INTUITION & EXPERIMENTAL RESTRICTIONISM

THE MOTHER-IN-LAW OF PHILOSOPHY:
INTUITION & EXPERIMENTAL RESTRICTIONISM

By

IAN WRIGHT, B.A.

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AUTHOR: (McMaster University)	Ian Matthew Wright, B.A.
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Abstract

This project is a response to recent criticisms against the use of intuitions in analytic philosophy, raised by adherents of experimental restrictionism. I begin by discussing and eventually rejecting the restrictionist characterization of analytic philosophy, which I take to be inaccurate and unfair. I examine several accounts of the nature of intuitions in the history of philosophy, in contemporary discourse and as they are understood by restrictionism. I reject these accounts in favour of a more modest characterization of intuitions. I conclude with an overview of the restrictionist methodology. After considering some of its past criticisms, I offer my own critique based on the restrictionists' false presupposition that intuition is a natural kind.

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Dedication

For my Father. I hope he would have approved.

Table of Contents

INTRODUCTION

1.1 What is Experimental Philosophy?

Google 'experimental philosophy' and among the top hits returned will be the experimental philosophy blog[1], the experimental philosophy website[2] (maintained by Joshua Knobe) and a Youtube music video for the "Experimental Philosophy Anthem" featuring a flaming armchair.[3] Experimental philosophy is a research programme of the Internet Generation. But there is much more to "x-phi" than blogs, web pages and Youtube videos; it may be trendy[4], but behind the trendiness is a dedicated group of ambitious philosophers with a growing list of impressive credentials.

The dialogues on the experimental philosophy blog often involve professors at leading universities. Knobe's website is currently hosted by Yale University, where Knobe recently accepted a tenure track position. The work of experimental philosophers, whether lauded or decried, has received attention from the top philosophical minds of our generation.[5] For all its irritating trendiness, experimental philosophy cannot be easily dismissed.

The very same thing that makes experimental philosophy so trendy, despite (or perhaps because of) its relative nascency, is what also makes it

[1] http://experimentalphilosophy.typepad.com/
[2] http://www.unc.edu/~knobe/ExperimentalPhilosophy.html
[3] http://www.youtube.com/watch?v=tt5Kxv8eCTA
[4] To get your very own x-phi t-shirt, visit http://www.cafepress.com/xphi.140908989
[5] I leave it to you to decide who falls into this category, but if you're reading this at all, the odds are good that most of the philosophers you would pick have said *something* for or against x-phi.

distinctive: experimental philosophy is thoroughly and unabashedly empirical. It is not empirical in the sense that experimental philosophers look to findings in the sciences to support their claims (though they do that too). Lots of (armchair) analytic philosophers use scientific findings to buttress their arguments. What sets experimental philosophers apart is their commitment to getting their hands dirty. Experimental philosophers conduct their own empirical research, using methods borrowed and adapted from social psychology.

While it's tempting to classify experimental philosophers as psychologists or sociologists on purely methodological grounds, x-phi's are quick to rebut this claim by pointing out that although employing the methods of psychology and sociology is what makes them "experimental", what makes them "philosophers" is their interest in philosophical questions. The topics that most experimental philosophers have been working on for the last ten years are the same ones that most analytic philosophers have been working on for the last hundred. Experimental philosophy is thus a hybrid of analytic philosophy and empirical psychology and based on its fecundity thus far, it seems to be a hybrid that has found a vacant academic niche.

So, x-phi is trendy, it's relatively new and it keeps analytic philosophy's questions but rejects its methods in favour of a more scientific approach. Now comes the important question: It's all well and good to talk about "getting out of the armchair", but what does it mean? What exactly is one getting out of? The short answer is that x-phi rejects the use of intuitions (for which no justification is

provided) as evidence for or against philosophical claims. This is what x-phi takes to be the standard method of armchair analytic philosophy. Skim through practically any introduction to experimental philosophy and you'll find the phrases they take to be characteristic of the analytic method, phrases like "Obviously we would say..." and "Intuitively...". Such phrases are expletives for experimental philosophers.

Thus far, I've painted experimental philosophy as a unified movement, but that picture is misleading. What makes experimental philosophy distinct from analytic philosophy is largely a matter of method, but what makes experimental philosophers distinct from one another is a matter of ideology. In the next section, I outline what I take to be the two major projects within experimental philosophy.

1.2 How Many Kinds of X-Phi Are There?

Experimental philosophy is now so diverse that any critique not focused exclusively on its methods is bound to be too coarse grained. Thus, the aim of this section is to pinpoint a single project as the primary target of my critique.[6] To that end, I consider three suggested taxonomies of experimental philosophy, contrasting them based on the stipulated divisive issue amongst x-phi's. Though different criteria are used to mark the boundaries between projects, there is still a general consensus on who should be separated and who should be grouped together, which suggests that these divisions are not arbitrary.

[6] That being said, some issues raised in later chapters will pertain to the experimental philosophy research programme as whole

In one of its first major critiques, Antti Kauppinen divides experimental philosophy into two groups on the basis of its practitioners' attitude toward a particular thesis about folk concepts. This "positive" thesis avers that, "Survey studies *are* a reliable source of evidence for philosophically relevant claims about folk concepts." (Kauppinen, 97, emphasis is author's) Kauppinen labels those who embrace this thesis "optimistic experimentalists" and those who reject it, "pessimistic experimentalists".[7] Joshua Knobe and Eddy Nahmias are cited as exemplars of the former group, Stephen Stich and Jonathan Weinberg as exemplars of the latter. Kauppinen also highlights a negative thesis of experimentalism as one which both groups agree upon. It states, "Armchair reflection and informal dialogue are *not* reliable sources of evidence for (philosophically relevant) claims about folk concepts." (ibid, emphasis is author's) As Kauppinen sees it, experimental philosophers are united on the philosophical value (or lack thereof) of the armchair methodology but divided on the philosophical value (or lack thereof) of the experimental methodology.

In responding to Kauppinen's critique, Thomas Nadelhoffer and Eddy Nahmias suggest an alternative delineation of experimental philosophy which is motivated, at least in part, by their aim of defending x-phi from critics like Kauppinen. Rather than defining the divisions within experimental philosophy in terms of adherence to a particular thesis, Nadelhoffer and Nahmias divide x-phi

[7] More specifically, Kauppinen notes that the emphasis of the pessimistic experimentalists' rejection of positive experimentalism is on its point about philosophical relevance. No doubt this is to avoid implying that pessimistic experimentalists hold the glaringly contradictory position that survey studies are not a reliable source of evidence for claims about folk intuitions, despite having made claims about folk intuitions on the basis of their own surveys.

into three projects based on the respective goals of their adherents.

Those in the first group are interested in "*what* the folk think in order to ascertain which philosophical theories best accord with and account for commonsense intuitions." (N&N, 126) This group, which Nadelhoffer and Nahmias label Experimental Analysis (EA), can be roughly identified with Kauppinen's optimistic experimentalism, though their account is more detailed. Those in EA assert that the theories which best accord with and account for the majority intuitions (ascertained via surveys) should enjoy squatter's rights unless they can be shown to be defective for other reasons. Among those cited as doing EA are Knobe, Nadelhoffer and Nahmias themselves, and Shaun Nichols, though some of these names appear in other categories as well.

The second project outlined by Nadelhoffer and Nahmias is concerned not with what folk intuitions are, but with the cognitive mechanisms that underlie these intuitions. Unlike EA, this second project, labelled Experimental Descriptivism (ED) has no counter-part in Kauppinen's taxonomy (nor, as we shall see, in Alexander and Weinberg's). Of the three projects outlined, ED seems to lean the most toward psychology and the least toward philosophy. This is not to say that practitioners of ED are not philosophers,[8] just that their interest in how the mind works, though philosophically motivated, is much more empirically oriented. According to Nadelhoffer and Nahmias, the aim of ED is to square philosophical theories with how the mind works; theories which do not comport

[8] It's worth noting however that Joshua Greene, the biggest exemplar of this category, is appointed in a psychology department at Harvard, despite having done his graduate work in philosophy.

with their findings should be rejected. Another interesting difference between ED and the other projects outlined by Nadelhoffer and Nahmias is that in some cases it involves not surveys, but fMRI imaging as well.[9]

The third and final project discussed by Nadelhoffer and Nahmias is essentially identical to Kauppinen's pessimistic experimentalism. This project is critical of the practises of analytic philosophy: its practitioners argue that the use of intuitions should be expunged from or at least severely restricted in philosophical discourse, hence the label 'Experimental Restrictionism (ER).[10] Nadelhoffer and Nahmias described ER as "an empirically informed battle against the use (and abuse) of intuitions in philosophy." (ibid, 128) More precisely, the claim made by practitioners of ER is that if intuitions vary across axes which philosophers have previously taken to be irrelevant (e.g., culture, socio-economic status, etc.) and if they have no principled reason for privileging their own intuitions, then there is no justification for using intuitions in philosophical practise. Practitioners of ER claim that their results have already established prima facie evidence for the first half of the antecedent and have asserted that there has yet to be a response which denies the second half. This is nicely summed up by Nadelhoffer and Nahmias when they write, "The worry expressed by ERs is that in light of the gathering evidence concerning the wide-scale diversity of intuitions, intuition-driven philosophy ends up being both epistemologically xenophobic and intuitionally question-begging." (ibid)

[9] This kind of methodology is exclusively (at least thus far) used by Joshua Greene.
[10] This label is borrowed from Alexander and Weinberg's taxonomy.

Alexander and Weinberg distinguish two views regarding the relationship between experimental philosophy and the future of standard philosophical practise. The first, *the proper foundation view*, asserts that the results of experimental philosophy should be used to provide an empirical foundation for various philosophical claims and projects. The second, *the restrictionist view*, asserts that the findings of experimental philosophers should figure into a radical restriction of the use of intuitions as evidence in philosophical practise. According to the former view, since claims about the intuitions of philosophers and/or the folk are "straightforwardly empirical" (61), philosophers should be concerned with gathering the data that indicates what these intuitions are. Thus, on the proper foundation view, the results of investigations conducted by experimental philosophers would be an additional tool at the disposal of analytic philosophers. According to the latter view, the supposed instability of intuitions across such diverse variables as culture, education and socio-economic status challenges the use of intuitions in any sort of evidentiary role in philosophical investigation. It is interesting to note that, as a result, the restrictionist view not only challenges the standard philosophical practise, but the proper foundation view as well.

The amount of overlap between these three taxonomies and the fact that they have been made by both proponents and critics of experimental philosophy seems to me ample justification for singling out a single project within experimental philosophy for criticism. The two questions that need answering now are: (1) Whose taxonomy do we use? (2) Which project do we target for

criticism? My answers to these questions are actually entangled. Of the three taxonomies discussed, I'm most inclined to use Alexander and Weinberg's for two reasons. First, both Alexander and Weinberg are self-identified x-phi's and by adopting a taxonomy of experimental philosophers by experimental philosophers, I minimize the risk of being overly coarse grained in my critique. Second—and this is the point that entangles my answer to the first question with my answer to the second—is that both Alexander and Weinberg are restrictionists and restrictionism is my target.

Regarding my now evident answer to the second question, I have again two reasons for answering as I do. The first is that while there is disagreement between the three taxonomies concerning the more positive project(s) in experimental philosophy, everyone seems to agree on what the negative project is and who is associated with it. More importantly, restrictionism is the project which is most critical of analytic philosophy; the others are (for lack of a better term) much more benign. There can be, if not cooperation, at least congeniality between positive projects in x-phi and analytic philosophy in general. Indeed, the additional-tool-in-the-toolbox metaphor used by Alexander and Weinberg with regard to the proper foundations view is meant to suggest we can all get along quite amiably. The restrictionist view, however, demands a response. Before analytic philosophers can go on with business as usual, they need to rebut the claims central to experimental restrictionism.

1.3 A Case Study in Experimental Restrictionism

Since experimental philosophy is still a relatively recent development, it behoves us to begin with an examination of an exchange between, on the one side, Stephen Stich and Jonathan Weinberg (representing experimental restrictionism) andm oin the other, Frank Jackson (representing analytic philosophy) regarding Jackson's book *From Metaphysics to Ethics*. The goal of this summary is to set the stage for a more detailed discussion of experimental restrictionism in subsequent chapters.

This exchange is useful as a case study in x-phi criticisms of analytic philosophy for several reasons. First, it comprises one of the earliest debates between experimental and analytic philosophers, predating every other piece of experimental philosophy literature discussed throughout this project. Second, Stich and Weinberg's review refers to only one experiment, as opposed to the wealth of experimental evidence used in later restrictionist critiques. This narrow focus helps give one a feel for experimental restrictionism without getting bogged down in a surfeit of data. Finally, the exchange between Jackson and Stich and Weinberg works well as an introduction because it is more or less typical of the debates that have thus far transpired between restrictionists and analytics. The summary proceeds with an outline of Jackson's conceptual analysis, followed by Stich and Weinberg's critique and concludes with Jackson's response.

Jackson's Conceptual Analysis

The aim of Jackson's conceptual analysis is to provide an elucidation of folk concepts which, for Jackson, *must* be the subject matter of philosophical inquiry. Concerning questions about determinism and intentionality he writes,

> What we are seeking to address is whether free action *according to our ordinary conception*, or something suitably close to our ordinary conception, exists and is compatible with determinism, and whether intentional states *according to our ordinary conception*, or something suitably close to it, will survive what cognitive science reveals about the operations of our brains. (Jackson, 31, emphasis is author's)

Though we can of course define the subject matter of our inquiry in any way we like, Jackson claims that doing so "turn[s] interesting philosophical debates into easy exercises in deductions from stipulative definitions together with accepted facts." (ibid) Hence, for Jackson, the interesting philosophical questions are those predicated on most people's ordinary conceptions.[11]

The target of conceptual analysis is clear: the ordinary conceptions held by most people. How do we figure out what those ordinary conceptions are? Jackson's answer is that we consult our intuitions about actual and possible cases pertinent to the concept under investigation. Each person's intuitions reveal her theory of a concept and to the extent that these intuitions coincide, they reveal the

[11] That being said, Jackson grants that inquiry is not *only* of interest when it deals with ordinary conceptions; the conceptions of numbers and functions investigated by mathematicians are rarely identical to ordinary "folk" conceptions, but this does not mean that mathematical inquiry is uninteresting. This caveat suggests that we could import scientific conceptions for philosophical inquiry, rather than using ordinary ones. Though this method might prove to be far less objectionable to restrictionist philosophers, it would also be problematic in areas where the conceptions of science and philosophy are inter-dependent (e.g., in meta-theoretical work in the philosophy of science) or where there are fundamental divisions between philosophical and scientific conceptions. See Osbeck (1999) for an example of the latter problem.

"folk theory" of that concept.[12] For Jackson, intuitions reveal the proper subject matter of philosophical inquiry. Without intuitions, philosophical inquiry is reduced to mere stipulative exercise.

Stich and Weinberg's Review

Stich and Weinberg begin by acknowledging Jackson's book as "the most sophisticated defense of the use of conceptual analysis in philosophy that has ever been offered." (Stich and Weinberg, 637). That being said, they qualify their statement by adding that they are not entirely sure what Jackson thinks conceptual analysis *is*.[13] Moreover, they contend that without knowing precisely what Jackson's version of conceptual analysis is, they are rather uncertain as to whether it is even possible.

The thrust of Stich and Weinberg's paper is that Jackson's conceptual analysis (whatever it is) rests on the false assumption that his own intuitions are representative of those of the folk. If they are correct and Jackson's assumption that his own case is typical is indeed mistaken, then his project of conceptual analysis is in serious trouble.

In attacking Jackson's justification for generalizing his own intuitions, Stich and Weinberg quote a later passage from Jackson's book at length:

[12] In a footnote, Jackson writes, "I here take the controversial view that folk conceptions should be thought of as amalgams of individual conceptions. Thus, my intuitions reveal the folk conception in as much as I am reasonably entitled, as I usually am, to regard myself as typical." (ibid, 32, n. 4) Hence, Jackson's analysis depends on his being reasonably sure that his intuitions are typical of the folk.

[13] It is hard to see how this qualification doesn't nullify their compliment.

> I have spoken as if there will be, at the end of the day, some sort of convergence in moral opinion in the sense that mature folk morality will be a single network of input, output, and internal role clauses accepted by the community as a whole. In this case we can talk simply of mature folk morality without further qualification. Indeed, I take it that it is part of current folk morality that convergence will or would occur. We have some kind of commitment to the idea that moral disagreements can be resolved by sufficient critical reflection—which is why we bother to engage in moral debate. To that extent, some sort of objectivism is part of current folk morality. (Jackson, 1998, 137)

Stich and Weinberg deny the plausibility of a convergence in moral opinion for two reasons. First, they note that a significant number of their own undergraduate students claim to be moral relativists. Under the assumptions that their students are both truthfully espousing their own concept of morality and representative of undergraduate students generally, Jackson's claim that objectivism is a part of current folk morality would indeed be dubious. The second reason to doubt the plausibility of Jackson's claim that folk morality has to do with those who are intractably entrenched in their own cultural or religious convictions. Since it is often the case that these convictions are so utterly unshakable that no amount of critical reflection can dislodge them, Jackson's claim about that there is an assumption of eventual convergence in the folk concept of morality seems even more implausible.

In addition to their anecdotal evidence against Jackson's claim about how typical his own case is, Stich and Weinberg offer some preliminary empirical evidence gathered from some of the earliest experimental philosophy experiments. The example they cite comes from an "epistemic intuition probe"

modelled on a Gettier case[14]:

> Bob has a friend Jill, who has driven a Buick for many years. Bob therefore thinks that Jill drives an American car. He is not aware, however, that her Buick has recently been stolen, and he is also not aware that Jill has replaced it with a Pontiac, which is a different kind of American car. Does Bob really know that Jill drives an American car, or does he only believe it? (Stich and Weinberg, 642)

Of the European subjects surveyed, 74% said that Bob only believes that Jill owns an American car, thus having intuitions in agreement with what is taken to be the correct response to a Gettier case. However, 57% of East Asian subjects and 61% of subjects from India or Pakistan had intuitions contrary to what the Gettier case is supposed to evoke and answered that Bob really knows that Jill owns an American car. The intuitions of the majority of subjects from East Asian, India or Pakistan would seem to be in direct opposition to those of Jackson and, apparently, the majority of European subjects.

What Stich and Weinberg take from these results is that intuitions are not as widely shared as Jackson has assumed. As we have seen above, the targets of Jackson's conceptual analysis are folk concepts and these are accessed by consulting intuitions about actual and possible cases, which are supposed to be generalizable to the folk. If Jackson's intuitions are *not* generalizable, as Stich and Weinberg have ostensibly shown, then he will be unable to access folk concepts and thus unable to proceed in conceptual analysis as he envisions it.

Jackson's Reply

[14] See Section 1.2 in Chapter One for an explication of Gettier cases.

Jackson begins his brief response to Stich and Weinberg by acknowledging the need for caution when making claims which generalize his own intuitions to those of the folk. However, he is clear that he takes someone who fully understands a Gettier case and yet insists that it *is* a case of knowledge to be using the word 'knowledge' in a different way than those who identify the Gettier case to be one of mere belief. Jackson does not take these individuals to be mistaken in their application of their concept of knowledge, but

> they are, of course, missing out on an interesting way of grouping together cases—the way we effect with the term knowledge—that cuts across the grouping effected in terms of true justified belief , and which has its own distinctive role to play in epistemology. (Jackson, 1998, 32)

This suggests that Jackson considers those who do take Gettier cases to be cases of knowledge have a different concept of knowledge than those who do not.[15] Thus, the fact that there are people who do not share Jackson's intuitions is not a threat to his project. It merely means that he is addressing a smaller, though presumably still substantial, audience.

Concerning Stich and Weinberg's reference to the moral relativist claims of their undergraduate students, Jackson's response is brief, but decisive. He points out that the students' claims may not reflect their actual beliefs; they may in fact hold moral objectivist assumptions without introspectively realizing it. Anyone who has experienced the reaction to a threat to fail any and all students

[15] Concerning his use of the word 'concept', Jackson writes, "Our subject is really the elucidation of the possible situations covered by the *words* we use to ask our questions...I use the word 'concept' partly in deference to the traditional terminology which talks of *conceptual* analysis, and partly to emphasize that though our subject is the elucidation of the various situations covered by bits of language according to one or another language user, or by the folk in general, it is divorced from considerations local to any particular language." (Jackson, 1998, 33)

who are moral relativists will probably agree with Jackson's observation.[16]

With regard to those whose views seem intractable because of some religious or cultural indoctrination, Jackson's response is less satisfying; he essentially suggests that Stich and Weinberg are being overly pessimistic about the effectiveness of critical reflection. A better response would have been to point out that the intractably entrenched may nevertheless assume that moral convergence will eventually occur, but only when those who disagree with them see the light and realize the error of their ways. Though this is a clearly biased prediction, it is nonetheless a prediction that moral convergence will occur.

Jackson's response to the experimental data cited by Stich and Weinberg is typical of early analytic reactions to the restrictionist view: he suggests that their results are inconclusive because the subjects may not have understood the question they were asked. He also notes that it would be interesting to see whether subjects had intuitions for barn cases similar to those they have in Gettier cases.[17][18]

We should keep in mind that the above case study occurs at an abstracted level; the issues Jackson, Stich and Weinberg are discussing are meta-philosophical. Typically, the criticisms levelled against analytic philosophers by

[16] Moreover, assuming that a person's behavior is a more accurate indication of their beliefs than their responses to questionnaires is just good folk psychology.
[17] For a thorough and humorous explication of barn cases, see (Gendler and Hawthorne, 2005)
[18] This suggested alternative is meant to better capture the point that Bob's belief is only accidentally true. Jackson points out that, "It is far from unusual for the owner of an American car to replace it with another American car." (Jackson, 2001: 662) Jackson seems to be suggesting that subjects who took Bob to have knowledge could have assumed that his belief was the result of an inference based on just such an implicit premise. This being the case, their intuitions would not conflict with those who had the standard Gettier intuitions. As we shall see in chapter three, some of the restrictionists' experiments seem to confirm Jackson's hypothesis.

experimental restrictionists focus on how ER takes AP to be using intuitions in specific philosophical investigations. The point is that analytic philosophers need not be identified as doing conceptual analysis; any approach that allegedly uses intuitions as evidence for philosophical claims is a potential target for ER.

1.4 Conclusion and Outline of Subsequent Chapters

The aim of the introduction has been to give the reader a better understanding of experimental philosophy generally and experimental restrictionism specifically. Furnished with these resources, we can now proceed to a more detailed analysis and critique of experimental restrictionism. The first two chapters should be understood as defensive moves. Chapter 1 examines how restrictionists have characterized analytic philosophy and its appeals to intuitions. Restrictionists claim that analytic philosophers unjustifiably appeal to intuitions as evidence. I evaluate their reasons for this claim and find them wanting. Chapter 2 is devoted to a detailed examination of the past, present and future of intuitions in philosophy. I argue that intuitions can be useful epistemic tools if assigned a somewhat more modest role in philosophical inquiry.

Chapter 3 takes a more aggressive approach to experimental restrictionism. In it, I examine restrictionism's methodology, past criticisms of it and the restrictionist responses to them. This chapter culminates with an original argument against experimental restrictionism, predicated on the false presupposition made by restrictionists that intuition constitutes a natural mental

kind. This faulty assumption underlies restrictionism's method and thus undermines its conclusions.

CHAPTER ONE: Restrictionist Characterizations of Analytic Philosophy

"The most savage controversies are about matters as to which there is no good evidence either way."
-Bertrand Russell

1.1 Introduction

For all the pride that most philosophers take in being precise, look to practically any philosophical debate and you will find over-generalization (or charges thereof), straw men (or charges thereof) and claims of being misread, misinterpreted or generally misunderstood. The debate between analytic philosophers and experimental restrictionists is no exception. Hence, the aim of this chapter is to get clear on how ER has characterized the use of intuitions in analytic philosophy and to determine the legitimacy of this characterization.

In order to help frame our investigation, this chapter will take examples from three of the most important ER papers to date: *Normativity and Epistemic Intuitions, The Instability of Philosophical Intuitions: Running Hot and Cold on Truetemp* and *Semantics, Cross-Cultural Style*. These papers discuss the work of such prominent analytic philosophers as Nelson Goodman, Alvin Goldman, Edmund Gettier, Keith Lehrer and Saul Kripke. Direct references are made to the work of these philosophers at first for explication and later in partial response to the restrictionists' claims.

As will become apparent, the central concern of restrictionism is analytic philosophy's apparent use of intuitions *as evidence* for or against philosophical

claims. Addressing this concern leads naturally to the question of what it is about intuitions that makes them a poor source of evidence. The answer, according to one leading restrictionist, is that intuitions are "unmitigatedly fallible" or, to use his terminology, "hopeless". In the final sections of this chapter, I argue first that intuitions are not hopeless and second that hopelessness itself is an inadequate standard by which to judge the epistemic value of intuitions or indeed, of any source of evidence at all.

1.2 Three Key Papers from Experimental Restrictionism

Normativity and Epistemic Intuitions

In *Normativity and Epistemic Intuitions*, Weinberg, Nichols and Stich outline a project within normative epistemology they call Intuition-Driven Romanticism (IDR). The project is intuition-driven in the sense that it depends crucially on taking epistemic intuitions as data or input. As Weinberg et al. understand it an epistemic intuition is, "a spontaneous judgement about the epistemic properties of some specific case—a judgement for which the person making the judgement may be able to offer no plausible justification."[1] (Weinberg et al., 19) The 'Romanticism' in IDR (by the authors' own admission) is added just to be provocative. They note that 'Platonism' could also be used as a label for this

[1] It's tempting to think that WNS are winning their debate with analytic philosophers by mere definition, since it would surely be irrational to treat as a premise a judgment that is both spontaneous and unjustified. In fact, this definition of 'intuition' agrees perfectly well with the one given in Goldman & Pust (1998: 179). Whether such a definition is—or should be—accepted by the majority of analytic philosophers is discussed in depth in the next chapter.

project, since both Platonism and Romanticism share the assumption that there are truths within us waiting to be discovered, in this case, truths about epistemic norms.

There are three criteria that Weinberg et al. take to be necessary for a project to be considered an IDR strategy. The first has already been stated: the strategy must take epistemic intuitions as data or input, though they grant that it can also exploit various other sorts of data. The second criterion concerns the strategy's output: "It must produce, as output, explicitly or implicitly normative claims or principles about matters epistemic." (Weinberg, et al., 20) Under explicitly normative claims, Weinberg et al. group, "regulative claims about how we ought to go about belief formation, claims about the relative merits of various strategies for belief formation, and evaluative claims about the merits of various epistemic situations." (ibid) Under implicitly normative claims, Weinberg et al. group "claims to the effect that one or another process of belief formation leads to justified beliefs or to reveal knowledge or that a doxastic structure of a certain kind amounts to real knowledge." (ibid) This more or less covers the stock and trade of epistemology and since nothing in Weinberg et al. seems to hang on the distinction between explicitly and implicitly normative claims, I won't dwell on it. Finally, as already implied, for a strategy to count as IDR, its output must depend—at least in part—on the epistemic intuitions it takes as input. The extent to which the output must depend on intuitions is borne out in the caveat that the input of significantly different intuitions should yield a significantly different

output. All of this can be summed up quite simply: IDR depends crucially on epistemic intuitions.

Though it is conjectured that much of what goes on in normative epistemology can be classified as IDR, three specific examples are cited. The first is Nelson Goodman's reflective equilibrium.[2] This should not be surprising since Stich has a history of criticizing Goodman's strategy on the basis of its dependence on intuition.[3]

Briefly paraphrased, Goodman argues that inductive inferences can be justified in much the same way that deductive inferences are justified. The validity of a deduction depends on its conformity with valid rules of inference. What makes a rule valid is its generation of acceptable inferences. A rule that yields an unacceptable inference is amended. Likewise, if an inference violates a rule that we are unwilling to amend, it is rejected. Though this may seem circular, Goodman argues that it is a virtuous circle, since the aim is to make "mutual adjustments between rules and accepted inferences; and in the agreement thus achieved lies the only justification needed for either." (Goodman, 64) Weinberg et. al claim that intuition is what motivates our willingness or unwillingness to accept or reject rules and inferences. Thus it is alleged that on Goodman's account, our intuitions ultimately ground both our deductive and inductive practises.

[2] It should be noted that the term 'Reflective Equilibrium' was not Goodman's; its first use is found in Rawls (1971).
[3] See Stich (1990), Chapter 4 and Stich (1988)

The second example of an IDR strategy offered by Weinberg et al. is drawn from Alvin Goldman's *Epistemology and Cognition* (1986). One of Goldman's central arguments in the book is that an epistemological theory should provide a set of rules that tell us which of our beliefs are justified and which are not. Goldman calls these *J-rules* (Goldman, 60). Since different epistemic theories are likely to advocate different sets of *J-rules*, Goldman avers that in addition to *J-rules*, we also need what he calls *a criterion of rightness*, which determines which system of *J-rules* is right. However, as Weinberg et al. point out, "now the theoretical disputes emerge at a higher level, for different theorists have suggested very different criteria of rightness." (Weinberg et al., 21) According to Weinberg et al., Goldman's answer to the question of how we can decide between various criteria is to test them against our "pre-theoretic intuition." As evidence for their claim, Weinberg et al. cite Goldman's assertion that, "A criterion [of rightness] is supported to the extent that implied judgements accord with such intuitions and weakened to the extent that they do not." (Goldman, 66)

A Brief Digression on Gettier Cases

The final exemplar of Intuition Driven Romanticism is not constituted by one specific strategy, but is instead characterized by a clump of literature that focuses on a particular problem: the now infamous Gettier cases. Since Gettier cases have been a central interest of the ER project, it's worth devoting some space to explaining them in depth.

In one of the landmark papers in epistemology, Edmund Gettier argues that having a justified true belief (JTB) is not sufficient for having knowledge. He begins with two assumptions: first, that it is possible for a person to be justified in believing a proposition that is in fact false[4], and second, that for any proposition P, if S believes P and P entails Q and S deduces Q from P and accepts Q on the basis of this deduction, then S is justified in believing Q. With these assumptions stated, Gettier moves on to outline two scenarios in which a subject does not have knowledge, despite having a true belief. To avoid getting bogged down in details, I will only paraphrase one of them.

Gettier asks us to imagine that two men, Smith and Jones, have applied for a job and that Smith has strong evidence for believing the following conjunctive proposition:

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

Gettier suggests that the strong evidence Smith has for believing (d) is that the president of the company has assured Smith that Jones will get the job and that Smith himself has recently counted the number of coins in Jones' pocket. Gettier then states that (d) has the following entailment, which Smith sees, accepts and is justified in believing:

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

[4]In order to save the JTB account of knowledge, this first assumption was rejected by Armstrong (1973), p. 152, and Clark (1963). However, reformulations of the Gettier problem have since been proposed that do not rely on this assumption—for example in Goldman (1976). I use one of Gettier's original thought-experiments in the following explication out of deference to the historical significance of his paper.

Gettier concludes by suggesting that we imagine that Smith is in fact the man who will get the job and furthermore, Smith also has ten coins in his pocket, though Smith is unaware of either of these facts. Hence, it is clear that (e) is still true, Smith still believes (e) and, most importantly, Smith is still justified in believing (e). But, as Gettier points out,

> it is equally clear that Smith does not *know* that (e) is true; for (e) is true in virtue of the number of coins in Smith's pocket, while Smith does not know how many coins are in Smith's pocket and bases his belief in (e) on a count of the coins in Jone's pocket, whom he falsely believes to be the man who will get the job. (Gettier, 122)

Although there have been numerous modifications of the original scenario,[5] the important point here is the assertion made by Weinberg et al. that, "just about all of the vast literature that arose in response to Gettier's classic paper uses intuitions about specific cases to test proposed analyses of the concept of knowledge."[6] (Weinberg et al., 21)

Running Hot and Cold on Trutemp

Another significant ER paper which criticizes the use of intuitions in epistemology was recently published by Stacey Swain, Joshua Alexander and Jonathan Weinberg. Entitled *The Instability of Philosophical Intuitions: Running Hot and Cold on Truetemp,* the paper examines Keith Lehrer's use of intuitions as evidence against reliabilism in his Truetemp Case. Epistemological reliabilism is

[5]For an overview of the Gettier literature see Pollock (1986) and Shope (1983)
[6] One can't help but wonder what *else* could be used to test the concept of knowledge. Unfortunately, Weinberg et al. do not offer any suggestions.

defined broadly by Swain et al. as holding that, "a person's true belief that *p* counts as knowledge just in case it is caused, or causally sustained, by a reliable cognitive process." (Swain et al., 139) They admit that this is a very general characterization of reliabilism, but note that since their aim is to critique the use of intuitions and not any particular version of reliabilism, this general characterization should be sufficient. They also choose to gloss over the distinction between a belief's being causally produced and its being causally sustained for the same reason.

Lehrer's Truetemp case is cited by Swain et al. as being "standardly appealed to as an argument against reliabilism" (ibid). The case is quoted verbatim by Swain et al. and it is worth doing the same here:

> Suppose a person, whom we shall name Mr. Truetemp, undergoes brain surgery by an experimental surgeon who invents a small device which is both a very accurate thermometer and a computational device capable of generating thoughts. The device, call it a tempucomp, is implanted in Truetemp's head so that the very tip of the device, no larger than the head of a pin, sits unnoticed on his scalp and acts as a sensor to transmit information about the temperature to the computational system of his brain. This device, in turn, sends a message to his brain causing him to think of the temperature recorded by the external sensor. Assume that the tempucomp is very reliable, and so his thoughts are correct temperature thoughts. All told, this is a reliable belief-forming process. Now imagine, finally, that he has no idea that the tempucomp has been inserted in his brain, is only slightly puzzled about why he thinks so obsessively about the temperature, but never checks a thermometer to determine whether these thoughts about the temperature are correct. He accepts them unreflectively, another effect of the tempucomp. Thus, he thinks and accepts that the temperature is 104 degrees. It is. Does he know that it is? (Lehrer, 163-164)

According to a reliabilism, Truetemp does know that it is 104 degrees because his belief has been produced by a reliable cognitive process. But, write Swain et al., "Lehrer claims that there is something lacking in Mr. Truetemp's epistemic position, such that his temperature beliefs do not count as knowledge." (Swain et al., 140) They allege further that Lehrer asserts that if we consider this case, we

will have the intuition that Mr. Truetemp does not know that it is 104 degrees. "Reliabilism's inability to account for this intuition is supposed to be reason to reject reliabilism." (ibid)

Swain et al. state that philosophers have generally accepted the appeal to intuitions about the Truetemp case as evidence against reliabilism. Even proponents of reliabilism grant that Truetemp presents a serious objection to their theory. Finally, Swain et al. offers an account of what they call "standard practise", whereby a philosophical claim is *prima facie* good to the extent that it accords with our intuitions and *prima facie* bad to the extent that it does not. They conclude their summary by stating that, "Given that intuitions about thought-experiments are standardly taken as reasons to accept or reject philosophical theories, then we should be interested in finding out what the relevant intuitions are." (ibid)

Semantics, Cross-Cultural Style

The other area of analytic philosophy that has garnered much ER attention is the philosophy of language, specifically in theories of meaning and reference. In *Semantics, Cross-Cultural Style*, Edouard Machery, Ron Mallon, Shaun Nichols and Stephen Stich discuss the longstanding debate between the descriptivist view and the causal-historical view of reference. For ER purposes, the important commonality between these two views is the assumption that a correct theory of reference will accord with our intuitions, at least for the most

part.

The descriptivist and causal-historical views picked out by Machery et al. are each delineated by a pair of theses. The first is that all accounts of descriptivism assert that there is a description consisting of a set of properties associated with every proper name invoked by a language user. The second thesis asserts that the referent of a name is necessarily and sufficiently determined by the object that entirely and uniquely satisfies its associated description. Two slightly contentious caveats to this second thesis are that if there is not an object that entirely satisfies the description, then the proper name refers to the unique individual that satisfies most of the description and that if the description is not satisfied at all or if it is not uniquely satisfied then the name does not refer.

In contrast, all variations of the causal-historical view hold that a name is introduced into a linguistic community with the intention of it referring to a unique individual. Successive users of the name refer to that same individual so long as they are causally linked to the first user who introduced the name. Although the proponents of the causal-historical view grant that users may associate a description with the name, their second thesis holds that once the name is introduced, the description plays no role in fixing the referent. What matters is the causal chain from user to user; the referent may entirely fail to satisfy the description associated with the name.

According to Machery et al., the descriptivist view is most closely

associated with Frege and Searle[7], while the causal-historical view is most closely associated with Kripke. They write,

> Indeed, Kripke's masterstroke was to propose some cases that elicited widely shared intuitions that were inconsistent with traditional descriptivist theories. Moreover, it has turned out that almost all philosophers share the intuitions elicited by Kripke's fictional cases, including most of his opponents. (Machery et al., 48)

The point here is not just that Kripke's thought-experiments provoked the creation of a new theory of reference, but that even those who were unwilling to grant the correctness of the causal-historical view still considered it necessary to account for the intuitions elicited by Kripke. Hence the target of ER critique in the philosophy of language is the validity the intuitions pumped by Kripke in *Naming and Necessity*.

Two of Kripke's scenarios form the focus of the charges levelled by Machery et al. They offer a brief summary of each. In the first, the description of a name is best satisfied by someone other than the individual to whom we would *intuitively* think the name refers. He asks us to imagine that the description associated with the name 'Gödel', which is 'the discoverer of the incompleteness of arithmetic', is actually satisfied by a man called Schmidt, whose body was found under mysterious circumstances. Kripke then points out that on the descriptivist view,

> since the man who discovered the incompleteness of arithmetic is in fact Schmidt, we, when we talk about 'Gödel', are in fact always referring to Schmidt. But it seems to me that we are not. We are simply not. (Kripke, 84)

[7] The claim that Frege was a descriptivist is actually contentious since it depends on how his conception of the relationship between names and descriptions is understood. For Searle's view, see (Searle, 1982)

In this scenario, the descriptivist view is *intuitively* making a mistake about the referent of a name and from this Kripke concludes that there must be a defect in the view itself.

In Kripke's second thought experiment, the description associated with a name is not satisfied at all. His example is the name 'Jonah', the description of which is 'the prophet who was swallowed by a whale'. Kripke suggests that even if no prophet had ever in fact been swallowed by a whale, *intuitively* it would not follow that Jonah did not exist. He writes,

> There still seems to be the question whether the Biblical account is a legendary account of no person or a legendary account built on a real person. In the latter case, it's only natural to say that, though Jonah did exist, no one did the things commonly related to him. (Kripke, 67)

On the descriptivist view, one would have to conclude that since no prophet was ever swallowed by a whale, Jonah did not exist, but again, this is *intuitively* mistaken, therefore there must some fault in the conditions for reference laid out by the descriptivist view.

The charge made by Machery et al. is that in both cases the elicited intuitions have been tacitly assumed universal and hence taken as evidence against the descriptivist view. If the relevant intuitions turn out *not* to be universal, then, "This would raise questions about whose intuitions are going to count, putting in jeopardy philosophers' methodology." (Machery et al., 49)

Summing Up

All five authors are characterized as taking intuitions to be evidence for or against philosophical claims. In Goodman's case, intuition constitutes our

judgements about the acceptability of inferences, as well as the validity of rules of inference, underpinning the entire process of reflective equilibrium. In Goldman's case, intuition is what decides between various criteria of rightness and thus determines which set of J-rules we adopt and thus determines what counts as a justified belief. In Gettier cases, we have the intuition that Smith does not have knowledge and from that intuition alone, we draw the conclusion that a belief's being true and justified is not sufficient for its being knowledge. In Lehrer's Truetemp scenario, our intuition that Truetemp does not know that the temperature is 104 degrees—despite the fact that the belief was reliably produced—is sufficient to demonstrate the inadequacy of reliabilism. Finally, Kripke concludes that the descriptivist theory of reference is inadequate because it generates counter-intuitive results in the Gödel-Schmidt scenario.

1.3 Unmentioned Caveats in Goodman, Goldman & Lehrer

Though the restrictionists have demonstrated their claims with direct quotations in most of the cases outlined above, it may surprise the reader to learn that often these quotations have left out caveats made by the authors which significantly weaken the evidentiary role of intuitions in their work. For example, shortly after outlining the process of reflective equilibrium, Goodman compares the mutual adjustments between rules and inferences of induction to the mutual adjustments between definitions and established usages of words. However, he grants that,

> Of course this adjustment is a more complex matter than I have indicated. Sometimes, in the interest of convenience or theoretical utility, we deliberately permit a definition to run counter to clear mandates of common usage. We accept a definition of "fish" that excludes whales. Similarly we may decide to deny the term "valid induction" to some inductive inferences that are commonly considered valid, or apply the term to others not usually so considered. (Goodman, 66)

This indicates quite clearly that Goodman does not take our intuitions to have the final word on the validity of rules and inferences. Indeed, this should not be surprising since the aim of Goodman's reflective equilibrium is to justify inductive practices which often involve scientific theories that run counter to our intuitions.[8] Hence, though intuitions may play an evidentiary role in Goodman's reflective equilibrium, their output is far less definitive than is suggested by Weinberg et al..

There is also a surprising omission in the restrictionist characterization of Goldman. While it's true that Goldman wrote, "A criterion [of rightness] is supported to the extent that implied judgements accord with such intuitions, and weakened to the extent that they do not." (Goldman, 66) Immediately following that sentence Goldman writes, "But our intuitions are not final. They can be pruned and adjusted by reflection on candidate rule systems. There are other tests of a criterion's adequacy as well." (ibid) As examples of such tests, Goldman cites the criterion's ability to generate rule systems and the comprehensiveness of the rule systems generated. Like Goodman, intuitions are not as foundational in Goldman's theory as Weinberg et al. would have us believe.

[8] For example, Richard Feynman writes: "Because atomic behavior is so unlike ordinary experience, it is very difficult to get used to and it appears peculiar and mysterious to everyone, both to the novice and to the experienced physicist." (Feynman, 1: 7)

Like Goodman and Goldman, Lehrer also makes an important disclaimer about the relevance of intuitions to his project which is not mentioned by Swain et al. Concerning the Truetemp case, he writes, "The preceding example is not presented as a decisive objection against externalism [reliabilisim] and should not be taken as such. It is possible to place some constraint on relationships or processes converting belief to knowledge to exclude production by the tempucomp." (Lehrer, 164)

The fact that these three caveats made by Goodman, Goldman and Lehrer are not mentioned in the restrictionist papers discussed above suggests that their authors either didn't notice them, noticed them but took them to be unimportant or noticed them but chose to exclude them for rhetorical purposes. The first and last possibilities are seriously damning, since they imply either that the authors conducted a poor survey of the literature their experiments are meant to attack or that they are more concerned with making a rhetorically forceful point than with portraying their opponents fairly. In either case, one cannot help but wonder at the purity of the restrictionists' motives.

For this reason, I want to give restrictionism the benefit of the doubt and assume that the authors of the papers discussed above were aware of the caveats made by Goodman, Goldman and Lehrer but took them to be insufficient to erode their claim that these philosophers are using their own intuitions in an evidentiary capacity. Still, if this is indeed the case, one cannot help but wonder *why* these caveats were not taken to be at least worth mentioning.

1.4 Using Intuitions as Evidence

In addition to maintaining their claim that Goodman, Goldman and Lehrer are using their own intuitions as evidence in the cases outlined above (despite their caveats), the restrictionists could also point out that the preceding section only mentioned three of the five authors they cited as using intuitions as evidence. Neither Gettier nor Kripke place any qualification on the evidential status of the intuitions their thought-experiments are meant to evoke.[9] Considering their influence on epistemology and the philosophy of language respectively, their use of intuitions as evidence—if dubious—could undermine not only their own work, but any and all work that has taken the evidential status of these intuitions for granted. If nothing else, this demonstrates how damaging using a dubious source of evidence can be.

With that in mind, I want to shift focus from particular instances of (supposedly) using intuitions as evidence to the use of evidence in analytic philosophy in general. Even though intuitions are not the only source of evidence to which philosophers defer, they are the only source that has been singled out for restrictionist critique. This suggests that there is something about intuitions which sets them apart from the other sources of evidence that restrictionists accept. But before we explore the viability of intuitions as a source of evidence, I want to pause to consider some of the other sources of evidence that analytic philosophers defer to.

[9] However, it is at least worth noting that they don't actually use the word 'intuition' or any of its cognates.

Examples of (Non-Intuitive) Philosophical Evidence

Even the staunchest restrictionist critic of analytic philosophy will not deny that intuitions are only one of many sources of evidence to which analytic philosophers defer.[10] A complete list of sources would be cumbersome, but a few examples should suffice to establish this point. If nothing else, there are certain basic sources of evidence to which analytic philosophy—along with every other discipline—often defers: sense perception, testimony and memory. One might object that the *a priori* nature of analytic philosophy forbids its practitioners from deferring to such sources, but such an objection merely betrays a naive understanding of analytic philosophy.[11]

In addition to these basic evidential sources, analytic philosophers also use the results of scientific inquiry as sources of evidence for philosophical claims. The Churchlands—to use an obvious example—often use the findings of neuroscientists as evidence for their own claims or against those of their opponents.[12] Jerry Fodor makes an explicit abductive argument in his latest book,

[10] Evidence can be understood in two distinct—but arguably interdependent—ways. A psychological understanding of evidence treats it as though it were identical with justification, e.g. Weinberg (2008) "However, even if the appeal to an intuition is never taken to be the end of the argument on the whole, it is usually meant to be the terminus of a particular chain of reasoning." (321) On the other hand, evidence can also be understood causally, wherein the effects of a given event are taken to be evidence for it (e.g., "Smoke is evidence of fire") and would be so *even if no one was aware of the evidential relation* as in Williamson (2009) "Why is it bad for an assertion to be inconsistent with the evidence? A natural answer is: because then it is false. That answer assumes that evidence consists only of *true* propositions." (209, emphasis is author's) The former sense permits faulty sources of evidence whereas the latter does not. However, one could also say that the good sources of evidence in the psychological sense are those which exploit the relations upon which the causal sense is based. In what follows, I shall assume that the former sense is the one at play since it is the sense most apparent in the restrictionist literature.

[11] See Williamson (2008: 165-9)

[12] See Churchland (2007) for an overview of Paul Churchland's recent work.

using recent findings in cognitive psychology as evidence for his thesis.[13] Even without explicit references like these, it should still be clear that analytic philosophers rely on non-intuitive sources of evidence all the time (e.g. logical axioms, formal deductions, etc.). Having considered some examples of other sources of evidence, we can now ask what it is that makes these sources epistemically better than intuitions.

The Problem with Intuitions

It's clear that restrictionists must take at least *some* sources of evidence to be legitimate. If they didn't, it's hard to see how they would go about justifying their own claims, since that is by definition what evidence is used for. The results of scientific inquiry are obviously endorsed as a good source of evidence by restrictionists (hence their experimental method), but it's less obvious whether restrictionists would count the other examples listed above as good sources of evidence or not. Either way, there's no question that intuitions would not make the cut. If the function of these sources is the same, i.e., to provide justification for one's assertions, then there must be some other way that restrictionists can discriminate between them.

Robert Cummins has argued that the problem with intuitions is that they cannot be calibrated in the same way that other sources of evidence can.[14] Using

[13] See Fodor (2008: 185-91)
[14] It should be noted that Cummins is an analytic philosopher skeptical of intuition, but not a restrictionist.

the example of Galileo's telescope, Cummins points out that the reason its deliverances were trusted was due to the fact that they could be compared with the deliverances of other sources independent from it. It was difficult for Galileo's contemporaries to accept that a heavenly body like the moon could be pockmarked with craters and mountains, but by aiming his telescope at mountain ranges, the features of which were known by observation with the naked eye, they were able to verify that the telescopes deliverances were accurate. Hence, at minimum calibration requires "that there be, in at least some cases, access to the target that is independent of the instrument or procedure to be calibrated." (Cummins, 117) But because calibrating a source requires independent veridical access to its deliverances, Cummins concludes that:

> [E]ven if philosophical intuitions *can* be calibrated, it never *is* calibrated, because philosophers could have no possible use for intuition in a context in which the relevant theory was well enough settled to form the basis of a credible calibration test. (ibid, 118)

The problem with Cummins calibration objection is that it can be made just as easily against perception, memory and mathematical judgement.[15] Indeed, it is our inability to non-circularly calibrate these sources that gave Descartes reason to doubt them in the Meditations. The calibration objection is a good start, but for a more robust objection to the validity of intuitions as a source of evidence, we must turn to one of the leading proponents of experimental restrictionism.

[15] Another problem with Cummins objection is that it tends to conflate intuition understood as a mental faculty with intuitions understood discretely. One might also point out that Galileo's opponents did not accept that just because the telescope worked on Earth, it would also work in the heavens, since they took it that the heavens were made of different stuff with different optical properties, but this is more a matter of historical accuracy than argumentative force.

1.5 - Jonathan Weinberg and the Hopelessness of Intuitions

Despite his notoriety as a leading member of the restrictionist project, Jonathan Weinberg does not have a problem with intuitions. Rather, his issue is with analytic philosophy's practise of appealing to intuitions in an evidentiary capacity; what he calls "philosophers' appeals to intuitions" or PAI (Weinberg, 321). Thus his aim is to narrow the target of restrictionism's critique to "a *particular* flavour of philosophical intuition" (ibid, 319, emphasis is author's); wisely so, since there are many intuitive judgements that restrictionists do not want to reject.[16]

Weinberg avers that the restrictionist must argue that PAI has some undesirable epistemic characteristic which makes it untrustworthy, but whatever this characteristic is, it cannot be so wide as to rule out other sources of evidence which the restrictionists do not want to attack, (e.g. sense perception) nor can it undermine the source(s) of evidence used for the restrictionists' own argument. Ergo, general fallibility is ruled out as a potential epistemically deleterious characteristic of PAI, since it would also rule out sense perception: a source of evidence the empirically-minded restrictionists would certainly *not* want to give up.

But although fallibility *simpliciter* is insufficient for the restrictionist thesis, Weinberg argues that *unmitigated* fallibility is a prime candidate. He

[16] Weinberg cites, "the ordinary application of concepts to particulars (Bealer), or the claim that no object can be red all over and green all over (BonJour), or elementary mathematics (Sosa)." as examples of intuitive judgements that are not and should not be under restrictionist attack. (ibid, 320)

defines "unmitigated fallibility"as"fallibility uncompensated by a decent capacity for detecting and correcting the errors that it entails." (ibid, 323) Because of its unwieldiness, Weinberg replaces "unmitigatedly fallible" with "hopeless" and its converse (mitigatedly fallible) with "hopeful".[17] This, then, is the epistemically deleterious characteristic which Weinberg takes intuitions to posses and sense-perception (as well as other sources of evidence) to lack.

Why Reject Hopeless Sources of Evidence?

Weinberg offers three reasons for adopting hopefulness as an epistemic virtue and hopelessness as an epistemic vice. The first is pragmatic: "relying on sources of evidence that are hopeful will make us less likely to make unrecoverable errors in our inquiries." (ibid, 327) In other words, a source of evidence's being hopeful makes it more likely to avoid falsehoods than its being hopeless does. Provided that our epistemic goals involve avoiding falsehoods as well as pursuing truths, hopefulness is a desirable epistemic characteristic and hopelessness an undesirable one.

A second reason to prefer hopeful sources of evidence to hopeless ones is that hopefulness is conducive to resolving disagreements between researchers (ibid, 328). Suppose there are two rival theories with equal amounts of evidence

[17] A source of evidence that is not practically infallible is hopeful to the extent that we have the capacity to detect and correct its errors." (ibid, 327) Note that by their very definition, infallible sources of evidence cannot be hopeful: a source of evidence cannot have the capacity to detect and correct errors if it lacks the capacity to make errors in the first place. This seems to suggest then that infallible sources of evidence (assuming there are such things) are necessarily hopeless, though perhaps Weinberg's criterion is only meant to apply to fallible sources in the first place.

in their favour. If the sources of evidence for both theories are hopeless, then, according to Weinberg, there is no way of determining which theory is correct and which mistaken. The mere existence of two conflicting theories suggests that *someone* must be mistaken, but that cannot be enough, especially given the live possibility that *both* theories are false.

The final reason to adopt hopefulness as an epistemic norm is that doing so increases our chances of discovering truths. If our sources of evidence are hopeful, then we can be confident that any falsehoods our inquiry yields will eventually be discovered and rejected. This epistemic confidence permits us to take a wider range of evidence into account, thereby increasing our odds of discovering truths. (ibid, 329) This last point may be hard to swallow without adding an important caveat made by Weinberg, namely that: "hope does not always come from intrinsic aspects of the source of evidence itself so much as from the particular practises of using it." (ibid, 331)

More attention is given to this point below, but for the moment it is worth simply pointing out that this entails that one can have (relatively) more hopeful sources of evidence dragged down by hopeless practices and (relatively) less hopeful sources of evidence bolstered by hopeful practices. This carries the further implication that one can have degrees of hopefulness; Weinberg grants this point, though he denies that hopelessness is also a matter of degree (ibid).

Weinberg's Four Sources of Hope

Having outlined Weinberg's cited reasons for adopting the

hopeful/hopeless distinction we can now ask what it turns on. In other words: What makes a device[18] amenable to error detection and correction? Weinberg identifies four sources of hope which are taken to be exhaustive. Hopefulness is constituted by some combination of the following factors; all four need not obtain in order for a source to be hopeful, but if none obtain then it's safe to say that the source is indeed hopeless.

The first factor highlighted by Weinberg is *external corroboration*: a device is more likely to be hopeful if its deliverances can be compared to the deliverances of other devices. This factor can be seen in the value placed on having replicable results in the sciences. The second source of hope is *internal coherence*: a device is more likely to be hopeful if its deliverances are transparently in agreement with one another.[19] The *detectability of margins* is the third source of hope: "The practices are sensitive to the conditions in which the device is less likely to give good results." (ibid) For example, we know that our vision (a source of evidence Weinberg takes to be hopeful) functions less well at night, and so we are more suspicious of its deliverances (or reports based on its deliverances) during that time. The fourth and final source of hope is *theoretical illumination*: we know how the device works when it does and why it fails to work when it doesn't. Examples of this source are most easily found in the

[18] Weinberg seems to use the term "device" to refer both to the source of evidence and to our practises surrounding it. For brevity's sake I will use the term in the same way.
[19] The qualifier "transparently" is meant to reflect the fact that internal coherence requires not only that the device's deliverances agree, but that they also be comparable with one another "both within and across subjects" (ibid, 330).

instruments of modern science.[20]

Hopeless Intuitions (and the Practises of Appealing to Them)

The four factors outlined above are all that constitutes hope. With his sources exhaustively delineated, Weinberg sets out to argue that philosophers' appeals to intuition lack all of them. In other words, that they are hopeless. Note before we proceed that restrictionists must deny that PAI does not have a single source of hope, since even one source would be enough to make it (at least) minimally hopeful.

Regarding the first factor, Weinberg avers that there simply aren't any other devices whose deliverances we could compare with those of intuition. Moreover:

> In some of the cases where we clearly have gotten alternative access to the subject matter—for example, physics' discoveries about space and time—no effort has been made to use this overlap with philosophy to develop any general means of error detection within philosophical practice. If I may paint with a very broad historical brush, generally once someone has found another way of working on a domain, the practitioners of intuition based-philosophy have retreated from it. (ibid, 339)

Against PAI's having internal coherence, Weinberg defers to the results of the three restrictionist papers focused on earlier in the chapter. In addition, he suggests that even within philosophy there is less intersubjective agreement than typically thought. Weinberg argues that this is the result of,

> The current structure of the discipline [which] practically ensures that those who share the key intuition may participate in the literature, and those who don't will simply be left out of it—their "negative results" just won't be published, as it were. (ibid, 337)

[20] Weinberg cites the electron microscope as an example of a hopeful source.

The detectability of intuition's margins is disputed on the grounds that it lacks the richness of signal enjoyed by more hopeful sources like sense perception. Weinberg takes the output of intuition to basically be a 1-bit signal, either "yes" or "no", "possible" or "not possible", etc. Sense perception, on the other hand, is probably more than a 1,000,000,000,000-bit signal. Doing some auto-phenomenology, Weinberg grants that there is something of a phenomenal feel to intuiting, characterized as a weaker or stronger subjective sense of certainty. But this anomaly is quickly discarded by pointing out that current philosophical practice has not explored nor exploited this gradation.

One might object that since Weinberg's theory allows for good practices to bolster weaker sources of evidence, intuitions could still be appealed to so long as our practices with them could locate their margins. Weinberg entertains this potential objection by considering how we might determine when our intuitions are more or less likely to be correct. He suggests that we could avoid appealing to intuitions that deviate greatly from the more quotidian ones, thereby benefitting from the hopefulness of everyday intuition.[21] However, Weinberg takes the current philosophical literature—replete with zombies, nigh-omniscient colour scientists, and water that isn't—to indicate that such a methodological amendment has yet to be made.

Concerning the theoretical illumination of intuitions, Weinberg notes that we have two ways of gaining a theoretical understanding of intuition:

[21] Like the intuitions cited by Bealer, BonJour and Sosa in Footenote 3.

introspectively or psychologically. The problem with the former is that intuitions have inherently opaque origins from an introspective standpoint. That, according to Weinberg, is part of what it is to be an intuition. The psychological approach is just as much a non-starter, but only contingently so; since psychology has yet to take an interest in the sort of intuition philosophy is concerned with.[22]

Having demonstrated that our practises of appealing to intuitions lack any of the four sources of hope, Weinberg concludes that intuitions are indeed hopeless. This is not due to some inherent flaw with "intuitive cognition", but is instead the result of "flaws in our practices involving those intuitions." (ibid, 340) Without the ability to detect and correct errors in our intuiting, we will be unable to decide which intuitions should be trusted and which should not. By Weinberg's lights, this is reason enough to reject PAI as a legitimate source of evidence. Weinberg does not deny that our practises with intuitions can be improved, but as they currently stand he does not take philosophers' appeals to intuitions to be a respectable source of evidence.

1.6 Don't Lose Hope Just Yet

Weinberg's intention to realign the current debate between restrictionists and analytics is commendable, but good intentions only go so far. Below I rebut his rejections of the four sources of hope as being applicable to philosopher's appeals to intuitions. But before I do, it's worth noting that even if we grant that

[22] For a thorough discussion of the contrast between philosophical and psychological conceptions of intuition, see Osbeck (1999).

the concept of hope provides a good framework for evaluating the trustworthiness of evidential sources, we need not see it as the only framework for doing so. A source of evidence can also be trustworthy because it is reliable or because it derives from some epistemic competence.

However, the trouble with having different evaluative frameworks is that they tend to produce different evaluations and the only way to choose between them is to step outside the frameworks and consider their broader implications. Should we prefer a very hopeful source that is nevertheless unreliable to a very reliable source that is nevertheless hopeless? The answer most likely depends on various non-epistemic factors, such as the practical risks and/or rewards of trusting one source over the other. What this shows is that even if philosopher's appeals to intuitions are hopeless, they need not be useless—provided they possess some other epistemically desirable feature.[23] However, we need not go as far as rejecting hope in order to establish the trustworthiness of philosopher's appeals to intuitions.

PAI: Hopeful After All

In spite of Weinberg's objections, a plausible case can be made for PAI being hopeful by demonstrating that it satisfies all four sources of hope to at least a minimal degree. There are several ways to demonstrate that philosophers' appeals to intuitions are externally corroborated. Weinberg himself grants that

[23] For more on this point, see Chapter 2.

some areas of philosophy have somewhat hopeful intuitions as a result of being externally corroborated. The three examples he cites are logic and mathematics, folk psychology and epistemic normativity (ibid, 339). Using Weinberg's reasoning for citing these domains as having hopeful intuitions we can extend hopefulness to virtually all domains of philosophy.

Logic and mathematics are included because:

> We have cross-checking from different sorts of intuitions: We have intuitions concerning the semantic properties of models, intuitions concerning the syntactic properties of proofs, and indeed our straight-on intuitions about mathematics itself. (ibid)

If cross-checking can be derived from having different sorts of intuitions in the same domain then it seems as though we should include any philosophical practise that involves reflective equilibrium. In reflective equilibrium, we have intuitions concerning our rules of inference, intuitions concerning the deliverances of those rules and indeed our straight-on intuitions about reflective equilibrium itself. To object to this claim, the restrictionist must show that there is some principled difference between these sorts of intuitions and the sorts quoted above, but aside from their content, it's hard to see what that difference could be since the two sets of intuitions are isomorphic.

Intuitions in folk psychology are cited because they often make empirical predictions which we can learn from should they fail. Is this all that is required for a source to be hopeful? If so then again we seem to have justification for granting hopeful status to PAI, unless the restrictionist wants to claim that philosophers are incapable of learning from the predictive failure of their appeals to intuitions. In

fact there are many examples of philosophers learning from the failures of their appeals to intuitions. Searle's Chinese room thought-experiment has undergone various modifications since it was first proposed, as has Frank Jackson's knowledge argument.[24] Implicit claims that intuition-based philosophy has not progressed are often found in restrictionist literature, but such arguments tend to have the same sort of rhetorical hand-waving as Weinberg's "broad historical brush" comment above.[25]

Weinberg's last example is indeed surprising, given the vehement restrictionist critique of normative epistemology. What makes epistemological norms checkable and correctable is our "long and well-documented history of trying out different norms to guide our inquiries, and we can learn from our historians which norms have been active when and what results they seem to have yielded." (ibid) But of course the same could be said for ethics: we have a long and well-documented history of trying out different norms to guide our social interactions, and we can learn from our historians (and perhaps anthropologists as well) which norms have been active when and what results they seem to have yielded. For example, we might look to correlations between a society's attitude towards capital punishment and its crime rate.[26]

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[24] For an overview of the former, see Searle (2008: 67-86). For the latter, see Jackson et. al (2004: 409-443)

[25] *Pace* Weinberg's claim that intuition-based philosophy retreats from a domain once another method of working on it are found, see Shahen (2006), Falkenburg (2004), Weinert (2005) and Bitbol (1998) for examples of attempts to integrate Kantian metaphysics with modern understandings of space-time and quantum theory.

[26] Granted, judgements about the success or failure of various ethical norms are bound to be controversial, but so too will judgements about the success or failure of adopting or rejecting any

Weinberg's exclusion of external corroboration from PAI seems unwarranted. I've only listed a few examples of externally corroborated PAI above, but doubtless many more can be found in analytic philosophy. Thus we can conclude that the first of Weinberg's four sources of hope is indeed satisfied by philosophers' practise of appealing to intuitions.

Weinberg's unsubstantiated suspicious of schisms aside—there are clear areas of intuitive agreement across subjects within philosophy's standard practices. I sincerely doubt that so many philosophers would have bothered responding to Gettier's paper had they not shared his intuitions. It's difficult to say whether or not the restrictionists' experiments have demonstrated significant intersubjective disagreement outside of philosophy; here I will grant the restrictionist point that more research would need to be done. Exactly how much more is difficult to say, since the level of agreement needed for a source to qualify as hopeful is never specified.

Regarding the intrasubjective agreement aspect of internal coherence, Weinberg cites various papers in the psychology of categorization which suggest that "we form our categories according to the demands of the moment, task-specifically and on-the-fly" (ibid, 337). Deferring to psychological studies is a common tack of restrictionists, but the incommensurability of many philosophical and psychological concepts (a problem acknowledged by Weinberg himself) suggests that such deference should carry little argumentative weight without

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set of norms, including epistemic ones.

further argument for the commensurability of the subject matter of the studies and that of philosophical literature. Once again, reference to reflective equilibrium is warranted. Indeed, the entire aim of the process is to produce internal coherence. Unless intuition-driven philosophers have been failing miserably in one of their core practises, denying that PAI has internal coherence, both within and across subjects, seems sorely mistaken.

Understood as 1-bit signals, intuitions do indeed seem to lack the necessary richness for us to discern their margins. Nevertheless, there are surely *some* margins that can be established for intuitions. As a cognitive process, it seems safe to say that intuition is less likely to perform well when the person doing the intuiting is drunk, fatigued or otherwise cognitively impaired. One could also argue that our practises of appealing to intuitions are sensitive to the fact that they may not always be accurate by awarding them less evidential weight than, say, deductions or empirical evidence. Recall that on Weinberg's account, a weak source of evidence can be bolstered by good practices. Thus, even if intuitions turn out to be somewhat unreliable, they can still be a hopeful source of evidence provided we are at least partially aware of their margins and accordingly adapt our practices as much as possible.

Weinberg is correct to point out that part of what makes intuitions what they are is their introspective opacity and for the most part I agree with his observation that the intuitions psychologists are interested in are quite different from the intuitions to which philosophers appeal. However, this is not to say that

finding theoretical illumination for our practise of appealing to intuitions is not possible. If we focus on PAI specifically, rather than intuitions in general, then it's not at all difficult to see why it works when it does and why it fails when it doesn't. Appeals to intuitions "work" when the intuitions appealed to are widely shared and fail to when they are not. This may be a fairly impoverished theoretical illumination, but it certainly isn't any worse than that enjoyed by sense perception prior to the development of modern psychology. Though this last factor is arguably the least well-established for PAI, its minimal satisfaction should accrue at least some hopefulness to the practice.

Taken together, these arguments demonstrate that philosophers' appeals to intuition are not nearly as hopeless as Weinberg has suggested. Even if each source of hope is only minimally obtained by PAI, it should still be sufficient to elevate it from hopeless to minimally hopeful. Moreover, the above comments represent only the earliest attempts to demonstrate the hopefulness of philosophers' appeals to intuitions. If Weinberg has his way and hope becomes the new locus of debate between restrictionists and analytics there will no doubt be even more robust demonstrations of the hopefulness of PAI.

1.7 Hope is Not Enough

Although I take the previous section to be an adequate rebuttal of Weinberg's argument for the hopelessness of philosophers' appeals to intuitions I don't take it to be worth much else. On its own, hope is neither a necessary nor a

sufficient condition for the trustworthiness of sources of evidence. As such, it is inadequate both as a standard for judging the trustworthiness of intuitions and for framing the debate on their epistemic status. I've already cited the reason for the failure of the first condition above: multiple frameworks exist for evaluating trustworthiness of evidential sources and when their results conflict there is no principled epistemic reason to prefer the concept of hope to any other relevant epistemic concept. Hence, a source of evidence need not necessarily be hopeful in order to be trustworthy.

The insufficiency of hope for trustworthiness can be demonstrated by attending to the fact that our determination of whether or not a given source of evidence is hopeful, (i.e. trustworthy) must itself rely on one or more sources of evidence. If these secondary sources of evidence must be hopeful as well, then the risk of an infinite regress becomes palpable: if primary and secondary sources need to be hopeful in order to be trustworthy then surely tertiary sources do as well and so on *ad infinitum*. I am not suggesting that sources of evidence fall into a hierarchy, but as Weinberg himself has granted, "Any epistemology that attends to our justificatory practices has to say *something* about where we stop offering more reasons and are right to do so..." (ibid, 320, n. 2, emphasis is author's) Of course, regardless of where our stopping point is under the hopefulness framework, it is guaranteed to be hopeless. Nevertheless, if there is a point at which we are "right" to stop offering reasons (or in this case, to stop providing evidence for the hopefulness of our sources of evidence) then the source we stop

at must be trustworthy despite not being hopeful. Hence we have an example of a trustworthy source of evidence that is not hopeful and thus hope is not a sufficient condition for the trustworthiness of a source of evidence.

1.8 Conclusion

In this chapter, I have outlined three central restrictionist characterizations of the use of intuitions in philosophy. In the cases of Goodman, Goldman and Lehrer, these characterizations turned out to be somewhat exaggerated. Concering the less mitigated cases of Kripke and Gettier, I asked what it is about intuitions themselves that engenders mistrust on the part of restrictionists. Jonathan Weinberg has argued that intuitions' supposedly unmitigated fallibility entails their epistemic untrustworthiness. Contra Weinberg, I argued that intuitions can be hopeful but that this accomplishes little, since hopefulness itself is an insufficient criterion for the trustworthiness of evidential sources. This focus on intuitions continues and broadens in the next chapter.

CHAPTER TWO: Intuitions and their Philosophical Import

"I use my intuition.
It takes me for a ride."
-John Lennon

1.1 Introduction

Given intuitions' pivotal role in the analytic/restrictionist debate, I would be remiss if I did not devote a chapter to discussing intuitions as they figure into the debate as well as philosophy more broadly. As we will see in Section 1.2, intuitions have had a long and illustrious history in philosophy, stretching all the way back to its very beginnings with Plato and continuing into the early modern period with Descartes and Kant. Section 1.3 examines extant accounts of intuitions in analytic philosophy by George Bealer, Ernest Sosa and Timothy Williamson. Section 1.4 covers the (admittedly sparse) restrictionist account of intuitions, examining definitions and usages from Jonathan Weinberg and Stephen Stich, among others. With the aim of finding a compromise between the analytic and restrictionist accounts, Section 1.5 details my own view of intuitions.

1.2 Intuitions in the History of Philosophy

Plato

For almost as long as there has been philosophy, there has been a practise of appealing to intuitions. Every philosopher is familiar with the Socratic Method found in Plato's dialogues. In order to discern the Form of the subject under investigation (be it knowledge, justice, piety, etc.) Socrates asks one or more

interlocutors for their understanding of it. He then proceeds to undermine his interlocutor's view by eliciting conflicting intuitions through various hypothetical cases.[1] Plato took dialectic of the sort epitomized by Socrates' method to be the highest form of enquiry. The rare fruits of Platonic dialectic yield *a priori* truths in the apprehension of Forms. So it would seem that Platonic intuitions are of the highest epistemic value since they form the foundation for the refutation of false accounts of the Forms, ultimately leading to the truthful ones. But despite the ubiquity of intuitions in the Platonic dialogues, there is, as far as I know, no actual Platonic analysis of intuition itself. The most probable explanation for this is that Plato simply did not distinguish intuition as being of a kind distinct from human rationality. In any case, the importance of intuitions in Platonic philosophy, understood as the motivators of counter-examples in the Socratic method, can hardly be denied.

Descartes

Descartes also placed a great deal of importance on intuition and like Plato, used intuition to great effect. But unlike Plato, in Descartes we find an explicit explanation of intuition itself. Descartes' views on intuitions are expounded in his *Rules for the Direction of the Mind*, written sometime in the late 1620s but published only posthumously. The work was intended to comprise thirty six rules divided evenly into three sections, though only the first section was

[1] As far as I know, Socrates never identifies his method with intuition pumping. Nevertheless, it's hard to imagine what else he could be eliciting other than intuitions. It's also worth noting that both sides of the current debate over intuitions have identified the Socratic method as intuition-seeking, so this suggestion is hardly controversial.

completed. Fortunately, it is in this section that Descartes offers his account of intuition:

> By 'intuition' I do not mean the fluctuating testimony of the senses or the deceptive judgement of the imagination as it botches things together, but the conception of a clear and attentive mind, which is so easy and distinct that there can be no room for doubt about what we are understanding. Alternatively, and this comes to the same thing, intuition is the indubitable conception of a clear and attentive mind which proceeds solely from the light of reason. ("Rules", 14)

Despite its explicitness, Descartes' definition of 'intuition' still requires some explanation. To begin with, it should be noted that Descartes attributed clarity to perception. In *Principles of Philosophy*, he explains what it is for a *perception* to be clear:

> I call a perception 'clear' when it is present and accessible to the attentive mind—just as we say that we see something clearly when it is present to eye's gaze and stimulates it with a sufficient degree of strength and accessibility. (ibid, 207)

Hence, Cartesian *intuitions* are clear in the sense that they are fully and unambiguously accessible to the mind.

In addition to their clarity, Cartesian intuitions are also distinct in the sense of being discernible other perceptions. (ibid, 208) This qualification entails that the actual content of a Cartesian intuition must be restricted to a single proposition, or at most to the connection between two propositions. In fact, if we continue to give the definitions laid down in the *Principles* priority, the clarity requirement becomes redundant, since any perception that is distinct is also clear by definition.[2]

[2] However, a perception that is clear need not be distinct. Descartes illustrates this point by giving the example of a subject experiencing intense pain. The subject perceives the pain clearly, but not necessarily distinctly, since she may confuse the sensation with "an obscure judgement they make concerning the nature of something which they think exists in the painful spot and which they

What is most significant about Cartesian intuitions is their indubitability. Descartes took intuitions to be certain; immune to even the most dire scepticism. Thus it should not be surprising that the examples of intuitions Descartes offered are somewhat limited: "Thus, everyone can mentally intuit that he exists, that he is thinking, that a triangle is bounded by just three lines, and a sphere a single surface, and the like." (ibid, 14) The first example offered makes clear the importance of intuitions to the Cartesian system; intuition provides the foundation from which deduction proceeds, ultimately leading to certain knowledge.[3]

Kant

Compared to Platonic and Cartesian intuitions, Kantian intuitions are a radical historical diversion, so much so that one might be tempted to dismiss Kantian intuitions as not being intuitions at all. But such dismissal would be premature, since a closer look reveals several important connections between Kantian, analytic and even experimental intuitions. The remarks below constitute little more than a first pass at such an analysis, but this is more than enough for present purposes.

The majority of Kant's writing on intuition can be found in his first

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suppose to resemble the sensation of pain; but in fact it is the sensation alone which they perceive clearly." ("Principles," 46)

[3] "Hence we are distinguishing mental intuition from certain deduction on the grounds that we are aware of a movement or a sort of sequence in the latter but not in the former, and also because immediate self-evidence is not required for deduction, as it is for intuition; deduction in a sense gets its certainty from memory. It follows that those propositions which are immediately inferred from first principles can be said to be known in one respect through intuition, and in another respect through deduction. But the first principles themselves are known only through intuition, and the remote conclusion only through deduction." (CSM, I, 15)

Critique and from this work, we can glean two important characteristics of Kantian intuitions. Briefly: Kantian intuitions are perceptual (this term will require further unpacking) and, like both the Platonic and the Cartesian, they are necessary for knowledge. Regarding the first point, intuitions are perceptual in the sense that they mediate between a subject's knowledge of a thing and the thing in itself. (Kant, 65) Moreover since Kantian subjects can never have direct apprehension of things as they are in themselves, their thoughts must be directed not toward things, but always ultimately to their intuitions of them. (Kant, 88)

Given these points, Kant's assertion that intuitions are necessary for knowledge makes a great deal of sense. They are not, however, sufficient for knowledge, since on the Kantian scheme, intuitions also require concepts in order to constitute knowledge (and vice versa). (Kant, 92) According to Kant, the distinction between intuitions and concepts is that, "The former relates immediately to the object and is single, the latter refers to it mediately by means of a feature which several things may have in common." (Kant, 315) Though this excerpt suggests that Kantian intuitions really are nothing more than perceptions, another distinction from much earlier in the same *Critique* shows this conclusion to be misguided:

> When they [intuitions and concepts] contain sensation (which presupposes the actual presence of the object), they are empirical. When there is no mingling of sensation with the representation, they are pure. Sensation may be entitled the material of sensible knowledge. Pure intuition, therefore, contains only the form under which something is intuited; the pure concept only the form of the thought of an object in general. Pure intuitions or pure concepts alone are possible *a priori*, empirical intuitions and empirical concepts only *a posteriori*. (Kant, 92)

The fact that there can be *a priori* Kantian intuitions in the absence of sensation

entails that Kantian intuitions are not merely perceptions. These pure intuitions are those which Kantian subjects use in mathematical reasoning, hence the reference to the form under which something is intuited. Thus, Kantian intuitions are just as essential as the Platonic and Cartesian, either mediating between subjects and the things in themselves (when sensations are mingled with representations) or forming the basis of geometric and mathematical judgements.

Kantian intuitions are quite unlike their Platonic and Cartesian counter-parts and, as we shall we, they also differ radically from intuitions in a more contemporary context. But this does not mean that Kantian intuitions are wholly unconnected to their contemporary counter-parts. Because of the mediating role they play between subjects and things in themselves, Kant's account is not unlike what Sosa calls "the perceptual model" of intuitions (Sosa, 2007: pp. 45-50).

Though numerous others throughout the history of philosophy have offered accounts of intuitions, the three discussed above are perhaps the most influential.[4] At the very least, the preceding overview should give the reader a sense of how ubiquitous and important intuitions have been throughout the history of philosophy. In the next section, we shall see that this tradition continues in analytic philosophy today.

[4] Spinoza takes intuition to be the highest form of knowledge (Spinoza, II, P40, S2, n. IV) and Locke (Locke, 2.1) and Hume (Hume, 4.1.1) both uses definitions of 'intuition' that are strikingly similar to Descartes'. One might also discuss Intuitionism in the philosophy of mathematics, but this lies somewhat outside the scope of my survey. For an overview of Intuitionism, see (Iemhoff, 2008)

1.3 Analytic Accounts of Intuitions

In an entry on intuition for the 1972 edition of the *Encyclopedia of Philosophy*, Richard Rorty writes:

> The broadest definition of the term "intuition" is "immediate apprehension." "Apprehension" is used to cover such disparate states as sensation, knowledge, and mystical rapport. "Immediate" has as many senses as there are kinds of mediation: It may be used to signify the absence of inference, the absence of causes, the absence of the ability to define a term, the absence of justification, the absence of symbols, or the absence of thought. Given this range of uses, nothing can be said about intuition in general. (Rorty, 204)

While he is correct regarding the multifarious nature of the term 'intuition', the numerous analyses of intuition conducted over the last decade or so in analytic philosophy show that a great deal has indeed been said about intuition in general. An overview of several contemporary analytic accounts of intuition is provided below.

George Bealer

The most prominent view of intuitions in contemporary analytic philosophy is as intellectual seemings.[5] This characterization can be found in Bealer (1996), where he argues that, "For you to have an intuition that A is just for it to *seem* to you that A. Here 'seems' is understood, not as a cautionary or "hedging" term but in its use as a term for a genuine kind of conscious episode." (Bealer, 123) In a later paper, Bealer further qualifies his account by making a distinction between *rational* or *a priori* intuition and *physical* intuition, the difference between them being that the former presents itself as necessary

[5] Intellectual seemings are contrasted with perceptual or introspective seemings in Bealer (2000: 3)

whereas the latter does not. (Bealer, 2000: 3)

As seemings, Bealer distinguishes intuitions from beliefs by claiming that the latter are not seemings and thereby concludes that intuitions are not merely beliefs. This distinction is motivated by examples where we believe a proposition despite its not seeming true to us, such as (though this is not one of Bealer's examples) the correct solution to the Monty Hall Problem.[6]Examples like the Monty Hall Problem also demonstrate that "the classical modern infallibilist theory of intuition is incorrect." (ibid) Though not stated explicitly, Bealer presumably attributes the infallibilist theory of intuition to Descartes, and possibly Locke and Hume as well. He further distinguishes between intuitions and beliefs by pointing out that the latter are far more plastic than the former, i.e., "Using (false) appeals to authority, cajoling, intimidation, brainwashing, and so forth, you can get a person to believe almost anything. Not so for intuitions." (ibid, 124) Intuitions are also distinct from judgements, guesses and hunches, since "There are significant restrictions on the propositions concerning which one can have intuitions; by contrast, there are virtually no restrictions on the propositions

[6] The Monty Hall Problem involves a contestant on a game show who is trying to win a car. She is presented with three identical doors: one conceals the car while the other two conceal goats. The contestant is asked to choose the door which she thinks conceals the car. Once her choice is made, the game show host (who knows the location of the car and the goats) opens one of the other two doors that the contestant did not pick to reveal a goat (since there are two goats, there will always been at least one door available to the host to open). The contestant is then given the choice of sticking with the door she originally chose or switching it to the other door that the host did not open. To most people it seems that switching makes no difference to the contestant's odds of winning the car, but in fact switching increases her odds from one in three (which is what they were when she initially chose her door) to one in two (since the game show host's action has eliminated one of the goat-options). The point is that once one knows the solution to the Problem, one believes that switching is the optimal strategy even though one may still have the intuition that it makes no difference.

concerning which one can make a judgement or a guess or have a hunch." (ibid) For the same reason, Bealer also denies that intuition is identical to common sense.

For Bealer, intuitions are foundational, in the sense of being a basic source of evidence. He justifies this claim by invoking *modal reliabilism*, which states that, "something counts as a basic source of evidence iff there is an appropriate kind of modal tie between its deliverances and the truth." (Bealer, 2002: 102) The modal tie between the deliverances of our intuitions and the truth is itself justified by our understanding of our concepts: our intuitions are true provided we understand the relevant concepts.[7] Interestingly, this link between intuitions and concepts harkens back to Jackson's account of intuitions, mentioned in the Introduction.

Ernest Sosa

Although Ernest Sosa agrees that intuitions are intellectual seemings, he takes issue with some of Bealer's arguments in Sosa (1996). In Sosa (2007) we get a full-fledged account of intuition markedly different from Bealer's. Sosa argues that intuition is a type of intellectual seeming in that,

> An intellectual seeming is *intuitive* when it is an attraction to assent triggered simply by considering a proposition consciously with understanding. (Of course one may so much as understand the proposition only through a complex and prolonged process that includes perception, memory, testimony, or inference.) (Sosa, 2007: 60-1, emphasis is author's)

An unusual aspect of Sosa's account is that it takes intuition to be our attraction to

[7] Specifically, we must understand the relevant concept *determinately*. See Bealer (2000) for further information.

assent to a proposition rather than the cause of our attraction to assent.[8] This claim is the result of Sosa's conclusion that the reason for the attraction to assent to the content of an intuitive proposition, "lies in nothing more than my conscious entertaining of that content." (ibid, 53) Sosa's account of intuition is thus more rigid than Bealer's since it restricts the source of our attraction to intuitive propositions to the content of the propositions themselves. Here's another way to put this point: while both Sosa and Bealer agree that all intuitions are intellectual seemings, Bealer also (implicitly) asserts the converse whereas Sosa denies it.

On the other hand, one could also argue that Bealer's account is more rigid than Sosa's, since the former requires a phenomenological feature that the latter does not. While Bealer argues that (rational) intuitions must present themselves as necessary, Sosa's account makes no such requirement.[9]

What Bealer and Sosa do agree on is that intuitions are sources of evidence for philosophical claims. However, Sosa adds the caveat that this is not because they are foundational in the sense of being "beyond justification and unjustification." (Sosa, 2007: 55) Rather, it is only *rational* intuitions with which we can be justified in placing our epistemic trust.[10] The final result is a more

[8] "We have found intuitions to be best understood as intellectual seemings or attractions, and these, unlike visual experiences, are not factors *that attract us to assent* (whether the *facie* be *prima* or *ultima*). They are rather the attractions themselves. When such attraction is exerted by one's entertaining a proposition, with its specific content, then the attraction is intuitive. But the entertaining is not the intuition, not what is distinctively characteristic of *intuitive* justification. … What is distinctive of intuitive justification is rather *its being the entertaining itself of that specific content that exerts the attraction.* So, intuitions are attractions of a certain sort, with no rational basis beyond the conscious grasp of its specific propositional content." (ibid, 54, emphasis is author's)

[9] This is most likely due to Sosa's dim view of modal reliabilism as made apparent in Sosa (1996).

[10] "S *rationally* intuits that p if and only if S's intuitive attraction to assent to <p> is explained by a

conservative view of the usefulness of intuitions, but an optimistic view nonetheless.

Timothy Williamson

A contemporary account of intuitions that contrasts with Bealer's and Sosa's can be found in Timothy Williamson's latest book *The Philosophy of Philosophy*. In it, Williamson argues against intuitions being intellectual seemings on the grounds that, "intellectual seemings typically lack the rich phenomenology of perceptual seemings." (Williamson, 2009: 217) Conducting some of his own auto-phenomenology, Williamson writes,

> For myself, I am aware of no intellectual seeming beyond my conscious inclination to believe [for example] the Gettier proposition. Similarly, I am aware of no intellectual seeming beyond my conscious inclination to believe Naive Comprehension, which I resist because I know better. (ibid)

Ultimately, Williamson agrees with David Lewis' view of intuitions, namely that, "Our "intuitions" are simply opinions" (Lewis, 1983: x).[11] However, a great deal of what other philosophers have taken to be intuitions (e.g., that the subjects in Gettier cases do not have knowledge) Williamson takes to be modal judgements. This is entailed by Williamson's account of thought experiments, in which he concludes that, "Paradigmatic thought-experiments in philosophy are

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competence (an epistemic ability or virtue) on the part of S to discriminate, among contents that he understands well enough, the true from the false, in some subfield of the modally strong (for the necessarily true or necessarily false), with no reliance on introspection, perception, memory, testimony, or inference (no further reliance, anyhow, than any required for so much as understanding the given proposition)." (ibid, 61)

[11] Interestingly, in the same passage, Lewis concedes that although "Philosophical theories are never refuted conclusively. … Gödel and Gettier may have done it." (ibid) It's unlikely that anyone would charge Gödel with intuition pumping, but Gettier is supposed to be the paradigm case of the use of intuitions in philosophy.

simply valid arguments about counter-factual possibilities." (ibid, 207) Our capacity to construct and evaluate such arguments properly is itself vindicated by the adroitness of our counter-factual thinking, the roots of which are traced to our interactions with a spatio-temporal world. "To deny that such thinking ever yields knowledge is to fall into an extravagant skepticism." (ibid, 178)

Two morals can be drawn from these claims. There is a dialectical point that what other philosophers take to be "intuitions" (and what Williamson takes to be modal judgements) can and should be used in philosophical inquiry.[12] But there is also a rhetorical point that philosophy is probably better off avoiding the term 'intuition' and its cognates, since "Their main current function is not to answer questions about the nature of the evidence on offer but to fudge them, by appearing to provide answers without really doing so." (ibid, 220)

Those discussed in this section are not the only contemporary analytic philosophers who have written on intuitions. Indeed, there have been two separately published collected works on intuition[13] in addition to journal articles and books by both venerable[14] and up-and-coming[15] analytic philosophers.

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[12] Because he appears to regard only "intuitions" from thought-experiments as epistemologically respectable, Williamson falls under Joel Pust's "particularist intuitionism" which "treat[s] only particular case intuitions as evidence" (Pust, 12) The alternatives are "generalist intuitioinsm" which "treat[s] only general intuitions [e.g., the intuition that consequentialism is the correct moral theory] as evidence" (ibid) and "global intuitionism" which "treat[s] *all* intuitions, general or particular, as *prima facie* evidence." (ibid, emphasis is author's)

[13] DePaul & Ramsey (1998) and Davis-Floyd & Arvidson (1997) feature not only philosophical accounts of intuitions, but psychological and sociological accounts as well.

[14] Hintikka (1999) is an account that lies somewhere between Cummins' and Weinberg. Like Weinberg, he argues that the historical failures of intuitions makes their evidential worth dubious, but he also raises a calibration objection similar to Cummins and as such draws a pessimistic (and cautionary) conclusion regarding the use of intuitions in philosophy.

[15] Pust (2000) offers an expansive survey of the use of intuitions in philosophy, though it heavily

Nevertheless, the views discussed above make for a more than adequate cross-section of the recent literature on intuitions, providing a clear view of the tension between restrictionist and analytic understandings of intuitions.

1.4 Restrictionist Accounts of Intuitions

In contrast to the historical and contemporary accounts discussed in the previous two sections, the restrictionist account of intuitions is rather impoverished. In another paper by Nichols, Stich and Weinberg, 'intuition' is defined as "simply a spontaneous judgement about the truth or falsity of a proposition—a judgement for which the person making the judgement may be able to offer little or no further justification."[16] (Nichols et al., 246, n. 3) 'Intuition' has also been defined negatively (i.e., rather than stating what they are, we are told what intuitions are *not*) as in Weinberg (2007), where intuitions are described as,

> intellectual happenings in which it seems to us that something is the case without arising from our inferring it from any reasons that it is so, or our sensorily perceiving that it is so, or our having a sense of remembering that it is so. (Weinberg, 318)

Curiously, explicit definitions of 'intuition' are often lacking in restrictionist papers, possibly on the assumption that what the restrictionists' experiments probe

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favours Bealer's account of intuition and has the misfortune of being completed shortly before experimental philosophy (and hence intuitions) became a central topic of concern in analytic philosophy.

[16] This definition can be traced back twenty years to Stich (1983) in which he writes, "Intuitions are simply spontaneous judgements." (p. 51) Interestingly, Stich's view of intuitions then is far more optimistic than now: "Despite their fallibility, however, intuitions are often the best and most systematic evidence available in determining the contours of a folk concept. In the absence of an argument that intuitions in some domain are particularly likely to be mistaken or misled, it would be folly to ignore them." (p. 52)

is identical to whatever analytic philosophers take intuitions to be. No account of intuition is offered in either Swain et al. or Mallon et al., though in both cases it is implicitly assumed that intuitions are elicited by thought-experiments.[17] In fact, we can safely assume that this assumption is made in all extant restrictionist literature, since all of the restrictionist probes created thus far have been modelled on famous philosophical thought-experiments[18] and the conclusions drawn on the basis of the results of these probes are made with reference to intuitions.

It is odd that the above constitutes the entirety of restrictionism's account of intuitions, considering how concerned its practitioners are with critiquing the use of intuitions as evidence. However, since restrictionists model their intuition probes on thought-experiments and since, *pace* Williamson, thought-experiments have generally been assumed to elicit intuitions, restrictionists can be confident that they are testing intuitions, even if their account of what intuitions *are* is less than robust.

1.5 An Alternative Account of Intuitions

So what are intuitions? Seemings? Opinions? Judgements? As far as I'm concerned, specifying the particular mental kind to which intuitions belong is

[17] "Purportedly, if we consider [the Truetemp] case, we will have the intuition that Mr. Truetemp does not know that it is 104 degrees." (Swain et al., 140) "Thus, the common wisdom in philosophy is that Kripke (1972/1980) has refuted the traditional descriptivist theories of reference by producing some famous stories which elicit intuitions that are inconsistent with these theories." (Machery et al., 47)

[18] The exception is the Coin Flip Case used in Nichols et al. (2003) and Weinberg et al. (2008). However, as a hypothetical scenario from which one is asked to draw a conclusion should still meet even the most stringent definition of 'thought-experiment'.

beside the point. Williamson was right:

> In general, the objection "That's only an intuition" is ill-posed in the same way as the objection "That's only a judgement." Some judgements are indeed objectionable, but the mere fact that a proposition is judged is not even a *prima facie* reason for doubting it. (Williamson, 220)

It doesn't matter whether intuitions are seemings, opinions or judgements. What matters is whether or not they are epistemically useful. If form really does follow from function, then the proper approach is to figure out the role intuitions play in philosophy and worry about their taxonomy after having determined their epistemic status. In light of the evident controversy over the nature of intuitions, trying to defend or critique intuitions by first subsuming them under some other class of mental phenomena is far too roundabout.

A good starting point for investigating the function of intuitions is to consider where they most commonly feature in philosophical discourse and on this point both sides agree: thought-experiments. [19] It is no coincidence that thought-experiments are often pejoratively referred to as "intuition pumps".[20] However, this term is something of a misnomer, since the intuitions in intuition pumps are more a means than an end; thought-experiments may "pump" particular intuitions, but only with the aim of establishing a conclusion that tends to be much more general.

So, for example, that the subject in a Gettier case does not have

[19] Granted, this is not the case for Goodman and Goldman as discussed in the previous chapter. However, both have made caveats to the effect that intuitions should not be considered conclusive. Neither Gettier nor Kripke made such caveats regarding their own thought-experiments and for this reason it is these particular (rather than general) intuitions on which I will focus.
[20] See Dennett (1980, 1984) and Dennett & Hofstadter (1981)

knowledge is the intuition pumped to establish that knowledge is not (merely) justified true belief. The same can be said for Kripke (1972/1980), where the intuition that 'Gödel' does not refer to Schmidt is pumped in order to demonstrate that description (alone) is insufficient for reference. So too for Lehrer's Truetemp case: the pumped intuition that Mr. Truetemp does not have knowledge makes evident the inadequacy of reliabilism.

The preceding examples also illustrate a feature common to all of the thought-experiments that have thus far been of interest to restrictionism. All of the thought-experiments on which restrictionists have modelled their intuition probes are of a class which James Robert Brown (1991, 1996/2007) calls "destructive", meaning that they are intended to undermine a theory. These are contrasted with "constructive" thought-experiments, which are intended to establish a theory. [21]

I think the majority of thought-experiments (at least in philosophy) are of the destructive sort, but I have not conducted a literary review of sufficient scope to be wholly confident in this claim. It might also be objected that one theory's destructive thought-experiment is another's constructive thought-experiment (e.g., Kripke's Gödel case is intended to establish the causal-historical account of reference, though Kripke himself has denied this) and so the distinction borrowed from Brown is a false one. But neither of these points worries me because our focus here is on the particular thought-experiments at play in the analytic/restrictionist debate and these are generally taken by restrictionists to be

[21] It should be noted that Brown's focus is on thought experiments in science rather than in philosophy.

destructive.[22] It's also worth pointing out that a thought-experiment that is destructive for one theory is unlikely to be constructive for another theory unless it involves a suggested alternative. Jackson's Mary seems just as constructive for dualism as it is for non-reductive materialism.

To recap: the majority of intuitions restrictionists are interested in feature in destructive thought-experiments. Does this give an indication of whether or not intuitions are epistemically useful? I think it does. When considering any intuition pump, we can always ask ourselves *why* it is successful; why we have the particular intuition that we do. The restrictionists may be right in some cases: I think that Gettier subjects lack knowledge because of my enculturation or because of some rhetorical or psychological trick in Gettier's thought-experiment. But I may also have the Gettier intuition because there's something right about his conclusion; perhaps there is a flaw in the justified, true belief theory of knowledge and the Gettier thought-experiment simply made me aware of this fact.[23] Indeed, that so many epistemologists had the Gettier intuition despite their (at least tacit) endorsement of the JTB theory of knowledge suggests that there must be *something* significant about Gettier scenarios. One would think that if our intuitions are just acculturated by-products then the majority of epistemologists should have resisted the Gettier cases straight away.

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[22] "Keith Lehrer's Truetemp Case is standardly appealed to as an argument *against* reliabilism." (Swain et al., 139, emphasis added) "Thus, the common wisdom in philosophy is that Kripke has *refuted* the traditional descriptivist theories of reference by producing some famous stories which elicit intuitions that are inconsistent with those theories." (Machery et al., 47, emphasis added)

[23] Though it does not tell us what that flaw *is*. But realizing that we *are* mistaken is the first step toward correcting the mistake.

This is the reason the destructive nature of thought-experiments is relevant. In cases like those of Gettier and Kripke, philosophers should seriously consider why their intuitions run contrary to their theories. Are they culturally biased, guilty of holding inconsistent beliefs or is there some other explanation? Thus, if nothing else, intuitions pumped by destructive thought-experiments can be therapeutic: helping to ferret out inconsistencies and biases in theories—or at least in theorists—ultimately leading to greater and greater degrees of consistency.[24]

Of course, consistency is not a sufficient condition for truth, but inconsistency *is* sufficient for falsity. The hard part, the part that falls squarely within the purview of analytic philosophy, is figuring out where exactly the inconsistency lies and then how to go about resolving it. Hence, a widely-shared intuition from a destructive thought-experiment gives us, at minimum, reason to suspect the consistency of theorists and thus the correctness of their theory. If we want to avoid falsehoods in addition to pursuing truths, intuitions are ideal tools for doing so.

1.6 Conclusion

Intuitions have had a long and largely venerable history in philosophy and there is no reason they cannot continue to serve an important, albeit diminished, function. The account of intuition I have sketched is far more conservative than

[24] This may sound somewhat like reflective equilibrium as discussed in the previous chapter. However, the sort of process I am envisioning is continuous, rather than being aimed at some stable endpoint.

any of the analytic accounts discussed in this chapter but it is also far less vulnerable to restrictionist critique. I grant that intuitions alone are insufficient indicators of truth, but it is the bold philosopher indeed who would claim "Intuitively *x*, therefore *x*."[25]

Rather than serving as premises in arguments, intuitions can make for excellent indicators of bias and inconsistency and thus help us to avoid falsehoods, which helps us to attain truths. The only remaining question is how to determine the source of a given intuition: whether it is the result of enculturation or cognitive error or something deeper. There are most likely many ways of doing so, but, as I shall argue in the next and final chapter, intuition probing of the kind thus far practised by experimental restrictionists is a poor candidate.

[25] This is arguably what Descartes does with the *cogito* and doing so produced no end of trouble for him. For an excellent discussion of the *cogito* as an intuition see (Sarkar, 2007)

CHAPTER THREE: The Methodology of Experimental Restrictionism

"Aw people can come up with statistics to prove anything, Kent. 14% of all people know that."
-Homer Simpson

1.1 Introduction

The preceding chapters have focused on experimental restrictionism's characterization of analytic philosophy, its arguments against the evidential value of intuitions and its account of intuitions themselves; in other words, on what makes experimental restrictionism philosophical. But what sets ER apart from other projects in philosophy is its methodology; in other words, what makes it experimental. No critique of the restrictionist project would be complete without a detailed look at its empirical practises. This is the subject of the third and final chapter.

We begin with an overview of intuition probing, followed by an in-depth examination of the experiments documented in the three central ER papers discussed in chapter one. The penultimate section outlines several significant methodological critiques of experimental philosophy in general and the restrictionist responses to them. As will become apparent, a successful methodological criticism of restrictionism must be one that holds regardless of increasing methodological sophistication. In the final section, I offer an argument which I take to satisfy this criterion by illuminating a presupposition in

restrictionism's methodology that continues the leitmotif of the previous chapters, namely, that intuition constitutes a natural mental kind. I argue that this presupposition on the part of restrictionism's methodology is both illegitimate and inescapable.

An Introduction to Intuition Probing

With only a few exceptions, what unites experimental philosophers is their method of using "intuition probes" in order to test the intuitions of their experimental subjects. Put simply, intuition probes are thought-experiments administered to experimental subjects in the form of survey questionnaires. These are modelled on thought-experiments from philosophical literature and are intended to be as close as possible to their source material, though some changes are made in order to take account of subjects' lack of philosophical background. Specific details of the probes discussed below can be found in the appendix, though in some cases, these are less detailed than one would prefer.

In most cases, the subjects of experimental restrictionist research have been drawn from various undergraduate courses. The exception to this in the cases discussed below is the subjects of the experiment comparing the intuitions of high and low socio-economic status groups conducted by Weinberg et al. The subjects in this experiment,

> were approached near various commercial venues in downtown New Brunswick, New Jersey, and (since folks approached on the street tend to be rather less compliant than university undergraduates in classrooms) they were offered McDonald's gift certificates worth a few dollars if they agreed to participate in [the] study. (Weinberg et al., 33)

Subjects are also typically given a demographic questionnaire intended to establish one or more of the following: age, gender, sex, ethnic identity, and/or education. It's worth noting that in some cases a subject's education is surveyed as an indication of another variable being tested, i.e., socio-economic status (we are told in Weinberg et al. that this follows "much other research in social psychology"[1]), and in other cases it is the subjects' education itself that is the relevant variable.

The procedures involved in the distribution of these surveys is rarely detailed, presumably on the assumption that the restrictionists' audience is more interested the results of ER studies and the conclusions drawn from these results than in all the gory details of their proccdures. One exception to this trend can be found in the most recent of the papers discussed, where in a footnote Swain et al. explains that,

> Researchers visited 12 undergraduate classes to recruit students for the survey. At the end of each class, the researcher(s) addressed the students, explaining the survey and inviting them to participate. Students were given randomly assigned versions of the survey. Students who chose to participate turned in their completed surveys as they left the room. (Swain et al., 141, n.7)

Although this is the most comprehensive account of method offered by any of the three central ER papers, there remain a number of unanswered questions. How was the class introduced to the researcher(s)? What level of detail did the explanation of the survey involve? How were the students monitored during the administration of the survey? In the absence of a more detailed account, our best

[1] "subjects were classified as low SES if they reported that they had never attended college . Subjects who reported that they had one or more years of college were coded as high SES." (Weinberg et al., 33)

option is to examine specific cases of the restrictionist method in action. It is that examination to which we now turn.

1.1 Three Examples of ER Methodology

Normativity and Epistemic Intuitions

Of the three ER papers with which we are concerned, *Normativity and Epistemic Intuitions* has by far the largest number of intuition probes and hence the largest number of reported results. In each experiment Weinberg et al. divided their subjects into two groups on the basis of either ethnicity or socio-economic status. It should be noted that in each experiment, subjects were given only two options in responding to the epistemic intuition probes: Really Knows or Only Believes.

The first set of experiments was designed to determine whether Western (W) and East Asian (EA) subjects' epistemic intuitions differed on the matter of epistemic internalism versus epistemic externalism. Internalism is defined by Weinberg et al. as, "the view that *only* factors within an agent's introspective grasp can be relevant to whether the agent's beliefs have [some epistemically evaluable] property." (Weinberg et al., 26) Factors beyond the scope of an agent's introspection, e.g., the reliability of the psychological mechanisms that produce an agent's beliefs are epistemically external to the agent and are therefore discounted as relevant by internalism. The first probe discussed by Weinberg et al. is modelled on Lehrer's Truetemp case, though the mechanism of the tempucomp is

replaced by a falling rock which, upon striking the head of the agent, rewires his brain to produce accurate temperature-beliefs.[2] Since the mechanism in this case is reliable, Weinberg et al.aver that, "to the extent that a subject population is unwilling to attribute knowledge in this case, we have evidence that the groups 'folk epistemology' may be internalist."

The results reported by Weinberg et al. are that while the majority in both groups denied that the agent had knowledge, East Asian subjects were much more likely to deny knowledge than Western subjects. In attempting to find a formulation of the Truetemp case in which a difference would not appear between the two groups, Weinberg et al. found that by replacing the rock with a socially sanctioned intervention, the significant difference between the groups did indeed disappear.[3] The final reformulation of the Truetemp case involved positing a reliable mechanism shared by an entire community but to which no introspective access was available.[4] As in the previous case, there was no significant difference between Western and East Asian subjects.

In a second set of experiments, Weinberg et al. tested and compared the intuitions of Western and East Asian subjects regarding various Gettier cases. Based on findings by Norenzayan and Nisbett, Weinberg et al. hypothesized that EAs would be less inclined than Ws to deny knowledge to the agents of Gettier cases.[5] Based on the results of an experiment involving a modified Gettier case,

[2] See Appendix 1.1
[3] See Appendix 1.2
[4] See Appendix 1.3
[5] Norenzayan and Nisbett found that "EAs are more inclined than Ws to make categorical

they concluded that their hypothesis was correct.[6] Though the majority of Western subjects had the same intuition about the Gettier case that most philosophers have had, the majority of East Asian subjects had the opposite intuition, namely that the agent in the Gettier case really does have knowledge.

The intuitions of subjects who identified as being from the Indian subcontinent (SC) were also compared to the intuitions of Westerners regarding the Gettier case. It turned out that the difference between the intuitions of SCs and the intuitions of Ws was even more dramatic than the difference between the intuitions of Ws and EAs.[7] Encouraged by this result, Weinberg et al. compared the intuitions of SCs to those of Ws on two cases which did not yield statistically significant differences between Ws and EAs.[8] In both cases, a statistically significant difference was found.[9]

In another pair of experiments, Weinberg et al. investigated the role subjects' socio-economic status has on their intuitions. The two probes used in these experiments were similar to those which yielded statistically significant differences in the comparison of the intuitions of Ws and SCs, but no differences between Ws and EAs. Based on research on moral intuition conducted by

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judgements on the basis of similarity. Ws, on the other hand, are more disposed to focus on causation in describing the world and classifying things." (Norenzayan & Nisbett, 135)

[6] See Appendix 1.4

[7] See Appendix 1.5

[8] Considering the professed scope of their results (which has a tendency to contract or expand depending on whether they are on the defensive or not), it is odd that the restrictionists made nothing of the fact that Western and East Asian intuitions did not significantly diverge on a probe modeled after Dretske's zebra case (Dretske,1970). Considering how similar Dretske's zebra case is to the fake barn cases, these particular results appear to confirm Jackson's hypothesis that intuitions for fake barn cases are less variable than those for Gettier cases.

[9] See Appendix 1.6

Jonathan Haidt, Weinberg et al. predicted that low SES subjects would be less sensitive to counter-factual conditions than high SES subjects. Their results seem to confirm this hypothesis, indicating statistically significant differences between the intuitions of high and low SES subjects.[10]

The final experiment discussed by Weinberg et al. was designed to detect whether subjects were inclined to treat mere subjective certainty as a case of knowledge.[11] The results they report indicated that subjects in all of the groups discussed above (Westerners, East Asians, people from the Indian Subcontinent and people of high and low socio-economic status) were equally unwilling to attribute knowledge to an agent who is only subjectively certain.

The majority of these findings indicate exactly the sorts of conclusions for which experimental restrictionists tend to argue. Although the intuitions of analytic philosophers tend to converge with ethnically Western subjects of high socio-economic status, they tend to diverge from the intuitions of East Asians, people of the Indian Subcontinent and people of low socio-economic status.

Running Hot and Cold on Truetemp

The experimental research conducted by Swain et al. involved only one experiment in which one of eight different versions of a survey were randomly assigned to each of their 220 subjects. The only difference between these eight versions was the order in which they presented the same four thought-

[10] See Appendix 1.7
[11] See Appendix 1.8

experiments.[12] Each thought-experiment involved an agent forming a true belief and in each case subjects were asked to give their response to an attribution of knowledge to the agent using a Likert Scale.[13]The thought-experiment with which the researchers were most concerned was modelled on Lehrer's Truetemp case. The other three were, "a clear case of non-knowledge, a clear case of knowledge, and a Ginet-style [i.e., standard] fake barn case." (Swain et al., 142)

The primary reason for including these other cases was the experimenter's hypothesis that subjects who were first presented with the clear case of non-knowledge would be more likely to attribute knowledge in the Truetemp case than those who received the Truetemp case first, since it would "seem more plausibly a case of knowledge by comparison." (ibid) The researchers had the converse expectation of subjects first presented with the clear case of knowledge. The fake barn case was presented in order to determine "the effects of presenting a mixed case before the Truetemp Case." (Swain et al., 143)

According to Swain et al., all three of their hypotheses were confirmed in the analysis of the collected data.[14]Moreover, the researchers not only found that subjects were more willing to attribute knowledge to the agent in the Truetemp case after reading the non-knowledge case, and less-so after reading the clear-knowledge case, but that subjects were generally unwilling to attribute knowledge to the agent in the Truetemp case when it was presented first. It should also be

[12] See Appendix 2.1
[13] The Likert Scale is the most widely used scale in survey research. In asking subjects to rate their agreement or disagreement with a statement, the scale provides five familiar options: "Strongly Disagree", "Disagree", "Neither Agree nor Disagree", "Agree" and "Strongly Agree".
[14] See Appendix 2.2

pointed out that in the fake barn case, Swain et al. found that, "subjects intuitions about this case were, given the Truetemp Case's lability, surprisingly stable across presentation position."[15] (Swain et al., 146)

The authors suggest that their findings raise a question about which, if any, intuitions are resistant to the effects of irrelevant factors. They also assert that, "any attempt to answer this question will need to rely on empirical research about intuitions. There is no *a priori* way to figure which intuitions are stable." (ibid) Hence, the implicit claim is that the research conducted by Swain et al. not only poses a threat to the intuitions elicited by the Truetemp Case, but also to the use of intuitions in philosophy in general. Moreover, their assertion seems to be that in order to justifiably use intuitions at all, analytic philosophers *need* empirical evidence that confirms the viability of the intuitions being used.

Semantics, Cross-Cultural Style

Based on the research conducted by Richard Nisbett and his collaborators into the effects enculturation has on cognitive processes, Machery et al. hypothesized that Westerners would be more likely than the Chinese to have intuitions that square with the causal-historical theory of reference. This is due to Nisbett's findings that Westerners are more inclined to base their judgements on

[15] However, it should be noted that the counter-factual stipulation that the subject would have made the same judgement had she been looking at a fake barn was absent from the relevant probe. It is suggested that this demonstrates "the potential for experimental philosophy to identify which aspects of thought-experiments are most salient." (ibid, 146, n.19) For my part, the saliency of the counter-factual stipulation in the fake barn cases has always been apparent. I imagine most epistemologists would agree.

causal factors, whereas East Asians are more inclined to base their judgements on factors relating to similarity.

The sample size used in Machery et al. is considerably smaller than those of Weinberg et al. or Swain et al.. A total 82 subjects were initially polled, though 11 were excluded from the experiment based on the results of the researcher's "standard demographics instrument" (Machery et al., 51).[16] Presumably, this is the same instrument used by Weinberg et al., i.e., an ethnic identification questionnaire. The subjects were presented with four intuition probes, "counterbalanced for order" in a classroom setting and in all cases the probes were presented in English.[17] (ibid) All of the subjects were identified as fluent English speakers. Two of the probes were modelled on Kripke's Gödel Case and two on his Jonah Case.[18]The two probes given to East Asian participants (one Gödel-type and one Jonah-type) featured names with which they would be familiar. The Gödel-type probe given to Western subjects was "closely modelled on Kripke's own example." (ibid)

The analysis of the results of these surveys yielded a significant difference

[16] The subjects were drawn from Rutgers University, representing Westerners, and The University of Hong Kong, representing the Chinese. Nine participants were excluded from the Western group and one from the East Asian Group, leaving 31 and 41 subjects in each group respectively.
[17] See Appendix 3.1
[18] The Jonah Case is described in Kripke (1972/1980) as follows: "Suppose that someone says that no prophet ever was swallowed by a big fish or a whale. Does it follow, on that basis, that Jonah did not exist? There still seems to be the question whether the Biblical account is a legendary account of no person or a legendary account built on a real person. In the latter case, it's only natural to say that, though Jonah did exist, no one did the things commonly related to him. I choose this case because while Biblical scholars generally hold that Jonah did exist, the account not only of his being swallowed by a big fish but even going to Nineveh to preach or anything else that is said in the Biblical story is assumed to be substantially false. But nevertheless there are reasons for thinking that was about a real prophet." (Kripke, 67)

in the responses of Western and Chinese subjects to the Gödel Cases.[19] Western subjects were more likely to have intuitions consistent with Kripke (and analytic philosophers in general) whereas Chinese subjects were not. However, no such difference was found in the responses to the Jonah Cases. From these experiments, Machery et al. conclude that the intuitions elicited by Kripke's thought-experiments are culturally variable. The intuitions of the subjects are characterized as being causal-historical, in the case of Westerner subjects and descriptivist in the case of Chinese subjects. Regarding the lack of difference in the Jonah Case, Machery et al. suggest that,

> Setting out the Jonah cases precisely requires a lengthy presentation, so it is possible that our probes were simply too long and complex to generate interpretable data. Another, more interesting possibility hinges on the fact that in the Jonah cases, the descriptivist response is that the speaker's term fails to refer. It might be that for pragmatic reasons, both the Westerners and the Chinese reject the uncharitable interpretation that the speaker is not talking about anyone.[20] (ibid, 52)

1.2 - Past Methodological Objections

Though the results of the studies discussed above have never been considered decisive, they are generally taken to provide *prima facie* evidence that the intuitions of philosophers are not reliable indicators of truth since they vary with respect to factors that analytic philosophers have, for the most part, taken to be irrelevant. There have been many objections raised against the methods used to reach this conclusion. In what follows, I outline the methodological criticisms

[19] See Appendix 3.2

[20] Much like the EA responses to the Zebra and Cancer Conspiracy cases from WNS, this seems to be another instance of restrictionists emphasizing the results which confirm their hypothesis while glossing over those which do not.

made by Ernest Sosa, Brian Scholl and Simon Cullen. These three criticisms thus provide perspectives from well-established philosophers (Sosa), experimental psychologists (Scholl) and the next generation in philosophy (Cullen).

Ernest Sosa – Linguistic versus Conceptual Divergence

As we saw in the previous chapter, Ernest Sosa's work on intuitions is older than the experimental philosophy movement itself. As such he has been a notable critic of experimental philosophy. His objection to the conclusions of restrictionism can be summed up in six words: "verbal disagreement need not be substantive" (Sosa 2007b, 100). Sosa elaborates on this objection by arguing that the results of restrictionist research only pertain to people's willingness or unwillingness to use certain ambiguous terms in particular contexts. If this is correct, then what experimentalists need is some justification for thinking that the apparent disagreement between their subjects is not merely the result of contextual or definitional factors. In other words, if I live in Toronto and you live in Beijing, our verbal disagreement about whether or not it is cold today need not, for contextual reasons, entail a conceptual or intuitive disagreement about coldness.

Elsewhere, Sosa has raised more specific concerns about the ambiguity of the implicit questions in the surveys conducted by Weinberg et al. For example, he points out that,

> When we read fiction we import a great deal that is not explicit in the text. We import a lot that is normally presupposed about the physical and social structure

> of the situation as we follow the author's lead in our own imaginative construction. And the same seems plausibly true about the hypothetical cases presented to our WNS subjects. (Sosa, 2009, 12)

Sosa's point here is that different subjects may be importing different assumptions into their readings of x-phi intuition probes. Indeed, the fact that the restrictionists' subjects have different cultural or socio-economic backgrounds would explain their importing different assumptions about the behaviours of the agents in the probes. Of course, this criticism would only pertain to the studies conducted by Weinberg et al. and Machery et al.

Sosa also conjectures that subjects only presented with two choices may simply lack the opportunity to express what they actually think about the agents in the restrictionists' probes. As an example of a third option, he suggests an answer similar to those often found on various aptitude tests: there is not enough information given to properly evaluate the agent. Again, this criticism does not seem to apply to the experiment conducted by Swain et al., since subjects were given the option of neither agreeing nor disagreeing.[21] Given the oddity of the scenarios presented, it would not be surprising if many subjects did choose this option.

[21] However, answering that one neither agrees nor disagrees with an attribution of knowledge is not the same as saying that it is not possible to determine whether or not the agent has knowledge. Indeed, it's hard to predict exactly what subjects would have said in response to Swain et al.'s probes if they really did think that it wasn't possible to determine whether or not the agents in the probes had knowledge since they were confronted an attribution of knowledge and asked to respond to it rather than to make or withhold the attribution themselves. They might "Strongly Disagree" because in these cases attributing knowledge to the agent was an unfounded assertion. Alternatively, they might "Neither agree nor disagree" because they were unsure of how to evaluate the attribution.

Brian Scholl – Four Methodological Challenges

In a brief lecture on the prospects for experimental philosophy, experimental psychologist Brian Scholl outlined four challenges which he takes to be endemic to the methods of experimental philosophers. Some of these are strikingly similar to the criticisms raised by Sosa. For example, Scholl argues that "Subjects often fail to interpret the questions in the way you would intend." (Scholl) Furthermore, he points out that since x-phi experiments have not often involved querying the subjects about *why* they answered the way they did, it's impossible to conclude whether and how often subjects really are misinterpreting the aims of the experiment. As evidence of this problem, he cites a recent study by Shaun Nichols in which Nichols queried his subjects about their answers and found that, "Of the 30 participants, 13 gave explanations that indicated that they failed to abide by the conditions of the thought experiment." (Nichols, 2006: 70). If it did turn out that almost half the participants in other studies had also failed to abide by the conditions of the thought-experiments presented, the restrictionist conclusions from these experiments would be highly suspect.

Again, much like Sosa, Scholl also raises concerns about contextual factors which may influence the answers given by experimental subjects. These factors would explain the diversity of the subjects' answers without impugning intuitions. His most interesting example of this is drawn from Norenzayan and Schwarz's "letterhead" studies, in which they found a consistent variation in subjects' answers depending on whether the letterhead at the top of the survey

said *Institute for Social Research* or *Institute for Personality Research* (Norenzayan & Schwarz 1999). This suggests that there may be a host of variables that experimentalists are not controlling for which could seriously undermine the validity of their conclusions.

In his third challenge, Scholl argues that it is extremely difficult to design survey studies that lack "some confounding 'surface' variable" (Scholl, 2008). We are asked to imagine being in the position of an experimental subject who has no intuition either way about a given case and then asked how we might respond:

> To always give the same answer to every probe would seem like you're being an uncooperative scoundrel. So you have to answer systematically in some way. But how to do that? Well, there is a single salient feature that is obviously being varied across the questions (within-subjects), so it seems natural to vary your response accord to the variable. (Scholl, 2008)

The point of this hypothetical case is clear: the subjects of experimental surveys may answer in such a way that they appear to validate the experimentalists' hypotheses *without actually validating it*. Moreover, Scholl advocates that asking the subjects to explain why the answered the way they did will not necessarily solve this problem, since their answers may simply point to the surface variable without indicating that they did or did not have the relevant intuitions.

Scholl's fourth challenge actually has the least relevance for experimental restrictionism. His challenge is that even if the survey methods of experimental philosophers manage to draw out intuitions, they are extremely unlikely to succeed in drawing out and isolating the cognitive processes that produced

them.[22] This is not so much a challenge to experimental restrictionism, since its goal is to demonstrate that intuitions themselves are unreliable, and this can still be reached even if the processes that generate intuitions are left undiscovered. However, this challenge does give an indication of Scholl's more general objection to experimental philosophy's methodology, namely that it has been self-identified as being borrowed from the methods of experimental psychology. In response, Scholl avers that "I haven't bothered to check, but my hunch is that if you picked up a random recent issue of each of the top experimental psychology journals, you'd find that <1% of the papers use any such methods." (Scholl) Though this does not amount to a direct objection to the methods experimental philosophers are using, it does suggest that they have at least been misrepresenting themselves.

Simon Cullen – Survey Driven Romanticism

In what is no doubt the most ambitious response to experimental philosophy's methodology, Simon Cullen has conducted a series of his own experiments in what might be described as an empirical *reductio ad absurdum.* For example, Cullen suggests that the different order of cases presented to Swain et al.'s experimental subjects—rather than eliciting different intuitions—elicited different interpretations of the researchers' intentions because they created different conversational contexts. Hence, his hypothesis that, "it should be

[22] Recall that this is the apparent goal of experimental descriptivism.

possible to substantially reduce the contrast effect [of presenting the cases in different order] by manipulating the conversational context in which subjects consider the cases." (Cullen, 7) This hypothesis was tested and *prima facie* confirmed by telling subjects explicitly that each case they were presented with should be considered independently of the others.[23]

Cullen also points out that in drawing their conclusions, Swain et al. (as well as many other x-phi's) have made the unjustified assumption that, "the context in which their philosophically innocent and experimentally naive subjects consider the Truetemp case is *in all relevant respects* identical to that in which philosophers consider Lehrer's Truetemp thought-experiment." (Cullen, 8, emphasis is author's) But, as Cullen points out, there are a number of significant differences between philosophers and experimental subjects, including their respective conversational contexts, how they unpack the ambiguity of relevant terms and the reasons behind formulating the thought-experiment in the first place (i.e., while philosophers understand that the point of Lehrer's case is that Mr. Truetemp lacks introspective access to reliably formed beliefs, experimental subjects may not).

Over eight months, Cullen replicated several of the restrictionist experiments discussed above using a much larger sample size in an effort to test their methodological soundness.[24] For the restrictionist, his results are startling, to say the least. For example, Cullen found significant differences depending on

[23] See Appendix 4.1
[24] See Appendix 4.2

whether subjects were given the option of answering questions dichotomously or not (i.e., choosing between "S does know that p" and "S doesn't know that p" vs. using a Likert scale in response to an ascription of knowledge) and even depending on the formulation of the dichotomy (i.e., S knows that p vs. S really knows that p).[25]

Two conclusions can be drawn from these results. First, they suggest that restrictionists have unjustifiably—and in some cases falsely—assumed that their subjects' intuitions can be read directly from their survey responses. Second, and more importantly, they are a first indication that the restrictionists' results may not be repeatable.[26] If that is the case, then the empirically-oriented restrictionist project is in serious trouble indeed.

1.3 Restrictionist Responses

Considering the severity of the above methodological critiques, it should not be surprising that many of restrictionism's best and brightest have come to its defence. Stephen Stich, for example, has responded to Sosa's critique in typical experimentalist manner. He grants that Sosa is correct in asserting that the experiments thus far conducted have not established intuitive disagreement beyond a reasonable doubt. However, he adds the caveat that x-phi is still in its nascency and that, "More and better studies are needed, including experiments that address the concerns that Sosa raises, and a variety of other concerns as

[25] See Appendix 4.3 and 4.4

[26] Barry Lam has recently conducted a study which casts doubt on the repeatability of the results of Machery et al. in particular. See Lam (2009)

well." (Stich, 2009, 223) In fact, it is rare to find an experimentalist response that doesn't involve advocating further empirical research.[27]

Stich's response to Sosa's point about an added question potentially invalidating the conclusions drawn by experimentalists is that although such evidence is possible, Sosa has not offered a reason to think that it is likely. For the moment, Stich claims, "these studies stand as noteworthy straws in the wind, and most of the straws seem to be blowing in the wrong direction for those who champion intuition-based philosophy." (ibid) The point here is that, at the moment, the *prima facie* evidence is on the restrictionists' side, not the analytics'.

Thus far, very little has been said in the x-phi literature in response to Scholl's criticisms. The most that has been said in defence of restrictionism specifically has come from Jonathan Weinberg's response to Scholl's challenges on the always active experimental philosophy blog. Though he agreed with Scholl's worry that surveys are unlikely to reveal the cognitive process that underlie intuitive judgements, Weinberg suggested that getting at these mechanisms is not always the aim in experimental philosophy. As an example, he points out that in the case of the restrictionists, it is sufficient to demonstrate that intuitive judgements are influenced by philosophically irrelevant factors; finding

[27] A similar point is made in Swain et al.'s reply to Sosa's objection that, "we have to be *careful* in how we use intuition, not that intuition is useless." (Sosa 2006, 105). Swain et al. respond with the claim that, at the moment, it's not clear what it means to be careful when using intuitions. In responding to Sosa, they conclude that, "it falls on those philosophers who wish to continue relying on intuitions to figure out what it means to be careful. We do not rule out *a priori* the possibility that they will be able to do so. What we rule out is the possibility that they will be able to do so *a priori*." (Swain et al., 149)

the causes behind these influences is extraneous to the restrictionist project.[28] Of course, whether subjects' intuitive judgements can be read off of their survey responses is a problem raised by all three critics discussed above.

The responses to Cullen's critique tend to rely on assumptions about where the burden of proof lies in the analytic/restrictionist debate. Cullen claims that it is up to experimental philosophers to demonstrate that the responses of their subjects are representative of the intuitions of philosophers while the experimentalists claim that it is up to Cullen to demonstrate that they are not.[29] Moderating this sort of debate is difficult, especially when both sides have a vested professional interest in the outcome. However both sides at least seem to agree that further empirical investigation is needed in order to reach a resolution.

1.4 - Intuition as a Natural Mental Kind

There is, I think, a general weakness in the methodology of experimental philosophy that has yet to be fully investigated. A broad criticism that Sosa, Scholl and Cullen all seem to make is that the differences between the subjects of restrictionist experiments and analytic philosophers are severe enough to warrant rejecting the restrictionists' conclusions about the intuitions of the latter group based on the responses from the former group. The reason this criticism has been raised by so many is that it points at an unjustified assumption that is crucial to

[28] For Weinberg's full response, see: http://experimentalphilosophy.typepad.com/experimental_philosophy/2008/08/scholl-on-exper.html?cid=127451718#comment-127451718

[29] No doubt the response to Sosa and Scholl on this point would be similar.

the success of any restrictionist project: that intuition constitutes a natural mental kind. But before I can elaborate on this claim, we must first consider natural kinds themselves.

Natural Kinds and Natural Mental Kinds

What are natural kinds? There is a certain irony to be found in the sheer number of answers to this question, since natural kinds can be most broadly defined as categories of particulars, the delineations of which *do not* depend on humans. The idea, as Hilary Kornblith has noted, is simply that there are kinds which are not imposed upon nature by us, but rather are already there, waiting to be discovered.[30]

Perhaps the best way to get a feel for natural kinds is to list some examples which are more or less uncontroversial. Water is the most often cited natural kind. The referents of the periodic table have also been taken to be natural kinds. Slightly more controversial than the natural kinds of chemistry are those of biology (e.g., species) and psychology (e.g., emotions). Note that in each case, the grouping together of particulars is in some sense non-arbitrary, i.e., the particulars which fall under a heading like *water* share more in common than those which fall under a heading like *was born on a Monday*. Most agree that *water* is a natural kind and that *things born on a Monday* is not.

Determining the correct criteria for natural kind-hood is a thesis topic in

[30] Whether we can discover natural kinds (on the assumption that they exist) has been a point of contention going at least as far back as Locke. See Kornblith (1993) for an excellent exigesis.

itself. To save space and to avoid objections over the legitimacy of the criteria employed below, it seems prudent to simply borrow the criteria from a leading experimental philosopher, Edouard Machery.[31] Here then is what Machery calls the causal notion of a natural kind:

> A class C of entities is a natural kind if and only if there is a large set of scientifically relevant properties such that C is the maximal class whose members tend to share these properties because of some causal mechanism. (Machery, 447-48)

This notion yields three independently necessary and jointly sufficient conditions for *x* to count as a natural kind. First, there must be a large[32] set of scientifically relevant[33] properties that the members of *x* tend to posses. The use of 'tend', according to Machery, allows that natural kind terms don't have to feature in laws but only in *ceteris paribus* generalizations. This permits entities like those of geology, climatology and psychology to qualify as natural kinds in addition to those of the more "basic" sciences like physics and chemistry. Second, the members of *x* must possess these properties because of some causal mechanism. This is meant to capture the fact that the properties shared by the members of *x* are not shared accidentally. It is also intended to allow that possessing essences[34] or underwriting laws[35] are sufficient, but not necessary

[31] Machery's criteria are themselves borrowed from Richard Boyd and Paul Griffiths. See Boyd (1991, 1999) and Griffiths (1997: chs. 6 and 7) It should be noted that this is only of many notions of a natural kind acknowledged by Machery. I use this one in particular because it is intended to be broad enough to apply to psychological kinds, as Machery's focus is on concepts.
[32] Machery avers that the vagueness of the term 'large' is unproblematic since he takes the notion of a natural kind to be itself vague, much like 'heap'. (Machery, 2009, 233)
[33] It's natural to wonder what 'scientifically relevant' means in this context. I must admit I am not entirely certain. However, it is no cause for concern since I am only interested in borrowing Machery's criteria, not analyzing it in depth. For our purposes, it is enough to assume that 'scientifically relevant' means something like "pertaining to scientific research."
[34] As in Locke ([1690] 1997), Kripke (1972/1980) and Putnam (1975)

conditions for natural kind-hood. Finally, the set of properties must be specific to the members of *x*. In other words, for *x* to be a natural kind, it must not be a subset of a larger class to which all of the same properties apply. This requirement restricts the number of possible natural kinds: it rules out a class like *brown dogs* since it can be subsumed under *dogs* without ignoring any scientifically relevant properties.

Does Restrictionism take Intuition to be a Natural Kind?

As the preceding paragraphs suggest, natural kinds are typically taken to be the primary subject matter of empirical investigation. Hence, empirical success can be measured in part by the discovery of natural kinds and the formation of laws—or at least *ceteris paribus* generalizations—about them.

This explains why restrictionism makes the presupposition that intuition is a natural kind. For the results of their investigations to be generalizable (if not nomologically then at least with *ceteris paribus* clauses), the subject matter of the restrictionists' studies must be a natural kind. The only way that restrictionism can make a legitimate inference from what they study in the laboratory to what philosophers appeal to in the armchair is under the assumption that the subject of their study is the very same subject of the philosophers' appeals. In other words, the restrictionist thesis depends on the assumption that laboratory intuitions are *of the same kind* as armchair intuitions.

[35] Cf. Fodor (1974)

Now, one might object that rather than taking intuitions to be a natural kind, restrictionists could just as easily take them to be a non-natural kind and still make inductive generalizations about them, e.g., that they are cross-culturally variable. Such is the case with the non-natural kind that consists of things which have been worshipped as gods or of the one which consists of mothers-in-law. But notice the disanalogy between intuitions and these other examples: whether locally or globally, what constitutes a thing worshipped as a god or what constitutes a mother-in-law is a matter of convention, and it is only after the convention has been made that empirical investigation can begin.

The same cannot be said for intuitions. As understood by restrictionists, intuition is a psychological phenomenon, hence their emphasis on using the methods of experimental psychology to probe intuitions. With intuitions understood as psychological phenomena, no convention is needed prior to conducting an empirical investigation. Resisting this characterization and claiming that intuitions are in some sense conventional reduces the analytic/restrictionist debate to a quibble over the proper definition of 'intuition'; a shift that would be unproductive for both sides and irrelevant to the matter at hand. We decide what the word 'intuition' means, but what intuitions are must be discovered. Taking an empirical tack at such a task (a la restrictionism) requires the presupposition that intuitions are a natural kind.

Is Intuition a Natural Mental Kind?

Having established that restrictionism must presuppose that intuition is a natural mental kind, we must now determine whether or not this is a legitimate presupposition for restrictionists to make. The three jointly sufficient conditions of natural kind-hood borrowed from Machery give us three options for rebuttal: (i) we can deny that the members of *x* have a large set of scientifically relevant properties in common, (ii) we can argue that the members of *x* only have these properties accidentally, or (iii) we can argue that the members of *x can* be subsumed under a larger class *y* which is itself a natural kind.

But before attempting any of these strategies, it's worth noting that there is another way to rebuke the claim that intuition is a natural kind. Because it is a mental kind, one can deny that intuition is a natural kind by denying that there are any natural mental kinds at all. [36] However, this requires that one adopt an eliminativist position, and using eliminativism in this way—as a sort of blunt instrument against experimental philosophy—strikes me as rather crude. Whether or not eliminativism is true is still a live question in the philosophy of mind and adopting a contentious theory in order to critique a methodology seems far too roundabout. Nevertheless, if one already *is* a committed eliminativist, dismissing intuition qua mental kind as a natural kind is an option.

With the easy objection set aside, we can now turn to the three options for rebutting the implicit claim that intuition is a natural kind. In fact, I think intuition does not meet any of the three conditions outlined by Machery. What set of

[36] Note that this strategy must assume a different notion of natural kinds than Machery.

scientifically relevant properties do intuitions have in common? There seems to be a general consensus on both sides of the analytic/restrictionist debate that intuitions share certain phenomenal properties: feelings of intellectual attraction or assurance, for example. But phenomenal properties are not scientifically relevant; indeed that was the whole point of Nagel's famous thought-experiment.[37] Introspective opacity is a more promising candidate for a scientifically relevant property of intuition, especially in light of recent work in cognitive science.[38] However, just as one grain of sand does not a heap make, one scientifically relevant property does not make for a large set. In the absence of further scientifically relevant properties, intuition seems far too impoverished to constitute a natural kind under Machery's causal notion. Furthermore, the second of Machery's criteria, being parasitic on the first, will clearly not be satisfied by intuition.[39]

The final criterion discussed by Machery provides the most serious problem for intuition's candidacy as a natural kind. Recall that the experimentalist definition of intuition is roughly, "a spontaneous judgement with subjectively opaque origins". This definition clearly violates the third of Machery's criteria, since it entails that intuitions can be subsumed under a larger class which is itself a natural kind: judgement. This subsuming can only be objected to on the grounds

[37] Nagel (1974)
[38] See Jack & Roepstorff (2003)
[39] This redundancy may suggest a defect in Machery's causal notion, but again, I am only interested in borrowing it, not critiquing it. Even if Machery's criteria for natural kind-hood turn out to be faulty, I suspect that other, less liberal notions would also exclude intuitions as a natural mental kind.

that there are scientifically relevant properties that intuitions have and judgements lack, which, as the previous paragraph suggests, seems highly unlikely.

1.5 - Conclusion

The typical response to criticisms of restrictionism's methodology is to ask for more time. Restrictionists seem quite willing to grant that their conclusions have yet to be proven beyond a reasonable doubt while maintaining that their research provides *prima facie* evidence for them. To avoid this sort of brush off, a methodological criticism of restrictionism's methods must point to an inherent flaw in the practises of experimental restrictionism; one which cannot be extricated by developing a more sophisticated methodology. In this chapter, my aim has been to offer one such criticism. The empirical justification of restrictionism's central thesis rests on their implicit assumption that intuition is a natural kind and this assumption is wholly dubious. Fortunately for analytic philosophy, and as established in the previous chapter, no such assumption is needed.

CONCLUSION

Thus ends my analysis and critique of experimental restrictionism. At this point, it seems reasonable to take a step back and consider experimental restrictionism—and, for that matter, experimental philosophy as a whole—in relation to philosophy more broadly. I hope the reader will forgive the occasional hyperbole in what may follow.

As one of the oldest disciplines, philosophy has seen more paradigm shifts than most. In some cases, these shifts have radically altered the entire discipline: the pre-Socratic to the ancient, the medieval to the renaissance, the modern to the post-modern. These transitions were marked by striking changes in how philosophers conceived of their aims and their practices. But philosophy has also seen its fair share of false starts, or at least frustrated expectations. Despite its best efforts, the Vienna Circle did not do away with those nagging questions of metaphysics, nor did phenomenology tame conscious experience into something systematic and analyzable.

I grant that this paints the history of philosophy with brushstrokes far too broad. The illustration above is bound to be controversial; even Whitehead's famous remark that all of philosophy is but a footnote to Plato still has its adherents. Nevertheless, there is something that ties philosophers together beyond history or vocabulary. We must all agree that there have been major turning points in the history of our shared discipline, even if we differ on where and when they were. Does Experimental Philosophy represent another such

turning point; a new paradigm?[1]

While we might hope to be able to anticipate such drastic changes in philosophy, it seems more likely that these sorts of intellectual revolutions can only be seen in hindsight. Moreover, given its relative nascency, any predictions made about the future of x-phi are practically guaranteed to be speculative. But seeing as this is the conclusion of of my project, some speculation about experimental philosophy's future—based on its present course---seems warranted. I am afraid this is as close to a testable hypothesis as I can come.

To my mind, there are four plausible scenarios: two in which the x-phi research program succeeds and two in which it fails. Let's start the more pessimistic possibilities. Under the first, which I'll call *burnout*, x-phi gets undermined either by some ingenuous argument or (more likely) by a piece or pieces of incontrovertible empirical evidence. I take the latter to be a more likely source of refutation only because experimental philosophers are so empirically oriented that they tend to respond to compelling *a priori* arguments like Sosa's by demanding empirical support for them. The second pessimistic possibility I'll call *fade away*. In this scenario, x-phi enters something like a normal science phase, but lags there and is eventually discarded when it runs out of testable hypotheses or is slowly overwhelmed by unexplainable anomalies. It's difficult, probably

[1] The notion of paradigm at work here is somewhat looser than its Kuhnian counter-part. Strictly speaking, the only thing that unites all of the projects at work under the category of experimental philosophy is their methodology, which, as we have seen previously, is not itself unique to x-phi, being borrowed from social psychology. But there also seems to be a certain spirit that unites the experimental philosophers themselves, perhaps because of their extensive online community or as a result of a common threat in the form of criticisms that tend to lump all x-phi projects together.

impossible, to say how long such a scenario might take to play out. For the moment, there is a sufficient wealth of thought-experiments to be mined for years of intuition probes.

The fact that analytic thought-experiments have been the primary source for experimental intuition probes suggests the first of our optimistic scenarios. *Pace* restrictionism's hostility toward the analytic method of intuition pumping, it seems perfectly plausible that x-phi and a-phi could eventually develop a stable and mutually beneficial relationship of the kind envisioned by Knobe and Nichols, where:

> The philosopher on one end of the hall can be developing complex mathematical theories about the relevance of Bayesian inference to causal modeling, while the philosopher at the other end of the hall can be developing complex theories about how people's causal intuitions reveal some fundamental truth about human nature.[2] (Knobe and Nichols, 12)

I call this scenario *symbiosis*.

The final optimistic scenario for experimental philosophy's future would probably be seen as a pessimistic scenario for analytic philosophy's. In what would come close enough to a Kuhnian *paradigm shift* to be dubbed as such, x-phi would completely change philosophy's practises as well as its self-conception. This could also result in a radical revision of our view of philosophy's history. As the grandfather of restrictionism put it, philosophers in the new paradigm would probably conclude that "a great deal of what has gone on in the past [in

[2] This actually strikes me as a rather odd characterization of philosophy's future. The former philosopher's project sounds more mathematical than philosophical (but perhaps I simply don't know enough about Bayesianism) while the latter's sounds badly ad hoc. Surely we would prefer simple theories to complex ones. Perhaps my preference for simplicity is an unjustified intuition on my part, but at least in this case I am in good company. See Reber et al. (2008)

philosophy], belongs in the rubbish bin." (Stich, 2009: 6) Such is the fate of any past paradigm. Many would undoubtedly see this conclusion as liberating, but by my lights one of philosophy's greatest strengths is its strong connection—and in many cases its identification—with its roots.

At this stage, the realization of any of these four scenarios—*burn out, fade away, symbiosis,* or *paradigm shift*—is possible. However, the more moderate of the four: *fade away* and *symbiosis* strike me as the most likely. *Symbiosis* is just what the more positive projects in x-phi have advocated and *fade away* seems to be what many analytic philosophers are hoping for, at least with regard to the restrictionist project. To be honest I wouldn't bet money either way.

But amidst all the speculation and excitement surrounding experimental philosophy, and restrictionism in particular, there are certain familiar elements. There is the desire to make philosophy more empirically respectable; more scientific. One often sees expressed astonishment that there could be those who doubt the project's fecundity or its relevance. Above all, there is the hopeful feeling that an unwarranted orthodoxy has finally been dragged into the light and can now be overturned to make way for a better, more *rational* approach to fundamental questions about life, the universe and everything. The Enlightenment saw much of the same sentiments, but then again, so did logical positivism.

APPENDIX

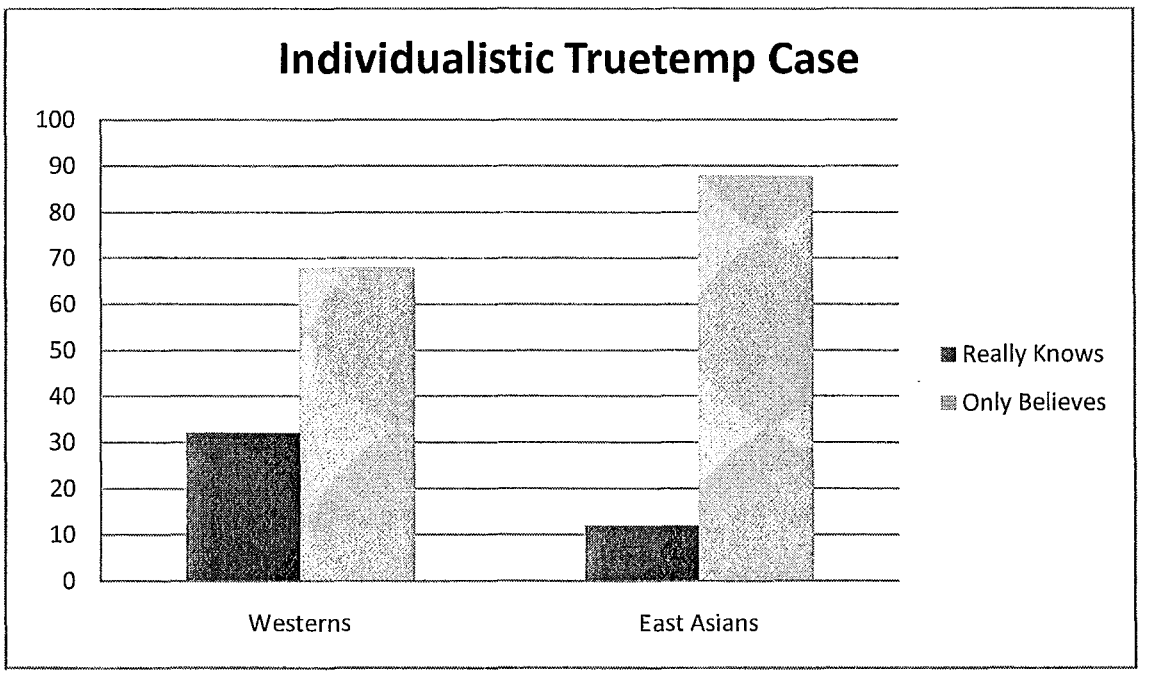
1.1 – Individualistic Truetemp Case

Sample Probe

One day Charles is suddenly knocked out by a falling rock, and his brain becomes re-wired so that he is always absolutely right whenever he estimates the temperature where he is. Charles is completely unaware that his brain has been altered in this way. A few weeks later, this brain re-wiring leads him to believe that it is 71 degrees in his room. Apart from his estimation, he has no other reasons to think that it is 71 degrees. In fact, it is at that time 71 degrees in his room. Does Charles really know that it is 71 degrees in the room, or does he only believe it?

REALLY KNOWS ONLY BELIEVES

Results



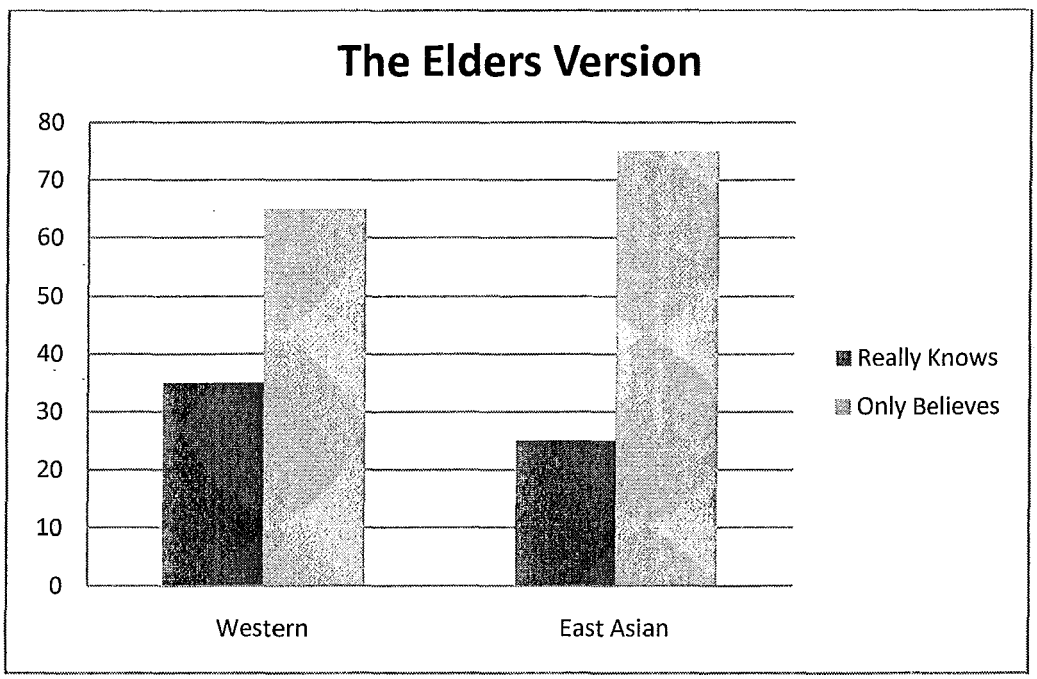
1.2 – The Elders Version

Sample Probe

One day John is knocked out by a team of well-meaning scientists sent by the elders of his community, and his brain is re-wired so that he is always absolutely right whenever he estimates the temperature where he is. John is completely unaware that his brain has been altered in this way. A few weeks later, this brain re-wiring leads him to believe that it is 71 degrees in his room. Apart from his estimation, he has no other reasons to think that it is 71 degrees. In fact, it is at that time 71 degrees in his room. Does John really know that it was 71 degrees in the room, or does he only believe it?

REALLY KNOWS ONLY BELIEVES

Results



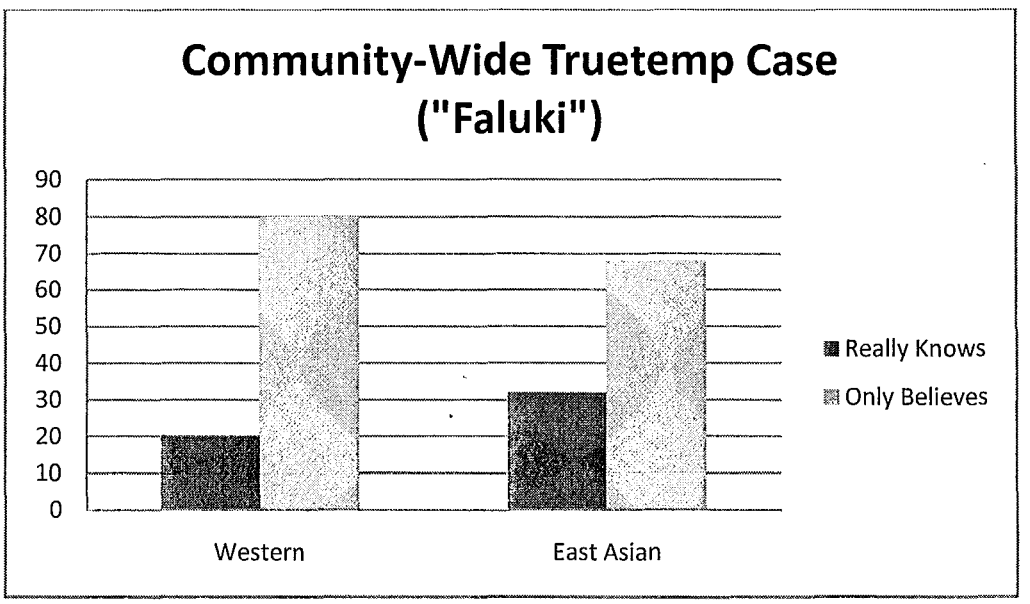
1.3 - Community-Wide Truetemp Case ("Faluki")

Sample Probe

The Faluki are a large but tight-knit community living on a remote island. One day, a radioactive meteor strikes the island and has one significant effect on the Faluki—it changes the chemical make-up of their brains so that they are always absolutely right whenever they estimate the temperature. The Faluki are completely unaware that their brains have been altered in this way. Kal is a member of the Faluki community. A few weeks after the meteor strike, while Kal is walking along the beach, the changes in his brain lead him to believe that it is 71 degrees where he is. Apart from his estimation, he has no other reason to think that it is 71 degrees. In fact, it is at that time exactly 71 degrees where Kal is. Does Kal really know that it is 71 degrees, or does he only believe it?

REALLY KNOWS ONLY BELIEVES

Results



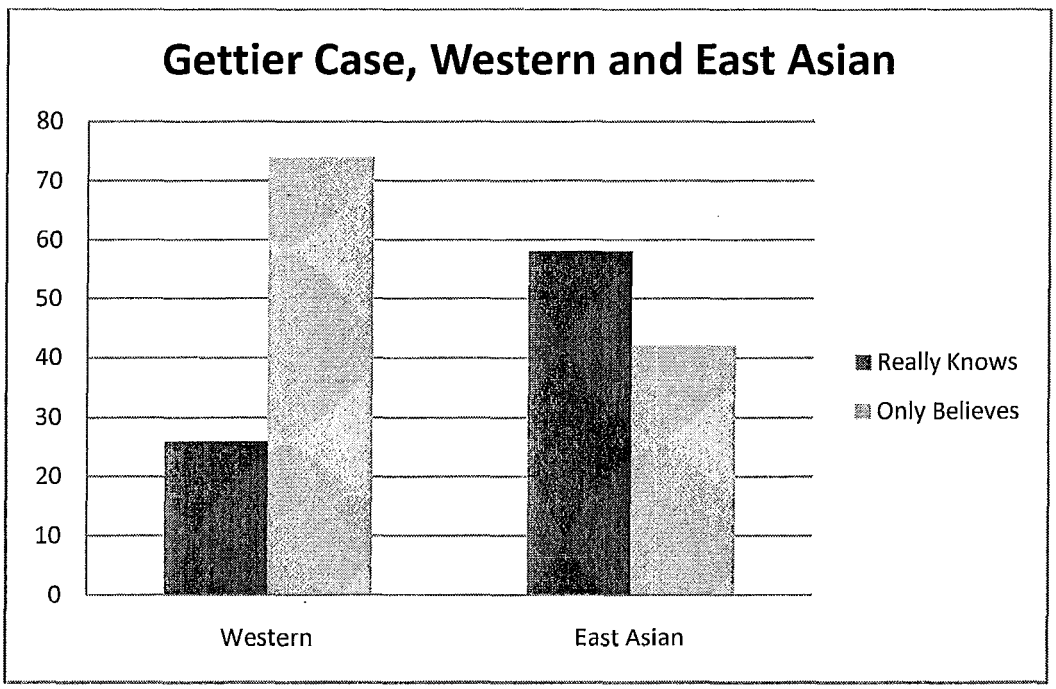
1.4 – Gettier Case, Western and East Asian

Sample Probe

Bob has a friend, Jill, who has driven a Buick for many years. Bob therefore thinks that Jill drives an American car. He is not aware, however, that her Buick has recently been stolen, and he is also not aware that Jill has replaced it with a Pontiac, which is a different kind of American car. Does Bob really know that Jill drives an American car, or does he only believe it?

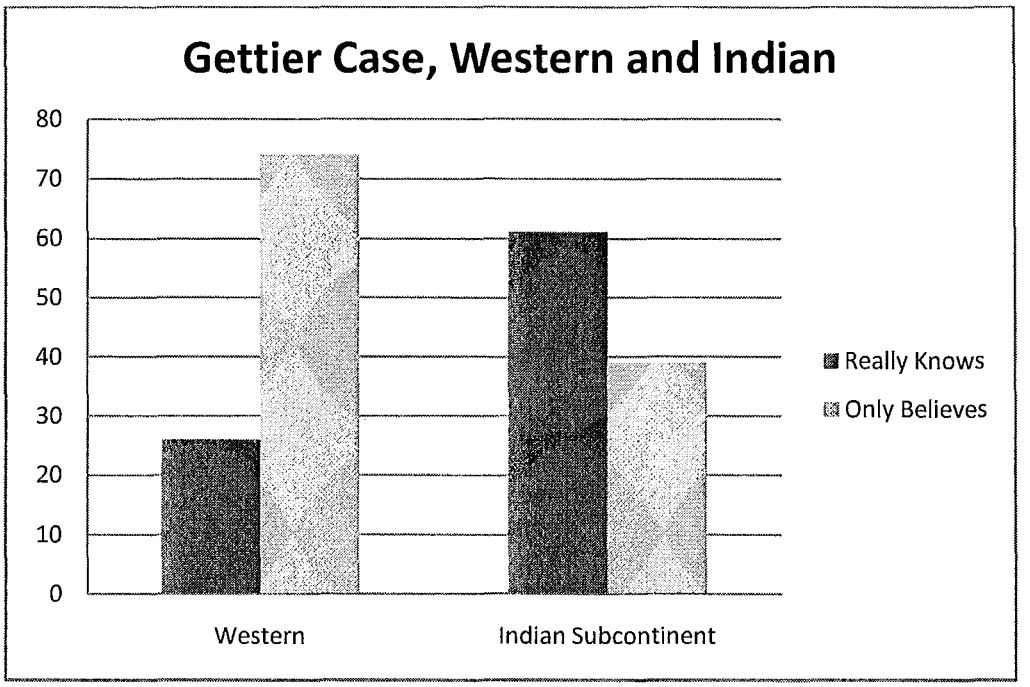
REALLY KNOWS ONLY BELIEVES

Results



1.5 – Gettier Case, Western and Indian

Results



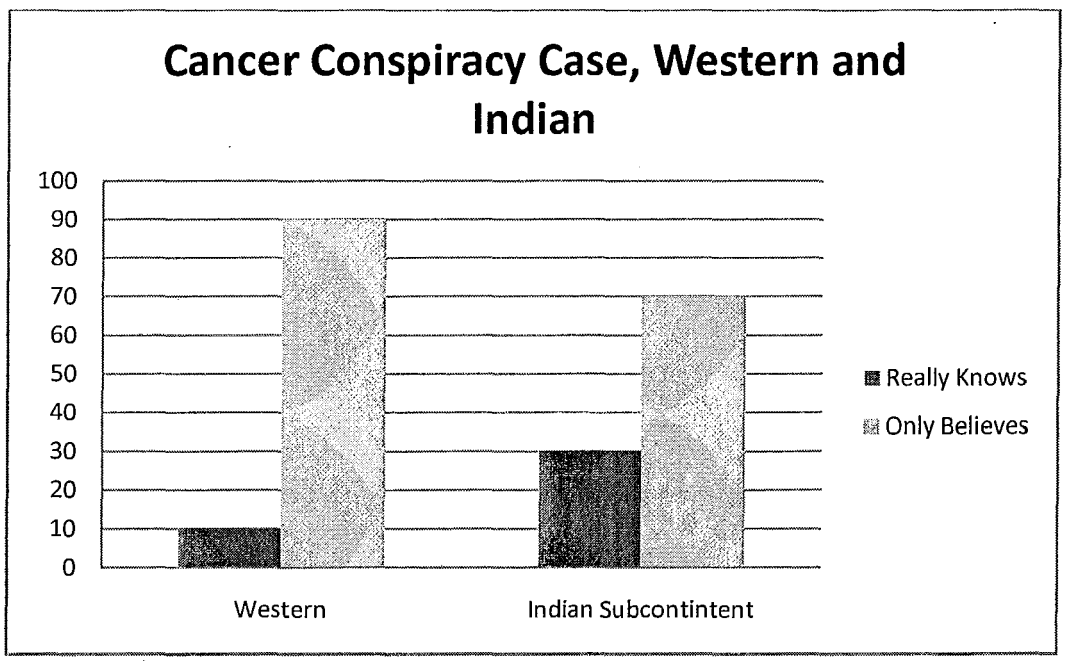
1.6 – Cancer Conspiracy and Zebra Cases, Western and Indian

Conspiracy Case – Sample Probe

It's clear that smoking cigarettes increases the likelihood of getting cancer. However, there is now a great deal of evidence that just using nicotine by itself without smoking (for instance, by taking a nicotine pill) does not increase the likelihood of getting cancer. Jim knows about this evidence and as a result, he believes that using nicotine does not increase the likelihood of getting cancer. It is possible that the tobacco companies dishonestly made up and publicized this evidence that using nicotine does not increase the likelihood of cancer, and that the evidence is really false and misleading. Now, the tobacco companies did not actually make up this evidence, but Jim is not aware of this fact. Does Jim really know that using nicotine doesn't increase the likelihood of getting cancer, or does he only believe it?

REALLY KNOWS ONLY BELIEVES

Cancer Conspiracy Case – Results

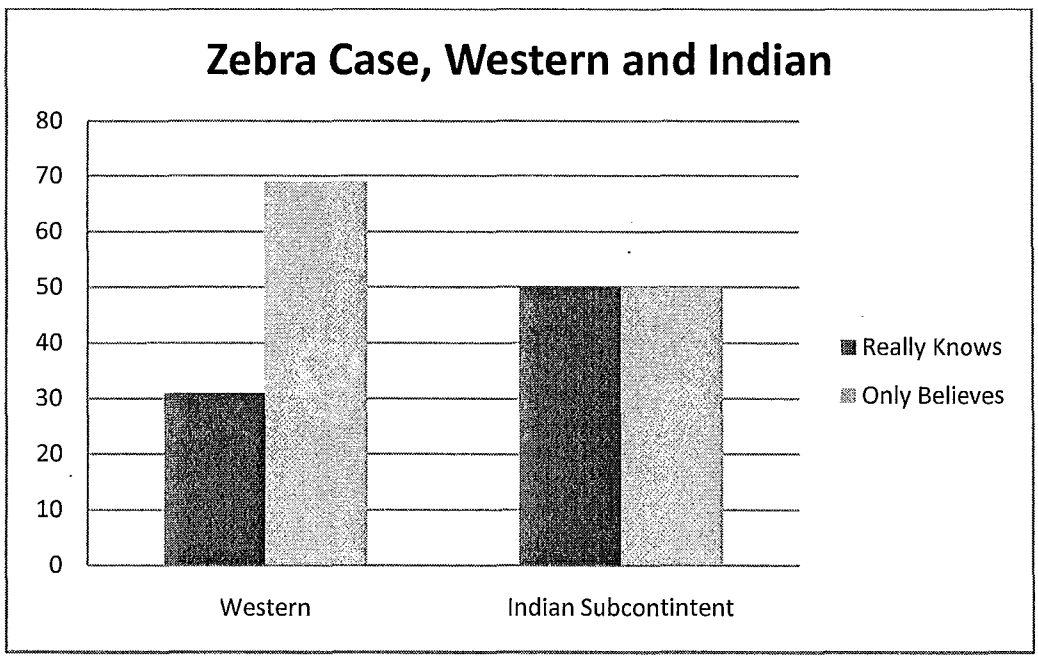


Zebra Case – Sample Probe

Mike is a young man visiting the zoo with his son, and when they come to the zebra cage, Mike points to the animal and says, "that's a zebra." Mike is right—it is a zebra. However, as the older people in his community know, there are lots of ways that people can be tricked into believe things that aren't true. Indeed, the older people in the community know that it's possible that zoo authorities could cleverly disguise mules to look just like zebras, and people viewing the animals would not be able to tell the difference. If the animal that Mike called a zebra had really been such a cleverly painted mule, Mike still would have thought that it was a zebra. Does Mike really know that the animal is a zebra or does he only believe that it is?

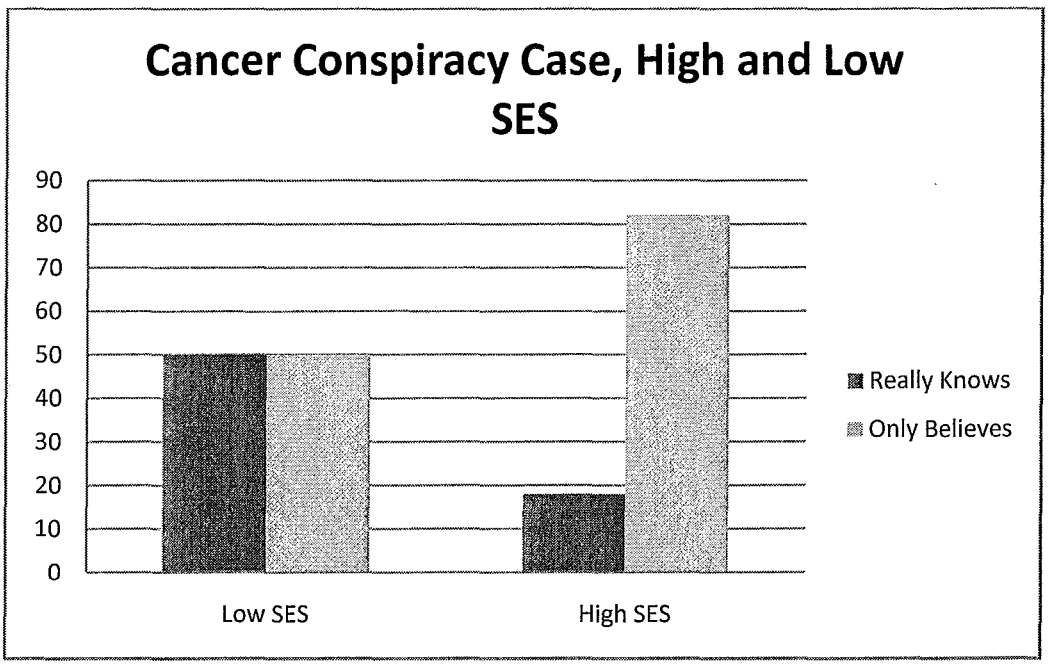
REALLY KNOWS ONLY BELIEVES

Zebra Case – Results



1.7 – Cancer Conspiracy and Zebra Cases, High and Low Socio-Economic Status

Cancer Conspiracy Results

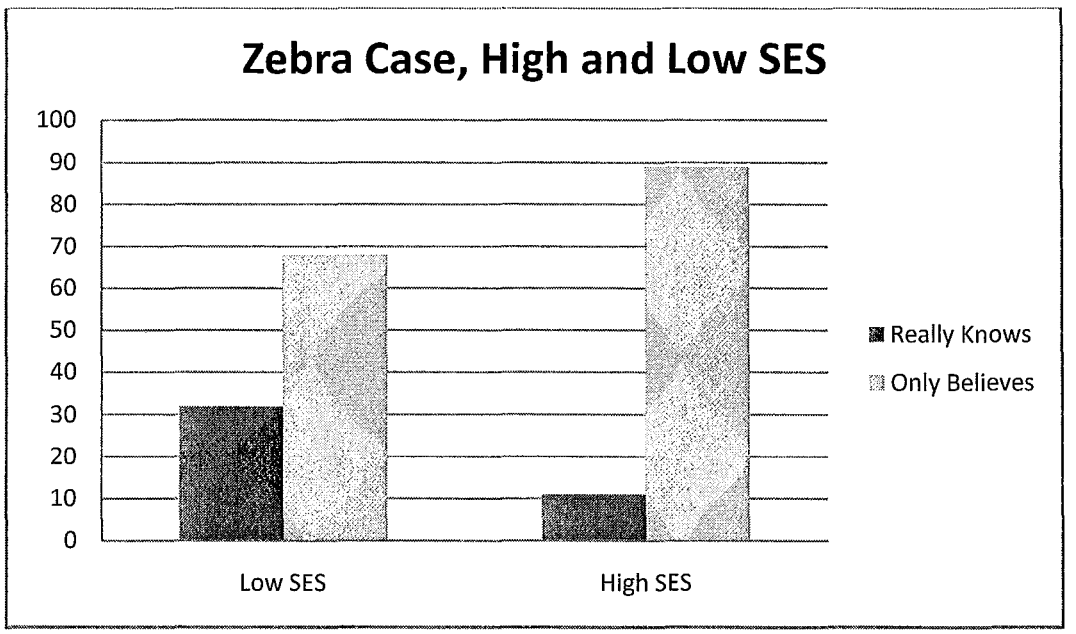


Zebra Case – Sample Probe

Pat is at the zoo with his son, and when they come to the zebra case, Pat points to the animal and says, "That's a zebra." Pat is right—it is a zebra. However, given the distance the spectators are from the cage, Pat would not be able to tell the difference between a real zebra and a mule that is cleverly disguised to look like a zebra. And if the animal had really been a cleverly disguised mule, Pat still would have thought that it was a zebra. Does Pat really know that the animal is a zebra, or does he only believe that it is?

REALLY KNOWS ONLY BELIEVES

Zebra Case Results



1.8 – Coin Flip Case

Sample Probe

Dave likes to play a game with flipping a coin. He sometimes gets a "special feeling" that the next flip will come out heads. When he gets this "special feeling," he is right about half the time, and wrong about half the time. Just before the next flip, Dave gets that "special feeling," and the feeling leads him to believe that the coin will land heads. He flips the coin, and it does land heads. Did Dave really know that the coin was going to land heads or did he only believe it?

REALLY KNOWS ONLY BELIEVES

2.1 – Sample Survey from Swain et al.

We are investigating what different people's opinions are about knowledge. In each question, please indicate to what extent you agree or disagree with that statement.

1. Dave likes to a play a game with flipping a coin. He sometimes gets a "special feeling" that the next flip will come out heads. When he gets this "special feeling," he is right about half the time, and wrong about half the time. Just before the next flip, Dave gets that "special feeling," and the feeling leads him to believe that the coin will land heads. He flips the coin, and it does land heads.

Please indicate to what extent you agree or disagree with the following claim:
"Dave knew the coin was going to land heads."
___Strongly agree ___Agree ___Neutral ___Disagree ___Strongly disagree

2. One day Charles was knocked out by a falling rock; as a result his brain was "rewired" so that he is always right whenever he estimates the temperature where he is. Charles is unaware that his brain has been altered in this way. A few weeks later, this brain rewiring leads him to believe that it is 71 degrees in his room. Apart from his estimation, he has no other reasons to think that it is 71 degrees. In fact, it is 71 degrees.

Please indicate to what extent you agree or disagree with the following claim:
"Charles knows that it is 71 degrees in his room."
___Strongly agree ___Agree ___Neutral ___Disagree ___Strongly disagree

3. Suzy looks out the window of her car and sees a barn near the road, and so she comes to believe that there's a barn near the road. However, Suzy doesn't realize that the countryside she is driving through is currently

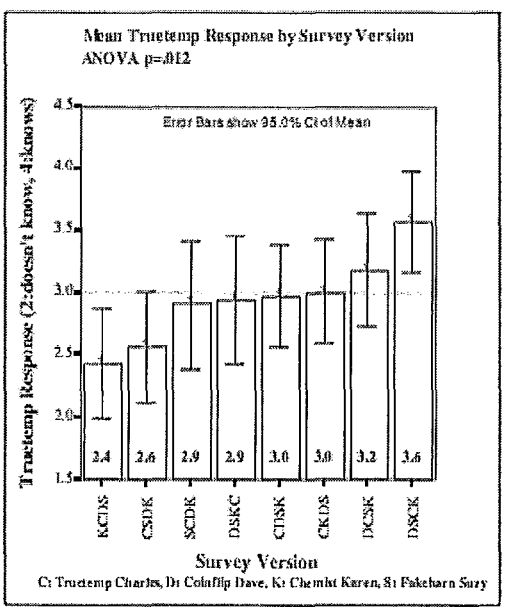
being used as the set of a film, and that the set designers have constructed many fake barn facades in this area that look as though they are real barns. In fact, Suzy is looking at the only real barn in the area.

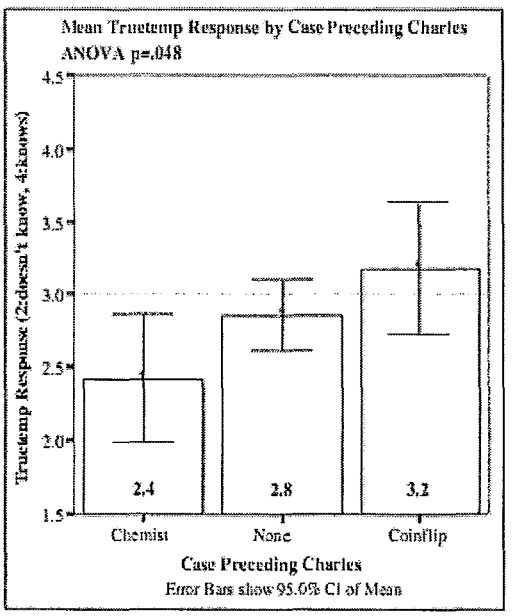
Please indicate to what extent you agree or disagree with the following claim: "Suzy knows there is a barn near the road."
___Strongly agree ___Agree ___Neutral ___Disagree ___Strongly disagree

4. Karen is a distinguished professor of chemistry. This morning, she read an article in a leading scientific journal that mixing two common floor disinfectants, Cleano Plus and Washaway, will create a poisonous gas that is deadly to humans. In fact, the article is correct: mixing the two products does create a poisonous gas. At noon, Karen sees a janitor mixing Cleano Plus and Washaway and yells to him, "Get away! Mixing those two products creates a poisonous gas!"

Please indicate to what extent you agree or disagree with the following claim: "Karen knows that mixing these two products creates a poisonous gas."
___Strongly agree ___Agree ___Neutral ___Disagree ___Strongly disagree

2.2 – Data from Swain et al. (2008)





3.1 – Intuition Probes from Machery et al. (2008)

Western Gödel Case

Suppose that John has learned in college that Gödel is the man who proved an important mathematical theorem, called the incompleteness of arithmetic. John is quite good at mathematics and he can give an accurate statement of the incompleteness theorem, which he attributes to Gödel as the discoverer. But this is the only thing that he has heard about Gödel. Now suppose that Gödel was not the author of this theorem. A man called "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 claimed credit for the work, which was thereafter attributed to Gödel. Thus he has been known as the man who proved the incompleteness of arithmetic. Most people who have heard the name "Gödel" are like John; the claim that Gödel discovered the incompleteness theory is the only thing they have ear heard about Gödel.
When John uses the name "Gödel," is he talking about:

(A) The person who really discovered the incompleteness of arithmetic?
or
(B) The person who got hold of the manuscript and claimed credit for the work?

East Asian Gödel Case

Ivy is a high school student in Hong Kong. In her astronomy class she was taught that Tsu Ch'ung Chih was the man who first determined the precise time of the summer and winter solstices. But, like all her classmates, this is the only thing she has heard about Tsu Ch'ung Chih. Now suppose that Tsu Ch'ung Chih did not really make this discovery. He stole it from an astronomer who died soon after making the discovery. But the theft remained entirely undetected and Tsu Ch'ung Chih became famous for the discovery of the precise times of the solstices. Many people are like Ivy; the claim that Tsu Ch'ung Chih determined the solstice times is the only thing they have heard about him.
When Ivy uses the name "Tsu Ch'ung Chih," is she talking about:

(A) The person who really determined the solstice times?
Or
(B) The person who stole the discovery of the solstice times?

Western Jonah Case

In high school, German students learn that Attila founded Germany in the second century A.D. They are taught that Attila was the king of a nomadic tribe that migrated from the east to settle in what would become Germany. Germans also believe that Attila was a merciless warrior and leader who expelled the Romans

from Germany, and that after his victory against the Romans, Attila organized a large and prosperous kingdom.
Now suppose that none of this is true. No merciless warrior expelled the Romans from Germany, and Germany was not founded by a single individual. Actually, the facts are the following. In the fourth century A.D., a nobleman of low rank, called "Raditra", ruled a small and peaceful area in what today is Poland, several hundred miles from Germany. Raditra was a wise and gentle man who managed to preserve the peace in the small land he was ruling. For this reason, he quickly became the main character of many stories and legends. These stories were passed on from one generation of peasants to the next. But often when the story was passed on the peasants would embellish it, adding imaginary details and dropping some true facts to make the story more exciting. From a peaceful nobleman of low rank, Raditra was gradually transformed into a warrior fighting for his land. When the legend reached Germany, it told of a merciless warrior who was victorious against the Romans. By the 8th century A.D., the story told of an Eastern king who expelled the Romans and founded Germany. By that time, not a single true fact remained in the story.
Meanwhile, as the story was told and retold, the name "Raditra" was slowly altered: it was successively replaced by "Aditra", then by "Arritrak" in the sixth century, by "Arrita" and "Arrila" in the seventh and finally by "Attila". The story about the glorious life of Attila was written down in the 8th century by a scrupulous Catholic monk, from whom all our beliefs are derived. Of course, Germans know nothing about these real events. They believe a story about a merciless Eastern king who expelled the Romans and founded Germany.
When a contemporary German high school student says "Attila was the king who drove the Romans from Germany," is he actually talking about the wise and gentle nobleman, Raditra, who is the original source of the Attila legend, or is he talking about a fictional person, someone who does not really exist?

(A) He is talking about Raditra.
(B) He is talking about a fictional person who does not really exist.

Lau Mei Ling is a high school student in the Chinese city of Guangzhou. Like everyone who goes to high school in Guangzhou, Mei Ling believes that Chan Wai Man was a Guangdong nobleman who had to take refuge in the wild mountains around Guangzhou in the 11th century A.D, because Chan Wai Man was in love with the daughter of the ruthless Government Minister Lee, and the Minister did not approve. Everyone in Lau Mei Ling's high school believes that Chan Wai Man had to live as a thief in the mountains around Guangzhou, and that he would often steal from the rich allies of the Minister Lee and distribute their goods to the poor peasants.
Now suppose that none of this is true. No Guangdong nobleman ever lived in the mountains around Ghangzhou, stealing from the wealthy people to help the peasants. The real facts are the following. In one of the monasteries around

Guangzhou, there was a helpful monk called "Leung Yiu Pang". Leung Yiu Pang was always ready to help the peasants around his monastery, providing food in the winter, giving medicine to the sick and helping the children. Because he was so kind, he quickly became the main character of many stories. These stories were passed on from one generation of peasants to the next. Over the years, the story changed slowly as the peasants would forget some elements of the story and add other elements. In one version, Leung Yiu Pang was described as a rebel fighting Minister Lee. Progressively the story came to describe the admirable deeds of a generous thief. By the late 14th century, the story was about a generous nobleman who was forced to live as a thief because of his love for the Minister's daughter. At length, not a single true fact remained in the story.
Meanwhile, the name "Leung Yiu Pang" was slowly altered: it was successively replaced by "Cheung Wai Pang" in the 12th century, "Chung Wai Man" in the 13th, and finally by "Chan Wai Man". The story about the adventurous life of Chan Wai Man was written down in the 15th century by a scrupulous historian, from whom all our beliefs are derived. Of course, Mei Ling, her classmates and her parents know nothing about these real events. Mei Ling believes a story about a generous thief who was fighting against a mean minister.
When Mei Ling says "Chan Wai Man stole from the rich and gave to the poor", is she actually talking about the generous monk, Leung Yiu Pang, who is the original source of the legend about Chan Wai Man, or is she talking about a fictional person, someone who does not really exist?

(A) She is talking about the generous monk, Leung Yiu Pang.
(B) She is talking about a fictional person who does not really exist.

3.2 – Data From Machery et al.

Mean scores for experiment 1 (SD in parentheses)

	Score (SD)
Gödel cases	
Western participants	1.13 (0.88)
Chinese participants	0.63 (0.84)
Jonah cases	
Western participants	1.23 (0.96)
Chinese participants	1.32 (0.76)

"Each question was scored binomially. An answer consonant with causal-historical accounts of reference (B) was given a score of 1; the other answer (A) was given a score of 0. The scores were then summed, so the cumulative score could range from 0 to 2." (Machery et al., B7)

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