counter-example, demonstrating that knowledge is not equivalent to justified true belief. Here the situation is more complicated. It might seem that an adequacy condition for an analysis is that it necessarily accounts for intuitions inasmuch as *no* counter-examples should be found for a successful analysis, e.g., the 'strong' MOC.

Are Gettier cases, and those like it, instances of the 'strong' MOC Cappelen describes? There are two problems with an answer in the affirmative. First, the Gettier case is a

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[116] "I am braced for the information that is going to make zombies inconceivable, even though I have no real idea what form the information is going to take." Stephen Yablo, "Textbook Kripkeanism and the Open…" 119.

counter-example to a proposed analysis, but Cappelen claims there are no conceptual analyses and no analytic truths. This line runs into the problems I discussed, above: it is one thing to re-describe intuition-talk in terms of some other, non-intuitive vocabulary, and another thing entirely to make a *philosophical* case against analyticity. This assumes a much greater level of self-deception about disciplinary practices.

Cappelen argues that if philosophers rely on intuitions as experimentalists claim, they do so using the strong MOC; but they do not in fact rely on the strong MOC; therefore they do not rely on intuitions (and, corollary, experimental philosophy is 'a big mistake.') How do we know they can't rely on strong MOC? Well, for one, there are no conceptual analyses or analytic truths to ground such strong reliance. But this is, to say the least, controversial; and even if there were no analytic truths, philosophers who take themselves to be engaged in analysis could mistakenly be engaged in ersatz intuition-mongering, a practice to be abandoned, not re-described—as many experimental philosophers urge.

And in any event the argument runs into a second problem: it is not the case that if philosophers rely on intuitions, they endorse strong MOC, even in the 'direct' mode. For one thing, many instances of the direct mode, over time, drift into indirect 'best fit' arguments: there are many Gettier-like cases now, requiring discernment and balance.[117]

Further, there are certainly theories where no intuitions are present one way or another. Are these theories inadequate because they account for no intuitions? According to strong MOC, there are, because philosophical theories are adequate only if they account for, predict, or explain intuitions. In effect strong MOC, combined with 'centrality,' claims that accounting for intuitions are a necessary desiderata of all philosophical theorizing: it is *the* distinct method of philosophy, and the deliverances of intuitions settle philosophical issues with no higher appeal. Again, this is implausible.

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[117] P. Blouw et al., *Gettier Cases: A Taxonomy*.

Even 'counter-examples' are more methodologically akin to weak than strong MOC. The reason is simple. We can, for the sake of argument, agree that an analysis is adequate 'only if' there are no counter-examples. But this is not the same as endorsing the strong MOC, which asserts that 'accounting for intuitions' is a necessary condition for theoretical adequacy. The reason is simple: a counter-example need not be intuitive, nor an intuition necessarily yield a counter-example. Counter-examples and intuitions are distinct.

For an instance of the latter: the Banach-Tarski 'paradox' yields the counter-intuitive result that a sphere can be decomposed and re-assembled into two identical spheres. The 'paradox' crucially relies on the axiom of choice in set theory. But it is not assumed that this counter-intuitive result shows the axiom of choice is mistaken. Thus it is not the case that a theory is adequate *only if* it accords with our intuitions; otherwise the Banach-Tarski result would show that the axiom of choice is mistaken.

Likewise, not all counter-examples are intuitive. The Condorcet voting paradox shows that ranked preference systems can violate transitivity, such that A can be preferred to B, B to C, and C to A, yielding cyclic preferences. This counter-example to transitivity in ranked voting systems is more than a little counter-intuitive; it takes some working out. Again this shows us the difference between counter-examples and intuitions. We can consistently hold that absence of counter-examples is necessary for the adequacy of a proposed analysis, while holding that accounting for intuitions is only a sufficient condition for adequacy. The dialectical role of intuitions is weaker than Cappelen suggests.

In effect we need to separate out the notion of 'counter-example' from that of 'intuition.' Plausibly, a necessary condition of theoretical adequacy is the absence of successful counter-examples. What makes for a successful counter-example is a complex question that goes beyond intuition and the intuitive. For instance, dialetheists (who hold that propositions can be both true and false) claim that the liar paradox is a counter-example

to the law of non-contradiction.[118] The claim is counter-intuitive if anything is, though other theoretical considerations are taken to trump this.

There are counter-intuitive counter-examples, and counter-intuitive non-counter-examples, and so on. Perhaps, all other things being equal, we should prefer successful counter-examples to intuitions. But it is not always possible to generate counter-examples, nor is it always trivial to distinguish a counter-example from an unexpected consequence. There is such a thing as the 'intuitive counter-example,' too: the classic Gettier case might be one: here the evidential weight of intuition supports the claim that the counter-example is successful. (For the dialetheist, the counter-example labours *against* intuitive weight.)

The basic upshot is that we can consistently endorse something like the 'strong method' for counter-examples and the 'weak method' for intuitions. The existence of the former does not demonstrate that we do not use intuitions as evidence, ever: it is obvious that the possibility of one kind of argument does not preclude another; and the existence of links between the phenomenon of 'intuitiveness' and the status of counter-examples does not collapse the distinction, either.

Cappelen's argument against centrality (and against experimental philosophy) leans on the assumption of the strong MOC in the case of intuitions, but it is not at all clear that philosophical reliance on intuition has been construed as such by philosophers, including experimental philosophers. So it is not surprising that Cappelen finds no evidence of strong reliance on intuitions in the literature, for no one really holds that theories are only adequate if they fully account for our intuitions or some privileged subset thereof. Rather a theory's accounting for intuition is, *prima facie*, defeasibly sufficient for adequacy; while counter-examples are another matter altogether.

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[118] Graham Priest most famously; the discussions in the collection *The Law of Non-Contradiction* demonstrate the difference between counter-example and intuition (the latter are appealed to as the former is sought). Graham Priest, JC Beall, Bradley Armour-Garb (eds), *The Law of Non-Contradiction.*

Indeed, it is clear that experimental philosophers themselves do not endorse strong MOC. In *Against Arguments from Reference* Machery et al. provide a definition of the MOC as they find it in the literature on intuitions of reference. It is rather different than Cappelen's:

> The method of cases: The correct theory of reference for a class of terms T is the theory which is best supported by the intuitions competent users of T have about the reference of members of T across actual and possible cases.[119]

In this instance the methodological role intuitions play is the 'indirect' mode I outlined earlier. Note that the experimentalists are explicit that the case method, when being used, should seek for the theory best supported by competent users across many cases. To paraphrase the experimental version of the MOC: a theory is correct if (not only if) it is the one best supported by the intuitions of competent speakers across cases. Cappelen's argument against experimental philosophy fails to describe their own account of methodology.

Further, experimental philosophy's account of the evidential uses of intuitions are often context-specific. In epistemology, the authors of *Normativity and Epistemic Intuitions* describe the role of intuitions in even weaker terms than the 'weak' MOC I gave in contrast to Cappelen's. In this context, an intuition-driven strategy in normative epistemology, they claim, requires only three minimal conditions: first, that epistemic intuitions serve "as data or input;" second, that the strategy "produce, as output, explicitly or implicitly normative claims or principles about matter epistemic;" finally, the output "must depend, in part, on the epistemic intuitions it takes as input."[120] This dependency is left unspecified; in particular it is not claimed that the resulting theory must directly account for intuitions, but only that a change in input results in a change of output in *some* way.[121] Again we see that 'strong' MOC is not accurate, and a means of loading the

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[119] Ron Mallon et al., "Against Arguments from Reference," 333.

[120] Jonathan Weinberg et al., "Normativity and Epistemic Intuitions," 20.

[121] e.g., this dependency could well be 'make the output the negation of the input.'

issue in favour of eliminativism by defining phenomena out of existence via stringent criteria.

3.2.4   Cappelen's Case Studies

This is especially clear if we attend to Cappelen's case studies. His basic strategy is to claim, first, that the typical features of the intuitive are not present in the original texts where cases were first presented; second, that the conclusions drawn from the cases were explicitly argued for and thus did not rely on 'intuition.'

Two points about this strategy in general before going into more detail. One: we may genuinely worry about close reliance on original texts while eliding the secondary literature; the claim that philosophers do not rely on intuition, 'even a little,' surely includes not just seminal papers, but subsequent discussion. It is much harder to motivate eliminativism when one attends to the vast secondary literature 'canonical' case have generated. Second, we should be wary of a move that distinguishes between intuition and argument as mutually exclusive. Part of the dialectical strategy of intuition is to use intuitions as premises in arguments; we don't have intuitions of theories, we make intuitive judgments that then figure as premises in arguments. So it is no surprise that Cappelen finds arguments in the vicinity of canonical cases. I will argue that intuitions, in the weak mode, figure in these arguments.

A good example of this is Cappelen's discussion of Trolley cases. I already discussed briefly in section 2.3 that our moral 'intuitions' in such cases can be pushed around by ordering effects, wording of vignettes, and so on. Experimental philosophers wish to understand these sorts of effects: the goal is to provide a theoretical framework of such judgments that achieves 'explanatory depth' in the language of cognitive science. Nothing in Cappelen's discussion shows that this goal is, as he puts it, a 'big mistake.' Indeed, Cappelen immediately *grants* that Thomson appeals to a 'pre-theoretically accepted' common ground.[122] She does this, he claims, not because these intuitions are a form of

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[122]Herman Cappelen, *Philosophy Without Intuitions,* 161.

rock-bottom justification: rather "the goal of the paper is to look for reasons and evidence beyond the pre-theoretic judgment," he writes.[123]

But this line does not show that intuitions are not appealed to. In fact, it shows the contrary. Cappelen's own characterization of the Thomson article is that the tension between the Trolley case and the Transplant case shows further investigation is required. They have the same outcomes; but while we are willing to throw the switch in the Trolley case, we are not so willing to save five patients by transplanting organs from one healthy donor. Cappelen writes:

> A central goal of Thomson's paper is to question the answers given to questions asked about the cases. There is a tension between the two answers and that tension shows further reflection and investigation is needed. Thomson and Foot articulate the reasons that can be given for the initial answer.[124]

This directly contradicts Cappelen's claim that philosophers do not rely on intuitions, 'even a little.' Cappelen takes for granted that the pre-theoretic answers given to the Trolley and Transplant cases show a genuine philosophical tension requiring a philosophical analysis. In order to generate the tension in the first place we must believe that the pre-theoretical answers have some epistemic weight. Otherwise there would be no puzzle to 'solve.' Worse yet the 'puzzle' to be solved is the articulation of reasons for why we should accept the intuition despite the tension: it is not just that we accept the intuition as *prima facie* evidence that there exists a genuine philosophical puzzle (and not a pseudo-problem generated by faulty moral judgment) but that the intuitions should be preserved in a satisfactory resolution to the puzzle.

The Trolley argument relies on intuition to state the puzzle, and relies on the epistemic weight of these intuitions to sustain it and motivate a specific solution. If we did not care about moral intuition, the asymmetry in judgment between the Trolley and Transplant cases would not require an analysis. We'd only care about what our moral theory states,

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[123]Ibid.

[124]Ibid, 160.

and simply assume our intuitions about these cases are wrong—but this is not how the argument proceeds.

Cappelen's version of the MOC requires that intuitions be a sort of rock-bottom justification, a court with no higher appeal. This is not what we have identified in the Trolley case, and this is not the way intuitions are typically deployed by philosophers. Rather, as in the Trolley / Transplant contrast here, they are a sort of raw material to be analyzed, granted a certain epistemic weight but not absolute authority. The question experimental philosophers ask is simple: 'where are you sourcing this raw material from?' on the perfectly reasonable methodological assumption that philosophical output bears some relation to its input.

Nothing in Cappelen's discussion of the Trolley case shows that intuitions are not part of the input, or that they bear no relation to the output, as experimental philosophers understand appeal to intuition. But because Cappelen thinks that the significance of experimental philosophy rests on the assumption that intuitions are a foundational source of justification (the feature he calls 'rock'), absence in philosophical practice of such uses shows that experimental philosophy is misguided.[125] But this is wrong. Intuitions can play a role in philosophy without being 'rock-bottom' justification; and scientific investigation of intuition can be relevant to philosophical practice by enhancing our understanding of this 'raw material,' however used. This is the explicit approach of experimental philosophers, who defend their approach only on the grounds that, methodologically speaking, intuitive input bears some relation to theoretical output.[126]

This basic problem appears again and again in Cappelen's case studies. His discussion of Chalmer's version of the 'Zombie Argument' against materialism denies that any premise in the argument bears the hallmark features of the 'intuitive.' The obvious candidate

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[125] Cappelen identifies three 'diagnostic' feature of the intuitive: one, a special phenomenology, two, a special epistemic status, and three, a reliance on conceptual competence (ibid, 112-113). Absence of these features, he says, "is evidence of absence of a reliance on the intuitive" (ibid, 114). It should be clear that, even absent these features, appeal to pre-theoretic judgment as in the Thomson article is the kind of thing that could be empirically studied, even if we call them "schm-intuitions" instead.

[126] Jonathan Weinberg et al., "Normativity and Epistemic Intuitions," discussed above.

premise is that 'the zombie world is conceivable'; indeed, conceivability judgments appear to be perfect examples of conceptual competence providing a compelling kind of justification for a belief. Cappelen's move once more is to separate out pre-theoretic judgment from his definition of the 'intuitive,' arguing that we might have the former, but certainly not the latter. What's really at play in the Zombie case is conceivability under ideally rational reflection, and

> What is conceivable on ideal reflection is not something that we can settle without careful reflection and argumentation and so will be a conclusion we reach inferentially [...]. There's an interesting phenomenon that we pre-theoretically would characterize as the prima facie conceivability of a zombie world.[127]

As I discussed with regard to the Trolley case, pre-theoretic judgment is an appropriate target of empirical investigation, and of philosophical importance to the extent that such judgments figure in philosophical argumentation. It is hard to deny that this 'pre-theoretic' conceivability is what motivates the debate about 'ideal conceivability': if philosophers did not have, in fact, this *prima facie* intuition of the conceivability of a zombie world, they would not be motivated to ascertain whether or not this conceivability stands up to scrutiny under 'ideal conditions.'[128] If we replace the target of experimental philosophers from Cappelen-intuitions to 'pre-theoretic judgments' we get the exact same project, viz., an empirical account of this form of judgment and a subsequent discussion of any possible methodological implications.

Suppose that we discard any reliance on pre-theoretical judgment and focus on 'ideal conceivability,' or 'ideal reasoning' generally. Does this mean that there is nothing at all intuition-like in the vicinity? Cappelen asserts that ascertaining what is the case under 'ideal reflection' is necessarily inferential, since it requires "careful reflection and argumentation."[129] A belief's not being inferential is a diagnostic feature of the intuitive

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[127] Herman Cappelen, *Philosophy Without Intuitions,* 186, 187.

[128] Those philosophers who do not have the 'intuition,' such as Daniel Dennett, argue as such: "My conviction is that the philosophical tradition of zombies would die overnight if philosophers ceased to mis-imagine them," he writes. (Dennett, "The Unimagined Preposterousness of Zombies" 323.)

[129] Herman Cappelen, *Philosophy Without Intuitions,* 186.

that follows from its status as 'rock,' or foundational. I will grant this for the sake of argument. Here Cappelen again makes the mistake of supposing that because there are arguments present, intuition, or anything intuition-like, must be absent.

From the perspective of a non-ideally rational agent, working out the 'ideal solution' will naturally require some inferential work. It should also be clear that 'ideal rationality' is a promissory note: we have no such account.[130] Ideal reasoners are typically *idealizations* governed by context-sensitive assumptions: the ideally rational economic agent is assumed to wish to maximize their utility, the ideally rational chess player wishes to win via legal moves, and so on. What is it to be the ideally rational *conceiver*, or just ideally rational *simpliciter*? Perhaps they should reason using only truth-preserving rules of inference, and are able to do so without cognitive limitation with perfect information. Such an ideal agent might not have 'intuitions' (or perhaps every truth, and only truths, would strike them as intuitive!).

The question we should consider relevant is whether or not, in the course of trying to determine whether or not something is indeed ideally conceivable, something intuition-like would be appealed to at any point. I think there are several such points: as I've already noted, the pre-theoretic conceivability motivates the question in the first place. Chalmers' own conception of methodology admits this: even if we do use strictly "*a priori* methods to draw conclusions about what is possible and what is necessary," such arguments, Chalmers claims, comprise of three distinct steps:

> ...first, an epistemic claim (about what can be known or conceived); from there to a modal claim (about what is possible or necessary); and from there to a metaphysical claim (about the nature of things in the world).[131]

As I've argued already, the first 'epistemic' step is explicitly about some studiable mental phenomena. To the extent that what agents can conceive, or claim to know (in particular cases and circumstances) structures philosophical argument, the *prima facie* case for

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[130] Chalmers, quoted in Cappelen, defines the ideal reasoner as "free from all contingent cognitive limitations" or as "undefeatability by better reasoning ," (ibid).

[131] David Chalmers, "Does Conceivability Entail Possibility?," 145.

experimental philosophy argues that we have an epistemic obligation to study and understand the mental phenomena underlying 'conceivability.' Cappelen's argument denies the *first* premise, that intuitions play a role in philosophical methodology, but (importantly) *not* the further premise which asserts that intuitions ought to be studied.

What he does argue is that the significance of experimental philosophy rides on 'centrality' being the case; but centrality is much too strong, and misdescribes actual philosophical practice. A more accurate account of the use of intuitions, while 'weaker' than centrality of the 'strong MOC' is sufficient to vindicate the *prima facie* case. This much is clear not just from Chalmers' own argument but from what would 'count' as an *objection*, a second 'intuition-like' feature we can draw out of the notion of 'ideal acceptability.' Indeed Chalmers claims that

> …the only route available to an opponent here is to claim that in describing the zombie world as a zombie world, we are misapplying the concepts, and that in fact there is a conceptual contradiction lurking in the description. [132]

For his part, Chalmers claims that "I can detect no internal incoherence; I have a clear picture of what I am conceiving when I conceive of a zombie."[133] Note that Chalmers' having a 'clear picture' of the conception is evidence for the lack of any internal incoherence. If I tried to picture a four-sided triangle, I would have no such clear conception; it is not so for the possibility of a materially exact duplicate being lacking consciousness, for which a 'clear picture' is possible.[134] Note, too, that despite Williamson's Cappelen-endorsed case against *conceptual analysis* that conceptual competence is a criterion of a successful 'first epistemic step' in the argument.

This appeal to a type of inner experience (of 'clarity') based on conceptual content is similar to my earlier case studies (Twin-Earth, Gettier, Sobel Sequences): first, an agent

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[132] David Chalmers, *The Conscious Mind* 99.

[133] David Chalmers, *The Conscious Mind* 99.

[134] I myself lack this clear picture in the Zombie case: it strikes me as contradictory precisely the same way a 'materially identical computer' to mine but *not* running a word processor is contradictory. So it goes. I am interested in what factors give rise to such sharply distinct judgments.

reflects on a case and reports a judgment, and, second, nothing could substitute for the agent doing this. Which is not to say that an argument for this same conclusion could not be given: but that in this instance, Chalmers does make an appeal to 'intuition,' even if it does not meet Cappelen's particularly stringent criteria. Further questions could be raised about this intuition: whether or not it reflects ideal competence, whether or not conceivability is a genuine route to possibility, and so on. Cappelen is right to conclude that the intuition does not *settle* the matter: it is not 'bedrock.' As I've emphasized, however, it does play a significant dialectical role. In virtue of this it is a reasonable target of methodological analysis, for the intuition figures as a premise in the over-all argument.[135]

At this point I will turn to another form of eliminativsm: Deutsch's.

3.3   Deutsch: Apriority, Not Intuitiveness

3.3.1   A Similarity: Deutsch and the Strong MOC

In a pair of recent papers, Max Deutsch has also argued that experimental philosophers have mischaracterized philosophical methodology by asserting that philosophical arguments somehow *depend* on the 'intuitiveness' of propositions. As Deutsch puts it,

> My argument begins with the observation that it is common, even outside of experimental philosophy circles, to misrepresent arguments in philosophy as depending on the intuitiveness, or counterintuitiveness, of some proposition or other.[136]

As I noted earlier, something's *being intuitive* is not necessarily the same thing as being a 'genuine counter-example.' According to Deutsch it is the latter, not the former, that philosophers are *really* after: "rational enquirers everywhere attempt to falsify

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[135]Chalmers himself responds to Cappelen in a broadly similar way, though our arguments differ on a number of points. See his *Intuitions: A Minimal Defense*, Philosophical Studies.
[136]Max Deutsch, "Intuitions, Counter-Examples, and Experimental Philosophy," 448.

generalizations with counterexamples,"[137] he writes, further arguing that "whether these counterexamples are intuitive for anyone is a separate, and purely psychological, matter."[138] That a proposition strikes an agent as intuitive or not is some empirical fact about psychology. But philosophers do not take their conclusions to be susceptible of refutation by an opinion poll about intuitiveness, or any other empirical observation. According to Deutsch, a (genuine) counter-example is an instance of 'direct' *a priori* knowledge, which has little to do with the psychological phenomenon of intuitiveness.

Deutsch's basic position is subject to some of the same criticisms I presented to Cappelen's argument. Deutsch claims, as Cappelen does, that experimental philosophers believe that philosophical methodology is such that it *requires* intuitive assent: per the strong MOC, a theory is adequate *only if* it accounts for our intuitions. For example, Deutsch maintains that experimental philosophers believe "that Gettier's anti-JTB argument *requires* that we find it intuitive that the agents in his cases fail to know despite justifiably and truly believing," but "the characterization is a misrepresentation."[139] Likewise, when discussing Kripke's Gödel-Schmidt case, Deutsch claims that experimental philosophers believe that "Kripke has refuted descriptivism *only if* his cases are intuitive counterexamples."[140]

I have already argued that this is too strong: experimental philosophers do not believe that accordance with intuitions is required for theoretical adequacy, or that theories are adequate only if they account for intuitions. I won't rehearse the case here.

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[137] Max Deutsch, "Experimental Philosophy and the Theory of Reference," 446.

[138] Max Deutsch, "Intuitions, Counter-Examples, and Experimental Philosophy," 448.

[139] Ibid. The emphasis on 'requires' is Deutsch's, not mine.

[140] Ibid. The emphasis on 'only if' is Deutsch's, not mine.

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3.3.2   Opinion Polls and Intuitiveness

Instead I will focus on three further elements. First, Deutsch claims that philosophers don't take their theories to be refutable by 'opinion poll,' showing that the psychological phenomenon of intuitiveness is not really at play:

> When a philosopher claims that *p* is intuitive, we should not understand this as, 'Most people would pretheoretically judge that *p*.' After all, if it were understood in that way, how could anyone fail to recognize the relevance opinion polls might have to the claim? Instead, the claim is best interpreted as, 'I know p directly, without inference.' … It is therefore also a mistake to think that intuition surveys and their results might pose a threat to this or that conceptual analysis.[141]

Instead, Deutsch proposes, we shouldn't worry about intuitiveness, but about genuine counter-examples:

> …it is precisely by failing to keep questions about whether an alleged counterexample is genuine separate from questions about whether an alleged counterexample is intuitive that allows experimental philosophy to gain a foothold.[142]

I agree that counter-examples need not be intuitive. I disagree that in the kinds of cases Deutsch is considering that intuitions play no role. The basic problem is that counter-examples and intuitions are not mutually exclusive: both can be appealed to in the course of philosophical argument, with intuitions as a mere sufficient and defeasible condition for adequacy. Since in many cases there is nothing else to go on but the judgment of an agent in a case, empirical investigation into intuitive judgment could provide indirect insight into philosophical issues even while granting the distinction between intuitiveness and counterexample. It is not typically the case that the results of 'surveys' are taken to demonstrate that some theory or other is the correct one: rather, as I've discussed, experimental philosophers seek 'explanatory depth' about judgment, which then is applied to philosophical theorizing in different ways. It is never suggested, either in the literature surrounding theories of reference, Gettier cases, moral judgment, or indeed any

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[141] Max Deutsch, "Intuitions, Counter-Examples, and Experimental Philosophy," 454.
[142] Ibid, 448.

other topic, that we should read our theories off the results. Instead the evidence is to be carefully integrated, along with other evidence, into a broader argumentative framework.

Deutsch claims that *if* intuitions played any evidential role (distinct from being counter-examples), these intuitions *would* be some sort of studiable psychological phenomena (something's 'being intuitive') and, as such, philosophers should be swayed by majoritarian 'surveys.' But, second premise, philosophers are not so swayed. Conclusion: intuitions play no role. I've already argued that experimental philosophers do not generally understand their methodology in such terms.

Even on his own assumptions, however, Deutsch is not entitled to his second premise. In philosophy of language, for instance, it is often granted that the meaning of a term is a fact about the conventions speakers have adopted about it. If this is so, philosophers *should* defer to the majority on particular descriptive semantic issues. As a result, many in fact do grant the relevance of 'survey results.'[143] Commenting on the Twin-Earth case, Hughes writes that

> If it should turn out that only philosophers baulk at classifying XYZ as water, I am ready to defer in my usage to the non-philosophical majority and say that "water", like "glue", is not the name of a kind with a chemical essence.[144]

The underlying issue is that it is water's *being a natural kind term* that subtends the *a posteriori* necessity that is of philosophical significance. But being either a natural kind term or another kind of term is a contingent fact about the conventions speakers have in fact adopted. This by itself of course does not show that there are no natural kind terms, or that there are no *a posteriori* necessities. What it does show is that the structure of the Twin-Earth argument isn't solely *a priori* counter-example. It relies, in its particular choice of example, on an 'intuitive' premise about the meaning of a specific term.

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[143] There is often a casually dismissive tenor to these remarks: they are not experiments, just 'surveys' or 'opinion polls.' The distinction is important to keep in mind.
[144] Christopher Hughes, *Kripke: Names, Necessity and Identity*, 63.

This is not exactly the kind of 'intuition' Deutsch and Cappelen claim experimental philosophers think is crucial or 'central' to philosophical methodology. But, as with their characterization of the *use* of intuitions, their *definition* of intuition is far too stringent. Intuitions can play a lesser epistemic role, as pretheoretic, *prima facie* evidence, while remaining an important target of empirical investigation. In the case of Twin-Earth, empirical data about the actual status of the term is directly relevant to evaluation of the case; the intuition figures as a premise in the argument, not something from which one can, or should, read a conclusion off directly.

Of course if 'water' turns out to not be a particularly good example of a natural kind term, so be it: there probably is a better one anyway. That's not the point. Deutsch denied any reliance on 'intuition,' and in particular denied that empirical facts about intuitiveness as collected in survey data could possibly impinge on philosophical argumentation. To his claim that only counter-examples matter there is a counter-example: survey data could show that a putative natural kind term isn't in fact one, and so the specific proposed counter-example fails. This possibility is explicitly granted in the secondary literature. Similarly uncontroversial is the view that intuitions (broadly defined) can generate evidence for the linguistic conventions in play in a population.

The point generalizes beyond philosophy of language. Individual reflection on the content of our concepts structures the ensuing investigation in many domains of philosophy, as closer inspection of Cappelen's case studies showed. This is a valid target for empirical investigation.

Suppose, for instance, that the majority of competent users of 'know' turned out to be intuitive contextualists; then perhaps the term 'to know' in that language is, indeed, countextually sensitive.[145] Even if the intuition did not *decide the issue*, pre-theoretic input conditions theoretical output by framing the issue and, in effect, setting the agenda. This is the weakest possible interpretation of the role of intuition in philosophy that

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[145]Indeed, a slim plurality of philosophers are contextualists (40%) per PhilPapers, ahead of the invariantists (31%).

remains mildly plausible; in many cases, I think, intuitions *do* play a much stronger role. Yet even on this weak version eliminativism fails, and empirical study of intuition is worth pursuing.

Moreover, typically such investigation is not pursued in an attempt to 'settle' what conceptual contents competent speakers actually have: what experimental philosophers are after is "an account of the factors that influence applications of a concept," more precisely, the "internal psychological processes that underlie such applications."[146] The goal of investigation is thus not to determine which philosophical theories are intuitive, or which intuitions are common in some population or other. Instead, the goal is to understand the phenomenon of intuitive judgment: whatever it is that agents cognitively do when they consider a case. The resultant theories of conceptual application are then used to philosophical effect, as in the case of moral luck (2.3). It is not typical of experimental methodology to proceed as Deutsch suggests, that is, to conduct a survey and read a philosophical theory directly off the results.

3.3.3   Causal Routes to, and Grounds for, Apriority

Deutsch wishes to avoid this line of argument by claiming that what is at issue isn't anyone's *concept* 'knowledge,' but *Knowledge,* itself. How do we access knowledge of *Knowledge*?

> The traditional answer is: By *thinking* about the case. Giving this answer appears to commit one to the existence of *a priori* knowledge. And not just a priori knowledge of the content of our concepts or the meanings of our words—the knowledge in question appears to be knowledge about *knowledge itself*, not merely about the concept *knowledge*, or the meaning of the English word 'knowledge.'[147]

This brings us to our second issue, the methodological question of 'direct *a priori* knowledge' acquired by 'thinking.' Isn't this just what an intuition *is*? Yes, it is, Deutsch

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[146] Joshua Knobe, Shaun Nichols, "An Experimental Philosophy Manifessto," 5.

[147] Max Deutsch, "Experimental Philosophy and the Theory of Reference," 459. Deutsch is not claiming that all deliverances of 'thinking' are *a priori* knowledge,

replies, but this is not problematic: an intuition could be the causal source of *a priori* knowledge without being its *justificatory* source:

> I do not mean to be distinguishing 'knowing directly' from 'intuiting' … Intuition may be the causal source of the judgment without being its justificatory source, and without the fact that the judgment is intuitive serving as a premise in an inference to the judgment's truth.
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> […]
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> ...there is nothing to prevent philosophers from maintaining that, on at least some occasions, the belief that a counterexample is genuine qualifies as direct, noninferential knowledge that the counterexample is genuine. The intuition that p would not be evidence for p, on this metaphilosophical picture, but would instead be a manifestation of one's direct knowledge that p.[148]

This is surely too quick. For one, the methodological role intuitions play in philosophy might not be 'justificatory' in the sense that, necessarily, from *p*'s being intuitive we infer that *p* is true; as I noted above, most experimental philosophers take the weaker position that intuitive input and theoretical output bear some defeasible relation. If we do accept that intuition is a legitimate, though non-justificatory causal route to the *a priori,* it is also clear that the intuitions of philosophers will play an important role in structuring the debate by declaring what is the case in the form of 'direct knowledge.'

Deutsch's picture immediately invites regress: a "manifestation of one's direct knowledge that *p*," in order to not be doubted, to be used, acted on, argued from, etc., must be known to be known. In virtue of what is it so? We could say that 'direct knowledge' is such that it is foundational just in the right way to prevent regress. How do we realize we are having direct knowledge, and not being mistaken? Just by 'thinking.' Then it seems we are right back to having intuitions according to Cappelen's diagnostic criteria: 'direct knowledge' is a non-inferential, foundational (in that it blocks regress) and has a 'special phenomenology' of thought. Deutsch, in other words, does nothing to distinguish 'direct knowledge' from intuition except to say it does not have a justificatory role.

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[148] Max Deutsch, "Intuitions, Counterexamples, and Experimental Philosophy," 454; 453.

That it is not justificatory is hard to sustain. 'Causal routes' are indirect forms of justification just in virtue of their assumed reliability in use *qua* manifestation of knowledge: so we are back more or less where we began, with a question about the reliability of intuition even if they are 'manifestations' of knowledge rather than evidence. It is not a question of internalism against externalism in epistemis justification: we may or may not have access to the actual basis of one's 'direct knowledge that p.' Rather, Deutsch is faced with a dilemma: either intuitions *qua* manifestation of *a priori* knowledge are a reliable (though non-justificatory) indicator of knowledge or they are not.[149] If they are, then I can rely on intuition, the investigable psychological phenomena, to guide me to knowledge (and the *prima facie* case proceeds as usual: what are the parameters of this reliability? Are there cases where reliability breaks down?, etc.) If they are not so reliable, we should abandon the deliverances of 'thinking about a case'—for recall that Deutsch does "not mean to be distinguishing between 'knowing directly' and 'intuiting'."[150]

Suppose we do accept Deutsch's picture. If intuitions are not justificatory, how *do* we justify our counter-examples if, for some reason, we are pressed? There is no one single answer:

> The answer foisted on philosophy by its experimental philosopher critics is that we know whether a counterexample is genuine by checking to see whether it is intuitive … It presupposes that there is some one way according to which we know whether a counterexample is genuine. There is no one way.[151]

Of course, experimental philosophers are not committed to the view that intuitions are the *one* way to verify whether a counter-example is genuine. Again, the claim is that *given* the relation between intuitive judgment and theoretical output, which everyone grants, the

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[149] Deutsch distinguishes, in other words, the justification of some judgment from the judgment's causal source, which is intuition ("Experimental Philosophy and the Theory of Reference," 451). Fair enough: we should also wish to distinguish honey from the causal source of honey viz., bees. Yet I might be inclined to think honey is close by if I see a beehive; likewise I might be inclined to think I am justified in believing that *p* if I intuit it. An example might be the conceivability of philosophical zombies (3.2.4).

[150] Max Deutsch, "Intuitions, Counterexamples, and Experimental Philosophy," 454.

[151] Ibid.

sources of intuitive judgment are worth investigation. Perhaps Deutsch's view is that intuitions are in a sense part of the 'context of discovery' while the 'context of justification' is a motley of reasons that can be given for accepting the intuition.

This leads to my third point in regards to Deutsch's 'eliminativism.' Deutsch claims that while intuitions are not justificatory, proposed counter-examples are typically supported by 'reasons,' or sundry arguments, to show that the counter-example are genuine. One might wonder why these arguments don't support the conclusion directly, or why have a theory that intuitions are a non-justificatory causal source of 'direct, noninferential' *a priori* knowledge when it would suffice to claim that the conclusions of philosophers were in fact supported by argument all along (as Cappelen argued).

The obvious move Deutsch could make would be claim that what we thought of as appeals to intuition were in fact *examples*, or illustrations of arguments. But this would involve an implausible re-reading of the original texts, hence the need for *some* account of 'intuition.' (As Deutsch writes, "the causal source of Kripke's judgment about the Gödel case is intuition; this much is fairly clear."[152]) However, Deutsch's claim that arguments justify counter-examples stands in tension with the claim that there is direct, uninferential *a priori* knowledge. We are left with the strange methodological view that intuitions are a non-justificatory causal route to *a priori* knowledge then *post facto* justified by standard inferential argument, though of course it was knowledge all along.

One might reasonably worry that whatever the 'context of discovery' serves up will end up being subject to such *post facto* justification as can be found; or what it even *means* to have an *unjustified* source of 'direct, noninferential' *a priori* knowledge. If common-sense 'intuitiveness' doesn't play an evidential role in justifying 'direct knowledge' we still have a very peculiar mental phenomena on our hands and no account of its deliverances. It seems a mistake, thus, to conflate the common-sense meaning of 'intuitiveness' (the folk psychological phenomenal feeling) which surely is *not* 'necessarily justificatory,' with the

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[152] Deutsch, "Experimental Philosophy and the Theory of Reference," 451. Exegetically speaking Deutsch realizes how much work is involved in reading intuition completely out of the literature—Cappelen's heroic attempt nonwithstanding.

special philosophical sense, which was introduced *just to* talk about so-called direct *a priori* knowledge—which, after all, is an agent's.

Leaving that aside: Deutsch does claim that reasons, or grounds, can be offered in the service of identifying *genuine* from spurious counter-examples. Yet no one has ever seriously claimed that only intuitions can support philosophical arguments, but that intuitions have done so, and prominently.[153] If Deutsch means to argue that we should not rely on intuitions, then he is sure to find friends within the negative wing of experimental philosophy's methodological project; it also does not affect the positive project, which seeks to understand patterns of intuitive response (and there are such things, whether or not they are used in philosophical argument). It is unclear what Deutsch means to do precisely: is it that we have *both* direct knowledge *and* independent reasons for accepting particular philosophical conclusions, or that the 'grounds' are the reasons for accepting the 'direct knowledge'?

He writes that

> reminding ourselves of these differing grounds will help dispel the impression that the case for the genuineness of the counterexamples bottoms out in an appeal to what is or is not intuitive.[154]

This suggests the former: we have direct *a priori* knowledge, but, when pressed, can offer other reasons. What kind of grounds can be presented in favour of philosophical conclusions? This depends on the specifics of the theory. Deutsch considers two examples: the Gödel-Schmidt and Gettier cases, offering three 'grounds' for accepting that both cases are 'genuine' counter-examples.

On the Gödel-Schmidt case, we have an imagined scenario that appears to refute descriptivism for proper names. Suppose all one knows of Gödel was that he proved the incompleteness theorem. But in fact Gödel stole the proof from Schmidt, who really

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[153] Even arch-intuiter Bealer has said it is the *standard* justificatory procedure, not the only one possible.

[154] Max Deutsch, "Intuitions, Counterexamples, and Experimental Philosophy," 455. As I've stressed repeatedly, experimental philosophers are not committed to the view that intuitions are foundational.

proved the result. If proper names like 'Gödel' refer via description, the satisfier of the description is in fact Schmidt. But, our intuitions say, we refer to *Gödel* via 'Gödel,' not Schmidt. Descriptivism is thus refuted.

Deutsch argues that we have grounds for accepting the genuine nature of the counterexample since (a) there are real-life analogues to the case "in which we correctly judge"[155]; (b) descriptivism *also* suffers from the problem of 'unwanted necessity'; and (c) there is an alternative theory, causal-historical reference, that explains the counterexample's intuitive pull. Fair enough; but as I've already argued, the possibility of other routes to the conclusion is not enough to show that 'intuition,' however defined, doesn't play a role worth investigating.

These 'grounds' are not enough by themselves to show that there is no room for any empirical work on intuitive judgment, however. The first 'ground' suggests that real-life analogues to the case are evaluated the same way, against descriptivism. Yet experimental work on reference has suggested that there is demographic variation in intuitive judgment, with some populations more likely to have the 'wrong' descriptivist intuition; *ipso facto* it is worth asking whether 'real-life' analogues match, or fail to match up, with the hypothetical cases for these populations.[156]

Consideration of hypothetical cases plays a larger role than Deutsch lets on even on his own account. It is typical of methodology in semantics to consider cases as 'empirical data,' not as *a priori* counter-examples; in other words, to take the 'indirect mode' and use 'intuition'-like information as evidence for and against theories. An excellent example of this sort of procedure is found in a recent paper arguing for the view that proper names are best represented as *variables*, which have their referents assigned to them by Kaplan-

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[155]"All that many of us 'know' about Peano is that he was the discoverer of certain axioms concerning the natural numbers. But it turns out that Dedekind discovered those axioms. If descriptivism is true, many of us have been referring all along to Dedekind with our uses of 'Peano'. But we have not been referring to Dedekind with those uses." Ibid.

[156]Machery et al, "Semantics, Cross-Cultural Style." I have also long worried that there are *no* real-life analogues to the 'pure' Gödel-Schmidt case, e.g. since the Dedekind-Peano case in fact evaluates a larger and well-established social practice.

style[157] contextual parameters. The argument is that certain verbs (like 'think') act as operators that introduce 'hyperintensional' contexts. In these cases, names no longer act as rigid designators, but "vary their reference," which "conforms to the intuition that the content of attitude ascriptions encapsulates referential uncertainty."[158]

The position outlined by the article is largely motivated by intuitive consideration of a panoply of cases, generating evidence that the author explicitly considers empirical: he writes in the concluding paragraph of the paper that "I have shown that substantial inroads can be made by the careful assessment of a variety of empirical data," including intuitive data about 'concealed questions;' the occurrence of bound names in discourses of more than one sentence; and licensed inferences in the scope of hyperintensional operators.[159] The details are interesting in their own right; for our current purposes, it shows that, in practice, the philosophical literature on the semantics of proper names does rely on intuitive data.[160] Deutsch would be hard-pressed to argue that in every acceptability judgment presented by Cumming in "Variabilism," there corresponds an *a priori* counter-example justified by independent argument. It's just not how the argument proceeds.

This is particularly so in philosophy of language, where 'intuitive data' is somewhat akin to the 'acceptability judgments' of linguists. Just in the literature on proper names, in addition to the Gödel-Schmidt case, we have Ibn Khan's Urn, Julius (inventor of the zip), Jonah, Dedekind/Peano, Pierre (who thinks London is ugly), Hesperus and Phosphorus, and so on; almost every paper either discusses a case, or proposes one. What intuitively seems to be the case can constrain a theory, show a theory is implausible, implicitly propose a new theory, or just raise a puzzle.

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[157] Kaplan, who wrote in the preface to his classic paper on Demonstratives that the goal was "providing a treatment … which does not distort our intuitions about their proper use." David Kaplan, "Demonstratives," 483.

[158] Samuel Cumming, "Variabilism," abstract.

[159] Ibid., 550.

[160] The Cumming paper counts 19 instances of the intuit- stem: intuition, intuitive, and intuitively.

Deutsch's brief against experimental philosophy proceeds by claiming that *intuitiveness* qua psychological phenomenon simply does not figure in philosophical argumentation. (Intuitiveness is a question "for a social scientist," but is not a "properly philosophical project."[161]) In philosophy of language, at least, this claim is inaccurate. Judgments about cases are everywhere, and they are not best understood as *a priori* counter-examples: rather, philosophers reflect on specific linguistic phenomena, and draw appropriate generalizations.

Deutsch's second 'ground' for the causal-historical intuition is the problem of 'unwanted necessity.' The basic claim is straightforward: for (almost) any descriptive property we associate with some entity (considered *de re*), it could be the case that the entity had not had that property. The problem first arises in a logical context such that one wishes to refer directly and to make certain kinds of modal claims about these entities. As it happens, it appears that our linguistic conventions surrounding naming do allow one to be mistaken in sentences of the form '**N** is the *F.'* The strictly logical observation about the modal profile of *de re* entities has no purchase on natural language without the concomitant (and often correct) observation about what speakers *do*.[162]

There is nothing inconceivable or contradictory about a 'possible world' where speakers do not directly refer, but always use descriptions. (The language might be impoverished.) In reflecting on *our* use of language we return to 'intuition.' Indeed as Cumming's article shows 'unwanted necessity' qua modal-logical problem is not sufficient to show that the causal-historical intuition elicited by the Gödel-Schmidt case can *ground* the 'explanatory theory' proposed.[163] At best it shows us an inadequacy between description and our uses of names. More systematic exploration of possible cases and responses to cases is required in order to understand the dimensions of proper name use. The problem with the traditional philosophy of language literature, one might say, is that it approaches the issue

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[161] Max Deutsch, "Intuitions, Counterexamples, and Experimental Philosophy," 459.

[162] Unwanted necessity can be shown to generalize into a skeptical problem about *a priori* knowledge *in toto* (Derek Ball, "One Dogma of Millianism"). If this is correct, then Deutsch cannot both endorse unwanted necessity and the *apriority* of the putative counter-example. I will not develop this line further.

[163] Deutsch neglects to mention that the causal-historical 'theory' has yet to be adequately fleshed out.

of cases in too haphazard a manner, relying on flashes of insight rather than a thorough investigation of use.

If this is roughly correct, then experimental philosophy certainly has a mandate in philosophy of language, following the outline of the *prima facie* case I have already presented above. We have an ineliminable, evidential mental component in the context of theorizing that requires some account.

What about Deutsch's other example, the Gettier case? Gettier is often treated as the paradigm case for reliable, indubitable *a priori* knowledge. Here it is important to separate out a normative and descriptive aspect of the case. The normative aspect is a question about what epistemic schema we ought to have. The descriptive aspect simply notes an unexpected conceptual distinction deriving from an application of ordinary logic: there are 'accidentally' true entailments of false beliefs. (We can substitute 'accident' for some appropriately formalizable notion of 'relevance' if accident is too epistemically loaded.)

A subject S could have a justified false belief that P. On this basis, S infers that P v Q. As it happens, Q is true. Therefore, P v Q is a justified true belief. The normative question: are these entailments instances of knowledge? The purely *descriptive* aspect of the scenario, which demarcates a conceptual distinction, is not a particularly interesting subject of intuitive evaluation. Rather, it describes a *state of affairs*: the possibility of 'accidentally true entailments' (ATEs).[164] Then we ask: is ATE=K? (Or, better: ought we say that ATE=K?) Here judgment does come into play, in many forms (including intuitive evaluation).

Deutsch is right that the conceptual (call it provisionally intensional) distinction between ATEs and non-ATEs is not a matter of 'intuition.' The distinction is objective. It is less clear, however, that reflection on (Gettier, and Gettier-like) cases does *not* figure in our evaluation of the epistemic norms we 'ought' to adopt in the face of the puzzle. We have a

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[164]Or, again, 'irrelevant entailments,' following the axioms schema of a suitably weak relevant logic, to be distinguished from 'non-irrelevant entailments.'

sense that something is epistemically amiss with the Gettier scenario, but it is surprisingly difficult to say what it is. Deutsch claims that the "disconnection between what causes Smith to believe and what makes Smith's belief true justifies the judgment that Smith does not know," thus 'grounding' the intuition.[165] But this principle (call it *disconnect*) doesn't work as 'grounds': you *can* acquire knowledge from false belief. Or so it seems. Consider this case:

> Hans brings 100 copies of his handout to the talk. He wonders whether he brought enough for every attendee. He does a careful head-count, concludes there are 53 attendees, and infers that his 100 copies suffice. But Hans's head-count was wrong: there are only 52 attendees. One person, Franz, unobtrusively switched seats and got counted twice.[166]

According to Deutsch's principle, Hans does not *know* that he has enough handouts, because he infers this on the basis of a false belief. What to do about this case, though? Intuitively, Hans *does* know (I claim). Intuition is a causal route to the *a priori*. So is it that Deutsch's principle is in fact refuted by a genuine counter-example? Or was the original intuition the correct one, and Deutsch's 'grounds' (the disconnect principle) means that we should discard the intuition of the Hans case? It is clear that we cannot *both* defend the Gettier intuition on the basis of 'disconnect' and also have the intuition that 'Hans knows' which is a defeater for 'disconnect.' Here Deutsch's story falls apart. If the Hans case defeats 'disconnect,' then intuitions are evidence. Alternatively, if we accept Hans on the basis of some 'Hans-grounds' then we have directly conflicting grounds for two opposed instances of direct *a priori* knowledge.[167]

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[165] Max Deutsch, "Intuitions, Counterexamples, and Experimental Philosophy," 456.

[166] John Turri, "Manifest Failure*,*" 2.

[167] The 'Hans' case also applies, *mutatis mutandis*, to Deutsch's alternate proposal that the Gettier intuition is grounded in the principle that presence of a defeater entails lack of knowledge: Hans has a defeater for his belief that there are 53 people in the room, but still knows he has enough hand-outs. Now the Hans intuition might be criticized on the basis that the inference is not in fact made from a false belief: yes, Hans has a false belief about the number of people in the room, but a true belief that he has more than enough handouts because he also believes that *if* he had a mistake in counting, the mistake would not be large. I don't think this objection works: Hans still relies on a false premise, even if we add another true one, which is enough to trigger Deutsch's 'disconnect' principle.

Deutsch could perhaps maintain he has an 'intuition' that Hans does not know, and can provide grounds for that; but then we're back to the case I presented against Cappelen, which is that intuitions are at least the pre-theoretic sense of what is plausible, in turn structuring subsequent argument---which is enough to justify the *prima facie* case!

There are many cases like 'Hans' in the literature surrounding Gettier.[168] 'Fake-barn' cases have the agent perceive veridically, but in a context where they are rather lucky to have done so. 'Barcelona' cases introduce a belief in the form of a disjunction with the one arbitrary disjunct true, and the justified one false. The 'Pen' case starts with a truth-maker (perception of a pen on a table), which is then replaced with an identical one; the agent has a justified true belief that their pen is on the table. These all differ along some axis.[169] It is simply undeniable that sifting through our intuitive judgments is an important part of methodology here, and so we again affirm the *prima facie* case.[170] This is so even if argument and the process of giving reasons is *also* important.

As I've suggested in section (2.1), mature subdisciplines tend to generate indirect modes of intuitive argument: consideration of multiple cases along many parametric dimensions constrains theoretical output in a 'best fit' model. Experimental philosophy's basic proposal is that we should add more dimensions in a systematic effort to achieve explanatory depth about the nature, structure, stability, etc., of our judgments. Deutsch, like Cappelen, thinks that because reasons can sometimes be given (almost always *post faco*), we can safely ignore the entire process of intuitive evaluation. This is a serious

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[168] "A wide range of thought-experiments are labelled 'Gettier cases' in contemporary epistemology … there might be as many Gettier cases as there are authors who have written on the topic." (John Turri, "Knowledge Judgments in Gettier Cases," 10). Blouw et al. argue that the diversity of Gettier and Gettier-like cases "merits abandoning the notion of a 'Gettier case' in a favour of more finely grained terminology. We propose such terminology, and use it to effectively sort the myriad Gettier cases from the theoretical literature in a way that charts deep fault lines in ordinary judgments about knowledge." Peter Blouw et al., "Gettier Cases: A Taxonomy," 1.

[169] John Turri, "Knowledge Judgments in Gettier Cases," 15.

[170] "...a very popular litmus test for any proposed theory of knowledge is whether it correctly classifies a wide range of Gettier cases. The procedure of developing new theories of knowledge and then testing them against new Gettier cases has, as a result, become business as usual in epistemology. And make no mistake, there have been a lot of different Gettier cases proposed over the past fifty years." Peter Blouw et al., "Gettier Cases: A Taxonomy," 2.

mistake. Intuitions, at the very least, offer up *prima facie* plausibility and structure subsequent discussion; and, quite frequently, intuitions are used to constrain theories in more fundamental ways.

I thus conclude that the 'eliminativism' of Deutsch and Cappelen fail.

3.4   Summary

Let's quickly recap. I first presented experimental philosophy as an empirical investigation into whatever mental phenomena underpins certain kinds of judgments, commonly termed 'intuitions,' as they appear in given examples of philosophical argument. I distinguished between a *positive program* that seeks explanatory depth, and a *methodological program* that applies the insights of the positive program to philosophical reliance on intuition (even if, historically, the latter preceded the former).

In lieu of a definition of 'intuition,' I provided examples: Twin-Earth, the Gettier case, and Reverse Sobel Sequences. Each case, I argued, had an ineliminable *mental* element: an agent judges the case, and nothing can replace the agent. We later saw this pattern again when considering the Trolley / Transplant Case, Zombies, the Bathtub case, Gödel-Schmidt and the many variations on Gettier. On this ground I built the *prima facie* case. In brief, the *prima facie* case argues that if 'intuitions' (however defined) are used as evidence in the course of philosophical theorizing, then we are owed some account of this type of evidence.

The antecedent of the preceding conditional has been challenged. The *eliminativist* argument claims that philosophy does not rely on intuitions as evidence. I considered three versions: the first, due to Cappelen, argues that there are no intuitions at all, only careless 'intuition-talk.' The second, Williamson's, collapses 'intuition' within ordinary counterfactual evaluation. Finally, Deutsch's version interprets philosophical 'intuition' as a non-justificatory s*ource* of *a priori* knowledge, while distinguishing it from the psychological phenomena of 'intuitiveness.'

I argued against all three versions of the eliminativist challenge. While the details differed in each case, two central points were constant. First, eliminativists generally

argued against a conception of intuition that was much too strong (as central, necessary, and foundational). As a result, they misdescribed both philosophical methodology and the aims of experimental philosophy. Second, eliminativist reconstruction of philosophical methodology did not manage to avoid commitment to the type of 'judgment' experimentalists are interested in. In sum, while it is true that concordance with intuitions is not a necessary condition for theoretical adequacy, this does not show that philosophy does not use intuitive evidence, or that experimental philosophy has nothing to study.

These two central points underpinned the over-all discussion. It is perhaps worth recapitulating in more detail the arguments I presented against Williamson, Cappelen, and Deutsch. Below are the major claims I pressed against the eliminativist challenge:

Positive Accounts. Eliminativists, if correct, must not only show that there are no intuitions in philosophy, but claim that extant positive accounts of intuitions in metaphilosophy are deeply confused. While *possible*, this is unlikely, and places a heavy burden of argument on the eliminativist. Cappelen takes the most thorough eliminativist position and argues philosophy does not, and has never, relied on intuition. Williamson and Deutsch concede that there are 'intuitions,' but either deny them a special status (Williamson) or deny they play a justificatory role (Deutsch).

Negative Concordance. Eliminativists agree with many experimental philosophers that intuitions are not evidence in philosophy, apparently vindicating the negative wing of the methodological program (while leaving the positive program untouched).

No Conceptual Analysis. Cappelen, following Williamson, claims not only that intuition-talk has been misdescribed at the meta-philosophical level, but is committed to the more radical view that there are no conceptual truths and no analyticities, requiring a more substantive and less plausible revision of philosophical practice.

No Analyticity. Williamson's argument against analytic truths does not contradict but is continuous with the positive program in experimental philosophy. Further, to the extent that Cappelen's argument is bound up in a controversial denial of the analytic/synthetic distinction his version of eliminativism loses force.

Thing Themselves. Williamson's argument that philosophical questions are about thing themselves, entirely unmediated by concepts, fails. He does not show that there are no metalinguistic elements present, and misunderstands the direction of ontological explanation in formal semantics. As a result, the view that philosophy is about direct *a priori* insight into 'things themselves' (like Knowledge, not our concept 'knowledge') rather than empirical facts about our concepts and their complex relation to reality is undermined.

Against Skepticism. Williamson claims that experimental philosophy over-generates skepticism by requiring that intuitions (understood as 'counterfactual evaluation') be subject to empirical investigation. This is wrong: we do not over-generate skepticism in the context of theorizing about difficult cases by applying higher epistemic standards than in ordinary situations.

Strong Method of Cases. Cappelen and Deutsch both claim that experimental philosophers assume that concordance with intuitions are a necessary condition for theoretical adequacy. This is incorrect. No experimental philosophers claim this, and, further, it is clear that intuitions have typically played a weaker role.

Conditional Relevance. Cappelen claims that experimental philosophy's relevance to philosophy *depends* on assumption of the strong MOC and the 'centrality' of intuition.[171] This is not the case. Experimental philosophy's methodological project is significant even on a much weaker construal of the methodological role of intuitions. Furthermore, the importance of the 'positive program' is independent of any purported 'conditional relevance.'

Case Studies. Cappelen and Deutsch's case studies fail to demonstrate that intuitions are not relied on. The opposite is true: in every case, appeal to pre-theoretic 'intuitive' judgment played a significant role in structuring subsequent discussion.

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[171]Herman Cappelen, *Philosophy Without Intuitions* 220. Cappelen terms this the "conditional insight of experimental philosophy."

Argument Excludes Intuition. Cappelen and Deutsch argue that because in many cases philosophical conclusions can be supported by argument, intuitions in fact played no role in establishing them. This is false: in many cases what seemed intuitively plausible shifted the burden of proof, structured debate, provided supporting evidence, or just posed a puzzle to be resolved.

Opinion Polls. Deutsch claims that if philosophers used intuitions as evidence, they would obviously see the relevance of empirical data on intuitiveness. But they do not; so they do not rely on intuitions. This is not so. First, in some cases (particularly in the philosophy of language), the relevance of empirical data is explicitly admitted. The point can be generalized to other areas. Second, intuitions could be empirical but not amenable to survey, showing that the initial condition is false: philosophers might rely on intuition, but deny the direct relevance of survey work on other grounds. This reflects the methodology of experimental philosophers who generally seek explanatory depth in patterns of intuitive judgment and do not wish to read philosophical theories off survey data.

Re-labeling. Deutsch claims that intuitions aren't used as evidence, but they are a 'causal route' to *a priori* knowledge. We can sensibly ask if the causal route is reliable (particularly when the secondary literature contains a large amount of variations on a case). This brings us back to the original question about intuition, and Deutsch's move is only a re-labeling.

Grounds. Deutsch claims that the deliverances of the 'causal route' can be checked against various sorts of 'grounds.' While the methodological picture is itself strange, Deutsch does not in fact show that the 'grounds' themselves are completely free of any appeal to intuition, or that in each instance we can replace intuition with some 'ground.' Further, it is unclear what to do in cases of intuitive conflict.

Based on these arguments, I conclude that eliminativism fails. Intuitions, broadly defined, do play various evidential roles in philosophical theorizing, which in turn show that the first premise of the *prima facie* case is correct.

Chapter 4

4   The Language We Ought to Have

4.1   Introduction: The Expertise Defense

In this chapter I will consider the 'expertise defense' in the context of the philosophy of language. I will first describe the expertise defense in general terms, and argue that since the phenomenon of expertise is domain-specific, expertise defenses should be considered on a case-by-case basis. An ideal case study is provided by experimental philosophy's research into semantic reference. I will present this research and discuss how the expertise defense has been deployed, before presenting a novel challenge to 'expertise' in philosophy of language. I will conclude with some general lessons about expertise defenses in philosophy.

The 'expertise defense' offers a distinct challenge to experimental philosophy. While eliminativism denied that philosophers rely on intuition as a source of evidence, expertise defenses claim that we are justified in accepting the deliverances of philosophers' intuitions in virtue of acquired expertise. The expertise defense thus accepts that intuitions are a source of evidence, and even that this source of evidence should be 'accounted for' in some way (2.2). The difference lies with the type of account proposed: it is argued that no scientific account of intuitive judgment is required (premise 5, section 2.2), as the acquired expertise and training of professional philosophers is enough to secure the reliability of intuition. As Steven Hales puts it,

> Intuitions are and should be sensitive to education and training in the relevant domain … the modal intuitions of professional philosophers are much more reliable than either those of inexperienced students or the 'folk.' In other words, philosophical intuitions are more like the physical intuitions of professional scientists than they are like Chomskian (sic) grammatical intuitions.

Continues Hales,

> It frankly doesn't matter what the surveys of non-philosophers show about … [the] Gettier case. These surveys shouldn't lead us to dismiss the intuitions of philosophers any more than Richard Feynman would dismiss his intuitions about

quantum electrodynamics on the basis of polling his Introduction to Physics students.[172]

This line is endorsed in roughly similar fashion by a wide variety of authors.[173] The basic idea is straightforward, though as expressed in Hales and elsewhere it tackles a relatively simplistic conception of experimental philosophy's aims. First, it seems only targeted at the 'metaphilosophical' project, which focuses on the reliability of appeal to intuition. As I noted earlier, the positive program can be pursued independently of such metaphilosophical scruples. Second, it construes the methodology of this metaphilosophical project as consisting largely in the generation of survey data, rather than in the conducting of experiment. The difference is important: a survey tabulates response, whereas an experiment seeks to understand and explain patterns of response under specified parameters. Experimental philosophers rarely, if ever, read theories off survey data; instead, they test specific hypotheses and infer conclusions from the results. Third, the expertise defense presented above treats philosophical intuitions as a uniform 'kind': the intuitions of philosophers are like those of a physicist concerning physics. Philosophy appears as a less unified domain that physics, however: is there really something that brings together moral with logical with epistemic intuitions? And is philosophy relevantly akin to physics?

Friends of the expertise defense have replies at hand to all these problems. In regards to the first, it is sometimes claimed that the 'positive program' is not philosophy, but a question for social science or experimental psychology.[174] Indeed, if folk response is

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[172] Steven Hales, *Relativism and the Foundations of Philosophy*, 171-172. Williamson also makes the analogy to physics: "we do not expect physicists to suspend their current projects … on the basis of evidence that undergraduates untrained in physics are bad at conducting laboratory experiments." (Timothy Williamson, "Philosophical Expertise and the Burden of Proof," 217.) As I argued in 2.1, though, the analogy to physics can be misleading, for philosophical thought-experiments must rely on an agent's judgment about the case. There are no double-blind thought-experiments or instrumental versions of these cases. The appeal to the authority of physics is not a rhetorical accident, either.

[173] Antti Kaupinnen, "The Rise and Fall of Experimental Philosophy"; Kirk Ludwig, "The Epistemology of Thought-Experiments"; Timothy Williamson, "Philosophical Expertise and the Burden of Proof."

[174] Max Deutsch, "Intuitions, Counterexamples, and Experimental Philosophy," 459; Antti Kauppinen, "The Rise and Fall of Experimental Philosophy," 108.

systematically different from expert response, the positive program will not achieve any interesting 'explanatory depth' about philosophical intuition, no matter the methodology (survey or experiment). Kauppinen has been forceful on this last point, arguing that the 'survey model' can only deliver psychological facts about what he terms 'surface intuitions':

> …when philosophers claim that according to our intuitions, Gettier cases are not knowledge, they are not presenting a hypothesis about gut reactions to counterfactual scenarios but, more narrowly, staking a claim of how competent and careful users … would pre-theoretically classify the case in suitable conditions. The claim … is not about what I will call *surface intuitions* but about *robust intuitions* which are bound to remain out of reach of the Survey Model of experimentalists.[175]

This is because 'robust intuitions' are acquired via active participation in complex philosophical dialogue and reflection, the kind of thing philosophers specialize in. Philosophical training consists in exposure to these debates and reflections to improve one's philosophical acumen generally and in specific domains. As Williamson puts it,

> …the expertise defense does not imply that a good philosophical education involves the cultivation of a mysterious *sui generis* faculty of rational intuitions, or anything of the sort. Rather, it is supposed to improve far more mundane skills, such as careful attention to details in the description of the scenario and their potential relevance to the questions at issue.[176]

While one's thinking is generally improved by philosophical training, more specific forms of expertise are critical. Exposure to debates in ethics, for instance, leads to improvement at thought-experiments with a moral component; studying logic improves one's intuitions about modality; and so on. Ludwig argues that experts can be expected to be more reliable and less susceptible to ambiguities and sundry irrelevant factors in the formulation of thought experiments because of domain-specific forms of expertise, specifically "because they have some relevant expertise about hard-won distinctions

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[175] Antti Kauppinen, "The Rise and Fall of Experimental Philosophy," 97.

[176] Timothy Williamson, "Philosophical Expertise and the Burden of Proof," 216.

developed in the field" conferring, if not immunity, then resistance to bias.[177] This commonsense view should be enough to vindicate the expertise defense against the skepticism of experimentalists.

In answering the expertise defense, experimental philosophers have generally responded that the phenomenon of 'expertise' is itself an object of empirical study. Experimentalists claim the burden of proof is on those who assert the reliability of expert intuitions; opponents claim the opposite. I will briefly discuss such moves and counter moves below (4.3), but will pursue an entirely different, and novel, line.

One thing experimentalists and expertise defenders do agree on, however, is that 'expertise' is, to a large extent, domain-specific: training and education in specific topics confers expertise in that topic, which does not readily translate to expertise in other areas.[178] In order to determine whether expertise does confer reliability (an empirical question), we would do well, then, to consider specific cases. Thus I will focus on one 'domain' of expertise, philosophy of language, and argue that in this domain the expertise defense backfires: far from securing reliability, acquired philosophical expertise systematically distorts intuitive judgment. The divergence between folk and expert intuition is then explained not by the lower reliability of untrained subjects, but the systematic distorting effect of philosophical training.

The case I will consider concerns intuitions of reference. Empirical evidence collected by experimental philosophers has suggested that the 'canonical' referential intuitions elicited by Kripke's famous Gödel-Schmidt case are not universally shared between and within populations.[179] This divergence was, in turn, predicted from independent cross-cultural

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[177] Kirk Ludwig, "The Epistemology of Thought Experiments," 150.

[178] Jonathan Weinberg, "Are Philosophers Expert Intuiters?" reviews the empirical literature on expertise; Timothy Williamson comes closest to arguing for a general philosophical ability to evaluate counterfactuals, but is also committed to expertise as domain-specific ("expertise in solving logical problems is not closely correlated with expertise in reading historical texts," Timothy Williamson "Philosophical Expertise and the Burden of Proof," 223).

[179] Edoucard Machery et al., "Semantics, Cross-Cultural Style"; reproduced in Edouard Machery et al.,

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differences in cognitive 'style,' suggesting a deep interaction effect present. Since intuitions about cases are taken to constrain theories of reference this raises the question of how to proceed: whether to ascribe different theories of reference to distinct populations, for instance.[180] In the present case, the expertise defense was brought to bear: it was argued that the intuitions of philosophers working in the philosophy of language should be privileged over those of 'the folk,' and that apparent variability in folk intuitions of reference need not affect philosophical theorizing.[181]

I will argue that the 'expertise defense' fails to adequately respond to the data provided by experimental philosophers because of an ambiguity between *descriptive* and *normative* aspects of semantic theory. In effect, philosophical expertise in this domain consists in expertise at applying disciplinary norms acquired by philosophical training. These conventions are normatively-governed desiderata that reflect specific epistemic, logical and metaphysical commitments of philosophers. I will not argue against these directly. Instead I will claim that an adequate and explanatory theory of natural language meaning, which is what philosophers of language take themselves to be doing, should cover the actual usages of speakers—which requires empirical investigation of the sort done by experimentalists. Thus the expertise defense fails.

In what follows I will describe the work of experimental philosophers on intuitions of reference. I will then outline the expertise defense in the case of reference and consider some extant responses. I will argue that the expertise defense is fatally committed to a view of the 'target phenomena' of semantic intuitions that is normative and discipline-specific. I will consider two such norms: the *naturalness constraint* and the *modal*

"Linguistic and metalinguistic intuitions …", Edouard Machery et al.,"Speaker's reference and cross-cultural semantics," and James Beebe, Ryan Undercoffer, "Individual and Cross-Cultural Differences in Semantic Intuitions."

[180] Ron Mallon et al., "Against Arguments from Reference." Again, experimental philosophers are cautious not to simply read theories off survey response.

[181] Michael Devitt, "Experimental Semantics"; Kirk Ludwig, "The Epistemology of Thought Experiments," Brian Weatherson, "What Good are Counter-Examples?".

constraint, and argue they are not descriptive of natural language. Philosophy of language according to the proponents of the expertise defense, I will claim, are outlining a normative linguistic proposal as semantic theory; or, more charitably, conceive of semantic theory as a normative proposal for the language we 'ought' to have. I will go on to suggest in the conclusion that this type of problem might be specific to the philosophy of language.

4.2   Reference (and Experiments on Reference)

Let's begin our discussion of experimental work on theories of reference with some general background. Reference is typically understood as a relation between expressions and what those expressions pick out in the world. The guiding philosophical assumption when theorizing about reference is that

> ...contemporary philosophers use 'reference' as a natural kind term. Thus, it has no stipulative definition—instead, it is intended to pick out a semantic relation that (a) is a pretty good fit with respect to the traits and paradigms with which it is associated and (b) carves semantic reality neatly at its joints.[182]

For example, I may use a name such as 'Barack Obama' to refer to a specific individual. How is this possible? How does reference work? The goal of a theory of reference is to answer these questions by giving an account of the 'kind' that underlies referential uses of language. What makes reference to Barack Obama possible via the words 'Barack Obama' is a relation that is 'real' in the sense that it can be the subject of a genuinely explanatory semantic theory. Reference is central to theories of meaning, and the key to semantics, so it is crucial we get it right, though I will remain agnostic on particulars.[183]

Importantly, semantic theories here are understood as theories of natural language semantics; that is, of actual languages as they are spoken and used. Kripke, whose seminal work introduced the Gödel-Schmidt case later investigated by experimentalists,

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[182]John Hawthorne and David Manley, *The Reference Book* 243.

[183] In what follows I will assume that there is more to semantics than reference (e.g., there are propositions, and contents), but will not endorse any particular approach beyond that.

writes in the preface to *Naming and Necessity* that he understands his work to be *about* names as they figure in ordinary language. Summing up the central semantic thesis of the book, he writes that "the natural intuition that the names of ordinary language are rigid designators can in fact be upheld," while carefully distinguishing between what he terms "theses of philosophical logic" from "what we call 'names' in actual language," stressing their independence.[184] His main semantic goal in *Naming and Necessity* is to apply insights from modality to semantic theorizing about ordinary language. This is a project he takes as obvious and not worth much explicit discussion, though there are allusions: for example, when discussing use / mention errors, he writes that such "loose usages [have been] adduced as counterexamples to the applicability of the present theory to ordinary language," and it is difficult to interpret such comments except in light of an explanatory project aimed at natural language meaning.[185]

Kripke is right to think that the work a theory of reference does should apply to ordinary language. There is a historical dimension at play: philosophy of language, as Stainton cogently argues, "has long paid attention to facts about actual, spoken languages," adducing several examples.[186] It might be argued, nevertheless, that Kripke is wrong. Philosophy of language is not about ordinary languages as they are actually used, but about something else: languages are abstract entities and theories of language are about mind-independent logical truths about these entities. This line, as Stainton argues, doesn't work either, for linguistic meaning is necessarily linked to mental phenomena: "some contents are out-and-out mentalistic," and "in general, linguistic contents are linked necessarily with human psychology."[187] Erasing the mental leaves us with no means of working on (and distinguishing) language as a distinct topic from formal logic. So it is

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[184] Saul Kripke, *Naming and Necessity* 5, 4. Kripke is consistent on this point, saying much the same in "Identity and Necessity" (7-8), and in "Vacuous Names and Fictional Entities" (fn.2).

[185] Saul Kripke, *Naming and Necessity* 62.

[186] Robert Stainton, "The Role of Psychology in the Philosophy of Language," 3.

[187] Ibid., 4.

crucial we understand philosophy of language—and theories of reference—as about natural language, and to keep this in mind as we proceed.

So, to return to our earlier question: How does 'Barack Obama' manage to refer to Barack Obama? Theories of reference for proper names in analytic philosophy have been traditionally divided into descriptivist and causal-historical views. Descriptivist theories associate a number of properties with a term, and the referent is the object which best or uniquely satisfies the associated set of properties.[188] Causal-historical theories suggest instead that certain terms, such as proper names and natural kinds, are introduced into a community of speakers for the purposes of referring to a specific object or individual (the initial baptism) and that successive users of the term directly refer via an unbroken causal chain of usage.[189] Extending this picture for other classes of terms is an ongoing project.

What is important for us is that descriptivist theories, despite being virtual philosophical orthodoxy for decades, were largely abandoned after a series of imagined cases showed descriptivism to have unforeseen counter-intuitive consequences. These cases were presented in Kripke's influential *Naming and Necessity*, landing a decisive blow to descriptivism. As experimental philosophers note, cases

> ...played a crucial role in the challenge posed to traditional descriptivist theories of reference by the causal-historical theories championed by Kripke and others. Indeed, Kripke's masterstroke was to propose a number of cases that elicited widely shared intuitions that were inconsistent with traditional descriptivist theories.[190]

One of the most famous of these cases is the 'Gödel-Schmidt' scenario. The case, so the argument goes, is taken to provide intuitive evidence that the referential prediction made by the descriptivist theory is falsified. The implicit methodology suggested is that the

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[188] Developed by Gottlob Frege, "On Sense and Reference," Bertrand Russell, "Knowledge By Acquaintance…"; John Searle defends descriptivism against causal-historical theories in *Intentionality*, having provided the 'cluster' view in his "Proper Names."

[189] Saul Kripke, *Naming and Necessity*. Scott Soames' *Beyond Rigidity* further develops the account. Gareth Evans first proposed 'hybrid' theories in *The Varieties of Reference*, but I won't discuss these here.

[190] Ron Mallon et al., "Against Arguments From Reference."

correct theory of reference is the one that best supports the referential intuitions of competent speakers.

Much like the earlier cases considered, cases involving reference require an agent to make a judgment. To see why a simple example might help. Consider a simple theory of reference for proper names we can call 'naïve descriptivism' (ND). For the ND theorist, a proper name is a label for a set of jointly necessary and sufficient properties. The object that has these properties is the referent of the name. Thus, 'Barack Obama' might be associated with a set of properties such as 'human being; born in Hawaii; the 44th president of the United States' and so on. An utterance of 'Barack Obama' refers to whatever object has all these properties. Under this construal, ND predicts that reference fails where no object in fact satisfies all the associated properties.

Suppose a speaker associates 'Barack Obama' with the properties listed above, along with an idiosyncratic and false association such as 'communist sleeper agent.' Question: does the speaker fail to refer when, in the course of ordinary conversation about American politics, he uses the name 'Barack Obama'? Intuitively, it appears not: for one thing, an interlocutor might be unaware that the speaker believes this, and still know who is being discussed. We can imagine talking to someone with some idiosyncratic beliefs about a person, and still both realize we are talking about the same person with no failure of reference involved—refuting ND 'from the armchair.' How do we really know that we are indeed successfully referring? It simply seems intuitively 'obvious,' and a problem for the theory.[191]

The 'cluster descriptivist' theory taken to be falsified by Kripke's case is more sophisticated than the naïve version above. As Kripke describes the view,

> To every name or designating expression 'X' there corresponds a cluster of properties, namely the family of those properties φ such that A believes 'φX.'

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[191] Which is not to say that other considerations, imagined facts about the conversations, etc., could not be brought to bear; only that cases like these are readily available to the imagination, and we help ourselves to our intuitions about them.

[…] if most, or a weighted most, of the φ's are satisfied by one unique object *y*, then *y* is the referent of 'X'.[192]

To unpack the theory: it states that for any proper name (such as 'Barack Obama') there exists a set of weighted properties that a speaker will associate with a name. When a speaker uses the name in ordinary circumstances, they are referring to whatever object has most, or a weighted most, of these properties. Cluster descriptivism is different from naïve descriptivism, then, in that it does not require that all associated properties be satisfied. If a speaker associated some idiosyncratic property with 'Barack Obama' that does not in fact hold, reference may still succeed, for Barack Obama still satisfies most of the rest of the properties.

While cluster descriptivism obviously fares better than the naïve version in the Barack Obama case, Kripke provides another imagined scenario showing that it, too, must be incorrect. He begins by asking us to imagine a person for whom the only thing they've heard about Gödel is that he discovered the incompleteness of arithmetic. Kripke then asks whether or not it would be the case that "whoever discovered the incompleteness of arithmetic is the referent of 'Gödel'," which, given the parameters of the scenario, would be the prediction of cluster descriptivism.[193] Then Kripke asks us to

> ...imagine the following blatantly fictional situation ... Suppose that Gödel was not in fact the author of [the incompleteness theorem]. A man named `Schmidt', whose body was found in Vienna under mysterious circumstances many years ago, actually did the work in question. His friend Gödel somehow got hold of the manuscript and it was thereafter attributed to Gödel. On the view in question [descriptivism], then, when our ordinary man uses the name Gödel, he really means to refer to Schmidt, because Schmidt is the unique agent satisfying the description 'the man who discovered the incompleteness of arithmetic' ... but it seems to me that we are not. We simply are not.[194]

According to the scenario, the only thing our fictional speaker knows about Gödel is the one property: the prover of a certain theorem. Which object, Kripke asks, satisfies that

[192] Saul Kripke, *Naming and Necessity*, 64.

[193] Ibid., 83.

[194] Ibid., 83-84.

property, prover-of-this-theorem, in our imagined example? Well, Schmidt. Descriptivism predicts, then, that when our imagined man uses 'Gödel', he "really means to refer to Schmidt;" but, continues Kripke, "it seems to me we are not."[195] We'd want to say he is in fact talking about Gödel when using the name 'Gödel.' Therefore, this form of descriptivism gets it wrong. In its stead, Kripke proposes a picture of reference where causal links to the bearer of the name determine reference, in line with our referential intuitions.[196]

'Our' intuitions? Who is 'us'? Does everyone share this intuition about the reference of proper names? There has been an increasing concern in the psychological sciences with the replication of experimental results across demographic groups. Psychological studies typically assay North American or European undergraduates; this is hardly a 'random' sample of our species. Rather, it is distinctly 'weird.'[197] The worry is that the results obtained by studying this restricted group will be culturally-specific, and not general. To the extent that philosophy relies on intuition, the same worry applies—arguably, the situation is even more dire: as 'weird' as undergraduates are, professional philosophers are almost certainly stranger. What makes our intuitions correct and reliable rather than parochial or professionally conditioned? Should we expect the intuitions of philosophers to be universal or even widely shared?

In light of all this, one hypothesis experimental philosophers explored was whether cultural differences in cognitive 'style' could predict response to cases such as the Gödel-Schmidt.[198] Experimental philosophers considered whether systematic differences uncovered in perceptual, memory, classificatory, attention, prediction, belief revision,

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[195] Ibid.

[196] On intuition, earlier on Kripke comments that "…some philosophers think that something's having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence in favor of anything, myself. I really don't know, in a way, what more conclusive evidence one can have about anything, ultimately speaking." Ibid., 42.

[197] 'White, educated, industrialized, rich, democratic.' Joseph Heinrich et al., "The Weirdest People in the World?"

[198] Following the work of Nisbett et al., "Culture and Systems of Thought."

etc., tasks between 'analytic' and 'holistic' thinkers would also extend to reference. In particular, it seems that East Asians (EAs) tend towards 'holistic' thought, while Westerners (Ws) tend towards 'analytic' thought. If, as seems to be the case, human culture shapes aspects of cognition, we should like to know which cognitive tasks are prone to cultural effects and which, if any, are truly universal. Based on Nisbett's work, experimental philosophers suspected that referential intuitions might well be culturally-specific:

> Holistic thought … is characterized as "involving an orientation to the context or field as a whole, including attention to relationships between a focal object and the field, and a preference for explaining and predicting events on the basis of such relationships." Analytic thought … is characterized as "involving detachment of the object from its context, a tendency to focus on attributes of the object in order to assign it to categories, and a preference for using rules about the categories to explain and predict the object's behavior."[199]

The prediction the experimental philosophers made, on the basis of these differences, was that holistic thinkers would be more likely to have the 'incorrect' descriptivist intuition, while analytic thinkers would be more likely to share Kripke's intuition about the case. This would reflect the focus of analytic thinkers on decontextualization and categorization, and that of holistic thinkers on contextual relationships in a larger field. As I argued earlier, the goal of experimental philosophy is not to tabulate response, but explain it. The hypothesis being tested is the existence of an interaction effect between cognitive style and referential intuition, which, in turn, could have implication for theories of meaning.

To test the prediction that referential intuitions would reflect cognitive style of respondents, Machery et al. presented a series of probes to roughly 40 undergraduates and Rutgers and another 40 at the University of Hong Kong.[200] These 'probes' were modeled on Kripke's Gödel-Schmidt; some were based on Kripke's original text, while others used examples that would be familiar to Chinese students. They all were presented

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[199] Machery et al., "Semantics, Cross-Cultural Style." The authors are quoting Nisbett, "Culture and Systems of Thought."

[200] Machery et al, "Semantics, Cross-Cultural Style."

in English to both the Rutgers (W) and Hong Kong (EA) participants, with two possible answers: one for the descriptivist intuition, and another for the causal intuition. The results matched the initial predictions of the experimentalists: Ws were twice as likely as EAs to have causal-historical intuitions. This result has been replicated since, controlling for ambiguity in the vignettes, language used, and the speaker / semantic reference distinction.[201]

The existence of a statistically significant difference in response between EAs and Ws scenario, the experimentalists argue, carries important methodological implications for philosophers who rely on intuition. It is not just that there existed differences in response, but that these differences could be predicted from measurable differences in cognitive style, that is, the different cognitive strategies deployed in the course of evaluation. The 'cognitive style' hypothesis is that *because* Ws are more likely to make causation-based judgments generally they gravitated towards the Kripkean inuition. But if intuitions of reference are the product of cognitive processes that vary between cultures (and perhaps individuals, situations, etc.) then the reliability of such intuitions will ultimately be a function of the epistemic status of these processes. In particular, we'd have to affirm that one or other style is the 'correct' one for reasoning about semantics and the theory of reference: presumably the 'causal' style that predominates among professional philosophers.

Alternatively perhaps it is the case that theories of meaning should reflect the relevant facts about cognitive style. Kripke claims that

> a symbol of any actual or hypothetical language that is *not* a rigid designator is so unlike the names of ordinary language that it ought not be called a 'name.'[202]

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[201] For fuller discussion of replication experiments, and the criticisms that led to them, see James Beebe, Ryan Undercoffer, "Individual and Cross-Cultural Differences in Semantic Intuitions." There has been no small amount of criticism of the various experimental designs; I will not discuss this here, but focus solely on the expertise defense instead. Perhaps this unfairly *assumes* that the data *does* show genuine variation when it is, in fact, an illusion or artifact of design. I hope the reader will indulge the *what if* underlying the argument: suppose the data is robust, replicable, etc. Then what?

[202] Saul Kripke, *Naming and Necessity* 5 fn.8.

If holistic thinkers are indeed 'descriptivists' about the reference of proper names, we may be forced to say theirs is a language with no 'names' strictly speaking, or where not all 'names' are names. Or, more radically still, systematic variation might force a re-evaluation of the central role reference has played. The experimentalists discuss the possibilities without endorsing any particular conclusion save that caution is required.[203]

Absent a justification for the privileging of a certain class of intuitions it is difficult to see how traditional philosophical theorizing about reference could proceed; why should we simply assume that philosophers have the 'correct' intuitions? Assuming the data is correct, if 'reference' is a semantic kind, as the common assumption goes, it would seem that populations with different referential intuitions are not using the same semantic categories in their respective languages ('referential pluralism)'; or that reference is not a semantic kind at all (various forms of pragmatism and internalism); or that we should be agnostic on the correct theory (referential skepticism). Worse still for philosophers, the 'metaphysics of reference' that have been elaborated on the basis of the causal theory might be without adequate foundation on any of these options.[204]

4.3   The Expertise Defense in Semantics

As we've seen, one *prima facie* reason that we might be inclined to prefer the intuitions of philosophers is that philosophers are trained to be careful and deliberate thinkers, whose reflection on cases is more likely to be reliable than 'the folk.' Instead of worrying over adopting referential pluralism or skepticism, we discard the 'erroneous' folk intuitions in favor of those of trained semanticists or philosophers.[205] As Devitt argues,

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[203] Ron Mallon et al., "Against Arguments from Reference."

[204] Viz., philosophical arguments that rely on the truth of the causal theory as a premise (discussed ibid). I will not pursue this aspect here; see chapter 5 (conclusion) for some brief remarks.

[205] An interesting exception is Brian Weatherson's take: he characterizes intuitions as a kind of pre-theoretic snap judgment that can serve as a semi-independent check against our theories (assuming snap judgments are not theory-laden; they can become so, in which case, they become *less* reliable, not more. Brian Weatherson, "Centrality and Marginalisation," 526).

we should prefer the intuitions of semanticists, usually philosophers, because they are much more expert (which is not to say, very expert!). Just as the intuitions of paleontologists, physicists, and psychologists in their respective domains are likely to be better than those of the folk, so too the intuitions of the semanticists.[206]

It is not just that general errors of inattention, bias, misconstrual, ignorance, and so on, are more likely to be caught and corrected by professional philosophers, but that the specific training of semanticists *in the field of semantics* results in a domain-specific expertise that is more likely to result in accurate judgments. We've discussed earlier Hales' claims that

> …we should acknowledge that not all intuitions are created equal.... For example, the physical intuitions of professional scientists are much more trustworthy than those of undergraduates or random persons in a bus station.[207]

Again, the claim is that expertise in a domain (for example, in physics) can be acquired via training, and this expertise translates into reliable 'intuitions' when considering cases. Ludwig, like Devitt, argues that in the specific case of semantics (e.g., reference) there is a domain-specific expertise to be acquired:

> We should not expect antecedently that untrained subjects should be in an especially good position to give judgments in response to scenarios involving difficult questions about the semantics of proper names, for this is a domain of considerable complexity where our ordinary vocabulary is not especially precise. We should instead expect that the relevant experts in the field of philosophical semantics will be better placed to give answers which focus on the right features of the cases and what they are supposed to be responding to.[208]

While far from being the only objection to the studies on reference conducted by experimental philosophers, the expertise defense is widely endorsed and, by many, taken as self-evidently to be true: experts in general, including philosophers, are just going to

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[206] Michael Devitt, "Experimental Semantics,"426.

[207] Why always buses? Steven Hales, "Relativism and the Foundations of Philosophy" 171.

[208] Kirk Ludwig, "The Epistemology of Thought-Experiments,"150. Further: "What is called for is the development of a discipline in which general expertise in the conduct of thought-experiments is inculcated and in which expertise in different fields of conceptual inquiry is developed and refined. There is such a field. It is called philosophy." (Ibid.)

be 'better intuiters' in their domains of expertise. It follows, then, that we have a reason to prefer one class of intuitions in theorizing about reference. The apparent challenge posed by variability is thus defused, since the philosophical consensus *has* been that largely unanimous concerning the Gödel-Schmidt.

In responding to the expertise defense, experimental philosophers have made two general moves. First, they claim that the phenomenon of expertise is itself an object of study, and offer evidence that alleged philosophical expertise does not translate into greater intuitive reliability. Second, they argue against the analogy between philosophical and other forms of expertise: while most forms of intuitive expertise can be 'calibrated' against independent data, it is not clear what, if any, external corrective is available to philosophers. I will describe these two counter-arguments briefly before moving on to my own objection to the expertise defense.

First, there is the question of whether or not training in a relevant domain does in fact lead to a consensus of intuition on the 'correct' answer (the causal-historical intuition). Experimental philosophers have worried that

> …given the intense training and selection that undergraduate and graduate students in philosophy have to go through, there is good reason to suspect that the alleged *reflective* intuitions may be reinforced intuitions.[209]

In other words, the worry is that 'expertise' may lead to reinforcement and selection bias instead of increased reliability. There is some evidence for this worry. Machery et al. asked 250 scholars from various disciplines a version of the Gödel-Schmidt case and found a significant difference between the answers of philosophers of language and colleagues from related fields: sociolinguists, historical linguists, anthropological linguists and discourse analysts were less likely to have the 'causal-historical' intuitions, possibly because their respective fields make them more sensitive to 'holistic' meaning

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[209] Edouard Machery et al, "Semantics, Cross-Cultural Style." Weatherson raises a similar point in his "Centrality and Marginalisation," as I noted in a footnote earlier, but does not think this raises a deep issue. At the same time, the paper Weatherson co-authored with Jonathan Ichikawa and Ishani Maitra, "In Defense of a Kripkean Dogma," argues—among other things—that the experiments did not disambiguate between speaker's and semantic reference, ambiguity a philosopher would catch and thus implicitly endorsing an 'expertise' defense after all.

features. Perhaps they are wrong, too; in any event, if the expertise claim is just that academic expertise in a relevant domain (in this case, domains related to language) will make one more likely to have the 'right' intuition, this seems to falsify that claim.[210]

One might argue in turn that the relevant intuitions are more specific, and those of philosophers of language and semanticists—who remain near-unanimous—are the 'real' experts. But, in the field of syntax, there is also empirical evidence that theoretical commitment makes one more likely to have 'intuitions' in line with one's theory.[211] This is a form of bias that is hard to eliminate if one insists on only hewing to an 'expert' judgment. Nor is expertise a blanket against known forms of bias (ordering effects, framing effects, and so on): in Weinberg et al.'s wide-ranging survey of the literature on expertise across scientific domains, the authors state "we conclude that philosophers at this time cannot take it for granted that they are experts," in virtue of the complex and difficult facts about bias and acquired expertise.[212]

Furthermore, there are domains of professional activity in which 'expertise' of the sort under consideration yields no improvement in intuitive judgment.[213] In many instances, expertise creates over-generalization from small sample sizes, resulting in decision-making outcomes that are often as bad as novices—but for different reasons. With expertise, we

> …run a very real risk of introducing *new* systematic sources of error. For example, the expert's extensive experience will make them even more likely to induce a general configural rule from a small sample and then overgeneralize.[214]

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[210] Edouard Machery, Stephen Stich, "The Role of Experiment in the Philosophy of Language."

[211] Edouard Machery, "Expertise and Intuitions about Reference."

[212] For example, even very advanced chess players were susceptible to ordering effects when presented with a series of mating problems. Jonathan Weinberg, "Are Philosophers Expert Intuiters?" 335.

[213] Jonathan Weinberg et al., "Are Philosophers Expert Intuiters?" 340.

[214] Ibid. Over-generalization from small sample size of cases is, I think, a problem in philosophy generally; on this see Jaakko Hintikka, "The Emperor's New Intuitions."

What we need is detailed empirical examination of 'philosophical expertise' in specific domains, the experimentalists contend; we can't be confident until we test.

A second, and in many ways more fundamental, counter-argument to the expertise defense is that philosophical expertise is manifestly disanalogous with expertise in other domains. Expertise is typically acquired from a feedback model where judgment is corrected by independent sources of evidence confirming or 'calibrating' the intuition. As Weinberg puts it,

> One of the most robust consensus findings of the study of expertise is that expert judgments can only become more reliable where experts are readily confronted with clear, reliable feedback on which to train.[215]

Experts in scientific fields—from biology to medicine to physics—have access to the sort of independent feedback required for acquired expertise: a radiologist learning to interpret x-rays can point to independent diagnostic sources to 'confirm' and 'calibrate' their judgments and knowledge-claims across a wide-ranging set of cases in a short amount of time with great confidence in the accuracy of the feedback provided. What can a philosopher performing a thought-experiment point to? How do we find independent calibration of the intuition that the subject in a Gettier case does not *know*?

As I argued in chapter 2, it seems distinctive of philosophical intuition that nothing can substitute for an agent's judgment in generating 'data' that is pressed into service as a form of evidence. The difference is quite significant: the physicist's intuitions can typically be subject to controlled experiment and, while they certainly play a role in what we may call the 'context of discovery,' are not taken to be part of the context of justification. Proponents of the expertise defense are not necessarily committed to the claim that intuitions are likewise part of the 'context of justification,' though *if* (1) intuitions do serve as evidence, (2) the intuitions of experts are reliable enough for serious theorizing and (3) cannot be independently calibrated, the combination of these commitments leads to a view of intuitions as part of the 'context of justification.'

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[215] Jonathan Weinberg et al., "Are Philosophers Expert Intuiters?" 340.

As I noted earlier, the problem of feedback, or 'calibration,' was perhaps first raised in Cummins' "Reflections on Reflective Equilibrium," but it is worth recapitulating his argument briefly. As suggested above, Cumming notes the profound difference between the 'instrument' of intuition in philosophy (when put to evidential usage) and instruments in science:

> Every scientific subdiscipline spends a good deal of effort identifying and correcting errors and artifacts. What is important for present purposes is that an observational technique is deemed acceptable just to the extent that it can be relied upon to produce accurate representations or indicators of its target.[216]

Cummins points to a well-worn example in the philosophy of science: Galileo's telescopes. How to test whether or not the observations Galileo made when pointing the instrument at heavenly bodies were accurate or an artifact of the instrument? The proper response was to point the telescope at something of known size, shape, color, and so on to determine what distortions it introduced; to calibrate it, in short.[217]

The problem is as follows: how are we to calibrate intuition? The targets of philosophical intuition are typically ineffable: it is not as if we can place an experimental subject in a Gettier scenario and wait to see if we can 'observe' knowledge using, say, an fMRI scan. We could calibrate against other experts, but this introduces a regress: against what are *their* intuitions calibrated? "Intuitions could be calibrated," Cummins concludes, "but only on the assumption that there is some nonintuitive access to its targets."[218] This raises the question: what is the actual target of a referential intuition by philosophers? I will argue that it is a norm, not a fact.

4.4   The Target Phenomena of Semantics

Of course, many philosophers resist the idea that referential intuitions require calibration. Eliminativists deny that intuitions play an evidential role at all: if intuitions aren't

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[216] Robert Cummins, "Reflections on Reflective Equilibrium" 116.

[217] Ibid, 116-117.

[218] Ibid.

evidence, there is no instrument ('the faculty of intuition') to calibrate. Deutsch, for instance, argues that the relevant questions in semantics are *a priori*. Intuitions—and, especially, surveys of intuitions—are *a posteriori*. Speaking of 'intuitions as evidence' and 'calibrating the instrument' are simply a form of category mistake. The point generalizes, he argues, to all thought-experimentation in philosophy:

> Suppose we philosophers know that a subject in a Gettier case does not know. How do we know this? The traditional answer is: By *thinking* about the case. Giving this answer appears to commit one to the existence of a priori knowledge [...] for the philosopher who is not a skeptic about *a priori* knowledge, and who conceives of the philosophical method as including a significant *a priori* component, the results of the experimental philosophers' surveys are irrelevant. For the surveys to be relevant, the philosophical questions upon which they allegedly bear must be conceived as being answerable via *a posteriori* methods. But non-skeptical philosophers do not conceive of the questions this way.[219]

Intuitions, for Deutsch, might be the 'causal source' of *a priori* knowledge. However, it is not its *being intuitive* that somehow provides evidence that some judgment is true. Rather, according to Deutsch,

> Kripke's argument against descriptivism succeeds if the Gödel case is a *genuine* counterexample. Whether it is an intuitive counterexample is not clearly relevant, and there is nothing in Kripke's presentation of the case that would lead one to believe that Kripke *thinks* it is relevant.[220]

How do we *know* whether or not a case is a 'genuine' counter-example? Deutsch's answer is the one already given above: we *think* about it. This is enough to acquire *a priori* knowledge. At least, if one is a philosophical expert. As I claimed in the previous chapter, this amounts of a re-labeling, from 'intuitive' to 'genuine,' without addressing the underlying epistemic issue.

A similar move is to mark a distinction between 'genuine' and 'spurious' intuition. This has the same upshot. Whereas Deutsch wants 'genuine' *a priori* counter-examples, not

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[219] Max Deutsch, "Experimental Philosophy and the Theory of Reference," 459, 460.

[220] Ibid., 451.

mere intuitions, Ludwig's 'expertise defense' of intuition claims that any variation in intuition is proof positive that not all intuitions are created equal:

> The very fact that in both the examples we considered there is variation in the subjects' responses shows that, on the assumption that all the respondents are fully competent speakers of English and that there is no ambiguity in the terms used to describe the scenarios and in the questions posed, the responses are not data in the form of intuitions about the application conditions of the concepts involved in their judgments in response to the described scenarios. For identical competencies would yield identical judgments in response were the responses made solely on the basis of those competencies and identical understandings of the scenario, task, question, and adequate thought.[221]

Giving the 'wrong' answer on a case, such as the Gödel-Schmidt, indicates that there was a failure of some kind: attention, bias, misunderstanding, pragmatic effects, and so on, and thus was not a real 'intuition' all along. There is, however, *one* correct answer, the genuinely intuitive one. Incorrect intuitions are not intuitions.

At this point we might suspect a 'parting of the ways.' If one is not sympathetic to intuition, the obvious rejoinder to all this is to ask for criteria to differentiate 'genuine' intuitions and 'genuine' counter-examples from spurious, incorrect judgments. Many philosophers, in contrast, are simply baffled by continued skepticism. Surely it is *obvious* that a Gettier case isn't knowledge and that we refer to Gödel, not Schmidt? And equally obvious that answers to the contrary must be somehow confused?

One way to move forward from this apparent impasse in both the eliminativist and expertise arguments would be to return to the issue of calibration and attempt to clarify what the target phenomena is. The point of calibration is to determine the relative reliability of an instrument by assessing its deliverances about some target phenomena via other means of acquaintance with the target. These other means don't necessarily need to be antecedently known to be reliable (though this is nice): it might be enough that independent means of observation converge to the same input data—on the assumption that both means of observation are not prey to the same 'errors and artifacts.' So what is

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[221] Kirk Ludwig, "The Epistemology of Thought-Experiments," 144-145.

the 'target data' of semantic intuitions? After all, haven't we already established that nothing that substitute for the agent's judgment?

Devitt suggests that referential intuitions can be checked against 'linguistic reality':

> A language is a system of representations or symbols, governed by rules that scientists sometimes posit in a species to explain its communicative behavior ... The 'linguistic reality' that concerns us here is the language in a particular community of humans. Each time members of the community speak the language, we have a piece of 'linguistic usage.'[222]

Devitt claims that intuitions of reference are a form of *indirect* evidence, but that we can, by attending to 'linguistic reality,' acquire "direct evidence about the reference relations themselves" which are simply manifested in usage.[223] It is not entirely clear how to read referential relations directly off usage, but we can leave that to the side; what is important for now is what linguistic reality *consists in*: a rule-governed representational system. On this, there is unanimity.

Deutsch, too, wishes to ground 'the semantic' in roughly the same way:

> More generally, questions about who or what a speaker is 'talking about' in using a term, or questions about who or what a speaker 'refers to' in using a term, are ambiguous questions that can be interpreted as questions about the semantic reference of the term, i.e. *the object assigned as referent by the conventions of the language to the term*, or else as questions about the speaker's reference of the term, i.e. the object to which the speaker intends to refer in using the term.[224]

What is *properly* semantic, Deutsch and Devitt are saying, has to do with the rules, practices and conventions that govern language for a community of speakers. Ludwig, too, claims that semantics consists in "rules" which are "extractable from the practice for their use," which is what Kripke's case asks us to do:

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[222] Michael Devitt, "Whither Experimental Semantics" 27.

[223] Ibid., 27.

[224] Max Deutsch, "Experimental Philosophy and the Theory of Reference" 454. We're interested in semantic reference, of course.

> Using one's own competence in this way to arrive at knowledge of public meanings depends on knowing that one's competence aligns with the norms of the public language ... where questions arise, further checking against the practice of relevant others in the community is called for.[225]

For all three, semantic competence is ultimately grounded in the conventions of a shared language, which is required for communication; the commitment is likely inherited from Lewis' influential work. This construal of the 'target' provides a neat explanation of the *apriority* of referential intuitions (or judgments) for Deutsch: the consequences of rules, whether of language or chess (or chmess!) are discernable *a priori*.[226] Reflecting on a hypothetical case is, effectively, to apply the (implicit) rules governing the usage of a term or class of terms to make explicit what the conventions, and their consequences, really are. Sometimes the results are surprising.[227]

This leads us to the ambiguity I wished to exploit: philosophers take themselves to be carrying out a descriptive project when they are, in fact, engaged in a normative practice. None of the theorists defending referential intuition on the basis of expertise (or *a priori* knowledge, even if not understood as "intuition") quoted above deny the *possibility* of, say, descriptivist names.[228] Rather, they claim that as a matter of contingent fact, the causal-historical theory is for the most part true of our practices:

> If Kripke were wrong about the Gödel/Schmidt case, that would at most show that there are a few more descriptive names than we thought there were. But since the existence of some descriptive names is consistent with the causal-historical theory of reference, the existence of a few more is too ... the truth of the causal-historical theory of reference doesn't turn on whether there are few descriptive names, or very few descriptive names.[229]

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[225] Kirk Ludwig, "The Epistemology of Thought-Experiments," 132.

[226] Daniel Dennett, "Higher-Order Truths about Chmess."

[227] "Working out these a priori truths about chess is not child's play. Proving just what is and is not possible within the rules of chess is an intricate task, and mistakes can be made that get perpetuated." Ibid. 39.

[228] Devitt: "This is certainly possible—even the most ardent causal theorist should allow that there could be "names" covered by a descriptivist theory—but it seems to me extremely unlikely." *Experimental Semantics* p. 428. I've already pointed out in 4.2 that Kripke disagrees on this point: non-rigid names aren't names worthy of the name 'name.'

[229] Jonathan Ichikawa et a., "In Defense of a Kripkean Dogma" XX

This applies not just to the correct theory of reference for proper names, but to other terms, such as natural kinds. As Frank Jackson writes,

> …we should agree that the word 'water' might have been used to refer to XYZ on Twin Earth and H_2O on Earth. Hilary Putnam's Twin Earth case isn't an impossibility proof … [philosophers] find it intuitively compelling that 'water' doesn't refer to XYZ.[230]

The pleasing methodological conclusion is that philosophical expertise consists in expertise at discerning the norms, practices, and rules governing a given linguistic community—which, first, could have been otherwise, but, second, also admit of internal plasticity and malleability: there *could* be descriptivist 'names,' Devitt, Ichikawa et al., admit. Just very few in our language, as it turns out. These two features leave open the possibility that for some other language, there could be more 'descriptivist' names and, *eo ipso*, variation in the conventions surrounding semantic reference.

The 'expertise defense,' on this interpretation, asserts that philosophers are experts at discerning the actual conventions that happen to govern our shared ordinary language (which could have been otherwise). I think this is incorrect. Philosophers of language take themselves to be *describing norms*; in contrast, I think they are *applying norms*. These norms reflect, in turn, metaphysical, epistemological, and modal commitments specifically acquired in philosophical training. Further, when theorizing about natural language, an appropriately explanatory theory should refrain from importing such commitments: as I argued in 4.2, Kripke himself took the explanatory task to be about describing the actual norms of ordinary language, and the accounts of the 'target phenomena' above suggest the same. Semantic theory should follow Wittgenstein's dictum: 'don't *think*, but *look*.'[231] It has not done this; and philosophers of language have, instead, proposed languages we should be speaking. I will explain and defend both claims in the next section.

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[230] Frank Jackson, review of *Experimental Philosophy*. I noted this already in my previous discussion of the Twin-Earth case (2.1).

[231] Ludwig Wittgenstein, *Philosophical Investigations* §66.

4.5   The Language We Ought to Have

Philosophy of language, I contend, is concerned with the language philosophers seem to think we *ought* to have; or, at least, the language we would need to make certain kinds of philosophical claims. In particular, philosophers who theorize about language engage in idealizations that are specifically designed to facilitate philosophical theorizing. As Ludwig writes, in his defense of appeals to intuition,

> Thought-experiments are usually conducted under certain idealizations about language, namely, the terms used to describe scenarios and ask questions and which are to express the target concepts are all semantically complete and consistent. However, many natural language predicates are vague or in other respects semantically incomplete, or, alternatively, inconsistent.[232]

The proposal is, roughly, that conceptual analysis should remove this undesirable vagueness and inconsistency, and the theories of language put forth by philosophers are the means to that end. The expertise of philosophers consists in the expertise of applying the normative ideals guiding this project. This has little to do, I submit, with a genuinely explanatory account of natural language. There is little evidence that natural languages are typically governed by the idealizations prevalent in philosophical analysis. Philosophy has given us a proposal for using language masquerading as a theory of language.

We should note that the 'troublesome' features of natural language were apparent all the way back to Frege, and in fact served as one of the motivations for the elaboration of a formal alternative. Frege, however, did not think that the predicates of natural language were in need of theoretical correction. He was acutely aware of the "softness and instability of language which nevertheless is necessary for its versatility and potential for development," and understood the development of formal systems as a way of introducing "a system of symbols from which every ambiguity is banned, which has a

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[232] Kirk Ludwig, "The Epistemology of Thought-Experiments" 134.

strict logical form from which the content cannot escape."[233] But nowhere did Frege indicate that he thought natural language should be corrected:

> Whether features of a language count as virtues or defects will depend on the purpose for which we want to use the language. Features of natural language that are defects, given Frege's specialized purposes, are desirable for other purposes. *Begriffsschrift* is not an ideal language. It is 'a device invented for certain scientific purposes and one must not condemn it because it is not suited to others.' [...] The demands that Frege identifies as the demands of truth should be seen as part of a regulative ideal for science. But there is no reason to assume that any sentences of natural language actually satisfy the demand.[234]

What philosophers are doing when they claim that, for instance, modal desiderata impinge on the 'correct' theory of reference, is put demands to language that go well beyond the conventions of any naturally-formed group of speakers. It is not a *theory* of any natural language. It is, rather, the proposal that meaning should hew to a set of philosophically-inspired constraints.

These are radical claims. I will make the following few points in defense of the claim that 'expertise' in intuition is, in fact, expertise in a normative project of correcting perceived shortcomings of natural language. I will consider two components of the philosophical normative ideal, *naturalness* and *logical form*; and then argue that these norms are inessential to (and often empirically absent in) natural language. I will conclude that philosophical expertise consists in a form of trained expertise at these disciplinary norms, not expertise at discerning the conventional features of natural language.[235]

4.5.1   Naturalness

I will begin with 'naturalness.' While the Gödel-Schmidt case is about the reference of proper names, the guiding intuition of the case has been extended to 'natural kind' terms as well. The argument I wish to present is simpler in this instance and will serve as a

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[233] Gottlob Frege, *On the Scientific Justification of the Concept-Script* quoted in Joan Weiner, "Understanding Frege's Project," 42.

[234] Joan Weiner, "Understanding Frege's Project," 43, 48.

[235] I choose two for brevity's sake, but this is not exhaustive: for example, 'semantic determinacy' is often assumed, but I lack the space to discuss it here.

rough template for the apposite argument with regard to proper names. In brief I will argue that the 'expert' intuitions of philosophers in the domain of semantics are biased in favor of specific normative commitments and restrictions that result in descriptively inadequate semantic theories.

The 'naturalness' constraint is this: many philosophers defending the use of intuitions believe that natural languages ought to be classificatory systems that aim to represent theoretically significant concepts. Sometimes the point is explicitly acknowledged; more often it is simply assumed. Frank Jackson is of the former camp: in a review of a collection of papers on experimental philosophy, he writes that

> One might have expected that 'Gödel' and 'water' had to refer to whatever has the properties that Gödel and water, respectively, are most famous for. Kripke and Putnam showed that this expectation is a mistake. They also showed that it would be good to have words that worked as sources of information about baptized objects and that picked out kinds, and this is true independently of whether or not some word is or is not doing its job.[236]

In other words, the moral to be drawn from the Gödel-Schmidt and Twin-Earth thought-experiments is, first, that descriptivism doesn't necessarily hold of certain kinds of terms (not that causal-historical theories *do* hold); and second, that it might be *good* to have words operating in certain ways and this is so whether or not our terms are 'doing their job.' We can imagine a 'bad' language that ought to reform its names and predicates: there is nothing necessary about the semantic task words are put to. As I argued above (section 4.4) it is commonly granted that there could be 'descriptivist' words and 'functional' predicates. Jackson here suggests that the upshot of thought-experiments is normative, telling us something about what would be 'good' in language.

Other philosophers of language are less explicit, but the commitment is still detectable. For Devitt, while the direction of theoretical significance in semantics is primarily psychological, he still characterizes the "basic semantic task" as saying "what meanings

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[236] Frank Jackson, "Review of *Experimental Philosophy*."

are."[237] To accomplish this task, Devitt proposes a methodology 'for descriptive tasks in general.' The question is,

> What constitutes some property we ascribe, being an *F*? … How then do we tell what is common and peculiar to *F*'s 'in all possible worlds?' … First we must identity some apparently uncontroversial examples of *F*'s and non-*F*'s. Second, we must examine the examples to determine the nature of being an *F*. … this second stage is a straightforwardly scientific one … thus we may conclude that some of the things identified as *F*'s are not; for example, whales are not fish.[238]

The methodology here is that in identifying the 'meanings' of the terms of our language, we should reflect on necessary and sufficient properties across possible worlds, using scientific methodology to ascertain the nature of the thing itself—in other words, generating the relevant necessary *a posteriori* truths about being an *F*. This 'naturalness' restriction claims that the categories of language will, over time, evolve in the direction of theoretical significance; and systematically applying this method to elucidating the nature of being an *F* will accomplish this: for example, we will discover that whales are not fish and never were, and revise our language accordingly to accommodate this fact.

Brian Weatherson makes the same point as Devitt, arguing for the naturalness restriction when appealing to intuition during conceptual analysis:

> ...the concept so analyzed should be theoretically significant, and should be analyzed in other theoretically significant terms. This is why we now analyze 'fish' in such a way that whales aren't fish, and 'star' in such a way that the sun is a star. This is not just an empirical fact about our language. Adopting such a constraint on categories is a precondition of building a serious classificatory scheme, so it is a constraint on languages, which are classificatory schemes *par excellance*. Even if I'm wrong about this, the fact that we do reform our language with the advance of science to make our predicates refer to theoretically more significant properties shows that we have a commitment to this restriction [...] When the ancients said "Whales are fish", or "The sun is not a star", they simply said false sentences.[239]

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[237] Michael Devitt, *Coming to our Senses*, 54.

[238] Ibid, 72-73.

[239] Brian Weatherson, "What Good are Counter-Examples?" 7.

Again, the claim here is that ordinary language meanings are intended to track theoretically significant concepts, which is not 'just an empirical fact,' but something more, a 'constraint' on languages. Weatherson adverts to our apparent practices: we 'intend' for our predicates to refer to natural properties, and reform our language accordingly. I will focus on Weatherson's arguments in what follows, though the point generalizes.

Linguistic intentions have always played an important role in the causal theory of reference: in *The Meaning of Meaning*, Putnam claims of the Twin-Earth case that 'we' intended 'water' to serve as a natural kind term, because our form of ostensive definition presupposes a theoretical relation of sameness across samples.[240] But as I argued in section 2.1 and 3.3.2, intuition is what ultimately supports philosophers' appeal to linguistic intent. And, if we do wish to affirm that the 'target phenomena' of semantics are the conventions of ordinary language used by actual speakers, the following conditional cannot be easily rejected:

> If it should turn out that only philosophers baulk at classifying XYZ as water, I am ready to defer in my usage to the non-philosophical majority and say that "water", like "glue", is not the name of a kind with a chemical essence.[241]

That there are plenty of such 'functional' terms cannot be denied: the question is, why isn't 'water' a functional term? It can't be *because* there is a candidate essence: this denies the conventional nature of language that proponents of the expertise defense claim is their object of study (as we've already seen, the connection is not necessary). If it is a contingent fact about what speakers actually do, then we cannot assume the naturalness restriction always constrains meaning, and should instead conduct careful experimental investigation of actual speakers. If it is instead a normative project of rational reconstruction, then philosophers of language should not criticize experimental philosophy, for both are engaged in separate projects.

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[240] Hilary Putnam, "The Meaning of 'Meaning,'" 142: "The key point is that the relation 'same' is a theoretical relation: whether something is or is not the same liquid as *this* may take an indeterminate amount of scientific investigation to determine."

[241] Christopher Hughes, *Kripke: Names, Necessity and Identity*, 63.

Consider, to make this perhaps more clear, Devitt's and Weatherson's example (above) that 'whales are not fish,' and that 'the ancients' were simply uttering false sentences. We should begin with the observation that the ancients, whoever is meant by that, certainly never said that whales were or were not fish. (They obviously never *uttered* the words 'whale' or 'fish.') It is true that the ancient Greek word κ⬚τος (ketos) was used to denote *any* large creature that lived in the sea, including what we now consider 'fish' but also seals, sharks, whales, and sundry purported sea monsters and serpents. 'Ketos,' if the corpus of ancient Greek is any indication, was just a functional category like 'glue,' and not a 'kind' category.[242] The ancients weren't wrong or saying 'false sentences.' More likely they had a different concept that cannot be assimilated to our own 'fish' and 'whale,' and any usage-change would be driven by practical considerations.

Why, then, the focus on naturalness? The 'naturalness' of a predicate of ordinary language is a constraint that was already seen, in the early days of the turn towards the causal-historical theory, as a commitment required to make sense of the theory. David Lewis writes

> This constraint looks not to the speech and thought of those who refer, and not to their causal connections to the world, but rather to the referents themselves. Among all the countless things and classes that there are, most are miscellaneous, gerrymandered, ill-demarcated. Only an elite minority are carved at the joints, so that their boundaries are established by objective sameness and difference in nature. Only these elite things and classes are eligible to serve as referents. The world—any world—has the makings of many interpretations that satisfy many theories; but most of these interpretations are disqualified because they employ ineligible referents. When we limit ourselves to the eligible interpretations, the ones that respect the objective joints in nature, there is no longer any guarantee that (almost) any world can satisfy (almost) any theory. It becomes once again a worthy goal to discover a theory that will come true on an eligible interpretation, and it becomes a daring and risky hope that we are well on the way toward accomplishing this.[243]

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[242] Aristotle's *History of Animals* distinguished whales from fish in virtue of morphology, so it wasn't a question of *not knowing* a distinction could be made. Rather, why bother?

[243] David Lewis, "Putnam's Paradox" 227.

Reflecting on Lewis' remarks here, it becomes perfectly clear that the kind of 'expertise' he has in mind is a normative exhortation to have certain kinds of predicates and concepts and not others.[244] (Part of the motivation might have been to articulate an ontology that evaded some of Kuhn's relativistic-sounding arguments in the philosophy of science: under a causal theory, reference is preserved across theory-change). But of course if we actually attend to natural language, it is perfectly clear that 'unnatural,' functional categories—vague, shifting, contextual categories that reflect local human interests—are everywhere, and perhaps predominate. Off the top of my head: chalk, rubber, fire, swim, fly, fruit, bug, vegetable, almost all intentional talk ('the computer wants, the internet thinks, the polls urge'---only the most pedantic squirm at such natural usages), ash, blue, slug, berry, and so on.

To expand on just a few examples. 'Chalk' refers to any chalk-like substance, not just calcium carbonate but also magnesium silicates, calcium sulfates (blackboard chalk, technically a form of salt), and other functionally similar compounds. But according to the 'naturalness constraint', if I call my climber's chalk (pure magnesium carbonate) 'chalk' I am speaking falsely, since 'chalk,' on analysis, ought carve at some theoretically significant property like CaCO3, not some gerrymandered functional description of superficial properties. (You could say "technically, X is not chalk, since 'chalk' is in fact Y," but the qualifier 'technically' is doing half the work by asserting a dubious philosophical norm—the correct response is "stop being a pedant.") We do, in fact, have a disambiguating vocabulary (that of chemistry). This does not mean we should drop the functional, non-kind definition of 'chalk' from our everyday language, which is perfectly suited to our interests, purposes, and lives.

Another example: 'Slug' refers to any slug-like creature, not to a specific species or group of species. Rather, it is a polyphyletic term of ordinary language that brings

[244] It is particularly telling that Weatherson's *What Good are Counter-Examples?* (2003) take pains to 'refute' the work of Rosch et al. on prototype theory (the section titled "Against the Psychologists"); it indicates he takes the project to be *about* ordinary language, and that such empirical work needs to be addressed for it threatens his account. But on my view, Weatherson is the one making a category mistake: he needn't worry about Rosch et al. at all if he interpreted his own proposal as essentially normative.

together various evolutionary lineages on the basis of superficial similarity. Within the domain of molecular biology polyphyletic classification is avoided, since it reduces predictive power (superficial similarity is typically the product of accidental convergent evolution). Thus 'slug' is not a natural kind term, or a term for which we should expect to find a 'natural' definition. Future linguists, hopefully, will not chastise us for falsely saying that *l. maximus* and *a. vulgaris* were the same sort of thing.

Of course the philosopher could retort that their story is compatible with the existence of some, or many, non-kind concepts, and certainly doesn't show that *philosophical* concepts are 'unnatural.'[245] But in the context of philosophy of language, this seemingly minor concession gives up too much: because then it becomes again an empirical matter of investigating which concepts are used in which way, and more importantly *why*. And if 'use' carries the day, then we *do* need to ask people at bus stations what they think of 'water' and 'XYZ.' Further, it becomes quite plausible indeed that the expertise of philosophers reflects theoretical commitment and training to these norms: analytic philosophers, because of their training, tend to over-apply these norms.

A similar story can be told for rubber (synthetic rubber is still rubber, making the case similar to Twin-Earth: we have an XYZ for rubber, and still call both 'rubber'). It is obvious that 'bug' does not aspire to carve joints but equally obvious it is in no need of linguistic reform. It is *not* obvious that XYZ wouldn't be called water should it be invented tomorrow (given a sufficient marketing budget); indeed, it is not clear that 'water' in the ordinary sense is a natural kind: tea is chemically closer to H_2O than seawater, but only the latter is water.[246] Iceberg lettuce is, apparently, 96% H_2O, approximately the same as seawater. As Chomsky once argued, a water filter that uses teabags produces water, but a chemically identical cup of tea is tea, not water: "whether

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[245] Ibid., 14. Though it is not clear to me why 'philosophically interesting' should happen to delineate a class of concepts that are all natural.

[246] The 'extract solids' added to the water by boiling and steeping the tea is estimated to be less than .5% of the average cup of tea. Matthew Harbowy et al., "Tea Chemistry" 423.

something is water depends on special human interests and concerns," at least in the non-technical sense of 'water.'[247]

Again, all parties to the debate admit that, should intuitions align to the verdict that XYZ is water, then 'water' is not the name of a natural kind. The philosopher says it would be good to have such terms, and has the corresponding intuition about the referent in the Twin-Earth case. Why would it be good? Well, it's good for science and classification. But even this is dubious. Passing familiarity with the jargon of any science shows a bewildering variety of kinds of terms. Geologists use the term *tephra* to refer to any material produced by volcanic eruption regardless of composition, size, and emplacement mechanism. 'Cerebral palsy' is an umbrella term for any motor impairment resulting from several different kinds of brain insult. Such examples can be multiplied endlessly.[248] It is not an obvious requirement or desiderata of science to only have terms that carve at the joints, where joints are defined via considerations of naturalness.[249]

How semantically widespread is unnaturalness? Opinions differ here. Paul Pietroski thinks most terms and predicates are 'unnatural':

> Since almost all natural language predicates are vague, this makes it hard to even *say* which function is alleged to be the valuation of a given predicate. This is often viewed as a sniggly point which does not really challenge the idea that the valuations of natural language predicates are functions. I find this attitude baffling.[250]

Weatherson disagrees:

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[247] Noam Chomsky, *New Horizons in Language and Mind*, 128.

[248] Physicist Stephen Hawking recently said there are no black holes since the notion of the 'event horizon' is incompatible with quantum theory; this assumes that 'black hole' is synonymous with a description. Putnam would disagree: when we use a word, "we intend to refer to whatever has the same nature as the normal example" of the thing. If it could turn out that "pencils are organisms," surely it could turn out that 'black holes have no event horizons.' Therefore, Stephen Hawking is wrong to say there are no black holes. I think this is too strong a conclusion to read off semantic theory, and a *reductio.* (Zeeya Merali, "Stephen Hawking: 'There Are no Black Holes'"; Hilary Putnam, "The Meaning of Meaning" 161-162).

[249] The utility of naturalness in the context of science is an issue I cannot discuss further. My suspicion is that the primary impetus is metaphysical, not scientific.

[250] Paul Pietroski, "The Character of Natural Language Semantics," 228.

> One occasionally hears people deride the assumption that there are necessary and sufficient conditions for the application of a term, as if this was the most preposterous piece of philosophy possible. Really, this assumption is no more than the assumption that dictionaries can be written.[251]

What I wish to suggest is that when philosophers claim that natural language must follow some such theoretical convention in absence of careful investigation of linguistic practice, they are adopting substantive normative commitments, not describing 'meaning' in general terms. The fact is we are not sure how much naturalnesss is actually in language, under what conditions naturalness develops, and what factors and constraints affect semantic development.

Sometimes the 'naturalness' issue is less clear because ordinary language practices allow for contextual sensitivity of reference and definition (as in the chemist's definition of chalk versus ordinary usage). I will give another, somewhat mundane example, following Weatherson's critique of Rosch et al.'s studies. Weatherson disputes that their experimental evidence demonstrates that ordinary terms like 'fruit' and 'vegetable' are Wittgensteinian cluster concepts. After all,

> It is not a brute fact language that tomatoes are a fruit. It is not just by magic that we happened to come to a shared meaning for fruit that includes tomatoes, and that if faced with a new kind of object, we would generally agree about whether it is a fruit … there will be a short list of necessary and sufficient conditions for being a fruit. But for this example … 'fruit' was relatively arbitrary, so there will be a short list of necessary and sufficient conditions for being an *F*, for pretty much any *F*.[252]

Weatherson appeals to the authority of the Oxford English Dictionary as further evidence: here's a definition, plain as day, which explains why tomatoes are fruit. But again here I think we have a case of a 'technical' definition running alongside an everyday definition: tomatoes are not a fruit until we cue the technical context, which is,

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[251] Brian Weatherson, "What Good are Counterexamples?," 13.

[252] Brian Weatherson, "What Good are Counter-Examples?," 14. I wish there was solid empirical evidence on whether or not 'most' terms in English have such conditions, but no such evidence exists so far as I can tell. Weatherson's argument that he chose the example arbitrarily, therefore something is demonstrated about the totality is clearly unsound.

in fact, rather counter-intuitive to the ordinary speaker. There's a culinary concept and a 'natural' concept, and we shift between these two definitional axes with relative ease—though in practice likely use the former more often than the latter.

The two definitions are typically called 'culinary' and 'botanical,' acknowledged in reference works, dictionaries, and even enshrined in law: a unanimous 1893 United States Supreme Court decision ruled that the tomato was a vegetable for taxation purposes, while acknowledging its botanical status as a fruit. The decision is worth quoting:

> Botanically speaking, tomatoes are the fruit of a vine, just as are cucumbers, squashes, beans, and peas. But in the common language of the people, whether sellers or consumers of provisions, all these are vegetables which are grown in kitchen gardens, and which, whether eaten cooked or raw, are, like potatoes, carrots, parsnips, turnips, beets, cauliflower, cabbage, celery, and lettuce, usually served at dinner in, with, or after the soup, fish or meats which constitute the principal part of the repast, and not, like fruits generally, as dessert … this is the principle use to which they are put. Beyond the common knowledge we have on this subject, very little evidence is necessary, or can be produced. [253]

Presumably confusion could have been avoided if the botanists had introduced a new nomenclature instead of terms already coined. In any event no clearer evidence could be provided of a distinction between ordinary usage and technical definition.

Consider another culinary example that might jar our intuitions: berries. Berries, as it turns out, have a nice botanical definition that provides necessary and sufficient conditions for berry-hood: a berry is "a fleshy fruit produced from a single flower and containing one ovary," which provides us with a short list of conditions and a means of identifying what counts as a berry.[254] Is this the meaning of berry in ordinary language? Only if strawberries, raspberries, blackberries and mulberries aren't in fact berries, while avocados, bananas, watermelons, and pumpkins *are* berries. As the Oxford English Dictionary notes 'berry' most often means "any small globular or ovate juicy fruit, not having a stone," even though per the botanical definition this implies that "many of the

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[253] Nix v. Hedden, 149 U.S. 304, 149.

[254] Wikipedia, "Berry."

fruits properly so called, are not berries" even though "botanically, the name … includes the cucumber, gourd, and even the orange and lemon."[255] Modern plant biology has discouraged use of the term berry in favor of more discriminate categories that reflect the development and structure of 'fruit,' and even this latter term is rife with controversy.

The Encyclopedia Britannica discusses fruit as an ordinary language concept, contrasting it with the relevant scientific one ('gymnosperm seed'): "the concept fruit," the articles notes "is based on … an odd mixture of practical and theoretical considerations," noting that it brings together a motley of variegated botanical phenomena.[256] The article goes on to say,

> As strikingly exemplified by the word *nut*, popular terms often do not properly describe the botanical nature of certain fruits. A Brazil 'nut,' for example, is a thick-walled seed enclosed in a likewise thick-walled capsule along with several sister seeds. A coconut is a drupe (a stony-seeded fruit) with a fibrous outer part. A walnut is a drupe in which the pericarp has differentiated into a fleshy outer husk and an inner hard 'shell;' the 'meat' represents the seed … a peanut is an indehiscent legume fruit. An almond 'nut' is the 'stone' … botanically speaking, blackberries and raspberries are not 'berries' but aggregates of tiny drupes. A juniper 'berry' is comparable to a complete pine cone. A mulberry is a multiple fruit … a strawberry represents a much-swollen receptacle … bearing on its convex surface an aggregation of tiny achenes (small, single-seeded fruits).[257]

Such caveats are a matter of routine in reference works, which carefully distinguish between the definitions of everyday usage and the specialized distinctions employed by specialists in the relevant field.[258] Ordinary usage tracks human interests; if we stopped cooking and all became botanists, perhaps usage would shift.

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[255] Oxford English Dictionary, "Berry."

[256] Encyclopedia Britannica, "Gymnosperm Seeds."

[257] Ibid.

[258] The United States Geological Survey, for instance, claims that there are no definitions for the geographical terms of everyday language such as river, lake, mountain or hill: "there are no official definitions for generic terms as applied to geographic features ... broad agreement on such questions is essentially impossible, which is why there are no official feature classification standards."

In light of this, it is not clear that Weatherson's appeal to the OED can sustain his views on naturalness restrictions in semantics. It seems more natural to the semantic facts that there are two concepts at play in the case of 'berry,' one a functional concept that is more widely used and the second a 'natural kind' concept that, while more theoretically significant is unwieldy and largely useless in everyday contexts. As is obvious, functional terms are rather distinct from natural kind terms in their modal profile: for example, if 'water' was a functional term then it would not be the case that water is necessarily H_2O. Many of the cases I have just surveyed are clearly functional terms (chalk, slug, berry) in ordinary language; further, there are no compelling reasons to adopt alternative specialist definitions.

The worry I am raising here is that expert intuitions about the meaning of terms will tend to favor specialist definitions over ordinary meanings, because of philosophical training and theoretical commitment. This is incompatible, I suggest, with the view that the target phenomena of semantic theory is ordinary language. Far from idle speculation, there is compelling empirical evidence for this. The form of natural kind classification Devitt, Weatherson and Lewis are suggesting as 'distinctive' of language is, in fact, a peculiarity of modern times. Evidence from IQ testing over the last 125 years shows gains in scoring that can *only* be accounted for in terms of large-scale adoption of new cognitive norms that reflect the desiderata of the tests. Noting the rise in average IQ scores over the last century (dubbed the 'Flynn Effect') that would make the 'average agent' a dullard today, Flynn writes of the new norms that

> …the first distinction is that between pre-scientific and post-scientific operational thinking. A agent who views the world through pre-scientific spectacles thinks in terms of the categories that order perceived objects and functional relationships. When presented with a Similarities-type item such as ''what do dogs and rabbits have in common," Americans in 1900 would be likely to say, ''You use dogs to hunt rabbits.'' The correct answer, that they are both mammals, assumes that the important thing about the world is to classify it in terms of the categories of science. Even if the subject were aware of those categories, the correct answer would seem absurdly trivial. Who cares that they are both mammals? That is the least important thing about them from his point of view. What is important is

orientation in space and time, what things are useful, and what things are under one's control, that is, what does one possess.[259]

Standard IQ tests favour abstract classification, causal relationships, and analytic thinking. As we've gotten better at this—through training—our experience of what is intuitive has changed. And analytic philosophers are exceedingly well-trained in these kinds of cognitive norms.

Such classification is only *one* of the things we can do with language, and it still does so in an often stilted, artificial way, as I argued above. But back a hundred years ago, it was *not* obvious at all that the naturalness constraint was distinctive of the semantics of language:

> Today we are so familiar with the categories of science that it seems obvious that the most important attribute things have in common is that they are both animate, or mammals, or chemical compounds. However, people attached to the concrete will not find those categories natural at all. First, they will be far more reluctant to classify. Second, when they do classify, they will have a strong preference for concrete similarities (two things look alike, two animals are functionally related, for example, one eats the other) over a similarity in terms of abstract categories.[260]

It is important to remember that the 'low' average IQ scores of a hundred years ago do *not* reflect shortcomings in the intellectual ability of our close ancestors. By today's scores, the average agent in 1900 would have a score anywhere from 50-70, which, for diagnostic purposes, is considered mentally handicapped; but the average agent a hundred years ago was not so handicapped. They had not internalized the norms of 'scientific' intelligence testing, which share much in common with the proposed linguistics norms of the philosophical community.

A dilemma presents itself to the philosophers committed to the naturalness constraint. To say the 'ancients' got it wrong, or were speaking falsely, with their predicate 'ketos' is, in effect, to claim that for most of human history most people were ignorant of the true

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[259] James Flynn, *What is Intelligence?* 24.
[260] Ibid, 26.

meaning of many terms in their language, since they had no idea what the 'natural' referents of their predicates in fact were. (Prior to this research, for instance, I was—according to naturalness—mistaken in thinking of strawberries as berries and gourds as not-berries, and, I wager, so is everyone else who is not a plant biologist. *Mutatis mutandis* for other terms.) The other horn of the dilemma is to say that there are probably more functional terms that we'd like to admit, and that the 'naturalness' constraint is normative. The fact is we have no idea how many ordinary terms are functional, whether or not most of our concepts are 'cluster-concepts,' and to what extent meaning in other languages, synchronically and diachronically (natural language has been around for at least 50,000 years), exhibited degrees of naturalness.

If we are genuinely interested in a descriptively adequate amount of meaning, we should take the second horn of the dilemma: most speakers are not speaking falsely, just as it is obvious that the average 19th-century person was not mentally handicapped. Again, we conclude that the theories presented by philosophers reflect normative constraints appropriate perhaps for a project of rational reconstruction. An adequate scientific theory of natural language in humans needs to go beyond such proposals and explain how natural languages are acquired and deployed. Reconstruction is not a project without its merits, yet a properly descriptive account of natural language should be able to tell us something useful about what kinds of norms are possible.

4.5.2   Modal Constraints

To get back to reference: the argument here is *not* that descriptivism is true or that the causal theory is false. What I *am* saying is that so-called philosophical 'expertise' reflects acquaintance with a sophisticated sort of linguistic proposal that can be justified in terms of its utility in carrying out certain kinds of endeavors. As such, philosophical expertise cannot be used to defend descriptive theses about 'ordinary language,' for the intuitions of philosophers are *about* disciplinary norms. Thus appeals to 'philosophical expertise' really only carry weight in the context of the normative proposal on offer. I chose to focus on *kinds* (and 'naturalness') first because it illustrates the point vividly. But the same applies to proper names. Just as 'naturalness' asks us to assign 'natural' semantic values to terms and predicates, modal constraints ask us to consider modal truths when attempting to

understand the semantics of proper names. However, it seems clear that we can use names in ways that fundamentally violate any purported modal restrictions: semantic facts and modal facts come apart.

Suppose I say: "Imagine Nixon, *that* man, had an identical twin." But distinctness, like identity, is necessary in S5 modal logic.[261] Monozygotic twinning, that is, spontaneous division of the zygote into two embryos, is a random, contingent event. If Nixon1 and his twin, Nixon2, are *necessarily* distinct as modal logic requires, then neither are identical to the sole Nixon in the possible world where the contingent event of twinning did *not* occur even if all three originated from the same zygote. For if the twins Nixon1 and Nixon2 are necessarily distinct from each other, neither can be identified with non-twin actual Nixon. (Imagine the reverse scenario: could the Olsen twins not have been twins? Well, the twinning event might not have occurred, and then only one child was born. In that world, which of our actual 'twins' corresponds to the possible single child? Choosing either seems arbitrary, and both violates distinctness: if a=c and b=c, by transitivity, a=b. 'Neither' is the only viable option.)

If 'Nixon,' in our language, is just a rigid designator, there is no possible world where *that* man had an identical twin. But it plainly makes sense to utter a sentence like "Imagine Nixon had an identical twin." It is quite natural to think thoughts for which there is no corresponding possible world, strictly ('rigidly') speaking, and this tells us that the semantical framework is not adequate to the perversities of natural language. We switch between description and rigidity fluidly—it does not seem to be an important cognitive boundary. That suggests to me that the semantics on offer, even if it could *emulate* aspects of natural language, probably won't make for empirically adequate psycholinguistics. Whatever meaning is, it is insensitive to the modal-ontological facts.

Stipulated psychic continuity also appears to violate modal restrictions: there are no possible worlds where the emperor Kuzco, a man, gets turned into a llama and remains

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[261] Proof by contradiction: suppose (a≠b) at some world w and (a=b) at w' (so that there exists one possible world where a and b are not distinct.) But if (a=b) at w' is true, it is necessary; and if it is necessary, it holds at all accessible worlds. w is accessible from w'; therefore, (a=b) & (a≠b) holds at w.

essentially the same person. Yet small children follow such stories, and concomitant referential attributions to Kuzco the man and Kuzco the llama, with no difficulty. What matters is not the metaphysics of modality, but the uses of the name and the conceptual resources we can bring to bear. This is particularly clear with place names. Chomsky writes that

> London is not a place. Rather, it is at a place, though it is not the things at that place, which could be radically changed or moved, leaving London intact. London could be destroyed and rebuilt, perhaps after millennia, still being Lonodn; Carthage could be rebuilt today, just as Tom Jones, though perfectly concrete, could be reincarnated as an insect or turned by a witch into a frog … but Tom Jones all along—concepts available to young children without instruction or relevant experience. The abstract character of London is crucial to its individuation. If London is reduced to dust, *it*–that is, *London*—can be rebuilt elsewhere and be the *same* city, London.[262]

We need to separate out two 'could' questions: the linguistic from the modal. On the former reading, all the usages above are perfectly natural for a speaker, even if, on the latter 'modal' reading none of these 'coulds' represent legitimate possibilities. It could be objected to a story that, technically, Tom Jones cannot be turned into a frog, for Tom Jones is essentially a human being; though this would be to misunderstand the practice of story-telling and it would constitute a mistake to 'reform' language to exclude such usages. Likewise, one might endeavor to discover the modally correct ontology of cities, and in doing so discover that London could not be destroyed and rebuilt and be the 'same' London; this has no bearing on whether or not we wish to say that London was destroyed and rebuilt (any more than the botanically correct definition of berry should convince me to refer to pumpkins as berries, but not strawberries).

Indeed, the distinctions and entailments drawn in modal logic appear to place no real semantic constraints on speakers whatsoever, however useful these may be in metaphysical analysis. Yet as I argued above, Kripke argues as if he is engaged in a descriptive project:

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[262] Noam Chomsky, *New Horizons in the Study of Language and Mind*, 126.

> I came to realize ... that the natural intuition that the names of ordinary language are rigid designators can be in fact be upheld ... it was a short step to realize that similar remarks applied to terms for natural kinds.[263]

It is clear that, for Kripke and those working in his wake that they conceived of their project not as a proposal, but as a description of existing norms in 'language'. They speak, again and again, of 'ordinary,' 'actual,' and 'natural' language. But actual language is manifestly *not* beholden to the logical restrictions imagined by philosophers. Therefore, again I conclude that the expert intuitions of philosopher reflect theoretical commitments that bias their responses: because philosophers carefully consider the modal profiles of entities when determining what is semantically acceptable to say about them (viz., 3.3.3 on the problem of 'unwanted necessity') they generate intuitions that are too restrictive.

The anthropological evidence is clear that, without specific training and education into scientific norms, restrictions of naturalness and modality as conceived by contemporary philosophers are largely absent or modified by different epistemic norms:

> If the everyday world is your cognitive home, it is not natural to detach abstractions and logic and the hypothetical from their concrete referents. It is not that pre-scientific people did not use abstractions: the concept of hunting as distinct from fishing is an abstraction. They would use syllogistic logic all of the time: Basset hounds are good for hunting; that is a Basset hound; that dog would be good at hunting. They would of course use the hypothetical: if I had two dogs rather than only one, I could catch more rabbits. But the reference is always to the concrete relationships that dominate the everyday world. Today we have no difficulty freeing logic from concrete referents and reasoning about purely hypothetical situations.[264]

It is not just that the classification norms were different, but the entailment relations within the language reflected practices incompatible with those proposed by philosophers. Consider the following exchange recorded by Soviet anthropologist Alexander Luria with Siberian peasants in the 1930s:

> Q: All bears are white where there is always snow; in Novaya Zemlya there is always snow; what color are the bears there?

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[263] Saul Kripke, *Naming and Necessity* (1980), p. 5.

[264] James Flynn, *What is Intelligence?* 25.

A: I have seen only black bears and I do not talk of what I have not seen.

Q: But what do my words imply?

A: If a person has not been there he cannot say anything on the basis of words. If a man was 60 or 80 and had seen a white bear there and told me about it, he could be believed.[265]

Note the emphasis on testimonial evidence over abstract reasoning. Only direct experience can tell us about concrete facts, not logic; we could trust someone else's direct experience, perhaps. The logic of the words is irrelevant, and it would require the assumption of different conventions to draw out the conclusion desired. Which is not to say that the truth of *modus ponens* is conventional. What I am saying is that *language* does not need to acknowledge *modus ponens* for it to be a language. Thus the claim that predicates of ordinary language have crisp valuations that carve reality at the joints and obey classical logic, and that names follow modal restrictions, either makes pre-scientific peoples ignorant of much of their own language, or—as I am urging—is simply a proposed norm for the use of language in certain contexts.[266] The norms appears seductive because they facilitate certain kinds of reasoning. What they are not is descriptive of the possibilities within natural language.

I do not mean to advance deep theses about the nature of semantics. In responding to the expertise defense in the context of the philosophy of language my goal was to provide evidence that the intuitions of philosophers could well be theoretically biased. Training in formal logic, for instance, might lead philosophers to assume that referential practices 'should' be modally coherent, and develop theories that reflect this constraint. For example, in the Nixon case I presented above, there corresponds no possible world where Nixon, rigidly designated, has an identical twin: a world where there are twin Nixons is a

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[265] Ibid, 25. There are more like these. These people are not unintelligent. They just haven't adopted the norms we take for granted.

[266] Let us recall how extreme the philosophical claims sometimes are. As I discussed in 3.2.2, Williamson, argues that the question "Was Mars always either dry or not dry?" has a determinate answer in virtue of logical form, *because* natural language is such that it obeys these restrictions (*The Philosophy of Philosophy*). This simply appears to be a false assumption about natural language. One could always propose a convention such that the sentence *is* determinate.

world where our Nixon does not exist; we are thinking 'descriptively,' as it were: a world much like our own, with twin-Nixon 'counterparts' picked out via description. This can be figured out *a priori*, so it's not a question of a metaphysically impossible yet epistemically possible world; rather, I can simply think of Nixon descriptively should the imagined situation require it, just as I can refer to Kuzco as a llama. But if this is so, then it raises the possibility of open-ended answers to cases such as the Gödel-Schmidt: it is not that some names 'turn out to be descriptive' (4.4) but that reference is contextualized to occasions of use, a possibility that had been excluded from intuitive consideration due to background assumptions in philosophy of language. More specifically it seems that for philosophers *de re* modal properties of referents trump other considerations: since any given individual might not have some associated descriptive property, these properties cannot secure reference. The question is whether this assumption is true of ordinary referential practices, or represents a normative constraint for rational reconstruction.

4.6   Conclusion

Let's recap. Experimental philosophers claim to have predicted, and then uncovered, statistically significant variation between (and within) populations' referential intuitions. This, they go on to suggest, jeopardizes philosophers' reliance on their own intuitions when theorizing, for they may be theoretically biased or not representative. Philosophers critical of this argument respond that the 'expert intuitions' of philosophers are more likely to be correct *because* of their training, not despite it. This is the 'expertise defense.'

In response to the 'expertise defense,' I argued that philosophical expertise is expertise at the application of certain normative constraints (naturalness, modality) to language that reflect putative theoretical utility for certain scientific and metaphysical purposes. Philosophers of language, however, do not take themselves to be proposing norms of usage; they take themselves to be describing the conventions of ordinary language. If this were so, the conclusion would be that most speakers utter false sentences far more often

than they realize (about the extension of predicates and the modal properties of individuals);[267] that they are ignorant of the meaning of many of the terms in their language; and are wrong about the correct entailment relations that hold between sentences. This, I think, is absurd. I conclude, therefore, that the 'expertise defense' fails to provide compelling reason to prefer the intuitions of philosophers when theorizing about natural language, for philosophers will tend to have intuitions about meaning that are sensitive to modal and ontological considerations speakers do not have. Rather, we should undertake careful examination of language-use across demographic groups *without* the prior assumption that the underlying meaning-generation practices follow these constraints.

This argument does *not* show that the norms proposed by philosophers of language are 'wrong.' Indeed they may be quite useful for the purposes they are intended. I do not wish even to deny that these norms are truth-conducive. Possibly they are; in this context we should certainly prefer the intuitions of philosophers. In fact, the *definition* of philosophical semantics typically incorporates a commitment to truth-conditional representation.[268] If this is so, the enterprise of semantics is *by definition* the elaboration of linguistic conventions that serve this purpose. The extra step—that the conventions of ordinary languages follow these strictures—is empirically dubious.

In arguing this way, perhaps I fail to distinguish Lewis' 'two topics' in philosophy of language:

> First, the description of possible languages or grammars as abstract semantic systems whereby symbols are associated with aspects of the world; and second, the description of the psychological and sociological facts whereby a particular

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[267] Most speakers utter false sentences about berries, chalk, rubber, vegetables, etc.; they mistakenly think Nixon could have had an identical twin; that Kuzco could have been turned into a llama; perhaps, pending a satisfactory metaphysics, that London could be destroyed and rebuilt 100 km north; and so on.

[268] Soames: "The central fact about language is its representational character. Exceptional cases aside, a meaningful declarative sentence S represents the world as being a certain way." *Philosophy of Language* (2010), p. 1. The constraints above--'naturalness' and 'logical form'–follow from a commitment to representation and the correspondence account of truth.

one of these abstract semantic systems is the one used by a person or population. Only confusion comes of mixing these two topics.[269]

Have I invited this confusion? I do not think so. The distinction I am drawing between ordinary usage and normatively-governed semantic theory is not about Lewis' two topics. Rather, it has to do with the nature of representation: what 'aspects of the world' are associated with what 'symbols.' Philosophical 'expertise,' I am arguing, tends to bias the question of representation in directions that reflect metaphysical desiderata. The aspects of the world represented by symbols are assumed by philosophers to be theoretically significant, viz., natural predicates and modally-coherent names, since these are the important aspects to focus on for metaphysical theorizing (with proper names, we should want to be able to express all and only true claims about specific individuals). What is less clear is that speakers hew to these constraints, and that the resulting theories really are about ordinary language.

All I wished to demonstrate was that these 'philosophical theories' *are* proposed norms of usage, not a *theory* of natural language meanings. To get such a theory requires empirical investigation into the different possible uses of language, which appear to outstrip purely formal constraints. In the particular case of the theory of reference, it may very well be that there is no 'theory' of reference—in the weakly internalist sense that only speakers refer. I take no position on this issue for now, though I suspect that empirical investigation will lend credence to the internalist hypothesis. For the time being it is enough that we are clearer now about what philosophical expertise is expertise *of.*

The problem I outlined above appears peculiar to ordinary language (and hence to philosophy of language). Epistemology, for instance, can wear normativity on its sleeve: it is easier to argue that we are after the knowledge-practices we should have, not an analysis of the epistemic concepts we happen to have. Philosophy of language has tried to have it both ways: to provide an analysis of the language we ought to have and thereby

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[269] Lewis, "General Semantics" 19.

explain the one we do in fact have. These are incompatible projects, and the expertise defense fails because of it.

The biasing power of theoretical commitment is less problematic outside of philosophy of language, and my brief against the expertise defense cannot be extended to other domains of philosophy without significant modification and weakening. I will then close with two brief thoughts on how this might proceed. First, it is clear that in cases where philosophical theories rely, implicitly or explicitly, on claims about ordinary concept-possession we should certainly be on guard for theory-driven bias of the type I have been discussing. Second, however, when the target of philosophical theorizing consists in claims about the norms which should be adopted given some description of an extrinsic phenomena (Knowledge, itself, taking no stand on the well-formedness of this pursuit) theoretical bias is not as much of a liability. We may be left with other worries that could affect the evaluation of cases (framing and ordering effects, wording, and so on); I will not attempt to untangle the issues here. Rather, I wish to stress that the problem of theoretical bias—that is, the problem of philosophical expertise itself as a form of bias—does not neatly apply outside of philosophy of language, reflecting the domain-specific nature of expertise and intuitive judgment. The interaction effect discovered by the experimentalists, therefore, cannot be dismissed on the basis of 'expertise.'

Chapter 5

5   Conclusion: Defending Experimental Philosophy

5.1   Summary

Experimental philosophy, I have argued, has dual ambitions: a positive program continuous with cognitive science, and a methodological program concerned with the use of intuitions as a form evidence in philosophy. In reality the distinction is not so cut and dry: the methodological program requires input from the positive program, and, in turn, generates positive data of its own. Since the positive program is concerned with the nature of intuitive judgment, typically judgments of conceptual application or classification in general, however, its significance can be divorced from the methodological argument over the reliability of intuition.

Critics have sometimes claimed that experimental philosophy proceeds as if all philosophy was ordinary language philosophy: that, by surveying response to cases, experimental philosophers propose to solve problems of philosophy by appealing to ordinary practices.[270] I've argued that this misunderstands the positive project. The goal is to achieve explanatory depth about the nature of pre-theoretical judgment, not to tabulate survey results and read theory off the numbers. As Joshua Knobe stresses, experimental philosophy is not conceptual analysis by other means: a typical paper in experimental philosophy will not attempt to analyse a concept: "most typically, what you will find is an attempt to identify and explore a specific *effect*," Knobe writes, in an effort to understand the cognitive processes that give rise to certain kinds of judgments performed under certain kinds of conditions.[271] Reference provided us with a case study: the hypothesis was that culturally-specific differences in cognitive style would have an effect on intuitions of reference in the Gödel-Schmidt, and this is what experimentalists

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[270] Max Deutsch, "Experimental Philosophy and the Theory of Reference" 460.

[271] Joshua Knobe, "Experimental Philosophy is Cognitive Science" 6.

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found. How to interpret these results is another matter entirely; the authors of the study explore some possibilities, but do not directly endorse any.[272]

The main thrust of my argument was to defend the *prima facie* case for experimental philosophy. The *prima facie* case makes the straightforward claim that sources of evidence require some accounting, and that in the case of intuitive evidence, one way to so account is via the methods of scientific investigation. I have also urged that the philosophical significance of experimental philosophy can be decoupled from the use of intuitions as evidence, to the extent that understanding intuition and pre-theoretic judgment is of intrinsic philosophical interest. While I believe this is so, my primary focus throughout was the *prima facie* case as developed in section 2.2.; and so I have defended it against two main objections, eliminativism about intuitions and the expertise defense.

In defending the *prima facie* case against these objections, I declined to give a precise definition of intuition. Rather, I provided examples of their use, and used several words interchangeably: intuition, but also judgment, evaluation, and response. These capture the central fact about 'intuition': agents have intuitions, make judgments, evaluate scenarios, and respond to cases. Further, nothing can replace the agent. No apparatus can independently detect the presence or absence of knowledge in a Gettier scenario, just as no instrument can track causal chains of reference. It could be objected that this is a feature of all experiment: even in the laboratory, someone needs to operate the equipment, devise the protocol, and interpret the results. This is indeed so; the difference is that in a thought-experiment, the equipment and interpreter are one and the same. A temperature reading is as reliable as our knowledge of the thermometer, and even then it is better not to know what one is measuring, just in case (the double-blind experiment). Likewise, an intuition is only as reliable as our knowledge of the intuiter and of the intuitive faculties, and the situation is complex because one cannot help but know what

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[272] Ron Mallon et al., "Against Arguments From Reference."

one is 'measuring.' There are no double-blind thought-experiments when we are asking ourselves the question.

The case I considered (2.1) all shared this distinct agent-centric feature. Twin-Earth required a judgment about the meaning of 'water'; Gettier about knowledge (or, possibly, Knowledge); and, in the more complex case of reverse Sobel sequences, several delicate pragmatic judgments reflecting ordering effects in counterfactual sentences. Later on we saw this feature again with Trolley cases and Zombies (3.2.4). Understanding intuition as a cognitive phenomenon allows for empirical purchase: again, the goal is not to tabulate response, but to explain judgment.

The *prima facie* case then proposes to investigate intuition broadly understood as an agent-centric set of cognitive phenomena in the hopes of achieving explanatory depth about intuitive judgments. Nothing rides on whether or not 'intuitions' are a 'kind,' a special and distinct form of insight. It could very well be that the kinds of judgment that are deployed in moral philosophy differ significantly from 'intuition' in formal logic. It could also be that some 'intuitions' are more reliable than others; or that certain intuitions are more amenable to empirical investigation than other types. These are issues to be sorted out *by* investigation, not fiat, and motivate rather than challenge the *prima facie* case. The broad theoretical vocabulary of intuition acknowledges this mentalistic component in ways that can guide our agent-centric heuristic as we proceed in the investigation.

We can be fairly certain such an investigation is possible. Non-eliminativists—that is, those philosophers who have explicitly theorized about intuitions and their role in philosophy—have offered competing positive accounts of 'the intuitive.' Clusters of features have accrued to intuition, resulting in a sort of 'family resemblance' (meta-) concept that can be discerned from the literature.

Below, I have sorted these 'clusters' of features in four distinct groups: epistemic features, material features, psychological features, and methodological role. Positive accounts of intuitions pick and choose some cluster of the features below, though there is widespread disagreement on which cluster is 'the' correct account (if there is one):

Epistemic Features.

- Apriority: Intuitions are *a priori* or are a source of *a priori* knowledge.

- Non-inferential: Intuitions are not 'inferred' from other premises in an argument.

- Basic: Intuitions are 'brute,' 'bedrock,' and/or 'necessary.'

- Commonsensical / pre-theoretical: An intuition is not tainted by other theoretical commitments; it is "what we'd say," (the 'we' is sometimes imagined to be 'the folk.')

Material features.

- Unity: "...the intuitions of interest in philosophy constitute a single epistemic and psychological kind."[273]

- Modality: Intuitions are modal, counterfactual or have a "modal tie to truth."[274]

- Normative: An intuition has normative force to *change* our concepts.

- Determinacy: Intuitions are about *determinate* concepts and/or things.

Psychological features.

- Judgment: An intuition is a type of judgment (of conceptual application, etc.) about a case.

- Phenomenological: An intuition has a distinctive phenomenology in which it appears self-evident.

- Conceptual Competence: Intuitions derive from a kind of conceptual competence.

Methodological role.

- Central: Intuitions are the typical way philosophical theorizing proceeds.

- Distinctive: Intuitions are distinctive of philosophical theorizing.

- Evidential: Intuitions are evidence for and against theories.

- Casuistry: An intuition consists in testing the applicability of a concept to a case.

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[273] Joel Pust, "Intuitions."

[274] George Bealer, "A Theory of the A Priori," 22.

CSI Jenkins has independently argued in a similar vein.[275] She distinguishes between four broad forms of 'clustering' these features of intuitions: one cluster identifies 'intuitions' with 'common-sensicality' (folk response, pre-theoretic judgment); another that identifies intuitions with features associated with 'apriority' and necessity; a third with 'immediacy' either in the form of non-inferentiality or a sense of self-evidence or obviousness; a fourth cluster views intuitions as foundational, distinct, and a source of warrant.[276] Jenkins warns that because of these competing clusters of features, philosophers run an appreciable risk of talking at cross-purposes: for instance, "a prioricity and commonsensicality differ significantly in extension," though philosophers have sometimes been cavalier about the difference.[277] The solution proposed is that 'intuition-talk' is contextually sensitive between different intended meanings, demonstrated by the acceptability of a sentence such as "this is something I rationally intuit, but I admit that it is not pretheoretically intuitive."[278]

I agree with Jenkins' diagnosis of the situation. Fortunately for experimental philosophy, it does not matter which cluster of features is the 'correct' one—if there is one—or whether 'intuition' is used contextually to pick out different clusters. This is because across all permutations the mental component remains, which is all we require for empirical investigation to proceed. This is so whether intuition is understood as the pre-theoretic response or *a priori* knowledge (viz. my discussion of Deutsch in 3.3) or foundational insight; indeed, careful experiment can help us determine what sort of intuition is really in play.

Jenkins is of the view that empirical investigation into intuition is not necessary, as we can be confident of their reliability. Two problems with this view. First, achieving

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[275] C.S.I. Jenkins, "'Intuition,' Intuitions, and the A Priori." I discovered her paper late in my research, and similarities between our accounts are coincidental.

[276] Ibid, 3-5.

[277] Ibid, 6.

[278] Ibid, 7.

explanatory depth about the mental phenomena surrounding 'intuition' is important outside of reliability. Second, the argument for reliability is suspect, for reasons I've already outlined. Jenkins writes that

> To argue that intuitions are unreliable … on the grounds that intuition *sometimes* goes wrong is no better than to argue that vision is unreliable … on the grounds that we are *sometimes* subject to optical illusions.[279]

As I indicated already (3.2.2.) in the context of fundamental theorizing, it is natural that higher epistemic standards should apply. A major reason for double-blind studies is precisely to prevent investigators from 'seeing' what they wish to see. We do not besmirch the basic reliability of vision when we ask that an investigator peering into a microscope not know which sample they are looking at. The analogy with optical illusions is potentially misleading as well: philosophical thought-experiments, because they go beyond the ordinary case, might be systematically deceptive. If we are sometimes subject to optical illusions, it would be nice to have a theory of vision that would help us avoid making mistakes: to know that they are illusions. Absent a good theory of intuitions, as the *prima facie* case asks us to do, we do not know the limits of reliability. Surely if philosophy is in the business of providing fundamental metaphysical insight it would only be of benefit to have a theory about the limits of intuition, even if it turns out appeal to intuition was innocuous all along.

If there are no independent checks on intuition—recall that nothing can substitute for the agent—how do we achieve empirical purchase on the mental phenomena of 'intuitive judgment' to both understand it and gauge reliability? In 2.3, I discussed the positive program's approach, arguing that understanding patterns of intuitive response can be helpful in solving, or dissolving, philosophical problems. An intuitive problem is often the starting point of philosophical discussion: in the example I presented, the conflict between moral luck and the principle that one is morally responsible only for things under one's control. Unlucky outcomes seem to spur harsher moral judgment. But, it turns out, the intuitions might have been misdescribed: it is not that we intuitively

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[279] Ibid, 20.

condemn unlucky *outcomes* more forcefully (in conflict with the control principle). Rather the primary driver of moral judgment appears to be mental-state assessments, more particularly evaluation of an agent's justification for their belief that a bad outcome would not occur. This is a surprising result that took careful experimental investigation to discover. A better understanding of patterns of intuitive response then puts the problem of moral luck in a new light, allowing new theories to emerge. In this sort of case we are less interested in determining whether or not intuitions are reliable and concerned, instead, with understanding what the intuitions actually are.

Reliability, the ostensive concern of the methodological program, must be approached indirectly. This is because, with few exceptions, for some intuition p the truth-maker of p isn't that a majority of respondents assent to p under experimental conditions (one such partial exception, I've suggested, occurs in the philosophy of language). What makes p true is presumably the fact that p is the case. Without independent access to p, however, the best we can do is verify that p is robust, invariant, and generated by a trustworthy cognitive process. For example, if some intuition that p is robust under different conditions, this is evidence that p is genuinely intuitive and not the result of a subtle unconscious association, biasing or interaction effect, and so on. In turn p's being robustly intuitive is evidence that the intuition is reliable.

As I've emphasized, the idea is not to tabulate response (in the sense of an opinion poll) but to understand patterns of intuitive response. From these patterns one might conclude that an intuition is fairly reliable, not so reliable, reliable only under certain conditions, or simply misdescribed. Systematic investigation of patterns of intuitive response can then speak to the reliability question by separating the genuinely intuitive from the parochial or conditioned, and by providing an explanatory theory for the patterns that are found.

A more direct argument against the *prima facie* case is that philosophers do not, and have never, relied on intuition. This is the view I dubbed 'eliminativism,' with two variants: Cappelen's blunt version, which denied the existence of intuition altogether, and Deutsch more subtle form that did not deny intuition, only its use as a form of evidence. In arguing against eliminativism, I attempted to grant as much of my opponent's case as I

could, and still show that the *prima facie* case went through. Cappelen's reliance on Williamson's metaphilosophy was not without problem; ultimately, however, it was Cappelen's own case studies that demonstrated the role intuitions play in argument: the weakest plausible construal, as contributing to the context of discovery, still merited investigation due to intuition's initial role in shaping subsequent investigation. Cappelen's definition of intuition defined them out of the process entirely, an overly restrictive account that requires implausible re-reading of the literature.

Deutsch, for his part, argued that philosophers do not rely on intuitions as evidence, understood as appeals to something's being intuitive; rather, philosophers proceed on the basis of *a priori* knowledge of which intuitions are one potential causal source. Against his view I argued that sources of knowledge are valid targets of investigation, and the *prima facie* case goes through much the same. Deutsch was also wrong that psychological phenomena have no place in philosophical theories, at least in the case of semantics. Furthermore, Deutsch's characterization of *apriority* meets much of the criteria of intuition (above), and amounts to a sort of re-labeling.

Finally, both Cappelen and Deutsch make the mistake of supposing that since one can also support philosophical conclusions with argument that this means intuitions in fact played no role, and are dispensable. As I endeavored to show, many of these arguments rely on some implicit appeal to intuition-like phenomena; and, even if they did not, this does not mean that intuitions did not serve as an additional source of evidence for the claim.

The eliminativist challenge thus answered, I considered the 'expertise defense' in the context of the philosophy of language. Since expertise is widely acknowledged to be domain-specific I considered the claim that philosophical expertise secures the reliability of appeals to intuition in semantics (taking the Gödel-Schmidt case as a starting point, but extending to referential intuitions generally). I argued that, far from securing greater reliability, philosophical expertise is more likely to be tainted by theoretical commitment. I offered two examples: naturalness and modality. In the case of the former, I claimed that philosophers of language, because of their training and dominant assumptions in the

field about the representational nature of language, are more likely to assume that predicates reflect 'natural' properties in the world. In the case of the latter, I argued that the referential intuitions of philosophers are more likely to reflect modal constraints, specifically a semantics that enables true modal claims about *de re* entities, while ordinary language appears to have no such constraint: ordinary referential practices do not obviously discriminate between rigid designation and descriptivist reference in many sorts of cases (mostly having to do with impossible worlds). Because of this, we cannot be certain that the 'expert' intuitions of philosophers are reliable guides to the conventions of ordinary language that is the purported explanatory target. Rather, these are intuitions about the language we 'ought' to have, much as epistemology is about the epistemic concepts we 'ought' to deploy. I take no position on this normative aspect, but only note the explanatory inadequacy such a normatively-governed theory inevitably has in relation to natural languages as they are spoken. It will take careful empirical investigation to produce a theory of the latter. It seems experimental philosophy has some work yet to do.

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