Intuitions in Metaphysics:

A Methodological Critique

by

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ABSTRACT

This thesis is concerned with the methodological role of intuitions in metaphysics. It is divided into two main parts. Part I argues that an academic field can only employ a method of gathering evidence if it has established some agreed-upon standards regarding how to evaluate uses of this method. Existing meta-philosophical disputes take the nature of intuitions to be their starting point. This is a mistake. My concern is not the epistemic status of intuitions, but rather how metaphysicians appeal to intuitions as a form of evidence. In order for intuitions to play a viable role in research they must be subject to certain constraints, regardless of whether they allow individual researchers to know that their theories are true. Metaphysicians are not permitted to use intuitions as arbitrarily having different evidential status in different circumstances, nor should they continue to use intuitions as evidence in certain disputes when there is disagreement amongst disputants about whether intuitions should have this evidential status.

Part II is dedicated to showing that metaphysicians currently use intuitions in precisely the sort of inconsistent manner that was shown to be impermissible in Part I. I first consider several competing theories of how intuitions function as evidence and argue that they all fail. As they are currently used in metaphysics, intuitions are analogous to instruments in the sciences in that they are taken to be a substantial non-inferential source of evidence for theories. I then analyze several major metaphysical disputes and show that the source of controversy in these disputes boils down to inconsistencies in how the different parties treat intuitions as evidence. I conclude that metaphysicians must abandon appeals to intuition as evidence—at least until the field can agree upon some general standards that can resolve these inconsistencies.

DEDICATION

For my father, who taught me that if one wishes to know the truth one must cease to cherish one's own opinion.

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1. General Methodology

One of the main points of a methodological inquiry is to provide a field with a better way of adjudicating between competing theories. Part I of this thesis will argue for some constraints on how and when it is appropriate to appeal to intuitions when deciding between competing metaphysical theories. Part I is broken into four main subsections. §1.1 establishes some broad preliminary points. §1.1.1 discusses the purpose of research and how this relates to the methodological evaluation of research methods. It argues that the purpose of academic research is for the academic community to arrive at a consensus theoretical description of phenomena, and that the methods employed in research must be evaluated in terms of how they can be employed in order to accomplish this task. §1.1.2 explains why our methodological enterprise is concerned primarily with *appeals to intuition* rather than intuitions themselves. §1.1.3 makes a few brief points about the nature of evidence and theory confirmation.

§1.2 is concerned with outlining a specific view of philosophical methodology called Individualism, which is roughly the view that theoretical success should be analyzed in terms of the epistemic states of individual researchers. This is contrasted with Collectivism, which is the view that theoretical success is determined by constitutive rules laid down by the academic community. §1.2.1 argues that Individualism has structured the meta-philosophical debate about intuition. §1.2.1.1-4 consider various examples in which specific meta-philosophers discuss the methodological role of intuition in Individualistic terms. §1.2.1.5 speculates about why Individualism is so dominant, and argues that the specific philosophers considered need not have explicitly assumed Individualism when they allow it to structure their arguments.

§1.3 is concerned with showing that Individualism is implausible. The section is broken down into two main sections, which correspond to the two primary lines of argument against Individualism. §1.3.1 provides the *Argument from Practice*. It is first argued that Individualism is clearly false as a methodological description for mathematics and the sciences. Since philosophy does not seem to differ from these disciplines in any relevant respect, there is no reason to think that Individualism is true for philosophical research either. §1.3.1.1 considers various ways in which the Individualist might try to break the analogy, and argues that all these strategies fail. §1.3.2 provides the *Argument from Methodological Primacy*. This argument proceeds in two stages. §1.3.2.1 considers various ways in which Individualism might be formulated and argues that these formulations either fail to render plausible results concerning when theories are considered successful, or tacitly rely upon Collectivist principles. §1.3.2.1 argues more generally that the knowledge of individual researchers concerning their theories is dependent in various ways upon the fact that the field employs Collectivist methodology. In either case, the Individualist has gotten the order of explanation backwards.

§1.4 draws some general conclusions given the rejection of Individualism. It argues that given sociological facts regarding the nature of philosophical research and the reason why such research receives funding, researchers are obligated to remain reflective about methodology. In §1.4.1 argues that the methods employed in philosophical research must be subject to some broad constraints. In particular, it argues for a general principle (MC), which dictates that philosophers are not permitted to employ methods that can be used to achieve contradictory results without it being in principle decidable how to resolve the contradiction. This principle will be crucial to the evaluation of intuition in

Part II. §1.4.1.1 explains how fields might fail to pass (MC). It elaborates two general ways in which fields can be unacceptably dogmatic. One form of dogmatism occurs when a subset of a field can disregard (MC) by forming echo-chambers in which outside objections are ignored. A second form of dogmatism, dubbed *schismatic complacency*, occurs when various members of a field disagree about what should count as evidence in these disputes, yet continue to use the disputed evidence when engaging in first-order disputes without attempting any higher-order methodological inquiry in order to settle what should count as evidence. §1.4.2 concludes the chapter by summarizing the implications of our rejection of Individualism and the acceptance of (MC), and by outlining the role these assumptions will make throughout Part II.

1.1. Preliminaries

This essay is a methodological inquiry into the role of intuitions in metaphysics. *Methodology* is the study of methods. While I will not bother to provide a full-fledged analysis of 'method', I will make the following remarks:[1]

(MP1) A method is a general technique that can be employed in multiple circumstances. A method can also be described as a *procedure*: a set of general instructions for a series of actions.[2] Methods are determinable: they are realized (i.e., 'employed') when one actually carries out a particular set of actions.

[1] My aim here is not to analyze the concept METHOD as it occurs in English, but to capture the notion of method that is essential to accomplishing the goals of philosophy as an academic discipline. These are not meant to be a set of necessary and sufficient conditions something must meet in order to fall under our intuitive notion of a method. Rather, they are meant to be important general marks that methods possess. Intuitive counterexamples will be odd cases that have no bearing upon the current discussion.

[2] If one wanted to get into the ontology of methods, they would likely be some sort of abstract entity: perhaps an ordered set of proposition-like constructions corresponding to instructional commands. But this needn't really concern us. What is important is that they can be employed in multiple occasions. For example, when building a bird house, one puts the sides together first and then nails them to the floor, otherwise the birdhouse will likely end up crooked. This method can be employed with a number of materials and configurations.

(MP2) Methods need not be algorithmic (i.e., they need not be capable of being performed by a Turing machine). However, they do need to be specific enough to determine whether or not some particular set of actions properly follows the procedure in question.[3]

(MP3) Methods serve *functions*.[4] The function(s) of a method is determined by the goals that the method is employed to achieve. Methodology is the evaluation of how efficiently methods fulfil their function.

This chapter aims to establish general conclusions about the nature of philosophical research as well as some very general restrictions on how intuitions can be used as methods for establishing conclusions in metaphysics. However, before we can proceed to these specific arguments we need to first elaborate some very basic points about intuitions and philosophical research.

1.1.1. The Aim of Philosophical Research

The question we are concerned with is: under what conditions will a philosophical theory receive confirmation from a claim of the form '*p* because it is intuitive that *p*'? It follows from (M3) that we can only evaluate the methods employed in philosophical research by first determining the goal of philosophical research itself.[5] Thus the first step in evaluating intuition as a philosophical method is to determine the goal of philosophical

[3] For instance, in the bird-house example one just looks to see the order in which the bird-house builder nailed the wood together. More complex examples abound in the physical sciences. It is fairly easy (in principle) to tell whether a biology graduate student treated her cells in the proper way.

[4] There is no need to posit primitive teleological properties to understand what a method is, or to say that methods *exist for* something in some incomprehensible sense. Rather, the claim that methods serve a function is simply means that they will not be *implemented* in human activity unless they bring about a desired end. We may cash out our talk of the functions of methods in terms of straightforward counterfactual claims about human behavior. If it were not the case that a certain method got us to our goals, then we would not employ that method. This reductive approach may not work for many seemingly teleological concepts, but this needn't concern us. Even if one wished to claim that telic properties are irreducible, this would not be relevant to the characterization of method as explained below.

[5] A large part of the value of academic philosophy is pedagogical (e.g., the fact that it teaches students critical thinking skills, etc.). However, we are here concerned with the methodology behind philosophical research. The question is: Why does there exist funding for philosophical journals?

research. For the purposes of this and any other discussion of philosophical methodology one should assume that philosophical research receives funding because the content of this research itself possesses some non-arbitrary value.[6]

Cynical explanations to the contrary can be put aside for dialectical reasons. I shall disregard the possibility that funding for philosophical research is merely considered a necessary expense to maintain the pedagogical value of philosophy departments. I shall also disregard the possibility that philosophical research is simply funded due to an academic *status quo* wherein established fields of research are funded regardless of whether that research has any value simply to boost the university's total prestige. First of all, these explanations are independently unreasonable.[7] Second, even if these explanations were true, this would obviate the need for this thesis rather than count as an objection to it.

To see this, consider the fact that there are two conclusions one can draw from these cynical answers. Suppose that philosophical research is not funded because in order to produce an objective body of knowledge. This could imply that philosophical research is (1) a confused waste of time because philosophy is a confused waste of time, or (2) is not a confused waste of time, but also does not have a purpose grounded in public

[6] We can put aside the question of whether philosophical research is of intrinsic or merely instrumental value, for this question would not affect philosophical practice any more than it affects scientific practice. Even if one is an instrumentalist one should acknowledge that scientific research should be practiced as though it were a general quest for knowledge. First, one is rarely certain in advance that a research project will lead to new technology. Second, it is almost universally true that a better understanding of the fundamentals of the subject matter is necessary before any new technology is invented. Nuclear power is a very obvious example, as it simply was not possible until quantum mechanics was fairly well-developed, and it was far from obvious that developing a systematic way of explaining the behavior of light and electrons would lead to the specific technological breakthroughs. Analogously, it was far from obvious to Frege and Russell that their development of the predicate calculus would help lead to the eventual development of computers.

[7] Philosophers have produced many important results such as advancements in mathematical logic, large contributions to cognitive science, as well as a strong influence in political theory, to name just a few.

interest. If (1) is case, this thesis would be meaningless busywork. However, this could hardly be used to vindicate the use of intuition in philosophy. Both my arguments and any objections raised against them would be worthless. Thus (1) is as reasonable an objection to a methodological examination of philosophy as external-world skepticism is to an examination of the methods used by cognitive scientists to study perception.

Suppose that (2) is the case. This would imply that philosophical research receives funding either for reasons that have nothing to do with the content of this research, or for a multiplicity of reasons given the interests of different benefactors.[8] The problem is that this would mean that philosophical research is a free-for-all. In particular, this would imply that one cannot analyze the contents of different publications as a source of methodological evidence because different publications and articles are liable to have different purposes. The only way to arrive at such a unified methodological approach is to presuppose that there is a unified set of interests behind all research. If (2) were the case this would render a unified methodological study of philosophy impossible.[9]

The most straightforward explanation for why the content of philosophical research is valuable is that the goal of philosophical research is to arrive at some interesting description of some portion of reality. This thesis shall assume that the aim of

[8] If (2) is the case, then it follows from (M3) that the methodological study of philosophical research should be conducted according to how the methods employed in academic research suit the interests of individual philosophers and the people who run philosophical publications.

[9] It is important to note that one cannot use (2) to justify Individualism (see following section). Indeed, (2) is just as destructive for Individualism as it is for Collectivism. Both positions presuppose that the unified aim of philosophical research is to get at the truth, and differ only in how they measure the *success* of research. Individualism holds that a philosophical field reaches its goal only if the individual philosophers who comprise that field all know that a particular theory is true. The claim that philosophical research lacks a general purpose can hardly be mustered in support of Individualism. We will discuss this in greater detail in §1.3.1.1.2.

metaphysical research is to provide a description of reality that is capable of being accurate or inaccurate. We shall call this presupposition *Methodological Realism*.[10] The reasoning for this presupposition is fairly straightforward. A methodological criticism of intuition that presupposed that Methodological Realism is false in regard to metaphysics would be fairly trivial.[11]

1.1.2. Intuitions as Methods

We will now make a couple preliminary observations about how exactly intuitions can be considered methods. The first point we must make is that we are concerned with whether and how intuitions can be used as valid *research methods*. The research method we are concerned with most directly are *appeals to intuition*, not the intuitions themselves. The former is a statement made in a journal article, while the latter is an occurrent mental state possessed by an individual. Our primary methodological approach will be to examine how appeals to intuition function in philosophical discourse, thus our primary source of evidence will consist in what is said in publications. We will consider the nature of intuitions themselves only after we have established their methodological role later in Chapter 3. One reason for this approach should be fairly obvious: an epistemological analysis of intuitions will be rather useless if it turns out that intuitions do not play a significant methodological role.

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[10] Note that Methodological Realism merely dictates that metaphysical theories admit of being *accurate*. To say that these theories admit of being *true* is a much stronger position. This will become extremely relevant in our discussion of metametaphysics later on.

[11] If one begins by denying Methodological Realism there no longer seems to be any motivation to discern the proper methodological role for appeals to intuition in metaphysics. For instance, the claim that intuitions cannot be used to establish metaphysical truths follows trivially from the premise that metaphysical questions are meaningless (i.e., not truth apt). Alternatively, one could argue that metaphysical theories are analogous to works of art, and then argue that intuitions are incredibly important for metaphysical methodology in that they are expressions of aesthetic taste. But this would imply that there is no way to dissolve disputes about whose theory is correct; undoubtedly an unpalatable position for most metaphysicians.

There is another straightforward reason for focusing on appeals to intuition before focusing on intuitions themselves: what counts as an intuition is *itself* controversial. As we shall see later on, there are multiple ways in which one might choose to individuate intuitions. One option is to individuate intuitions as occurrent mental states. However, authors disagree about the phenomenology of the mental states in question. Some identify intuitions as simple *sui generis* propositional attitudes, where these are understood to be affective states (i.e., as "intellectual seemings").[12] Others identify intuitions with imaginative states with complex sensory contents, such as visualizations in mathematics.[13] Still others take a "doxastic views" which identify intuitions are mere beliefs or counterfactual dispositions to believe.[14] Another point of disagreement concerns the propositional content of intuitions. For some rationalists, something is an intuition only if it has a content that is necessarily true (e.g., Bealer 1996). Other philosophers treat intuitions as being the result of tacit cognitive heuristics, such that they can have virtually any content (e.g., Weatherson Forthcoming). Moreover, these positions tend to be correlated with the view one takes about the role of intuitions in philosophical methodology. Thus rationalists deliberately argue for restricted notions of intuition so that they can more easily explain why philosophical intuitions have special evidential status, and so that they can avoid counterexamples generated by unreliable judgments in

[12] E.g., Bealer 1996a, 123-4.

[13] E.g., Chudnoff 2013c, 25-80.

[14] I borrow the term 'doxastic view' from Chudnoff (2013c, 25). Chudnoff attributes the view to a number of philosophers, including Timothy Williamson (esp. with regard to the passage in Williamson 2007, 208-220). However, there is some ambiguity in the use of the term. For instance, it is perfectly reasonable to deem a seeming *qua* propositional attitude as being phenomenologically closer to mere belief than the sort of complex visualizations that Chudnoff considers. In fact, I think Williamson is better read as treating 'inclination' not as a counterfactual disposition to believe, but as a genuine occurrent *sui generis* propositional attitude that is simply weaker than belief. For an explicit defense of the counterfactual analysis, see Ernest Sosa's response to George Bealer (Sosa 1996, 153-4).

other areas. In contrast, philosophers who are skeptical of the use of intuition in philosophy are thereby invested in showing that there is no special difference between philosophical intuitions and mere hunches.

Given the vast array of disagreement about the specific nature of intuition, it would be foolhardy to begin our methodological analysis by presupposing a specific account. Fortunately, by focusing on appeals to intuition we can avoid these complications. An appeal to intuition is simply an evidential claim of the form: *p* because it is intuitive that *p*. Often times appeals to intuition will be explicit. However, even when they are not one can usually tell whether a philosopher has made an appeal to intuition by looking at what that philosopher has written. If it is clear that one of the crucial premises in a philosopher's argument is un-argued, is taken by the philosopher to be obvious and cannot be reasonably taken to be justified by any other means, then it is reasonable to conclude that this premise is justified by an appeal to intuition. It is clear whether a philosopher has appealed to intuition regardless of how clear the notion of intuition is.

This is not uncontroversial. Herman Cappelen has recently provided extensive arguments for the descriptive claim that philosophers do not, in fact, appeal to intuitions (Cappelen 2012). Cappelen's arguments proceed by cherry-picking eight historical arguments and showing that these arguments do not seem to depend upon intuition (2012, ch. 8).[15] There are three primary problems with Cappelen's methodological approach. First, his project is purely negative. Even if Cappelen could show that the relevant arguments do not depend upon intuitions, he would not have thereby *justified* these

[15] I am ignoring the first half of Cappelen's argument; i.e., his objection to the 'argument from intuition talk' (2012, 25-93). This is because my diagnostic criteria for appeals to intuition fall squarely under what Cappelen calls the 'argument from practice' (2012, 94-218). Thus it is only his objections to the latter argument that are of any real consequence to our current approach.

arguments. On the contrary, this is completely compatible with those premises (and thus the conclusions of the arguments) being totally *un*justified. Thus without providing a positive alternative account of how these arguments are justified, Cappelen's analysis can only *undermine* the methodological status of philosophy.

The second problem with Cappelen's argument is that two of his three proposed diagnostic criteria for determining whether intuitions are at work in an argument are clearly based on partisan accounts of intuition (i.e., intuitions have a special phenomenology and are based solely on conceptual competence (2012, 112-3)). The most his arguments can show given these diagnostics is that the particular theories of intuition that make these assumptions cannot form the basis of an adequate methodological account of the cases in question. In using these presuppositions as a diagnostic for determining whether or not philosophers depend upon appeals to intuition, Cappelen has simply ignored alternative accounts.

This demonstrates that Cappelen has taken the wrong approach to methodology. Before one can analyze the specific characteristics of the methods employed in a field of inquiry, one must first establish what these methods *are*. In order to do this, one must look at how theories within a field appear to be selected. In essence, one must start with the widest and most abstract top-down perspective on how a field functions and then gradually narrow down one's focus until one is capable of identifying specific methods. By starting with a specific conception of what the methods must be and then looking only for what meets that conception, Cappelen has fallen prey to (dis)confirmation bias. However, the most salient problem with Cappelen's arguments for our purposes is simply the fact that none of the actual examples he considers come from pure metaphysics.

Rather, they come from thought experiments about particular cases in philosophy of language, ethics, epistemology, and philosophy of mind.[16] The situation in metaphysics is quite different. In particular, we shall see that metaphysicians depend heavily on intuitions concerning very abstract and general principles (see §2.4.2). For these reasons it will be far better for us to first look at how metaphysicians appeal to intuitions before we consider particular theories of how intuitions work.

Nevertheless, it is still convenient to adopt a few general presuppositions regarding intuitions. First, it is universally accepted that intuitions are intimately related to beliefs and propositions. As we noted above, some intuition theorists directly take intuitions to be propositional attitudes, while others take them to be something like sensory or perceptual states. Nevertheless, even those who take intuitions to be perception-like experiences with something like non-conceptual content still hold that these experiences directly support belief in propositions (e.g., Chudnoff 2013a,b,c). Thus we can conveniently individuate intuitions by their propositional content without losing any generality or ruling out any particular account of intuition. We can treat the claim 'it is intuitive that *p*' as being true just in case the relevant subjects have the relevant intuitive experiences that support *p*.

We needn't take a very specific stand on what exactly counts as such an experience. However, we can assume the following. First, intuitive experiences are occurrent mental states, not mere counterfactual dispositions to believe something.[17]

[16] The distribution of the cases he considers runs as follows: 2 philosophy of language (§8.1, §8.6), 2 philosophy of mind (§8.2, §8.8), 2 ethics (§8.3, §8.4), 3 epistemology (§8.5), and 1 personal identity (2012, 130-187).

[17] While this does rule out specific accounts, this is permissible insofar as there is no reasonable way in which a mere counterfactual disposition to believe could serve as good evidence.

Second, intuitive experiences are non-inferential. By 'non-inferential', we mean 'not a product of *explicit* conscious step-by-step inferences'. Thus we have *not* assumed that intuitive experiences cannot be the result of subconscious inferential processes or cognitive heuristics. A belief that *p* is intuitively justified (at least in part) because it is based upon the agent's intuition that *p*, but it is *not* based upon the belief that *p* is intuitive. Third, intuitive experiences are distinct from normal perceptual experiences. However, we do not thereby rule out the possibility that intuitions might be the results of a faculty of 'intellectual perception', or that they involve the use of imaginative experiences with sensory qualities. Moreover, we do not rule out the possibility that they might be somehow grounded in perceptual experience, although not explicitly based on particular perceptual episodes. For now, we may simply define intuitive justification as the justification or warrant that an agent has for a belief that *p* in virtue of its intuitively seeming to that agent that *p*.[18] In contrast, an appeal to intuition is simply a claim of the form '*p* because it is intuitive that *p*', where the 'because' is understood as denoting an evidential rather than an explanatory relation.[19]

1.1.3. Theoretical Evidence

Evidence is propositional and factive: something is evidence for us only if it is a true proposition that we have the right sort of access to. These assumptions are somewhat

[18] Every account of intuitions treats them as being attached to a proposition that is the content of a belief they justify (if they justify anything at all), so we shall treat intuitions as though they are individuated according to propositional content. Thus according to most theories one does not need infer *p* from the fact that one finds *p* intuitive. An intuition that *p* is therefore distinct from an intuition that $p \vee q$. The latter can either be justified inferentially from the former, or it could be justified by its own distinct (albeit related) intuition.

[19] In other words, the claim should really be read as '*p*, and my evidence for *p* is that it is intuitive that *p*'. However, this is a mouthful so I will stick to the original formulation. The claim should not be read as 'the fact that *p* obtains is grounded in the fact that *p* is intuitive'. This would be an explanatory claim. As we shall see later, this latter claim might actually serve to ground the first one in the case of constitutivist views of intuition. However, even in this case it still remains distinct from the original formulation.

controversial because they imply that we cannot always know what are evidence is.[20] Evidence can either confirm or disconfirm a theory. This can be represented using probability functions. Take a theoretical claim *p*, an initial body of evidence *e* and a new body of evidence e^* that is the result of either the addition or subtraction of individual pieces of evidence to or from *e*. For *p* to be confirmed or disconfirmed by *e** is for Prob(*p* | *e**) > Prob(*p* | *e*) or for Prob(*p* | *e**) < Prob(*p* | *e*) respectively.[21] A research method is something that modifies our existing body of evidence by either increasing or decreasing the probability of particular pieces of evidence. A theory is successful (or established) given our evidence at time *t* if its claims receive a high degree of confirmation from the appropriate body of evidence at *t*.

[20] See Williamson 2000a, ch. 9 for a defense. This issue can be avoided by shifting to talk of *putative evidence*, to which we do in fact have privileged access. For the same reasons, we can avoid the issue of whether intuitions themselves are factive by speaking instead of *putative intuitions*, where these are understood to be mental states that have all the non-factive features of regular intuitions (e.g., phenomenological characteristics, propositional content, etc.). If Williamson's anti-luminosity arguments are successful we will not even have perfect access to our putative evidence (2000a, ch. 4). However, this will obviously be the case for the notion of theoretical evidence anyway.

[21] This is roughly equivalent to Williamson's condition EV (2000a, 187). That is, after one accepts a conditionalization principle like Williamson's ECOND (2000a, 220). Like Williamson's position, our formulation is nonstandard insofar as it allows for the subtraction of evidence, which implies that evidence is non-monotonic. However, I think this is acceptable in the current context. First of all, we are not discussing the credences of individuals, but the degree to which the evidence that a field possesses supports a certain theory. The considerations that favor traditional Bayesian conditionalization for individual beliefs do not obviously carry over to the relevant notion of theoretical evidence. While the Individualist can attempt to build the relevant notion of theoretical evidence out of individual credences, it is still far from obvious that the resulting notion should utilize the same conditionalization principles as individual credences. Second, the monotonicity of evidence seems implausible even in regard to individual credences. Williamson uses the example of someone who sees a black ball and a red ball put into a bag, and then proceeds to see a red ball drawn and replaced ten thousand times in a row (2000a, 219). It is only rational for this person to lower her credence that she really did see a black ball placed in the bag, no matter how certain this seemed at the start. Our current evidence for a claim can in the future be subjected to defeaters no matter how certain it seems to be initially. (This claim is distinct from skepticism insofar as it does not require that we actually possess these defeaters. The mere possibility of such defeaters is enough to show that evidence is not monotonic.) For instance, if the sky suddenly broke open and the visage of a giant demon appeared who then proclaimed to us that all we perceive is but an illusion, this would assuredly undermine our belief that we are not in fact being deceived by a demon. Even logical beliefs do not seem to be subject to absolute certainty, as research programs investigating non-classical logics clearly indicate. Thus the subjective Bayesian will likely have to deny the existence of any credence of degree 1. But then there would seem to be no difference between her approach and one on which monotonicity fails, aside from the fact that her approach makes everything harder to calculate.

1.2. Methodological Individualism

Does the fruit of academic research constitute a body of evidence that transcends what is known by individual researchers, or is this evidence nothing more than the sum of what is known by certain individuals? On the first conception, academic research is the collective enterprise wherein an accurate description of reality is obtained by building up a shared body of evidence and evaluating this evidence according to agreed standards. Let us call this first position *Methodological Collectivism* (hereafter 'Collectivism'). According to the second conception, academic research is the result of organizing and maximizing the efforts of individuals as they independently pursue knowledge. On this conception, the success of an academic research program is measured by what individual researchers know. Let us call this second position *Methodological Individualism* (hereafter 'Individualism').

One of the primary goals of this chapter—indeed, one of the primary goals of this thesis more generally—is to show that Individualism is not only generally unreasonable, but has been particularly destructive in how it has influenced the debate about the methodological role of intuition. In order to demonstrate this point, it will be worthwhile to consider some more precise formulations of Individualism. Collectivism and Individualism are best characterized as competing theses regarding the conditions for theoretical success. Roughly speaking, theoretical success conditions are the conditions that a theory must meet in order for that theory to be the best among all available options. Theoretical success conditions are important insofar as they guide how one evaluates the methods employed by a field. One must evaluate a method in accordance with how efficiently it accomplishes its goal. Since theoretical success conditions determine what

the goal of research methods are, different formulations of these success conditions will lead to different standards regarding how to evaluate those methods.

The purpose of establishing success conditions for a theory is to get clear on what our methods should be aimed at doing. By (MP3), this will provide us with a starting point for evaluating these methods. By defining theoretical success in terms of the epistemic states of individual researchers, the Individualist thereby commits herself to evaluating the methods employed in theoretical inquiry in terms of their contributions to the epistemic states of individual researchers. In contrast, the Collectivist defines the successfulness of a theory in terms of objective metrics such as simplicity, explanatory power, etc., and in terms of how the theory is supported by a single communal body of evidence. The Collectivist attempts to explain how something falls within this body of evidence in terms of methods that are subject to objective standards of evaluation that are agreed upon by the community of researchers. For the Collectivist, academic research is a team sport, and like any team sport it is subject to rules.

As we shall soon see, there is a stark contrast between Individualism and Collectivism when it comes to the evaluation of intuitions as philosophical tools. As far as the Individualist is concerned, the question as to whether intuition is a valid philosophical method simply reduces to a question about the epistemic status of intuitions. Thus for Individualists, showing that individual researchers can know a theory to be true by relying upon intuition is tantamount to showing that appeals to intuition are acceptable philosophical methods. In contrast, the Collectivist holds that appeals to intuition must pass additional tests in order to be considered valid forms of evidence for philosophical theories. In particular, they must be subject to objective standards of

evaluation that (1) can tell us whether they are being used properly, and (2) tell us how they should be weighted in relation to other forms of evidence. The purpose of this section is to show that Individualism is false, and that the Collectivist is correct. This will have very important implications for the methodology of intuition.

Before we begin to examine Individualism, we should say a bit more about what exactly is meant here by 'theoretical success conditions'. Our current aim in arriving at a notion theoretical success is to describe what our methods are aimed at achieving, which will in turn allow us to evaluate those methods according to how well they accomplish those aims. The relevant notion of theoretical success should thus be robust enough to guide our evaluation of the methods employed in theoretical inquiry even if we are ignorant of the truth of our theories. We should thus avoid characterizing theoretical success solely in terms of the truth or falsehood of our theories.[22] Of course, neither do we need to deny the factivity of theoretical success (or related notions such as evidence) on a conceptual level. It is simply more convenient for us to focus on the putative evidence for theories instead of their truth.[23] Yet it is also be obvious that the notion of theoretical success should not be relativized to what individual researchers think counts

[22] A purely factive conception of theoretical success would also likely limit the evidential base for our methodological theories to only those methods employed in theories that we *now know* to be true. This would render the methods employed by past theoretical projects that turned out to be false irrelevant. However, it is quite obviously possible to learn from past mistakes. Moreover, the methodological theories concerning the sciences (e.g., the theories offered in the philosophy of science) also tend to take a non-factive approach insofar as they too examine past theories in terms of a notion of relative success by comparing these theories in terms of qualities such as simplicity and predictive accuracy. Since there is no clear reason why we should diverge from this practice regarding the methodology of philosophy, it is fairly clear that a purely factive conception of theoretical success will not be useful for our purposes.

[23] Note that this also helps explain how the current approach relates to Timothy Williamson's account of evidence, which is factive because it is identified with knowledge (2000a, 184-208). While we shall commonly proceed as though evidence is not factive this in no way commits us to a substantive conflict with positions such as Williamson's, as this sort of talk can be easily paraphrased by replacing occurrences 'evidence' with 'what appears to be evidence' and so forth without it thereby affecting the truth of our claims. Obviously if we know that a theory is false then that theory will conflict with our current evidence and will therefore not be successful even according to our quasi-objective definitions.

as a good theory. We can easily imagine individuals with obviously unacceptable beliefs in this regard (e.g., someone who believes a theory is successful iff it agrees with the tea leaves, etc.). Thus the notion we are after is thus *quasi-objective* insofar as it will aim to define theoretical success in terms of accessible factors such as consensus, successful prediction, explanatory power, simplicity, etc.

It is therefore important to note that Individualism is not a form of methodological subjectivism. The Individualist takes the conditions for the success of a theory to consist in the epistemic states of individual researchers regarding the content of the theory in question; *not* in their beliefs about whether the theory is successful. If Individualism requires knowledge it will be quite compatible with denying theoretical success in conditions where researchers may think it is present, which is obviously incompatible with subjectivism. Thus the Individualist will obviously deny that a theory *t* is successful for agent *s* simply because it agrees with *s*'s standards for theoretical success (e.g., because it agrees with the tea leaves). Rather, what is important is whether *s knows* the appropriate claims made by *t* to be true. This is an objective standard for theoretical success because it holds that there is a fact of the matter as to whether a theory *t* is successful that does not vary according to the perspective of each individual.

Generally speaking, the Individualist treats the conditions for theoretical success as consisting in what individual researchers know or believe in regard to the theory in question. Thus the first step toward a formulation of Individualism is to specify the propositional content of the relevant epistemic states. The Individualist is concerned with what individual researchers need to know in order for a theory to be considered successful. Now, it obviously will not do to simply have the proposition be about the

truth of the theory in question, i.e., the proposition [Theory *t* is true]. The problem is that someone could know [Theory *t* is true] simply on the testimony of a competent specialist without being at all familiar with the content of *t* itself. This piece of knowledge is obviously parasitic upon the knowledge of the specialist, and it is the latter that concerns the Individualist. While the Individualist can require that [Theory *t* is true] be among the relevant propositional contents, the Individualist is really after the epistemic states that regard the substantial content of *t*.

Let '*S*(*t*)' stand for 'theory *t* is successful'. Thus an analysis of success conditions will take the traditional form of '*S*(*t*) IFF …'.[24] Our purpose is to provide an *analysans* that can be used to guide our evaluation of methods. Once we know how to fill out '…', we can begin to evaluate methods in terms of how they can be said to lead to the conditions specified by '…'. The Individualist's analysis will need to explicitly show how the epistemic states of the individual regarding the theory relate to the evidence that supports the theory. We can handle this as follows. For any theory t_i, let $T_i = \{ p : p$ is relevant to the truth of $t_i\}$. (I leave the notion of 'relevance' here deliberately inchoate, except to note that the notion is intended to be objective rather than subjective.) For any propositional attitude *A* and any individual *s*, the sentence '*s A*s that *t* is true on T' is true iff *s* has *A* with regard to the proposition [*t* is true] and *A* is related in some suitable way

[24] Of course, as with most analyses, we are unlikely to arrive at a biconditional that achieves true synonymy and avoids all counterexamples. Fortunately, this is not what we are after. Thus we can add a tacit preface to the biconditional such that it reads 'Under non-extreme conditions, *S*(*t*) IFF …'. Here, 'non-extreme conditions' can be understood as the sort of conditions that have held for all theoretical investigations hitherto engaged in by mankind. Since we are only after a quasi-objective notion of theoretical success, we needn't be too worried about extreme skeptical scenarios anyway. For either the Collectivist or Individualist, a theory like special relativity should be considered equally successful in a qualitatively identical skeptical scenario wherein humanity is trapped within a simulation. The important point is that the Individualist and Collectivist would both offer the same methodological evaluations in either scenario because they would be operating on the same *putative* evidence. The same remarks about factivity apply as above.

to the contents of T.[25] Consider the following two formulations of Individualism:

(ISK) *S*(*t*) IFF philosophers who specialize in the area to which *t* belongs know that *t* is true on T.[26]

(ISJ) *S*(*t*) IFF philosophers who specialize in the area to which *t* belongs are justified in believing that *t* is true on T.

These formulations suggest the following approach to the evaluation of philosophical methods: establish that the method in question can lead to knowledge or justified belief.

Let us see briefly how the Individualist would take this approach to the evaluation of intuitions as methods. Suppose that part of the argument for theory *t* relies on appeals to intuition. This means that either one of the propositions that belong to T or one of the important premises in one of the arguments that serve as evidence for one of the propositions that belong to T are supported by an appeal to intuition. Let us call the proposition in question *p*. Let us define a propositional operator 'I' such that for any proposition *x*, I(*x*) = [It is intuitive that *x*]. Thus in our imagined case the evidence in support of *p* consists of the proposition I(*p*). According to (ISK) and (ISJ), theory *t* will be successful only if specialists know that *p* or are justified in believing *p* based on I(*p*).[27]

[25] Different forms of Individualism will vary in how they specify the content of T and what they take this suitable relation to be. The weakest formulation will simply demand the null case, where T = {[*t* is true]} and the suitable relation is simply identity. This would imply that all that is necessary for the success of *t* is that researchers hold the relevant attitude to the proposition [*t* is true]. Stronger formulations will require T to contain good evidence for *t*, and will treat the suitable relation to be doxastic justification. We will return to these issues in §1.3.2.

[26] Obviously we are ignoring the fact that knowledge is factive. If the Individualist could show that a theory is true, then she will have obviously shown that the theory is successful, regardless of how it might appeal to intuition. We will also largely ignore the distinction between knowledge and justified belief. Since knowledge involves a form of safety, the Individualist might be better off with (ISK) (see Williamson 2013b,c). However, this makes little difference for our considerations.

[27] Note that for now I am deliberately leaving the phrase 'based on I(*p*)' as ambiguous between either one of two meanings. It could mean that the individuals in question have based their belief in *p* on the belief that I(*p*); that is, on the belief that *p* is intuitive. This would seemingly imply some sort of inference from I(*p*) to *p* on behalf of the individuals in question. Alternatively, it could mean that the individuals in question form their belief in *p* non-inferentially in virtue of the *intuitive experience* they have when they consider *p*.

Now, suppose that someone wished to question whether I(*p*) is a legitimate form of evidence for *p* in this instance. There are three general ways in which they can do this. First, they can simply question whether I(*p*) is true (i.e., whether *p* is actually intuitive). Second, they can acknowledge that I(*p*) is true but question whether any proposition of the form I(*x*) should be counted as evidence (i.e., whether the appeals to intuition should count as a valid method, in general). Third, they can acknowledge that I(*p*) is true and that propositions of the form I(*x*) should be counted as evidence in general, but argue that in this particular circumstance there is contrary evidence *q*, such that *q* undercuts the evidential status of I(*p*), *q* is strong evidence for ~*p* that overrides the evidence for *p* provided by I(*p*), or *q* does both. In order to deal with these claims, the Individualist will need to answer the following questions: (1) What is an intuition? (2) Can an individual know that *p* based on I(*p*)? (3) How does the justification provided by I(*p*) interact with other forms of evidence for and against *p*?

For the Individualist, the question of whether appeals to intuition can serve as valid methods for establishing philosophical conclusions is to be answered by an epistemological account of how intuition works. If it can be shown that individual philosophers can come to know the appropriate propositions through intuition, this is sufficient for showing that appeal to intuition is a valid method. Alternatively, if it can be shown that individual philosophers *cannot* know the appropriate propositions through intuition—either because beliefs based on intuitions are not justified in general or because there are other forms of evidence that serve to defeat whatever justification intuitions provide—then this is sufficient for showing that the appeal to intuition is not a valid method.

This should sound familiar. The meta-philosophical debate about the role of intuition in philosophy largely turns on just these sorts of disagreements about the epistemology of intuition. However, the commitment to Individualism is hardly explicit. Indeed, I suspect that many of the philosophers who allow Individualism to guide their arguments would reject it upon hearing it explicitly formulated. Nevertheless, the fact is that Individualism, tacit though it may be, has managed to structure the entire debate over the methodological legitimacy of appeals to intuition. The meta-philosophical discussion regarding intuition never attempts to answer any question beyond whether intuitions are a source of *prima facie* justification. The supporters argue that is does, while the detractors argue that it doesn't. Virtually no one bothers to ask the question: "Even if we grant that intuitions are a source of *prima facie* justification, does this thereby imply that they are legitimate philosophical tools?" The assumption of Individualism provides the best explanation for this omission, for only on the assumption of Individualism is this inference warranted without additional argument. Nevertheless, it is worth providing a few examples to illustrate how widespread Individualism is as a methodological assumption.

1.2.1. Individualism as a Methodological Assumption

Before we show the folly of Individualism it will first be necessary to show how exactly it drives meta-philosophical discussions of intuition, lest we be accused of a non-sequitur. As indicated above, the primary influence of Individualism is established not in the explicit methodological commitments of meta-philosophers, but rather in the conspicuous absence of important questions in the meta-philosophical debates regarding intuition (i.e., in what is *not* said). This makes it a bit difficult to establish the influence of

Individualism. In order to accomplish this, we shall focus on the influence of a position dubbed by Timothy Williamson as *Philosophical Exceptionalism* (2007, 3) (hereafter 'Exceptionalism') in the meta-philosophical debate concerning intuitions. By showing how Exceptionalism presupposes Individualism, it is thereby easy to demonstrate the influence of Individualism.

Exceptionalism is the thesis that philosophy employs a different methodology than the sciences and attempts to operate independently of the sciences. As Williamson points out, Exceptionalism arises from a natural interpretation of the observation that philosophical research is conducted largely from the armchair. This is taken to imply that philosophy employs *a priori* methods, in contrast to the *a posteriori* methods employed by the sciences.[28] This Exceptionalist picture naturally presupposes Individualism, for it evaluates philosophical research in terms of what the individual philosopher can know from the armchair. Indeed, the armchair metaphor itself immediately suggests that philosophy is an individualistic enterprise.[29]

In his argument against Exceptionalism, Williamson provides a useful characterization of the position has influenced the current meta-philosophical space. Williamson identifies three extreme methodological positions one might adopt in regard to philosophical practice. *Crude rationalism* affirms the supposed *a priori* methods of

[28] Williamson describes Exceptionalism as the assumption that "the *a priori* methodology of philosophy is profoundly unlike the *a posteriori* methodology of the natural sciences; it is no mere difference between distinct applications of the same underlying methodology" (2007, 2).

[29] Williamson begins his discussion of philosophical exceptionalism with the observation that "traditional methods of philosophy are armchair ones: they consist of thinking, without any special interaction with the world beyond the chair" (2007, 1). Williamson states that "To do justice to the social and not solely individual nature of philosophy, as a dialectic between several parties, we should add speaking and listening to thinking, and allow several armchairs, within earshot of one another" but then quickly shrugs this off by claiming that "methodologically that brings philosophy little closer to the natural sciences" (2007, 1).

philosophy by arguing that these methods produce reliable knowledge of facts independently of empirical methods (2007, 1). In contrast, *Crude empiricism* takes the supposed *a priori* methods of philosophy to be unreliable because they cannot be confirmed through empirical means (ibid). The third approach, *Conceptualism*, takes the supposed *a priori* methods of philosophy to be reliable, but at the cost of only revealing facts about the nature of our concepts or language rather than the world.[30] All three positions presuppose Exceptionalism insofar as they take the observation that philosophy is practiced from the armchair as an explanatory guide.

According to these positions, the methodology of philosophical boils down to a dispute about whether individual philosophers can actually attain *a priori* knowledge just by sitting in their armchairs. This comes to a head in the debate about intuitions. Crude rationalists attempt to vindicate the legitimacy of philosophical research by showing that intuitions give us *a priori* knowledge. Crude empiricists attempt to undermine the legitimacy of philosophical research by showing that *a priori* knowledge through intuition is impossible. Conceptualists attempt to limit the legitimacy of philosophical research by showing that our intuitions are based in our conceptual competence, and thus can only give us *a priori* knowledge of conceptual truths.[31]

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[30] According to Williamson, the conceptualist holds that *a priori* methodology is not capable of "answering ordinary factual questions" as this "is best left to the natural sciences with their *a posteriori* methodology" (2007, 2). Rather, because "confinement to an armchair does not deprive one of one's linguistic competence, whatever can be achieved through the exercise of that competence and reflection thereon will be a feasible goal for philosophy" (ibid).

[31] As Williamson puts it, "One apparently distinctive feature of current methodology in the broad tradition known as "analytic philosophy" is the appeal to *intuition*. Crude rationalists postulate a special knowledge-generating faculty of rational intuition. Crude empiricists regard "intuition" as an obscurantist term for folk prejudice, a psychological or social phenomenon that cannot legitimately constrain truth-directed inquiry. Linguistic or conceptual philosophers treat intuitions more sympathetically, as the deliverances of linguistic or conceptual competence." (2007, 2-3) Note also that we are ignoring related questions concerning philosophical "naturalism", primarily because the notion of naturalism itself appears to be hopelessly unclear (see Williamson 2014a,b).

All these positions share the assumption that the legitimacy of certain philosophical research projects and the evaluation of the methods employed in these research projects should be understood as a debate about whether individual philosophers can gain knowledge through intuition. These positions thereby also presuppose Individualism. Of course, one might object that these positions are only rough caricatures offered by Williamson to motivate his project. As Williamson himself points out, not many contemporary philosophers "have the nerve to be crude rationalists", and while many realist philosophers may "have some sympathy for crude empiricism," this "sympathy sometimes has little effect on their philosophical practice" (2007, 1-2).[32] Aside from a few idiosyncratic cases, one might question whether Exceptionalism really has that much of an influence on philosophical practice.

However, Williamson is not alone in describing philosophical methodology in terms of disputes about the validity of "armchair methods". For instance, in the introduction to recent anthology on philosophical methodology, Matthew Haug describes the current state of philosophical methodology as follows: "The last few years have seen a surge of interest in philosophical methodology. If there is a single question that unifies the disparate currents of this surge it is whether philosophical questions can successfully be answered "from the armchair."" (2014, 1). As long as this question serves as a framing device for the methodology of philosophy, Individualism is unavoidable.

[32] Moreover, Conceptualism seems to function largely as a foil for Williamson's project regarding vagueness. Williamson characterizes Conceptualism as claiming that the question "Was Mars always either dry or not dry?" and similar formulations are questions about language (2007, 23-47). While Williamson shows that this position is unreasonable, the characterization of the position itself is somewhat hard to swallow. Instead of making the semantic claim, a philosopher of a Conceptualist bent can just as easily admit that the question itself is about Mars, but that given our background knowledge we can see that there is no obvious way to answer the question and thus take it to *pragmatically* express a question about language. This, of course, does not invalidate Williamson's argument, though it does suggest that it is rhetorically misdirected.

Drawing on Haug's discussion, we can identify at least four ways in which one might attempt to answer this question:

	Pro-Armchair	Anti-Armchair
Exceptionalist	Rationalism: Armchair methods are validated by the "autonomy and authority of a priori reflection, introspection, or the use of intuitions" (Haug 2014, 2).	Empiricism: The "concerns about the reliability of armchair methods" constitute "an argument for abandoning some or all of these methods" (Haug 2014, 1).
Anti-Exceptionalist	Expertism: Judgments made from the armchair are valid because they are grounded in the expertise of philosophers, not because philosophers possess some special faculty of intuition.[33]	Collaborationism: There needs to be "cooperation and mutual reinforcement, rather than conflict, between empirical methods and armchair reflection" (Haug 2014, 1). [34]

What is important for our current purposes is the fact that the apparent disagreement between these positions is primarily framed in terms of epistemological claims concerning modes of knowledge, and not broader questions concerning intersubjectively available standards of evaluation. The focus is the distinction between the types of knowledge gained from reasoning versus observation. There is generally no mention of the fact that the sciences also differ from philosophy in terms of the fact that they employ methodological standards such as the demand for replicability and a low *p*-value. Thus most of the people who take part in the debate about intuition seem to have presupposed

[33] Armchair "methods are viable, not because they are fundamentally different from those used by the natural sciences, but because they are *no different in kind* from empirical methods" (Haug 2014, 2).

[34] It is trivial that most metaphysicians are 'Collaborationists' in the sense that they usually will not dismiss evidence from the sciences (e.g., the role of special relativity in the metaphysics of time, etc.). But I take it that the real notion of Collaborationism that Haug is concerned with is one on which x-phi is used as a way to evaluate the use of intuition. Thus Haug describes the difference between Expertism and Collaborationism is that the latter sees armchair and scientific inquiry as employing "epistemically distinct kinds of method (perhaps mapping onto the a priori/a posteriori distinction), which, in principle, could either support or conflict with one another", whereas the former view claims "either that the a priori/a posteriori distinction is itself confused or that armchair methods are themselves a posteriori" (Haug 2014, 22).

some form of Individualism. To demonstrate this we will focus on what particular authors have said. Our examples include a Rationalist (George Bealer), an Empiricist (William Lycan), an Expertist (Timothy Williamson), and a Collaborationist (Jonathan Weinberg). (I have chosen these authors as examples because they have engaged one another explicitly on the issue of the use of intuitions in philosophy.)

1.2.1.1. Example 1: How Rationalists Assume Individualism

Let us first consider an example of how Rationalists presuppose Individualism. The work of George Bealer is illustrative because his view of methodology is based on epistemological arguments regarding intuition. Bealer argues for two major methodological theses:

> **The Autonomy of Philosophy** Among the central questions of philosophy that can be answered by one standard theoretical means or another, most can in principle be answered by philosophical investigation and argument without relying substantively on the sciences. (1996a, 121)

> **The Authority of Philosophy** Insofar as science and philosophy purport to answer the same central philosophical questions, in most cases the support that science could in principle provide for those answers is not as strong as that which philosophy could in principle provide for its answers. So, should there be conflicts, the authority of philosophy in most cases can be greater in principle. (1996a, 121)

Bealer provides two arguments for these positions: the "Argument from Concepts" and the "Argument from Evidence" (1996a, 121). The Argument from Concepts proceeds as follows. First Bealer argues that the only way to show that intuitions are legitimate sources of evidence is to show that they are "modally reliable".[35] He then argues that

[35] Bealer's argument for this point is patchy at best. He does attempt to defend the claim against specific versions of empiricism, coherentism and Platonism in the context of whether these views can support "scientific essentialism" (i.e., the view that we can know some necessary truths like water = H_2O *a posteriori*) (1987). However, there are many more theories that he does not consider. In a later publication he refers readers to these arguments before making the following proclamation: "*Philosophical Limits of Science* I develop these arguments [for modal reliabilism] in detail, dealing there with various alternative explanations—pragmatist, coherentist, conventionalist, contextualist, and

intuitions are modally reliable because they utilize terms that are "semantically stable" (1996a; 1996b; 1998). Bealer argues that there are good reasons to think that the intuitive judgments made in philosophy use semantically stable terms, and concludes that these judgments operate independently of any empirical data from the sciences (1999, 49).

The Argument from Evidence proceeds in two stages. In the first stage, Bealer outlines what he takes intuitions to be and how they differ from other forms of belief. Given this characterization, he then argues for the (seemingly descriptive)[36] claim that standard philosophical practice utilizes intuitions as evidence for philosophical theories by engaging in what he calls a "standard justificatory procedure":

> Among our various theoretical beliefs, some are deemed to have *a priori* justification. This occurs for beliefs arrived at by a procedure that suitably approximates the following idealization: (1) canvassing intuitions; (2) subjecting those intuitions to dialectical critique; (3) constructing theories that systematize the surviving intuitions; (4) testing those theories against further intuitions; (5) repeating the process until equilibrium is approached. The method philosophers standardly use to establish answers to central philosophical questions closely resembles this procedure of *a priori* justification. (1996a, 122)

In the second stage, Bealer attempts to show that empiricism and any other view that diverges from this methodological picture by denying that intuitions are evidence will be self-defeating.[37]

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rule-based (or practice-based). In the present context, I will assume that these arguments are successful and that we must turn to a truth-based explanation" (1999, 34). However, to the best of my knowledge this work has never been published.

[36] Bealer states that he intends to lay out what he thinks are "some plain truths about the procedure we standardly use to justify our beliefs and theories" which he takes to show that "according to our standard justificatory procedure, intuitions are used as evidence (or as reasons)" (1996a, 122).

[37] Roughly, his arguments attempt to show that these philosophical theories themselves can only be justified by appeal to the standard justificatory procedure, and thereby depend for their justification upon appeals to intuition (1992; 1996a; 1996b). We will deal with these arguments in greater detail later.

It is clear from the structure of both arguments that Bealer has not thought deeply about the distinction between how individuals are justified in their beliefs and how the philosophical community gathers evidence.[38] Insofar as it focuses on the judgments made by individuals and the concepts they possess, the Argument from Concepts quite obviously presupposes Individualism. In the Argument from Evidence, Bealer draws no real distinction between the process in which individual philosophers receive *a priori* justification for their beliefs and the methods employed by the field of philosophy to evaluate theories. Thus Bealer is clearly committed to Individualism.

1.2.1.2. Example 2: How Empiricists Assume Individualism

Since Bealer does not acknowledge the gap between individual and community he thinks that the only challenge to using intuitions as evidence must come in the form of epistemological skepticism regarding intuitions. This is why he directs his Argument from Evidence against views that would deny that intuitions are *prima facie* forms of evidence. This is perfectly reasonable from a dialectical standpoint. The philosophers who argue against rationalists like Bealer tend to present their objections by arguing for general skepticism about intuition.

For instance, William Lycan accepts Bealer's Exceptionalist description of philosophical practice but rejects the validity of this practice because he thinks that beliefs formed from intuitions are epistemically unjustified. Lycan states that "Bealer is absolutely right about the indispensability of "intuitions" to philosophizing, and that the

[38] Indeed, the only important difference that Bealer recognizes is that philosophers sometimes use empirical evidence: "Perhaps the most important difference is that philosophers make occasional use of empirical evidence - specifically, we invoke actual "real-life" examples and actual examples from (the history of) science" and then tries to dismiss this by claiming that "such examples can, at least in principle, be dropped and in the their place one can use *a priori* intuitions affirming corresponding (not to say identical) *possibilities* which have equivalent philosophical force" (1996a, 122-3). The possibility that different philosophers might have different intuitions or give different intuitions different evidential weight seems to have never even crossed his mind.

method of philosophy is ultimately that of "reflective equilibrium" … based, at least in large part, on intuitions" but goes on to state that "contra the spirit of Bealer's paper, that method and that basis for philosophy are feeble, and of very little epistemic authority" (1996, 143). Lycan's primary arguments against Bealer are supposed to show that Bealer's particular account of intuitive justification fails because "philosophical intuition is and always will be *laughably un*reliable" (1996, 144). Lycan provides a twin earth like counterexample designed to show that the term 'consciousness' is not semantically stable in Bealer's sense, which is supposed to show that intuitions about consciousness are *not* modally reliable (1996, 147-8). Lycan concludes by providing a general argument for the unreliability of intuition from the fact that the method of appealing to intuition has historically failed to drive toward consensus (ibid, 148-9). Bealer responds by criticizing Lycan's counterexample as confused (1996b, 165-7), and by arguing that intuitions *have* in fact produced consensus in many occasions (ibid, 163). The point of contention centers on whether and how intuitions can supply individuals with evidence, with each side arguing that intuitions are either globally justified or unjustified.

The notion of evidence used by Bealer and Lycan is Individualistic. First of all, Bealer explicitly defines evidence as relativized to individual subjects.[39] Second, Bealer argues that it is the propositional content of an intuition that is the evidence, not the proposition that one has an intuition.[40] It is the fact that this propositional content is accompanied by an occurrent intuitive experience where the content *seems* true that

[39] For instance, Bealer defines 'derived evidence' as follows: "something is a derived source of evidence relative to a given subject iff it is deemed [by the subject] (perhaps unreliably) to have a reliable tie to the truth by the best comprehensive theory based on the subject's basic sources of evidence" (1996a, 128-9).

[40] Thus Bealer states that "When we say that an intuition counts as *prima facie* evidence, we of course mean that the *content* of the intuition counts as *prima facie* evidence", and goes on to argue that the introspective proposition that one is having an intuition counts as separate evidence (1992, 130).

makes this proposition count as evidence for the individual. Thus the only sense in which Bealer can allow that individuals collectively have the same intuitive evidence is if all those individuals happen to have the same mental states regarding the proposition in question. This is precisely what makes the distinction between Individualism from Collectivism so important, for Bealer's account of intuition can only function as a methodological guide so long as there is no disagreement amongst individual philosophers about what is, in fact, intuitive.

Although Lycan does appeal to consensus in his argument that intuitions do not constitute good evidence, his argument is *not* that philosophical evidence must be intersubjectively available or that a method can only be permissible if it can lead to consensus. Rather, his point appears to be that knowledge of the fact that intuitions historically do not lead to consensus acts as a defeater for whatever evidential status these intuitions might originally have had *for individuals*.[41] Since his appeal to consensus is confined to how said consensus affects individual belief, he is no less an Individualist than Bealer.

This sort of back-and-forth appears to be the status quo for the debates surrounding intuition. Those who support the use of intuition offer global epistemological theories that are intended to show that all intuitions have default evidential status, while those who are opposed to the use of intuitions offer equally global epistemological considerations that are intended to show that all intuitions *lack* default evidential status.

[41] Note that Lycan in fact endorses a form of defeasibilism about intuition insofar as his particular position grants that intuitive seemings possess evidential status "until *at least some slight reason* is given for suspecting them guilty" (1996, 146). Thus he appears to be arguing that intuitions are globally *defeated* by the fact they do not produce consensus, which is distinct from the claim that they lack evidential status to begin with. However, this fact becomes lost in the dispute since Bealer focuses solely on Lycan's criticisms of modal reliabilism.

For instance, consider Joel Pust's entry on intuition in the *Stanford Encyclopedia of Philosophy*.[42] When summarizing the extant criticisms leveled against the use of intuition in philosophy, Pust only considers objections that come in the form of global skepticism about intuition.[43]

As noted above, these general epistemological disputes seem to be driven by the assumption that philosophy is an "armchair" pursuit. Those like Bealer who defend the evidential status of intuition are clearly motivated by a more general desire to preserve the traditional status of philosophy. Correlatively, those like Lycan who attack intuition tend to do so because they think that very little progress has been made in philosophy, and that this is precisely because philosophers *cannot* know things from the armchair. Insofar as Lycan accepts Bealer's description of philosophical practice, he too is committed to Exceptionalism (as a descriptive claim). Thus we might ask whether meta-philosophers who reject Exceptionalism might thereby manage to avoid Individualism. Unfortunately, we have no such luck.

1.2.1.3. Example 3: How Expertists Assume Individualism

While Williamson explicitly rejects Exceptionalism he still ultimately adopts an account where philosophical theories are evaluated from the perspective of individuals.

[42] I take the example to be informative for three reasons: (1) the SEP is a selective and peer-reviewed publication that servers as a standard and generally well-respected general resource, (2) SEP entries are intended to be accurate summaries of the work that has been done specific philosophical topic, and are expected to be updated in light of new work, and (3) SEP entries are expected to present the different views on a subject accurately and with a fair amount of neutrality. Taken together, these constitute good reason to take the failure to present an argument within an SEP entry as evidence that that line of argument has not been presented or has been ignored. (Of course, this is only evidence of an oversight if the line of argument in question happens to be *good*: it is perfectly reasonable to ignore bad arguments. However, since my later defense of defeasibilism is intended to establish that it does, in fact, constitute a promising criticism of intuition, we can here focus solely on the ubiquity of Individualism.)

[43] In particular, he considers three main types of objections: arguments that intuitions are not evidence because they cannot be independently calibrated (2014a, §3.2); arguments that intuitions are not evidence because they can be shown to be generally unreliable (2014a, §3.3); and arguments that intuitions are not evidence because their evidential status cannot be explained (2014a, §3.4).

At several points Williamson says things about communal evidence and the social nature of philosophy that have clear Collectivist implications.[44] Unfortunately, when Williamson actually considers the methodological role of intuition he allows these statements seem to fall by the wayside. We will discuss Williamson's argument in greater detail in §2.1.4. For now, it is enough to note the following. First, Williamson suggests that "intuitions" are really just judgments, and thus he takes those who question intuitions to really be "judgment skeptics" (2007, 215-225). Williamson argues that appealing to intuition inappropriately psychologizes philosophical evidence, and that we should instead simply treat the content of our judgments themselves as evidence. In order to establish this Williamson rejects "Evidence Neutrality", which is the principle that we should accommodate challengers by only admitting evidence that is neutral between our position and theirs.[45] Williamson argues that Evidence Neutrality is false because there is no need for one group to try to persuade another by giving up what they take to be evidence.[46] On its face, this argument would appear to imply the denial of any legitimate methodological requirement that the philosophical community use a standard set of methods subject to collective evaluation, regardless of what Williamson says elsewhere.

[44] At various points Williamson acknowledges that theoretical inquiry involves communal knowledge. See the quote by Williamson in §1.2.1.5 below as well as the discussion in §2.1.4.2.

[45] Evidence Neutrality is roughly the idea that disputants should try to reach agreement about what counts as legitimate evidence (2007, 208-246). We will return to this in §2.1.4.

[46] For instance, Williamson summarizes why it is reasonable to reject Evidence Neutrality as follows: "Having good evidence for a belief does not require being able to persuade all comers, however strange their views, that you have such good evidence" (2007, 212). This quote is hardly idiosyncratic. Here are two more examples of such Individualistic language (my underlining): (1) "Sometimes, in self-defense, one must abandon skeptics to their fate" (2007, 239). (2) "In reaching one's views, one does not restrict oneself to premises and forms of inference acceptable to judgment skeptics, for one regards their restricted evidence base as too willfully impoverished to constitute a reasonable starting-point for inquiry" (2007, 239).

1.2.1.4. Example 4: How Collaborationists Assume Individualism

Collaborationists hold that empirical methods should be used in concert with armchair methods. In particular, what is really at issue is whether empirical methods can be applied to *intuitions*. The Collaborationists under consideration are philosophers who advocate 'Experimental Philosophy' (or 'x-phi' for short) in order to evaluate the use of intuitions in philosophy.[47] Such Collaborationists can be broken down into two main camps in regard to the role they envision for the use of experimental methods in regard to intuition. Some envision a positive role, where experimental evidence is largely used to confirm the accuracy of philosophical intuitions. Others see such evidence as playing a negative role, in that they suspect that it will show philosophical intuitions to be unreliable. However, for our current purposes this distinction is not important. What is important is the fact that both camps expect experimental evidence to have some bearing upon the evidential status of intuitions.

Insofar as said empirical evidence can be seen as a methodological check on the use of intuitions, much as replicability is a methodological check on empirical evidence gathered from experiment, Collaborationists would obviously benefit from rejecting Individualism in favor of Collectivism. Collectivism would allow Collaborationists to argue that we should use experimental data to check and calibrate our use of intuitions because it is our obligation as researchers to critically reflect on our use of methods. Unfortunately, Collaborationists also tend to focus on claims about individual knowledge.

[47] The exact nature of this experimental evidence should be understood broadly as consisting of (1) evidence from psychology and neuroscience concerning the cognitive mechanisms underlying intuitions, (2) evidence from social psychology about the effects of social factors on intuitive judgments, and (3) evidence about the intuitive judgments of experts or non-experts that is gathered from surveys in which subjects are presented with philosophical cases. The term 'Experimental Philosophy' is largely associated with the latter sort of method, but the distinction does not make much of a difference for our current purposes.

They have underplayed their hand insofar as they have not explicitly questioned Individualism.

As an example, let us consider how Jonathan Weinberg defends the use of experimental evidence against Williamson's argument. Weinberg starts off in a promising manner by distinguishing intuitions from the use of intuitions as a research method:

> Although Williamson focuses largely on judgments, really it is not a type of mental activity *per se*, but rather a *method*—a set of practices—that is both the positive target of Williamson's arguments, and the negative target of mine and other critics.[2] The powers of human judgment may be fine for some purposes, and not for others, and most interesting methodological questions here can only be framed in terms of what we might want to *do* with these judgments, and in what practices[3] we might look to deploy them. (Weinberg 2009, 456)[48]

Weinberg seems to acknowledge the fact that it is not the epistemic status of the intuitions themselves that is at issue in his disagreement with Williamson, but rather the status of the method of appealing to intuitions in order to establish philosophical claims. If Weinberg were to take this distinction to heart, we would expect his argument to center on whether *appeals* to intuition are reliable, rather than whether the intuitions themselves are reliable. Unfortunately, this is not what Weinberg does when he presents what he calls "*the experimentalist's challenge*" (2009, 456). The experimentalist's challenge consists of two claims. The first claim is that experimental evidence suggests that the judgments themselves are subject to variations that imply that they are unreliable.[49] The

[48] Weinberg is following Williamson in treating intuitions as mere judgments. However, the point is irrelevant to our current concerns.

[49] Weinberg states that the experimentalist's challenge is "based on a growing body of experimental work that suggests that judgments of the sort that philosophers rely upon so centrally in this practice display a range of inappropriate sensitivities" (2009, 456). Thus the primary focus of the experimental challenge is the claim that "there is some evidence that the judgments vary systematically with factors that one would not expect to track the relevant philosophical truths" (ibid).

second claim attempts to link these findings to a methodological judgment by claiming that these sorts of variations present a *prima facie* challenge to the method of appealing to intuitions.[50] The problem is that Weinberg spends all of his time arguing for the first claim, while taking the second claim for granted.

This becomes apparent when one considers Williamson's response to Weinberg. The main point of contention between Weinberg and Williamson concerns who bears the burden of proof. Weinberg and other Collaborationists argue that the presence of evidence suggesting that philosopher's judgments might be skewed should be enough to force philosophers to rethink the use of these judgments as evidence. They argue that the presence of variability in folk judgments is enough to accomplish this, and that those who would defend the use of intuitions bear the burden of proof with regard to the claim that the intuitions of experts differ in some relevant respect from those of the folk. In contrast, Williamson argues that since experimental evidence could potentially undermine the judgments of any professionals, the mere possibility that such evidence might be produced is not enough to show that professionals should give up such judgments. The burden of proof lies on the experimentalist to show that the specific judgments that philosophers use are in fact faulty. Williamson's objection turns on the fact that the intuitions that are actually used in philosophy in order to establish philosophical conclusions are the intuitions that belong to professional philosophers. The problem is that the studies that Weinberg utilizes in his experimentalist's challenge gather the intuitions of non-experts. Thus Williamson argues that in the absence of evidence linking the intuitions of non-experts to the intuitions of professional philosophers, the

[50] Weinberg states that "Unwanted variation[8] in any source of evidence presents a *prima facie* challenge to any practice that would deploy it" (2009, 457).

experimental evidence gathered thus far does not make a direct ruling on the appeal to intuitions as a philosophical method.[51]

The main point of focus in this dispute concerns the nature of the psychological data regarding intuitions.[52] However, the real underlying point of contention concerns when it is appropriate for philosophers to question their methods. The problem is that this question can only be solved by a general reflection on the nature of methodology, but both Weinberg and Williamson are too focused on the individual intuitions to really

[51] Weinberg does not offer an explicit argument that philosophers lack expertise, but rather states that "Whether or not philosophers are, in the sense appropriate here, experts in their judgments, is just one more philosophically-important question that cannot be addressed well from the armchair" (2009, 462 [footnote 12]). The only thing that Weinberg offers in support of the link between the judgments of non-experts and experts is the existence of two psychological studies on expertise. Williamson thus spends most of his reply analyzing what is said in those articles, and the "literature on expertise that Weinberg cites does not constitute even a *prima facie* challenge to the natural assumption that there is real expertise in armchair philosophy" (2009b, 473). Williamson argues that even though "Experimental results can in principle undermine the procedures of any intellectual community," it does not follow from this general fact "that every intellectual community should suspend its procedures until the relevant experiments have actually been done and shown to have reassuring results, otherwise all inquiry would come to a halt, since the procedures for interpreting experimental results would themselves have been suspended" (2009b, 471-2). Because of this, Williamson argues that "the experimentalist challenge to the armchair methods of philosophy must do better than appeal to the mere sceptical possibility of seriously disquieting experimental results" (2009b, 472). Rather, the experimentalist must show that these "results must actually have been obtained, if the challenge is to attain any urgency" (ibid). This dialectic is continued in Weinberg et. al. 2010 and Williamson 2011. In the former paper, Weinberg and colleagues argue that Williamson's expertise defense "deploys a substantive empirical claim, that philosophers' training indeed inculcates sufficient protection from such mistakes" that occur for non-experts (Weinberg et. al. 2010, 331). They "canvass the psychological literature on expertise, which indicates that people are not generally very good at reckoning who will develop expertise under what circumstances" (ibid). They then argue that under any specific conception of expertise the psychological evidence still does not "seem to provide us with good reason to endorse this key empirical premise of the expertise defense" (ibid). In response, Williamson argues that Weinberg and company "have misconstrued the dialectical situation" (2011, 215). Williamson argues that since Weinberg et. al. "have produced no evidence that philosophical training is less efficacious for thought experimentation than for other cognitive tasks for which they acknowledge that it produces genuine expertise, such as informal argumentation, they have produced no evidence for treating the former more sceptically than the latter" (ibid).

[52] While I am sympathetic to the experimentalist's challenge, it is hard to deny that Williamson is correct regarding the burden of proof. Clearly what is relevant to the use of intuitions in philosophy is whether *philosophers'* intuitions are subject to the sort of unwanted variability in question. Of course, this is not to say that the psychological data about folk judgments cannot undermine other claims. Indeed, this sort of data is capable of undermining appeals to common sense, as well as claims about conceptual competence that require a majority of the non-expert population to make specific sorts of judgments. For instance, consistently faulty judgments among non-philosophers *are* a problem for someone who defends an understanding-based view of intuition that appeals to conceptual competence. However, this is beside the point.

engage in this general reflection. Since both parties generally agree on the nature of the psychological evidence, the real question that must be settled concerns the duties of philosophers regarding when they should and should not use certain methods. In order to answer this question one must establish the *source* of these duties. The Individualist will claim that philosophers are only required to reflect on their methods insofar as evidence might undermine their justification for believing in the theories obtained by these methods. The Collectivist will argue that philosophers have a duty to reflect on their methods that is imposed upon them by a more general duty to work together to get at the truth. The different answers have different implications regarding what we should do.

1.2.1.5. Why Individualism is Dominant

The goal of §1.2.1 was to establish that the current dialectical space regarding the methodology of intuition has been tacitly constrained by Individualist assumptions; not that these philosophers themselves are deeply committed to Individualism at a personal or professional level. The upshot is this: if Individualism is implausible (as it will be shown to be in §1.3), then the current debate about the methodology of intuition is ill-formed. It is not necessary for our arguments that philosophers like Bealer, Lycan, Williamson, or Weinberg be *explicitly* committed to Individualism. Indeed, given other things they say it is reasonable to think that many of these philosophers would explicitly embrace Collectivism, given the chance. For instance, if we put aside his specific argument against judgment skepticism,[53] Williamson expresses Collectivist views of philosophical methodology at many points. This becomes particularly prevalent in the Afterword of

[53] In the next chapter we will discuss Williamson's arguments in greater detail. It will become obvious from that discussion that Williamson's arguments against Evidence Neutrality would incur devastating results for his own positions if taken as a response to anything beyond general skepticism about judgment. Thus it is almost certain that Williamson, when faced with a choice, should choose to give up these arguments rather than Collectivism.

The Philosophy of Philosophy, wherein Williamson gives us as explicit an endorsement of Collectivism as can be hoped for:

> Much contemporary analytic philosophy seems to be written in the tacit hope of discursively muddling through, uncontrolled by any clear methodological constraints. That may be enough for easy questions, if there are any in philosophy; it is manifestly inadequate for resolving the hard questions with which most philosophers like to engage. All too often it produces only eddies in academic fashion, without any advance in our understanding of the subject matter. Although we can make progress in philosophy, we cannot expect to do so when we are not working at the highest level of intellectual discipline. That level is not achieved by effortless superiority. It requires a conscious collective effort. (2007, 286)

Between this and other things Williamson says, I think it is reasonable to conclude that he is more likely to resolve any tensions in his view by giving up those arguments that conflict with Collectivism. I suspect the same is likely to be true for philosophers like Lycan and Weinberg.

The point is that Individualism guides the dialectical structure of arguments. Thus it is not what these philosophers say that is important but rather what they have neglected to consider.[54] As §1.2.1 demonstrates, the primary methodological disputes concerning intuitions are cashed out in terms of how individual philosophers are justified in believing that their theories are true. What is important is the fact that these philosophers do not provide *additional* arguments that link their claims about what individual philosophers can know to claims about how the success of a philosophical theory is to be measured. Since they presumably intend their arguments to have implications for philosophical practice, some such link is required. The most obvious way to provide the necessary link is by assuming something along the lines of (ISK).

[54] Though of course there are also members of the philosophical community that openly embrace Individualism, if not outright subjectivism. A particularly salient example is provided by Michael Martin's criticism of Williamson, which we shall consider in §1.3.1.1.2 below.

The dominance of Individualism is not too difficult to understand. The primary source of data for the methodology of philosophy consists of disputes between individuals, which are naturally understood in terms of competing knowledge claims. So it is hardly unexpected that philosophers would attempt to analyze these disputes in terms of epistemological theories that they are already familiar with from first-order philosophical theorizing. The likely assumption is that the link from individual knowledge claims to claims about theoretical success can be provided straightforwardly in principle by simply considering how individuals interact. Moreover, the evidence that individual practitioners possess often corresponds with the evidence possessed by the community. This suggests that one can account for the success of a philosophical theory just by considering disputes in idealized epistemological terms while taking the link between individual belief and collective evidence for granted. As it turns out, these assumptions are not at all as innocent as they seem to be. The next section is devoted to showing exactly what is wrong with Individualism.

1.3. Against Individualism

The aim in arguing against Individualism is to rule out the following type of objection to the conclusions we shall draw about the methodological status of intuitions:

> The only thing relevant to the role of intuitions in philosophy is whether individual philosophers can know their claims to be true through intuition. Unless presented with definitive proof that this is not the case, individual philosophers who reasonably believe that such knowledge is possible are under no burden to give up their intuitions simply because other philosophers question them.[55]

It is doubtful that any knock-down skeptical argument against intuitions can be provided. Thus if this objection were successful, it would leave philosophers who subscribe to some form of rationalism free to appeal to intuitions as they see fit, even if others do not accept their intuitions as viable evidence.

This objection only works on the assumption of Individualism. Individualism, in turn, only works if one assumes that the methodology of academic research is reducible to epistemological considerations regarding individual researchers. In essence, Individualism rests upon the assumption that academic philosophy is not really a team sport. The objector is implicitly committed to the view that academic philosophy consists of a haphazard group of individuals pursuing questions that interest them and who occasionally share their results in the promise of mutual benefit. If philosophers reach

[55] Note that I am ignoring an even cruder response in which the objector simply asserts that some form of methodological subjectivism is true. Such a response would imply that different metaphysicians writing about the very same topic can legitimately employ whatever methods they see fit, even if these methods lead to contradictory results. I ignore this response simply because (1) it is deeply implausible on its very face, and (2) most of my arguments against Individualism will apply equally to methodological subjectivism *a fortiori*. Thus in the interest of charity and time constraints I have simply presumed that the objector embraces Individualism. Understood in this way, the objector is claiming that the disagreement about intuitions does not concern the general criteria for what should count as a valid method: the assumption is something like (ISK) is correct. Rather, the dispute is over the particular epistemological facts concerning whether appeals to intuitions meet this criteria. Since which of the competing epistemological theories is correct is an open philosophical question, one side is in no better a position than the other. Why, the rationalists ask, should they give up a method they consider to be valid simply because other philosophers embrace competing epistemological theories? Why shouldn't those philosophers be forced to accept intuitions instead?

intractable disagreement there is no longer mutual benefit and thus no reason for these philosophers to continue to interact with one another.

If the objector were correct, it would mean that academic philosophy is a free-for-all. It would mean that the ability to publish one's research is only limited by one's ability to find a publisher that is willing to print the idea. It would mean that the discipline as a whole has no legitimate authority over the methods employed by individual researchers. It would mean that individuals are free to ignore criticism so long as it has not been definitively shown that their views are inconsistent. It would mean that different researchers are free to consistently use different methods to arrive at contradictory results without being subjected to any real pressure to square their methodological principles.

For many philosophers, these implications should already be enough to dismiss Individualism. For these philosophers (myself included), academic philosophy very clearly *is* a team sport, and like any team sport it can only function if there are enforceable rules. To build upon this metaphor: one needs to establish what the rules are before one starts playing a match. If one allows individual players to disagree about what the rules are, chaos will inevitably ensue. If different players continue to play according to different rules, the game will simply break down entirely. Analogously, we cannot engage in meaningful philosophical inquiry when different philosophers use different methods and standards of evidence. By continuing on in this manner we will only end up spinning our wheels.

Nevertheless, there are likely many other philosophers who would not be willing to admit this picture of philosophical practice. Most of the self-identified rationalist

philosophers who appeal to intuitions more than likely belong to this latter group. These rationalists are more likely drawn to the romantic Cartesian image of the individual philosopher struggling to shut all the distractions of the world (including the objections of her contemporaries) in order to achieve knowledge of fundamental truths through the Light of Reason.

As the objection above demonstrates, the question of whether Individualism or Collectivism is true must be settled *prior* to our particular discussion of the methodological role of intuition in philosophy. Given that we can expect at least some philosophers to be sympathetic to Individualism, we must spend some time showing what is wrong with the position. We will outline some of the particular methodological problems posed by appeals to intuition at the end of the chapter. For now, we must concern ourselves solely with the viability of Individualism as a methodological assumption. In what follows I shall provide two arguments that—when presented in tandem—show that Individualism is deeply implausible.

The first argument presented against Individualism is *The Argument from Practice*. The argument proceeds in two stages. First, it is argued that Individualism fails as a description of how academic research is actually carried out in the sciences and mathematics, and that if Individualism were true it would mean that the sciences and mathematics should change the way they operate. Second, it is argued that there is no good reason to think that philosophical research differs from the research conducted in the sciences and mathematics in any way that is relevant to Individualism. This argument undermines any *prima facie* plausibility that Individualism might have had in virtue of preconceptions about the nature of philosophy.

The Argument from Methodological Primacy shows that Individualism has gotten the proper order of explanation backwards in assuming that theoretical success is dependent upon individual knowledge. First it is argued that the formulation of Individualism itself must tacitly presuppose Collectivist principles in order to achieve plausibility. Second, it is argued that the knowledge of individual researchers regarding the truth of their theories depends in several important ways on the collective actions of the discipline. This argument establishes that there are very good reasons to think that Individualism is false.

1.3.1. The Argument from Practice

One of the best arguments against Individualism is simply that it constitutes a deeply implausible explanation of academic practice. The primary purpose of academic research appears to be the production of publicly available explanations of phenomena.[56] In the sciences, these explanations are constructed in a very careful and deliberate manner. For instance, if a scientific experiment produces a result that appears to be in conflict with other well-established theories this will often lead to the demand that the methods employed in the original experiment be reexamined, or that the experiment itself be run again under further constraints. This examination might reveal that the instruments were flawed or that the results were misinterpreted and this will allow the results to be dismissed. Alternatively, the experiment might be reproduced without issue and this will demonstrate that there is a legitimate problem with the incompatible theory. Nevertheless, research that relies upon results that have been called into question is considered problematic, even if it is expected that the results will be reconfirmed.

[56] Recall from §1.0 above that the only non-cynical explanation for why academic research receives funding is that it is aimed at producing an objective description of reality.

The best explanation of this practice is that there are objective rules set down by the scientific community about what counts as good evidence and what counts as a valid method, and that experiments that do not meet these standards must be rectified as a matter of principle, even if everyone within the scientific community thinks that the results in question are correct. However, this practice becomes much more difficult to explain if one assumes that theoretical success is measured by individual knowledge, as Individualism demands. After all, if one has good reason to think that some oversight should not have had any significant effect on the results in question, why bother running the experiment again? Indeed, if one already has good reason to think that some theory is true then why run an experiment in the first place?

Our descriptions of research suggest that it is inappropriate to explain theoretical success in terms of the epistemic properties of individual members of a field. For instance, suppose a colleague asks one whether one is aware of anything in the literature to support *p*. The appropriate thing to say is something of the form "*S*'s research shows that *p*", or "*S* has given some convincing arguments that *p*", etc. One's colleague would almost certainly find it odd if one were to answer her by saying "*S* knows that *p*", or "*S* is justified in believing that *p*". This would sound like some sort of hedge, similar to "*S* thinks that *p*".[57] When one asks a scientist whether a scientific theory is established or has been proven successful, the scientist is likely to reply in terms of whether the theory has received a high degree of confirmation, not whether individual scientists know the theory

[57] Analogously, when we ask whether a philosophical theory is well-established or successful we do not usually phrase these questions as "Is *S* justified in believing *p*?" or "Does *S* really know that *p*?" Rather, we ask questions of the form "Has someone shown that *p* is plausible?", "Does *S*'s argument succeed in establishing *p*?" and "Has anyone succeeded in defending *p* against *q*?" Indeed, even the questions "Most people know that *p*" and "It has been established that *p*" seem to obviously differ in their implications. This is a problem for Individualism, since it essentially amounts to the thesis that the former of these two sentences (or some closely related sentence) is equivalent to the latter.

to be true.[58] Scientists prefer to speak of confirmation and disconfirmation because this is how they measure theoretical success. To say that a theory has received a high degree of confirmation from experiment and that those results have been successfully reproduced is precisely to convey the appropriate information when questioned whether a theory is successful. Even if one asks whether individual scientists know the theory to be true, this is generally only a roundabout way of asking about the level of confirmation the theory has received. We are often inclined to criticize scientists for claiming that they know a theory to be true when it has not yet been confirmed according to the field's own standards.

Consider a more concrete example. One might ask a set theorist whether she knows that the Axiom of Choice (AC) or the Continuum Hypothesis (CH) is true, and she may very well respond in the affirmative because she finds the theses intuitive and does not know of any reason to think that they are false. However, if one then asks her whether it has been shown or established that AC or CH are true, she will answer in the negative. This is because these theses have not been established according to the standards of her field, which require explicit proofs. Unless mathematicians decided to change their standards, this would still be the case even if *every* set theorist knew that these theses were true. To ask about whether a mathematical theory is successful is to ask whether or not the main claims of the theory have been proven or disproven. To ask whether it has been established that a particular mathematical claim *p* is known to be true is to ask

[58] In fact, the sciences encourage an attitude of epistemic detachment. Students are often told that having their hypothesis definitively disconfirmed constitutes an exciting result and should be considered successful research. While one does hear grandiose knowledge claims made by physicists on science programs, one is unlikely to encounter this same attitude when speaking to experimentalists. All of this, I take it, should be fairly controversial to readers who have actually engaged in conversation with scientists. However, since I cannot provide the appropriate sociological evidence to back these claims I will not dwell on them (though I would also point out that it is no more reasonable to deny them without such evidence).

whether someone has produced a proof that *p*. More generally, the question 'Has it been established that p?' has different connotations than the question 'Do you know that p?' in theoretical contexts.

An Individualist might attempt to explain these general incongruities in terms of focus effects or some other pragmatic phenomenon. However this move is clearly unmotivated in the current instance. A pragmatic explanation is only warranted for the use of language that deviates from the established core semantics about the language in question. The current examples concern assertions of whether a theory is successful or established, and these assertions clearly form the core relevant use of the terms 'establish' and 'success' in theoretical contexts. To show that this is a merely pragmatic phenomenon the Individualist would already need to have shown that the semantics of 'established' and 'successful' as the terms are used in theoretical contexts should be given an Individualist analysis. But what evidence could they appeal to in order to do this? They certainly cannot appeal to how the terms are used by mathematicians and scientists.

Knowledge that a theory is true on the part of members of a field is not generally taken to be a necessary condition for the success of that theory. It would simply be false to claim that a scientific theory is disconfirmed by the fact that most scientists do not believe it. This is simply not how confirmation works. A theory is confirmed when scientists test, record and report the results of experiments. What the scientists who are involved in this procedure believe is simply irrelevant. Indeed, it is not very difficult to think of potential examples where the most successful theory happens to be one that very

few of the members of a field believe.[59] It hardly matters whether the members of the field find a theory psychologically satisfying if it has received massive confirmation through experiment. Indeed, it is *prima facie* conceivable that scientific and mathematical theories might eventually become so complex that no individual researcher will even be capable of understanding them. Suppose, for instance, that in this future scenario research is conducted by incredibly advanced artificial intelligences, and that these AIs simply are not capable of distilling their theories into terms that human beings are capable of understanding. While this might be depressing, it is far from obvious that the theories produced by these AIs would be unsuccessful simply because human beings do not understand them. These theories might still have untold practical benefits, lead to new technology, etc. These practical benefits seem to be as legitimate marks of theoretical success as anything else. Moreover, to stipulate that humans must be able to understand a theory in order for it to be successful would—aside from being a crass form of anthropocentrism—clearly subject the notion of theoretical success to irrelevant psychological contingencies.[60]

Just as it is unnecessary for the members of a field to know that a theory is true in order for that theory to be successful, neither is it the case that knowledge that a theory is true on the part of members of a field is *sufficient* for the success of that theory. It is a quotidian fact that claims made in academic research are governed by stricter norms than

[59] This seemed to be the case in chemistry for early atomic and molecular theory prior to empirical confirmation from physics. Also, it is not hard to imagine most physicists agreeing that Quantum Mechanics seems incredible and that there must be some better explanation that we will arrive at later, while nevertheless agreeing that it is the most successful theory currently have, simply because it makes the most accurate predictions.

[60] If successful, this also serves as a counterexample to the claim that theoretical success is to be measured in terms of what the members of the field are rationally required to believe given their knowledge of the evidence. One obviously cannot be rationally required to believe something that one is incapable of understanding.

ordinary testimony.[61] In order to make a claim in an academic journal one must also provide evidence for this claim. One cannot simply state one's conclusion, even if one knows it to be true. The point of research is not merely to share the end result of one's research, but to explain how one got to this result by sharing the evidence that led one to it. Failure to do so invites righteous criticism. It is perfectly conceivable that everyone in a field could agree that they know something to be true while also agreeing that they have not successfully established that it is true. As indicated above, AC and CH are plausible real-world examples. As another example, consider a physicist who claims to know that the Higgs Boson exists before CERN produced the necessary experimental results to confirm the claim. *Prima facie*, her belief in the existence of the Higgs may be justified inductively by (1) her knowledge that it has to exist according to the Standard Model of particle physics, and (2) her knowledge that the Standard Model of particle physics has been incredibly successful at making true predictions. Given similar reasoning it is not hard to imagine a scenario in which the entire field of experimental physics knew that the Higgs Boson existed prior to confirmation by experiment.[62] Nevertheless, unless the physicists in the imagined scenario decided to change their methodological standards, they still would not have successfully established that the Higgs Boson exists without experiment.

[61] Thus this need not contradict the knowledge account of assertion. Rather, making a claim in the context of academic research could easily be understood as a distinct sort of speech-act governed by more stringent rules.

[62] Note that the Individualist might be tempted to respond to this by claiming that no physicist could know a theoretical claim to be true prior to empirical confirmation. However, this merely shifts the problem for the Individualist. For the only obvious way to explain this would be to claim that theoretical success established according to collective standards of the field is *prior* to individual knowledge, which in turn simply amounts to a falsification of Individualism. (See next section.)

1.3.1.1. The Argument from Practice: Objections and Replies

These examples clearly indicate that Individualism fails as a description of theoretical success in mathematics and the sciences. The Individualist try to claim that scientists and mathematicians are "doing it wrong", but this is simply *ad hoc*. For this claim to be successful the Individualist would need to show that these stringent methodological standards are somehow hurting these fields. It would require a monumental amount of work in the philosophy of science to show that this is the case. *Prima facie*, these fields seem to be more successful than they ever have been in the past, so the default assumption must be that they know what they are doing. There is no reasonable hope of defending Individualism by arguing that these fields should loosen their standards. The only remaining way to defend Individualism in regard to philosophical methodology is to show that philosophy differs from mathematics and the sciences in some relevant way such that philosophy is better off employing an Individualist methodology.

1.3.1.1.1. Objection 1: Individualism Explains Current Philosophical Practice

One strategy would be to argue directly from the nature of philosophical methodology to Individualism.[63] However, the fact that philosophical research appears to be conducted in a certain way does not show that this is the correct way for research to be conducted. Of course, the same thing can be said for the sciences and mathematics. However, the sciences and mathematics also have an excellent track record, so there is

[63] We have already seen ample evidence in §1.2.1 that meta-philosophers tend to allow something like Individualism to constrain their discussions of philosophical methodology. However, the mere fact that philosophers tend to describe philosophical methodology in a certain way does not thereby imply that this is the correct way to understand philosophical methodology; especially not when this tendency appears to be unreflective, as it does in this case. Thus the objection must appeal directly to the way philosophy is practiced.

good reason to think that their methodology is well-motivated. As noted above, it is unreasonable to claim that these other fields are incompetent without showing that their stringent methodology hurts them. In contrast, the methodology employed by philosophy has no obvious history of success to cite in its favor. No one could reasonably deny that there is widespread disagreement in philosophy about central positions. Moreover, unlike the sciences there are very few practical benefits of philosophy that one could appeal to in order to claim that the discipline is on track. This is enough to suggest that philosophical methodology might be flawed. Thus it is clear that the Individualist cannot simply appeal to how philosophy *is* practiced in order to establish that Individualism is true as a *normative* description of philosophical methodology without providing a principled distinction between philosophy and these other fields.[64] As stated, this objection fails.

1.3.1.1.2. Objection 2: Philosophy is Funded for Different Reasons

One way to build a more successful version of the previous strategy is to deny that philosophical research receives funding for the same reason as research in the sciences and mathematics. The best explanation for the Collectivist methodology employed by mathematics and the sciences is that these fields are aimed at establishing a unified and official explanation of phenomena that is either considered a social good in

[64] It is important to get clear on the dialectical situation regarding this last point. First, Individualism fails as a methodological description of scientific and mathematical research. Second, scientific and mathematical practice is extremely successful. Third, these fields have been refining their methodology for hundreds of years, and have only seemed to become more successful because of it. This suggests that these fields have good reason not to employ an Individualist methodology. Fourth, there seems to be no principled difference between scientific and mathematical research on the one hand and philosophical research on the other that would be relevant to whether those fields should employ an Individualist methodology. This objection is a response to the last claim, so it needs to supply a *principled* distinction between research in philosophy and research in science and mathematics. Since we have already provided several reasons to think that philosophy should be governed by the same methodology as these other fields, the burden of proof falls upon the Individualist to show otherwise. Since philosophical research does not seem to be very successful, simply appealing to the *status quo* will not do the job.

itself or is intended to provide benefits to society. The fact that these fields are aimed at providing unified explanations explains why they are so careful with what they count as evidence and why they are unwilling to publish partial or questionable results. If the Individualist could show that philosophical research receives funding for some reason other than the value of a unified explanation of phenomena, this would undermine the methodological link between disciplines these disciplines and philosophy.

The problem with this approach is that the Individualist must then explain why philosophical research receives public funding at all. Individualism not only implies that that such a sustained lack of consensus is permissible, but that there is not even a legitimate demand that researchers re-evaluate their methods in order to avoid such sustained controversy. (If it did not imply this it could not be used to defend appeals to intuition.) It is quite obvious that there is little public benefit from a field in which there is sustained controversy and lack of consensus. While it seems relatively clear how a unified explanation might be considered intrinsically good, it is totally unclear why this would be the case for a set of inconsistent explanations. Since disagreement among experts means that non-experts have no way of telling which theory to implement in her decisions, it also undermines extrinsic value of research.

The Individualist needs to explain why such nonchalance about the lack of consensus is compatible with the mission-statement of philosophical research. The only remotely viable option for the Individualist is to argue that philosophical research is funded in order to promote individual attainment of knowledge.[65] Thus the Individualist

[65] As we have already noted in §1.0, it would make no sense for the Individualist to appeal to a cynical explanation for why philosophical research is funded (e.g., for institutional bragging-rights). This would imply that there is no real purpose behind philosophical research at all, which would in turn obviate the entire notion of theoretical success in philosophy. Note also that a consumerist approach that claims that

must claim that the government and other benefactors fund philosophical research in order to sponsor individual philosophers in their pursuit of knowledge. As a descriptive claim this is deeply implausible. If this is to be understood as a normative claim then it would need to presuppose that Individualism applies to philosophy but not to the sciences, which is to beg the very question at hand. The simple fact is that there is no reason to think that philosophical research is funded for different reasons than research in the sciences and mathematics.

Recall the fact that Timothy Williamson appears to explicitly endorse Collectivism in the Afterword to *The Philosophy of Philosophy* (see the quote from Williamson given in §1.2.1.5). Michael Martin provides the following criticism of Williamson's apparent Collectivism:

> In the afterword, he [Williamson] suggests that we have made some progress over the last 50 years and that it should be possible to make significant progress in philosophy, at least collectively, if only proper methods were employed by the discipline. … My immediate reaction to these exhortations, leaving aside the rhetorical flourish involved, is that this doesn't sound like the discipline that most of the readers of this piece work in. It doesn't seem to be a condition of the discipline being in good health within the academic world that it is in the business of making progress. … Some disciplines think of themselves (and are thought of by others) in terms of the progress they make, others are not. What would show that philosophy is or ought to be in the former camp rather than the latter? (2009, 446)

In other words, Martin thinks that Williamson's calls for more stringent methodological standards are unwarranted because there is no reason to think that philosophy is aimed at making collective progress. Indeed, Martin seems to openly embrace Individualism, if not

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the purpose of philosophy is to produce interesting and entertaining theories is equally implausible for two reasons. First, it is clearly inaccurate as a descriptive claim: academic philosophy is incredibly dry and is only consumed by other philosophers. Second, this approach would imply that philosophical research should not aim at the truth but rather at entertainment value, which I take it is equally unacceptable.

outright methodological subjectivism:

> philosophy, like many other disciplines, does not seem to have one over-arching self-image which all of its successful practitioners can agree upon; it doesn't need such for philosophers from very different cultures within the discipline to recognize others as engaged in the very same discipline, even if they work in very different ways. So one might predict that manifestoes of how philosophy should be pursued are liable to be partial, encompassing just one or some of the varieties in which the discipline successfully manages to reproduce itself. However a given manifesto may claim a uniqueness of insight into the goals and manner of pursuing the discipline, we might respond with a certain ironic distance. (2009, 448)

While it may sound uncharitable, the only reasonable interpretation of this passage is as the claim that individual philosophers are free to pursue their goals in whatever ways they wish, while scoffing at the attempts of others to establish a unified methodology.

It will be worthwhile to consider Martin's argument point by point, so we can show exactly where it goes wrong. First of all, Martin acknowledges that the issue in question concerns philosophy more narrowly as an academic discipline rather than an intellectual pursuit.[66] Martin argues that in order to understand what makes a discipline successful, one must understand the role that discipline plays in the university system, the nature of the university system itself.[67] Moreover, Martin also acknowledges that philosophy is generally aimed at the truth.[68] Thus Martin accedes to the initial assumptions we made in §1.1. Unfortunately, these are about the only things that Martin

[66] While Martin states that "philosophy has its origins outside of the academy and could survive the replacement of all universities by the more useful addition of theme rides and industrial parks", he also acknowledges that "Williamson's exhortations are most likely to be interpreted as directed towards academic philosophers, and to express a concern with philosophy as a discipline within modern universities" (2009, 447).

[67] Martin states that "the story of philosophy as an academic discipline and the account of under what conditions it thrives is a story about universities and how the various elements within them prosper and survive" (2009, 447).

[68] Martin remarks that he should not "be mistaken for someone who wishes to dethrone truth from being a central concern", and that his skepticism about the claim that philosophy "must be in the business of progress does not mean that individual disciples of that discipline can properly avoid being concerned with progress" (2009, 448).

gets right. Martin's point is that philosophy can be a successful academic discipline even if it isn't aimed at collectively making progress. In arguing for this point Martin makes several faulty assumptions. The first of which is the claim that a discipline's success should be measured according to how well that discipline succeeds in surviving within the university system. Martin puts the point as follows:

> the minimum they [i.e., academic disciplines] need do in order to be doing well within the university is to reproduce themselves, so that generation by generation within a given university, and across different universities, there are departments of *X*. In turn, the minimum that this requires is that there should be students in departments of *X* who are sufficiently engaged by the lifestyle of being in a department of *X* that eventually they become members of such departments and teach students of their own. … in sociological terms a discipline can be doing well—those who actively are employed within it have a sense of purpose and direction; the discipline remains relatively well-funded and supported within universities—without any mention being made of what progress it has made in increasing the sum of human knowledge. Given that, one might think that the ultimate answer to what makes a given discipline or cluster of departments do well rests with an element of the anthropology, history, or sociology of universities: how is it that philosophy departments have retained a role within universities and continued to have themselves funded from generation to generation? (2009, 447)

Martin has committed the naturalistic fallacy. He has presupposed that descriptive sociological facts about how well certain academic disciplines succeed in perpetuating themselves translate straightforwardly into normative facts regarding the worth of these disciplines and the methods they employ. In particular, he has assumed that the natural processes through which academic philosophy might ensure its continued survival also make academic philosophy *good*. Martin's sociological observations concern a prudential sense in which disciplines *do well for themselves*. However, what is at issue in our methodological dispute is the normative sense in which a discipline *does its job well*. Not only are the two senses not equivalent; they are often at odds. For instance, an

incompetent employee who remains employed due to lack of oversight can do very well for herself while simultaneously doing her job very poorly. This is *precisely* what we should be worried about. Due to lack of oversight, academic philosophy might be doing well for itself while simultaneously doing its job poorly.

To further illustrate the point, consider the following analogy. Academic disciplines are obviously dependent upon universities for their survival. Hence one might liken different departments to function as different organs within the university, consider as an organism. The funding departments receive can be likened to the vital nutrients that organs receive through the body's circulatory system. Thus claiming that a discipline is successful if it manages to replicate itself with regard to each generation is a bit like claiming that the organs that manage to maintain themselves through each generation are useful. However, a colony of parasites can also be very good at self-replication. It would be absurd to conclude from this that having parasites is a good thing. Analogously, the mere fact that philosophy is successful at perpetuating itself as an academic discipline does not imply that philosophy is anything more than a drain upon the university system.

Another serious problem with Martin's argument is the analogy he attempts to draw between philosophy and other humanities such as history. After considering Williamson's claim that philosophy is aimed at making collective progress, Martin makes the following comparison:

> Compare this, say, to a declaration by a leading historian that history as a discipline needs to make progress and that it can if it adopts suitably rigorous methods. Historians may not doubt that historiography might be improved by utilizing new techniques, but few would be roused by the thought that history as such needs to make progress. Now I don't mean to suggest that philosophy is history, or that we should take too seriously the classification of both as humanities disciplines in contrast to branches of science. Much of this seems arbitrary. But really philosophy seems no

> closer to mathematics than it does to history: it has some similarities with each as an academic discipline, and some differences too. (Martin 2009, 446)

The problem with this analogy is simply that there *is* good reason to think that historians are concerned with making collective progress. On this point it is worth quoting Williamson's response to Martin in full:

> Does the comparison with history help Martin's case? Of course, historians tend to distance themselves from the idea of progress in history as the succession of human doings and sufferings. But that is not what Martin means. He is questioning whether history as a discipline makes progress. He says very little to support a negative answer. Yet significantly more is known about a vast range of historical matters than was known fifty years ago. That is not just fact-grubbing. Significantly more is understood about those matters too. For instance, the role of religious belief in the English Civil War is better understood than it was. Again, history has vastly extended the range of its inquiries, for instance into the lives of members of marginalized groups. Its methods have developed, not only through the application of advances in science, such as statistics and DNA analysis. More critical and more imaginative ways of learning from documents are available. It seems merely quixotic to deny that if such changes really have taken place, they constitute progress in history as a discipline. Of course, someone may deny that the role of religious belief in the English Civil War is really any better understood now than it was fifty years ago. But, if so, that neither would nor should be a matter of indifference to the historians concerned. It would be a failure on their own terms. Martin does not seem ready to endorse such scepticism about history as a discipline. Although the progress it makes is piecemeal, the sort of progress I suggested philosophy makes is piecemeal too. Thus the comparison with history, to the extent to which it is relevant, undermines Martin's case against progress in philosophy. (2009b, 469)

The reason why Martin's analogy fails is that historians are concerned with describing things that actually took place. Historians assume that there is some fact of the matter concerning historical questions, such as the role of religious belief in the English Civil War, etc. Should historians be happy with a number of different incompatible claims concerning the role of religious belief in the English Civil War? Of course not. Since they assume that there is a fact of the matter, they must also assume that their job is

to get at the facts by deciding which of these competing claims is true. This implies two things about the methodology of history. First, it implies that historians can make progress, both in obtaining more relevant evidence (e.g., by uncovering more documents, etc.) and in obtaining a better overall theoretical understanding of the subject matter by analyzing the available evidence. Second, it leads to the natural assumption that there are good and bad ways of getting at these facts. This, in turn, inevitably leads to a necessity for methodological evaluation concerning the ways that historians attempt to get at these facts. Historians are and should be Methodological Realists.

The same considerations hold for any field that is aimed at describing facts and is capable of doing so accurately or inaccurately. A better strategy would be to compare philosophy to the fine arts. Since the fine arts are quite obviously not aimed at describing some portion of reality, they are far more likely to embrace methodological pluralism. If someone can use some new method to produce art that people enjoy and find thought-provoking, then the method is validated. But in this case the analogy would quite obviously fail. Philosophers are not concerned with producing theories simply because they have aesthetic value, and if they were then they are quite obviously incompetent and should seriously reconsider their methods. Virtually no metaphysicians would be willing to make this argument in order to defend their appeal to intuitions.

The question relevant to our methodological project is: Why should philosophical *research* receive funding? Whether appeals to intuition are an acceptable method is ultimately a question of how we should treat appeals to intuition as evidence in official research. No one is claiming that philosophers are not permitted to appeal to intuitions outside of official research in blogs or casual conversation. Philosophers can pursue

questions on their own time as they see fit. However, one should not dismiss the importance of research funding for the field. Departments hire faculty members at least in part due to their research contributions. Thus if funding for research were cut it would also mean that our hiring practices need to change. Indeed, it is not immediately obvious that philosophy departments should still exist if it is determined that philosophical research is not worth funding. After all, philosophers' research is directly tied to what they teach: if the former is not worth the funding then why should the latter be?

Martin's rhetoric is dangerous. Many of the humanities have been on the chopping-block in various states in America, and the situation in Martin's native Britain is not much better given austerity measures. Imagine what would happen if Martin actually attempted to justify public funding of philosophy to a conservative government official by appealing to the fact that thus far philosophy has been successful in securing funding for itself within the university system. The official asks: What is the public good produced by research in your field? Why should your research receive taxpayer money? These are perfectly valid questions. We need answers. Merely appealing to the academic *status quo* will not do the job.[69] The mere fact that we are currently able to secure funding does not constitute an excuse for methodological complacency. The public funding that goes to philosophical research can only be justified if philosophical research constitutes a public good. What makes philosophical research a public good, if it is not aimed at collective progress towards the truth?

[69] Part of the problem is that universities compete in terms of reputation. Once a certain discipline is established as important, allowing that discipline to slide or be cut will affect a university's reputation. In this manner we can expect a certain amount of inertia regarding the funding for any well-established academic field, even if that field has ceased to be useful. Recall our discussion of cynical explanations for funding from §1.1.1.

Why should public funding go to conferences and philosophy journals?[70] It is clear that Martin's reply gives us no answer to this question. What are the alternatives? Individualists could suggest that the individual knowledge of philosophers is itself an intrinsic public good, even if this knowledge is not shared with the public. However, this claim is deeply implausible. Why would the knowledge of certain individuals constitute an intrinsic public good? Moreover, if this is meant to be a description of the actual reasoning behind our current funding it is sheer fantasy. There is no evidence to suggest that universities and public officials fund philosophical research just because they wish to help a few individuals in their pursuit of knowledge.

Nor can the purpose of academic journals and conferences be to produce a consumable good. If the aim of philosophical research were to produce consumable goods, then the primary concern would be popularized books that can be easily digested by non-professionals. But it is the technical peer-reviewed journals that receive the bulk of public funding through library subscriptions. The public doesn't have access to these journals or conferences, and even if they did the subject-matter contained is typically so dry and technical that very few non-professionals would find them interesting. Moreover, even if philosophical theories were meant to be consumable goods, it is clear that a large part of what would make them attractive is that they are *good theories*. This implies that the purpose of technical research is to help produce better theories, which makes for better reading when popularized. But this would dictate that the aim of technical research *is* collective progress. For these reasons the funding objection clearly fails.

[70] In order to even ask this question, one has to already ignore the fact that philosophy makes contributions to other fields such as logic, semantics, biology, psychology, statistics, cognitive science, etc. Since part of the goal of philosophical research is to contribute to these fields, and since the purpose of these fields dictates that they employ Collectivist methodologies, this would appear to impose a transitive demand on said philosophical fields to employ a Collectivist methodology as well.

1.3.1.1.3. Objection 3: Philosophy is A Priori

Another strategy would be to argue for the type of Exceptionalism endorsed by Bealer and others by claiming that the sciences are *a posteriori* while philosophy is *a priori* (see §1.2.1). One significant problem with this strategy is simply the fact that much of philosophy depends on resources that are not *a priori* (e.g. philosophy of mind, philosophy of science, etc.). However, a more immediate problem with this strategy is that it is incapable of handling our examples from mathematics, which is even more clearly *a priori* than philosophy. What is especially relevant for our discussion is that the starkest methodological difference between philosophy and mathematics lies precisely in the fact that mathematicians no longer treat intuitions as forms of evidence that can confirm a theory.[71] Of course, this is not to deny that mathematicians *have* intuitions. Indeed, intuitions appear to play important auxiliary roles in mathematical practice.[72] Nevertheless, the fact that intuitions play these auxiliary roles does not imply that they are officially recognized as evidence.[73] It was argued in §1.3.1 that the best explanation for mathematical practice is that the purpose of mathematical research demands that it employ a Collectivist methodology. The fact that the subject-matter of mathematics is

[71] As Hans Hahn notes, "even in the branch of mathematics which would seem to be its original domain [i.e., geometry], intuition gradually fell into disrepute and at last was completely banished" (1980, 76).

[72] For instance, they appear to have a very important role in mathematical pedagogy. Intuitions can sometimes even have an important role in establishing mathematical theories. It may very well be the case that many mathematicians need to see a result intuitively before they can figure out how to prove it. However, the fact that intuitions might play this *etiological* role does not imply that they thereby have an *evidential* role. The intuition is merely a ladder, which can be cast away once the proof has been reached. If a mathematician cannot arrive at a proof, her intuition is largely useless. Of course, it does sometimes help to show that a solution to a seemingly complex problem follows from an intuitive premise, but this does not thereby *establish* the solution. Rather, it merely shows that one can establish the solution by establishing the premise.

[73] Unlike philosophers, mathematicians do not even begin to endorse something like the method of reflective equilibrium. Rather, they only seem to appeal to intuitions in an extremely tentative manner to justify certain premises that cannot otherwise be proven, but which are nonetheless indispensable for their purposes (AC and CH are prime examples).

known *a priori* has no obvious bearing on this fact. Why should the case be any difference for philosophy? This objection also fails.

1.3.1.1.4. Objection 4: The Use of Arguments Presupposes Individualism

A final strategy would be to establish that the specific methods employed in philosophy can *only* work on the assumption of Individualism. If one could establish that philosophers' use of arguments can only be explained by Individualism, this would separate philosophy from other disciplines and constitute a "partner in the guilt" objection to Collectivism. Our methodological criticism of intuitions could only result in a pyrrhic victory if it also implied a ban on the use of arguments, as there would simply be no method left with which to do philosophy.[74]

Arguments can often be evaluated using something like (ISK) without falling into serious error. The evidence provided by arguments is inferential, and inferential evidence is intersubjectively transferrable in a way that non-inferential forms of evidence are not. This is because non-inferential evidence is partially grounded in conditions that are specific to particular agents. Such evidence cannot be easily transferred between individuals without changing status. For instance, the non-inferential evidence provided by a perceptual belief *p* for an agent *s* is partly grounded in the sensory states of *s* and the causal relations that those states bear to the content of *p*.[75] But suppose agent *s* attempts

[74] Note that the objection cannot be the claim that philosopher's use of intuitions can only be justified by Individualism. First of all, intuitions clearly *can* be treated in a Collectivist manner by treating them in the same way as observation in the sciences (x-phi can be seen as being motivated in this way). Given this fact, the only option left for the Individualist would be to argue that Individualism must be true for philosophy because our current use of intuitions does not pass (MC) (see §1.4.1). However, in order for this appeal to work the Individualist must rule out the possibility that our current use of intuition is simply impermissible. She can only do this by assuming that Individualism is true, which begs the very question at hand.

[75] Recall that we are assuming that evidence is factive. This might be seen as controversial when it comes to the link between evidence and justification. For instance, if one thinks that one's justification is

to relate *p* to another agent s_2. Unless s_2 is also in a position to have the same perceptual experience as *s*, *p* can only be justified for s_2 as a piece of testimony. This implies that *p* has much stronger evidential status for *s* than it does for s_2.

In contrast, inferential evidence can be transferred directly simply by relating the argument to another speaker. Here the evidence is not testimonial but is grounded in the hearer's understanding of what is said, which will not vary from individual to individual in the way that perceptual evidence does. (Of course, a speaker could simply testify to the conclusion of her inference, but such cases are obviously irrelevant.)[76] There is typically no meaningful gap between the evidence the argument provides to individuals and the evidence that it provides to a community as long as the community shares the relevant non-inferential evidence. Because of this, one can usually evaluate how arguments affect theoretical success through something like (ISK) without getting the wrong result.

However, this is hardly enough for the Individualist's purposes. The Individualist needs to show that something like (ISK) provides the correct way to analyze arguments *in principle*. If something like (ISK) were true, one would expect philosophers to evaluate the success of each argument individually according to how it provides the relevant specialists with knowledge. For instance, (ISK) one would evaluate how a particular

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dependent upon one's evidence and one is an internalist about justification, then one will only hold that it is the subjective states of *s* that makes *p* evidence. This position is generally known as 'Evidentialism', which can be captured by the following principle (EvJ): "Doxastic attitude *D* toward proposition *p* is epistemically justified for *S* at *t* if and only if having *D* toward *p* fits the evidence *S* has at *t*" (Feldman & Conee 1985, 15). However, since the entirety of (EvJ)'s intuitive plausibility can be recaptured simply by reformulating it in terms of *putative evidence*, there is good reason to treat the dispute as a terminological one. See §1.1.3 above.

[76] We are setting aside cases in which the hearer has unusual beliefs about what counts as a valid inference (e.g., the 'Peter and the foxes' example from Williamson 2007, ch. 4). The reason why we can set such examples aside is simply that they would constitute genuine theoretical disagreement within the community, which is hardly a counterexample to our current claim. This would be a bit like two people seeing the same thing, but giving different weight to their perceptual evidence because one of them thinks she is likely being deceived by a Cartesian demon.

argument from analogy affects the success of a particular theory according to whether the relevant specialists could reasonably be said to know that the theory is true on that argument. However, inference only converts and manipulates existing evidence into new forms of evidence. Thus the manner in which any argument contributes to knowledge will be dependent upon the non-inferential evidence possessed by the individual who considers the argument. The problem is that different theorists have different background beliefs that affect how they are influenced by the argument in question.

For instance, suppose the theoretical claim in question is that abortion is morally permissible, and that the argument by analogy in question is the Violinist case (Thomson 1971).[77] For those who were already inclined to think abortion is permissible Thomson's argument may provide very good evidence. Indeed, it might be enough to ground their knowledge of the claim. However, it is also rational for someone who has a prior commitment to the position that abortion is impermissible to be skeptical about Thomson's argument. After all, arguments from analogy are extremely nuanced and can go wrong in many ways, not all of which are immediately obvious. The subsequent literature on the Thomson's paper has produced a vast array of analyses and reconstructions of her original argument, as well as a vast array of counter-arguments.

Since belief in the theoretical result is obviously a precondition for (ISK), and since many of the relevant specialists did not believe that abortion was permissible on hearing Thomson's argument, it is unclear how the Individualist is supposed to determine

[77] I am here assuming that the reader is familiar with this case. Roughly Thomson's argument runs as follows. Suppose someone is kidnapped by a doctor and has a dying violinist surgically attached to her such that her body is what is keeping the violinist alive. Thomson argues that this person would be permitted to detach the violinist. She then argues that this case is identical to a case of abortion in all the relevant non-moral details. If this is true, it would be arbitrary to say that detaching the violinist is permissible while abortion is not. Thus it is supposed to follow that abortion is also permissible. Of course, both of the major premises in Thomson's argument are controversial, but this is irrelevant for our current purposes.

how Thomson's argument affects the evidential status of the theoretical claim that abortion is permissible. The Individualist obviously cannot arbitrarily choose which specialists count towards the success of the theory. In order for an Individualist to make a ruling about whether Thomson's argument (taken on its own) increased the evidence for the theoretical claim that abortion is permissible, she would have to abstract away from all of these features of individual researchers. But it is unclear how the Individualist is supposed to do this, or whether she even *can* do this without tacitly committing herself to Collectivism. (We will return to this point in the next section.)

Over the past several thousand years, philosophers have developed fairly standard measures for evaluating types of arguments (e.g., deductive, inductive, analogy, etc.) in abstraction from their particular content.[78] For instance, most of the specific counterarguments against Thomson's violinist argument consist of attempts to show that the analogous case differs from the abortion case in some relevant aspect. All of these counterarguments appeal to a more general established fact: arguments from analogy tacitly depend for their success upon *certeris paribus* claims. This gives us the following plausible general methodological claim: the level of evidential (dis)confirmation a theoretical claim receives from an argument by analogy will depend upon how well the analogy has been considered. The best explanation for this general principle is that it is a

[78] When a particular philosopher presents an argument, we can evaluate her argument in three ways: (1) we can evaluate the truth of her premises, (2) we can assume that her premises are true and evaluate whether they establish her conclusion, or (3) we can evaluate whether her argument as a whole will succeed in serving her general purposes, even if it is sound. Thus we can disagree about whether a particular argument is successful simply because we cannot ascertain whether its premises are true. We can also agree about what it would take for a particular argument to be successful while disagreeing about whether the argument actually is successful because we disagree about the nature of the concepts involved in that argument. This is because particular arguments have both a general formal aspect (the type of argument to which it belongs) and a semantic aspect. Disagreements over how to analyze specific arguments can be rendered intractable due to disagreement over some semantic aspect of the argument. For instance, we can agree in general that arguments by analogy are only successful if the analogous case shares all the relevant aspects, but what counts as a 'relevant aspect' of the case will depend upon how one analyzes the concepts involved in the argument.

Collectivist norm that we have established because of our general understanding of how arguments by analogy work. In contrast, the Individualist will have considerable trouble deriving this general methodological claim. Indeed, the Individualist will have trouble accounting for any general methodological norm about types of arguments because she is restricted by (ISK) to talking about what individuals know about specific claims.[79] It is hard to see how the Individualist could achieve such generalizations given this constraint.

The same linguistic phenomena that we noted at the beginning of §1.3.1 hold in regard to how philosophers talk about arguments. When a philosopher asks whether someone's argument is successful, she is not obviously asking whether that philosopher or anyone else knows the conclusion of the argument. Her question should be understood straightforwardly: she is asking whether the argument itself provides good evidence for the theoretical claim in question. This requires no obvious reference to what any individuals know or would know. Similarly, when a philosopher asks another philosopher whether it has been established that *p*, she is not obviously asking about whether the relevant specialists know that *p*.

Moreover, similar counterexamples to the claim that knowledge is necessary or sufficient for theoretical success as given in §1.3.1 can be extended for arguments in philosophy. It can generally be agreed that a particular argument is valid, but so long as there is still substantial disagreement over the argument's premises we would not

[79] Since (ISK) simply depends on knowledge this will make it extremely difficult for the Individualist to account for degrees of theoretical success. Individuals cannot be reasonably expected to foresee all possible objections and responses to those objections; their epistemic status varies as they consider new arguments. Thus an individual can go from knowing a claim *c* in virtue of argument a_1 at t_1, to having her belief in *c* undermined by counterargument a_2 at t_2, to once again knowing *c* when she comes up with a counter-counterargument a_3 at t_3. But according to (ISK) this will simply result in a binary vacillation between success and failure over time. The Individualist could try to shift to something like subjective probabilities, but this would be far too weak for the Individualist's purposes. We shall return to this point in §1.3.2.

consider the argument to be successful. This would clearly continue to be the case even if the author of the argument had good evidence for the disputed premise that she simply did not share with the rest of the community, and even if the rest of the community would know the conclusion if they knew this premise.[80] Conversely, it certainly seems possible for philosophers to tentatively agree that one theory is better than its competitors due to a certain argument while still withholding belief due to worries about as-yet unknown problems that might arise in the future. While these counterexamples are not definitive, they do provide significant hurdles for the claim that arguments must receive an Individualist treatment.

We established in §1.3.1 that Individualism fails as description of scientific and mathematical practice. We have now considered various ways in which the Individualist might attempt to break the analogy between these fields and philosophy. We have seen that each of these strategies fails. First, we saw that philosophical research appears to receive funding for the same reasons as scientific and mathematical research. Thus the Individualist cannot say that philosophical research is aimed at providing individuals with knowledge. Second, we saw that the fact that philosophy is *a priori* does not appear to have any effect on whether Individualism is true, because this reasoning would apply equally to mathematics. Finally, there is no reason to think that Individualism is the only way to explain—or even a *good way* to explain—how arguments are evidence.

[80] For example, in arguing that modal reliabilism provides the only reasonable explanation for why intuitions can be justified, Bealer references unpublished arguments that supposedly rule out other theories of justification such as coherentism, contextualism, etc. (see footnote 35). These arguments are necessary in order to establish his claims about intuition. (For instance, Bealer clearly needs to rule out Chudnoff's phenomenal dogmatism, and any other competing theories of intuitive justification if his own is to succeed.) Even if Bealer's unpublished arguments succeeded, it is clear that Bealer's theory cannot be considered successful. The fact that the rest of the philosophical community is unaware of these arguments very clearly undermines the claim that Bealer's theory is successful, even if Bealer himself knows that his theory is true.

No doubt there are more ways in which the Individualist might try to break the analogy, but we shall not consider them. Our aim in this section has merely been to undermine whatever *prima facie* plausibility Individualism might have had in virtue of preconceptions about the nature of philosophical practice. It may be that at one point philosophy was something that was done by a few titanic individuals who arrived at their conclusions independently of everyone else (though even this historical claim is questionable). However, there is no reason to think that this is still the case in modern academic philosophy. The nature and quality of academic research has changed drastically over the years. Modern philosophical research is the result of a communal effort, just like the sciences and mathematics. What has been said thus far should go some way towards illustrating this point. However, the preceding arguments have merely shown that there is no good reason to assume that Individualism is true. They do not show that Individualism is false as a normative claim, or that it cannot be independently motivated. This will be the goal of the following section.

1.3.2. The Argument from Methodological Primacy

So far we have merely provided evidence that there is no *prima facie* reason to assume that Individualism is true. In order to assess the plausibility of Individualism itself we will need to examine the position itself. In so doing it will be shown that Individualism is *not* plausible. We will provide two arguments for this claim. First, an analysis of Individualism will show that the Individualist must make tacit Collectivist assumptions to produce reasonable results. Second, it will be argued that the individual knowledge of researchers is dependent upon theoretical success, rather than the other way around.

1.3.2.1. An Analysis of Individualism

The specific definitions of theoretical success we are considering are supposed to act as heuristics for determining what theory is best supported by our current evidence. Hence in order to judge whether Individualism is accurate, we should test whether it can be formulated in such a way that it makes accurate predictions about what should count as successful in various scenarios. In order to evaluate intuitions the Individualist is really only concerned with the kind of justification they provide.[81] There are two remaining dimensions along which formulations of Individualism can vary in strength: (1) in terms of the contents of the epistemic state that each researcher is required to have, and (2) in terms of the requirements placed upon the number and status of the researchers that are required to possess this state.

Along the second dimension, we can already note one restriction. As we argued at the end of the last section,[82] the mere fact that one individual could know that their theory is true on evidence that they have not shared with anyone else is clearly not sufficient to establish that their theory is successful. The irony of this is that a strong formulation of Individualism is itself counterintuitive. We judge that this sort of case is unacceptable because the relevant notion of theoretical success is *social*. Thus a theory is successful

[81] This relates to the point about the relevant notion of theoretical success being quasi-objective made at the beginning of §1.2. Any formulation of Individualism requiring something weaker than justification is implausible: the mere fact that many researchers might believe a theory clearly is not enough to establish that the theory is successful. Moreover, given that the purpose of describing success conditions is to achieve something that could actually be applied to theories, the distinction between justification and knowledge doesn't make much of a difference. In order to rule out theoretical success in Gettier cases it will be better for the Individualist to in principle require knowledge rather than mere justification. If (ISJ) is used to evaluate a particular use of a method in what is known to be a Gettier-type scenario, it will tell us that that that particular application of the method is valid and gives us good evidence because it provides justification, when in fact that evidence would be unreliable. However, in order to get this result we would already need to know that we are in a Gettier case. Since knowledge of the fact that we are in a Gettier case would itself undermine our justification, this is a rather moot point. On a practical level, applying (ISJ) and (ISK) will amount to the same thing.

[82] See footnote 80.

only if it is backed by evidence that is *intersubjectively available* to the community of researchers. If we were merely concerned with what one individual knows we would not need a notion of theoretical success to begin with.

The Collectivist offers a straightforward interpretation of this fact: the notion of theoretical success is social because research itself is a social enterprise. It cannot and should not be reduced to what individual researchers know. The Collectivist takes the social nature of evidence to heart. According to the Collectivist, the body of evidence that can be used to either confirm or disconfirm a theory consists of the body of claims that explicitly occur in publications, and the peer-review process acts like a filter for this evidence. In contrast, the Individualist must describe the relevant evidence as somehow consisting in the epistemic states of individual researchers. The Individualist cannot avoid this sort of result by explicitly requiring the evidence to be intersubjectively available. This would make it difficult to distinguish the Individualist's position from Collectivism. More importantly (given our present purposes), it would cripple the Individualists' ability to defend the use of intuition as a method. Intuitions are quite obviously *not* intersubjectively available forms of evidence. Thus if the Individualist is to make her notion of theoretical success acceptable, she must exclude the bad cases by requiring that many researchers know the relevant claim to be true. Whether the Individualist can accomplish this will depend upon the content of the relevant epistemic states.

Recall from §1.2 that the clause 'that *t* is true on T' was needed in order to make sure that the propositional attitudes in (ISK) and (ISJ) were grounded in the right sort of evidence for the theory *t*. It will be difficult for the Individualist to balance the complexity of these grounding relations and the evidence required with the number of

researchers who are required to hold the relevant attitudes. For instance, if all (ISK) requires is that the researchers in question know that *t* is true the result will be completely implausible. It is possible for the relevant researchers to know that a theory is true on testimony without even knowing the specific content of the theory.[83] Since it would be ridiculous to say that a theory is successful or has been established when no one even knows what the content of the theory is, something more is required.

The Individualist might object that this is a result of making the content of the relevant propositional attitude metalinguistic. However, this is necessary for two reasons. First, one needs to be able to refer to a theory in order to say that it is successful. Since (ISK) and (ISJ) start off with a name in the *analysandum*, they need to use this name in the *analysans*. Second, the individual obviously needs to at least be aware that there is such a theory. The Individualist could insist that the individuals have the relevant propositional attitudes toward the major claims of the theory instead. However, this would allow for cases in which the individuals in question have these attitudes for each claim without realizing that these claims actually come together to form a theory.

Theories can be individuated in a coarse-grained or fine-grained manner. For instance, one can hold that numbers exist, or both that numbers exist and that they are

[83] To illustrate this point consider the following case. Once upon a time a brilliant philosopher, Bob, came up with a theory about a specific aspect of perception. Bob's name for this theory is mouthful so we will simply call it '*t*'. Bob was, first and foremost, a philosopher of science. Indeed, Bob only came up with *t* in order to justify an important claim about the use of instruments in physics. When Bob finally published his *magnum opus* on the philosophy of science he didn't bother to explain any of the details of *t*. When the question about perception arose Bob told the reader that of all the various theories concerning the specific aspect of perception in question, he had found that *t* was the best. Bob lamented that while he did not have the space to explain any of the details concerning *t* he would do so at some point in the future. Tragically, Bob passed away before he ever got around to publishing his results. Bob's work is now considered a classic. Indeed, it is considered such a work of genius that most modern scholars of perception think that Bob's theory *must* be true. In fact, not only is *t* true, but Bob's lost manuscript on *t* provided some of the most convincing arguments ever provided in favor of a philosophical position. But alas, no one alive today is aware of these arguments, or even of what *t* is exactly supposed to entail. Epistemologists today only know that *t* is true on Bob's authoritative testimony.

sets. Moreover, one can be justified in believing a certain general theoretical claim, while being unsure about how exactly the claim should be cashed out in more specific terms.[84] For the same reasons, someone can know that a general theoretical claim is true without even being aware of some the specific formulations in which one might make this claim. Since no reasonable epistemological theory will restrict knowledge only to cases where one has considered all possible alternatives, this also means that one could even know a *specific* formulation of a claim to be true while being unaware of other specific alternatives.

If the Individualist holds that theoretical success is determined by whether researchers believe in the major claims of a theory without accounting for how these claims are related to one another, this would allow extremely specific formulations of theories to be considered successful without researchers even having considered them in this specific manner. This can happen when researchers come to know a theory for reasons that are neutral between that theory and reasonable alternatives that they simply have not considered, such that their justification would be undermined were they to become aware of the alternatives. This can also happen when researchers make seemingly innocent assumptions about background considerations that turn out not to be innocent after all. This would leave the Individualist with no way to distinguish between cases in which a theory is successful because researchers have convincing evidence for favoring it over its alternatives from cases in which the researchers come to know this theory through a certain amount of luck without even being aware of the alternatives.

[84] For instance, someone can be convinced by indispensability arguments that numbers exist, while still being unsure whether numbers are sets or *sui generis* entities. Someone can be convinced by semantic arguments that possible worlds exist, and yet be unsure whether they are concrete or abstract. Someone can be convinced that propositions exist, but can be unsure whether they are structured propositions, sets of possible worlds, etc. The list goes on.

Here are several ways in which this might happen.

CASE A: Consider two theories, t_1 and t_2, and two sets of claims $\{p_1, p_2\}$ and $\{p_1, p_3\}$, where the following conditions hold: (1) p_2 and p_3 are the major theoretical conclusions of $t_1$and t_2, respectively. (2) p_2 and p_3 are incompatible. (3) p_1 offers good sufficient explanations for both p_2 and p_3. (4) The majority of specialists believe p_2 on p_1, but are unaware of the fact that p_3 is equally supported by p_1, and are not aware of any further reasons that could be used to decide between p_2 and p_3.

CASE B: Consider two theories, $t_1 = \{p_1, p_3\}$ and $t_2 = \{p_2, p_3\}$, where the following conditions hold: (1) p_3 is the major theoretical conclusion of $t_1$and t_2. (2) p_1 and p_2 are mutually incompatible. (3) Both p_1 and p_2 seem *prima facie* plausible when considered individually for reasons that do not relate to p_3. (4) Both p_1 and p_2 respectively offer good competing explanations for p_3 when considered properly. (5) The majority of specialists happen to believe p_3 on p_1, but are unaware of the fact that p_2 offers a competing explanation for p_3 and are not aware of any good reasons to favor p_1 over p_2.

CASE C: Consider two putative formulations of the same theory, $t = \{p_1, p_2, p_5\}$ or $\{p_3, p_4, p_5\}$, where the following conditions hold: (1) p_5 is the major theoretical conclusion of t. (2) p_1 and p_3 are compatible background theories, where p_1 makes stronger claims and is more controversial than p_3. (3) The majority of specialists whose beliefs are relevant to the success of p_5 are not aware of any reason to favor p_1 over p_3. (4) p_2 constitutes a good explanation of p_5. (5) p_2 is constructed out of background theory p_1. (6) The majority of specialists believe p_5 on p_2. (7) The majority of specialists assume that $\exists x(x = p_4)$, where p_4 is an explanation for p_5 that is ultimately based on the same reasons as p_2 but is constructed entirely in terms of background theory p_3. (8) $\sim\exists x(x = p_4)$.

CASE D: Consider two formulations of a theory *t*: {p_1, p_2, p_4, p_5} and {p_1, p_3, p_4, p_5}, where the following conditions hold: (1) p_5 is the major theoretical conclusion of *t*. (2) By itself p_1 provides a good explanation for p_5. (3) p_2 and p_3 are mutually incompatible. (4) Both p_2 and p_3 seem *prima facie* plausible when considered individually for reasons that do not relate to p_1 or p_5. (5) p_4 serves as a defeater for p_5 if all one has in support of p_5 is p_1. (6) p_4 is implied by the conjunction of p_1 and p_5, but is implied by neither individually. (7) Either p_2 or p_3 are needed as a defeater-defeater for p_4 in order to justify p_5. (8) The majority of specialists happen to believe p_5 on p_1, are unaware of the fact that p_4 is implied by the conjunction of p_1 and p_5, believe p_2 for reasons that are independent of any considerations relevant to *t*, and are not aware of any good reasons to favor p_2 over p_3.

The common factor that unites these cases is the possibility that researchers can have some good reasons in favor of a general formulation of a general theory but also happen to believe some claim that is relevant to a more specific formulation of this theory without realizing it. Clearly it would be misleading to say that the more specific formulation of the theory should be considered successful in these cases.[85] In order to say

[85] For instance, let us consider a specific example of Case C. Suppose that in considering arguments for and against cognitivism, most metaethicists formulate cognitivism in terms of a theory of truthmakers, simply because they wish to avoid the intricacies of formal semantics. They assume that this is not really relevant and that one could get the same result by working with other semantic theories. Suppose that, unbeknownst to the metaethicists, it turns out that truthmaker theory is actually needed to formulate the appropriate version of cognitivism due to the susceptibility of other semantic theories to "creeping minimalism" (see Asay 2013 for an argument to this effect). To say that a formulation of cognitivism in terms of truthmaker theory is more successful than a formulation of cognitivism in terms of a theory of structured propositions, we would need to show that the former is supported by better evidence than the latter. Even if we suppose that the specific formulation happens to be true, the mere fact that metaethicists happen to believe this formulation for irrelevant reasons (i.e., ease of use) clearly does not establish that it is more successful than its competitors. Indeed, if the metaethicists explicitly believe that general formulation of cognitivism is supported by various other semantic background theories without really considering how this might be done, then it might be that only their belief in the specific formulation is justified. In this case, Individualism would tell us that the truthmaker formulation is successful by sheer luck, since no one would be aware of the evidence supporting it over its competitors.

that one theory is more successful than its competitors, one needs to show that it is backed by more evidence than its competitors. This shows us that the Individualist must pack much more into (ISK).

In order to avoid these sorts of cases, the Individualist will need to cash out the claim that researchers 'know that *t* is true on T' as follows. First, let us define an operation '*' on T, such that $T^*=\langle T_c,T_r\rangle$ where the following conditions hold: (1) $T_c \subseteq T$ and $T_r \subseteq T$, (2) $T_c=\{c_1, c_2, \ldots, c_n\}$ where each c_i is a claim made by *t*, and (3) $T_r=\{r_1, r_2, \ldots, r_n\}$ where each r_i is evidence for some c_i.[86] The Individualist will then say that that for any individual *s*,

> (IKT) *s* knows that *t* is true on T IFF the following three conditions hold. (i) *s* knows that *t* is true. (ii) For each $c_i \in T_c$, *s* knows that c_i and there is some $r_i \in T_r$ such that *s*'s belief that c_i is based at least in part on r_i. (iii) *s*'s belief that *t* is true is based at least in part on *s*'s belief in each $c_i \in T_c$.

The idea is that (IKT) will allow the Individualist access to the body of evidence in favor of a theory through the reasons upon which individual researchers base their beliefs in the theory.

There are several problems with this. At the most general level, the Collectivist is likely to object that this maneuver lacks independent motivation and introduces unnecessary complexity into the notion of theoretical success that Collectivism avoids.[87]

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[86] There are two things to note about this. First, we needn't assume that $T = T_c \cup T_r$. It might be the case that we are leaving out some things. Second, we shouldn't assume that T_1 and T_2 are disjoint. For instance, T_1 and T_2 would overlap if one of the major claims of the theory in question also happened to be evidence for another major claim. This needn't imply any sort of vicious circularity so long as the first claim itself is based on good evidence.

[87] There are actually two objections here. The first objection is dialectical: the Collectivist is accusing the Individualist of trying to save her theory through an *ad hoc* modification. It seems that one of the primary benefits of Individualism is its simplicity: i.e., that it cashes out theoretical success in terms of whether individuals know/are justified in believing that a theory is true. If the Individualist has to modify her theory significantly beyond this original claim then Individualism appears to lose much of its original

If we are interested in the evidence for or against a theory, then why not simply appeal to that evidence directly? Why use the epistemic states of individuals as a middle-man? In order to avoid these objections the Individualist will need to provide further motivation for her theory, or provide some evidence that Collectivism is not as simple an alternative as it seems. We will return to these objections and possible responses after we have considered the Individualist's position in greater detail.

The first problem with (IKT) is that the Individualist will need a principled way of deciding what counts as the major claims of a theory and what counts as evidence. How much (ISK) requires for theoretical success will depend upon how many beliefs (IKT) requires individuals to have, and the sort of reasons on which these beliefs need to be based.[88] The more claims one adds to T_c, the less likely it is that the necessary number of researchers will believe it. Virtually any theory will still have substantial implications that even its proponents have not yet considered. However, if the Individualist leaves out too many important claims from T_c then (ISK) will be left too weak to do the necessary work. This would allow for counterexamples similar to the ones we considered above where one theory happens to be more successful than another simply because researchers are lucky enough not to have considered one of the former theory's claims.[89]

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motivation. The second objection is more direct insofar as it concerns the relative simplicity of the two theories. By adopting something like (IKT) the Individualist's theory seems to introduce an unnecessary layer of complexity that the Collectivist's theory avoids by appealing directly to a body of evidence.

[88] Recall that T is the set of all propositions that are relevant to the theory in question. The strength of the requirements imposed by (IKT) will depend upon how selective the operation '*' is.

[89] For instance, suppose that the individual leaves out one of the major claims c_n of a theory t_1 from T_c in order to accommodate the fact that individuals have not considered that claim, and it turns out that there is another competing theory t_2 that does not make c_n that is supported by the same evidence that researchers use to support t_1, and which the researchers simply have not considered. Then the Individualist would say that t_1 is more successful than t_2 even though the researchers have not considered t_2, even if on their current evidence t_2 might even be a better theory.

So where should one draw the line? The Collectivist has a straightforward answer: one should consider the claims and evidence that have been explicitly considered thus far in the literature. Since the Collectivist individuates theories and the evidence that supports them by simply analyzing what has been said in the literature, she avoids making theoretical success dependent upon considerations that no one has made. The Collectivist will sometimes say that a theory is established by our current evidence even when researchers are not aware of this fact, but this will generally only happen when researchers have overlooked something that someone else has said. It is difficult for the Individualist to get the same results because each individual researcher is unlikely to be familiar with everything that has been said about a topic.

The primary structural weakness of Individualism lies in the fact that beliefs are likely to vary from researcher to researcher. This leads us to another obvious requirement for Individualism: the epistemic states of the researchers in question should be based on the same evidence. One might opt for the weaker condition that these beliefs be based on bodies of evidence that are mutually compatible. However, this is almost certainly too weak for the Individualist's purposes. We can easily imagine cases where one half of the required majority believes the theory on one body of evidence b_1 while the other believes on b_2, where b_1 and b_2 are compatible because they have to do with very different aspects of the theory in question. But in this case both subsections of the population would be basing their theoretical beliefs on only *part* of the important evidence for the theory. The situation is exacerbated by the fact that b_1 and b_2, while not overtly incompatible, might still manage to conflict with one another insofar as they might each suggest one of the competing explanations regarding the other aspect of the theory in question.

As a rough example, consider "Plenitudinous Platonism" (PP), which is, roughly, the claim that every consistent mathematical theory accurately describes some mathematical entity or structure (Balaguer 1998).[90] Now, suppose that half of the necessary specialists believe in a form of PP because it is implied by a deflationary conception of ontology that they find independently plausible. Suppose the other half believe in a form of PP because they think it is the only way to solve the various epistemological and semantic problems that face Platonism, to which they are committed in virtue of an ontological conviction that mathematics describes substantial, mind-independent abstract objects that exist in a "realm" beyond space and time. For the first group, PP is uninteresting because the existence of mathematical objects is uninteresting. In contrast, the second group will find that PP is an exciting an unpredictable result, similar to how a biologist would react to the discovery of life on countless worlds aside from Earth. Even if we suppose, for the sake of argument, that the argument from deflationary ontology and the argument from the semantics and epistemology of mathematics are both good reasons for each group to endorse PP, this does not help the Individualist. For it is still obvious that the two groups are in epistemic conflict (they would reject each other's reasoning). A Collectivist could easily point out that both arguments provide evidence for the theory regardless of the particular background assumptions of both groups (in particular, she can point out that these arguments increase the probability of PP conditional on both substantivist and superficialist assumptions, which should give us a higher degree of confidence in PP). But this option is not open to the Individualist. Because the Individualist takes individual knowledge to be the key to

[90] For exposition, see Balaguer 1998 and Linsky & Zalta 1995. For criticism see Cheyne 1999 and Restall 2003. For a further defense see Rabin 2007.

theoretical success, she cannot afford to abstract away from the particular reasons that ground that knowledge. The Individualist must be able to add up the knowledge of individuals in a way that results in a meaningful agreement, and the only way to accomplish this is if this knowledge is based on a *common body of evidence*.

In order to deal with these various issues the Individualist might shift to a counterfactual formulation of (ISK). Thus instead of considering what theorists actually believe with regard to *t*, the claim can be reformulated in terms of what the appropriate specialists *would* believe if they reflectively considered the evidence for *t*. Thus the following counterfactual claim produces a much more plausible version of Individualism:

(ISK_C) *S*(*t*) IFF a majority of philosophers who specialize in the area to which *t* belongs *would* know that *t* is true on T if they rationally considered the available evidence for *t*.

Of course, counterfactual analyses are not without their own problems. For instance, the similarity relation that serves as the commonly accepted basis for semantic analysis of counterfactual claims might not give us the right scenario.[91] It might be that the counterfactual is made false by the fact that the "closest" possible world in which a majority of specialists consider the appropriate body of evidence currently available for *t* is a world in which these specialists have very different concerns than our own, and thus might also happen to be a world in which researchers are not disposed to form the same

[91] The problem is that counterfactual analyses such as (ISK_C) often underspecify the antecedent of the conditional such that the nearest possible world that satisfies the antecedent condition differs in such a way from the actual world that the consequent is not met. For instance, Williamson considers this problem in his treatment of appeals to ideal rational agents in establishing an objective notion of evidence. As Williamson puts it, such analyses fail "in the way in which counterfactual analyses usually fail, by ignoring side-effects of the conditional's antecedent on the truth-value of the analysandum" (2000a, 209). As Williamson notes, the observation is originally due to Robert Shope (1978). Specifically, the example considered below would be an instance of what Shope calls the *ceteris paribus fallacy* (1978, 405). It is also an instance of the second version of what Shope calls the "conditional fallacy" if the scenario truly describes the similarity relation (1978, 403).

beliefs on the same evidence as we are.[92] However, these sorts of issues can be largely avoided by adding further reasonable stipulations about the antecedent scenario (i.e., that our counterparts share our interests, etc.).

One serious problem for (ISK_C) lies in its reliance upon the notion of rational consideration. If too much is built into the notion of rational consideration, then (ISK_C) will simply become disconnected from how we ordinarily reason and would thus become worthless methodologically.[93] Alternatively, if the notion of rational consideration is too weak then (ISK_C) produce the wrong results because it fails to rule out cognitive biases, or because the researchers in question do not consider some of the evidence that has been presented in favor of the theory (either because they simply can't be bothered, or because they are not aware of this evidence). In order for (ISK_C) to work it must supply a moderate notion of rationality that falls somewhere between the ideal rationality of a perfect logical computer and the irrationality that sometimes befalls human beings. The problem for the Individualist is that in order to meet the proper balance between ideal rationality and irrationality, (ISK_C) will have to tacitly presuppose some form of

[92] For instance, suppose that *t* is a metaethical theory, but that the primary evidence for *t* only becomes accessible after a meticulous comparative examination of competing theories in formal semantics. It might be the case that the closest world in which a majority of metaethicists actually have the patience to engage in such a project is one in which metaethics is only of interest because of its implications for theories of natural language semantics. This might be a world in which the appropriate specialists are all fictionalists or anti-realists who are only interested in providing models of natural language. In this case, the appropriate specialists are unlikely to actually form a belief about *t* on the available evidence, for they simply aren't interested in metaethics. It would then follow that the counterfactual conditional is false, *a fortiori* (since knowledge requires belief).

[93] If the notion of rational consideration involves incredibly sophisticated reasoning then (ISK_C) will not offer us a usable standard of theoretical success, for we would then need to possess this sophisticated reasoning in order to evaluate real world theories. There are many unproven theorems in mathematics that ideal rational agents would know, but since ideal rational agents would be far better at producing proofs than us, the standard would be utterly unhelpful as a guide to evaluating whether our actual theories are supported by our current evidence. It might be the case that a perfectly rational agent would give the Riemann Hypothesis a probability of 1 on our current evidence, but since we lack this agent's powers of deduction this fact is no more evident to us than the Hypothesis itself. Williamson makes the same point using Goldbach's Conjecture as an example (2000a, 209-210).

Collectivism. Finding the balance is not the serious issue. On the contrary, it should be easy. The notion of rational consideration that is required is obviously one on which the researchers consider the evidence that is contained in the current literature in accordance with standardly accepted norms of reasoning. It is the fact that the objective evidence supports a theory that makes it reasonable to assume that individuals would believe it if they considered the available evidence rationally. But this demonstrates that the counterfactual conditional in (ISK_C) must parasitize the Collectivist's notion of quasi-objective evidence in order to attain plausibility. To treat the knowledge of individuals in the counterfactual scenario as prior to the objective notion of evidence is to put the cart before the horse.

Another problem for Individualism lies in the fact that the Collectivist can give us general principles in terms of quasi-objective probabilities, while the Individualist cannot. Recall from our discussion of Thomson's Violinist case above that the Collectivist can offer plausible methodological principles regarding the effect of arguments by analogy on theoretical success. The first instance of an argument by analogy should be considered to increase the evidential probability of the theoretical claim only slightly. The longer an argument by analogy survives supposed analyses and counterexamples, the more it confirms the theory in question. If the argument does not survive, the probability of the theory should be recalculated by first returning it to its initial status prior to the introduction of the argument and then modifying this status according to the evidential impact of any additional considerations that do not depend upon the failed argument.[94]

[94] As we stated at the end of §1.1, this requires a non-standard probabilistic account of evidence that denies the monotonicity of evidence (see footnote 21). Thus this would require some form of record keeping. I think it obvious that we do in fact reason in this sort of record-keeping manner. Our ability think in a higher-order manner by imagining what would be rational if we had different evidence is itself a critical

In contrast, (ISK_C) seems to depend upon binary notions of justification on which theories can only vacillate between success and failure.[95] The Individualist could attempt to get around this by averaging individual credences (i.e., subjective probabilities/degrees of belief), but there is no guarantee that the result would be useful. First of all, credences break away from justification and it is the latter that the Individualist is concerned with. Second, the credences of different individuals are likely to vary in strength even if the individuals are operating with the same evidence. That is, some individuals are likely to give more credence to certain kinds of evidence than others. The fact of the matter is that an Individualist can only make sense of general methodological principles by assuming some form of Collectivism.

Perhaps the most serious problem with (ISK_C) is simply the fact that the counterfactual claim cannot easily be used as a heuristic for determining whether a theory is *actually* successful. In principle one could provide direct evidence that a theory meets (ISK) by simply surveying what philosophers actually believe and the reasons they give for this belief. Indeed, the ability to appeal directly to consensus in this manner would presumably be one of the main benefits of (ISK). If most specialists believe that a theory is true and appear to be justified in believing that it is true, then that would be good evidence that the theory is successful. In contrast, the claim that philosophers *would* know something if they considered certain evidence cannot receive obvious confirmation from surveys. This is particularly problematic given our more specific concern with

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part of our epistemic toolkit. It is hard to imagine how a restrictive updating schema using Bayesian or Jeffery-conditionalization could handle our ability to radically shift worldviews. Such updating methods are incremental by necessity.

[95] Individual researchers will vacillate from having knowledge on the argument to having this knowledge defeated when their opponents present counterexamples, to regaining their knowledge when they come up with ways to defeat these counterexamples. See footnote 79.

intuition. The claim that everyone would know or be justified in believing *p* if they rationally considered an argument for *p* should not be too difficult to assess so long as the only difference required between the actual world and the counterfactual scenario is whether or not the relevant individuals have considered the argument in question.[96] In contrast, a counterfactual scenario in which people have different intuitions is more likely to differ from the actual world in some meaningful respect. The people in the scenario might have different intuitions because their world has something that makes the intuition true, while ours does not.[97] In order to rule out these cases one would have to show that the intuition in question is in fact accurate before using (ISK_C) to evaluate it. But this would obviously defeat the entire purpose of (ISK_C).

1.3.2.2. The Priority of Theoretical Success

Our analysis of Individualism shows us that the view faces several issues. The problem is that the Individualist needs to arrive at an objective notion of evidence by appealing to the epistemic states of individual researchers, but these states are too prone to idiosyncrasies and variation to give the Individualist reliable results. Committed Individualists might not find this very convincing. They might think that their analysis can be further modified to avoid the counterexamples. However, such a response would

[96] Recall that the evidence provided by arguments is easily transferred from individual to individual so long as those individuals share similar evidence regarding the premises of the argument (see the end of §1.2.1). Thus presumably in this context (ISK_C) would be satisfied by a situation in which the relevant individuals share all the same evidence as their actual counterparts except for the fact that they have considered the argument in question without committing any fallacies. To assess this claim we merely need to know our current evidence plus the evidence provided by the argument in question.

[97] By analogy, consider the claim that everyone would know that some claim is true if they had different perceptual evidence. The problem is with this is that there often has to be a substantial difference in the world in order for people's perceptual evidence to differ. It is true that if people were to have perceptual evidence that Bigfoot exists, then they would likely know that Bigfoot exists. But one of the best explanations for the difference in perceptual evidence between the actual world and this counterfactual scenario is precisely the fact that Bigfoot exists in their world and doesn't exist in our own. Thus the fact that the people in the counterfactual scenario would know that Bigfoot exists is obviously irrelevant to whether Bigfoot actually exists.

miss the point. The primary problem with Individualism is not that it is difficult to provide an analysis that avoids counterexamples, but that it tacitly needs to rely on a Collectivist notion of evidence in order to be considered plausible. Moreover, we have seen that Collectivism appears to be superior to Individualism in that it can offer a simpler probabilistic treatment of evidence and can more easily construct general methodological principles.

None of this is a coincidence. The technical problems that face Individualism are merely a symptom of a deeper problem. The deeper problem with Individualism lies in the fact that the knowledge of individual researchers is dependent upon the collective efforts made by the field. The Individualist attempts to explain theoretical success in terms of individual knowledge, when in fact it is individual knowledge that is enabled by theoretical success. In fact there are several reasons why Collectivist methodological standards are needed in order for individuals to attain knowledge. First, without these methodological standards the field would not be able to supply individual philosophers with the tools and background knowledge they need to conduct their own research. Second, these standards provide individuals with both support and criticism that they are unlikely to come up with on their own. Third, the standards prevent a sort of *tragedy of the commons* from occurring by restricting the liberties that can be taken with evidence.

The first type of dependence is fairly straightforward. In order for individual philosophers to conduct research, they must first be educated. This education consists of two things. First, these philosophers must be taught how to use central philosophical tools. For instance, they must be trained to be competent with logic, shown how to spot faults in other philosophers' arguments, and taught how to construct arguments of their

own. There must be widespread agreement on what the valid tools are in order for this training to be successful. This, however, is only possible if there is widespread agreement on what contributes to theoretical success. If Individualism were true one would expect young philosophers to be told to use whatever methods best contribute to knowledge, and then be given an epistemological theory. However, this is generally not what happens. Students generally are not given complex epistemological explanations for why they are not justified in believing the conclusion of their arguments. Indeed, the fact that particular theories of epistemology are controversial would make it extremely difficult for instructors to evaluate their students' work in this manner. Rather, they are simply told whether their arguments are good or bad *simpliciter*.

The second part of the philosopher's education consists in explanations of what the current areas of research are and the positions that comprise this research. This too is obviously dependent upon the existence of Collectivist methodology. Research is built up over time (albeit obviously more slowly and with less consensus than in the sciences). Young philosophers are generally not told whether the authors are justified in believing their positions. Rather, they are told what the live positions are and what the arguments that support those positions are. This information is presented in terms of an objective body of evidence that confirms or disconfirms a position, not in terms of what particular individuals might know or believe. Thus philosophical pedagogy clearly depends upon a Collectivist conception of philosophical methodology.

It is also easy to see how a Collectivist methodology benefits individual researchers after they have received their degrees. On the reasonable assumption that there is an epistemic standard to seek evidence for or against one's position, it is clear

that a Collectivist methodology benefits individuals in their pursuit of knowledge insofar as it provides a way to filter and focus their access to evidence that is relevant to their beliefs. (It is exceedingly doubtful that individual researchers could come up with the arguments provided by their colleagues all on their own.) The mere fact that someone has published an article in defense or criticism of a position in a peer-reviewed journal provides a practical reason for thinking that this article will contain relevant evidence.[98] This *prima facie* justification is enabled by the fact that the peer-review process employs Collectivist methodological standards in order to screen out good or bad arguments. If individualism were true one would expect peer-reviewers to judge whether articles should be published according to whether or not they think people in the field will or would know the claims of the article. Not only would this be unnecessarily difficult, it would be simply inappropriate.[99] What is important is whether the argument affects the evidence for the claim in question according to standardly accepted practices (e.g., whether it uses standardly accepted arguments, etc.). This Collectivist standardization of evaluative practices is a large part of what enables meaningful discussion and disagreement, and thereby enables individual knowledge.

[98] Note that I am assuming that mere testimonial evidence is generally too weak to count as decent evidence for a developed philosophical position: it the structure and content of the argument that gives it its evidential status, not the fact that the argument was presented by some philosopher (e.g., philosopher *x*'s testimony that moral realism is the correct view does not count as very good evidence for moral realism). Note also that the claim here is not about the evidential status of the arguments themselves, but about the practical evidence that a researcher has to investigate an argument when attempting to gather evidence.

[99] In order to decide whether or not members of the field will or would come to possess justified beliefs on some new argument one would not only have to know whether the argument itself is good, but also how the argument interacts with the total evidence for the claim under consideration (not to mention a bevy of psychological facts about members of the field). Even if one could accomplish this, the mere fact that people would not know the conclusion even if the argument provides evidence is certainly no reason to dismiss the argument. If peer-reviewers had to base their decisions on speculation about what people could know their decisions would be based on controversial epistemological claims. This would mean that different authors could dismiss certain journals as providing irrelevant data.

The most important way in which Collectivist methodology enables individual knowledge rests in the simple fact that it ensures continued progress in getting at the truth. Without implementing and explicitly maintaining methodological standards we would be far more prone to make exceptions to demands for evidence when we think the claim in question is reasonable. Sometimes the claim in question will be correct, so no harm is done. However, there will undoubtedly also be instances in which the apparently uncontroversial claim is incorrect and our complacency prevents us from seeing it.[100] Once we allow exceptions to the rule, we face a tragedy of the commons: if researchers only occasionally make exceptions to standard practices when they think they are justified in believing something things will be okay, but if researchers commonly make exceptions inquiry is likely to be stultified. Since there is no way of knowing in advance which exceptions are safe, the best solution is to enforce standards universally.

For instance, no matter how independently plausible they find a claim to be, scientists will still demand that evidence be presented for the claim meeting standards such as replicability and a low *p*-value. The sort of knowledge these scientists pursue is exceedingly difficult for individuals to obtain on their own. Many extremely plausible scientific theories throughout history have failed.[101] If these theories had not been tested and subjected to these sorts of stringent standards, we would still believe them. The knowledge that individual scientists now possess is only possible because the justified

[100] The assumption that the parallel postulate was a necessary axiom of geometry is a clear example of this. The parallel postulate seems as basic and plausible a principle as one could get, yet it would have been a terrible blunder if mathematicians had simply never bothered to question it.

[101] No doubt at least part of the driving motivation behind the experimental method employed in the sciences is that we are *too* good at coming up with convincing explanations of phenomena. This is hardly a surprise; given cognitive and evolutionary parsimony there is reason to suspect that the same psychological mechanisms responsible for our finding explanations convincing are also involved in generating those very explanations.

beliefs of past scientists were undermined by the fact that they could not pass these stringent standards.

Recall our example of the physicist who claims to know that the Higgs Boson exists before CERN produced the necessary experimental results to confirm the claim. It is fairly clear that the physicist has very good reasons to think that the Higgs exists: it is predicted by the most well-confirmed theory of particle physics, and the competing theories are all extremely implausible by comparison (or at least we can suppose that this is the case). Suppose that she were to make this knowledge claim when listing the particles that are known to exist according to the Standard Model to a lay person unfamiliar with contemporary physics (e.g., "we also know that the Higgs Boson is responsible for mass"). Now suppose that another physicist overhears this and interrupts in order to point out that the existence of the Higgs has not yet been confirmed by experiment. It is fairly clear that we would be apt to criticize her. Perhaps this is because her knowledge of whether the Higgs exists is straightforwardly dependent upon whether the existence of the Higgs has been confirmed according to the standards of her field. [102] This would directly show that individual knowledge is dependent upon Collective standards. But even if we were to grant that she does in fact know that the Higgs exists, it is clear that the reasons on which this knowledge is based are themselves clearly dependent upon the existence of Collective standards. The most important part of her evidence for this belief is the fact that the Higgs is a prediction of the Standard Model,

[102] One way of explaining our reaction is by claiming that she *does not* in fact know that the Higgs Boson exists because there is something about her assertion that subjects it to higher epistemic standards. One might do this by appealing to some form of contextualism or interest relative invariantism about knowledge. Alternatively, one might explain our reaction by claiming that she in fact knows, but the fact that she is a physicist appears to give her knowledge assertion the pragmatic implicature that she is accurately representing what has been established according to the standards set by her field. In either case the salient fact is that the existence of the Higgs has not yet been confirmed according to the collective standards set forth by the community of physicists.

which has already received massive confirmation according to the standards of her field. Without the ability to appeal to the fact these collective standards, she would not be able to rule out competing explanations. In either case it is clear that her knowledge is dependent upon the collective standards of the field.

One final piece of evidence for the claim that methodology is prior to individual knowledge is the fact that Individualism seems to be at odds with academic specialization. Many of our counterexamples to Individualism in §1.3.1 played on the fact that researchers are unlikely to be aware of all of the background theories that are relevant to the claims they study. Individualism requires that individuals know all relevant background claims in order for their claims to be successful. But it would be an exceedingly poor division of labor if every researcher had to be intimately familiar with every theoretical claim that could be relevant to what she studies. Indeed, it seems perfectly possible for individual researchers to make progress on the problems with which they are concerned while lacking knowledge of background theoretical considerations.[103] If this is true, this would falsify Individualism. But even if we do grant that these individuals have knowledge in spite of their ignorance of background theories, it is still clear that their knowledge is dependent upon the fact that there are other specialists doing the work for them. That they are justified in leaving background

[103] For instance, a theory of cognitivism in metaethics tells us that sentences containing normative terms express propositions. Clearly a metaethicist need not be an expert in the theory of proposition in order to defend cognitivsm. In particular, a metaethicist need not even be particularly familiar with the general ontological arguments for the existence of propositions in order to argue for cognitivism. She need only be familiar with the arguments concerning the conditions that must be met by a sentence in order for that sentence to express a proposition. But clearly she cannot know or be justified in believing that sentences containing normative terms express propositions unless she also knows or is justified in believing that propositions exist, *a fortiori*. While one might say that she knows that propositions exist on testimony from other experts, it is less than clear that mere testimony is enough to justify such a sophisticated theoretical belief. Moreover, even if we imagine that most metaethicists are unconcerned with the claim that propositions exist, it seems as though this is no barrier to them successfully establishing metaethical claims.

considerations to others is only made possible by the fact that they are part of a field that employs collective standards such that mistakes made of ignorance can be corrected by other specialists.

For all of these reasons it is unreasonable to attempt to explain theoretical success in terms of what individuals know. When these arguments are added to the arguments presented in §1.3.2.1, they allow us to see how deeply implausible Individualism truly is.

1.4. Methodological Constraints

I take the preceding arguments to constitute definitive evidence against Individualism. At the very least they demonstrate that substantial arguments are needed in order to defend Individualism. Philosophy *is* a team sport. Academic research is funded because it is supposed to reach an objective description of the world from which society might benefit in various ways. By allowing individual researchers to simply do as they please, the entire field is essentially failing in its mission and wasting the resources that have been provided to it. Philosophers are *not* permitted to only write for audiences who share their worldview, while "agreeing to disagree" with those that do not. They are certainly permitted to pursue their own interests on their own time, but this is not why they receive funding. It is not only legitimate for the field to enforce a set of standards regarding what counts as a legitimate method: it is *obligatory*.

But let us not lose track of our primary goal: the methodological evaluation of the use of intuitions in metaphysics. The reason why we have spent so much time arguing against Individualism is that Collectivism has substantial implications for the use of intuitions. Collectivism implies that what is important is not whether individual philosophers can use intuitions to gain knowledge but whether the field can collectively use intuitions in a systematic way to establish philosophical conclusions. Of course, this is also true for arguments and any other method employed by philosophers. However, unlike arguments intuitions are mental states that only individual researchers have access to (see §1.3.1.1.4). We shall explore the question of how intuitions can function as evidence in Part II. The remainder of Part I shall focus on drawing out some general methodological constraints.

1.4.1. Consensus Constraints

Throughout the remainder of this thesis we shall assume that some form of Collectivism is true. According to Collectivism, whether or not a theory can be considered successful is grounded in whether or not that theory meets the constitutive rules that determine theoretical success that have been settled upon by the theoretical community. I use the phrase 'constitutive rule' in the same sense as it is used by Timothy Williamson (2000a, 238-240). To borrow Williamson's analogy, academic research is constituted by rules in the same sense that games like football or chess are constituted by rules. In order to successfully play a game one need only abide by the constitutive rules set down at the start of the game. Fun may be the *motivation* that drives people to play games, but it is not necessary that anyone to have fun in order for a game to take place. A football game can easily take place even if every single football player is miserable. This is likely to occur, for instance, when the game is played in +100°F heat. The conference officials will not refuse to record the score simply because no one had fun. Analogously, individual knowledge might be the motivation that drives people to engage in research, but it is not necessary that anyone to know a theory is true in order for it to be successful.

Simply by considering the nature of academic research we can arrive at some considerable constraints upon what can count as a valid research method. Recall from our discussions in §1.1.2 and §1.3.1.1.2 that the only reasonable explanation for why philosophical research is funded is that the description of the relevant phenomena achieved by the research itself has some non-arbitrary public value. The existence of competing contradictory descriptions is quite obviously not as valuable as having a single correct description. Thus metaphysics, like the sciences, must at least aim at ultimately

achieving consensus. While it is of course difficult to reach consensus, it nevertheless remains a regulative goal of all academic research that its practices meet standards that are conducive to eventually reaching a consensus.

This simple fact has significant methodological import. It implies that when there is a theoretical disagreement it is the job of members of the field to attempt to find some way to settle the dispute. If intractable disagreement continues without the addition of substantial new evidence, this can only be because the disputants disagree about what counts as evidence. When this happens, the field is obligated to engage in a higher-order methodological examination until some agreement can be reached about the nature of evidence. Even when the subject-matter of the dispute is so complex or so beyond our epistemic capacities that the dispute is intractable, those who are engaged in the dispute should at least aim at reaching a higher-order consensus regarding whether or not the first-order dispute should be set aside. To continue on with the first-order dispute without making any attempt to settle it or reach a consensus is simply negligent.

In other words, we must be reflective enough to insure that the methods we employ cannot be used in such a way that will damage our ability to reach consensus. This leads us to the following general and minimal methodological constraint:

> **(MC)** A field for which Methodological Realism is true is not permitted to employ methods that can be used to achieve contradictory results without it being in principle decidable how to resolve the contradiction.[104]

There are several things to note about (MC). First of all, (MC) is a negative demand. It is still largely up to the members of a field to decide what sorts of methods are best for their

[104] Note that (MC) does *not* imply that we must always know what our evidence is. All (MC) demands is that it be decidable *in principle* what we should do with our evidence. Even if we cannot know what our evidence is in every case, it is still easily decidable in principle that we should use the stronger evidence. Thus (MC) is unaffected by Williamson's anti-luminosity arguments (2000a, chapters 4 and 9).

purposes. (MC) only places a general restriction on these methods. Thus (MC) should not be confused with the positive demand that philosophers are obligated to adopt only methods that they know in advance to be efficient at producing a consensus. This would be far too strong.[105]

The second thing to note is that (MC) should be understood as applying to cases where the members of the field can be reasonably expected to know whether their methods produce contradictory results. However, if the methods in question have led to contradictory results, this will be revealed by theoretical disagreement. Thus the only way the researchers in question could be ignorant of these contradictory results is if they are ignorant about whether there is actual theoretical disagreement within their field, and this latter form of ignorance can only be due to negligence. Thus in the most significant cases, ignorance is not an excuse.[106]

There are two main ways in which a method can fail to pass (MC). First, a method can fail to pass (MC) if it can be used in different instances to establish contradictory results without it being decidable in principle which instance constitutes a proper application of the method.[107] For instance, imagine if scientists were using a rectangular box in order to collecting data about a certain set of phenomena. Furthermore, suppose that (1) the scientists have no real understanding of the inner workings of this instrument,

[105] For instance, one such method could consist in everyone agreeing that theoretical disputes are to be decided via rock-paper-scissors. This would lead to consensus, but would obviously not do so in a manner that tracks the truth of the relevant theories.

[106] As we shall see in Chapter 2, it is fairly obvious upon any sort of decent reflection on current metaphysical disputes that intuitions *do* in fact lead to contradictory results.

[107] Note that this says nothing about whether or not the method in question is *reliable*. An unreliable method can still be used improperly. A crude example: one could adopt a method of deciding which train to take by flipping a coin. Presumably, this would be a fairly unreliable way of getting to one's destination. Nevertheless, if one simply pulled the coin out of one's pocket and decided upon which side was facing upward, this would be an improper application of the method.

and (2) there is no way to independently calibrate this instrument (i.e., there is no other instrument that gives us data about the phenomena in question). The box has dimensions of 1′x2′x3′. At one of the 1′x2′ sides of the box there is a recording device, and on the opposing side there is an LED light that flashes once when a certain event is observed, and twice when it is not. Suppose that when using the box to test whether the relevant event occurs under a specific set of conditions scientists find that the light flashes once when the box is rested on one of the 2′x3′ sides and twice when it is rested on one of its 1′x3′ sides, even when all other conditions are controlled. It would clearly be negligent for different groups of scientists to selectively use one of these results in support of their theories without attempting to determine which method is correct.

However, there is another more subtle way in which methods can fail to pass (MC). Even if researchers can agree as to when something counts as the proper application of a method, they can still disagree about how the evidence delivered by this method should be weighted in terms of other forms of evidence. (MC) also prohibits researchers from using methods where one group of researchers takes the evidence provided by these methods to override other forms of evidence, while another group of researchers give primacy to these other forms of evidence, and where it is not decidable in principle who is correct given the field's understanding of the method in question. Note that this is *not* to demand that researchers be forced to stop pursuing any research program but the one that is currently best supported by the evidence. This would be foolhardy, to say the very least. Rather, it is to demand that the field in question attempt to get clear on which theories are best supported by the current evidence by making sure that everyone is on the same page about what should count as evidence. It is still

reasonable to pursue alternative theories because there is always a chance that we will run into some new form of evidence that will tip the scales in the other direction. Nevertheless, if the field cannot even reach a consensus about the general ways in which theoretical success should be measured then theoretical progress is likely to fracture. As different research programs adopt different conceptions of what counts as evidence these programs will begin to diverge from one another in more and more pronounced ways, both in terms of their theories and in terms of their methodology, until it becomes far too difficult to bring them back into contact with one another.[108]

1.4.1.1. Methodological Dogmatism

As we have already argued (in §1.1.1 and §1.3.1.1.2), the only remotely reasonable explanation for why research receives funding is that the goal of this research is to reach an explanation of a certain phenomenon, and that this explanation itself is something valuable. Thus it is also the duty of researchers to make sure they are employing the proper means for achieving this end. The existence of contradictory paradigms undermines the value of academic research. Disagreement is acceptable when there is general agreement that the evidence itself is indeterminate. This can happen when everyone generally agrees (1) about what sort of evidence counts in favor of the theories in question, (2) about how this evidence should generally be weighted, and (3) that the theories in question are almost equally well supported by the evidence (i.e., equally simple, well-confirmed, etc.). However, a field is not permitted to allow divergent

[108] For instance, consider a modification on the above scenario where the box in question produces the same results no matter what side it rests on. Suppose that there are two theories that only conflict with one another on the predictions that are tested by the box and are otherwise equally well-confirmed by experiment. Suppose the theory that correctly predicts the results given by the box is far more complicated and far less elegant than the theory that makes predictions that conflict with the results given by the box. It would clearly be negligent for different groups of scientists to adopt either theory without attempting to determine what the deciding piece of evidence should be, and even more negligent to continue to develop the different theories without attempting to settle this problem.

research programs to form due to differences in the methods employed by researchers and the standards concerning what counts as evidence.

The goal of academic research obligates members of a field to reflect upon the adequacy of the methods they employ when intractable disagreements arise; i.e., to meet the demands of (MC). Failure to meet this obligation results in

Methodological Dogmatism $=_{df}$ The phenomenon in which the members of a field are aware of a persisting first-order disagreement that is due to divergent use of methods, but continue to use these divergent methods in these first-order theorizing without reaching a higher-order consensus about which methods should be used.[109]

Put simply, Methodological Dogmatism (hereafter 'dogmatism' where the context is unambiguous) amounts to a form of employee negligence.

Some paradigm cases of dogmatism occur when members of a field form an *echo-chamber*. An echo-chamber is a community of researchers in which a particular research program is pursued without regard for either competing theories or other challenges posed by the broader community of researchers. An excellent example of an echo-chamber is the Discovery Institute [sic], which exclusively publishes pro-"intelligent design" material. The primary problem with intelligent design theories is methodological: they cannot be used to form testable biological hypotheses. Hence what makes the Discovery Institute an echo-chamber is that the material it publishes purports to be biology, yet does not abide by the methodological standards set down by the actual field of biology.

[109] Thus methodological dogmatism should be distinguished from what is commonly called *foundationalism*. Foundationalism claims that knowledge (or theoretical success) ultimately rests on first principles that themselves cannot be justified. One can be committed to such a theory without being guilty of methodological dogmatism. So long as first-order disputes do not rest upon a disagreement about fundamental methodological principles, foundationalists can avoid dogmatism.

Dogmatism can also occur when competing theories are continually developed using different evidence and methods while only making minor contact with one another. Let us call this form of dogmatism *schismatic complacency*. A field suffers from schismatic complacency when different members of the field disagree about what counts as good evidence, yet continue to develop their competing theories without earnestly attempting to resolve this disagreement. The problem in this case is not the lack of interaction, but that the interaction is superficial: the researchers cite different forms of evidence in support of their theories without engaging in any real attempt to square their methods and evidence.[110] Disagreement isn't a problem when it is simply due to a lack of evidence, as the addition of new evidence can eventually settle things. However, when researchers have different conceptions about what should even count as evidence they will end up building their theories on different evidence. This will almost invariably result in theoretical divergence. By continuing to engage in first-order theorizing with disputed methods these researchers are clearly neglecting their duties.

Schismatic complacency can occur in several ways.[111] The most obvious way in which schismatic complacency can occur is when, in a disagreement over theory *t*, different researchers continue to put forward different types of evidence for *t* while dismissing other forms of evidence put forward by their colleagues and without engaging in any sort of methodological inquiry as to who is correct. Schismatic complacency can

[110] Echo-chambers occur when one group diverges from the methodological norms of the community In contrast, schismatic complacency occurs when there are no established norms. Indeed, the entire problem with schismatic complacency is the lack of such norms.

[111] A good measuring stick for schismatic complacency is the degree of difficulty it takes to discern whether a particular theory or its competitors is favored by the current evidence by examining what has been published on the topic. One might also survey the opinions of specialists, but one would need to know what members of the field think about the evidence that supports their favored theory. Mere disagreement about which theories are true could be the result of indeterminate evidence as well as dogmatism.

also occur when the only methodological engagement occurs superficially occurs in the context of discussions concerning particular first-order disputes. Here the methodological disputes will be about the nature of specific pieces of evidence relevant to the research project in question. This level of engagement is unlikely to produce any general methodological agreement concerning the type of evidence involved that could settle these disputes.[112] Schismatic complacency can also occur when there is agreement about what counts as evidence, if there is disagreement about how this evidence should be weighted. For instance, suppose two groups of researchers differ in that they always give more weight to either theoretical simplicity or explanatory power respectively. [113] There is no guarantee that these researchers will *ever* reach any sort of agreement. On the contrary, the more they build their theories the more these theories will diverge.

1.4.2. Intuitions, Individualism, and Dogmatism

Without an account of what counts as an intuition and an account of the strength of the evidence provided by intuitions, it becomes possible for two people with different conceptions of the nature of intuitive evidence to settle on contradictory conclusions even

[112] To borrow the example above, this would be equivalent to different groups of scientists pursuing different research programs in which they use conclusions drawn from different versions of the box experiment to draw further conclusions about the phenomena in question, while only occasionally arguing with one another superficially about whether the 2′x3′ method or the 1′x3′ method is more reliable. It is clear in this scenario that the field needs to settle once and for all which method should be taken as legitimate. The reason for this is that, as was stipulate in the original presentation of the scenario, these researchers have no other way to get new evidence about the phenomena that the box gives them access to. Thus if the issue is to ever be resolved, it *must* be resolved through an in-depth methodological analysis.

[113] A recent example comes from economics. Here the disagreement is between researchers who take the goal of economics to consist in the production of rational decision-theoretic models that are allowed to diverge significantly from empirical data, and researchers who are willing to attempt a piecemeal psychology-based approach to economic theory because the simple decision-theoretic models do not make accurate enough predictions. This helps illustrate how destructive this sort of disagreement can be, as these different economic theories yield different results when they are used to make incredibly important public policy decisions (e.g., the decision to combat economic downturns through austerity measures vs. Keynesian government investment).

if they agree on all other forms of evidence. Thus if philosophers in a particular dispute continually disagree due to disagreement about what intuitions should count as evidence and how this evidence should be weighted in terms of the other available evidence in support of the theory, and there is no general methodological theory about intuitions that could be used to decide this dispute, then this is good evidence for the existence of schismatic complacency regarding intuition.

We should distinguish between an epistemological notion of justification and a methodological notion of justification. The question we are concerned with is whether the method of appealing to intuitions is methodologically justified in metaphysics. Our rejection of Individualism in §1.3 establishes that we cannot assume that the appeal to intuition is methodologically justified even if it can be shown that the beliefs we form on intuition are epistemically justified. An individual can have intuitively justified belief that *p* without being methodologically justified in claiming that *p*. Recall our discussion of the Axiom of Choice (AC). (Roughly, (AC) is the claim that for every set of non-empty sets, there is a set that contains one member from each member of that set.) Most rationalists are going to say that a belief in (AC) is justified on intuition. Most mathematicians might also believe both that (AC) is true and that they are justified in believing that (AC) is true. However, no mathematician would assent to the claim that (AC) has been *established* by the fact that (AC) is intuitive. This isn't because mathematicians need to be told the correct epistemology of mathematics. It is because mathematicians embrace a Collectivist methodology: they only recognize certain forms of evidence as being capable of establishing (AC). §1.3 has shown that philosophy is no different from mathematics in this respect.

What this shows is that one cannot assume that establishing an epistemological conclusion will be enough to justify a methodological claim. In particular, this shows that the disputes between Bealer, Lycan, Williamson and anyone else who debates the methodological role of intuition by making epistemological claims about intuitions are misguided. The methodological evaluation of intuition cannot be straightforwardly reduced to claims about the epistemic status of intuitions. One can be justified in believing *p* on intuition, but methodologically unjustified in appealing to this intuition to establish *p*. Thus even if rationalists *could* show that we can know things on intuition, this still would not justify the method of appealing to intuitions in order to establish philosophical claims. In order for appeals to intuition to pass (MC), philosophers must adopt standards for reasonably determining whether someone actually has an intuition, and for determining the evidential status of intuitions in regard to when they should outweigh or be outweighed by other forms of evidence. It is a form of negligence to attempt to use intuitions as evidence if appeals to intuition do not pass this test. These considerations are only possible once one has realized that Individualism is false and that all philosophical methods must pass something like (MC).

2. Intuitions in Metaphysics

Part II attempts to show how intuitions are in fact used as evidence in metaphysics. In particular, it aims to show that the appeal to intuition in metaphysics has resulted in a form of schismatic complacency in several debates. The plan for Part II is as follows. §2.1 examines inadequate theories of how intuitions function as evidence in philosophical practice. §2.1.1 argues against the claim that intuitions simply are not evidence. It is argued that this claim rests on a fairly weak body of sociological evidence and that the best response to this claim is simply given by the analysis of the roles intuitions play in various metaphysical disputes in §2.4. §2.1.2 argues against a general 'partner-in-the-guilt' objection that any criticism of the methodological role of intuitions in metaphysics will affect the rest of philosophy. This general objection fails because it must assume that intuitions cannot have different evidential strength in different domains, and this assumption could only be justified by appealing to a specific theory of how intuition works. Thus any partner-in-the-guilt style argument can only be successful by first showing that a specific theory of intuition is true. §2.1.3 argues against the claim that intuitions function to provide dialectical justification. It is shown that philosophical research cannot be reduced to particular dialogues and that the standards governing dialectical evidence are not sufficient to insure that claims made in research are methodologically justified. §2.1.4 argues that Timothy Williamson's arguments against Evidence Neutrality cannot establish anything of importance regarding intuition. §2.1.5 argues against the claim that the use of intuitions in metaphysics should ultimately be understood in terms of a Moorean appeal to common sense. It is argued that it would undermine the very purpose of academic research if this research were to treat common

sense as a fundamental and non-revisable form of evidence, and thus that this strategy would be self-defeating in that it would imply that research in metaphysics is pointless. It is then argued that the relevant claims in metaphysics are so abstract and technical that they cannot be subsumed under any reasonable conception of common sense.

§2.2 draws out some preliminary points about the nature of metaphysics. It is divided into two main sections. §2.2.1 describes the nature of metametaphysics and metaontology and how the differ from the methodology of metaphysics. In particular, it is shown that metametaphysics and metaontology concern the semantics, metasemantics and epistemology of metaphysics and ontology respectively, whereas the methodological study of these disciplines focuses on the methods the members of the field employ in order to evaluate theories. §2.2.1.1 describes the nature of ontology in greater detail. It establishes that the primary ontological tradition embraces a *Quinean Metaontology*, wherein existence claims are understood in terms of the existential quantifier of first-order logic. It then briefly relates how this assumption is used to formulate methods for evaluating ontological theses. §2.2.1.2 describes metaphysics. It establishes that only on a very narrow Quinean view is metaphysics understood solely in terms of existence claims. It establishes that metaphysics is more broadly concerned with constructing a holistic theory of how reality "hangs together" and explains how many metaphysical questions concern grounding claims. §2.2.2 describes the distinction between substantivism and superficialism in metametaphysics. It argues that the substantivity of a question can be measured in two dimensions: in terms of how difficult (or easy) it is to answer the question and in terms of how important the implications of the answer to this question are. §2.2.2.1 describes the various forms that superficialism might take. In particular, it

discusses various forms of superficialism that depend upon the claim that the language in metaphysical disputes is defective. In doing so, it elaborates upon the distinction between semantics and metasemantics and how different versions of either theory can result in different forms of superficialism. It is shown that too much emphasis is put on semantics and too little is put on metasemantics. This is demonstrated with an example concerning moral realism.

§2.3 draws out some preliminary points about the nature of intuitions. It is divided into two main sections. §2.3.1 describes the potential ways in which intuitions might be individuated phenomenologically, how this ties into theories of intuition, and the particular methodological problems that arise for these theories. In particular, it argues that there are two very different phenomenological categories that intuitions might fall under: sensory-like visualizations and affective states wherein one feels inclined to assent to a proposition. The former are best understood in terms of intellectual perception theories of intuition in which intuitions receive their evidential status in virtue of their phenomenology. The latter are best understood in terms of understanding-based theories that treat intuitions as arising from conceptual competence. §2.3.1.1 argues that most rationalist theories need to treat intuitions as having different evidential status depending upon their phenomenological force: i.e., that some propositions are *more* intuitive than others. It then points out that this means that these theories must supply some objective measures that can be used to determine how intuitive something really is in order to counteract the fact that philosophers will be predisposed to overstating the forcefulness of intuitions that support their theories. §2.3.1.2 argues that understanding-based views of intuition are best supported for the use of intuitions in other fields such as philosophy of

language where the subject-matter is in some sense dependent upon human judgments. Because of this, understanding-based views can only show that intuitions are justified at the cost of superficialism about metaphysics. §2.3.1.3 argues that the intellectual perception views developed thus far can only establish that intuitions have defeasible *prima facie* evidential status. Since intuitions are used in metaphysics to outweigh other very important forms of evidence, intellectual perception views cannot justify the current use of intuitions in metaphysics without providing significant additional arguments.

§2.3.2 describes the hermeneutic strategy for determining how intuitions are used in metaphysics. It argues that rather than looking for explicit appeals to intuition, one must examine the evidence that supports the current theories on offer. §2.3.2.1 applies this method to a preliminary case and shows how apparent appeals to intuition can be deceptive. It is then argued that the best way to tell how a particular dispute turns on intuition is to establish that the only way to make sense of the dispute is if one side treats some claim as having substantial evidential status, and that the only way to explain the evidential status of this claim is through an appeal to intuition.

§2.4 examines four specific disputes within metaphysics and shows how they all have become intractable because one side treats intuitions as evidence while the other does not. §2.4.1 focus specifically upon the dispute between necessitists and contingentists. It is shown that one side of the dispute, the necessitist, has substantial support for her theory in virtue of abductive considerations such as simplicity and expressive power. It is then shown that the only way for the contingentist to combat this is through appeal to intuition. This sets the stage for the following two sections. §2.4.2 describes how intuitions about abstract and general metaphysical principles play a

significant role in these disputes. It examines the dispute between Platonists and Nominalists, presentists and eternalists, as well disputes in mereology. It is argued that in each dispute, one theory possesses significant abductive advantages over the other. It argues that the only way to make sense of the dispute is in virtue of the fact that one of the parties treat certain general principles regarding what exists as evidence that is capable of overriding all of the benefits of the opposing view, and that the only way these general principles can be given such evidential status is through via appeal to intuition.

§2.4.3 describes another way in which appeals to intuition play a major role in metaphysical disputes. It describes how one of the major deciding factors concerning the success metaphysical theories is how well they fit with natural language. It then describes how metaphysical theories that appear to conflict with natural language use various forms of semantic compatibilism to show that this conflict is merely apparent. In particular, three different forms of semantic compatibilism are considered. It is argued that the most promising by far is metasemantic liberalism, which is the view that natural language semantics is not sensitive to issues in metaphysics. §2.4.3.1 argues that the only way in which the anti-compatibilist could argue against metasemantic liberalism is by claiming that they have direct intuitive insight into the structure of natural language. It is then shown that this sort of claim is deeply implausible simply because it heavily conflicts with practice in the philosophy of language. §2.5 summarizes the results of §2.4 and shows how they overwhelmingly demonstrate that the use of intuition in metaphysics has led to schismatic complacency. It concludes that the only reasonable course of action is for metaphysicians to give up appeals to intuition until an appropriate background theory has been established that can serve to evaluate the use of intuitions.

2.1. Bad Models of Intuitions as Evidence

What we are concerned with is how intuitions can be used as a method for establishing theoretical results. In order to understand how this might take place, we need to place intuitions in the context of a more general picture of philosophical evidence. The first problem with this is that philosophy is far too broad a discipline to meaningfully have only one method. Herman Cappelen puts the point rather succinctly:

> The various activities that get classified together as 'philosophy' today are so classified as the result of complex historical and institutional contingencies, not because philosophy has an essence that ties it all together as a natural kind. There are, of course, partially overlapping questions, methods, and interests, but there is no reason to think that a philosopher working on the semantics of quotation is more closely intellectually aligned with someone working on interpreting Plato's *Protagoras* or the ethics of eating meat, than to someone working in computer science or formal semantics. (2011, 21)

Fortunately, we are concerned with metaphysics, so our focus is narrow to begin with. Thus we should at least be able to arrive at some useful caricature of how metaphysics is practiced. Here is one such rough caricature. First, a philosopher presents an argument for a substantial conclusion. Then those who disagree with the conclusion respond to the argument with counterarguments. Eventually each premise of an argument is examined in great detail, and alternative formulations of the argument are considered and rejected. These arguments given in journal articles come to constitute a collective body of evidence shared by the entire field. Each article (hopefully) contributes an additional piece of evidence to that body. In what follows we shall consider and reject various proposals for how intuitions function as evidence. The simple fact of the matter is that metaphysicians currently treat certain claims as having special evidential status because they are intuitive.

2.1.1. Denialism

Prima facie, there appear to be enough examples of appeals to intuition in philosophical literature to warrant a concern with the role these intuitions play in philosophical methodology. As Herman Cappelen puts it:

> The claim that *contemporary analytic philosophes rely extensively on intuitions as evidence* is almost universally accepted in current metaphilosophical debates and it figures prominently in our self-conception as analytic philosophers. No matter what area you happen to work in and what views you happen to hold in those areas, you are likely to think that philosophizing requires constructing cases and making intuitive judgments about those cases. A theory of a topic *X* isn't adequate unless it correctly predicts intuitive responses to *X*-relevant cases. (2012, 1)[114]

Cappelen dubs the idea that intuitions are central to philosophical practice *Centrality*:

> **(Centrality):** "Contemporary analytic philosophers rely on intuitions as evidence (or as a source of evidence) for philosophical theories" (2012, 3).

Cappelen presents two arguments against (Centrality). His first argument is that the term 'intuition' is not used consistently by philosophers in a way that matches any of the various ways in which intuitions could be considered a special sort of evidence.[115] His second argument is that an examination of the 'method of cases' reveals that these cases do not depend upon intuitions.[116] He concludes from his argument that intuitions do not

[114] Cappelen already provides many examples of claims that intuitions are ubiquitous in the meta-philosophcial literature, which I shall not bother to repeat here (2012, 1-7). As an additional example, Saul Kripke gives an explicit endorsement of intuitions in *Naming and Necessity*: "Of course, some philosophers think that something's having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence in favor of anything, myself. I really don't know, in a way, what more conclusive evidence one can have about anything, ultimately speaking." (1980, 42)

[115] He claims that "when we are faced with argumentatively significant occurrences of 'intuitive', it will turn out that the most charitable interpretation of such talk provides no support for Centrality" (2012, 18).

[116] He considers "various philosophical cases or thought experiments from different areas of philosophy" and argues that in none of these cases "is there a reliance on judgments that have any of the features that are supposed to be hallmarks of intuitions" (2012, 18).

play a role in philosophical practice.[117] Since one can only examine the methodological role that intuitions play in metaphysics if they actually have such a role, we should say something in response to Cappelen's argument.

The first thing to be said in response to Cappelen's argument is that we by no means need be committed (Centrality) in order to discuss the role of intuitions in metaphysics. Consider a more restricted version of (Centrality):

> **(M-Centrality):** Contemporary analytic metaphysicians rely on intuitions as evidence (or as a source of evidence) for metaphysical theories.

(M-Centrality) is logically compatible with the denial of (Centrality). Cappelen's arguments are only directed at (Centrality) taken as a global descriptive claim about philosophical practice. Cappelen's arguments are supposed to establish that philosophers *in general* do not extensively appeal to intuitions. Even if his argument goes through, he certainly has not thereby proven that *no* philosophical disputes are grounded on appeals to intuition.

Cappelen's arguments fail to establish anything about (M-Centrality). His arguments turn on broad comparisons about how intuitions are used across multiple philosophical sub-disciplines. Recall from our earlier discussion that the primary problem with Cappelen's account is that he does not considers specific cases from pure modal metaphysics.[118] Another problem is that Cappelen's arguments turn on the idea that intuitions only play a role in the 'method of cases'. As will be become obvious later,

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[117] He claims that "it is not true that philosophers rely extensively (or even a little bit) on intuitions as evidence" (2012, 1 [my underlining]). He concludes that once one rejects (Centrality) one will "have no reason to be a pessimist, an enthusiast or even concerned" with the roles intuitions play in philosophy (2012, 18).

[118] The distribution of the cases he considers runs as follows: 2 philosophy of language (§8.1, §8.6), 2 philosophy of mind (§8.2, §8.8), 2 ethics (§8.3, §8.4), 3 epistemology (§8.5), and 1 personal identity (2012, 130-187).

metaphysicians appear to rely heavily upon appeals to intuitions about abstract principles in order to adjudicate major metaphysical disputes. These intuitions are flatfooted and general: they simply cannot be read as neatly fitting into the 'method of cases'. Cappelen simply has not provided the sort of evidence that would actually count against (M-Centrality). It is not even clear that Cappelen would *want* to object to (M-Centrality), as he appears to welcome examples of intuitions in specific areas of research.[119]

In fact, as a descriptive claim (M-Centrality) is largely unimportant for our purposes.[120] We are concerned with specific sorts of appeals to intuition that are methodologically inappropriate due to their content and the evidence that contradicts them. Whether or not it is true that analytic metaphysics relies heavily upon intuitions outside of these cases is irrelevant so long as these intuitions are not defeated by the sort of problematic conditions under consideration. Hence while Cappelen may be correct in rejecting (Centrality), nothing he says will have an impact upon the specific appeals to intuition that I am concerned with.

In §2.4 we shall show that the only way to make sense of specific seemingly intractable metaphysical disputes (e.g. disputes over the existence of abstract objects and the 'Special Composition Question') is by understanding different sides of the disputes as appealing to different intuitions, as there is general agreement about all other forms of evidence. Cappelen's various conservative strategies for paraphrasing 'intuition-talk'

[119] "If someone grants me that all the passages from Centrality proponents that I quoted in the preceding chapters are false and then goes on to make very specific and carefully investigated claims about the status of particular claims made in a particular argument, we would have made progress" (2012, 195).

[120] The defeasibilist account of intuition I present later implies a trivial form of (M-Centrality), as it implies that intuitions are ubiquitous. But, as Cappelen would put it, my construal of intuition is not 'Centrality-friendly' insomuch as it implies that intuitions do not have the sort of evidential status that one can lean on heavily. Specifically, it does not permit the use of intuition in the cases that I consider. Indeed, the entire point of my account is that it avoids both epistemological skepticism and methodological dogmatism.

simply will not work in these disputes. Cappelen argues that appeals to intuitions should be understood as dialectical devices that serve to either hedge a claim or point out that it is commonly accepted 'pre-theoretic' notion (2012, 61-83). If this is true, then the disputes I consider should have ended some time ago. For instance, if we grant that the Eleatic principle is simply a pre-theoretic commitment then why do we continue to bicker about the existence of abstract objects? The fact that this principle runs into conflict with what many consider our best semantic theory should be enough to set it aside, by Cappelen's lights. Yet Platonists and nominalists continue to bicker on and on in a manner that couldn't possibly be justified by adherence to mere pre-theoretic commitments. Indeed, §2.4 itself constitutes a knock-down objection to Cappelen's arguments.

2.1.2. Innocent Until Proven Guilty

Before we continue on to describe how intuitions are used in metaphysics, we should take time to consider a preliminary objection. This objection is essentially a 'partner in the guilt' argument. It runs as follows: intuitions are a critical part of philosophical practice, so any criticism of intuitions in metaphysics will also affect nearby fields such as epistemology, philosophy of language, etc. Karen Bennett echoes the sentiment:

> it might be the case that there is something deeply wrong with most of philosophy—perhaps because it relies so heavily on *a priori* reasoning. The status of the *a priori* is a pressing issue that I will not address in this paper. All I am saying now is that there is little reason to think that there is some characteristic problem that afflicts *all and only metaphysics*. (2009, 43)

My primary response to this objection is foreshadowed by Bennett in the next few sentences:

> Any problem that afflicts *all* of metaphysics surely afflicts neighboring fields, such as epistemology, logic, and philosophy of language, as well. And any problem that afflicts *only* metaphysics may well only afflict certain particular debates. Thus, not all of metaphysics has to stand or fall together. It is epistemically possible that some issues that metaphysicians talk about are well formed and substantive, and others are not. (2009, 43)

Nothing this thesis says about intuition implies a problem for *all and only metaphysics*. It will not affect all of metaphysics because we are only concerned with the role that appeals to intuition play in specific metaphysical disputes, and because metaphysics can get by with other methods.[121] This project should not be conceived as a global attack upon metaphysics *qua* metaphysics, or even an attack upon intuition *qua* intuition. Certain disputes should perhaps be dismissed as confused (e.g., Nominalism vs. Platonism), but these disputes have proven to be intractable anyway. This thesis is best conceived as (1) an attempt to improve the methods of metaphysics through criticism, and (2) an attempt to elucidate how, when and why intuitions should have evidential status in philosophical theories.

However, the initial 'partner in the guilt' objection can be bolstered with what I shall call the 'Argument from Univocal 'Intuition'-Talk':[122]

(AUIT-1) Philosophers use the term 'intuition' and its cognates univocally throughout different subfields to characterize evidence.

[121] Nor is it intended to affect *only* metaphysics. Perhaps the problematic uses of intuition we shall criticize also play a destructive role in other fields. However, as we shall see in §2.3.1.2, there are special problems for intuitions in metaphysics.

[122] This is a modification of what Cappelen calls the "Argument from 'Intuition'-Talk" (2012, 25). Note that I am ignoring the other argument that Cappelen considers, i.e., 'The Argument from Philosophical Practice' (2012, 95). This is because Cappelen's original arguments were descriptive, whereas the objection we are now considering is normative in character. (AUIT 1) adds a semantic claim to the original argument that Cappelen considers, and this is what enables it to have normative implications. In contrast, there is no straightforward way to reconstruct a similar normative claim by appealing to philosophical practice. Even if it were a *descriptive* fact that philosophers appear to use intuitions in the same way across different subfields, it does not follow from this that philosophers *should* use intuitions in this way.

(AUIT-2) If philosophers use the term 'intuition' and its cognates univocally throughout different subfields to characterize evidence, then the term 'intuition' and its cognates denote a single important epistemological/methodological kind.

(AUIT-3) If 'intuition' and its cognates denote a single important epistemological/ methodological kind then all intuitions have the same default evidential status.

(AUIT-4) If all intuitions have the same default evidential status then they provide the same type of evidence for all philosophical theories, regardless of subject-matter.

∴ Intuitions provide the same type of evidence for all philosophical theories, regardless of subject-matter.

If (AUIT) is sound it would mean that it is methodologically inappropriate to criticize only particular appeals to intuition made in metaphysics.

There are several ways to criticize (AUIT), corresponding to the different parts of the argument. (AUIT-3) seems fairly plausible. Thus the weak-points of the argument are (AUIT-1), (AUIT-2) and (AUIT-4). (AUIT-1) obviously depends upon sociological claims (i.e., that philosophers use the term 'intuition' in a certain way) that can be challenged empirically. (AUIT-2) depends on assumptions in theoretical semantics. (AUIT-4) makes unreasonable assumptions about the nature of evidence. We shall challenge (AUIT-1), (AUIT-2), and (AUIT-4) in this order.

2.1.2.1. Reject (AUIT-1)

There isn't much to say in response to (AUIT-1) that hasn't already been put forth by Cappelen in the first half of his book (2012, 25-93). What is important to note for our purposes is that in order to support (AUIT-2), the use of the term 'intuition' and its cognates must not only be univocal, but must be univocally used in such a way that they will take a single important kind as their referent. On this account, Cappelen provides

fairly definitive evidence that (AUIT-1) is a highly implausible sociological claim. Philosophers just don't tend to use the term 'intuitive' consistently in a substantial way.

Even the philosophers who object to Cappelen's arguments against (AIT) do not provide any arguments that the terms in question are used to pick out a single important kind. For instance, David Chalmers' response runs as follows:

> Regarding 'intuitively': Cappelen argues that there is no plausible reading of this sentence operator on which it serves to mark what philosophers take it to mark. In response, I think it is at least a hypothesis worth considering that philosophers use 'Intuitively' to mark a claim that they take to have a broadly noninferential dialectical justification. Roughly, it is way of marking that they take the claim to be justified in a way that does not require argument or other forms of broadly inferential support. (Forthcoming, 4-5)

If 'intuition' is univocally used to pick out "non-inferential dialectically justified"[123] claims, then there is as little reason to think that 'intuition' picks out a unique kind as there is to think that 'argument' does.[124]

2.1.2.2. Reject (AUIT-2)

In response to (AUIT-2), consider the following claim regarding the semantics of 'intuition' in philosophy:

> **(ISP):** Because the term 'intuition' primarily serves a dialectical function it is contextually sensitive. It picks out the relevant psychological state that serves as evidence for a philosophical claim when there is one.

[123] Chalmers puts forth a distinction "between epistemic justification, or what supports a subject's belief, and dialectical justification, or how a subject supports a claim to someone else" and goes on to claim that "intuitive claims are taken to have a dialectical justification that is broadly noninferential" in that they do "not depend on an inferential, perceptual, memorial, introspective, or testimonial dialectical justification" (Forthcoming, 3-4).

[124] Similar remarks apply to John Bengson's response to Cappelen. Bengson explicitly admits that his treatment of philosophers' use of the term 'intuitive' and its cognates is "fully compatible" with the claim that "there is a family of distinct states or events (e.g., sui generis seemings, attractions to assent, presentations, inclinations to believe, and noninferential assentings—or, perhaps, some subset of these), and that all of the members of this family are legitimately regarded as intuitions, which are denoted by one or more discriminative uses of 'intuition'-terminology" (Forthcoming, 10-11).

(ISP) is supported by many of Cappelen's arguments.[125] There is good reason to think that the dialectical function of the term 'intuitive' in philosophy serves as the reference fixing mechanism for the term 'intuition' in meta-philosophical contexts.[126] If this is the case, then we should expect the term 'intuition' in its particular use to pick out the relevant psychological state that serves as a basis for philosophers' judgments in these dialectical contexts. Once we grant (ISP), it opens up the possibility of the following pluralist claim:

> **(IMP):** Many philosophical sub-disciplines utilize distinct types of intuition that have varying degrees of evidential status.

If (ISP) is plausible then even if we grant (AUIT-1), it will still be possible for something like (IMP) to be true. Thus (ISP) undermines (AUIT-2). In fewer acronyms: the term 'intuition' does not seem to be primarily used to pick out a natural psychological or epistemic kind, so it is at least plausible that many fields might use different types of intuitions, some of which may be better forms of evidence than others.

For (AUIT) to have dialectical force it must depend entirely upon the semantics of the term 'intuition'. The problem is that semantics can only get one so far. One cannot simply say that 'intuition' *looks* like a natural kind term, therefore intuitions are natural

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[125] Cappelen provides explicit arguments that the term 'intuitive' is context sensitive based upon its origin and use (2012, 25-60). However, Cappelen also argues that this indicates that the term 'intuitive' indicate that it is defective, and takes this to be a strike against (Centrality). As Bengson notes, Cappelen's argument seems to turn on a false dilemma, which claims that in philosophical contexts 'intuition' must either take on its ordinary meaning or else a highly specialized meaning if it is to avoid being defective (forthcoming, 2-5). What is important for our purpose is that even if Bengson is correct about the 'discriminative' meaning of 'intuition', it still does not establish that 'intuition' picks out a distinct epistemic kind (see footnote above).

[126] In addition to the argument made by Chalmers above, we can make the following remarks. In first-order philosophical contexts we primarily use the term 'intuitive' as a predicate that attaches to *propositions* (e.g., "it is intuitive that *p*", "*p* is intuitive", or "intuitively, *p*" etc.). In contrast, terms that are used to pick out distinct psychological kinds (e.g., 'anger', 'belief' etc.) are used as predicates for individuals. The term 'intuition' primarily occurs in higher-order meta-philosophical contexts which parasitize first-order use of the term 'intuitive'. Thus the term 'intuition' as it occurs in philosophy seems to differ in a substantial way from other psychological kind terms.

kinds: the term 'intuition' can only pick out what is already there. There is no *prima facie* reason to think that all intuitions have the same evidential status. If we suppose that intuitions must be 'hooked up' to their subject-matter, this suggests that metaphysics might utilize a different type of intuition than epistemology or the philosophy of language.[127] In order to argue against pluralism, one will actually have to engage in in substantial disputes about the nature of intuition (e.g., phenomenological, psychological, epistemological, methodological, etc.). Since one has to do this in order to defend (AUIT-2), the entire argument loses its force.

If this makes it appear as if we are arguing against a straw-man, it is because (AUIT) *is a bad argument*. (AUIT) is supposed to be a way of cutting off arguments about the epistemology or methodology of intuitions in metaphysics *tout court*, prior to any particular considerations regarding the nature of intuitions. By no means have I attempted to establish pluralism. What (ISP) and (IMP) demonstrate is the obvious fact that one actually has to examine the nature of intuitions in order to determine their evidential status. The idea that one can dismiss any arguments about the evidential status of intuitions particular to a certain field regardless of the content of those arguments is simply absurd. One must already be committed to a substantial theory regarding the nature of intuition in order to establish the 'partner in the guilt' argument.

[127] The subject-matter of metaphysics appears to be quite different from the subject-matter in other fields: metaphysics seems to deal with universal, abstract and necessary truths, whereas other fields do not. For instance, the question 'Does every material thing have a proper part?' seems to concern the basic structure of reality. In contrast, the questions 'Is knowledge justified true belief?' and 'Do proper names refer directly to people through causal-historical links or is their reference determined via some definite description commonly associated with the name?' appear to rely upon contingent sociological phenomena. Of course, the exception to this is the account presented by Bealer, who appears to take intuitions about reference to be both necessary and of the same kind as mereological intuitions (1996a). However, Bealer's claims depend upon what seems to simply be an *ad hoc* distinction between semantically 'stable' and semantically 'instable' words (1996a, 134-138).

2.1.2.3. Reject (AUIT-4)

The preceding considerations should be enough to defeat (AUIT). Nevertheless, it is important to see why (AUIT-4) fails, as it will be a feature of objections later on in §2.3.1. The important point is that all intuitions can have the same default defeasible *prima facie* evidential status, while having additional evidential strength that varies according subject-matter. In other words, the antecedent of (AUIT-4) can be affirmed while denying the consequent. The fact that all intuitions can have the same default justificatory status does not imply that all intuitions provide the same type of evidence for all philosophical theories *tout court*, regardless of subject-matter.[128]

It is clear that (AUIT) fails. That being said, I suspect (AUIT) or something like it will still have some residual force for many philosophers. The underlying sentiment is that it is somehow 'cheating' to isolate the use of intuitions in metaphysics. There is little one can say to combat such inchoate sentiments except to point out that they do not constitute arguments. Of course, many philosophers defend accounts in which all philosophical intuitions are accounted for in the same way.[129] The problem is that the

[128] Here I can only summarize how such an argument would go. The important point is that the default *prima facie* evidential status of intuitions can justify only certain premises when they are not contradicted by other forms of evidence. The fact that intuitions are used in certain areas as a central form of evidence does not imply that all intuitions possess such strong evidential status by default. The reason why certain philosophical subfields treat intuitions as having stronger evidential status than this is because they possess *additional* reasons to think that the particular intuitions in question are an important and reliable form of evidence. It makes sense for intuitions to count as an important source of evidence in certain areas like epistemology and philosophy of language because we have reasons to think that our judgments are constitutively related to their subject-matter in these areas. What is important for our current project is that we do not possess these additional reasons in support of intuitions as they are used in metaphysics. There is no way to claim that the subject-matter of metaphysics is constitutively related to our judgments in the same way as the subject-matter of epistemology or philosophy of language without embracing some form of superficialism.

[129] This seems to be one of the driving concerns in the work of rationalists. For instance, George Bealer spends a significant portion of his work delineating philosophical intuitions according to their necessary modal content in order to distinguish them from intuitive judgments made in the sciences (1996a,b,c, 1998, 2000). Elijah Chudnoff also seems pressed by the worry, as he devotes an entire chapter of his

guilt argument will then rest upon whether this theory intuition is better than the competing theory that we shall provide.

2.1.3. Intuitions and Dialectical Justification

Following what was said in Part I, we should draw a distinction between intuitively justified beliefs and methodologically justified appeals to intuition. The question is: what makes appeals to intuition methodologically justified and what kind of evidence does appealing to intuition give us? David Chalmers takes appeals to intuition to be methodologically justified in virtue of the fact that they provide a sort of 'dialectical justification':

> What matters for the use of intuitions in philosophy is their dialectical justificatory status. Here we can distinguish between epistemic justification, or what supports a subject's belief, and dialectical justification, or how a subject supports a claim to someone else. Dialectical justification is sometimes explicit, taking the form of assertions by an agent, and is sometimes implicit, as when the speaker takes the intended justification (a perceptual justification, say) to be clear to other parties. Sometimes an assertion is made without any dialectical justification other than the mere fact of the assertion; we can regard the dialectical justification offered here as testimonial. What is distinctive about appeals to intuition is that intuitive claims are taken to have a dialectical justification that is broadly noninferential. That is, they are taken to be dialectically justified in a way that does not depend on an inferential, perceptual, memorial, introspective, or testimonial dialectical justification. (Forthcoming, 3-4)

According to Chalmers, an appeal to intuition is simply an assertion with a sort of dialectical legitimacy grounded in how the disputants are epistemologically justified in believing the content of the assertion. The underlying idea is that appeals to intuition are methodologically justified insofar as philosophers need to make certain assumptions in order to make progress. Intuitions are what justify these assumptions in virtue of the fact

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book to showing how his phenomenalist account of intuition can apply to a broad range of intuitions in both mathematics and metaphysics (2013c, 58-80).

that they justify the disputants' beliefs in these assumptions. Chalmers' position comes very close to a form of Individualism. However, he attempts to ground the methodological justification of appeals to intuition in dialogue. Since dialogue is essentially a collective endeavor, it is not obvious that his position implies a form of Individualism of the kind we considered in Part I. Thus it will be necessary to examine his position independently.

The first problem with Chalmers's notion of dialectical justification concerns the relation of common ground claims to justified beliefs. Chalmers explains how the supposed non-inferential dialectical justification attained through appeals to intuition is distinct from mere 'common ground' claims as follows:

> Propositions in the common ground typically have a broadly inferential dialectical justification: it is just that this justification is in the background, stemming from how the proposition entered the common ground in the first place. Often the justification will be a testimonial or perceptual justification, deriving from previous communications or from external sources. As before, these dialectical justifications need not be explicitly articulated by the parties to a conversation; they merely need to be mutually recognized. By contrast, with intuitions as I am characterizing them, there need be no broadly inferential justification that the parties recognize; there will only be a broadly noninferential justification, perhaps associated with the obviousness of the claim in question. (Forthcoming, 4)

What Chalmers misses is that sometimes everyone accepts a common premise simply out of habit, without ever bothering to reflect upon whether it is justified. The common ground can include presuppositions that are held out of blind prejudice without any sort of inferential justification. Here Chalmers faces a dilemma: either these unreflective presuppositions also count as being non-inferentially dialectically justified, or his account of dialectical justification must be more explicitly defined in epistemic terms so that it can rule out such presuppositions. The first horn would render non-inferential dialectical

justification worthless. The second horn would once again require a sophisticated epistemological account of how intuition in order to distinguish blind prejudices from claims that are non-inferentially dialectically justified. This, however, would collapse disputes over dialectical justification into epistemological disputes about the nature of intuition, which is what Chalmers was attempting to avoid in the first place.[130] In either case, Chalmers' notion of non-inferential dialectical justification needs work.

Chalmers' account also ignores the fact that philosophical disputes are carried out with specific goals in mind. Dialectical justification is always relative to a particular dispute. Dialogues are always too limited in scope to establish anything substantial on their own, as they never allow one enough time to examine every part of an argument in conversation.[131] Thus dialectical justification has to do with what is acceptable according to the standards of a particular dialogue. What is considered dialectically legitimate will be determined by what the disputants aim to accomplish. In other words, a claim will be dialectically legitimate just in case everyone in the dispute is inclined to agree to it, regardless of whether it is objectively reasonable.[132] Again, this shows that Chalmers'

[130] "As long as philosophers rely on claims with a broadly noninferential dialectical justification, I think a version of the widespread view that philosophers rely on intuitions can reasonably be said to be vindicated" (forthcoming, 4). Chalmers is here responding to Cappelen's argument that philosophers do not rely upon intuitions. Chalmers' response involves avoiding specific commitments regarding the psychology of intuition.

[131] This is why winning a debate about one's theory is not taken to establish that one's theory is successful. Debates are often won not on the merits of the theory, but through the cleverness of the disputants. This is illustrated beautifully by Plato's dialogues, wherein Socrates' interlocutors are drawn into a conversation about a rather abstract topic and leave before the conversation is finished. The reader is usually left with the impression that Socrates has won the debate, but despite this apparent victory we usually do not take Plato to have established philosophical conclusions with any certainty.

[132] If Chalmers takes the second horn of the dilemma above and defines 'dialectical justification' in terms of the epistemic qualities of the disputants, he will simply be changing the subject. This is more than a mere terminological issue. For the purposes of philosophical methodology it is important that we be able to tell the justificatory status of a claim in part by looking at whether it is acceptable according to the standards of the philosophical community. But if one defines dialectical justification in terms of epistemic justification one will no longer be able to tell whether a claim is dialectically appropriate

notion of dialectical justification must still rely heavily on background assumptions about what is objectively justified if it is to do any work.

What may be a reasonable assumption for one dispute may be unreasonable in the context of another. It is perfectly legitimate for philosophers who are concerned with specialized problems to take as premises claims that are considered controversial and unconfirmed conclusions in the context of ignore higher-order disputes. Applied ethicists might assume consequentialist moral principles that would be rejected by a large portion of deontologists at the level of normative ethics. Meanwhile, deontologists, virtue ethicists and consequentialists all tend to presuppose some form of moral realism, which is, of course, still highly controversial position at the level of metaethics. One sees the same thing in metaphysics and epistemology.

One might be tempted to argue that a similar point applies to the sciences. However, this would be misleading. It is true that the sciences that study higher-level phenomena tend to operate somewhat independently of those that study lower-level phenomena.[133] Apparent conflicts arise because the higher-level sciences tend to use crude models of lower-level phenomena. However, these models function as abstractions of the descriptions provided by the lower-level sciences, not competing theses.[134] What is important for the higher-level scientist is that the crude models can be used to generate

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without analyzing the psychological states of the disputants. We have already seen in Part I that this sort of Individualism is unreasonable.

[133] I am here assuming that the sciences are naturally described as falling into loose hierarchies according to relations of dependence between the phenomena they study (e.g., economics > psychology > cognitive science > biology > chemistry > physics). The notion of dependence here is weak: this is *not* a claim about reduction.

[134] For instance, biochemists might use a crude electron-orbit model of molecular bonds in their explanations, fully realizing that this overlooks the true nature of these bonds as described by physics. While biologists and economists might use inaccurate models of atoms and psychological motivation, both explicitly acknowledge that these models are crude and explicitly justify their use by the fact that more accurate models would involve a burdensome level of complexity for their purposes.

the right predictions regarding the phenomena they study, not whether they are literally true descriptions of the lower-level phenomena.[135] Thus despite appearances, scientists who study higher-level phenomena explicitly defer to their colleagues. Occasionally scientists that study higher-level phenomena overstep their intellectual bounds and mistakenly declare that some aspect of lower-level phenomena will not have an effect at the higher-level. However, even in these cases the former still do not intend to contradict their counterparts regarding lower-level phenomena.[136]

The problem with construing intuitions in terms of dialectical justification is that dialectical justification shifts whenever one ascends to a higher-level dispute. Without some further non-dialectical account of what gives intuitions their justification, Chalmers' account would imply that intuitions lose their evidential status as soon as one bothers to question them. It is clear that someone who attempts to justify a claim in metaphysics or normative ethics by appealing to an intuition is obviously at odds with someone who claims that their position makes false assumptions at a higher theoretical level, regardless of the dialectical context. A normative ethicist who assumes moral realism in one of her arguments *is* explicitly contradicting a metaethicist who argues for

[135] Sometimes these models are used incorrectly due to ignorance, in which case the lower-level sciences must intervene. Crude abstractions inevitably sacrifice accuracy for generality and usefulness. They can easily lead one astray if one attempts to use them in conditions that require fine-grained predictions, or in situations that deviate from the presupposed ideal conditions. However, the existence of these occasional straightforward mistakes hardly constitutes a genuine intellectual conflict.

[136] One illustrative example is the claim that quantum mechanical features of physical phenomena become negligible at a certain size scale and thus quantum mechanics is irrelevant to biology. This idea has been recently debunked by biophysicists. For instance, it has been shown that chloroplasts utilize quantum mechanical features in order to maximize the amount of energy absorbed from light. Even in this case, the vast majority of biologists still did not intend to contradict the claims of physicists or dispute the nature of quantum mechanics. No reasonable biologist would have claimed that it fell to *biology* to determine the size-scale at which quantum mechanics can be safely ignored. Rather, they had (falsely) assumed that the quantum mechanical phenomena "died down" and became irrelevant at the size scale with which they were concerned. This was due to ignorance of physics; they were likely working off of what they had heard physicists say about the subject. It would be simply devastating for a biological theory if it actually contradicted the claims of a well-confirmed physical theory.

nihilism or subjectivism.[137] Likewise, a metaphysician who argues that a specific kind of entity exists or that a specific kind of phenomenon cannot be reduced to another *is* explicitly contradicting a meta-metaphysician who claims that metaphysical disputes are meaningless. Thus intuitions shape philosophical research in ways that cannot be reduced to what researchers are justified in saying in specific dialogues.

Academic research cannot be reduced to particular debates any more than it can be reduced to the epistemic states of individual researchers. Research articles are more than just casual dialogues between disputants. Philosophical research involves rigorous argumentation across multiple publications and through an extended period of time. In contrast, dialogues are invariably constrained by time, both in terms of what can be said by the disputants and in terms of the level of ingenuity and attention each disputant is capable of bringing to the discussion. If intuitions are to be methodologically justified, they must be justified in virtue of how they support theoretical claims overall, regardless of specific dialectical context.

2.1.4. Intuitions and Evidence Neutrality

Recall our brief discussion of Timothy Williamson's view of intuition from §1.2.1.3. Part of Williamson's argument is that appeals to intuition are misguided because they inappropriately psychologize our evidence. Williamson argues that appeals to intuition arise from the idea of *Evidence Neutrality*, which is the demand that all our evidence must be neutral so that we can use it to decide between competing theories. Williamson describes Evidence Neutrality as follows:

[137] For instance when an applied ethicist claims bases her argument at least partly on the claim that it would be permissible to detach oneself from the violinist, she is assuming that this is a fact. It is doubtful that she would be willing to simply relinquish this evidence as soon as the dialectical context shifts. Indeed, this would be troublesome for her position. I am here drawing on our previous example using Thomson's Violinist case in §1.3.1.1.4.

> As far as possible, we want evidence to play the role of neutral arbiter between rival theories. Although the complete elimination of accidental mistakes and confusions is virtually impossible, we might hope that whether a proposition constitutes evidence is *in principle* uncontentiously decidable, in the sense that a community of inquirers can always in principle achieve common knowledge as to whether any given proposition counts as evidence for the inquiry. Call that idea *Evidence Neutrality*. Thus a debate over a hypothesis **h**, proponents and opponents of **h** should be able to agree whether some claim **p** constitutes evidence without first having to settle their differences over **h** itself." (Williamson 2007, 210)

The motivation for Evidence Neutrality is that if our evidence is not theory neutral, then it would be question-begging to use it as confirming or disconfirming a given theory. Philosophers claim that their evidence consists of their psychological states in an attempt to satisfy Evidence Neutrality: the claim that someone finds *p* intuitive is obviously less contentious than the claim *p* itself, when *p* is one of the claims at issue. Williamson rejects Evidence Neutrality. Rather than treating our evidence as the claim 'it is intuitive that *p*', he argues instead that we should simply treat the claim *p* itself as evidence. Insofar as Evidence Neutrality bears structural similarities to our principle (MC), we might worry that Williamson's arguments against Evidence Neutrality will also weigh against (MC) and thereby undermine our methodological claims. Thus before we go on to consider how appeals to intuitions are used as evidence we will need to deal with Williamson's arguments against Evidence Neutrality.

2.1.4.1. The Dialectical Role of Evidence Neutrality in Williamson's Argument

Before we even consider Williamson's specific objections to Evidence Neutrality, we need to consider the dialectical role that these arguments are supposed to play. Williamson uses the notion of Evidence Neutrality in his broader argument against "judgment skepticism" (2004; 2007, Ch. 7). Williamson's ultimate aim in this argument is moderately conservative: he wishes to defend the standard armchair practices in which

philosophers consider cases against challenges posed by hardline empiricists and experimental philosophers. Williamson's argument is ultimately that these challengers bear a heavy burden of proof. He argues that those who would challenge philosophers' judgments about cases must provide substantial evidence that these judgments are mistaken, rather than appealing to the mere possibility that this is the case.[138]

A large part of his argument turns on his claim that the psychological and phenomenological evidence do not support the existence of any special class of mental states distinct from judgments that could count as intuitions (2004, 109-116; 2007, 215-220). If empiricists and experimental philosophers *could* identify a single distinct type of mental state and show that philosophers exclusively rely on this state when considering cases, they would then have a relatively narrow target that they could criticize without employing self-undermining arguments. However, Williamson argues that since there is no special class of mental states those who are skeptical about intuitions must really be skeptical about judgments made in specific scenarios.[139] Since a general form of skepticism about judgment would be self-undermining, those who challenge philosophical judgments must provide positive evidence that only these specific judgments are faulty in order for their arguments to have any methodological weight.

Neither Evidence Neutrality nor the denial of Evidence Neutrality has an immediate bearing on whether appeals to intuition are methodologically justified.[140]

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[138] This is fairly uncontestable given Williamson's arguments (2007, Ch. 6 and 7). Also see the end of §1.2.1.4.

[139] For instance, after concluding that there are no reasonable candidates that could serve as intuitions, Williamson goes on to consider the possibility that "skepticism about intuition consists not in skepticism about a special kind of judgment but in a special kind of skepticism about any judgment" (2007, 220).

[140] First of all, since psychological facts themselves can be contested there is no guarantee that even intuitions will pass Evidence Neutrality. As Williamson himself notes, "Not even psychological facts

Rather, Williamson's rejection of Evidence Neutrality is supposed to bolster his argument that there is no such special class of judgments. Williamson argues that our use of the term "intuition" arises when we attempt to find neutral evidence by appealing to psychological states. Since merely appealing to the fact that one believes or judges that *p* does not sound terribly convincing, we instead use the term 'intuition' because it has the connotation of being more reliable. This strengthens Williamson's claim insofar as it provides an explanation for why we use the term 'intuition' that does not depend upon the term referring to a well-defined category of mental states.[141] This places the burden of proof regarding whether there is such a category of mental states more firmly on the shoulders of those who would challenge philosophers' judgments. However, Williamson already has arguments for this conclusion that depend upon phenomenological and psychological evidence, and these challengers bear this burden of proof anyway, given that they must describe what intuitions are in order to make their objections clear. Moreover, if Williamson is correct in claiming that the tacit assumption of Evidence Neutrality explains our use of the term 'intuition', this psychological fact will undercut the evidence that our use of the term provides for the claim that intuitions constitute a special category of mental states *regardless of whether Evidence Neutrality itself is true*. Thus it is unclear whether the objections to Evidence Neutrality must succeed in order for Williamson's more general argument against judgment skepticism to work.

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really meet the demands of Evidence Neutrality. Whatever Descartes thought, facts about one's own present consciousness are not always cognitively accessible to one." (2007, 236) Second, denying Evidence Neutrality entails *less* restrictive standards about what can count as evidence. Such a denial does not restrict us from treating intuitions as evidence when taken on its own.

[141] This can be seen as an undercutting defeater for the sort of 'argument from intuition talk' that Cappelen considers (2012, 25-93). In fact, Cappelen himself argues that Williamson's treatment of intuitions as judgments is not conducive to the claim that philosophy depends heavily on intuitions (2012, 83-7).

But our arguments regarding the methodological status of appeals to intuition do *not* depend on anything like judgment skepticism. Our ultimate objection is that the way metaphysicians appeal to intuition does not pass (MC), which has little to do with whether these intuitions or judgments themselves are justified. This means that Williamson's overarching arguments against judgment skepticism simply do not make contact with our primary criticism concerning the role of intuition in metaphysics. We can even grant the dialectical goal that Williamson's rejection of Evidence Neutrality is supposed to accomplish without it affecting our argument. Even if Williamson is correct and intuitions are simply judgments, this hardly affects our analysis in §2.4. All this would mean is that instead of relying on appeals to a special cognitive faculty, metaphysicians have been relying upon inconsistent appeals to judgments. If anything, this only strengthens our criticism. Thus the only thing that concerns us is whether Williamson's specific arguments against Evidence Neutrality succeed in undermining (MC).

2.1.4.2. The Rejection of Evidence Neutrality Assumes Collectivism

Fortunately, Williamson's arguments against Evidence Neutrality do not undermine (MC). First, note that (MC) is a constitutive rule governing research *qua* official funded group activity. It does not rely upon any dialectical or epistemic norms, but rather issues from pragmatic norms regarding the purpose of research. Thus Williamson's arguments will only obviously affect (MC) if Evidence Neutrality itself is treated as a pragmatic norm. However, in his rejection of Evidence Neutrality Williamson appears to be considering it as a *dialectical* principle regarding what should count as evidence when two people are engaged in a dispute. For instance, Williamson states that

"Evidence Neutrality is false" because possessing "good evidence for a belief does not require being able to persuade all comers, however strange their views, that you have such good evidence" (2007, 212). However, as we saw in the previous section, dialectical evidence is not equivalent to official theoretical evidence.

In fact, Williamson's arguments against Evidence Neutrality ultimately rest on the distinction between dialectical norms regarding what counts as evidence in a conversation and non-dialectical norms governing what counts as evidence *simpliciter*. Williamson's primary argument against Evidence Neutrality is that the objective (non-dialectical) evidence used in inquiry cannot be constrained by the skeptic's challenges because this would make it impossible for inquiry would be unable to get off the ground. Williamson puts the point as follows:

> Some skepticism, like skepticism about reason, is so radical that it leaves too little unchallenged for what remains as shared evidence to be an appropriate basis for evaluating the claims under challenge. When one is warranted in refusing to play the skeptic's dialectical game, the dialectical standard of evidence becomes irrelevant. In refusing, one does not abandon one's claim to knowledge and reason, for the appropriate standard of evidence is non-dialectical. By that standard, the skeptic's preemptory challenge fails to disqualify the challenged fact as evidence. To neglect such evidence would be to violate the requirement of total evidence. (2007, 239 [my underlining])

After making this point about when it is appropriate to rebuff the skeptics' dialectical challenge, Williamson proceeds to apply this to the case of judgment skepticism:

> Is this attitude a legitimate response to judgment skepticism? For instance, may one take the fact that the subject in the a Gettier case lacks knowledge or the fact that there are mountains in Switzerland as evidence, even though the judgment skeptic challenges one's right to such evidence? In reaching one's views, one does not restrict oneself to premises and forms of inference acceptable to judgment skeptics, for one regards their restricted evidence base as too willfully impoverished to constitute a reasonable starting-point for inquiry. Such skeptics have not shown that the facts they allow as evidence are really more certain than the facts that

> they disallow. In particular, it is quite insufficient for them to point out that it is possible to judge that there are mountains in Switzerland even if there are no mountains in Switzerland, for a parallel objection can be made to any evidence worth having in the sciences. (2007, 239 [my underlining]) [142]

Williamson's aim is to show that the burden of proof falls on those who would challenge philosophers' judgments about cases to show that these judgments are in fact faulty. His argument rests on the claim that there is some objective (non-dialectical) notion of evidence that supersedes the notion of dialectical evidence used by the skeptic. The skeptic must show that the claims in question do not count as evidence according to the *objective* standard, but the possible scenarios that the skeptic appeals to do not accomplish this. Williamson's language in these passages appears to be Individualist. However, Williamson has given us no reason to think that the relevant objective evidence needs to consist of what individuals know. In fact, Williamson's argument has far greater force if one assumes Collectivism in regard to the evidence under consideration.[143] There is at least some plausibility in the claim that the skeptic's challenge can undermine the knowledge of individuals (otherwise skepticism would not have such a long philosophical history). However, when it comes to the collective body of evidence that a

[142] Williamson makes an identical point when discussing skepticism in *Knowledge and its Limits*: "One sceptical strategy is to exploit the dialectical effects of challenging propositions. If one is never entitled to rely on something under challenge, one will very soon be left with very little. … The sceptic relies uncritically on rules of dialectical engagement which evolved to serve more practical purposes, without questioning their appropriateness to the radical questions which scepticism raises. If challenging something thereby makes it dialectically unusable, then the power of challenge might hinder rather than help the pursuit of truth if it is not used with restraint." (2000a, 188 [my underlining])

[143] Indeed, Williamson himself acknowledges elsewhere the need for a distinction between individual evidence and communal evidence in regard to theoretical inquiry: "The communal case is needed: science depends on public evidence, which is neither the union nor the intersection of the evidence of each scientist. We can ascribe such knowledge by saying that *p* is known in community S, or that we know *p*, which is not equivalent to saying that some, many, most, or all of us know *p*" (2000, 185). In the statements "*p* is known in community S" and "we know *p*" the predicate 'know' is clearly intended to function collectively rather than distributively. Given this, it is more reasonable to read Williamson's use of Individualist language in the above passages as shorthand for claims about when it is reasonable for the *community* to acknowledge challenges to their collective evidence.

field of researchers takes as confirming or disconfirming their theories, the skeptical challenge is simply absurd. Researchers are not concerned with building a secure worldview from epistemically airtight foundations. Rather, they receive funding in order to provide a description of a certain set of phenomena. Only certain questions fall within the scope of their inquiry.[144] Far from showing that Collectivism is false, Williamson's argument against Evidence Neutrality works better if Collectivism is *assumed*.

Williamson's appeal to an objective notion of evidence functions as a double-edged sword. As we saw in Part I, it is the constitutive rules laid down by the field that determine what counts as part of this evidence. These rules are determined in part by the practical interests that drive research. Williamson's point is not that experimental results or other forms of evidence are incapable of overriding our judgments, but rather that it is unreasonable to demand that we stop using one of our primary research methods until the results of this method can be independently confirmed. We need positive evidence that these methods are unreliable before we abandon them.[145] What allows Williamson's argument to go through is the fact that researchers have a practical obligation to get on with their inquiry in spite of the fact that they might not employ perfect techniques. Too much caution would simply paralyze philosophical inquiry. Even if skepticism itself were a live epistemological option, this would not imply that we should suspend all other forms of inquiry until this question is settled. Again, this is because theoretical research is

[144] Not only would be totally unreasonable to demand that physicists prove that we are not being deceived by an evil demon, it would be downright *negligent* for physicists to waste their time and energy on this question, regardless of the fact that it can potentially undermine the truth of their theories.

[145] Thus Williamson argues that even though "Experimental results can in principle undermine the procedures of any intellectual community," it does not follow from this general fact "that every intellectual community should suspend its procedures until the relevant experiments have actually been done and shown to have reassuring results, otherwise all inquiry would come to a halt, since the procedures for interpreting experimental results would themselves have been suspended" (2009b, 471-2).See the overview of the dialogue between Williamson and Weinberg in §1.2.1.4.

driven by pragmatic concerns. But these same pragmatic concerns are precisely what drive (MC). While these practical considerations allow us to rule out unreasonable skeptical challenges, they also rule out unreasonable forms of pluralism regarding evidence. Evidence Neutrality is false in some circumstances because it would allow skeptical challenges to keep a field from engaging in inquiry. But it would be equally destructive for these fields to allow individual researchers to take whatever they want as evidence. It is not only doubt that can paralyze inquiry, but disagreement as well. The same reasons that serve to undermine Evidence Neutrality also serve to buttress (MC).

2.1.4.3. Intuitions as Instruments

I take the preceding points to establish that Williamson's arguments against Evidence Neutrality do not constitute a serious threat to our overall conclusion. In essence, this is because our argument simply does not make contact with Williamson's concerns. Williamson's arguments are only directed at defeating a very general form of skepticism, while our criticism is based on a series of specific methodological concerns. Nevertheless, in his rejection of Evidence Neutrality Williamson does make several claims regarding how intuitions inappropriately psychologize evidence that are worth considering. Williamson uses the example of Gettier cases to diagnose why he thinks Evidence Neutrality leads us into a mistake:

> Arguing from the Gettier proposition that the subject in a Gettier case lacks knowledge, I conclude that knowledge is not equivalent to justified true belief. Now I meet someone who thinks the Gettier proposition a mere cultural prejudice, not itself evidence. In this context, it is not in principle uncontentiously decidable that the Gettier proposition is evidence. Thus the only way to satisfy Evidence Neutrality is by ruling that the Gettier proposition does not constitute evidence. To argue that knowledge is not equivalent to justified true belief, I must go back a step to less contentious premises. What can they be? My opponent allows that I *believe* the Gettier proposition, and may even admit to feeling an

> inclination to believe it too (I am not merely idiosyncratic), while overriding it on theoretical grounds. Thus Evidence Neutrality tempts one to retreat into identifying evidence with uncontentious propositions about psychological states, that I believe the Gettier proposition and that both of us are inclined to believe it. How much that helps is questionable. For now I face the challenge of arguing from a psychological premise, that I believe or we are inclined to believe the Gettier proposition, to an epistemological conclusion, the Gettier proposition itself. That gap is not easily bridged. (2007, 211)

If Williamson is correct, we should not bother appealing to intuitions, but should simply take the propositional contents of our judgments to be our evidence. Thus instead of taking the fact that it is intuitive that the person in the Gettier case lacks knowledge as our evidence for the claim that knowledge is justified true belief, we should instead take the simple fact that the person in the Gettier scenario doesn't know as our evidence.

If we were to follow Williamson's advice, much of our evidence would simply be inexplicable. As we noted in §1.1.2, intuitions are generally non-inferential. The only way to evaluate the evidence provided by non-inferential judgments is by examining the conditions that enable these judgments. Thus we can evaluate the evidence provided by a perceptual judgment by evaluating the conditions that enabled the perceptual experience upon which that judgment was based. Were the lighting conditions good? How far away was the object? Has the person in question ingested hallucinogens? All of these questions are perfectly legitimate. Analogously, the challenger objects that our judgment that the person in the Gettier scenario does not know is "a mere cultural prejudice". This objection is deliberately aimed at undermining a theoretical claim (i.e., that knowledge is more than justified true belief) by undercutting the evidence that is cited in favor of this claim (i.e., by arguing that the judgment that the person in the Gettier case does not know is due to cultural prejudice). This objection focuses on the psychological faculties

underwriting our judgments precisely because this is the only way to question the evidence.[146] It is reasonable to take the Gettier proposition itself as evidence, but this does not thereby preclude the fact that we judge the Gettier proposition to be true from also being part of our evidence. Indeed, we *need* to keep this latter claim as part of our evidence. If we ever need to question the Gettier proposition, we need to know why we it counts as evidence in the first place, and the only thing that can explain this is our judgments that the Gettier proposition is true.[147]

Williamson is quite right to point out that evidence must be propositional.[148] As Williamson points out, the notion of evidence is used in inference to the best explanation and probabilistic reasoning, which take propositions as their objects.[149] In addition, we should note that whether a proposition counts as evidence is a relative matter. To consider a proposition as evidence is to consider it as evidence *for* some further claim. One must distinguish evidence from *raw data*. Our perceptual evidence may be

[146] While Gettier himself did provide arguments for these conclusions, it turns out that these arguments themselves were not very good. As Brian Weatherson puts it, while Gettier gave an argument for his conclusion "it isn't a compelling argument, since it takes as a premise that we can't get knowledge from a false belief, and that isn't quite right" (Forthcoming, 11). See also Warfield 2005 and Fitelson 2010. Indeed, the Gettier proposition appears to have its status as evidence regardless of any particular argument. This point is made by both Brian Weatherson (Forthcoming) and David Chalmers (Forthcoming) in response to Cappelen.

[147] This would also mean that we are unable to question claims such as *p = there could have been nothing identical to this table*, since *p* appears to have the same sort of non-inferential justification as the Gettier proposition. But if this were true then Williamson's necessitism (2013a) could never even get off of the ground. Williamson appears to treat the evidential status of *p* in virtue of its initial intuitiveness as being so weak that it is simply overridden by formal considerations. It is hard to reconcile this with the way Williamson treats the Gettier proposition.

[148] "One's evidence is propositional if and only if it is a set of propositions. Propositions are the objects of propositional attitudes, such as knowledge and belief; they can be true or false; they can be expressed relative to contexts by 'that' clauses. For present purposes, we do not need a developed theory of propositions. If evidence is propositional, we can refer to evidence by using 'that' clauses: my evidence for the conclusion that the house way empty is *that* it was silent, *that* no lights were on in the evening, *that* the telephone went unanswered" (Williamson 2000a, 194).

[149] Williamson 2000a, 195-7. Part of his reason for this is that evidence must be the kind of thing hypotheses explain, and "the kind of thing which hypotheses explain is propositional" (2000a, 195).

propositional, but part of what *makes* it evidence is the fact that it is grounded in the raw data of sensory experience that is causally related to the world.[150] The fact that intuitions are non-inferential means that they must be evaluated in the same general way as perceptual evidence. In this case, the raw data consists of the phenomenal properties of the intuitive experience. It is this experience that must ground the intuitive judgment in order to make it function as evidence. Another example of data vs. evidence would be the readout from an instrument vs. the proposition expressing what the readout says. Raw data has to be interpreted in order to function as evidence. The problem is that there are often multiple ways of interpreting a set of data. In the sciences one compensates for this with experimental controls, which isolate the phenomenon at issue from extraneous causal influences which might produce the same data. Thus the phenomenological qualities of our judgments and the psychological processes that underwrite them are just as relevant to whether the content of these judgments count as evidence as the facts about perceptual experiences and instruments are to whether our observations count as evidence.

In order to make his point, Williamson goes on to draw an analogy with how evidence is used in the sciences:

> Evidence Neutrality has no more force in philosophy than in other intellectual disciplines: philosophers are lucky if they achieve as much certainty as the natural sciences, without quixotic aspirations for more. If Evidence Neutrality psychologizes evidence in philosophy, it psychologizes it in the natural sciences too. But it is fanciful to regard evidence in the natural sciences as consisting of psychological facts rather than, for example, facts about the results of experiments and measurements. When scientists state their evidence in their publications, they state mainly non-psychological facts (unless they are psychologists);

[150] Williamson implicitly acknowledges this distinction in his treatment of perceptual experience, which he argues can only be used as evidence in virtue of the proposition that things appear to be a certain way (2000a, 198-200).

> are they not best placed to know what their evidence is? The psychologization of evidence by Evidence Neutrality should be resisted in the natural sciences; it should be resisted in philosophy too. (2007, 212)

However, this analogy is seriously misleading. Evidence Neutrality dictates only that one must accommodate challengers by shifting to less controversial evidence that is recognized by both parties. Nothing about this implies that the new evidence must be psychological.[151] The sciences *do* embrace a fairly standard principle of Evidence Neutrality. The skeptical challenge in these cases does not usually concern the psychological faculties of the scientists, but rather *the interpretation of empirical data*. The following scenario should be familiar. A group of scientists G_1 run an experiment that produces a set of data *d*. G_1 analyze *d* statistically and give it a causal interpretation such that it confirms hypothesis *h*, which in turn supports theory *t*. However, another group of scientists G_2 come along and question this result. For instance, they might claim that some specific aspect of the experimental conditions makes G_1's particular use of instruments unreliable, or they might find some fault in G_1's statistical analysis of *d*, or they might simply claim that G_1's data can also be interpreted as supporting a competing hypothesis. So long as G_2's claims are not too far-fetched, the scientific community will generally recognize that G_1's results are insufficient to confirm their hypothesis and that further experiments are needed.

According to the Collectivist, the body of evidence that can be used to either confirm or disconfirm a theory consists in claims that explicitly occur in publications. The benefit of this is obvious: it makes our evidence explicit and workable. The peer-review process acts like a filter for this evidence. Williamson's claim that we should not

[151] That the new evidence in the dispute involving the Gettier case is psychological is due to the specific features of the example (as we just saw).

psychologize our evidence by appealing to intuition is equivalent to the claim that scientists should not "instrumentalize" their evidence. To be sure, it would be much harder for G_2 to question G_1's claims if G_1 did not bother to publish the details about the methods they employed in order to get their results. G_1 could simply state that their hypothesis is confirmed and then call G_2 "experiment skeptics" when they ask them about how they arrived at these results. But this would be transparent intellectual dishonesty.[152] Recording the appeal to intuitions as evidence serves the exact same function in regard to philosophical theorizing that recording details about one's instruments serves in regard to scientific theorizing. Research in the sciences utilize procedures where instruments such as microscopes and particle accelerators collect data that is then analyzed statistically and interpreted such that it provides evidence for or against a hypothesis or theory. Having details about the various stages of this process explicitly recorded in a journal article allows other scientists to go back and evaluate the evidence it provides. While the main upshot of any scientific experiment should be the confirmation of the hypothesis, this does not permit scientists to skimp on the details of how they arrived at this conclusion. The rest of the scientific community needs this information in case they need to go back and evaluate the experiment. By having arguments and appeals to intuitions explicitly recorded as evidence philosophers receive the exact same benefits. By making our appeals to intuition explicit, we can better analyze the evidence they provide us.

[152] As we stated above, Williamson acknowledges that it is legitimate to raise specific concerns about specific judgments. Nevertheless, this analogy *does* undermine Williamson's more specific claim that we should not bother to record our intuitions in the first place.

2.1.5. Mooreanism

The final (bad) theory of how intuitions are used as evidence that we shall consider is Mooreanism. Mooreanism is roughly the position that claims belonging to common sense have a privileged evidential status simply in virtue of their belonging to common sense. As the name suggests, the position originated with (or, at least, was inspired by) G.E. Moore. For instance, Moore responds to various skeptical arguments presented by Bertrand Russell as follows:

> What I want, however, finally to emphasize is this: Russell's view that I do not know for certain that this is a pencil or that you are conscious rests, if I am right, on no less than four distinct assumptions: (1) That I don't know these things immediately; (2) that they don't follow logically from any thing or things that I do know immediately; (3) That, *if* (1) and (2) are true, my belief in or knowledge of them must be 'based on an analogical or inductive argument'; and (4) That what is so based cannot be *certain knowledge*. And what I can't help asking myself is this: Is it, in fact, as certain that all these four assumptions are true, as that I *do* know that this is a pencil and that you are conscious? I cannot help answering: It seems to me *more* certain that I *do* know that this is a pencil and that you are conscious, than that any single one of these four assumptions is true, let alone all four. That is to say, though, as I have said, I agree with Russell that (1), (2), and (3) *are* true; yet of no one even of these three do I feel *as* certain as that I do know for certain that this is a pencil. Nay more: I do not think it is *rational* to be as certain of any one of these four propositions, as of the proposition that I do know that this is a pencil. And how on earth is it to be decided which of the two things it is *rational* to be most certain of? (1973, 46)

Putting aside the specific nature of Russell's premises, the relevant[153] underlying Moorean argument should be understood simply as this: there are certain propositions regarding everyday facts (e.g., that I have hands, etc.) that automatically possess strong evidential weight simply given the fact that they belong to common sense, that any

[153] While Moore's use of rhetorical questions makes a historical interpretation of this passage somewhat difficult, what is important in regard to Mooreanism is the argument suggested by this passage. We are not here concerned with providing a historical analysis, but rather with reconstructing a view that could potentially pose a threat to our conclusion. Thus it is largely irrelevant to our current project whether Moore himself or anyone else actually subscribed to the view I am about to describe.

theoretical philosophical claims possess weaker evidential weight than these commonsense claims, and thus whenever philosophical claims and common sense claims conflict, the latter will override the former.

There are many problems with Mooreanism, which many authors have already discussed at length. We cannot hope to add much to this discussion here. However, given the context of our current discussion we possess two advantages. First, we are concerned with methodology not epistemology. As the passage above obviously shows, traditional Mooreanism is an epistemological position: the notion of 'evidence' in the argument just formulated concerns an *individual's* evidence. In other words, the Moorean is making a claim about what is rational for an individual to believe given the fact that she possesses two conflicting sources of putative evidence. In order for the Moorean to defend the metaphysical judgments we shall consider in §2.4, she will have to reformulate her position in terms of what it is rational for *a community of researchers* to take as evidence. But the link between the kind of evidence that individuals possess for their beliefs and the kind of communal evidence that is used in academic research is far from obvious. Indeed, we have already painstakingly argued in Part I that the latter cannot be reduced to the former. Thus even if we granted that Mooreanism were a reasonable epistemological position, it would hardly follow that it is a reasonable methodological position. We are not arguing for general skepticism about common sense. Rather, we are arguing that a specific academic field should not use certain claims as evidence until it can establish some methodological standards for evaluating these claims. Our second advantage simply concerns the nature of the claims we shall consider. As we shall see, many of the relevant appeals to intuition made in metaphysics concern judgments with contents that are very

technical, abstract, and theoretical. Thus a large part of why we can dismiss Mooreanism is simply the fact that many of the relevant appeals to intuition cannot be reasonably construed as appeals to common sense. In what follows we shall capitalize on these two advantages by arguing that (1) Mooreanism is bad methodology, and (2) Mooreanism cannot handle the relevant intuitions.

2.1.5.1. Mooreanism is Bad Methodology

What is the motivation for Mooreanism? As Theodore Sider puts it, "Why should the inherited prejudices of our forebears count for *anything*?" (2013, 246). Mooreanism is supposedly motivated by the claim that unless we can simply treat common sense as evidence *simpliciter* we won't have any viable evidence for certain claims. Sider presents what we may call the *Motivational Argument Against Mooreanism*, which runs as follows:

> Why are so many metaphysicians Mooreans? Partly because they fear that if we reject common sense, there will not be enough to go by.[25] Both metaphysics and inquiry generally, it is thought, would be paralyzed. … But again a flat-footed answer tempts. The dictates of common sense are often independently reasonable, and when they are, they do not need backing from common sense. Reason can stand on its own. (2013, 247)

This argument might fail against Mooreanism considered as an epistemic position, as the average person may be simply unaware of these independent reasons. But we (including Sider) are concerned with Mooreanism as a *methodological* position. As such, we are concerned with the claim that common sense is the only viable form of evidence that *researchers* have for deciding between certain *theories*. This latter claim is patently implausible. There are many viable forms of evidence in metaphysics aside from common sense (see §2.4). The fact that non-professionals are unaware of this evidence is simply irrelevant. Moreover, it is perfectly reasonable for researchers to allow pragmatic

interests to dictate their theoretical starting points when no further evidence is available (i.e., by choosing starting points that look promising). Indeed, there is no real pressure on researchers to claim that a theory is successful when evidence is lacking. It is better to reserve judgment until new evidence comes along. What is important is that theoretical inquiry makes progress; absolute foundations are unnecessary. Thus Mooreanism seems to be completely unmotivated as a methodological claim.

A parallel objection is that the Mooreans have simply gotten the explanatory order of things backward. It is not the fact that certain propositions belong to common sense that gives them their evidential status. Rather, there are other things that make these propositions independently reasonable, and the fact that these propositions belong to common sense is *explained* by the same things that make them independently reasonable. Common sense is nothing more than a conglomeration of seemingly reasonable assumptions made for pragmatic purposes. The demands of the human condition are what make these assumptions pragmatic, and thus an analysis of the human condition generally reveals the real evidence for these assumptions. For instance, the best evidence for the existence of the external world is that it is the simplest explanation of our experiences.[154] Indeed, no average person attempts to justify her belief in the external world in a Cartesian manner. Rather, people start off believing that there is an external world because this is the simplest way to think about the world.[155] Thus it is the *simplicity* of the

[154] As Sider puts it: "it is reasonable to posit a world of external objects because this posit best explains our sensory experiences.[26] However exactly we cash out the notion of inference to the best explanation, this sort of inference need not be rooted in its commonsensicality." (2013, 247)

[155] This is obviously not to say that everyone arrived at this belief explicitly through an abductive inference. Rather, this is an *etiological* or *functional* explanation of how the belief arises. What we are considering here is a general explanation of what makes common sense beliefs reasonable; not a description of the specific reasoning processes or the warrant individuals have for their beliefs. It is more than likely that most individuals arrive at their belief in the external world completely unreflectively. Any epistemology

theory that our sense data are caused by spatiotemporally extended objects that makes the tacit assumption of this theory a part of common sense. Not only is the inference to the best (i.e., simplest *ceteris paribus*) explanation for the existence of external objects not rooted in common sense; the fact that the belief in the existence of external objects belongs to common sense is *explained* by the fact that it is the best explanation of our experiences. The fact that people have not reflected on these assumptions is to be expected. It is the job of philosophers to reveal what makes these assumptions reasonable.

This leads us to a more specific objection to Mooreanism. Virtually no academic discipline aside from philosophy regularly cites common sense as evidence. There is good reason for this. Why would we bother to support academic disciplines if we assumed that all of the questions they asked must already answered by common sense? Even if we granted that Mooreanism is a good epistemological position (i.e., by granting that individuals are always more justified in believing common sense than other forms of evidence), the simple fact of the matter is that common sense has turned out to be wrong on many occasions. The whole point of academic research is to study phenomena in a more rigorous manner than is possible through informal reflection. Because of this, it makes no sense for these disciplines to take common sense as evidence. Even if we suspect that common sense is correct about subject *X*, the point of academic research would be to verify common sense by investigating *X* through methods independent of common sense. Treating common sense as the deciding piece of evidence simply defeats the entire purpose of research.

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that does not rob the average person of knowledge will need to be capable of accommodating mindless lack of reflection.

Psychology provides an excellent example. Psychologists do not take the fact that a certain psychological claim is common sense to count as evidence for that claim. While they may use common sense in order to form a hypothesis, they must still test this hypothesis using other means. On many occasions, common sense can be wrong. It would simply be absurd to assume that rigorously conducted psychological research is somehow incapable of overturning common sense without this implying some radical form of skepticism about all of our common sense beliefs. Even if a common sense psychological claim turns out to be correct, it is still the job of psychologists to vindicate this common sense claim through more rigorous methods and more trustworthy forms of evidence. To repeat the general argument of §1.3.1, why should we think that philosophy is any different in this regard?

2.1.5.2. What Counts as Common Sense?

For Mooreanism to work, it must have some way of determining what counts as common sense. Given our current purposes, this leads to a specific problem for Mooreanism: many of the most salient judgments in metaphysics are far too technical to reasonably count as being part of common sense. One principled way to distinguish what counts as common sense is in reference to what is commonly said in everyday conversation.[156] However, this criterion would quite obviously discount many of the judgments that we will consider in §2.4 below. For instance, it is simply false that the following claims occur in everyday conversation: that two things cannot be exactly

[156] One could attempt to extend this to what is immediately implied by what is said in everyday contexts. However, the problem with this is the fact that there are 'easy arguments' for certain ontological claims that rely only upon everyday statements (see Hofweber 2005 and Thomasson 2008, 2009a). This would mean that claims such as 'numbers exist' or 'propositions exist' belong to common sense. This seems highly implausible insofar as it is unlikely that the average person thinks about the ontology of numbers and propositions.

located at the same place at the same time, that one object cannot be located in two or more places at the same time, or that the whole is nothing over and above its parts, that only the present moment exists, that time doesn't move, etc. Moreover, this criterion would also fail to make a ruling on claims that should presumably defy common sense, simply because these claims are not generally considered in everyday conversation. For instance, while the claim that there are mountains might count as common sense it is less than clear that the claim that there are no trout-turkeys will meet this criterion, simply because people do not consider the question in the first place. One cannot claim that the existence of trout-turkeys violates common sense simply because trout-turkeys are never mentioned in everyday conversation. There are many important entities that are not so mentioned (e.g., gluons, muons, etc.). Presumably these entities do not violate common sense because they are theoretical posits. But in order to establish this one must already know how to draw the distinction between theoretical claims and claims of common sense, which is the very thing in question.

Thus if Mooreanism is to have any chance of explaining the use of intuitions in metaphysics, it will have to appeal to a broader counterfactual or dispositional notion of common sense. However, this analysis will run into significant problems. One of the problems is that it is far more difficult to tell whether something meets a counterfactual notion of common sense than a notion that simply depends upon what is commonly said. One way to do this is to simply survey what the average person thinks. Indeed, Mooreanism virtually demands that the relevant claims be confirmed through empirical work. One simply cannot argue that a claim has privileged evidential status in virtue of its belonging common sense while simultaneously refusing to acknowledge empirical data

about what the average person believes. Thus Mooreanism cannot offer any resistance to the experimental philosophy movement. Indeed, it is especially susceptible to experimental results. Positions that appeal to a faculty of intuition or to expert judgments have some means of resisting experimental results. However, if the majority of people do not assent to a claim that the Moorean claims belongs to common sense, this will simply wipe out the evidential status of this claim for the Moorean.

Another problem for Mooreanism is that it is doubtful that the average person will be able to make the relevant judgment without being given an appropriate setup. For instance, if the average person is asked whether the whole is nothing over and above its parts or whether only the present moment exists, they are likely to respond with a certain amount of perplexity. Some of these intuitive claims can only be understood if one understands their theoretical background. But one cannot say that a claim is common sense just in case the average person would believe it after being taught the relevant background theory. Moreover, there are certain judgments (i.e., about co-location) that the average person may assent to simply because they haven't considered counterexamples. Mooreans have no way of dealing with either of these problems.

Mooreanism is distinct from indirect arguments that attempt to show that a certain proposition is evidence by arguing for two claims: (1) the proposition in question belongs to common sense, and (2) the deliverances of common sense in this case should be given evidential weight. What is important for this sort of argument not whether the claim in question actually belongs to common sense, but whether the second claim is successful. Indeed, since (2) will invariably depend at least in part on claims concerning the nature of common sense, the fact that (1) appeals to common sense in particular will be irrelevant.

This sort of indirect argument is really just a specific formulation of a more general argument that consists of the following claims: (1*) the proposition in question belongs to class *X*, and (2*) propositions that belong to class *X* in this case should be given evidential weight. The argument in question could be mistaken in identifying *X* as the class of common sense propositions without it really affecting the conclusion of the argument, so long as (1*) and (2*) are both true.

What these arguments need to establish is the methodological claim that the particular judgments that we shall consider in §2.4 must be given evidential weight in spite of the fact that giving them such weight violates (MC). The specificity of our claims renders such indirect arguments extremely weak. The most obvious version of this indirect argument simply claims that the relevant judgments belong to common sense because they are intuitive and should have evidential status because intuitions have evidential status. But this obviously ceases to be a form of Mooreanism.[157]

[157] Sider considers several alternatives to the Mooreanism under consideration here and presents several arguments against each one that are, in my mind, fairly successful (2013, 245-6). His concern is with potential objections to mereological nihilism, but his points do not rely on any particular features of this position and thus generalize to other metaphysical theories. In addition, here are two potential arguments that might proceed in this manner by presenting different versions of claim (2): (2′) deliverances of common sense are reliable due to natural selection. (2″) our other theoretical judgments rely upon the deliverances of common sense and thus any attempt to override the latter in virtue of the former will fail. The first argument is simply implausible: it is doubtful that natural selection can even operate on our beliefs since this would imply that beliefs are inherited. Even if beliefs could be inherited, natural selection is far too sloppy to produce the sort of reliability required. The second argument fails for several reasons. First, many supposedly common-sense views that *have* been overridden by theoretical discoveries (e.g., that the earth is flat, etc.). Second, it may be that it would be destructive to give up all of our common sense beliefs at once, but what is primarily at issue in theoretical disputes is only a subset of these beliefs. Third, it is fairly clear that our common sense beliefs can change over time, in large part *because* of new theoretical discoveries.

2.2. Worrying About Metaphysics

Some of the lack of consensus in philosophy is no doubt due to the difficult and abstract subject-matter of philosophy. Philosophy is a difficult discipline. Moreover, metaphysics is perhaps the most difficult philosophical sub-discipline. Indeed, many have worried about whether we can ever achieve knowledge of metaphysical truths, whether individually or collectively. However, this is not an excuse for methodological leniency. On the contrary: it means that we must be disciplined if we ever hope to achieve real progress. The problem is that philosophers have a tendency to hold onto their positions, come hell or high water. This tendency is not limited to cases of intellectual dishonesty; it is simply a part of human nature. Without collective agreement on the standards for evaluating the evidential weight of intuitions it becomes possible for philosophers to give the intuitions that support their theories so much weight that they can effectively ignore all competing forms of evidence.[158]

The danger of echo-chambers arises especially when there are sociological pressures for everyone within a subfield to adopt a certain position. One major factor in this regard is selection-effect. It seems a fairly reasonable assumption that people will only enter an academic field if they find the subject-matter engaging. This suggests that most researchers will likely think that their field engages in at least some substantial or worthwhile disputes. It is hard to imagine that there are many people who actively engage in academic research who also think that research is essentially nonsense or unimportant.

[158] We cannot simply rely upon informal heuristics about what 'seems reasonable', as intuitions appear to determine what counts as reasonable in the first place. Virtually everyone thinks her position is reasonable and her adversaries' unreasonable precisely because she finds her theory so much more intuitive than that of her adversaries. At the end of the day it is likely this overall impression that determines what positions most metaphysicians hold, regardless of whether they currently have the arguments to back up their position. This is permissible on the level of individual belief (arguments come and go), but to use such impressions to evaluate research methods constitutes a form of negligence. We must be explicit with our methodological standards.

In general, there seems to be no need for concern about echo-chambers in philosophy due to selection effects. However, there do seem to be a few obvious exceptions. For instance, in a recent survey it was found that the highest correlation of philosophical position with academic specialization was between theism and philosophy of religion (Bourget & Chalmers Forthcoming, 18-19). It is not exactly a leap to suppose that this has at least something to do with selection-effects. Most of the philosophers who criticize religion belong to subfields such as philosophy of science.[159] The difference between the methods employed by people like Alvin Plantinga and people like Daniel Dennett is telling. Any conclusions drawn from these distinctions will, of course, be somewhat speculative. But let us be blunt for a moment. It is extremely doubtful that philosophers like Plantinga and Dennett are *ever* going to reach a consensus about the existence of the Judeo-Christian deity. The dispute between the parties is intractable because they simply cannot agree about what counts as evidence. Similar worries arise for metaphysical positions in other philosophical subfields.[160]

[159] Only 20.87% of philosophers of religion identify themselves as atheists, which stands in stark contrast to the dominance (86.78% self-identifying) of atheism within of the broader philosophical community (Bourget & Chalmers Forthcoming, 19). The philosophy of religion *qua* academic subfield typically employs *a priori* arguments to determine the nature and existence of the Judeo-Christian deity. In contrast, the atheist philosophers who criticize religion usually do so from the standpoint of psychology or evolutionary theory. Since these philosophers approach religion from a scientific standpoint, they usually belong to academic specializations such as philosophy of science, philosophy of psychology, philosophy of biology or cognitive science (e.g., Daniel Dennett). Since they too are saying something about the nature of religion what they're doing should be reasonably considered philosophy of religion. Thus it is somewhat questionable whether the survey has truly captured all of the relevant specialist opinions.

[160] For instance, one would also expect selection-effects against subjectivism in aesthetics. Sure enough, only 26.12% of aestheticians are subjectivists about aesthetic value, as compared to the 46.36% among the general philosophical population (Bourget & Chalmers forthcoming, 19). Note that there was apparently not as major correlation between moral realism and normative ethics. However, this may have something to do with the fact that 50.07% of normative ethicists responded favorably towards deontology, which on certain popular neo-Kantian formulations does not require moral properties but rather grounds morality in terms of rationality.

One worry is that there is a selection effect amongst metaphysicians to think that their discipline raises substantial questions. David Chalmers states the worry as follows:

> Outside the field of ontology, deflationary views are widespread, with many non-ontologists being skeptical of the heavyweight realism that has become common in the field. It is natural to suppose that there is some sort of selection effect at work here: those who think that ontological questions are deep questions with determinate answers are more likely to go into ontology than those who think that the questions are shallow or lack objective answers. (2009, 78)

While there is something to be said for this worry, selection effects can only have real influence on metaphysics if metaphysical methodology is undisciplined. Thus we should be more concerned about the methods that metaphysicians employ than such broad sociological concerns.

Metaphysics is most threatened by dogmatism when metaphysicians become overly defensive. The problem is that metaphysicians constantly see their field under assault not only by scientists and laymen, but by other members of the philosophical community. These assaults often come in the form of total dismissal regarding the very practice of metaphysics.[161] One can expect any human being who is constantly told that her life's work is a waste of time to eventually become rather defensive when it comes to methodology (if not downright bitter). It is only natural that after enough of these skirmishes metaphysicians would come to treat any methodological challenge as though it were an existential threat to be halted at all costs. The problem is that this attitude is liable to make metaphysicians blind to real methodological issues. As we saw in the first chapter, this is precisely what has happened with intuitions: the meta-philosophical landscape is torn between indiscriminate skepticism and unreflective dogmatism.

[161] Aside from the classic logical positivist arguments, more recent prominent examples include Ladyman & Ross 2007, and Hirsch 2011. Peter Unger's forthcoming *Empty Ideas: A Critique of Analytic Philosophy* promises to be one more in a long line of dismissive attacks.

The aim of this section is to provide evidence that the use of intuitions in metaphysics has led to schismatic complacency (see §1.4.1.1). The problem is two-fold. First, there is significant disagreement about intuitions should count as evidence. Second, there is significant disagreement about how much evidential weight intuitions should be given. In order to establish this we shall examine several major metaphysical disputes and show that each suffers from intractable disagreement that can only be due to disagreement about the evidential status of intuitions. The upshot is this: until we establish some agreed standards for evaluating intuitions we should stop appealing to them and rely on other forms of evidence instead.

2.2.1. Metametaphysics

There is a proud philosophical tradition of wringing one's hands about metaphysics. The sort of hand-wringing that is currently in vogue can be traced most directly to the influence of the logical empiricists at the turn of the 20th century. What is rather peculiar about this tradition is that the worry is not whether we can have knowledge of metaphysical truths, but whether there even *are* such things as metaphysical truths. More specifically, the worry is whether purported metaphysical statements are even capable of being determinately true. This, however, is not what we are concerned with here. Our current aim is not to destroy metaphysics. We shall not attempt to show that metaphysical claims are meaningless. Nor shall we attempt to argue that metaphysics truths cannot be known. On the contrary, our criticism of the use of intuition in metaphysics is intended to be constructive. Our aim is to make metaphysics more disciplined. However, we cannot simply ignore the traditional problems that face metaphysics either.

As we shall see, the nature of the subject matter of metaphysics makes appeals to intuition especially hard to justify. It is not unreasonable to think that our intuitions concerning how language works or who knows what are decent forms of evidence because language and knowledge are things we are exposed to every day. In contrast, metaphysics is traditionally taken to concern the most abstract and fundamental features of reality, such as the nature of possibility, space, time, causality, and the structure of physical objects to name just a few. The nature of these things is hardly revealed through every day experience. Indeed, these facts are supposed to be even more fundamental than the truths of particle physics. Why should we expect our intuitions to be any more useful in regard to these things than they are in revealing facts about particle physics? Since one cannot evaluate a research method in abstraction from the subject-matter that it is supposed to illuminate, we will need to briefly discuss metametaphysics.

In what follows my terminological choices will be pragmatic rather than descriptive: they are meant to capture helpful distinctions rather than the "true" meanings of the expressions given how they are used in the literature. A number of technical terms have been introduced by various authors to describe the theoretical space surrounding metametaphysics. Sometimes these terms are used as adjectives that describe the general attitude of either philosophers or their positions (e.g., 'realist', 'substantivist', 'anti-realist', 'deflationist', 'superficialist', 'fictionalist', 'semanticist', 'dismissivist' etc.), while in other instances they are used to form a name for a specific theoretical position (e.g., 'ontological realism', 'metaphysical realism', 'meta-ontological realism', etc.). Unfortunately, the use of these terms has been so promiscuous and variant that it is better for our purposes to simply start from scratch.

To illustrate how extreme this phenomenon is, consider the terminology of five authors from the same anthology:

(1) Theodore Sider uses the term 'deflationist' to describe philosophers who take ontological disputes to be confused, and then goes on to describe the specific theoretical positions that can be used to justify this attitude in terms of what they take the source of confusion to be (e.g., he labels the positions "**Equivocation**", "**Indeterminacy**", "**Obviousness**" and "**Skepticism**") (2009, 385-6). Sider also uses the phrase 'ontological realism' to denote what is in essence a *metasemantic position* (i.e., the notion is defined in terms of metasemantic notions such as 'naturalness' or 'structure', not in terms of truth or meaningfulness) (2009, 407-409).

(2) In contrast, David Chalmers defines 'ontological realism' (and 'ontological anti-realism') as a *semantic* thesis: "Ontological realism, at least in its strongest variety, holds that every unproblematic ontological existence assertion has an objective and determinate truth-value" (2009, 92).

(3) John Hawthorne uses the term 'superficialist' to describe a specific semantic position wherein the philosophers who are engaged in ontological disputes "are talking past one another, on account of having attached different meanings to the key terms of the debate" (2009, 213). While he describes superficialism as presupposing a "deflationary attitude", he does not go on to identify any other positions that might also express this attitude (ibid). This is undoubtedly because his article is completely devoted to considering objections to a single author's position (i.e., that of Eli Hirsch), so there is no need for him to describe other deflationary positions.

(4) Eli Hirsch describes his position as "roughly Carnapian", but goes on to say that "whereas Carnap's formulation sometimes seems to suggest an anti-realist or verificationist perspective, my position is robustly realist" (2009, 231).

(5) Karen Bennett deliberately stipulates that her term 'dismissivism' is intended to function as a "generic label for the view that there is *something* deeply wrong with these [ontological] debates" (2009, 39). Bennett then goes on to elaborate three versions of dismissivism, which she calls '*anti-realism*', '*semanticism*' and '*epistemicism*' (2009, 39-42). Bennett laudably clarifies her reasoning behind the use of 'dismissivism'. She states that "Neither 'skepticism' nor 'deflationism' are appropriate as a *generic* label" for the views she goes on to consider because "'Skepticism' carries epistemic connotations that are not appropriate for the first two views [i.e., anti-realism and semanticism], and 'deflationism' does not comfortably fit the third [i.e., epistemicism]" (2009, 39 [note 3]).

Given the general methodological nature of our discussion, this is a good opportunity to get a bit clearer on the relevant notions than is possible in normal metaphysical research. Ontology is the discipline wherein one aims at compiling a list of the general kinds of things that exist (e.g., numbers, material objects, events, etc.). Metaphysics is the discipline wherein one aims at creating a coherent picture of the world: it is the project of seeing how things "hang together". On most reasonable accounts, metaphysics is broader in its scope than ontology insofar as it is concerned with explaining how the kinds of things that exist relate to one another. These two disciplines could only be considered identical if one held that all metaphysical questions can be reduced to existence questions. However, this would be controversial.[162]

[162] One can put virtually any dispute in terms of an existence claim (assuming that existence claims are captured by quantified expressions). Thus one can describe essentially metaphysical debates about the

Let us dub the realm of inquiry concerned with the nature of metaphysics 'metametaphysics', and the realm of inquiry concerned with the nature of ontological disputes 'metaontology'.[163] Putting aside some thorny etymological issues,[164] there is a good reason to keep these two terms distinct. Insofar as there is good reason to treat metaphysics and ontology as distinct (albeit closely interrelated) disciplines, there is also good reason to treat metametaphysics and metaontology as distinct. It might be tempting to characterize metaontology and metametaphysics as the study ontology and metaphysics. This would describe metaontology and metametaphysics as specific forms of methodology, as we have described the term. However, given what has been written on these subjects, this would be inaccurate. It will be better for our purposes to stick to the notions of metaontology and metametaphysics that best fit how these positions are presented by various authors; i.e., as consisting of semantic, metaphysical and epistemological claims that describe the nature of disputes in metaphysics and ontology.

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nature of objects in terms of higher order quantification over properties and relations. For instance, one could describe the dispute over Humeanism as being concerned with the question "Does there exist some relation *X* such that *X* is primitive and holds necessarily between two distinct things in virtue of their intrinsic properties?" However, this sort of approach is clearly not in the spirit of ontology as it is commonly conceived. Moreover, insofar the sciences are also clearly concerned with existence claims, this attempted reduction is clearly too broad.

[163] Both terms occur frequently in Chalmers, et. al. 2009. Many authors prefer the term 'metaontology'. This term seems to have been originally popularized by Peter van Inwagen in his paper "Meta-ontology" (1998). For instance, almost all of the entries in Chalmers, et. al. 2009 focus on claims about ontology or metaontology: only the Hofweber 2009 and Price 2009 papers bear 'metaphysics' in their titles. However, as we shall see, this is likely due to the dominance of the Quinean mindset wherein metaphysics is simply conceived as ontology.

[164] Both terminological choices perpetuate an anachronistic use of the 'meta' prefix'. As scholars of Aristotle are apt to point out, ""Metaphysics" is not an Aristotelian term" but rather seems to be a "title later editors gave to the treatise" that we now call the *Metaphysics*, and that the best explanation for the name is that the later editors "were at a loss regarding a proper title for the treatise and just named it after its position in the corpus of Aristotelian writings, namely, as coming after the physical writings" (Frede 1987, 81). Indeed, van Inwagen openly acknowledges that "coinages as 'meta-language' and 'metaphilosophy' (which I am imitating) are based on a misconception about the origin of the word 'metaphysics'" (1998, 249 n. 1). He also notes that he spells 'meta-ontology' "with a hyphen to take account of the fact that in Greek the final vowel of the prefix 'meta' would be absorbed by the initial vowel of 'ontologia'; one might therefore maintain that 'metontology' would be the correct form", and goes on to report that "Heidegger actually has coined the word 'Metontologie'" (2009, 475 n. 5).

Our methodological study will consist in part of an analysis of both metaontology and metametaphysics, but it will also extend beyond these views by including sociological evidence that is relevant to the examination of the methods employed in metaphysical research.[165]

2.2.1.1. Ontology

Ontology is the philosophical subfield that is concerned with determining what general types of things exist. Metaontology is the subfield that considers interrelated claims that are lie in the background of ontological disputes.[166] Some common examples of ontological questions include "Do numbers exist?", "Do composite objects exist?", "Do concrete possible worlds exist?" In contrast, some examples of metaontological questions include: (1) questions concerning the semantics of existence claims; (2) questions concerning whether the existence of a certain type of entity is an important question that is worth worrying about, or whether existence itself is uninteresting in comparison to questions about what category things belong to; (3) questions concerning what we should treat as evidence for existence claims. The same sorts of distinctions apply to metaphysics and metametaphysics, *mutatis mutandis*.

At the broadest methodological level, there appears to be a single fundamental division amongst metaontological views. On one side is what one might call *Phenomenological Metaontology*. The two philosophers who most notably fall into this

[165] As we have already shown in our discussion of Individualism, meta-philosophers tend to treat the methodology of philosophy by focusing on the metaphysics, semantics and epistemology of philosophical disputes. This also tends to be the norm for discussions of metaontology and metametaphysics. As we have also shown in §1, while these sorts of questions are independently interesting and relevant to methodological considerations, the methodological study of philosophical research must also consist of more than this.

[166] For instance, Matti Eklund defines metaontology as follows: "Ontology is the study of what there is. *Metaontology* is the study of the nature of questions about what there is; that is, the nature of ontology." (2006, 317)

category are Martin Heidegger and Alexius Meinong.[167] Roughly speaking, Phenomenological Metaontology is distinguished by the fact that it treats existence (or 'being') in a pluralistic manner, and by the fact that it attempts to answer questions concerning the nature(s) of existence through broadly phenomenological methods. One common assumption of this tradition (if it can even be called such) is that being is an activity that can be performed in various different ways. (This claim is most explicitly formulated in the words of Heidegger.[168]) Over the past century Phenomenological Metaontology has fallen more or less completely out of favor in Anglophone academic philosophy. For this reason, as well as simple considerations of space, I will not spend any more time discussing this position.

[167] I have used the term 'phenomenological' rather than 'continental' because (1) it explicitly describes the methods employed by the tradition, (2) the idea that there is a single coherent tradition that can be called 'continental philosophy' is highly questionable, and (3) the term 'continental' is usually also taken to cover postmodernists such as Michael Foucault and Jacques Derrida who—if they can be understood at all—are best understood as simply rejecting the coherence of a project of ontology altogether. Putting aside postmodernists (who I shall ignore completely), it is possible to interpret various other "continental" philosophers as being committed to some form of Phenomenological Metaontology. For instance, G.W.F. Hegel, Jean-Paul Sartre, Edmund Husserl, and Emanuel Levinas all undoubtedly say things that could be interpreted in this manner. I mention Heiddeger and Meinong simply because they appear to be most explicitly committed to such a view. However, my purpose here is not even remotely historical (see following footnote).

[168] See McDaniel (2009) for a historical discussion. Note, however, that the metaontology offered by McDaniel himself should be understood in our categorization as Analytic. In effect, McDaniel's metametaphysical position is identical to the considered metametaphysical position of Theodore Sider (2011). McDaniel explicitly adopts Sider's notion of 'quantificational structure', and only disagrees with Sider insofar as he holds that multiple restricted and non-overlapping versions of the quantifier are all more natural or structural than the unrestricted version of the quantifier (2009, 305-314). Thus McDaniel and Sider are best understood as agreeing about metametaphysics while disagreeing about metaphysics. Of course, this is not to say that McDaniel should be understood simply as an apologist. Rather, McDaniel seems to argue that his position is ultimately very close to—if not simply an exposition of—the position of the historical Heidegger. I have no wish to express disagreement with McDaniel on the historical point. Indeed, it may very well be that there simply is no historical tradition that meets the description I have provided. Since my purpose in introducing the notion of 'Phenomenological Metaontology' is largely dialectical (i.e., to bring out salient features of Quinean Metaontology through contrast), I do not consider this a significant problem. In the worst-case scenario, I entreat the reader to simply ignore what I have said about it and focus solely on the exposition concerning Quinean Metaontology.

Phenomenological Metaontology began to lose force in Anglophone philosophy in the early twentieth century. This decline can be plausibly explained in part by developments in formal logic and the application of these developments to traditional metaphysical problems by people like Gottlob Frege, Ludwig Wittgenstein and Bertrand Russell. Eventually this trend culminated in the development of Logical Positivism by the members of the Vienna Circle, and the associated rejection of metaphysics and ontology as consisting of pseudo-questions. It is at this point—according to the orthodox narrative—that W.V.O. Quine steps in to save metaphysics and ontology from the positivistic threat. As legend would have it, Quine accomplished this by performing several feats. He is taken to have provided resounding arguments against verificationism, the analytic/synthetic distinction, and the notion of 'truth by convention'. In reality, this historical narrative has proven to be rather faulty. This much is generally uncontested: verificationism is universally acknowledged to be self-defeating. However, Quine's arguments have been shown to be unconvincing.[169] And since Quine's arguments against truth by convention have a similar structure, they should be treated with equal suspicion. We shall set these arguments aside.

[169] See Quine's arguments only show that intensional notions (i.e., meaning, analyticity, synonymy, necessity, etc.) form a definitional circle, but this also seems to be the case for extentionsional notions (i.e., existence, reference, truth), which Quine does not reject. Moreover, Quine does not seem to provide any specific reason to reject the intensional notions aside from the complaint that they do not appear to have any behavioral criteria, which is not terribly convincing (c.f. Creath 1991, 2007, George 2000, Stein 1992). The point is nicely summarized by Timothy Williamson: "Quine's arguments [against the analytic/synthetic distinction] are generally found much less compelling than they once appeared. Although he may succeed in showing that "analytic" is caught in a circle with other semantic terms, such as "synonymous," he does not adequately motivate his jump from that point to the conclusion that the terms in the circle all lack scientific respectability, as opposed to the contrary conclusion that they all have it. Given any science, someone may insist that it define its terms, and the terms used to define them, and so on until it is driven round in a circle. By itself, that hardly demonstrates the illegitimacy of the science. Every discipline must use undefined terms somewhere or other. "Two Dogmas of Empiricism" does not explain why we should regard the undefined terms of semantics as worse off than the undefined terms of other disciplines, except by dogmatic charges of unclarity. After all, semantics is now a thriving branch of empirical linguistics. It is not to be trashed without very good reason." (2007, 50)

Quinean Metaontology encompasses a range of positions. The most basic thesis that unites these various positions can be roughly stated as follows:

> **(QMO)** Any notion[170] of existence that is relevant to ontology will be captured by the existential quantifier (the common notation is '∃') of first-order predicate logic with identity, under its standard syntactical and semantical analysis.[171]

(QMO) is properly understood as the common core assumption shared by a family of metaontological views. However, as stated (QMO) is somewhat unclear. (QMO) is best understood as a highly abstract thesis belonging to *pure semantics*.[172] Pure semantics consists of general formal considerations concerning what is expressible in any language. (QMO) describes what it would take for any language to be capable of expressing claims about what exists. For this reason, one could more accurately capture the idea behind (QMO) by making this an explicitly metalinguistic and abstract claim about languages:

> **(QMO_M)** For any language *L*, *L* is suitable for the purposes of making ontological claims only if *L* features at least one syntactical device that is best described as having the same general syntactical and semantic properties as the existential quantifier of classical first-order predicate logic with identity.

Note that (QMO_M) only describes a *necessary* condition for some language to be capable of expressing existence claims: the mere presence of a quantifier does not guarantee that the language uses that quantifier to express the notion of existence.

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[170] Here I am using the term 'notion' in a deliberately inchoate manner because, as we shall soon see, it is problematic to speak of a "concept of existence" within the context of Quinean Metaontology.

[171] I take it that most readers should already be fully aware of the relevant notion. In the interest of total specificity, one version of this language is described in significant detail by Moshé Machover (1996, ch. 8).

[172] I am here drawing roughly on a distinction made by Carnap: "*Descriptive semantics* is the empirical investigation of the semantical features of historically given languages. *Pure semantics* is the analysis of semantical systems, i.e., systems of semantical rules." (1942, 11) The distinction is supposed mirror that between pure and applied mathematics. However, we needn't accept Carnap's stipulation that pure semantics is 'non-factive', as this has no real bearing on the distinction. The important point is that (QMO_M) is supposed to describe a general semantic thesis in abstraction from considerations concerning any particular natural language.

In order to achieve a fully fleshed-out metaontological position, one will need to link (QMO_M) to descriptive claims about natural languages and scientific discourse before one can use these as sources of evidence for one's ontological position. Thus Quinean metaontology must also include claims about how discourse is to be translated or 'regimented' into first-order predicate logic.[173] The process of regimentation reveals the 'ontological commitments' of the discourse in question. The claim is that one's ontology should be determined by one's ontological commitments. The general assumption is that it is intellectually dishonest to endorse some statement as true that ontologically commits one to some sort of entity and at the same time deny that this entity really exists. For instance, when summarizing the historical influence of Quinean metaontology for the introduction to *The Oxford Handbook of Metaphysics*, Michael Loux and Dean Zimmerman describe the notion of ontological commitment as follows:

> Quine's criterion of ontological commitment is understood to be something like this: If one affirms a statement using a name of other singular term, or an initial phrase of 'existential quantification', like 'there are some so-and-sos', then one must either (1) admit that one is committed to the existence of things answering to the singular term or satisfying the description, or (2) provide a 'paraphrase' of the statement that eschews singular terms and quantification over so-and-sos. So interpreted, Quine's criterion can be seen as a logical development of the methods of Russell and Moore, who assumed that one must accept the existence of entities corresponding to the singular terms used in statements one accepts, unless and until one finds systematic methods of paraphrase that eliminate these terms. (2003b, 4)

[173] For the classical presentation, see Quine 1948. For what is perhaps the most meticulous example of regimentation, see van Inwagen 2009, 492-506. However, in his exposition of these theses (especially (4)), van Inwagen commits himself to the view that the existential quantifier must be understood in terms of ordinary English. Though at points Quine himself seemed to endorse such a view, this is a claim about the epistemology of logic that is extraneous to the core Quinean thesis. Indeed, it conflicts with the view of authors such as Cian Dorr (2005) and Theodore Sider (2009, 2011), who claim that ontological disputes should be understood as occurring in a technical theoretical language called 'ontologese'. This process of regimentation can be understood either as translation proper between two distinct languages or as a sort of explicit codification of what is already contained in the deep logical structure of the language in question. Sider and Dorr understand the process of regimentation in the former way, while van Inwagen understands it in the latter way.

Technically, if regimentation reveals that a specific sort of discourse is ontologically committed to something, one has three options: (1) accept that those entities exist, (2) provide a paraphrase, or (3) admit that the relevant statements are false and abandon that way of speaking altogether.

Adopting a system of regimentation alone will still not suffice for a substantial metaontology; one will also need to adopt a position regarding what sort of discourse should be regimented in the first place. The classic Quinean adopts a holistic and naturalistic epistemological position that privileges scientific theorizing. The idea is that scientific discourse is both the best form of evidence we have, and that we should take what scientific theories say literally. Thus the classic Quinean will say that the best way to determine what exists is to regiment our best scientific theories into first-order predicate logic, and then determine what the values of the bound variables must be in order for the theory to come out true.[174] However, this is not the only position currently on the table. Many contemporary ontologists also take everyday language to be an important source of evidence for ontology. Again, Loux and Zimmerman provide a useful historical summary:

> For Quine, it is the deliverances of science alone that should determine our ontological commitments. As Chisholm saw it, this was the decisive point at which he departed from Quine and took inspiration from Moore: Why not assume, in the seminar room, the same things we take ourselves to know in everyday life? Why are we suddenly not entitled to them? Lewis, and the younger generation of metaphysicians who came into their own in the 1980's, by and large side with Chisholm and Moore. Once all our ordinary convictions are taken into account, the traditional problems of metaphysics return with a vengeance, as they do not for Quine. As a result, ontology must be responsive to other areas of philosophy; a particular

[174] For instance, if our scientific theories are committed to the existence of abstract entities, this will override any supposedly *a priori* evidence we might have for nominalism. The question of when discourse is ontologically committing is a matter some dispute. See Azzouni 1998; 2004b; 2007; 2010a,b; 2012a,b; and Colyvan 2005; 2010 for discussion.

> ontological scheme shows its adequacy by its usefulness in the resolution of problems elsewhere. Desiderata for an ontological scheme include both simplicity (a point about which Quine would agree) and scope. One metaphysical system is superior to another in scope in so far as it allows for the statement of satisfactory philosophical theories on more subjects—theories that preserve, in the face of puzzle and apparent contradiction, most of what we take ourselves to know. (2003b, 4-5)

For some putative entity *x*, Quinean ontology inquires as to whether *x* exists. Ontology asks questions like "does space exist", "does time exist", "do numbers exist", etc. Quinean Ontology is thus a procedure in which one lists what exists. According to the Quinean, the answer to the question "does *x* exist" comes in the form of a simple *yes* or *no*. However, when one asks whether *x* exists one has to be clear about what one takes *x* to *be*, or else there is no way to give a meaningful answer.[175] In considering whether *x* exists, one will invariably ask metaphysical questions about *x*.[176] When one considers the metaphysical qualities of *x*, one can easily find that *x* does not play well with the other objects in one's ontology. For example, ordinary objects such as tables and chairs do not fit well into our standard metaphysics of material objects, due to issues such as co-location, causal overdetermination and vagueness.[177] These metaphysical issues have led

[175] Take the question "Does consciousness exist?" Answers to this question will vary wildly to the point of meaninglessness unless one can provide a characterization of consciousness that serves to anchor the dispute. For instance, if one simply defines consciousness as the type of information-processing that occurs in mammalian brains it will follow trivially that mammalian brains are conscious. But this is clearly not the intent of the question.

[176] An analogy with biology is illuminating. Biological taxonomy does not merely provide a list of species; it attempts to explain how these species are related to one another through genetic descent and morphology. In order to generate a taxonomic system in biology one must already have a considerable body of knowledge concerning genetics and morphology. In other words, one must actually study an organism at the molecular, physiological and ecological level in order to even be capable of placing that organism within a taxonomic hierarchy. At the same time to study a particular organism one must already possess the concepts of cells, genes, niches, etc., which generally presuppose a taxonomic understanding of life. For example, the concept of genes was first developed by Gregor Mendel as a way to account for the inheritance of different traits in plants. Moreover, if one has the concept of cells one will also be able to differentiate single-celled and multicellular organisms.

[177] See Korman 2011 for a summary of these issues.

many to hold that ordinary objects can have no place within our ontology.[178] Those who think that ordinary objects must be part of our ontology are thereby pressured to find ways of dealing with these metaphysical issues.[179] Thus in order to arrive at a meaningful answer an ontological question one must already be engaged in metaphysics.

2.2.1.2. Metaphysics

Metaphysics is an attempt to *describe* the world, not just list what is in it. Of course, metaphysics depends at least in part on ontology. Metaphysics asks questions like "What is the *nature* of space and time?", "What is the *nature* of abstract entities?", "What is the *nature* of possibility?", "What is the *nature* of causation?", etc. These metaphysical questions have already presumed that there is such a thing as space, time, abstract entities, possibility, causation, etc.[180] While these metaphysical theses will also need some way to arrive at a putative answer regarding the existence of these entities, this does not imply that the primary metaphysical questions should be reduced to questions about what exists.

For instance, Jonathan Schaffer adopts a neo-Aristotelian metaphysics wherein the primary questions concern *grounding relations* between things.[181] For instance, are propositions grounded in states of affairs, or possible worlds, or are they classes of objects and thus grounded in particular objects? These questions will not obviously be

[178] See Sider 2013; Merricks 2000, 2005, 2007; Unger 1979, 1980; and van Inwagen 1990.

[179] See Baker 2007, 2008; Hirsch 1993, 2002a, 2002b, 2005, 2009; Lowe 1982, 1994, 1995, 2002, 2005a,b; Sider 2003; Thomasson 2006, 2007a, 2010.

[180] For instance, once we grant that time exists we then inquire into its nature. Does it pass or move, as it seems to from our perspective? Or do past, present and future moments all exist? Alternatively, once we grant that numbers exist as abstract entities, we might then ask whether they are *sui generis* primitive entities, or sets in disguise.

[181] `Schaffer states that his view aims to provide "an ordered hierarchy generated from (i) a list of the substances *F*, plus (ii) a list of the grounding relations *G*" (2009, 355). On this view, "categories just are *ways things depend on substances*" (2009, 355).

answered by ontology. Schaffer combines this position with a permissive ontology, and what appears to be a deflationary metaontology. In other words, he is permissive about what objects exist because he thinks existence questions are easily answered.[182] Thus a metametaphysical question would concern how we are to evaluate claims about grounding relations. What procedure should we use to determine whether one particular entity is grounded in another?

On a Quinean metametaphysics, metaphysics is simply identified with ontology: the only thing to do is to list the things. However, this is not the only picture on offer. Jonathan Schaffer rejects the Quinean picture in favor of a neo-Aristotelian conception of metaphysics, wherein the primary question is not which things exist, but how those things are related to one another.[183] Schaffer gives the following three models:

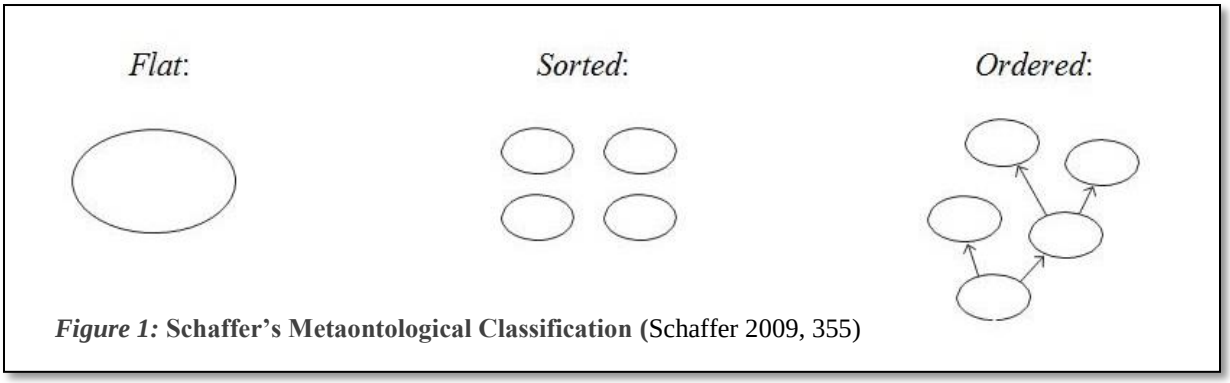

Figure 1: **Schaffer's Metaontological Classification (**Schaffer 2009, 355)

The Quinean notion of metaphysics corresponds to the *Flat Structure*, where "the target of metaphysical inquiry is an unstructured list of existents" (Schaffer 2009, 355).

[182] Schaffer acknowledges that "there are *other* central metaphysical questions which are existence questions", but states that "the neo-Aristotelian need not contest this, since she has room for both grounding and existence questions" (2009, 363). He goes on to say in a footnote that "One might even reserve "ontology" for these metaphysical [existence] questions", but that this would be "a revisionary usage" of the term, which "comes from Aristotle's definition of first philosophy as the study of being *qua* being, and is properly used for an account of the nature of *being*, not for a list of beings" (2009, 363 n. 20). Kris McDaniel (2009) offers one such approach to ontology.

[183] As Schaffer puts it, according to the Quinean view, "metaphysical questions are *existence questions*, such as whether numbers exist" (2009, 347). Schaffer argues that Aristotle is concerned with whether numbers "are transcendent substances, or grounded in concreta", and thus "The question is not *whether* numbers exist, but *how*" (2009, 348). According to the neo-Aristotelian view, the task of metaphysics is to construct "a *hierarchical view of reality* ordered by *priority in nature*" (2009, 351).

Schaffer states that the "*Quinean task*" of metaphysics "is to say what exists" or to "*list the beings*", and that this list "forms the domain of quantification", which "has no internal structure" (2009, 348). According to the *Sorted Structure* notion of metaphysics, one needs to break down reality into significant distinctions of categories. The Sorted Structure aims to list "(i) the number of categories *n*" and (ii) the "entities in each category" (Schaffer 2009, 355).[184]

Finally, Schaffer's own view corresponds to the *Ordered Structure* notion of metaphysics, where the goal is not only to break things down into categories, but to also to show how the things in each category are related to one another. On Schaffer's view, the goal of metaphysics is to tell us what entities are fundamental and how other entities are grounded in by the more fundamental entities. These sorts of questions simply cannot be answered in a Quinean manner.

Figure 2 below gives a rough example of what such an ordered structure might look like, based upon one of the most prominent categorical distinctions made in the literature: the distinction between *abstract* and *concrete* entities. For instance, Hoffman and Rosenkrantz claim that "the abstract-concrete distinction is, in fact, the *most fundamental* categorical distinction" (2003, 46). The bold blue arrows represent grounding relations, while the dotted arrows represent category specific relations.

[184] For a summary of the notion of metaphysical categories, see Thomasson 2013c.

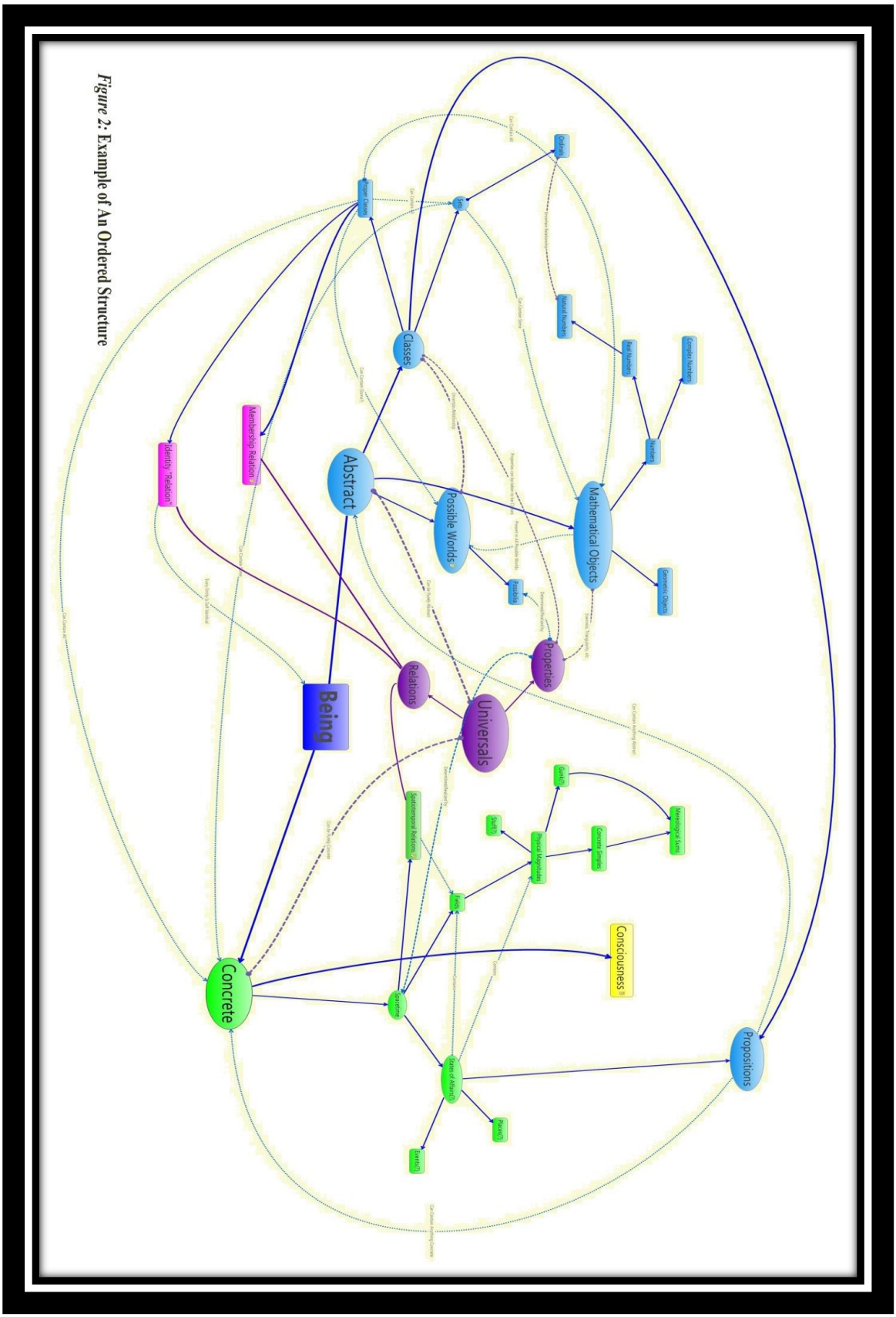

Figure 2: Example of An Ordered Structure

2.2.2. Substantivism vs. Superficialism

The primary question asked in metametaphysics and metaontology is commonly put as follows: "Are metaphysical/ontological questions meaningful?" However, this is a misleading way of framing the dispute. There are actually *two* relevant senses in which a question can be meaningful. First, a question can be meaningful in the technical semantic sense that it has specific answers with determinate truth values. Second, a question can be meaningful in the broad and non-technical sense that it is *substantive*. Consider a dispute between two people over whether a bird flew past their window at 12:13PM or 12:14PM,[185] wherein nothing in particular turns on the answer to the question. The dispute is clearly meaningful in the first sense. However, it is equally clear that the disputants' energy is best spent elsewhere. The question has a definitive answer, but it is not substantive. While there is definitely a fact of the matter, there is also no good reason to spend energy trying to come up with an answer.

We can distinguish two types of metametaphysical positions that one might hold in regard to the nature of metaphysical theories: *substantivism* and *superficialism.*[186] Generally speaking, there are two major senses in which a question of any sort can be considered substantive. The first sense of 'substantive' relates to how one arrives at

[185] That is, understood as being relative to a single inertial reference frame.

[186] I use the term 'substantivism' in spite of its ungrammaticality because it is intended to express a *de dicto* concept, and should thus be distinguished from 'substantivalism', which is used to express a *de re* concept. Alternatively, one can put this in terms of properties: 'subtantivism' is used to describe theories that posit or deny the property of *substantivity* (or *substantive-ness*) to certain linguistic items: to hold some form of substantivism about a specific subject is to hold that certain questions, theories, or disputes are substantive. In contrast, 'substantivalism' is used to describe theories that posit or deny the property of *substantiality* (or *substantial-ness*) to certain purported entities that are *not necessarily* linguistic in nature. For instance, substantivalism regarding space is a first-order metaphysical position that holds that points/regions of space (or spacetime) are distinct entities from the objects that occupy them. Thus in the context of our discussion, substantivism is a type of metametaphysical position regarding the nature of metaphysical disputes, whereas substantivalism is a type of first-order metaphysical position regarding certain classes of entities.

answers to the question. On first approximation, to say that a question is substantive in this sense is simply to say that the question is difficult to answer, and, correspondingly, to say that a question is superficial is to say that the question is easy to answer. While there are multiple ways of calculating difficulty, the important point is that the kind of difficulty under consideration relates to human effort; i.e., to how hard we must *work* in order to arrive at an answer.[187] The second sense in which a question can be substantive relates to the implications of arriving at an answer to that question; it concerns the reasons or motivations one has for expending energy trying to answer the question. To say that a question is substantive in this second sense is just to say that we are invested in arriving at an answer. Both senses are jointly necessary for the type of substantivism at hand: to be a substantivist about a certain question is to both hold that the question is difficult to answer and that a correct answer will have important implications. Likewise, to be a superficialist about a certain question is to hold that the question is either easily answered or unimportant.

Because our distinction between superficialism and substantivism is cashed out in terms of the notion of importance it is only meaningful (in the semantic sense) insomuch as one has a well-defined notion of importance in mind. As stated the distinction cannot be meaningfully applied to metaphysical disputes without further specifying what counts as 'important'. But this is not worrisome, as this distinction is not intended to do any heavy argumentative work on its own. Rather, the notion of 'importance' that is at play in

[187] Thus one should be careful when equating superficiality with triviality, computability, provability, or even analyticity. These are important technical notions, but they do not imply easiness. Some proposition may be computable/provable/analytic in the sense that it follows from first principles solely through the application of logical inference. Mathematical conjectures like the Riemann Hypothesis might be provable but, as mathematicians will surely attest, this does not imply that actually proving these conjectures is an easy task.

the distinction is only intended to meaningful relative to some human interest.[188] Since human interests will vary according to the subject-matter of the dispute, there is no reason to think that there will be only one way of understanding a dispute under this distinction. There may be multiple human interests at play in a given dispute, such that it makes sense to say that the dispute is substantive in one sense and superficial in another. In fact, this is an important methodological point insomuch as philosophers are prone to conflating different senses of importance.

2.2.2.1. Semanticism and Metasemantics

The most popular way to be a superficialist is to argue that metaphysical disputes are confused because their language is somehow defective. In fact, there are two[189] relevant ways in which the language of dispute can be defective: at the semantic level and at the metasemantic level. First, we need to distinguish semantics from metasemantics. On the most common understanding, a semantic theory is a theory that tells us what the

[188] A further objection at this point (one that is likely to come from heavyweight normative realists) is that (1) there *is* in fact one distinguished sense of 'important', (2) this notion of importance is not interest-relative, and (3) this is the *only* sense of 'important' that is relevant to our discussion of metaphysics. The objection would then be that I am merely changing the subject by introducing a technical term. One line of response to this objection would be to question (1) or (2). After all, these are non-trivial semantic claims, and ones that I think are not very obvious. So surely the burden of proof will fall on the objector to provide evidence for (1) and (2). Moreover, (3) is even more controversial insofar as it makes a non-obvious move from semantic claims to methodological claims. However, even if we grant the objector (1), (2), and (3), this still would not affect my account unless the objector can show that none of the forms of superficialism or substantivism I present utilize the relevant notion of 'importance'. Thus even if the objector does not bear a general burden of proof due to the controversial nature of her premises, in order to actually touch my arguments she will nevertheless have to provide an account of the relevant notion of importance. Moreover, my classification of the various positions below is fairly standard: Hirsch, Thomasson and Hofweber would all generally be considered superficialists by most metaphysicians. The objector might argue that the relevant notion of importance is "brute" or "primitive". But in this case we would not only lack a principled way of settling whether any dispute is actually important; we would not even have any heuristics that would allow us make any progress on the matter. This would reduce any debate over whether or not something is important to mere foot-stamping. Thus, as was argued in the section on methodology, even if it were true that the pre-theoretic notion (i.e. a notion that we possess prior to methodological theorizing) of 'importance' is primitive, we still would not be permitted to appeal to this notion in order to settle academic disputes.

[189] Technically, one might also worry that a dispute can be defective at a pragmatic level, but this position is usually not defended.

expressions of a language designate and how these expressions can be combined in order to express truth-evaluable claims about the world. In contrast, a metasemantic theory tells us *how* expressions receive their semantic contents (i.e., *how* they refer) through an explanation of the non-semantic facts (e.g., through facts about human behavior and interests).[190]

For instance, Eli Hirsch has argued for a form of superficialism about ontological disputes based on the idea of "quantifier variance", which is roughly the position that existence claims are subject to multiple equally good interpretations such that ontological disputes have no determinately correct answer.[191] Roughly, the idea here is that the way the reference of our terms is fixed is plastic enough to accommodate multiple ways of speaking. Thus according to Hirsch, since both disputants in an ontological dispute can be interpreted as being correct, these disputants must be talking past one another. We can call Hirsch's superficialism a species of *semanticism*, since it argues that metaphysical disputes are superficial due to their semantic properties. Hirsch's particular version of semanticism might be called *metasemantic individualism*, since he holds that the

[190] A "semantic theory aims to specify the contents of expressions of a given language", a metasemantic theory attempts to ground a semantic theory by explaining how this theory can be "made true by more basic facts about the world" (Pepp 2012, 4). Another way of putting it is that a metasemantic theory is "a metaphysical proposal about the nature of the reference relation; schematically: "reference is a relation of such-and-such a type"" (Sider 2011, 31). Here the 'such-and-such' will be cashed out in terms of non-semantic facts. Thus one way of understanding deflationary theories of truth and reference is as a denial that this sort of schema can ever be filled out. Bradley Armour-Garb provides a concise explanation of this position: "According to the disquotationalist, our quest to fill in the right-hand side of the following *reducing* formula, / (∀x) (∀y) (x refers to y iff R(x, y) (i.e., for all x and y, x refers to y if and only if x bears R to y), / is in vain; a statement of the form *"N" refers to N* holds, when it does, in virtue of the logic of 'refers'" (1999, 225-6).

[191] For instance, Hirsch introduces the notion in response to the dispute about when mereological composition occurs: "the expression "there exists something" can be interpreted in a way that makes the sentence true or in a way that makes the sentence false. Since both interpretations are available to us, we have a choice between operating with a concept of "the existence of something" that satisfies the mereologist or operating with a different concept that satisfies the anti-mereologist [i.e., mereological nihilist]" (2011, 69).

confusion in ontological disputes arises from the fact that different individuals can have the meanings of their statements fixed in different ways (or can be understood as speaking alternative languages), even though these statements have the same surface structure. The disputes in question are merely verbal because the disputants are actually saying different things. Hirsch justifies this position by adopting a number of controversial theses in the philosophy of language, and thus his position has been criticized on these grounds.[192] Another semanticist position is *Anti-Realism*,[193] which is the denial that the claims in question have determinate truth values in the first place. Positions of this sort have been put forward by David Chalmers (2009) and Stephen Yablo (2009). However, one can also be a superficialist even if one holds that the disputes in question have determinate truth-values and are not confused, by arguing that the answers to the relevant questions are to be found solely through conceptual analysis. For instance, Amie Thomasson, argues that ontological questions only make sense if they can receive easy answers through linguistic or conceptual analysis (2008, 2009a, 2009b).[194] This is a form of superficialism because it implies that ontological disputes are answered relatively easily and do not have important implications.

[192] First, he prioritizes whole sentences over their constituents in regard to what determines the meaning of these sentences (2009, 244-252). Second, he holds that the primary metasemantic principle that fixes reference must be the principle of charity (ibid, 238-244). Thus Hirsch's position has been criticized precisely on the grounds that these two theses are generally implausible (e.g., Hawthorne 2009, Sider 2009).

[193] I am following Karen Bennett's terminology, when she describes antirealism as the claim that "'There are *F*s' does not have a determinate truth-value" (2009, 39-40). Though Bennett reserves the term "semanticism" only for positions like those of Hirsch in which the disputes are verbal (ibid).

[194] A similar position has been advanced by Thomas Hofweber, who claims that all ontological questions are either answered in the positive directly by the sciences or answered in the negative when philosophers use conceptual analysis to show that scientific discourse in fact has no ontological commitment to the entities in question (2009). This counts as a mild form of superficialism insofar as it implies that philosophers on their own cannot establish that any entity exists, but rather only that certain entities do not exist.

What unites all of these positions is their claim that metaphysical claims (and ontological claims in particular) are dependent heavily for their truth on facts about language. Metaphysics is generally taken to be a description of the world, not a description of how we use language. In general one can think of metasemantic theories as falling on a sliding scale: on one end of the scale the structure of the world is taken to do most of the work in determining the meaning of our claims, while on the other far end it is the way we use language and our intentions that are taken to do all the work. The positions of people like Hirsch and Thomasson result in superficialism because they claim that metaphysical claims track our own linguistic proclivities rather than the real structure of reality.[195] One of the major problems that face metaphysics is that the primary form of evidence used in metaphysical disputes is linguistic. The question is: how can evidence from language tell us something about the world?

Metasemantic views vary according to what they take the relevant non-semantic facts to be. Metasemantic views must accomplish several things. First, they must establish what balance of factors determines the semantic content of an utterance. Such factors include interpretive charity (i.e., whether a particular reference assignment makes most of what we say true), knowledge maximization (i.e., whether a particular reference assignment makes it such that we know many of the statements we assert), the naturalness of the putative referent, and causal relationships.[196] Second, they must

[195] Hirsch and Thomasson fall on the latter side of the scale, while Sider and Hawthorne fall on the former. This is what makes Hirsch and Thomasson superficialists and Sider and Hawthorne substantivists. Indeed, the literature itself reveals that this metasemantic dispute is at the heart of the dispute between these different parties. Again, see Hirsch 2009 vs. Hawthorne 2009, and Sider 2011 (esp. pp. 23-35, 44-77) vs. Thomasson 2014.

[196] This is perhaps best explained in terms of the historical development of metasemantic views. Originally, one popular metasemantic view was one on which the only principle was interpretive charity, i.e. the claim that the reference of our terms is to be determined by the assignment that makes most of our

determine the syntactic level at which meaning is fixed (i.e., at the sentential or sub-sentential level). Third, they must determine *what* actually expresses semantic properties (i.e., individual token utterances or *types* of utterances). Fourth, they must determine the *social level* at which semantic properties are fixed (i.e., whether the meanings of expressions are determined by the cognitive dispositions of each individual speaker through some form of 'mentalese', or whether individuals can only use symbols that have their meaning fixed at the broad social level). All of these issues are obviously interrelated. A dispute can be defective at a metasemantic level even if the disputants are not talking past one another, simply because the dispute does not track anything interesting.

In order to illustrate this point, let us take the dispute about the nature of morality as an example. Recall that there are two ways in which someone can be a superficialist about a dispute. The first is by claiming that the dispute is either confused or answered easily one way or another. One way of making this claim in the context of metaethics would be to argue for a form of non-cognitivism about moral claims. The second way is to argue that even if one *could* get answers to moral questions, they would lack

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statements true. However, this view was shown to be implausible by Hilary Putnam's "model-theoretic argument" (1977; 1980), in which Putnam showed that for any reasonable model-theoretic assignment function of semantic contents to expressions, there is always another extremely unreasonable permutation on this assignment function that preserves the truth values of the claims in question. In response to Putnam's argument, David Lewis developed a metasemantic theory on which the correct reference assignment is determined by balancing the principle of charity with a principle on which predicates should not receive "unnatural" properties as their extensions (1983a; 1984). Lewis' theory of naturalness has since been developed extensively by metaphysicians, especially by Sider (2011, 23-35). Others have argued that this combination of naturalness and charity will inevitably fall prey to unfortunate forms of indeterminacy (Williams 2007). One response is to supplement this approach by further principles. For instance, Williamson proposes a metasemantic principle of knowledge maximization; i.e., that reference is determined by the interpretation that maximizes the knowledge of speakers (Williamson 2004; 2007, Ch. 8). Another approach is to stop thinking solely about abstract interpretation and to start formulating metasemantic principles by considering how language is situated in the physical world. Since our ability to refer to things must ultimately depend upon other human cognitive capacities like perception, there is good reason to take the causal relations that underwrite these cognitive capacities as a further metasemantic factor (Hawthorne 2007, 431-2).

importance. However, this option has largely been ignored in metaethical disputes. Presumably the implications of moral theory are straightforward: if one can provide arguments that establish that there is one true moral system, then one should be capable of using those arguments to convince any agent (who isn't clinically psychotic) to act in a certain way, regardless of that agent's social or cultural background.

Moral realism should technically be understood as the metaphysical position that there are distinctively moral entities or facts.[197] Moral realism can vary in strength. The weakest form of moral realism is simply the claim that there are moral properties or facts, whatever these properties might be. There is also a stronger sense in which one can claim that these moral facts are somehow mind-independent or objective. We define Strong Moral Realism (SMR) and Weak Moral Realism (WMR) as follows:

(SMR): There is a unique set of moral properties such that: (1) These properties serve as reference magnets for the terms of any possible natural language that broadly serve the same linguistic functional roles as the terms 'right', 'wrong', 'good' and 'bad' do in English. (2) Sentences involving these terms are either true or false according to how things are with these properties, such that for any two languages that have the appropriate terms, claims made using these terms can be translated straightforwardly between languages and will receive the same evaluation.[198]

[197] Note that moral anti-realism is distinct from moral skepticism, in that the latter should be understood as the claim that we cannot know about these properties (or, equivalently, that our moral claims are true). Thus one can be a moral skeptic without thereby being a moral anti-realist.

[198] There are several things to note about (SMR). First, note that these are metaethical positions and are thus non-committal in regard to particular normative theories; e.g., whether consequentialism or deontology is true. Second, note that the general idea behind (SMR) is that moral realism is supposed to explain the objectivity of morality. It is very difficult to capture this idea through a purely metaphysical description without relying upon a primitive concept of brute normativity. (SMR) can accommodate this idea with the assumption that it is this brute normativity that makes moral properties reference magnets for the terms in question, regardless of culture. Also note that (SMR) is compatible a form of Kantianism wherein moral claims are true in virtue of reason alone. Here the relevant properties would simply apply to reasons instead of actions. Finally, note that (SMR) is incompatible with the claim that the most fundamental/basic/non-derived moral norms vary by culture. However, (SMR) does not imply that moral properties are totally insensitive to cultural facts. It is just that if cultural facts *are* relevant to a moral evaluation, it is because there is some universal moral fact that is *non*-culturally sensitive that dictates this. Hence (SMR) is compatible with the claim that it is true for all cultures that one should obey cultural norms when these do not conflict with other moral norms, so long as this fact itself is understood to be general and culture-independent.

(WMR): Claims involving terms that serve the same functional roles as the term 'right', 'wrong', 'good' and 'bad' in English have truth values that do not vary according to context.

Both positions claim that there is something in out in the world that serves to ground the truth of moral claims. As the names imply, (SMR) implies (WMR), but (WMR) does not imply (SMR). We can now define the negation of these two positions: Strong Moral Anti-Realism (SMA) is the denial of (WMR) whereas Weak Moral Anti-Realism (WMA) is the negation of (SMR):

(SMA): Claims involving terms that serve the same functional roles as the term 'right', 'wrong', 'good' and 'bad' in English either do not have truth values at all, or their truth values are fixed relative to contexts involving emotional states or social facts.

(WMA): There are no unique *sui generis* moral properties that meet conditions (1) and (2) in the definition of (SMR).

(SMA) implies that first-order moral disputes are either merely verbal or otherwise confused because the claims of both sides are straightforwardly false.[199] Moral non-cognitivism and moral nihilism would both be forms of (SMA).[200] Thus the superficialism established by (SMA) falls under the first category: it implies that moral questions are easy to answer.

[199] Note that what is at issue here is superficialism about first-order moral disputes (i.e., superficialism regarding disputes about what one ought to do either generally or in a given situation). This is obviously distinct from superficialism about metaethical disputes. If non-cognitivism is true then moral disputes are superficial, but this does not imply that the question of whether non-cognitivism is true is itself superficial. The same reasoning applies to ontological disputes. Thus if either Hirsch or Thomasson are correct then ontological disputes are superficial. But even if Hirsch and Thomasson are correct, this does not imply that it will be easy for Hirsch and Thomasson to *show* that their positions are correct.

[200] While (SMR) and (WMR) make reference to linguistic facts, there is also room in theoretical space for the claim that there are moral properties but that we are unable to refer to them. However, I will ignore this possibility because it seems unmotivated and, as far as I am aware, no one explicitly embraces such a position. Such a position would only differ from nihilism or relativism in that it would operate with an additional metasemantic claim that seems to be poorly motivated on its own, and can thus be ignored.

However, all that is necessary for superficialism is (WMA), which is compatible with (WMR). (WMR) is compatible with the idea that some cultures might have a deontological value system while others might be consequentialist and that there is no fact of the matter about who is correct. (SMR) is supposed to capture is that the existence of moral properties should somehow ground our ability to criticize anyone according to one objective standard, regardless of cultural context. Thus by rejecting (SMR), (WMA) implies that the answers to moral questions will not have important implications, even if those questions have determinate answers. While (WMA) allows that there is a fact of the matter about what moral claims are true in English, this can be largely determined by our cultural practices and thus that we can choose to change these cultural practices and value things differently if we so wished.

The question is whether facts about how English language users speak can be used to establish something like (SMR). Much of the dispute between moral realists and anti-realists over the past century has been about whether or not moral terms have their truth values fixed rigidly and independently of context (i.e., about whether (SMA) is true). However, even if we allow that the moral terms in English function as rigid designators without any form of semantic relativity or context sensitivity, this still would not be enough to imply that (WMA) is false. All the weak anti-realist needs to deny is that it is something within the world independently of cultural or social facts that serves to fix the reference of normative-evaluative terms. If the metasemantic facts that ground the semantic value of moral terms vary by culture, then any disagreement across languages will be merely verbal, even if both parties mistakenly take themselves to be in

disagreement.[201] In order to avoid superficialism, moral realists must establish that there are culture-independent facts that ground the *metasemantics* of moral terms in any language whatsoever. Facts about how English speakers evaluate utterances are not enough to establish moral realism.

There are two senses in which the terms 'inflationary' and 'deflationary' can be applied to a theory: *quantitative* and *qualitative*. The quantitative sense applies to the number of commitments of first-order ontologies or metaphysical theories. A quantitatively deflationary theory is generally described as being *parsimonious* in its commitments, while a quantitatively inflationary theory is generally described as being *promiscuous*. In addition, we can draw a further distinction in which theories can vary quantitatively: *de re* or *de dicto*.[202] A *de re* quantitatively deflationary ontology says that

[201] Thus instead of claiming that there is *one* word 'wrong' with *one* semantic character that is sensitive to cultural context, the anti-realist need only argue that the symbol "wrong" in various iterations of English has been associated with various semantic values over time depending upon the relevant cultural and social factors. For instance, people who speak English in the 21st century will say that homosexuality is permissible and that people who spoke English in the 19th century were incorrect when they said that "homosexuality is wrong". But this, the weak moral anti-realist argues, is because the English speakers of the 21st century are only used to evaluating things from within their own language, and thus mistakenly take the 19th century English speakers' utterances to express the same proposition as their own utterances, individuated phonologically. They fail to recognize that 19th century English is different from their own because they are unaware of the metasemantics for normative-evaluative terms. No one within a linguistic community who uses the term need be aware of or acknowledge this form of semantic variability, as the underlying facts that ground this variability are distant from everyday experience. The facts that ground the variation in semantic content are not part of the character of the terms, but are rather due to broad sociological changes. Thus instead of changing semantic values in the same way as the terms 'I' or 'here', the symbol "wrong" instead changes semantic values in the same way as the symbol "Madagascar" did. Knowing how to use indexicals like 'I' and 'here' will involve an understanding of how the context of utterance helps to determine the semantic content of these terms, but the variance in terms like "wrong" are so gradual that no one need be aware of them in order to use the term competently in her own language, just like no one needs to know the symbol "Madagascar" was originally used to refer to part of mainland Africa in order to use it competently now. Understanding and linguistic competence simply do not demand knowledge of metasemantic facts. Moreover, there is little reason to think that intuitions will be reliable in this domain. Thus the dependence of moral realism upon metasemantic facts also implies that we should be dismissive of "Moral Twin Earth" arguments (e.g., Horgan & Timmons 1991), as Janice Dowell (Forthcoming) has recently pointed out.

[202] It is common practice to distinguish between "ontological" and "ideological" parsimony. However, this way of cashing out the distinction blurs the line between metaphysics and ontology. For example, according to Schaffer's metametaphysical view *ontological parsimony* (which is determined by the

reality consists of very few things and/or there are very few ways in which things might exist. A *de re* quantitatively inflationary ontology says that reality consists of many different things and/or that there are many different ways in which things can exist. A *de dicto* quantitatively deflationary ontology uses only one piece of ideology (i.e., one quantifier) to describe what exists, while a *de dicto* quantitatively inflationary ontology uses multiple pieces of ideology (i.e., multiple quantifiers) to describe what exists. A *de re* quantitatively deflationary metaphysical view treats reality as having a relatively simple structure, while a *de re* quantitatively inflationary metaphysical view treats reality as having a complex structure. A *de dicto* quantitatively deflationary metaphysical view uses very few primitive pieces of ideology in order to describe reality, while a *de dicto* quantitatively inflationary metaphysical view will involve a larger number of primitive pieces of ideology.

The qualitative sense of the inflationary/deflationary distinction concerns the *importance* of the theory and its implications. Superficialism is simply qualitative deflationism, while substantivism is qualitative inflationism. It is important not to run these two senses together, as they come apart in significant ways. For example, one way for a metaontological view to be qualitatively deflationary (i.e., to be a form of superficialism) is for it to be quantitatively inflationary. The idea here is that one can

number of things) is far less important than *metaphysical parsimony* (which is determined by the number of fundamental entities and the number of *grounding relations* between these fundamental entities and derivative entities) (2009, 361). Since Schaffer's view holds that grounding relations are *de re*, we can consider both of these as measurements of *de re* quantitative parsimony. To illustrate the distinction between *de re* and *de dicto* (i.e. ideological) parsimony, one need only consider metametaphysical disputes regarding the nature of hyperintensional notions. For instance, while Schaffer holds that the primary hyperintensional notion is a straightforward grounding relation between entities, Theodore Sider holds that the primary hyperintensional notion is that of *structure*; which is, roughly, a property of how languages match reality (2011 [see especially pp. 164-5 for a comparison with Schaffer's theory]). A theory that allows both a primitive *de re* notion of grounding and a primitive *de dicto* notion of structure would be more ideologically complex than either Schaffer's or Sider's theories (i.e., the theory would have both an undefined relation '*G*' that it predicates of entities as well as an undefined predicate '*S*' that serves as an operator on the ideology of a language).

deflate the importance of disputes about what exists by arguing that existence is *cheap*; i.e., that virtually everything we talk about exists in some manner or another.[203] Since the two senses in which theories can be inflationary or deflationary are quite distinct, we will henceforward reserve the terms 'parsimonious', 'promiscuous', 'inflationary', and 'deflationary' for only the quantitative sense. Instead of speaking of qualitative deflationary/inflationary views we will speak of superficialism and substantivism.

This provides us with two things to keep in mind while analyzing the role of intuitions in metaphysics. First, an account of intuitions that must assume superficialism in order to show that intuitions should count as evidence in metaphysics will generally be self-defeating in that it will render metaphysics unimportant. This point will become extremely important when we briefly consider such theories later in §2.3. Second, the different versions of quantitative simplicity will serve as one of the primary pieces of evidence for determining theoretical success in metaphysics. A view that has more *de re* or *de dicto* parsimony than another will be more successful than the other *ceteris paribus*. Moreover, it is generally agreed that the sheer number of individual things is not as important as the number of types of things, and that neither is as important as *de re* or *de dicto* metaphysical parsimony.

[203] For instance, this is the sort of view proposed by Matti Eklund as an alternative to ontological pluralism, which he dubs *maximalism* (2009).

2.3. How to Find and Evaluate Intuitions

A theory of how intuition works is only relevant to the methodology of metaphysics if metaphysicians actually appeal to intuitions as a method for establishing important conclusions. Thus it is pointless for us to discuss the nature of intuition until we first determine *whether* and *how* intuitions are used as evidence in metaphysics. Rather than examining particular epistemological and semantic theories of intuition and then drawing conclusions about metaphysical practice, we shall proceed in the reverse order. We will begin with an examination of how intuitions appear to be used by metaphysicians and then use this as a framework for our latter discussions of the nature of intuition.

Examining the role that appeals to intuition play in metaphysical practice will help us determine whether particular theories of intuition can actually vindicate this practice.[204] What we will ultimately show is that while metaphysics can get along fine in most circumstances, a few particularly intractable disputes must ultimately be caused by appeals to intuitions. However, we obviously can't identify appeals to intuition without at least some inchoate account of what intuitions *are*. Thus before we proceed to examine the problematic cases we must start with an inclusive *prima facie* characterization of intuition. This will serve as a neutral hermeneutic starting point insomuch as it will not discount any theory of intuition from the start. For now, we will only identify what the possible intuitions are and how the continued intractability of certain disputes must ultimately rest on appeals to intuitions of this sort because all other forms of evidence have been agreed upon.

[204] This will also allow us to significantly narrow the scope of the arguments presented in later chapters. Rather than showing that accounts of intuition are inadequate on independent grounds, we will only need to show that they cannot support the way intuitions are used in metaphysics.

2.3.1. Types of Intuitions

For now we shall assume that intuitions should be individuated according to their phenomenal character and propositional content.[205] Prior any commitment to a particular theory of intuition, introspection upon typical cases reveals that intuitions fall into two broad phenomenal categories: *de re* vs *de dicto*. Reflection upon common appeals to intuition reveals that the propositional content of intuitions can vary along three dimensions: *particular* vs *general* vs *theoretical*. An elaboration of these two dimensions will yield a useful framework for examining the different intuitions upon which theories might depend.[206]

The distinction between *de re* and *de dicto* intuitions is best characterized in terms of phenomenal character. *De re* intuitions are constituted by imagined sensory images of the objects in question. They seem to be directly about objects or entities themselves in a similar way to how one's visual experience is directly about the objects in front of oneself.[207] In contrast, a *de dicto* intuition attends to a *fact* or a *state of affairs* without any particular focus upon the objects involved therein. *De dicto* intuitions only have an ephemeral non-sensorial 'cognitive phenomenology' without any attendant sensory-like

[205] Every major account of intuitions worth treating assumes that intuitions are either a sort of propositional attitude or else a type of experience that immediately and intimately supports belief in a proposition. The only person who seems to fall into the latter category is Chudnoff, who identifies intuitions as perception-like experiences that immediately justify certain "basic" propositions (2013c, 84-98). This doesn't really bare upon our current project.

[206] Note that many of these categories will be considered illegitimate given different substantial *theories* of intuition. My goal in starting with this broad distinction is to capture as much hermeneutic ground as possible prior to any theoretical commitment regarding the nature of intuition. We will consider the general semantic and epistemological implications of these distinctions as we proceed.

[207] Consider Sydney Shoemaker's "object-perceptual" model of introspection: "While sense perception provides one with awareness of facts, i.e., awareness *that* so and so is the case, it does this by means of awareness of objects. One's awareness of the facts is explained by one's awareness of the objects involved in these facts. So, for example, I am aware (I perceive) that there is a book before me *by* perceiving the book-here the book is the (non-factual) object." (1994a, 252-3)

states. They are best characterized as the experience of 'grasping' the truth of a proposition by understanding its meaning. One has a *de dicto* intuition that *p* when *p* just *seems* to be true or 'sounds right'. Grammatical and semantic judgments would be obvious examples of *de dicto* intuitions.

The second distinction has to do with the propositional content of intuitions. Particular intuitions concern either particular entities or propositions. General intuitions concern propositions with either modal or universal content about general facts (e.g., *all water is H_2O*; *necessarily, all water is H_2O*; *possibly, there is something that is both water and not H_2O*). Theoretical intuitions, perhaps unsurprisingly, concern theories (e.g., *Deflationary theories of truth are not explanatorily satisfying*).[208] This presents us with a four-fold categorical distinction. It will be helpful to illustrate the *de re*/*de dicto* distinction with some ostensive phenomenology. I ask that the reader to please attempt to simulate the sort of states described by the following (very rough) examples:[209]

	Particular	General
p =	*The lump of clay survives being smashed while the statue does not.*	*It is not possible for two objects to occupy the same place at the same time.*
De re	One's intuition that *p* is grounded by one's visualization of the scenario where it looks as though the statue is gone while the lump of clay is still there.	One's intuition that *p* is grounded by one's failed attempt to visualize two distinct objects occupying the exact same location.

[208] From the standpoint of semantics, theoretical intuitions are no different from other particular intuitions. However, their content obviously differs in a relevant way for understanding the methodological role of intuition.

[209] Note that nothing turns on these examples either (1) being reliable or true, or (2) counting as "real" intuitions according to some theory. If the reader is bothered by the fact that they disagree with the content of these examples, I ask that he or she attempt to imagine the same sort of states in response to different propositions.

De dicto	One hears that there is a statue made of clay. One is then told that the statue has been smashed into an unrecognizable lump. The sentence "the statue has been destroyed" sounds correct, while the sentence "the lump of clay has not been destroyed" also sounds correct. This occurs without any robust visualization of the scenario.	One infers *p* from the *de dicto* counter-intuitiveness of ~*p*. In other words, the semantic decomposition of *p* leads one to have the immediate reaction: "What would it even *mean* to say that two objects occupy the same place at the same time?" One does this without visualizing anything.

Every major theory of intuition will (roughly) treat intuitions as falling into at least *one* of these categories, though they may characterize these categories somewhat differently. I expect many readers will react to this categorical breakdown negatively by claiming that many of these examples should not be counted as *real intuitions*. Some of these "intuitions" will no doubt look illegitimate. One might complain that I am hermeneutically 'stacking the deck'. In other words, the complaint would be that in adopting this categorical distinction we have read metaphysicians uncharitably as relying upon implausible claims when we should have instead read them as appealing to the kind of intuitions posited by the 'best theory'. At this stage of our project we are only concerned with establishing how and when metaphysicians appeal to intuitions, if at all. It would be dialectically inappropriate to assume one of these theories in our hermeneutic treatment of metaphysical practice.[210] These distinctions will guide us in providing multiple interpretations for each apparent appeal to intuition, which will in turn help us avoid ruling out any particular theory of intuition from the start.

[210] For instance, Elijah Chudnoff is (roughly) committed to a *de re* account of intuitions as 'intellectual perceptions' (2013c). Chudnoff would thus have a legitimate complaint if we claimed that his account of intuition is irrelevant to metaphysics because we only interpreted metaphysicians as appealing to *de dicto* intuitions in the first place. At the same time, George Bealer (1996, 1998), who is (again, roughly) committed to a *de dicto* account of intuitions as 'intellectual seemings', could also rightfully complain if we proceeded in the reverse order. At this stage in our argument it is better to be overly permissive and appear ridiculous than be overly restrictive and beg the question. Note also that we are ignoring doxasticism regarding intuition. Since doxasticism is far too weak to legitimize the cases I consider, it is safe to ignore it here.

Nevertheless, we should point out some preliminary points that are worth bearing in mind. In order for a particular theory of intuition to be relevant to metaphysical practice it is not only necessary to show how intuitions should have evidential status; it must also describe intuitions in such a way that it is reasonable to say that metaphysicians actually have them. These types of intuition correspond different theories. The *de re* / *de dicto* distinction roughly matches the common distinction between intellectual perception accounts and understanding-based views. The visual phenomenology of the *de re* account of intuitions corresponds to theories of intuition as a form of "intellectual perception" (e.g. Chudnoff 2011a,b, 2013a,b,c). In contrast, the cognitive phenomenology (if there is such a thing) of the *de dicto* accounts of intuition correspond to theories in which intuitions are based on understanding or conceptual competence (e.g. Bealer 1996a, 1998, 1999, 2000). In fact, these accounts would appear to be mutually exclusive. Visualizations cannot be understood as falling under most accounts of conceptual competence. The account of intuitive justification offered in the account of intuitions as intellectual perception depends upon intuitions having a sensorial phenomenology that appears to "make one aware" of their truthmakers (Chudoff 2013c, 83-113). This is obviously not available for *de dicto* intuitions, which are more properly described phenomenologically as occurrent affective states that immediately bind to propositions (i.e., the *feeling* of attraction or inclination to assent to *p*).

A second important point is that the different types of intuitions are better suited to different evidential roles. *De re* intuitions are suited to first-order metaphysical disputes about what exists or about what grounds what (e.g., the nominalist says that numbers can't exist because there's "nothing to them" or because they don't have any

"oomph"). Alternatively, *de dicto* intuitions are better suited to establishing claims about the nature of certain *concepts* (e.g., mereological realists deny that singular terms can refer to mere pluralities; one can just *tell* that the word 'table' can only refer to one thing if it refers at all). This point becomes important when we actually consider particular theories of how intuitions work.

2.3.1.1. Intuitive Strength

The most natural account of intuition will allow them to vary in their phenomenal strength; i.e., in how strongly or convincingly they convey their contents. Moreover, most rationalists will likely treat some intuitions as less forceful than others simply in virtue of their content. The more universal, abstract and divorced from everyday experience the principle, the stronger the intuition.[211] This should be independently plausible given the phenomenology of our intuitive judgments: principles of logic seem to present themselves with greater certainty than judgments about epistemology. However, rationalists *need* this assumption in order for intuitions to do the kind of evidential work that they want them to do. Philosophical practice reveals that we are generally less willing to give up principles of logic than we are metaphysical principles,[212] and are less willing to give up logical or metaphysical principles in order to achieve results in

[211] For instance, if a rationalist wishes to account for why intuitionist logic seems more plausible than other forms of non-classical logic, she will need to say that some relevant axioms of classical logic and their semantic counterparts (e.g., disjunctive syllogism and the law of the excluded middle) are less intuitively forceful than others (e.g., *reductio ad absurdum* and the principle of non-contradiction). Moreover, the intuitions regarding the principles of first order logic will present themselves with more force than metaphysical intuitions (e.g., that the whole is nothing over and above the parts), which will in turn treat themselves as being less forceful than intuitions concerning epistemology (e.g., the person in the Gettier case does not know) and ethics (e.g., one should always act in such a way at to maximize overall happiness).

[212] For instance, the outcome of the debate over contingent identity in regard to problems of persistence for material objects has obviously favored accounts that preserve Leibniz's principle and instead attempt to deal with the problem by positing a more complex metaphysical picture such as four-dimensionalism. For another example, consider how few metaphysicians have been willing to dissolve the 'Special Composition Question' by embracing a view on which composition is literally identity.

epistemology[213] or normative ethics,[214] etc. The rationalist will want to say that the evidence in each of these areas is supplied by intuition, so if there is to be a difference in how this evidence is weighted it must be grounded in a difference in the intuitions themselves.

However, the admission that intuitions can vary in strength makes intuitive judgments susceptible to the kind of vagueness that undermines their luminosity. The lack of standards of evaluation essentially allows metaphysicians to pick and choose how heavily they weight certain intuitions. There is substantial motivation to overestimate the evidential strength of intuitions that support one's theory while downplaying the evidential status of intuitions that conflict with it. Williamson puts the point as follows:

> An adequately fine-grained theory of intuitions would have to distinguish weaker ones from stronger ones in evidential impact. If the strength of intuitions is taken into account, the evidence will be recorded in something like the form "I have an intuition of strength *s* that P." The strength parameter *s* will have to be specified according to some common scale, in order to permit the comparisons between the strengths of sometimes conflicting intuitions which the theory of evidence will need to make. But that will give plenty of scope both for misjudging the strength of one's intuitions and for being accused by others of having done so. After all, philosophers have a powerful vested interest in persuading themselves and others that the intuitions which directly or indirectly favor their position are stronger than they really are. The stronger those intuitions, the more those who appeal to them gain, psychologically and professionally. Given what is known of human psychology, it would be astonishing if such vested interests did not manifest themselves in some degree of wishful thinking, some tendency to overestimate the strength of convenient intuitions and underestimate the strength of inconvenient ones. In trying to

[213] For instance, very few philosophers are willing to attempt to solve Benaceraff's problem in the epistemology of mathematics by either admitting that numbers are causally efficacious or that mathematical truths concern contingent features of reality.

[214] For instance, very few philosophers are willing to argue against causal determinism in order to preserve a libertarian view of free will when the former seems to be heavily supported by both metaphysical argument and evidence from the sciences (supposing that quantum indeterminacy occurs at the improper size-scale to ground decisions attributable to agents). A recent survey reveals that the majority of the field favors some form of compatibilism (59.1%), while only a small minority favors libertarianism (13.7 %) (Bourget & Chalmers Forthcoming, 12).

> compensate for such bias, one may undercompensate or overcompensate; the standpoint of consciousness gives one no privileged access to whether one has succeeded, for bias does not work by purely conscious processes. Its effects are much easier to observe in others than in oneself. (2007, 236-7)

In order to understand how intuitions serve as evidence we will need some objective measure that can be used to determine the actual strength of the intuition in question. As we shall see below, intuitions about general metaphysical principles are often given heavy weight as evidence. Just as it would be reasonable for a scientist to question the results of an experiment by questioning how the data was collected and analyzed, so it is also reasonable for metaphysicians to question whether the intuition backing some putatively important metaphysical principle really does have the intuitive support that its proponents take it to have.

2.3.1.2. Constitutivism and Understanding-Based Views

De dicto intuitions are most naturally subsumed by understanding-based views of intuition. Understanding-based views argue that intuitions are justified in virtue of our conceptual competence (e.g., Bealer 1996, 1998). This view is intimately tied to the 'method of cases'. Historically, the method of cases receives its legitimacy within the paradigm wherein philosophy is conceived of as *conceptual analysis*.[215] The idea is that philosophy of language, epistemology and other fields are in the business of analyzing our *concepts*. The judgments that serve as evidence in these analyses (e.g., that the person in the fake-barn case does not know) are supposed to be reliable in that they are generated

[215] For instance, Jakko Hintikka (1999) attributes the current popularity of appeals to intuition in philosophy historically to an attempt to emulate the methods employed by Chomskian linguistics. This comparison has been openly embraced by many who do experimental work on semantic intuitions in the philosophy of language. For instance, Edouard Machery and Stephen Stich argue that "there is a strong case to be made for philosophers of language to follow the lead of generative syntacticians and to replace the appeal to their own and their colleagues' intuitions with systematic empirical studies of ordinary speaker's intuitions" (2012, 499). However, the same idea can be extended beyond semantic concepts to other traditional philosophical analyses, such as the Gettier cases in epistemology.

by our conceptual competence. If all we are engaged in in epistemology is a study of how we use the word 'knows', then it makes perfect sense to trust our judgments. In this case, our judgments constitute the subject-matter of our study in that our ordinary judgments about knowledge determine the extension of the concept KNOWLEDGE.

One may call this meta-philosophical position regarding the evidential status of intuitions *constitutivism*. According to the constitutivist, "some intuitions are true simply in virtue of being intuited by the majority of people" (Seeger 2010, 238). It is not just that our intuitions are reliably formed in response to their subject-matter; it is that *they partly ground the subject-matter*. The subject-matter of metaphysics is different from the subject-matter in other areas of philosophy that employ intuitions in the method of cases, such as philosophy of language and epistemology.

This happens when the subject-matter in question intimately involves human beings. For instance, consider intuitions about what a particular term refers to (e.g., the case Gödel case and other thought experiments presented in Kripke 1981, 83-93). We can expect these intuitions to be at least somewhat reliable because our judgments about what terms refer to in part *determine* how these terms refer. It is difficult to imagine how everyone in a linguistic community could consistently use terms incorrectly, as the very way in which they use their terms is in large part determines how these terms refer. Of course, competence in using a term and competence in making metalinguistic judgments about the term are two different things. However, while the former does not immediately license the latter it is still difficult to imagine a case in which members of a linguistic community could use terms in a consistent way and yet make systematically incorrect metalinguistic judgments about these terms. To do this they would have to be blind or

ignorant of how they use their terms. This ignorance cannot be systematic without thereby also making the linguistic community incompetent on the whole.

Constitutivism also makes sense when there is good reason to think that the semantics of a term are fixed through stipulation. This can happen when terms are introduced in order to serve technical purposes. For example, Amie Thomasson describes how this happens for legal terms:

> More interestingly, application conditions for terms for institutional and legal kinds can generally be given in other terms, as terms for contracts, debts, touchdowns, etc. are explicitly introduced by way of specifying (in other terms) conditions under which a contract is made, a debt incurred, a touchdown scored. Though these may only involve (e.g.) an open list of sufficient conditions for application of the term, rather than a set of necessary and sufficient conditions, they may still be useful in evaluating existence claims. So, e.g., supposing it is sufficient for the term 'law' to apply that the members of the legislature vote for a bill which the president signs, we can infer that the term refers and so that there is a law if those conditions are fulfilled (where those conditions don't appeal to the existence of a law). (2008, 69)

There are several of problems with the constitutivist view. First of all, this view only works if one ignores the possibility that the philosophical subject-matter in question concerns *natural kinds*. For instance, if 'knowledge' is a natural kind term as the notion is understood by direct-reference theories, this opens up the possibility for widespread error in our judgments about knowledge.[216] Just as we were mistaken about whether whales were fish, so we can be mistaken about whether the person in the Gettier cases knows. Perhaps there is some interfering factor that makes our judgments about Gettier cases unreliable. This line of response undermines the evidential status of intuitions.

[216] If one accepts this semantic thesis then one can also reject the claim that our judgments about these cases come from a 'Voice of Competence'. Michael Devitt does precisely this. He argues that semantic intuitions (e.g., about reference) are not "justified because those intuitions are a priori of "the voice of competence" ", but should rather be seen as "empirical judgments" (2012, 563). Thus he argues that these judgments are "often good, albeit indirect, evidence" and that "we can find more direct evidence by looking to the linguistic reality that these intuitions are about" (ibid).

However, there is a bigger problem with constitutivism: it trades substantivity for reliability. Constitutivism might make sense for subjects like epistemology, but it severely undermines the claim metaphysical disputes are substantive. For instance, to claim that our judgments about what is necessary or possible somehow *constitute* the nature of metaphysical modality is to straightforwardly embrace superficialism about metaphysical modality. To say that our judgments about the irreducibility of tensed quantifiers somehow *constitute* the nature of time itself is to claim that the nature of time will vary relative to linguistic community. Thus it is impossible to justify intuitions in metaphysics by appealing to some form of constitutivism without thereby also embracing some form of superficialism about metaphysical disputes. This will make it very difficult for understanding-based views to account for intuitions in metaphysics.

2.3.1.3. Defeasiblism and Phenomenal Dogmatism

De re intuitions are most naturally subsumed under intellectual perception theories of intuition. These views attempt to show that intuitions are justified in different ways. For instance, Chudnoff argues for his intellectual perception view by arguing for what he calls *Phenomenal Dogmatism* (2013c, 83-113). Roughly, his position can be summed up by the following principle: "If your intuition experience representing that *p* justifies you in believing that *p*, then it does so because it has presentational phenomenology with respect to *p*" (2013c, 94).[217]

[217] Chudnoff's argument rests on an analogy between intuition experiences and perception. The argument is roughly this: the only reasonable explanation for why one is justified in believing perceptual experiences is because the phenomenology of these experiences provides one with immediate epistemic warrant. This point is essentially made through argument by elimination. Roughly, perceptual justification has to be basic because it cannot be justified in any other way without boot-strapping. Chudnoff extends analogous arguments to intuitive justification. A large part of his argument also rests on negative claims concerning the inadequacy of understanding-based views in accounting for many putative intuitions (2014). This is similar to Williamson's argument that the claim that logical principles are known on understanding alone can only be justified given a "thick" notion of understanding (2007, Ch. 4).

We have no need to question this part of Chudnoff's project. Rather, we need merely point out that even if intuitions are justified through their phenomenology, this does not show that they have anything more than an extremely defeasible *prima facie* justification. As we shall see in the next section, the role intuitions play in metaphysical disputes requires far more than this. It is not enough that these intuitions be *prima facie* justified: they are used in these disputes as heavily weighted evidence that is capable of overriding other theoretical considerations. Thus the phenomenal dogmatist must tell us why intuitions have *this* sort of evidential status. Phenomenal dogmatism simply doesn't do the job. Even if perceptual experiences receive their initial *prima facie* justification through something like phenomenal dogmatism, this will not be enough to override *specific challenges* to perceptual evidence. Perceptual experience is also known to be generally reliable. It is our knowledge of how perception works and what makes it reliable that allows us to determine whether a particular perceptual claim should be given up in the face of competing evidence. Thus the intellectual perception view will need further modification if it is to justify the use of intuitions in metaphysics.

2.3.2. When Are Intuitions Important?

At this point in our project we are merely concerned with *de facto* claims about how intuitions are used as evidence. In particular, we are concerned with answering *The Question*: Are appeals to intuition used as evidence in a consistent manner within metaphysical disputes? The *de jure* question of whether these appeals to intuition can be methodologically justified will then be settled by (MC): if the answer to *The Question* is negative (i.e., if appeals to intuition are used in a conflicting manner and there is of present no way to settle these conflicts) then we must abandon these appeals until we can

arrive at some general agreement as to how intuitions are to be used as evidence. This abandonment will not be a result of skepticism regarding intuition, but rather the result of the demands placed upon metaphysicians by their professional standards.

The Question actually concerns two forms of consistency: consistency regarding the criteria about what counts as a valid intuition, and consistency regarding the evidential weight intuitions possess in terms of other forms of evidence. In regard to the first form of consistency, the question is whether philosophers sometimes count one judgment as an important intuition while ignoring other inconvenient judgments that appear to share all the same relevant phenomenal characteristics. In other words, the first question is whether philosophers appear to pick and choose what counts as an intuition to suit their theories. In regard to the second form of consistency, the question is whether philosophers take certain intuitions to override other forms of evidence (such as explanatory power or theoretical simplicity), while taking inconvenient judgments that appear to share all the same relevant phenomenal characteristics to be overridden by these same forms of evidence in other circumstances. In other words, the second question is whether philosophers pick and choose when and how intuitions will outweigh or be outweighed by certain forms of evidence according to how it suits their theories.

2.3.2.1. Bad 'Intuition' Talk

Unfortunately, we cannot simply rely upon philosopher's use of the term 'intuitive'. For example, consider the following argument that Ted Sider gives against monism:

> What is *intuitively* wrong with monism is that it takes the fundamental facts to be facts about the whole world. When an object has an intrinsic property, for instance a certain shape, that seems to be a fact just about that object, not about the rest of the world. (Sider 2007a, 2)

We can attempt to reconstruct the intuition in several ways. First we identify the proposition that is intuitive:

(AP1$_+$) The fundamental facts are about particular objects and their properties.

(AP1$_-$) The fundamental facts are not about the whole world.

(AP1$_T$) Monism does not give a satisfying explanation of the fundamental facts.

The conclusion can be inferred straightforwardly from all three formulations, so the only thing that matters is the phenomenology of the attendant intuitions. Here are the most plausible versions of the intuition in question:[218]

(AP1$_+$ G$_r$) One imagines what one takes to be the states of affairs involved described by the fundamental facts (e.g., an electron "orbiting" a proton) and one is aware of only a few objects rather than the whole world.

(AP1$_+$ G$_d$) One considers the propositions that one takes to express the fundamental facts (e.g., *all electrons have negative charge*) and it seems based upon one's understanding of these facts that they are only about particular objects.

(AP1$_-$G$_r$) One attempts to imagine the sort of fundamental states of affairs that would involve the whole world and comes up empty.

(AP1$_-$G$_d$) One attempts to consider the sort of fundamental facts that would be about the whole world and comes up empty.

(AP1$_T$T$_d$) One attempts to consider the sort of fundamental facts that would be about the whole world and is left with no feeling of understanding.

Every single version of the underlying intuition is problematic. Given that monism has independent support, the use of (AP1$_-$G$_r$) and (AP1$_-$G$_d$) will simply result in an argument

[218] Here 'G_r' means General and *de re*, 'G_d' means General and *de dicto*', etc. Note that a *de re* Theoretical intuition for (AP1$_T$) would simply amount to one of the other formulations, so I have not bothered to include it. I have not included particular formulations because they would not be interestingly different from the general ones.

from lack of imagination.[219] ($AP1_{+}$ G_r) is problematic because modern physics tells us that the fundamental facts are incredibly hard to visualize, which renders any such *de re* intuition unreliable unless, perhaps it is the intuition of a physicist.[220] ($AP1\text{-}G_d$ +) is problematic for similar reasons. Much of physical theory suggests that particles are best understood as local excitations in the fundamental physical fields, in which case the fundamental physical facts *would* concern the whole world in an important sense.[221] We need not endorse this interpretation of physical theory; we need only note that ruling this sort of interpretation out will require significant technical considerations and that the intuitive reaction in ($AP1\text{-}G_d$ +) is obviously incapable of doing the necessary work. Finally, ($AP1_TT_d$) seems to be poor evidence because it is defeated by the simple observation that the most fundamental facts might impossible for us to understand. Again, one can cite the highly technical and bewildering nature of fundamental physics as evidence.

[219] A caveat: in occasions where there is no independently supported competitor theory, such negative intuitions might be used to defeat *weak challenges* to a popular theory that already has support. Here a *weak challenge* should be understood as something to the effect of "you haven't fully justified your claim that *p*" without the accompaniment of evidence against *p*. In these contexts, an intuition of this sort can be used to defeat the demand that *p* be justified by motivating a demand for an explanation of how *p* could be wrong. However, this is clearly not the case for monism.

[220] As Ladyman and Ross put it, "There are not, nor were there ever, any 'classical hydrogen atoms'. At the same time that physicists came to believe in protons, they also became aware that the laws of classical mechanics could not apply to electrons orbiting them. Indeed the notion of an electronic *orbit* has about as much relation to the common-sense notion of an orbit as the mathematical notion of compactness has to the everyday notion of compactness, which is to say hardly any." (2007, 20)

[221] For instance, Ladyman and Ross note that since "quantum field theories" the state of a particular system "may be a superposition of different definite particle number states" (i.e., there is no determinate number of particles in these states) and that "for a given state of the field (how many particles there are) is dependent on the frame of reference adopted", and conclude that "particles seem to lose their reality in the field theoretic approach" (2007, 139). One standard view is that "elementary particles are hypostatizations of sets of quantities that are invariant under the symmetry groups of particle physics" (2007, 147). They go on to note that there are interpretations of QFT "in terms of 'quanta' which are excitations of the field that may be aggregated like particles (we can say there is a state with so many quanta)" but that these quanta "cannot be enumerated (we cannot say this is the first, this is the second, and so on)" (2007, 139). An (extremely rough) example of a fundamental fact on this reading of physical theory would be *there is an excitation of field f of magnitude m at coordinates x,y,…* (Though note that this would not be in line with the 'ontic structural realism' endorsed by Ladyman and Ross.)

Fortunately for Sider, his *real* argument against monism is that it conflicts with basic principles in modal metaphysics having to do with modal recombination and intrinsicality (2007, 2008). Indeed, a cursory survey of the literature on monism reveals that the primary theoretical disputes are driven by highly technical concerns, rather than by anything like (AP1).[222] Thus (AP1) can be abandoned without significant effect upon the dispute over monism. It is doubtful that Sider meant this initial appeal to intuition to be anything more than a stage-setting device.[223] This is a fairly common practice. Cian Dorr manages to sum up the point nicely:

> Often, saying 'Intuitively, P' is no more than a device for committing oneself to P while signaling that one is not going to provide any further arguments for this claim. In this use, 'intuitively … ' is more or less interchangeable with 'it seems to me that … '. There is a pure and chilly way of writing philosophy in which premises and conclusions are baldly asserted. But it's hard to write like this without seeming to bully one's readers; one can make things a bit gentler and more human by occasionally inserting qualifiers like 'it seems that'. It would be absurd to accuse someone who frequently gave in to this stylistic temptation of following a bankrupt methodology that presupposes the erroneous claim

[222] For instance, the arguments given in Bailey 2011; Bohn 2012; Calosi 2014; Cameron 2010; Schaffer 2010a,c; Sider 2007, 2008; Skiles 2009; and Trogdon 2009 all center upon highly technical formal considerations regarding the relation of monism and pluralism to plural logic, emergence, intrinsicality, grounding relations, modal recombination principles and quantum mechanics. However, there is a related intuition about fundamentality that does seem to structure this debate: i.e., that reality must have a fundamental level. Matteo Morganti criticizes both the priority pluralist and priority monist for assuming that there is not an infinite regress by arguing against this assumption (2009). Ross Cameron examines this intuition and argues that while it is difficult to independently justify, its assumption has been methodologically beneficial (2008). Morganti states that Cameron's arguments are "unconvincing" because "there may be good reasons for endorsing 'metaphysical infinitism'" (2009, 271). However, Cameron's point is only that foundationalism is reasonable due to its usefulness. If someone presents a viable and fleshed out infinitist theory as an alternative to these theories, then this would be worthy of consideration. But the mere *possibility* of an alternative theory has no real effect on current theoretical space. Thus in this case intuitions have not been used to decide between theories.

[223] Everything Sider says throughout the rest of the paper suggests that this was merely a framing device for his real argument. For instance, he states that he takes "monism seriously enough to give arguments against it", and that while he "could just leave the argument there," he prefers "not to insist flatfootedly on premisses about the nature of fundamental properties" (2007a, 2-3). Indeed, Cappelen draws the same conclusion regarding this passage when he states that Sider's use of the term 'intuitively' "can naturally be construed as claims about what we are pre-theoretically committed to" and that these claims therefore "serve a hedging function, indicating that Sider does not take himself, at this stage, to have established that it is wrong to take fundamental facts to be facts about the whole world" (2011, 71).

> that things generally are as they seem. But the sprinkling of 'intuitively's and 'counterintuitive's around a typical paper in metaphysics is in most cases not significantly different from this. It may be bad style, but it is not bad methodology, or any methodology at all, unless arguing from premises to conclusions counts as a methodology. (2010)

Thus we will need some other way of determining whether metaphysical disputes rest upon appeals to intuition. First, we will need some other way of identifying whether an argument rests on intuition aside from the occurrence of terms like 'seem' or 'intuitively'. Second, we will need some way of identifying the importance of the intuition in question. Rather than looking for superficial 'intuition talk', we shall instead examine the structure of philosopher's arguments to see if they rest upon appeals to intuition. If a philosopher's argument ultimately rests in part upon a claim that seems incapable of being backed by any further argument, then the strength of her argument must rest in part upon an intuition. Otherwise it would be subject to defeat simply by having its premise questioned. Since our concern from a methodological standpoint is whether the field *as a whole* employs intuitions inconsistently as evidence, our primary evidence must be broad and cross-textual.[224]

When we combine these considerations, we arrive at the following hermeneutic strategy. Step 1: Find metaphysical disputes that have been subject to intractable disagreement where this intractability cannot be reasonably attributed to indeterminacy in the non-intuitive evidence for the theories in question. (That is, where it is not the case

[224] This is possible even if every individual metaphysician uses intuitions consistently and endorses a background theory of intuition that justifies this usage. So long as individuals continue to use intuitions in ways that contradict with one another without any sort of general agreement about how these intuitions are to be evaluated, then metaphysics will be in a state of schismatic complacency. Therefore it is important to note that we are *not* concerned with accusations of intellectual dishonesty. That is, we are not concerned with showing that *specific* philosophers use intuitions in an inconsistent manner. While this would provide additional evidence for an affirmative answer to question (B), it is not necessary. What would be important about individual cases of inconsistency is that this would not be possible if the field employed collective evaluative standards concerning the use of intuitions as evidence.

that each theory is roughly equal in simplicity, explanatory power, etc.) Step 2: ascertain whether there any claims (explicit or implicit) that seem to be treated as evidence by one party that cannot be reasonably supported by anything other than intuition. Step 3: examine the evidential role that these claims play in explaining why there is disagreement between parties. Step 4: Repeat Steps 1-3 until one gathers a body of appeals to intuition that can be comparatively evaluated for consistency.

Consider Cappelen's criteria for determining whether an argument rests upon intuition:

> (***Rock***): "intuitive judgments serve as a kind of rock bottom justificatory point in philosophical argumentation. *Intuitive judgments justify, but they need no justification*." (2011, 112)
>
> (***Rock Detection***): If in a context *C*, evidence and arguments are given for *p* and those arguments evidence plays a significant argumentative role in *C*, that is evidence that *p* is not Rock relative to *C*. The existence of a context *C'* in which you can get away with claiming *p* without providing argument or other evidence is irrelevant to *p*'s status in *C*, unless there is some clear evidence in *C* that what goes on in *C'* matters for *p*'s status in *C*. (2011, 121)

We shall adopt the following amended versions of Cappelen's principles:[225]

[225] Cappelen is primarily targeting a strong positive meta-philosophical thesis which claims that intuitions are a special sort of evidence because they are "*Based solely on conceptual competence*" (2011, 113). Thus Cappelen wishes to show that philosophers do not make claims that are backed by judgments with this status. He ultimately argues that major philosophical arguments do not exhibit anything like (*Rock*) because the cases he considers do not exhibit very specific characteristics. As we stated above in §1.1.2 and §2.1.1, the flaw in Cappelen's approach is that he has merely established a negative claim: i.e., that if philosophers appeal to something in these cases, they cannot be intuitions as he conceives of them. In other words, he has put the cart before the horse. What he *needs* to establish is that all of philosophical practice can be accounted for with arguments and other forms of theoretical consideration, without the need to treat appeals to intuition as having evidential status. The problem is that his argument does not rule out alternative accounts of intuition (for example, he would have a much harder time ruling out Chudnoff's account). Neither has he ruled out the possibility that philosophers have simply been mistaken about their intuitions and that the cases he considers are thus completely undermined. However, this is a bit of a moot point. As we also pointed out in §2.1.1, Cappelen simply considers the wrong cases. For instance, Cappelen would have an extremely difficult time explaining why claims such as "two objects cannot be exactly located at the same place at the same time" are justified through anything other than intuition.

(***Rock****): Certain philosophical claims are treated as though they have rock-bottom evidential status: they are used as evidence without support by independent argument.

(***Rock* Detection***): If in a philosophical dispute *D*, (1) denying *p* would render the motivation for *D* inscrutable because there is agreement on all other forms of evidence relevant to *D*, and (2) there have been no substantial arguments offered in favor of *p* that do not already depend on the assumption that *p* or something like *p* is plausible, then this is evidence that *p* is Rock* relative to *D*.

If we can show that a claim meets *Rock** and that the claim appears to be backed by some form of intuition, then this will show that the dispute in which the claim occurs is intractable because the disputants disagree about the evidential status of this intuition.

2.4. The Cases

We shall now consider some examples in which metaphysical disputes depend upon intuition. Following our hermeneutic strategy, we will focus on the following four debates: (1) *Necessitism vs. Contingentism*, (2) *Nominalism vs. Platonism*, (3) *Presentism vs. Eternalism*, and (4) various problems in mereology. There are four major reasons for this selection. First, these are four of the biggest metaphysical disputes, so no one can complain that we have selected obscure examples. Second, the literature on these disputes is still active and they still generate significant disagreement. Third, these disputes have also generated higher-order methodological disagreement, which allows us to examine explicit textual claims about the methodological role of intuitions. Fourth, and most importantly, considerable energy has been put into providing detailed formal models of the different positions in each dispute. In each case there is generally a strong consensus about which views are simpler, elegant, and have more expressive power. This makes it much easier to show that the source of continued disagreement in these disputes boils down to conflicting treatment of intuitions. We will devote an entire section to the first case (i.e., necessitism vs. contingentism), as this will also allow us to draw out some general points about how intuitions function as evidence. The remaining two sections will demonstrate that appeals to intuition are used by different parties in two important ways. First, they are used to provide evidential weight to extremely abstract metaphysical principles. Second, they are used to determine the nature of the claims that belong to the data that theories are supposed to predict or explain. In particular, they are used to insist that the data be interpreted in a specific way, where this insistence cannot be easily justified abductively though purely semantic considerations.

2.4.1. Necessitism vs. Contingentism

In *Modal Logic as Metaphysics* (MLAM), Timothy Williamson defines *Necessitism* and *Contingentism* as follows:

> Call the proposition that it is necessary what there is *necessitism*, and its negation *contingentism*. In slightly less compressed form, necessitism says that necessarily everything is necessarily something; still more long-windedly: it is necessary that everything is such that it is necessary that something is identical with it. In a slogan: ontology is necessary. Contingentism denies that necessarily everything is necessarily something. In a slogan: ontology is contingent. (2013a, 2)

In accordance with our first hermeneutic step, we should ask whether the dispute between the necessitist and contingentist can be explained in terms of disagreement about or simple indeterminacy of the non-intuitive evidence. What makes Williamson's defense of necessitism so illustrative is the fact that a large part of his defense is based on an explicit endorsement of a methodological picture on which metaphysical theories are to be evaluated by extremely specific and rigorous standards.[226] Williamson describes his methodology as follows:

> the methodology of this book is akin to that of a natural science. Both are abductive. Very general theories are formulated in a formal notation that facilitates complex rigorous deductions of their consequences. The theories are judged partly on their strength, simplicity, and elegance, partly on the fit between their consequences and what is independently known. The fit has at least two dimensions. Theories should not entail anything we are in a position to falsify, since then they are false. Equally, the more they entail of what we are in a position to verify independently, the better. 'Entail' here means by the standards of the theory in question, rather than by the correct standards, since we are trying to find out what the latter are: logic here is no mere background framework but the very thing at issue. (2013a, 423-4)

Following Williamson, we can identify the non-intuitive evidence in this case as consisting of two parts. The first part concerns formal aspects of the theory in question

[226] The "Methodological Afterword" to *Modal Logic as Metaphysics* is a straightforward continuation of the picture of philosophical methodology presented in the afterword to *The Philosophy of Philosophy*.

(here these are different systems of modal logic), such as simplicity, elegance, and expressive power. The second part concerns how well these theories fit the data.

Williamson argues that the general aim of the metaphysics of modality is to establish the correct system of modal logic according to a model theory that assumes a specific sort of interpretation that captures the core structural claims of metaphysical modality (2013a, 92-6). The core structural claims of metaphysical modality are "*metaphysically universal*": universal generalizations of sentences where all non-logical constants are replaced by variables, which come out true under the intended metaphysical interpretation.[227] For example, $\forall X(\Box X \supset \Box\Box X)$ will count as a metaphysically universal sentence if we accept that the notion of metaphysical modality must be captured by a propositional modal logic that is at least as strong as S4. Williamson argues that the aim of a theory of metaphysical modality is to establish which modal logic uniquely captures the semantic properties relevant to the intended model structure:

[227] The intended metaphysical interpretation is captured by a model structure that satisfies the metaphysically universal sentences, i.e., a structure that makes the right sentences about what is possible and necessary true. Thus a sentence "*A* is *metaphysically universal* if and only if the universal generalization of *A* is true on its intended interpretation, which treats the truth-functional operators as usual and makes □ express metaphysical necessity" (2013a, 93).In other words, Williamson holds that the truth of modal sentences is explanatorily prior to the model theory: "The link between metaphysical universality and an intended model structure comes later, as a necessary condition on the latter in terms of the former, in order to connect the purely mathematical model theory of Kripke structures for modal logic with the philosophically important standard of metaphysical universality" (2014d, 474). Thus Williamson explains that "The best we can hope for is a unique intended model structure ⟨W, R, *w*⟩, where in some sense to be specified W is the set of genuine worlds, R is the relation of genuine relative possibility, and *w* is the genuine actual world. The formulas valid on such an intended model structure should be exactly the metaphysically universal ones" (2013a, 95 [my underlining]). That is, we should evaluate the logic without regard to the particular nature of possible worlds or with regard to modal space, but rather in terms of what sentences should come out true. The exact nature of the domain of the model is irrelevant: "we are not concerned to specify a particular intended interpretation of the language, one that distinguishes 'tiger' from 'robot' in meaning" (2013a, 92-3). John Divers complains that this treatment will not be nearly as metaphysically illuminating as David Lewis' modal realism, which gives us substantial metaphysical principles regarding the structure of the intended models, such as principles of recombination and Humeanism (2014, 732). In response, Williamson simply points out that his treatment of necessitism *does* have many important consequences, that these results are compatible with the fact that other questions are to be answered through non-logical means, and that "answering all or even most questions in modal metaphysics is not a reasonable aim for any book" (2014d, 479).

> As system S of propositional modal logic is *sound for metaphysical universality* if and only if every theorem of S is metaphysically universal; S is *complete for metaphysical universality* if and only if every metaphysically universal formula of the language is a theorem of S. Exactly one system, individuated by the set of its theorems, is sound and complete for metaphysical universality. Its theorems are just the metaphysically universal formulas. Call that system MU. In that sense, MU is the uniquely correct logic of metaphysical modality for our propositional modal language. (2013a, 95).

Williamson argues for necessitism on the first set of considerations regarding the simplicity, elegance, and expressive power of necessitist vs. contingentist modal logical systems.[228] There can be no real dispute about this part of the evidence, as Williamson's formal results speak for themselves. Williamson summarizes his findings as follows:

> Overall, necessitism beats contingentism as a metaphysically interpreted higher-order modal logic. The comparison between them is fundamentally abductive, on grounds such as strength, non-causal explanatory power (bringing data under illuminating generalizations), simplicity, elegance, and consistency with the evidence. Thus it is made on similar general grounds to those used for comparing theories in other branches of science. The methodology for choosing first principles of mathematics is similarly abductive. Logic and metaphysics are much less exceptional than they look. (2014c, 716)

Thus if there *is* any dispute about the non-intuitive evidence, it must due to a dispute about something other than the simplicity, elegance or expressive power of the modal languages in question. In other words, it must boil down to a dispute about how well the different modal systems fit the data. The data, in this case, is a set of modal sentences that are judged as true. If the logical system in question implies something that conflicts with this set, then the theory that that logic system is MU is disconfirmed.

The problem is that the disputes over what counts as part of the data ultimately boils down to intuitions. Thus any disagreement *must* be due to differences in how

[228] This is the goal of chapters 4, 5, 6 and 7 of 3013a. Williamson cites Wittgenstein and Frank Ramsey as endorsing necessitism (2013a, 1-2). He also considers other arguments for necessitism aside from considerations regarding logical systems, but argues that they fail for various reasons (2013a, 5-9).

different theorists treat intuitions as evidence. For instance, in her review of MLAM, Meghan Sullivan contrasts Williamson's methodological with what she takes to be the orthodox methodology:

> The prevailing approach to metaphysics is best described as an *intuition-driven methodology*. Theories are stated as clearly as possible in any language (typically a combination of English and first-order logic). Their entailments are identified, and intuitions are cited as evidence for or against particular claims of the theories. The goal is to identify the theory that carries the balance of intuitive support. (2014, 734)

Sullivan then argues that according to this intuition-driven methodology, "necessitists fight an uphill battle" (2014, 734). She reasons as follows:

> The theory [i.e., necessitism] vindicates some intuitions—for example, that we can refer to merely possible objects.[2] But it predicts that one pervasive intuition is wildly mistaken—namely, that objects like the life-sized butter statue of Elvis Presley at the 1997 Iowa State Fair could have failed to exist. When the costs have been tallied, the strength of intuitions about contingent existence may very well be enough to swamp any other considerations. Indeed, the intuition-driven methodology explains why the incredulous stare has long been taken as a serious objection to other modal theories. (2014, 734-5)

Sullivan thus argues *explicitly* against Williamson's methodology by arguing that intuitions should be taken as evidence.[229] Phillip Bricker provides a similar objection:

[229] However, it is best to simply ignore Sullivan's primary example. When formulating her specific objection to Williamson's methodology, Sullivan selects an extremely unfortunate putative counter-example: the claim that every object is physical (2014, 739-40). This example seems to be totally at odds with Sullivan's initial objection. As Williamson himself points out, Sullivan's "alleged arbitrary exclusion of evidence turns out not to involve anything that could naturally be described as an 'intuition'… Sullivan does not say that her physicalist intuits, or claims to intuit, his physicalist principle. Rather, she has him accept physicalism on broadly abductive theoretical grounds" (2014d, 751). However, it would have been far better for Sullivan if she had stuck to her original counter-examples such as sentences like "the life-sized butter statue of Elvis Presley at the 1997 Iowa State Fair could have failed to exist" above. First, Williamson himself provides several independent reasons for the claim that scientific practice assumes necessitism (2014d, 753-7). Second, as Williamson also points out, such a physicalist theory will almost certainly *fail* on abductive grounds precisely because it denies the existence of non-physical objects: "given that mathematical objects are non-physical, (HP) [i.e., physicalism] is in danger of being inconsistent with physical theories, through excluding the mathematical objects they assume" (2014d, 757). As a final straw, we can also add to this pile of worries the fact that physicalists are often *skeptics* about metaphysical modality (e.g., Quine) precisely because the ontology required to ground modal claims can only avoid conflict with physicalism if it takes the

> Most contemporary metaphysicians approach debates in ontology rather differently than Williamson. Even if they allow some role to the sort of logical and semantical considerations that dominate Williamson's discussion, it is their commitment to fundamental metaphysical theses that tends to play the decisive role. For example, necessitism, with its positing of non-concrete possible objects, might be rejected on the grounds that it violates a general principle that the modal supervenes on the non-modal … For Williamson, claims such as that the modal supervenes on the nonmodal belong to speculative metaphysics. Such metaphysics, however legitimate as an enterprise, is too uncertain to play a role in the debate between necessitism and contingentism. … It is this shift away from speculative metaphysics and towards the firmer ground of logic and semantics that sets Williamson's methodology apart from the practice of most contemporary metaphysicians. (2014, 724-5 [my underlining])

What makes such fundamental metaphysical theses count as evidence? In particular, why should the principle that the modal supervenes upon the non-modal count as an important piece of evidence against necessitism? Certainly there is no empirical evidence for the claim. It would be equally ridiculous to attempt to justify it through an appeal to common sense.[230] It is just as difficult to come up with an abductive justification for this thesis. In order to establish this one would already need to have established that theories that assume this thesis are more successful than their counterparts. But as Bricker himself puts it, this thesis is supposed to play a "decisive role" in theory selection. In order to play this role it would need to be justified independently of the success of the theories that presuppose it. For essentially the same reasons it is equally unclear how the thesis could even receive a form of pragmatic justification. By process of elimination, the only thing that *could* justify treating this principle as evidence is an appeal to intuition. Thus if

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extravagant form of Lewisian modal realism (otherwise the possibilia will be non-physical). These problems demonstrate that if the real disagreement is about abductive criteria, it is a disagreement that Sullivan will inevitably lose.

[230] It is extremely doubtful that the average person would respond to the question "Does the modal supervene upon the non-modal?" with anything but sheer perplexity. Indeed, it is doubtful that the average person is even familiar with the terms 'modal' and 'supervene'. Nor does there appear to be any hope that the claim can be paraphrased into ordinary English. As Williamson himself spends some time pointing out, the claim *also* appears to be exceedingly unclear (2013a, 380-391). Thus

Bricker is correct in his claim that most metaphysicians treat claims like these as decisive pieces of evidence, it follows that metaphysical methodology must depend heavily upon appeals to intuition.

While it is sometimes dangers to speculate about such sociological matters without empirical evidence, it should be fairly uncontroversial to say that necessitism is the minority position. If we take Sullivan and Bricker's objections seriously we have no choice but to conclude that a significant part of the disagreement between necessitists and contingentists must ultimately boil down to a methodological dispute about the evidential status of intuitions. However, let us put aside the fact that Williamson's critics explicitly criticize his methodology for not including intuitions as evidence and instead focus on how the data can vary.

When the data and the theory come into conflict, one will have to either treat the theory itself as disconfirmed or otherwise resolve the conflict by revising the data. Sometimes when the theory itself is already highly confirmed, the most rational course of action is to take the *data* as flawed. Williamson himself describes the process as follows:

> As in natural science, the abductive method does not require us to be infallible about the data. We may easily misjudge particular modal claims, just as we may easily make errors of observation and measurement. We must be careful not to dismiss a true theory on the basis of false 'data'. Every science has its own ways of checking data. When a proposed law of modal logic is rejected (on its metaphysical interpretation), the grounds are typically putative counterexamples consisting of simple modal claims, available for everyone to assess. Moreover, the community is not usually satisfied with a single example, but checks whether it instantiates a general recipe for producing more falsifications of the proposed law, in case the original example had misleading incidental features. That is reminiscent of the need for experiments to be repeatable. Naturally, not even these controls guarantee that the community will never accept false 'data', and consequently reject a true theory. But in the long run such misjudgments tend to generate a proliferation of anomalies, spreading outwards from the original error. Once a good explanation is offered of how such a mistake

> could be made, and more advantages in other respects of the rejected theory over its competitors are brought to attention, the community eventually revises its view. (2013a, 426)

Whether it is rational to give up the theory or to revise one's data will not only depend upon the extent of the conflict between the theory and data, but also on how reliable one takes the methods that established the data in question to be.[231]

There are two relevant questions we might ask about the data relevant to evaluating systems of modal logic. (1) What method should we use to determine what sentences fall within the set? (2) What evidential weight should be given to the data vs. other considerations? The two questions are interrelated. In regard to our particular dispute, these two questions can only be answered given the appropriate background semantic and metasemantic theories.

As Williamson points out, the dispute between necessitism and contingentism is intimately related to considerations regarding the Barcan Formula and its converse:

(**BF**): $\Diamond\exists\mathbf{x}\varphi \rightarrow \exists\mathbf{x}\Diamond\varphi$ (**CBF**): $\exists\mathbf{x}\Diamond\varphi \rightarrow \Diamond\exists\mathbf{x}\varphi$ [232]

(BF) says that if there possibly exists something with a certain property, then there exists something that can have that property. (CBF) says that if there exists something that can have a certain property, then there possibly exists something with that property. Necessitists endorse these principles, while contingentists must deny them. Williamson explains the set of judgments that seem to contradict necessitism as follows:

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[231] This further illustrates the importance of treating intuitions as instruments (see §2.1.4.3 above). The controls that Williamson mentions, such as replicability, are put in place as a check to things like instrument malfunction. But there is also a more direct control: evaluate the instruments *themselves*. In particular, we might question whether our supposedly modal intuitions are giving us a false positive (or negative).

[232] I am roughly using the formulation in Williamson 2013a, 33. I have used Greek letters for formulas and bold un-italicized letters for variables to make more explicit the fact that these are axiom-schemas in a metalanguage.

> Things could have been otherwise. It is contingent how they are. Although the coin came up heads, it could have come up tails. Is it also contingent *what* things there are? 'Yes' seems the obvious answer. The universe could have evolved differently, so that there was never any money, or even any non-pecuniary physical object shaped like this coin. In those circumstances, not only would this coin not have been a coin, it would not have been anything at all; it simply would not have been. Thus there is actually something that could have been nothing. Conversely, there could have been something that is actually nothing, such as a coin of a design never actually produced. There could have been fewer things than here actually are; perhaps there could have been nothing whatsoever. Equally, there could have been more things than there actually are. Even keeping fixed how many things there are, the number could have been made up by different things. Not only is it contingent what general kinds of thing have instances, it is contingent what particular things there are to be instances of any kind at all. Or so it seems. (2013a, 1)

What is critical to note for our purposes is that if intuitions play any role at all in the controversy between necessitism and contingentism, it must be due to disputes about the intuitiveness or counter-intuitiveness of (BF) and (CBF). To illustrate this, consider what Williamson presents as the contingentists' "most straightforward argument" against (CBF) (2013a, 38). The contigentists substitute '$\sim\exists y(x=y)$' for 'φ' in the (CBF) schema above to yield:

(A-CBF): $\exists x\Diamond\sim\exists y(x=y) \supset \Diamond\exists x\sim\exists y(x=y)$ (2013a, 38)

The consequent is necessarily false: it says that there is a possible world in which there is something that is not identical to anything *including itself*. Hence the dispute boils down to whether the following claim is also necessarily false: [233]

(**AP2**): $\exists x\Diamond\sim\exists y(x=y)$

Contingentists hold that (AP2) is made true by ordinary material objects. As Williamson

[233] As Williamson notes, the necessitist is happy to apply *modus tollens* to (A-CBF) in order to yield '$\forall x\Box\exists y(x=y)$', which is "just the necessitist claim that everything is necessarily something" (2013a, 38). Of course, one can only do this if one has already assumed necessitism. Thus the necessitists' evidence for the necessary falsehood the formulas ⌜ $\mathbf{\exists x\Diamond\sim\exists y(x=y)}$ ⌝ must depend upon their general evidence for (CBF) from formal results in modal logic.

puts it, "typical contingentists" hold that "any ordinary material object verifies the antecedent: it is something that could have been nothing" (2013a, 38). In other words, contingentists hold that there could be things that do not exist, and that things that actually do exist might not have existed, and thus that necessitism is false.[234] The most plausible examples of the underlying contingentist intuitions run as follows:

(AP2 P_r) One first considers an actual object (e.g., a table) and then imagines a world in which there is nothing that looks like that object (e.g., a world in which the table was never constructed). In the imagined scenario, one is not aware of the object: it seems to be absent.

(AP2 P_d) One first considers an actual table and considers the proposition *this table would not exist if it had not been constructed*. It seems based upon one's understanding of the terms in question that the proposition is true (e.g., it seems as though it would be wrong to say that *the table could exist without being constructed*.)

Given that something like (AP2) must be true in order to claim that necessitism fails to meet the appropriate data, there is strong evidence that (AP2 P_r) and/or (AP2 P_d) meet Rock*. Moreover, the only way to make sense of the dispute between Williamson and contingentists like Bricker and Sullivan is in terms of a dispute about how judgments like (AP2 P_r) and (AP2 P_d) are to be weighed evidentially against formal results establishing the plausibility of (CBF). Thus the dispute between necessitists and contingentists must ultimately boil down to an as yet unsettled methodological dispute about the nature of intuitions.

It might be tempting to adopt a Moorean strategy in order to avoid this conclusion. As Sullivan and Bricker both point out, many statements such as 'the table might not have existed' appear to belong to common sense. Thus a Moorean might argue that Williamson has simply ignored his own advice by ignoring a large portion of the

[234] Formally, this translates as something along the lines of $\ulcorner \Diamond(\sim\exists x\varphi \wedge \Diamond\exists x\varphi) \urcorner$ and $\ulcorner \Diamond(\exists x\varphi \wedge \Diamond\sim\exists x\varphi) \urcorner$, which obviously contradict (BF) and (CBF) respectively.

data. However, the Moorean's data will consist of sentences of English, while the relevant theoretical claims consist of formulas belonging to formal logical theories. Some process of interpretation or translation is needed in order to put these two sorts of claims in contact with one another.[235] In order for the data to count against necessitism one must regiment the sentence 'the table might not have existed' in such a way that it can be used to infer the modal formula '$\exists x \Diamond \sim \exists y(x=y)$', where the existential quantifiers are totally unrestricted.[236] Williamson has a straightforward way of blocking this inference: when the average person makes statements like 'the table might not have existed' she has tacitly restricted her quantifiers to concrete objects, whereas in the scenario in question the table will be non-concrete (2013a, 14-21).[237] The problem for the Moorean is that it is *not* a matter of common sense that the folk are using unrestricted quantifiers. This is an interpretive issue that must be settled by theoretical considerations in semantics. In contrast, this becomes far less of an issue if (AP2 P_r) and (AP2 P_d) are treated as a special form of evidence *qua* intuitions, since the philosophers who have these intuitions will intend their quantifiers to be unrestricted. Given the other problems with Mooreanism (see §2.1.5), the most straightforward way to make sense of the contingentists' objections is as an appeal to (AP2 P_r) and (AP2 P_d) as intuitions.

[235] Recall our discussion of the process of regimentation in Quinean ontology above (see §2.2.1.2).

[236] Williamson is concerned with evaluating necessitism and contingentism as the theses are formulated with an absolutely unrestricted version of the existential quantifier, because this is the only version of the quantifier relevant to ontology (2013a, 14-21).

[237] As Bricker pointed out in the quote above, one might object to this on the grounds of general metaphysical principles regarding the nature of the modal or the abstract/concrete distinction. But as we pointed out in response to Bricker, these general principles can only really be justified by an appeal to intuition. Thus appealing to such principles in order to block Williamson's strategy would defeat the entire purpose of the Moorean response, in that it would once again ground the dispute firmly in a disagreement about the methodological status of intuition.

2.4.2. General Principles

Let us begin our discussion of the role of general principles in metaphysics with the dispute between Platonism and Nominalism. Platonists hold that things like numbers, properties and propositions exist and that they are abstract, i.e., they do not exist in space and time and do not have causal efficacy. Nominalists deny this. Of course, the real nature of the abstract/concrete distinction is itself a matter of some dispute due to odd cases (e.g., are space and time *themselves* concrete?).[238] Thus the first way in which intuitions influence this dispute is simply that they appear to be needed in order to draw the abstract/concrete distinction in the first place. One popular approach is to simply take the abstract/concrete distinction as the most fundamental and jointly-exhaustive categorical distinction in one's ontology (e.g., Hoffman & Rosenkrantz 2003). However, there are also those who oppose such a strategy. For instance, Williamson rejects the identification of the abstract with the non-concrete:

> It is tempting to paraphrase that conclusion [i.e., that things like sticks and stones are not essentially concrete] thus: given necessitism, something concrete could have been abstract and something abstract could have been concrete. However, that is to treat 'non-concrete' and 'abstract' as synonyms. They are not. 'Abstract' is not purely negative in meaning; it has its own positive paradigms, such as numbers and directions. Nor are 'concrete' and 'non-abstract' synonyms. 'Concrete' is not purely negative in meaning either; it has its own positive paradigms, such as sticks and stones. It is a fallacy to treat 'abstract' and 'concrete' as contradictories, although they may be contraries.[9] Perhaps some things are neither, even if nothing can be both. (2013a, 7)[239]

[238] See Szabó 2003 and Hoffman & Rosenkrantz 2003 for a summary of both positions as well as the abstract/concrete distinction.

[239] However, Williamson has also not given us any particular reason to *deny* that the terms are contradictories, and this denial appears to be unnecessary even for his own position. While Williamson is right that to infer from our typical use of 'abstract' and 'concrete' that the terms are contradictories would be a fallacy, it is not clear that anyone has ever made this inference. It would also be a fallacy to infer that the terms are *not* contradictories because they have positive paradigm examples. Metaphysicians are free to treat the terms *stipulatively* as though they are contradictories, since they are terms of art. Williamson's worry appears to be the strangeness of the claim that an actual concrete stick

The dispute between Platonism and Nominalism belongs to Quinean ontology (see §2.2.1.1). That is, it is a dispute over what types of things exist. The Platonist argues that abstract things exist, while the Nominalist argues that they do not. The dispute thus only makes sense in terms of a dispute about general ontological principles; i.e., principles concerning what exists. The first problem with the dispute is the fact that formulating any general ontological principles faces formal difficulties. If we put aside our particular background knowledge of ontology, we would expect an ontological principle to take the same general form as any philosophical principle; i.e., as a (likely necessitated) material biconditional. Thus, *prima facie*, we would expect the nominalist's ontological principle to look like this:

(**AP3**) Necessarily, something exists iff it is concrete.

However, according to Quinean ontology, existence claims are captured by logical sentences of the form '$\exists x(Fx)$', where F is some individuating property. The problem is that it is not possible to give this sentence a Quinean regimentation. Consider the following attempt:

(Nominalism$_{\exists}$) $\exists x(x) \equiv Cx$ [240]

This formulation fails for one simple reason: '$\exists x(x)$' is not syntactically well-formed. Given standard the standard treatment of the existential quantifier it cannot function as a declarative sentence or have a truth value: it is simply nonsense. It is a mistake to read

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could be abstract. However, instead of avoiding this result by rejecting the claim that the terms are contradictories, Williamson could just as easily point out that one should not read incidental features of the paradigm cases as being marks of the general distinction (i.e., the abstract sticks and stones are a different species of abstract object than numbers).

[240] Note that we have left out the modal operator for simplicity. The issues we shall consider arise solely for the formula as first-order logic, and thus carry over to modal predicate logic without any interesting complications.

'$\exists x(x)$' simply as 'there exists an x'. Rather, the construction '$\exists x(x)$' must be read as 'there exists an x, such that x'. The problem is that 'x' functions syntactically as a name, not as a statement. The incoherence of '$\exists x(x)$' is thus best illustrated by simple existential elimination. If one eliminates the quantifier phrase '$\exists x$' and replaces 'x' with the name 'Bob', one simply gets 'Bob'. Just as 'Bob' is not a declarative sentence, neither is '$\exists x(x)$'.

Even if it were possible to put this first issue aside (which it isn't), (Nominalism$_\exists$) faces another serious problem. The problem is that the first instance of 'x' in '$\exists x(x)$' is bound, while the second instance in 'Cx' is unbound. Thus even if we ignored the fact that '$\exists x(x)$' is nonsense, it would still be the case that the two instances of the variable 'x' could only co-refer by sheer accident (and this is on the highly tenuous assumption that the unbound 'x' in 'Cx' can refer to anything at all).[241] What this shows is that one cannot capture a general ontological principle using only the first-order existential quantifier. One alternative is to draw a distinction between 'heavyweight' and 'lightweight' quantifiers, where only the former are supposed to carry ontological commitment. Thus if we designate the heavyweight quantifiers as '$\forall_h$' and '$\exists_h$' and the lightweight quantifiers as '$\forall_l$' and '$\exists_l$', we can reformulate (Nominalism) as follows:

(Nominalism$_h$) $\quad \forall_l x(\exists_h(y{=}x) \equiv Cx)$

The coherence of this strategy will depend upon an explanation of the distinction between lightweight and heavyweight quantifiers. A common assumption is that the quantifier

[241] The mere fact that the marks of ink on the paper have the same sort of physical structure is irrelevant: the second 'x' might as well be 'y'. Thus even if we were to grant that '$\exists x(x)$' is coherent and simply said that x exists (which would essentially require a total syntactic redefinition of '$\exists x$' in some deviant version of first-order logic), '$\exists x(x) \equiv Cx$' would at best say something to the effect of 'something x exists iff some (potentially totally distinct) thing y is concrete'. Note that '$\exists x(Cx)$' also clearly will not do, as this only says that there is *something* that is concrete.

relevant to ontology is the totally unrestricted One True Quantifier and that other quantifiers only restricted versions of this One True Quantifier.[242] But (Nominalism$_h$) would clearly be rendered trivial if '$\forall_l$' were simply a restricted version of '$\forall_h$'.

Thus (Nominalism$_h$) could only make sense on some 'heavyweight' version of quantifier variance wherein the quantifiers are given fundamentally different meanings and cannot be coherently combined to form an all-encompassing domain of entities.[243] In particular, the only position that is capable of making sense of something like (Nominalism$_h$) appears to be Sider's metametaphysics. Sider holds that the aim of metaphysics is to arrive at an accurate description of the fundamental structure of reality, such that the quantifier relevant to ontology only occurs in a specific fundamental language that only ranges over fundamental objects such as electrons, and that this quantifier is different from inferentially similar quantifiers employed in natural language (2011).[244] Thus Sider would thus likely describe (Nominalism$_h$) as a sentence that occurs

[242] E.g., Eklund 2009; Quine 1951a; Sider 2001b, xix-xxiv; van Inwagen 2009; Williamson 2003b; 2013a, 14-18. For instance, Matti Eklund objects the form of quantifier variance that he takes to undergird Carnap's historical position by arguing that the different quantifiers and referential terms in each 'framework' must ultimately be understood in terms of a model-theoretic treatment wherein all linguistic expressions within all frameworks must be interpreted using the same domain of entities and the same assignment functions (2009). Thus Eklund argues that Carnap's supposed pluralism is self-defeating because the frameworks that use the restricted quantifiers must acknowledge the existence of the objects posited by other frameworks in order to acknowledge that sentences in those other frameworks are true (2009, 145-7). Of course, the general problem with Eklund's argument is that he has simply overlooked the fact that a comparison between the semantics of two languages will inevitably involve semantic ascent to a comparative metalanguage that will have more expressive power than either object-language. The problem is that each theorist will inevitably choose a different metalanguage to describe her opponent's language given her first-order commitments.

[243] Note that (Nominalism$_H$) will be trivial on a pluralist view like the one proposed by Kris McDaniel. McDaniel holds that there *is* a single univocal meaning for the existential quantifier; it is merely that this should be understood as being *posterior* to the more privileged restricted quantifiers (2009). Thus if '$\exists_h$' is supposed to capture 'extantness'—"the kind of existence had by objects primarily characterized by spatiotemporal features" (McDaniel 2009, 296)—(Nominalism$_h$) will be trivially true; if it is supposed to capture 'subsistence'—"the kind of existence enjoyed by abstract objects such as numbers and propositions" (ibid)— it will be trivially false.

[244] Note that Sider appears to have changed his position from early works in which he appeared to assume

in a non-structural language that attempts to describe a general claim about what exists *fundamentally* by borrowing the quantifier of the fundamental language (2011, 112-124). Neither '$\forall_l$' nor '*C*' would be considered structural according to Sider (2011, 137-141). Because Sider's position takes metaphysics to concern highly technical matters like particle physics and to be almost entirely divorced from natural language semantics, it is not at all amenable to the use of intuitions as evidence.[245]

Alternatively, one might treat '$\forall_h$' and '$\exists_h$' as the 'objectual' or model-theoretic interpretation of the existential quantifier and '$\forall_l$' and '$\exists_l$' as the 'substitutional' or 'inferential role' interpretation of the quantifier. The most outspoken advocate of this approach in the recent literature is Thomas Hofweber (1999, 2005, 2007, 2009). Hofweber argues for a position that he claims is a version of Carnap's internal/external distinction wherein both quantifiers and singular terms have an internal 'inferential role' reading as well as an external 'referential' reading (2009, 286-7). The issue with this approach is that (1) substitutional quantification is not nearly as well understood as the

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that there is only One True Quantifier that occurs in natural language (e.g., Sider 2001b, xix-xxiv). Recall Eklund's arguments to this effect (see footnote 242 above). David Liebesman and Eklund (2007) use a similar strategy to show that Sider's notion of naturalness is at odds with his "Argument From Vagueness" for unrestricted composition and four-dimensionalism (Sider 2001b, 120-139). In response to this attack, Sider acknowledged that different quantifiers are *not* to be treated as differing in their domains, but are rather only to be compared in terms of translation functions (2009a,b). Thus Sider's argument against Hirsch's superficialist version of quantifier variance (see Hirsch 2011) is simply that Hirsch must presuppose that there is no structural or fundamental interpretation of the quantifier, which is, in and of itself, a substantial metaphysical position for which Hirsch has not provided evidence (2009b).

[245] Sider's position puts him in direct conflict with those who would use intuitions to claim that they "just know" that different quantificational claims express specific quantifiers. When describing the sort of evidence that will be relevant to his project Sider says that he embraces a "vaguely Quinean" view on which "metaphysics is continuous with science" (2011, 12). He goes on to advance a largely abductive methodology where considerations of simplicity, elegance and explanatory power factor prominently (2011, 12-15). Moreover, Sider also rejects what he calls the "magical-grasp picture of understanding" in which theories are evaluated through an "inward search for a mystical mental state of UNDERSTANDING" (2011, 9). As he puts it, "Philosophy is not just the building of theories on previously existing concepts. We also build new concepts, by building theories that use them" (ibid). In other words, we need to develop our theories and see how they work before we can get a real handle on the concepts they employ.

standard model-theoretic quantifier (e.g., Lycan 1979; van Inwagen 1981), and (2) even if such a notion *were* developed, the resulting system of free-logic would have significant abductive costs. Williamson puts the point as follows:

> An important typical feature of first principles of logic and mathematics is their extremely wide applicability. Any theory needs logic to draw out its consequences; very many need mathematics. Consequently, restricting first principles of logic or mathematics tends to have knock-on effects in any well-developed branch of science. Weakening logic or mathematics typically weakens the derivable consequences of a given scientific theory and so its abductive power. Thus the global abductive loss implicit in such a weakening over the totality of our theories promises to outweigh any local gains made with respect to a particular branch of inquiry. … Deviant logicians who propose rejecting a principle of classical logic typically reassure us that the classical principle will still hold under ordinary or favourable conditions (for instance, when applied to finite totalities or to precise expressions). However, the resultant need for inserting complicated restrictions into simple classical principles and invoking extra auxiliary assumptions that the given restricting conditions hold in the cases at hand itself constitutes a major loss of abductive power. Proponents of non-classical logics often ignore or grossly underestimate the abductive costs of their proposals. (2014d, 753)

Given the complications with using different quantifiers, the most semantically straightforward way to cash out the nominalist's claim is as follows:

(Nominalism$_{\forall}$) $\forall x(Cx)$

Thus (Nominalism$_{\forall}$) simply states that everything is concrete. However, there are several problems with this claim. The first problem is that by embracing it, the nominalist will either have to claim that sentences such as 'there is a prime number between two and four' are false, or else give these sentences a non-standard semantic treatment, in which case we are back to the same issues as before.[246] The second problem is simply that '$\forall x(Cx)$' is not obviously a principle regarding what exists. That is, it does not tell us the

[246] That is, the Nominalist will need to employ some different quantifier when interpreting the claim 'there is a prime number between two and four', and will thus run into the same problems as the approaches above.

conditions under which things exist. Rather, it simply says that everything has a certain property, i.e., the property of being concrete. The problem is that (Nominalism$_\forall$) is supposed to function as a theoretical claim or principle that is to serve as an important disputed piece of evidence in the dispute between the nominalist and the Platonist. Consider the following dialogue between a Nominalist and a Platonist.

Nominalist: "Everything is concrete."

Platonist: "Numbers aren't concrete."

Nominalist: "Yes, but numbers don't exist."[247]

Platonist: "Why not?"

Nominalist: "Because everything is concrete."

The Platonist is asking the nominalist to give her a *reason* for why numbers do not exist. The problem is that if the nominalist can only respond with the claim "Everything is concrete" we will be back where we started.

Another alternative would be to simply abandon Quinean ontology altogether by taking the relevant notion of existence to be captured by a predicate, '*E*'. For instance, Kit Fine considers various ways of formulating the heavyweight/lightweight strategy and ultimately rejects them in favor of the predicate strategy (2009). One might then reformulate (AP3) as follows:

(Nominalism$_E$) $\forall x(Ex \equiv Cx)$

The Quinean approach at least provided a distinct method for determining what exists: determine the ontological commitments of our best theories. The problem with this approach is that it divorces existence claims from the notion of ontological commitment.

[247] In fact, the Nominalist should respond "There are no numbers", but this is even worse. The Platonist will simply respond, "Sure there are! I can list them: there's the number 0, 1, 2, 3…"

Once we treat existence as a predicate, the methodology of ontology becomes much less clear. How are we to determine what exists? On the simplest Quinean methodology, existence claims are acquired through a very simple process of regimentation into the simplest logical system available (i.e., first-order predicate logic with identity). But according to this methodology the Platonist/Nominalist dispute is settled easily in favor of the Platonist by the Quine/Putnam indispensability thesis. The Nominalist must thus reject traditional Quinean methodology by adopting a more complicated semantic theory that avoids commitment to abstract objects.

As Jody Azzouni has argued over several publications (1998; 2004; 2010a), the decision of whether to take ordinary quantification, 'heavyweight' quantification, or an existence predicate as ontologically committing will itself be determined by one's prior commitments to ontological principles. As Azzouni puts it, there is a difference "between criteria for what a discourse is committed to, and the rather different criteria for what exists" (2010a, 74).[248] As Azzouni puts it,

> arguments that support a criterion for what a discourse is committed to presuppose an antecedent criterion for what exists. If, for example, one takes causal interactivity as a property that all and only existing items have, then one should desert Quine's criterion because, given current science, that criterion forces ontic commitment to *abstracta*.3 One should instead treat quantification as ontically innocent, and invest the predicate 'is causally interactive' with ontic weight. (2010a, 45)

Given these considerations we can clearly see that the Platonism/Nominalism dispute cannot boil down to a disagreement about the semantic evidence.

[248] The method of regimentation in Quinean metaontology is an example of the first criterion. But as Azzouni also points out, there are other options, such as "the identification of the ontic commitments of a discourse either with the range of a second quantifier, or with the extension of a specialized predicate—*e.g.*, an 'exist' predicate (a transcription of something in the discourse to be regimented or something that's new in certain respects)" (2010a, 75). In contrast, existence criteria "are descriptions of (necessary and sufficient conditions for) what there is—characterizations of properties that all and only what there is shares", some examples of the "candidate criteria are *is causally interactive*, *is mind-independent*, or *is an arbitrary mereological sum of material in space-time*" (ibid).

However, things are slightly more complicated. The problem is that the simplicity of our semantic theory is not the only thing at stake in the Platonism/Nominalism dispute, for Platonism also apparently involves a complication of our epistemological theories. This tradeoff was famously described by Paul Benacerraf as follows:

> It is my contention that two quite distinct kinds of concerns have separately motivated accounts of the nature of mathematical truth: (1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of the language,[1] and (2) the concern that the account of mathematical truth mesh with a reasonable epistemology. It will be my general thesis that almost all accounts of the concept of mathematical truth can be identified with serving one or another of these masters *at the expense of the other*. Since I believe further that both concerns must be met by any adequate account, I find myself deeply dissatisfied with any package of semantics and epistemology that purports to account for truth and knowledge both within and outside of mathematics. For, as I will suggest, accounts of truth that treat mathematical and nonmathematical discourse in relevantly similar ways do so at the cost of leaving it unintelligible how we can have any mathematical knowledge whatsoever; whereas those which attribute to mathematical propositions the kinds of truth conditions we can clearly know to obtain, do so at the expense of failing to connect these conditions with any analysis of the sentences which shows how the assigned conditions are conditions of their *truth*. (1973, 661-2)

In other words, the problem is that both Nominalism and Platonism require tradeoffs. Because Nominalists deny that the truth of mathematical propositions demands the existence of mind-independent objects and properties, they are free to provide an account of mathematical knowledge that depends in some way either on our conceptual competence or our ability to manipulate symbols.[249] However, this is only possible at the cost making our semantics for mathematics significantly more complex.

[249] We are here focusing on mathematics, but the exact same argument can be given for knowledge of other abstract objects such as propositions and properties. Of course, the classic Nominalist strategy utilizes the analytic/synthetic distinction, but this has largely fallen out of favor. Paul Boghossian (1994; 1996; 2001; 2003) has recently revived a version of this view for his epistemology of mathematics based on a purely epistemic conception of analyticity. However, the most explicit Nominalist epistemological project in the current literature utilizes Neo-Fregean abstraction principles to argue that our knowledge of

In contrast, the Platonist can accept the very simple (quasi-)Tarskian truth schema where atomic sentences of the form '*F*(*a*)' are true iff *'a*' refers to *a*, '*F*' denotes the property *F*, and *a* is *F*, and where the truth of all other sentences are defined recursively in terms of atomic sentences. However, they can do so only at the cost of making our theory of knowledge more complex. Our knowledge of certain truths (i.e., about sticks and stones) is apparently causal and must ultimately rest on a combination of perception and inference. However, if the Platonist is correct we must apparently make our epistemological theory more complex by adding some epistemic faculty that is capable of putting us in reliable contact with abstract objects.

For example, in the *Stanford Encyclopedia of Philosophy* on mathematical Platonism, Øystein Linnebo reconstructs the epistemological argument against Platonism is as follows:

1. "Mathematicians are reliable, in the sense that for almost every mathematical sentence *S*, if mathematicians accept *S*, then *S* is true." (Linnebo 2013, §4.1)

2. "For belief in mathematics to be justified, it must at least in principle be possible to explain the reliability described in Premise 1." (ibid)

3. "If mathematical platonism is true, then this reliability cannot be explained even in principle." (ibid)

As Linnebo points out, the first two premises of the argument above are "relatively uncontroversial" (ibid). Rejecting the first premise would amount to skepticism about mathematics. Moreover, the second premise also seems reasonable.[250] Thus the real disagreement must boil down to the third premise.

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mathematical truths can be derived by abstracting from our more ordinary knowledge (Hale & Wright 2009).

[250] As Linnebo puts it, "If the reliability of some belief formation procedure could not even in principle be explained, then the procedure would seem to work purely by chance, thus undercutting any justification we have for the beliefs produced in this way" (2013, §4.1).

The third premise must be justified by the claim that any such reliability of our knowledge must be explained in causal terms. But here too the dispute seems to boil down to existence criteria. Unsurprisingly, the different sides seem to differ as to which type of theory they consider to be important in regard to preserving simplicity. Indeed, there are reasons to favor either side. On the Platonist's side, one may cite the fact that premise 3 of the argument above simply begs the question against the Platonist, who will usually also argue for a rationalist theory of intuition that is supposed to describe exactly the sort of reliability in question. Indeed, part of the motivation for a theory of rational intuition is that our knowledge of abstract truths seems to require more than perception and inference can give us. For instance, Chudnoff describes the motivation for his intellectual perception view of intuition as follows:

> It is not clear how memory, testimony, deduction, induction, and abduction *alone* could be the epistemic basis for any beliefs. Memory can preserve knowledge over time. Testimony can serve to transmit knowledge among persons. Deduction, induction, and abduction can modify a pre-existing body of knowledge by contradicting it, expanding it, or reorganizing the epistemic dependencies that hold among its constituents. Each of these components of reasoning, however, presupposes some pre-existing knowledge. And combining them does not seem to change the situation. … On the face of it, there should be some conscious episode that, just as sensory perception does for empirical reasoning, injects a priori reasoning with some content. Intuitions thought of as intellectual perceptions fit the bill. (2013c, 16)

In other words, the Platonist already has good reason to think that our epistemological theories will need to involve more than just perception, reasoning, and belief preserving mechanisms precisely *because* they think that there is a realm of abstract truths that we must have access to. Put somewhat differently, the Platonist already differs from the Nominalist in terms of what *data* she thinks our epistemological theories are supposed to explain. Since the Nominalist denies that there is a separate realm of abstract facts that

we need to know, she is content to have an epistemological theory that relies solely on perception and reasoning. In contrast, the Platonist thinks that the data that an epistemological theory must explain includes facts about an abstract realm, and thus she takes the Nominalist's epistemological theory to be insufficient at explaining the data because its resources are too limited. In other words, this epistemological aspect of the abductive dispute between the Platonist and the Nominalist must essentially reduce to their more basic dispute about existence criteria.

On the nominalist's side, there is the fact that the Tarskian semantics defines truth in terms of denotation. But this means that under the surface of the seemingly simplistic Tarskian truth-schema lies a far more involved theory of reference that does most of the work. Richard Creath puts the point as follows:

> Actually, even on 'standard' accounts there is a lot more [semantic] inhomogeneity than Benacerraf supposes. As Hartry Field has convincingly argued[4] Tarskian theories of truth involve a reduction of 'true' to 'denotes' (etc.), and without a non-trivial theory of denotation such theories of truth remain radically incomplete, though, of course, the Tarskian fragment may be completely sufficient for certain (e.g., model-theoretic) purposes. Now the non-trivial theory of denotation explicitly favored by Benacerraf is a causal one, though it manifestly does not apply to abstracta such as numbers. Thus, even if we seek to maximize homogeneity, it would seem that we face another dilemma: (A) We can add to the causal theory a special theory of denotation for numbers and other abstracta in which case we get denotational inhomogeneity and hence semantical inhomogeneity. (B) We can give a nondenotational theory of truth for discourse for which no causal theory of denotation is forthcoming, thus getting inhomogeneity once more. (1980, 336)

Put in slightly different terms, the cost of the Platonist's simplistic semantic theory is a more complicated *metasemantic* theory about how reference is fixed. Many of the current metasemantic theories rely heavily on reference-fixing mechanisms that depend upon causal links to the referent (e.g. Kripke 1981). The Platonist's semantic theory only tells

us that our numerical terms *must* denote abstracta in order to come out true. But the Platonist has not yet told us *how* numerical terms come to designate these abstracta in the first place.

However, the metasemantic question is closely tied to the epistemological objection to Platonism. The exact nature of the answer will of course depend upon the Platonist's other commitments. A Platonist that embraces a theory of rational intuition will likely take a knowledge-first answer to the metasemantic question: our intuitions give us access to the objects and this is what fixes the reference of our terms.[251] Another line of reply is to argue for a metasemantic principle in which our use of mathematical symbolism necessitates the reference of these terms. In this case we will have access to the objects in question transitively through our use of the symbols that represent them.[252] However, this latter approach will result in some form of superficialism about abstracta, and will thus likely be dismissed by the Platonist.[253]

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[251] The nature of this answer will, in turn, depend upon the nature of the theory of intuition in question. Thus a Platonists who holds a theory of intuition as intellectual perception (e.g., Chudnoff) can simply claim that the reference of numerical terms is fixed through ostension, just like the terms that refer to ordinary concrete objects. The only difference is that the latter involves perceptual awareness of the object in question while the former involves intuitive awareness of the objects. A Platonist who embraces an understanding-based view will likely attempt to answer the metasemantic question by arguing that the inferences we draw about the concepts in question determine their reference (e.g., Bealer 1998). This inferentialist strategy is best combined with some form of structuralism, where it is not the particular objects in question that matter (e.g., Benacerraf 1996).

[252] There are several ways of accomplishing this. First, one can simply embrace a deflationary semantics along with a deflationary conception of existence: i.e., to embrace a disquotational theory of truth and reference along with minimalism about existence. Another answer involves arguing for Plenitudinous Platonism (PP) in which Platonic objects are so abundant that we cannot help but refer to them (e.g., Balaguer 1995).

[253] First of all, to claim that the semantic properties of mathematical symbols are completely grounded in how human beings manipulate and apply those symbols to their understanding of the physical world implies that abstracta play no important role in our knowledge of mathematics. Second, the two strategies mentioned above will all only work on a superficialist view of semantics and ontology. For instance, PP faces serious problems when formulated directly as a metaphysical thesis (Restall 2003). The only real way to make sense of the thesis is as a metalinguistic claim that every consistent mathematical theory is

What this shows is that both concerns about the simplicity of semantic, metasemantic and epistemological theories are really dependent upon antecedent ontological commitments. If one is a Platonist, one will already assume that reality is composed in part of a realm of abstract entities and thus that any reasonable epistemological and metasemantic theory must involve principles that explain how we have non-trivial access to this realm. According to the Platonist, Nominalist theories of epistemology and metasemantics might be simpler, but they fail to explain the data. Likewise, the Nominalist holds that there are no such things as abstract entities. Thus she claims that the Platonist's semantic theories, though simple, must be considered failures because they offer an incorrect explanation of the data: they falsely predict that we refer to things that are not there.

Of course, Platonism involves a massively *de re* quantitatively inflation of ontology (see the end of §2.2.1.4). However, as we have seen the Nominalist's theory will invariably involve complications in our semantic and logical ideology such as the use of additional quantifiers or free logic. Moreover, the place where a *de re* quantitative inflation of ontology seems most objectionable is in the sciences, but, the Platonist will argue, this is due to the nature of the phenomena that the sciences study.[254] Finally, if Nominalists were simply concerned about the number of things then they should be equally concerned with arguing against the claim that spacetime is continuous.[255]

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true, in which case it begins to look suspiciously similar to a simple deflationary semantic theory (Rabin 2007).

[254] For instance, the positing of additional objects in the sciences is usually considered a bad thing because the sciences are aimed at explanations of causal phenomena. Thus considerations of *de re* quantitative simplicity are usually intimately tied to considerations of simplicity regarding causal laws and things like overdetermination. These sorts of problems do not obviously arise for absracta.

[255] The following is a theorem for the standard laws of arithmetic for infinite cardinals, "If μ is an infinite

Nominalists clearly are not driven by cardinality concerns. Rather, they are clearly driven by a concern that abstract entities are "spooky" or "ghostly" entities. Thus the dispute between Nominalists and Platonists must ultimately boil down to differences about extremely general ontological principles, such as the following claims: "Something exists iff it is concrete", "Something exists iff it is spatiotemporally located", "Something exists iff it is mind-independent", "Something exists iff it is substantial", "Something exists iff it possesses a 'this-ness'". Let us put aside the difficulty of formulating these principles, and simply ask *how* they are to be justified. There can be only one answer: intuition. The case is almost exactly the same as the principle that the modal is grounded in the non-modal in the previous section. First of all, these claims clearly cannot be said to belong to common sense in even the broadest sense, so Mooreanism is out. Second, they clearly cannot be justified by empirical evidence.[256] Finally, it is clear that these claims have no abductive benefit, as our consideration of the various nominalist proposals above indicate.[257] In other words, the *entire* dispute between the Platonist and the Nominalist becomes completely inscrutable unless one takes the general existence criteria that each view posits as disputed evidence. For instance, let us simply focus on the claim about concreteness in (AP3). The intuitions could be reconstructed as follows:

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cardinal and λ is any cardinal such that $\lambda \leq \mu$, then $\lambda + \mu = \mu$" (Machover 1996, 99). Thus if the Nominalist allows that there are uncountably infinite points of spacetime then she should have no problem with the existence of the natural numbers at the very least. Note that this is different from the common objection that a concern solely with the cardinality of the domain of one's theory will result in Pythagoreanism due to the Downward Löwenheim-Skolem theorem, which states that any theory can be satisfied by a model with a domain with a denumerable cardinality. This issue is far more specific, and still arises even if the Nominalist restricts the appropriate models according to metasemantic principles (i.e., to models that only assign to terms things that could reasonably serve as their referents).

[256] It would simply be question-begging to assume that they are justified inductively, as one would already have to rule out quantification over abstracta in order to make this claim.

[257] Specifically, the problem is that (as we have just shown) these principles are what determine the data that is used to decide between theories. Thus it would be circular to attempt to justify them abductively. Moreover, they clearly have no practical benefit of their own.

(AP3 G_{r+}) One attempts to imagine something that exists and every example that one comes up with is something that is concrete.

(AP3 G_{d+}) One considers the open proposition *x exists*, and it seems based upon one's understanding of this proposition that one can immediately infer from it the proposition *x is concrete*, and this seems to be the case regardless of what one might replace '*x*' with.

(AP3 G_{r-}) One attempts to imagine a realm of non-concrete entities, and immediately fails because any attempt to visualize such entities comes up empty in virtue of their non-spatiotemporality.

(AP3 G_{d-}) One considers the proposition that non-spatiotemporally extended things exist and one is left with nothing but mental blankness: one cannot seem to grasp what this proposition is supposed to mean.

(AP3 P_{r-}) One attempts to imagine the number four but cannot visualize anything. (E.g., perhaps the only thing that one can call to mind is the symbol "4", which cannot be equated with the number four itself.)

(AP3 P_{r+}) One attempts to imagine the number four and ends up visualizing some sort of ghostly entity that seems absurd. (E.g., perhaps the only thing that one can call to mind is a floating disembodied symbol "4".)

(AP3 P_{d-}) One considers the proposition *the number four exists*, but is left mentally blank. One cannot understand what the proposition is supposed to convey.

Very few of these examples seem to provide reasonable evidence. First of all, (AP3 G_{r+}) seems to constitute little more than an argument from lack of imagination. (AP3 G_{r-}), (AP3 P_{r-}) and (AP3 P_{r+}) manage to avoid this objection due to the fact that it will *never* be possible to visualize abstracta, but for this very reason they simply beg the question against Platonism. Furthermore, (AP3 G_{d+}) appears phenomenologically implausible (I for one am not able to draw any such immediate inference based upon my understanding of the term 'exists'). Thus the only plausible versions of the intuition left are (AP3 G_{d-}) and (AP3 P_{d-}). Thus the only reasonable way to justify the Nominalist's position is through an account on which such negative *de dicto* intuitions about existence claims are reliable. There are two things to note about this. First, this means that the Nominalist will

most likely need to appeal to an understanding-based view of intuition. Second, it is important to note that these particular intuitions are especially susceptible to a restricted-quantifier form of semantic compatibilism (which we shall consider in the next section). In other words, the Platonist can simply argue that the nominalist only has these intuitions because she has tacitly restricted her quantifiers to the domain of concrete entities.

However, it would be unfair to focus solely on the Nominalist. After all, if the Platonist is going to be a substantivist, she must establish that the existence of abstract entities is something we should be excited about. To see this, consider the fact that a deflationary ontologist will be completely nonchalant about the existence of abstract entities like numbers (e.g., Thomasson 2008; 2009a,b). Thus when the deflationary ontologist says that numbers and properties exist, she takes herself to be saying something totally banal: "*Of course* there are numbers! There is the number zero, the number one, the list goes on…" This is because the deflationary ontologist does not treat existence as any sort of special achievement. According to the deflationary ontologist, there is no interesting question about numbers beyond the questions asked by mathematicians. The only interesting properties that numbers possess are their mathematical properties (e.g., evenness, primeness, divisibility, etc.).[258] In contrast, a heavyweight Platonist wishes to say that she has discovered something really important that is independent of the concerns of mathematicians. Thus in order to distinguish her theory from that of the deflationary ontologist she too will have to adopt some general

[258] It is a category mistake to say that the numbers exist in some "realm". Numbers don't exist *anywhere*. Moreover, the deflationist continues, numbers are only "mind-independent" in the sense that one will not find the number four in anyone's head. We come to know about numbers in school by learning to manipulate the symbols that refer to them.

criteria regarding what exists that makes this claim interesting. In fact, some claim like this is needed in general in order to avoid rendering all ontological disputes trivial. What is the point of denying the existence of some class of entities if existence is something that is unimportant and easily achieved? Unfortunately, it is extremely difficult to pin down what exactly this general ontological principle would be, as the examples seem to be metaphorical at best.[259] Given the importance of such a principle, this suggests that all of ontology ultimately rests on appeals to intuitions that don't even exist!

2.4.2.1. General Principles in the Metaphysics of Time

Similar considerations apply to both the dispute between presentists and eternalists. In both instances one theory is clearly simpler than another. For instance, while the presentism would appear to be less *de re* ontological complexity than eternalism, it more than makes up for this in other respects. First of all, the presentist will need to adopt a vastly more complex ideology through its use of irreducibly tensed quantifiers, which will have significant abductive costs. Second, in order to deal with claims about the past and the future the presentist will likely have to add irreducible tense to her account of properties as well. Moreover, she will need to adopt a far more complex semantic theory in order to deal with statements about the past and the future. In addition, the notion of a moving present will also result in greater *de re* metaphysical complexity, in so far as it ascribes a sort of irreducible and non-relational element of change to the

[259] Candidates include the claim that what exists is somehow *substantial*, or that everything must exist in some sort of "realm", or that what exists must have some sort of "oomph". For instance, van Inwagen objects to the Meinongian view that one can quantify over non-existent objects as follows (here the non-existent object is supposed to be a unicorn): "If the Meinongian's assertion were true—this is what I want to say—and if I were made free of all space and all time, I ought to be able to find, encounter, or observe a unicorn. But this I should *not* be able to do: no magic carpet or starship or time machine could take me to a place where there was a unicorn" (2003, 140). But this is simply incoherent nonsense. There is no reasonable sense in which van Inwagen could use a "magic carpet or star ship or time machine" to encounter the number four. Thus if van Inwagen's objection were to have any force against the Meinongian, it would thereby equally defeat his Platonism.

fundamental structure of reality. Were this not enough, presentism also seems to openly conflict with the theory of Special Relativity in physics.[260] No one could reasonably argue that all these points are outweighed by the fact that presentism is committed to fewer entities than eternalism. The motivation for presentism must also ultimately boil down to some general metaphysical principle backed by intuition. It is difficult to pin down exactly what this principle is, but, just as with Nominalism, we can expect it to be intimately related to the claim that only the present moment exists.[261] We shall not bother to try to cash out this claim. The important point is that presentism would simply appear to be unmotivated without it.

However exactly this claim is supposed to be cashed out, it falls prey to the same analysis as the contingentists' principle that the modal is grounded in the non-modal, and the Nominalists' principle that only concrete objects exist. First of all, it clearly cannot be justified empirically. For the same reasons as above, it makes little sense to attempt to justify this principle abductively.[262] Finally, whatever the claim is, it is also clearly too abstract to be justified on a Moorean basis. The only justification in the neighborhood would be an appeal to the use of tensed quantifiers in natural language; e.g., that the folk are unwilling to assent the claim "Dinosaurs exist". However, whether this fact counts for or against presentism will depend upon how one analyzes sentences such as "Dinosaurs exist" semantically.

[260] On all these points, see Crisp 2003, 2007; Rea 2003; and Sider 2001b.

[261] Perhaps it should be understood as an appeal to the appearance that we are moving through time, that the past has moved out of existence or is behind us, or that the future has not yet come into being.

[262] Clearly the claim does not have any sort of obvious practical benefit, nor does it lead to a simpler theory. Once again, the presentist's principle would seem to imply something about the *data* that theories of time should be measured against; i.e., that eternalism is false because it commits one to past and future entities that do not really exist. But this means that the principle cannot be justified abductively.

The eternalist has a solution that is analogous to the necessitist and the Platonist: this sentence is false because its quantifier is restricted to only those things that exist in the present moment. The eternalist will treat the apparently tensed quantifiers in natural language simply as versions of the untensed quantifier that are restricted to specific time indexes according to the context of utterance. But in this case, the sentence "dinosaurs exist" will be false because it employs the present tense quantifier, which is restricted to objects that exist *at the time of utterance*. This, however, will have no real bearing on the overarching ontological dispute. Indeed, the eternalist seems to be in a better position than the presentist for two reasons. First, the logic that does not use irreducibly tensed quantifiers is much simpler than one that does, and the simplicity of the formal system into which things are analyzed is an important consideration in natural language semantics. Second, many parts of natural language discourse also appear to use untensed quantifiers (e.g. "there is an even number between five and seven" is best interpreted with an untensed quantifier).

The Moorean would also need to reject this interpretive maneuver. But this would take us into the realm of semantics, which transcends common sense. The folk may use apparently tensed quantifiers, but there is no reason to think that they also have a default view on how a semantic theory should interpret these quantifiers. Of course, this is not to say that one cannot appeal to intuitions in *semantics* to justify the presentist's position. It is merely to say that this will require something more than Mooreanism. We shall return to this in §2.4.3. Either way, the only way to justify this principle is through an appeal to intuition.

2.4.2.2. General Principles in Mereology

Some of the most important appeals to intuitions in metaphysics concern abstract principles in mereology, location and persistence. One example of the problem of colocation is illustrated by the case of Tibbles and Tib:

> A cat called Tibbles loses his tail at time t_2. But before t_2 somebody had picked out, identified, and distinguished from Tibbles a different and rather peculiar animate entity—namely, Tibbles *minus* Tibbles' tail. Let us suppose that he decided to call this entity "Tib." Suppose Tibbles was on the mat at time t_1. Then both Tib and Tibbles were on the mat at t_1. … But consider the position from t_3 onward when, something the worse for wear, the cat is sitting on the mat without a tail. Is there one cat or are there two cats there? Tib is certainly sitting there. In a way nothing happened to him at all. But so is Tibbles. For Tibbles lost his tail, survived this experience, and then at t_3 was sitting on the mat. And we agreed that Tib ≠ Tibbles. We can uphold the transitivity of identity, it seems, only if we stick by that decision at t_3 and allow that at t_3 there are two cats on the mat in exactly the same place at exactly the same time. (Wiggins 1968, 94)[263]

Since Tib has a tail and Tibbles does not, it follows from Leibniz's Law that Tib ≠ Tibbles.[264] Our everyday judgments tell us that Tibbles survives having his tail removed, and thus continues to exist at t_3. Since nothing seems to have happened to Tibs either, we appear to be suck with the claim that Tib and Tibbles are co-located at t_3. Thus the problem arises from a threefold conflict between everyday judgments about persistence, the indiscernibility of identicals, and the principle that distinct objects cannot be wholly located in the exact same place at the same time. To see the role that intuition plays in this discussion, we need to consider these sorts of examples in greater depth. In order to do so, we need to briefly lay out a basic formal framework in which to discuss them.

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[263] See Wasserman 2013 for general discussion. Wiggins credits this example originally to Peter Geach.

[264] Given what follows, it will be useful to formulate two distinct versions of Leibniz's Law::

(*LL*-1) **∀x∀y(x=y → (φ(x) ↔ φ(y)))**

(*LL*-2) **∀xx∀yy(xx=yy → (φ(xx) ↔ φ(yy)))**

Note that these are metalinguistic formulations of the laws for any object languages that behave like standard first-order singular and plural quantificational logic respectively. The expression '**φ(_)**' indicates that '**φ**' is an open formula in which the appropriate variable is free.

The issues we shall examine involve concepts such as plural quantification, a theory of location and a putative ontological category of 'stuff'. It is prudent to adopt a minimal formal language to serve as a general framework for discussing these issues.[265] The syntax of this language is comprised of (1) standard propositional connectives.[266] (2) Three syntactic categories of terms (i.e., variables and constants): singular ('*x*','*a*'), plural ('*xx*', '*aa*'), and *mass* ('x^m', 'a^m'), and three corresponding pairs of '∃' and '∀' quantifiers, which are differentiated by the variables they take.[267] (3) Three identity relations '=', '≕', and '$\overset{m}{=}$' defined in the typical manner as equivalence relations on the singular, plural and mass terms respectively such each holds only between terms of only one type. Syntactically well-formed identity statements include '*x*=*y*', '*a*=*b*', '*xx*≕*yy*', '*aa*≕*bb*', '$x^m \overset{m}{=} y^m$', and '$a^m \overset{m}{=} b^m$'. (4) A binary relation '≺' that holds between a singular or mass term and a plural term, which stands for *_is one of the _s*. Thus both '*x*≺ *xx*' and 'x^m ≺ *xx*' are well-formed. Thus one is able to quantify plurally over both objects and

[265] Different theories will deal with these notions in different ways. Some will attempt to reduce mass terms to mereology (Nicolas 2014, §2; Zimmerman 1995). Others will treat mass terms as denoting a special ontological category of stuff (e.g., Markosian 2004).

[266] We will use '∧' for conjunction, '∨' for disjunction, '∼' for negation, '⊃' for material implication, and '≡' for the material biconditional.

[267] Thus '∀*x*(φ(*x*))' means for all *x*, φ holds for *x*. '∃*x*(φ(*x*))' means there is some *x*, such that φ(*x*). '*x*=*y*' means *x* is identical to *y*. '∀*xx*(φ(*xx*))' means for all *x*s, φ holds for those *x*s. '∃*xx*(φ(*xx*))' means there are some *x*s such that φ holds for those *x*s. '*xx*≕*yy*' means the *x*s are the same as the *y*s. These mass terms should be understood as denoting *portions* of mass, which should be understood as primitive, analogous to things and pluralities. The mass quantifiers behave syntactically like singular quantifiers ranging over portions. Even Markosian is forced to resort to this strategy (2004, 409). A portion of stuff is different from a *quantity* of stuff, just as a thing is different from a number of things. Two different pluralities can have the same number, just as two different portions of stuff can be comprised of the same quantity of stuff. One could describe this distinction as holding between physical *entities* and *amounts*, where the latter are abstract numerical entities. Thus the language does not, strictly speaking, allow one to quantify over all *water*. Rather, it allows one to quantify over all *portions of water*. Note also that there is only *one* mass superscript. Differences in types of 'stuff' should be denoted through the use of mass predicates. We leave it open how many different types of stuff there are. This is a further (nasty) complication of the logic. '∀*x*(φ(x^m))' means for all portions of mass m, φ holds for those portions. '∃*x*(φ(x^m))' means there is some portion of mass m, such that φ holds for that portion. '$x^m \overset{m}{=} y^m$' means the portion *x* of m is identical to the portion *y* of m.

portions of stuff.[268] (5) A single syntactical category of *n*-ary predicates, which can take terms of any category as their arguments.[269] We may assume that the meta-logical properties of the system will *roughly* match that of standard first-order logic plus plural quantification.

There are two things worth noting before we move on. First, this system is intended to balance inclusiveness (regarding 'stuff' ontologies) with clarity and simplicity of expression. This treatment is motivated by our later discussions of semantic compatibilism, which present us with the need to capture an approximation of English grammar. Since the mass-count distinction appears to be even more fundamental than the singular-plural distinction (as the latter is a distinction amongst count terms), and since the distinction attaches directly to nouns (in English), it only makes sense for our formal language to mimic English syntax with its own primitive syntactic distinction (though see Pelletier 2012 for discussion). The simplicity of adding a new type of variable should make translations fairly straightforward for those who wish to take another approach to mass terms. Second, the entire point of this language is to function as a clear and general framework for describing a number of metaphysical theses. Different metaphysical theses will correspond to different models for this language.[270] The inclusiveness of this system is convenient for the following reason: if one holds that stuffs are nothing more than things or pluralities, then the model that one provides for the system will itself show that the system's syntax is redundant.

[268] (This will make the rules governing plural quantification more complicated).

[269] E.g., '$F\langle x,yy, x^m\rangle$' is considered syntactically well-formed.

[270] However, it should be obvious that the addition of a category of constants and variables which function like singular terms will be of no real consequence to the syntactic properties of a language that already has singular and plural quantification.

Next, we introduce a basic mereological system. For various reasons, I shall adopt a modification of the system, ***M*,** presented by Achille Varzi (2014).[271] We begin by defining the primitive relation '<' for (non-proper) parthood. We will use '<' univocally to stand for both part-whole relations between things and portions. For instance, one might think of '$a^m<b^m$' as meaning 'stuff *a* is a portion of stuff *b*'. Note, however, that we will leave the relation undefined on pluralities (i.e., formulas in which the relation is flanked by plural terms will be considered syntactically ill-formed). With this in mind, we define the following three axioms for '<':

(*M1*) ***Reflexivity*:** $x<x$ and $x^m<x^m$

(*M2*) ***Transitivity*:** $(x<y \wedge y<z) \supset x<z$ and $(x^m<y^m \wedge y^m<z^m) \supset x^m<z^m$

(*M3*) ***Anti-symmetry*:** $(x<y \wedge y<x) \supset x=y$ and $(x^m<y^m \wedge y^m<x^m) \supset x^m \overset{m}{=} y^m$

We will return to more general considerations regarding mereological theories later. For now, we can define the following additional predicates in terms of '<'.

***Proper Parthood*:** $x \ll y \equiv (x<y \wedge \sim x=y)$ and $x^m \ll y^m \equiv (x^m<y^m \wedge \sim x^m \overset{m}{=} y^m)$[272]

***Overlap*:** $O\langle x,y\rangle \equiv \exists z(z<x \wedge z<y)$ and $O\langle x^m,y^m\rangle \equiv \exists z^m(z^m<x^m \wedge z^m<^m)$[273]

Mereological Simple: $Sx \equiv \sim\exists y(y \ll x)$ and $Sx^m \equiv \sim\exists y^m(y^m \ll x^m)$ [274]

Mereological Fusion: $Fu\langle x,yy\rangle \equiv \forall z[\exists w(w \prec yy \wedge z<w) \equiv z<x]$[275]

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[271] ***M*** "is standardly viewed as embodying the common core of any mereological theory" (Varzi 2014, §3.0).Varzi's account is especially helpful because he presents it within the context of a general overview of mereological theories. I will therefore defer the reader to his remarks on various other formulations. For the sake of readability, I have left out universal quantifiers for the appropriate variables unless otherwise specified. Note, also that I am using '<' and '≪' for non-proper parthood and proper parthood respectively, whereas Varzi uses 'P' and 'PP'.

[272] 'PP*xy*' = '*x* is a proper part of *y*', which means that *x* is a part of *y* and it is not the case that *x*=*y*.

[273] 'O*xy*' = '*x* overlaps *y*', which means that there is some *z* that is a part of *x* that is also a part of *y*. Note that to say that two objects are disjoint is just to say that they do not overlap. Hence '*x* and *y* are disjoint' is logically equivalent to the claim ¬O*xy*.

[274] '*Sx*' = '*x* is a mereological simple', which means that *x* contains no proper parts.

We can now add a fairly inclusive relation '•⊸' of exact location, which will put us in a position to specify binary predicate for colocation. [For the sake of simplicity we only use singular versions of the variables and identity relations, but there should really be three separate definitions.]

***Exact Location*:** $x\bullet\!\!\multimap y \equiv \sim\exists z(x\bullet\!\!\multimap z \wedge z\neq y) \wedge [(x\bullet\!\!\multimap y \wedge Sx) \vee$

$[\forall z\forall v((z<x \wedge z\bullet\!\!\multimap v) \supset \exists w(w<y \wedge w\bullet\!\!\multimap v \wedge \sim\exists u((z\bullet\!\!\multimap u \vee w\bullet\!\!\multimap u) \wedge u\neq v))))$

$\wedge \sim\exists v\exists u(v<y \wedge v\bullet\!\!\multimap u \wedge \sim\exists w(w<x \wedge w\bullet\!\!\multimap u))]]$[276]

Colocation: $Col\langle x,y\rangle \equiv \exists z(x\bullet\!\!\multimap z \wedge y\bullet\!\!\multimap z)$

We can describe all these examples as philosophically troublesome only if the following principle is given evidential weight:

(AP4) $\sim\exists x\exists y Col\langle x,y\rangle$

The notion of constitution is supposed to account for the persistence conditions of objects like statues and lumps of clay without claiming that they become temporarily identical. People object to these theories of constitution precisely because they find (AP4) so appealing. For instance, Michael Rea describes the problems of constitution as follows:

> Intuitively, the problem of material constitution arises whenever it appears that an object *a* and an object *b* constitute one another and yet are essentially related to their parts in different ways. … it seems we can account for the relevant appearance only by somehow denying that it is veridical [i.e., by denying that there are two objects] or by sacrificing

[275] '$Fu\langle x,yy\rangle$' = '*x* is the fusion of the the *y*s'; i.e, *x* has all and only the parts of the *y*s as its parts (including the *y*s).

[276] Our aim here is not to provide an analysis of location, but rather only the technical notions necessary to discuss problems such as co-location. See Kleinschmidt 2011a,b for an in-depth discussion of theories of location. There are three things to note about this. First, while this allows for a primitive base case when *x* is a simple, it does not *presuppose* that there must be simples. Second, note that this definition is intended to be permissive regarding multi-location as well as the possibility of non-spatiotemporal parts. The reader may note that this definition will make it such that multi-located objects will be exactly located in bloated regions. However, this is not terribly problematic, as the notion of exact location here is intended to be technical. Third, note that it should be easy to construct a relation of exact location for pluralities from this, though we set this issue aside.

> some intuition about composition or identity—for example, by giving up our natural assumption that distinct objects cannot share all of the same parts at the same time, or that identity is necessary. (1995, 527)

This only becomes controversial if one gives significant evidential weight to a principle like (AP4).[277,278] Now, it is easy to see that (AP4) cannot be justified empirically, for we would need an explanation for why can't we see that the statue and lump occupy the same space at the same time. Nor can it be justified through a Moorean strategy, as it is simply to technical to belong to common sense. Since theories of constitution and issues of co-location have led to significant philosophical disagreement, this seems to indicate that an appeal to the intuitiveness of (AP4) functions as an important piece of evidence. This is good evidence that (AP4) meets Rock*.

Before we go on to analyze the intuitions behind (AP4), we first need to distinguish between two notions of grounding: *de re* and *de facto*.[279] The distinction is fairly straightforward: *de re* grounding relations hold between entities, while *de facto* grounding relations hold between facts. It should be exceptionally clear that the operative notion of grounding in examples of material constitution is *de re*. It is the lump itself that grounds the statue. Insofar as one can describe *de re* grounding relations, these relations will also obviously license the inference of certain *de facto* grounding relations. For instance, if we know that the statue is materially grounded in (i.e., constituted by) the

[277] Note that it would be confused to attempt to justify (AP4) on purported physical principles through examples where co-location would involve an impossible "smashing" of things together (e.g., one cannot get two tables made of distinct materials so that they are co-located, no matter how hard one might smash them together). This is because the cases do not involve objects composed of distinct matter.

[278] Note that while there are ways to argue for (AP4) via more fundamental principles, these more fundamental principles also seem to be governed by more or less the same sort of intuition. (AP4) is implied by the extensionality of parthood, which in turn is either taken to be intuitive in its own right (Varzi 2000, 2008; Sider 2007c), or else derivable from intuitive principles such as Weak Supplementation and Universalism (Varzi 2009), though of course others have also argued against this derivation (Bennett 2013 and Rea 2010).

[279] See deRossett 2011 for an overview of the "grounding problem" in the literature on constitution.

lump of clay then we are obviously thereby entitled to the claim that the fact that the statue is composed of such-and-such clay is grounded in the fact that the lump is composed of such-and-such clay.[280] These only make up a subset of *de facto* grounding relations. The notion of *de re* grounding is fairly restrictive, whereas *de facto* grounding can be extremely permissive. Here, the distinction can be quite usefully applied to itself. I here understand 'facts' to be true structured propositions.[281] Facts themselves are at least partially grounded *de re* in the things they are about and the relations that hold between those things. Because of this, many different propositions can be grounded *de re* by the same collection of things. The fact that we can describe more *de facto* grounding relations than *de re* grounding relations is grounded (*de facto*) in the fact that there are many ways of describing the same situation.

Following our analysis above, there are a number of possible mental states that could constitute the intuition behind (AP4):

(AP4 $\mathbf{P}_{r+/-}$) One visualizes a particular statue/lump of clay and (+) seems to be aware of only one object or (–) fails to be aware of two distinct objects.

(AP4 $\mathbf{P}_{d+}$) One considers the proposition *that statue is identical to that lump of clay*. It seems based upon one's understanding of the terms in question that the proposition is true. (For instance, it seems as though the demonstrative noun-phrases *that statue* and *that lump of clay* will pick out the same object in the context of utterance).

[280] Whether or not we count these inferences as 'analytic' is really only a question of interest to semantics. The notion of epistemic analyticity is supposed to explain how we know things *a priori* by appealing to a supposedly unproblematic knowledge of language. Even if we rejected the notion of analyticity on a semantic level, it should be fairly obvious that this has little effect on the epistemology of these inferences. If there is any sense whatsoever in the claim that some knowledge is unproblematically licensed by our grasp of language—and it seems simply absurd to deny this—it should be clear that it is this kind of knowledge that is at work in these inferences.

[281] I shall assume several things about propositions: (1) they are abstract, (2) they are expressed by sentences, (3) they have fairly fine-grained structures that roughly match the grammatical structures of the sentences through which they are expressed, and (4) they are connected to the things they are about. In other words, I am assuming they are something like Russellian structured propositions or fine-grained intensional functions.

(AP4 P_{d-}) One considers the proposition *a statue and the lump of clay of which it is composed are two distinct objects that are wholly located in the exact same space at the same time*. It seems based upon one's understanding of the terms in question that the sentence is confused. (For instance, it seems as though the demonstrative noun-phrases *that statue* and *that lump of clay* will pick out the same object in the context of utterance, and thus that the sentence false).

(AP4 G_{r+}) One imagines general instances in which a plurality of matter is spread across a determinate location and in every such instance one seems to be aware of only one object composed by that matter, or one set of qualities possessed by that matter.

(AP4 G_{r-}) One attempts to imagine any instance in which one is aware of two distinct objects that occupy the exact same region of space and one comes up empty.

(AP4 G_{d+}) One considers the proposition expressed by (AP4) and it seems based upon one's understanding of the phrase 'distinct objects' that the proposition is analytically true. (I.e., it seems that 'distinct' in this context *means* (in part) that the objects are located at different regions).

(AP4 G_{d-}) One considers the proposition *some distinct objects are wholly located in the exact same space at the same time* and one is unable to understand what this proposition means. (I.e., when one attempts to understand what 'distinct' means in this context one is simply left with bewilderment/mental numbness).

(AP4 T_{d+}) One considers (AP4) and it seems that it is a reasonable starting point/is not in need of additional argument or explanation.

(AP4 T_{d-}) One considers the proposition *some distinct objects are wholly located in the exact same space at the same time* and one is left with the feeling that this claim is in need of additional explanation or justification. I.e., a theory that accepts such a principle has to provide an account of how material objects are individuated that explains how this is possible.

Let us consider these intuitions in turn. We can rule out (AP4 T_{d+}) and (AP4 T_{d-}) from the start. If all we had in favor of (AP4) were (AP4 T_{d+}) and (AP4 T_{d-}) we should openly admit that co-location is possible and be done with the dispute.[282] Thus if

[282] The fact that theories need to account for *prima facie* counterexamples to (AP4) serves as an overriding defeater for whatever evidence is supplied by (AP4 T_{d+}) and (AP4 T_{d-}). First, all (AP4 T_{d+}) implies is that

something like (AP4) is to be given a good deal of evidential weight in philosophical theorizing, it will either have to be justified by additional argument or else one must show that one of the other putative types of intuitions behind it constitute strong enough evidence to outweigh our ordinary judgments about the persistence of objects.

The remaining intuitions behind (AP4) can be largely broken down into two categories. (AP4 $P_{r+/-}$), (AP4 G_{r+}), and (AP4 G_{r-}) all concern of *de re* grounding relations.[283] A more general way of capturing the content behind these intuitions is to say that there does not seem to be any quality possessed by the statue or clay that could ground the difference between the co-located objects.[284] Thus these intuitions are most plausibly backed by a theory of intuition as intellectual perception. In contrast, (AP4 P_{d+}), (AP4 P_{d-}), (AP4 G_{d+}), and (AP4 G_{d-}) are all essentially judgments that are backed by linguistic understanding, and will thus be most plausibly backed by an understanding-based view of intuition. The intuitions behind (AP4) will only be relevant to the apparent cases of co-location and other problems of persistence that arise for endurantist theories of material constitution, and not for perdurantism (i.e., the doctrine that ordinary objects

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(AP4) seems like a reasonable assumption, and reasonable assumptions are no longer reasonable in the face of counterexamples. If *all* one had to go on in support of (AP4) was (AP4 T_{d+}), one could accommodate the counterexamples by simply acknowledging that *in most cases* (AP4) is true but that this is because we are only tacitly considering objects that are composed of distinct materials, and that in cases like the statue and clay co-location is possible. Similar reasons apply to (AP4 T_{d-}). All (AP4 T_{d-}) demands is that someone who denies (AP4) explain why they do so. In response the co-location theorist can simply cite the existence of putative counterexamples to (AP4), and this is enough to override the evidence provided by (AP4 T_{d-}).

[283] It is important to get clear on our terminology here. The point is not just that the intuitions have a *de re* phenomenology, it is that they also concern grounding relations between putative objects. This is not a coincidence. The *de re* phenomenology of these intuitions allow them to make one directly aware of some relation between objects (or lack thereof), in the same sense that seeing one cup sitting on a desk next to another cup makes one directly aware of the spatiotemporal relations between the two cups.

[284] Insofar as being aware of only one object immediately implies a lack of awareness of two objects and *vice versa*, there is no substantial distinction between the positive and negative versions of these intuitions. Insofar as any general intuition in this instance will essentially involve visualizing the content of multiple particular intuitions, we can also put aside the distinction between particular and general. Thus (AP4 $P_{r+/-}$), (AP4 G_{r+}), and (AP4 G_{r-}) do not differ in content in any relevant way.

are either spacetime worms or can be individuated as stages of privileged spacetime worms) (see Rea 2003 and Sider 2001b). While perdurantism can offer solutions to this conflict, it does so at the cost of commitments regarding the philosophy of time (i.e., to eternalism rather than presentism). Thus Philosophers who adopt theories of constitution are often motivated either by a commitment to presentism. Alternatively, they can be motivated by an appeal to the supposedly Moorean fact that at each moment none of our parts seem to be missing.[285] (Though this objection only works against a particular form of perdurantism wherein ordinary objects are worms and not just *stages* of worms.)

However, this does not really affect our point. First of all, we have already seen that the dispute between presentism and eternalism must rest on a disagreement about intuition. Second, many of the objections leveled at constitution theorists clearly presuppose the plausibility of something like (AP4). The perdurantist will often cite the fact that her theory avoids co-location as one of the points in favor of it, especially in comparison to constitutionalism (e.g., Sider 2001b, 140-188).[286] Since the constitution theorist is commonly expected to provide some explanation for how her theory avoids violating (AP4), it is clear that (AP4) still plays a large role in the dispute. Nevertheless, we should expand our discussion to other intuitions.

[285] "Three-dimensionalists think we persist in virtue of being *wholly present* at each time. It seems, they claim, that we can just tell that we're wholly present — we can look down and see: none of us is missing, all of our parts are here." (Kleinschmidt 2011, 255) However, note that it is possible to be an endurantist eternalist if one thinks objects are wholly (i.e., multilocated) at different times rather than being "spread across" different times: "A three-dimensionalist who thinks non-present objects exist is going to posit multilocation of persisting objects through spacetime — those objects will be wholly present at more than one spacetime region. They are multilocated across time." (ibid) If one adopts such a view, then the problem of co-location will still arise.

[286] Perdurantism has the benefit of individuating objects diachronically: there are two distinct spacetime worms which happen to have their temporal parts coincide at certain points (the statue and lump are either the worms themselves or stages of these worms). According to the perdurantist there are never co-located objects in the sense that would seem to run afoul of (AP4). By restricting one's focus to a specific point in time one will only see *one* mereological sum that is a temporal part or stage of both the statue and the lump.

Even if we put aside issues of persistence and the attendant complexities, it is still clear that intuitions like those behind (AP4) play a significant role in mereology. The general problems of colocation that motivate both perdurantism and theories of constitution arise from a more general principle that the whole is "nothing over and above" its parts. The principle is hard to define. Sider describes the problem as follows:

> "The whole is nothing over and above the parts"—this slogan animates many theories of parthood, is intuitively compelling, and is arguably central to our ordinary conception. Yet it seems to make no sense. As I understand it, the slogan says that an exceedingly intimate relationship holds between a whole and its parts: in some sense the whole is *no different from* its parts. But how can that be? I am just one thing; my head, arms, legs, and torso are more than one in number; so how can I be "nothing over and above" or "no different from" them? (Forthcoming-c, 1)

The influence of this principle can be seen in the fact that the system ***M*** as described above is taken to be in need of modification in order to 'truly capture' the nature of the parthood relation. It is in the various proposed modifications to ***M*** that intuition comes to play a heavy methodological role in mereology. Following Varzi, we can identify two dimensions along which ***M*** might be modified through the addition of further principles:

> On the one hand, one may extend **M** by means of *decomposition principles* that take us from a whole to its parts. For example, one may consider the idea that whenever something has a proper part, it has more than one—i.e., that there is always some *mereological difference* (a "remainder") between a whole and its proper parts. ... On the other hand, one may extend **M** by means of *composition principles* that go in the opposite direction—from the parts to the whole. For example, one may consider the idea that whenever there are some things, there exists a whole that consists exactly of those things—i.e., that there is always a *mereological sum* (or "fusion") of two or more parts. (Varzi 2014, §3.0)

It is worth considering both ways of modifying ***M***, as this will illuminate the role that intuitions play in mereological theory.

The same sort of first-order *de re* grounding intuition underlying (AP4) above simultaneously motivate the decompositional principles and undermine the compositional principles. One reason why ***M*** is taken to be insufficient to capture our common concept of parthood is that it does not capture various intuitions about how objects decompose into proper parts. Thus ***M*** is usually modified by what are commonly called 'supplementation' axioms, which are supposed to capture the intuition that an object cannot have a single proper part (Varzi 2014, §3.1). In order to see just how central the role of intuition is in mereology it is worth considering the various formulations of this principle provided by Varzi (2014, §3.1-.3). In order of logical strength:

Company (Comp): $x \ll y \supset \exists z(z \ll y \wedge \sim z=x)$[287]

Strong Company (S-Comp): $x \ll y \supset \exists z(z \ll y \wedge \sim z<x)$[288]

Strict Supplementation (P-Sup): $(x<y \wedge \sim y<x) \supset \exists z(z<y \wedge \sim O\langle z,x\rangle)$[289]

Quasi Supplementation (Q-Sup): $x \ll y \supset \exists z \exists w(z<y \wedge w<y \wedge \sim O\langle z,w\rangle)$[290]

Weak Supplementation (W-Sup): $x \ll y \supset \exists z(z<y \wedge \sim O\langle z,x\rangle)$[291]

Proper Supplementation (PP-Sup): $x \ll y \supset \exists z(z \ll y \wedge \sim O\langle z,x\rangle)$[292]

Strong Supplementation (S-Sup): $\sim y<x \supset \exists z(z<y \wedge \sim O\langle z,x\rangle)$[293]

Complementation (C-Sup): $\sim y<x \supset \exists z \forall w(w<z \equiv (w<y \wedge \sim O\langle w,x\rangle))$[294]

[287] 'If *x* is a proper part of *y*, then there is some *z* that is distinct from *x* that is also a proper part of *y*'.

[288] 'If *x* is a proper part of *y*, then there is some *z* that is not a part of *x* that is also a proper part of *y* '.

[289] 'If *x* is a part of *y* and *y* is not a part of *x*, then there is some part of *y* that does not overlap with *x*'.

[290] 'If *x* is a proper part of *y*, then there are two non-overlapping parts of *y*.'

[291] 'If *x* is a proper part of *y*, then there is also some part of *y* that does not overlap with *x*'.

[292] 'If *x* is a proper part of *y*, then there is also some proper part of *y* that does not overlap with *x*'.

[293] 'If *y* is not a part of *x*, then there is some part of *y* that does not overlap with *x*'.

The most commonly accepted modification of ***M*** in the literature, which Varzi calls *Minimal Mereology*, or ***MM***, accepts (W-Sup) as the proposed axiom (2014, §3.1). Intuitions play a heavy role in the evaluation of these axioms.

These axioms are clearly supposed to capture the "nothing-over-and-above" principle. For instance, Varzi states that these axioms are supposed to capture the idea that "whenever something has a proper part, it has more than one—i.e., that there is always some *mereological difference* (a "remainder") between a whole and its proper parts" (2014, §3.0). However, the problem is that we do not yet have a precise definition that will capture the notion of a "remainder". We might try to capture the idea straightforwardly by simply identifying the "remainder" as another proper part, which yields (Comp).[295] But (Comp), while literal, does not capture the idea. To see why, we need to consider various mereological models:[296]

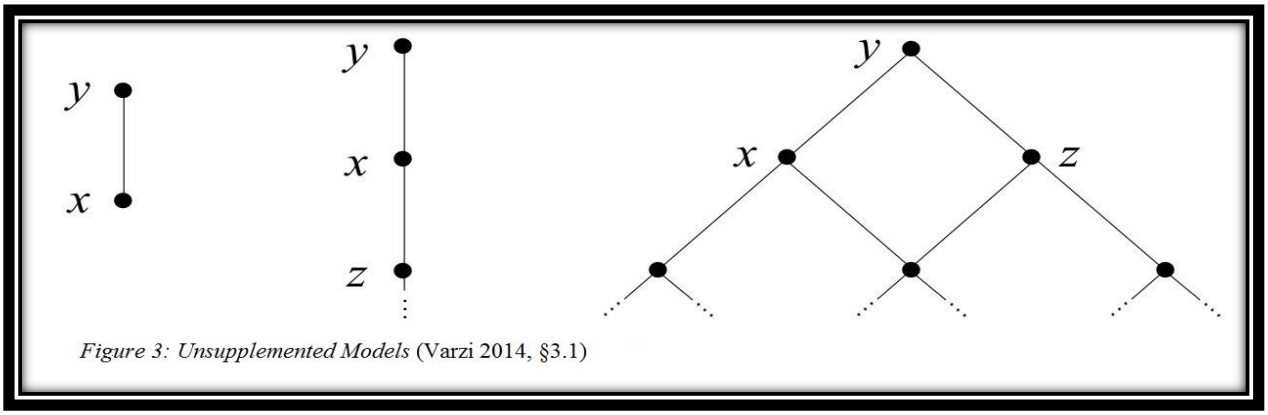

Figure 3: Unsupplemented Models (Varzi 2014, §3.1)

The problem with the two models on the left is that there is nothing left of *y* if one

[294] 'If *y* is not a part of *x*, then there is some further thing *z* that is comprised only of those parts of *y* that do not overlap with *x*'.

[295] As Varzi puts it, if all these axioms are supposed to capture is the "intuition according to which a whole cannot be decomposed into a single proper part", then (Comp) "is a literal rendering of the idea in question: every proper part must be accompanied by another" (2014, §3.1).

[296] *Fig.1-Left: y* has only one proper part, *x*, which has no other parts of its own. *Fig.1-Middle: y* has one proper part, *x*, which has one proper part, *z*, which has one …. etc. *Fig.1-Right: y* is decomposed into an infinite chain of proper parts that overlap at every stage.

"removes" *x*. The problem with the right-hand model is the parts of *y* all overlap one another such that one cannot "remove" all of *x* without removing *z*, and so on.[297] (Comp) manages to eliminate the left-hand model, but still fails to eliminate the middle one, which is taken to be just as unacceptable. (S-Comp) manages to eliminate both the left and the middle, but still allows the right-hand model. (W-Sup) avoids all three.

What these models violate is the intuition that the whole is "nothing over and above" its parts.[298] In general, this principle can be captured most directly by a principle governing the extensionality of proper parthood:

(**Extensionality**): $\Box\forall x\forall y[((\exists z(z\ll x) \vee \exists z(z\ll y)) \supset (\forall z(z\ll x \equiv z\ll y) \equiv x=y)]$[299]

The intuitions behind (Extensionality) are really no different than those behind (AP4). So long as one thinks that the only proper-parts a thing can have are material and that the statue and the lump share all of their material parts, (Extensionality) eliminates the possibility of co-location.[300] It is common for mereologists to reject any system that violates (Extensionality). What is at issue is a question of *de re* grounding. The question is: what is it that grounds the difference between *y* and the things of which it is composed in the above models? For instance, after elaborating a particular mereological system that does this Karen Bennett concludes with the following remarks:

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[297] As Varzi puts it, "the idea is that the removal of a proper part should leave a remainder, but it is by no means clear what would be left of *x* once *z* (along with its parts) is removed" (2014, §3.1).

[298] The problem with the figures on the left is that it is difficult to see what there is to *y* that is not already "contained" in *x* (or whatever the chain bottoms out in in the middle figure). Since there is nothing to *y* except its parts, it makes no sense to say that it only has one proper part. If there is nothing to *y* "over and above" *x*, then what makes *y* different from *x*? Where does the extra substance come from that makes *y* distinct from *x*?

[299] Roughly, 'If *x* and *y* are composite objects with the same proper parts, then *x* = *y*.' (Varzi 2014, §3.2)

[300] Thus perdurantist worm-theorists, hylomorphists (i.e., someone who holds that things also have their shape or form as a non-material part), and anyone else who takes objects to have more than just material parts can embrace (Extensionality) and still hold that the statue and lump are distinct without running into any difficulty.

> So, facing the question head on, do I honestly think that objects can have a part twice over? No. … I am not inclined to believe that objects can have parts twice over for the same basic reason that I am not inclined to believe in spatio-temporally coincident objects more generally: the grounding problem. I do not understand how objects like c and d can differ mereologically, but in no other way; I do not like bare mereological difference any more than I like bare modal difference. (2013, 102-3)[301]

It is not as easy to see that (Extensionality) *must* be backed by some form of intuition. It is easy to see that (Extensionality) cannot be backed by empirical evidence, and is also clearly too abstract to be backed by some form of Mooreanism. What is not as clear is whether (Extensionality) may have a more general abductive backing. While positing (Extensionality) does ensure a more limited ontology, the principle clearly has other costs (such as the fact that it has trouble dealing with cases of co-location). Moreover, it is important to note that (Extensionality) is a *preventative measure*. Even if one denied (Extensionality), this would not thereby *commit* one to the existence of composite objects that one finds objectionable. Thus it is unlikely that (Extensionality) can be backed solely by its effect on ontological parsimony. Nevertheless, (Extensionality) does provide one with an increase in expressive power (i.e., it allows one to derive an identity claim from other facts).[302] However, insofar as mereology is supposed to be a metaphysical tool for describing physical reality, it may be that the increase in power is not worth the cost in flexibility.

[301] Here Bennett is alluding to the following model that violates supplementation principles where there are only four objects and only the following parthood relations hold: a≪c, b≪c, a≪d, b≪d, c≪d (Bennett 2013, 84-5). Thus Bennett explains the objection as follows: "that d is a rather odd bird; it is the fusion of a composite object with its own parts. Indeed, the classical extensional mereologist will deny that the case is possible at all. She will claim that there can be no such d, distinct from c; there are only three objects in w, not four. … Her basic objection here is that there simply isn't enough to d. How can d have c as a proper part, when it has no parts that are disjoint from c? And how can c fail to have d as a part, given that d has no parts disjoint from it?" (2013, 84)

[302] The corresponding principle of extensionality in set theory is needed in proofs. Certainly some authors have intended to use it for this purpose; notably, Lewis 1991. The question is whether mereology in metaphysics is needed to serve the same purpose as set-theory.

In fact, it would appear that (Extensionality) *must* be backed by formal considerations rather than by intuition. The reason for this is simple: the same "nothing-over-and-above" grounding intuitions that appear to back (Extensionality) *also* back mereological nihilism. The mereological realist claims that there is *one* thing that is not identical to any of the parts that make it up. The nihilist only says that there are pluralities of simples. What is there to the object over and above the things that compose it? When one attempts to imagine this difference one is left with visualizations that have the same phenomenology as the intuitions behind (AP4). It is simply inconsistent to use one set of intuitions as evidence while dismissing the other, given that there seems to be no real phenomenological difference between them.

2.4.3. Intuitions and Semantic Compatibilism

Recall Williamson's response to the Moorean objection in 2.4.1. In general, this strategy is called *semantic compatibilism*. Before we move on to our next case, it is worth considering semantic compatibilism more fully, as it reveals the true methodological role of intuition. The original phrase is due to John Hawthorne and Michaelis Michael, who describe the strategy as follows:

> Quite often a metaphysician will propound some thesis that appears to conflict with claims that are commonplace in everyday life. Sometimes she will take that conflict at face value. Sometimes, however, she will attempt to explain away the apparent conflict by recommending that we interpret the ordinary remarks that seem to sort ill with her theory in a way that renders them compatible with it. Call any such attempt to reconcile an apparent conflict between ordinary talk and high theory a 'compatibilist semantics'. (Hawthorne & Michael 1996, 117)

Semantic compatibilism is a common strategy in which metaphysicians attempt to square their theories with common sense by arguing that the claims in question should not be taken as expressing propositions that would actually conflict with their theory.

Compatibilist strategies play a significant role in every single one of our examples.[303] This should hardly come as a surprise. A large part of the evidence for or against a theory consists in how it fits with and predicts the data.

The first and most common version of semantic compatibilism can be called *semantic exceptionalism*. The exceptionalist recognizes that the certain set of claims that are taken to conflict with the theory in question should, according to the standard compositional semantic treatment of the sorts of claims in question, be interpreted as expressing a proposition that does in fact conflict with the metaphysical theory in question. However, the exceptionalist argues that the particular claims in question differ from normal claims with the compositional structure due to the presence of a common semantic or pragmatic phenomenon, which undermines the standard interpretation in the cases in question. In other words, the claims relevant to the metaphysical theory in question are the exception to the semantic rule. For instance, some forms of exceptionalism include attributions of loose or metaphorical talk, the presence of fictional pretense, or pragmatic focusing effects. The appeal to restricted quantifiers is one

[303] As Hawthorne and Michael put it, semantic compatibilism is "very much orthodoxy in the metaphysics of the Anglo-American tradition" (1996, 117). They offer the following examples: "nearly all those who at some time subscribed to the theory that reality could be captured using statements about sense data (including counterfactuals about them) felt obliged to search for a compatibilist semantics for workaday discourse about tables and chairs; nominalists in this century have invariably sought to reinterpret those bits of ordinary talk that apparently quantify over abstract objects; accounts of human freedom have more often than not refused to take the apparent conflict between determinism and everyday claims of the form 'I could have done otherwise' at face value; those who propound a four-dimensional view of material objects have felt obliged to reconcile that theory with the apparent three-dimensionalism of ordinary discourse" (ibid). On top of these examples we can also add that eternalists will need to use some compatibilist strategy to deal with the fact that natural languages tend to use tensed quantifiers, while presentists will need some way to deal with claims about the past and future. Moreover, in their article Hawthorne and Michael themselves are concerned with evaluating a particular compatibilist strategy employed by Peter van Inwagen to make his form of mereological nihilism more amenable to common sense. Hawthorne & Cortens 1995 and Horgan & Potrč 2008 provide examples of compatibilist projects that are similar to van Inwagen's. For explicit arguments against various forms of compatibilism, see Hirsch 2002a; 2004; and Korman 2008; 2009; 2010; 2013. Unfortunately, we will not have the time to consider the various objections provided by Hirsch and Korman in any significant detail.

common form of exceptionalism.[304] Part of the exceptionalist's evidential burden concerns the semantic theory itself. Thus the exceptionalist must provide a well-developed formal framework for her semantic theory and must show that this theory successfully captures the relevant phenomenon in natural language. The exceptionalist must also show that her account is well motivated by showing that the phenomenon in question is significant enough to demand its own separate treatment, and that it is present in the disputed cases.[305]

Another form of semantic compatibilism may be termed *ameliorative hermeneutics*. Unlike the exceptionalist, the ameliorative hermeneuticist does not attempt to argue that the claims in question are literally true. Rather, the hermeneuticist sticks to the standard account of compositional semantics for the claim in question and recognizes the claims to literally express propositions that are false (or even meaningless). However, what makes the hermeneuticist a compatibilist is that the hermeneuticist argues that the literal semantic meanings of the claims in question are not important, but rather some other related claim.

[304] Note that it may actually be interpretations that use *unrestricted* quantifiers that break the norm, since the majority of the relevant *pure* quantificational claims may be tacitly restricted according to context (e.g., "there is nothing in the fridge"). Impure quantificational claims use a categorical predicate to automatically narrow their scope; e.g., "there aren't any golden mountains". In this case Williamson's job becomes incredibly easy, because a merely possible non-concrete golden mountain obviously would not fall under our categorical conception of a golden mountain (2013a, 10-14). If this is the case, then the burden of proof will actually fall on those who demand that quantificational claims of natural language be automatically given a unrestricted interpretation. In other words, it is questionable whether Williamson's strategy even need be considered a form of exceptionalism.

[305] For instance, this is the position adopted by Thomas Hofweber, who argues that apparently quantificational utterances regarding propositions, numbers, and properties are actually the result of focusing effects, and should thus be interpreted with a substitutional quantifier (2009). In order for Hofweber's account to succeed he must (1) provide a formal account of substitutional quantification as well as an abductive argument that the increased semantic complexity will not have burdensome costs, (2) provide evidence that the utterances in question do in fact display the sort of phenomena that he attributes to them, and (3) show that it is worth it for our best semantic theories to sacrifice their simplicity in order to treat utterances that feature the relevant phenomena differently.

The hermeneuticist has two ways of doing this. Her first option is to argue that the propositions that should be taken to constitute the data against which the theory in question is weighed are *not* the propositions that are literally expressed by the speaker's utterances, but rather the propositions expressed *pragmatically* according to the speaker's intentions. This option will require the hermeneuticist to show at least two things: (1) that a reasonable semantic theory can afford to treat the statements in question as false while still abiding by reasonable principle of charity, and (2) that the speakers in question *do* in fact have the intentions that she attributes to them. Because of these constraints, this option will not be open to the hermeneuticist for many claims.

The hermeneuticist's second option is to argue directly for the claim that while the speaker's utterance is literally false, this utterance can be replaced by a reasonable paraphrase that preserves the same important truth conditions but does not conflict with the metaphysical theory in question.[306] However, it is difficult to pin down exactly *why* a paraphrase helps matters. One option is to try to argue that the statements in question only have the particular syntactic form they do for extraneous reasons (e.g., to avoid awkwardness), and that the paraphrase expresses the proposition that captures the important part of what is said. Another option would be to claim that the paraphrase is synonymous or analytically equivalent to the normal claim. However, this option is

[306] Readers will perhaps recognize this as the primary strategy for Quinean ontology (see §2.2.1.2). However, there is some question as to whether the Quinean actually takes there to be a fact of the matter about the compositional semantics of utterances. Thus the Quinean demand for paraphrase *may* be understood as a form of metasemantic liberalism (which we shall discuss immediately below). That is, the Quinean may hold that if we can provide a paraphrase in our "home" language, then this is good enough to show that the *literal semantic* meaning of the utterances in question are metaphysically innocent. For example this appears to be van Inwagen's strategy for making his version of mereological nihilism more amenable: "I am not proposing an analysis of common language. I am offering a metaphysical theory. The only thing I have to say about what the ordinary man really means by 'There are two valuable chairs in the next room' is that … this sentence is sufficiently empty of metaphysical commitment that the proposition it typically expresses is consistent both with [mereological nihilism and realism]" (1995, 106-7).

unlikely to work given classical notions of analyticity if the paraphrase in question is highly technical in nature (as it quite often will be).[307] The biggest problem with the paraphrase option is simply that it is quite difficult to provide a paraphrase that will accomplish the necessary task.[308]

The difference in cost between semantic exceptionalism and ameliorative hermeneutics largely boils down to considerations regarding the distinction between semantics and pragmatics. There are some theorists who hold that semantic theories should operate by strict and simple rules such that they will be extremely insensitive to contextual variance or intention, while allowing pragmatics to take up much of the burden regarding communication (e.g., Cappelen & Lepore 2005). Given this assumption, ameliorative hermeneutics is quite reasonable while exceptionalism becomes unreasonable. If one is willing to adopt complex semantic theories to accommodate intention and contextual variance, then exceptionalism becomes the better bet. However, given how difficult it is to provide a paraphrase, most contemporary authors lean towards exceptionalism.

However, there is also a third option, which we may call *metasemantic liberalism*. Recall from our discussion of superficialism above (§2.2.2.1) that the nature of the non-semantic facts that are selected will have important metametaphysical implications. In particular, metasemantic views can be placed on a scale from views in which it is the actions and cognitive states of human beings that do most of the work in determining

[307] Note also that while (for obvious reasons) this strategy is not open to the historical Quine, this won't stop "Quinean" ontologists who accept some version of the analytic/synthetic distinction.

[308] As a notorious and illustrative example of this, see the dialectic concerning the existence of holes in Lewis & Lewis 1970. This option simply won't work for certain disputes without an incredible amount of intellectual labor (e.g., the nominalist strategy proposed by Field 1980).

semantic content, to views in which it is the structure of the world that does most of the work. Metasemantic liberalism is a position that falls somewhere in the middle of the scale. Metasemantic liberalism holds that the primary uses of terms should be given more metasemantic weight than theoretical uses, such that even if many of the more abstract claims that people make with the relevant terms turn out to be false the primary uses will still come out true so long as the world contains facts that make statements featuring a specific word accurate. For instance, the metasemantic liberal will say that sentences such as "Abagail has fallen ill" uttered in medieval societies will be true even if the people in medieval societies also commonly assent to the claim "Illness is caused by demonic possession" and "Abagail has fallen ill because she is possessed by a demon". This is because the term 'ill' has a primary function of conveying information about a person's health. Thus the term is fixed onto the actual cause of the decline in health, rather than what speakers *think* is the cause. However, this will obviously vary according to the term. Thus more technical terms such as 'cancer' will depend more upon speaker's background theories *precisely because* 'cancer' was introduced and is used primarily as a *theoretical term*.[309]

For instance, Sider endorses a form of metasemantic liberalism when defending his form of mereological nihilism against Moorean objections:

[309] Note that direct reference theories such Saul Kripke's causal theory of proper names counts as a form of metasemantic liberalism insofar as they allow speakers to be largely ignorant about the referents of their terms, while the descriptivist theories that Kripke argues against count as forms of metasemantic conservatism insofar as they require reference to be determined largely by speakers' background knowledge (1981). However, these should only be understood as *specific species* of metasemantic liberalism and conservatism. Thus one need not presuppose anything like Kripke's causal theory of proper names in order to be a metasemantic liberal. Indeed, Kripke's metasemantic theory claims that the referent of proper names is fixed by the context in which terms are introduced along with causal considerations, and is then passed on by a chain of speakers where each speaker intends to use the term in the same way as the last. This heavy focus on an initial baptism event presents problems for Kripke's theory regarding gradual change in reference. In contrast, the metasemantic liberal can take a synchronic view about the *current* important uses of the term in question that can avoid some of these difficulties.

> Whether [mereological] nihilism allows these sentences [about tables, chairs, etc.] to be true turns on a difficult issue in metasemantics. … The difficult issue in metasemantics is this: how distant from our ordinary ways of talking can the underlying facts get, before what we say counts as false? … the general issue of how much mismatch it takes to undermine truth is a hard one. Now, one response to the Moorean argument against nihilism would be to take a stand on this hard issue, argue for a liberal conception of when ordinary sentences are true in a hostile metaphysical environment, and conclude that nihilism doesn't after all conflict with common sense. (2013, 254-5)

Mereological nihilists attempt to provide paraphrases of the appropriate facts using plural quantifiers (e.g., 'There is a table' is paraphrased as 'There exist simples arranged tablewise'). There are two ways in which one can attempt to relate these paraphrases to the sentences of ordinary English. First, one can disregard the compositional semantics for ordinary language altogether, and instead treat these paraphrases as descriptions of the truthmakers for ordinary claims. This is the approach that Sider embraces.[310] Sider disregards how his view interacts with a formal semantic treatment of ordinary English altogether by arguing that ordinary English simply does not use the quantifier that expresses the fundamental notion of existence relevant to ontology (2013, 248-253). In other words, when Sider endorses mereological nihilism he is speaking a different language (ontologese), so there is no real conflict between his claims and natural language. In fact, according to Sider, his theory shouldn't have *any* effect on the application of compositional semantics to natural language, because the goals of linguists are better suited by using semantic theories that employ non-structural quantifiers.

Sider draws a distinction between the project of natural language semantics and the special project of "metaphysical semantics", where the latter is concerned with

[310] In regard to van Inawgen, see footnote 306 above. In contrast, van Inwagen appears to hold that there is only "one true quantifier" and that it is what is expressed by sentences in English (see footnote 173 above). Given how prominently this commitment features in van Inwagen's metaontology, he appears to be in a much worse position than Sider.

explaining what makes sentences of natural language true in fundamental terms (2011, 112-5). The general idea behind Sider's form of metasemantic liberalism is simply this: there is no particular reason to think that natural language is adapted to track the fundamental structure of reality. When taken in this light, Sider's claims seem extremely plausible. After all, natural language had to develop naturally. While it is dubious whether one can account for the structure of natural language in terms of biological evolution, there is nevertheless an obvious sense in which one must describe natural language as undergoing evolution through selective processes. The point is that there is no reason to suspect that natural language will track anything *fundamental*. Rather, the selective pressures that operate on natural language aim its expressive capacity toward conveying information that is most relevant to human existence in a concise way. Thus it is questionable as to whether natural language developed to track metaphysical truths.

Alternatively, one can treat the paraphrase as being a more conspicuous way of expressing the same proposition that is expressed by the normal sentence, in spite of the fact that the sentences vary in their syntactical structure. That is, instead of treating the sentence 'There is a table' as expressing the proposition expressed by the formula '$\exists x(Tx)$', where 'T' means *is a table,* one instead treats it as expressing the proposition expressed by '$\exists xx(T{*}xx)$', where 'T*' means *are arranged tablewise*.[311] In essence, this amounts to the claim that we have been using grammatically singular terms to refer to pluralities. This approach has been largely overlooked until only recently, when it received an explicit defense by Gabriele Contessa (2014).

[311] The reason for the awkward circumlocution 'expressing the proposition expressed by' is simply to avoid presupposing a particular theory of propositions. Regardless of whether one treats propositions as sets of possible worlds, states of affairs, Russellian structured propositions, algebraic constructs, or *sui generis* abstracta, it will still be the case that logical formulas are easier to interpret than sentences of natural language.

If this semantic claim were true then the mereological nihilist could avoid the elimination of ordinary objects, as this would imply that ordinary objects *just are* simples arranged in certain ways.[312] One drawback is that both strategies will need to employ higher-order plural quantification (or 'perplural' quantification) in order to deal with sentences that plurally quantify over composite objects (Uzquiano 2004).[313] This counts as a cost in the formal elegance of the theories. However, it is a fairly minor one. Perplural quantification is a very general logical device that provides expressive power similar to higher-order singular quantification, so it is hardly *ad hoc*. The addition of perplural quantification provides these theories with much greater expressive power, which may outweigh the cost in simplicity.[314]

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[312] This would allow the apparent vagueness of ordinary objects to be absorbed by the irreducibly plural reference relation. As a helpful metaphor, imagine the reference relation as a fuzzy cone of light that illuminates the objects in question, such that one cannot tell precisely where the borders of the light cone end (i.e., one cannot tell whether the light encompasses borderline particles). Of course, this still leaves the question of how this vagueness is to be handled formally. But it is not obvious why the use of plural reference would be any more problematic in respect to accounts of vagueness than anything else.

[313] It is important to note that Uzquiano's criticism is directed specifically at van Inwagen. Van Inwagen explicitly embraces a view in the epistemology of logic in which devices in formal logic can only be understood if they are translatable into statements of natural language (1995, 23-8). Moreover, this claim plays an essential role in van Inwagen's metaontology, both in his argument that existence is univocal and in his proposed system of regimentation (2009). Thus Uzquiano's criticism is effective not because perplural quantification is independently implausible. Rather, since there are no clear cases of perplural quantification in English, van Inwagen's paraphrase strategy for nihilism must fail by his own lights. As Uzquiano puts it, "If English contained quantifier phrases, which, one could argue, behaved like plurally plural quantifiers, then perhaps that would be some evidence, albeit inconclusive, for the coherence of plurally plural reference. In the absence of such evidence, however, advocates of plurally plural quantification must first make it plausible that plurally plural quantification could intelligibly be introduced into the language. … I should hasten to add that these concerns do not provide us with conclusive reasons to reject the coherence of plurally plural quantification; they simply amount to the observation that quite an impressive amount of work must be done before one is prepared to admit plurally plural quantification into the philosopher's toolkit" (2004, 439 [my underlining]). For the metasemantic liberal, the perplural quantifiers are not being *introduced* into the language. Rather, our metaphysical inquiries have shown us that the deep referential structure of our language must be analyzed with perplural quantification. In other words, the perplural quantifiers were, in some sense, *already there*. Regarding Uzquiano's worry about understanding perplural quantification, we should simply reject van Inwagen's epistemology of logic: a rigorous formal treatment of perplural quantification by itself is enough to provide us with an understanding of the subject.

[314] Sider simply adopts a fundamental language that uses the '$\in$' operator of set theory to do the work of perplural quantifiers due to considerations of ideological simplicity (2013, 282-8). This has the odd cost

There is another worry that a significant cost of this strategy will also introduce plural and perplural universals. However, this would only be a cost if these universals were *additions* to an already vast number of singular universals. If mereological nihilism were true then the only singular universals would be those that applied directly to mereological simples. Moreover, following Lewis (1983a) it is already widely accepted that there must be a distinction between the natural/fundamental properties and non-natural/non-fundamental properties. In this case, the logic of properties *itself* would draw this distinction: the perfectly natural properties are the singular universals that adhere to mereological simples. This could actually be seen as a *benefit* of the view.[315]

Metasemantic liberalism differs from ameliorative hermeneutics insofar as it still claims that the relevant statements are true. Metasemantic liberalism differs from exceptionalism in that the former is a very general metasemantic position whereas the latter is a specific semantic position regarding a certain class of terms. Both positions must provide abductive evidence to back up their theories by showing that they are either compatible with the standard formal semantic approaches, or by showing that the proposed modification to these standard approaches is worth the tradeoff in simplicity. However, the exceptionalist must also back up her position with specific empirical

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of implying that sets are concrete. However, this "cost" is deflated once one realizes that the '$\in$' operator, along with all the other syntactic devices, would be implicitly defined by its role within Sider's fundamental language. Thus it should not be taken as expressing the regular concept of set membership any more than the quantifier of the fundamental language should be understood as expressing the regular notion of existence. However, Sider seems to overlook the fact that ideological simplicity is not simply measured in the number of symbols that one's theory possesses, but also by the axioms through which those symbols are implicitly defined. Thus in order for his fundamental language to function properly it will have to tacitly assume the axioms of set theory. This may end up being more of an ideological cost than the addition of plural and perplural quantification.

[315] Note that another common objection to nihilism is that the view is incompatible with the possibility of "gunky" worlds (i.e., worlds in which everything has a proper part). However, it is important to note that this incompatibility is not a *cost* of nihilism. Rather, the claim that gunk is possible constitutes a competing theory that is to be weighed *against* nihilism abductively. Sider provides such a comparison (2013, 270-282).

evidence that people use terms in a certain way. In contrast, the metasemantic liberal can largely ignore natural language semantics so long as she can give a plausible account of why natural language is not a good guide to the metaphysical subject in question.

It is important to draw a distinction between the simplicity of a program for natural language semantics and the simplicity of the metaphysical theory itself. The latter only influences the former if one presupposes that metaphysical theories should be intimately tied to natural language semantics. But, as Sider's example demonstrates, this is questionable. It would be equally difficult to attempt to translate theories in particle physics or advanced mathematics into natural language, but we generally do not take this as counting against these theories. Thus metasemantic liberalism has a significant advantage over the competing forms of semantic compatibilism. In order for an anti-compatibilist to argue against the metasemantic liberal, she must already provide some evidence that we should assume that metaphysical theories should be evaluated in terms of how easy it is to use them in natural language semantics. Currently, this latter claim appears to be nothing more than an unquestioned meta-philosophical dogma.

2.4.3.1. Semantic Intuitions

As our examples have demonstrated, the use of formal models in evaluating metaphysical theories has made it much easier to determine which theory has the advantage in terms of simplicity, elegance and expressive power. Given this fact, the continued existence of metaphysical disputes must ultimately boil down to disputes over data. As we saw in our examples above, simple Mooreanism by itself cannot hope to deal with semantic compatibilist strategies.[316] One option is to simply abandon Mooreanism

[316] As we have already seen given our previous discussions, the Necessitist can make the judgments behind (AP2) compatible with her theory by arguing that the quantifiers in this judgment are restricted to

and instead treat the relevant data as being established by appeals to intuitions about general metaphysical principles. In this case, our analysis in §2.4.2 above will be vindicated. Alternatively, the anti-compatibilist could appeal to philosopher's semantic insight. The claim would be that the relevant judgments cannot be subjected to compatibilist interpretations because they are made by philosophers who deliberately suppress any extraneous linguistic or conceptual phenomena that might interfere with folk judgments. However, the success of this response will depend upon the nature of the linguistic phenomenon that the compatibilist posits. The response will only work if it is reasonable to suppose that the philosophers in question *can* deliberately suppress the effect in question. This might work for exceptionalist strategies like quantifier-restriction, but it will not work against metasemantic liberalism.[317] The only way that the Moorean anti-compatibilist can hope to combat metasemantic liberalism is by claiming that we have direct intuitive insight into the semantic structure of natural language.

Recall our brief sketch of a formal mereological system above with the addition of mass terms. There are those who endorse the notion that stuff is irreducible to things. Most notably, this view is explicitly embraced by Ned Markosian, who takes "the notions

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concrete objects. The Platonist can argue that the Nominalist has inadvertently restricted her quantifiers when she claims that only concrete objects exist. The Nominalist can argue that the apparent quantification over abstracta is really just substitutional quantification. The Eternalist can argue that apparently tensed quantifiers in natural language are best interpreted in terms of an untensed quantifier that is suitably restricted according to context of utterance. In the same manner, an anti-stuff theorist can argue that mass terms should really be given a reductive semantic analysis in terms of some other device such as plural quantification.

[317] In essence, metasemantic liberalism allows one to put forward a metaphysical theory that is justified in virtue of fairly abstract considerations without worrying too much about how this view fits with ordinary language. Thus one might complain, to borrow a common turn of phrase, that metasemantic liberalism has all the benefits of theft over honest labor. However, this is not quite accurate. The use of perplural quantification in a potential "metaphysical semantics" still requires quite a bit of work. The key to metasemantic liberalism is its claim that there is no particular reason to think that the structure of natural language tracks the fundamental structure of reality. It is this claim that the anti-compatibilists must respond to.

of thing, stuff, and constitution to be unanalyzable" (2004, 407). Markosian continues on to endorse a substantive thesis regarding stuff: "*The stuff that constitutes a thing is distinct from that thing*" (2004, 409). We can capture Markosian's claims by modifying our open formal framework above as follows. First, our model would validate the syntactical distinctions between singular, plural and mass expressions as necessary. Second, all mereological axioms would need to be duplicated for mass terms.[318] Third, we require the addition of the following fundamental metaphysical relation:

Material Constitution: $MCon\langle x^m,y\rangle \equiv [Col\langle x^m,y\rangle \wedge \forall z(z<y \supset \exists v^m(v^m<x^m \wedge MCon\langle v^m,z\rangle))]$[319]

The resulting view incurs massive costs in simplicity. The addition of a stuff-ontology will result in (1) a massive increase in *de re* ontological complexity (by positing an additional fundamental ontological category). (2) A massive increase in *de dicto* quantitative complexity for both metaphysics and ontology insofar as it will involve far greater complexity regarding fundamental pieces of ideology such as identity, parthood and location. (3) A massive increase in *de re* metaphysical structure (by requiring additional fundamental relations between ontological categories).

What evidence could possibly outweigh the massive ideological and metaphysical costs of this view? The only available evidence is either a direct intuition regarding a general metaphysical principle that stuff is irreducible to things, or a semantic appeal to

[318] Markosian goes on to endorse two other major theses worth noting: "*The relation* of parthood *that relates a thing to a thing and the relation of* parthood *that relates a portion of stuff to a portion of stuff are the same relation*", and "*The relation* of occupation *that relates a thing to a region of space and the relation of* occupation *that relates a portion of stuff to a region of space are the same relation*" (2004, 411). This will require two sets of axioms for parthood and location.

[319] Thus Markosian describes the situation as follows: "Another important concept that I will also take to be unanalyzable is the relation of *constitution.* This is a peculiar and intimate relation that can obtain between a thing and some stuff. It is the relation that occurs when a particular thing is *made of* or *constituted by* a certain portion of stuff." (2004, 407)

the mass/count distinction in natural language.[320] However, this latter response falls prey to the same problems as the Moorean attempt to directly interpret English claims in the disputes above. How can the anti-compatibilists hope to combat these interpretive strategies? Sometimes they can argue that the interpretation is unmotivated and more burdensome than the standard one. However, this option simply is not available to the stuff-theorist or Presentist. In these cases, the only remaining response for the anti-compatibilist is to claim that philosophers have some special insight into their judgments such that they know immediately that the judgments in question cannot be subjected to the interpretive strategy in question.

The only way to accomplish this is through an appeal to *semantic intuitions*: intuitions that are somehow supposed to directly reveal the semantic structure of language. There are a couple ways of cashing this claim out. For instance, the anti-compatibilist will argue that we can *just see* that singular terms like 'table' *must* refer to only one thing if the sentences in which the term occurs are to be true. This would block the mereological nihilist's metasemantic liberal strategy of claiming that the terms in question refer plurally. Alternatively, the anti-compatiblist can argue that we can directly see the structure of our propositions. That is, we can just see that terms such as 'table' can only contribute a single entity to the proposition in question.

[320] Note that Markosian appears to stick to the principle primarily in order to escape objections to his view of simple. But Markosian appears to justify his view of simples rather explicitly through appeals to intuition about what should be possible (1998b, 219). Thus this does not really affect our point on a dialectical level. Nevertheless, there are independent arguments that the mass/count distinction should be taken seriously due to considerations in natural language semantics (e.g., Laycock 1972; 1975; 1998). However, a broader analysis across languages casts some doubt on this argument insofar as the importance of the mass/count distinction might simply be an idiosyncrasy of certain Indoeuropean languages, which would make it inappropriate grounds for a fundamental metaphysical distinction (Pelletier 2012). Note also that some might posit stuffs as a solution to problems of co-location (see Zimmerman 1995 for discussion) there are there are problems with this approach, as it will be subject to specific counter-examples that undermine its original motivation (Kleinschmidt 2007).

But is this reasonable? Even without engaging in theorizing about the nature of intuition, we can simply note that *if* such intuitions *were* possible then the philosophy of language would look very different. If we could simply tell what the structure of our propositions are, then why is there still a dispute about the nature of propositions? Indeed, the anti-compatibilist's intuitions would appear to immediately dismiss the possibility of theories on which propositions are coarse-grained sets of possible worlds. But these theories have been popular for quite some time. How can the anti-compatibilist explain the popularity of this view? Did the people who originally proposed this view simply lack the intuitions in question? This is deeply implausible. Similar remarks apply to the claim that we can have intuitions about how our terms refer. We can certainly have intuitions about the *grammatical number* of terms under the guise of sentences, but the semantic question of how these terms refer is an extremely technical one. As stated above, the metasemantic liberal already has arguments for why we should not expect the surface structure of our grammar to track metaphysical truths. The simple answer is that such intuitions are deeply implausible, and appear to arise only for metaphysicians when they are needed to combat an opponent's claims.

2.5. Concluding Remarks

At the end of Part I we saw that schismatic complacency is unacceptable. We have now seen that this is exactly what seems to have taken place in contemporary metaphysical disputes. Metaphysicians argue for their views by presenting detailed arguments showing that their opponents' views face internal difficulties. However, these sorts of arguments are almost never decisive. The real disagreement persists because different philosophers take different things as evidence. Take the dispute between presentists and eternalists. Eternalists may grant the initial intuitiveness of the presentist picture, but argue that other technical considerations—such as the overly complicated semantics for tensed quantifiers and the apparent incompatibility of presentism with Special Relativity—should be given substantially more weight. In contrast, many presentists appear to appeal to claim that their view is 'common sensical' (i.e., intuitive),[321] and consider this evidence to outweigh any of the supposed evidence provided by the eternalist that rests on technical considerations, even apparent conflict with Special Relativity.[322] This disagreement over the weight of evidence is what blocks

[321] The appeal to 'common sense' here cannot simply be Moorean because unlike a claim such as 'I have hands', the claim that only objects in the present moment exist is abstract and divorced from everyday experience. Thus the claim must ultimately be about intuitions. For instance, in the introduction to his defense of presentism, Ned Markosian states that "I endorse Presentism, which, it seems to me, is the "common sense" view, i.e., the one that the average person on the street would accept" (2006, 307 [my underlining]). Here the term 'would accept' must be understood as implying 'would accept if the issue were properly explained to them'. In contrast, the claim 'I have hands' is reasonably described as a claim that the average person on the street *does*, in fact, accept.

[322] For instance, Markosian argues that the *a priori* evidence in favor of presentism is enough to override the evidence for any version of special relativity that is not compatible with presentism (Markosian 2006, 327-8). (Note: Markosian's argument assumes that there is in fact some formulation of Special Relativity that is empirically equivalent to the theory employed by physicists but that does not make any claims that are incompatible with presentism. This is a controversial interpretive claim.) His objection the approach of Thomas Crisp in his entry on presentism in *Oxford Handbook of Metaphysics* when he states that presentism "is a natural position to take given certain metaphysical and linguistic commitments" that he takes "to accord well with the deliverances of 'common sense'", and then argues that "presentism is the only metaphysic of time consistent with these commitments" (2003, 215).

consensus. Unless there are some systematic standards governing the nature and degree of evidence that intuitions are supposed to provide, appeals to intuition can be used to support virtually any theory come what may.

We have seen that the only way to make sense of the dispute between necessitists and nontingentists, Nominalists and Platonists, and presentist and eternalist is through disagreement about how much evidential weight intuitions about general principles should get in relation to other considerations such as theoretical simplicity. There is still significant disagreement about these positions.[323] Contingentists, Nominalists, and presentists all need to rely heavily on these general principles in order to make their positions viable. One considerable problem is that it is difficult to even cash out many of these principles. Upon closer inspection, it becomes unclear what exactly the Nominalist and Platonist are arguing about, or at least *why* they are arguing about it. The dispute only makes sense if the Nominalist possess some reason to be restrictive about what exists. But it is difficult to see what this reason is, or even what it *could be*.

When we compare the different intuitions across disputes the picture becomes even worse. The default position in mereology seems to be extensionalism plus universalism (i.e., that for any physical things, there is a fusion of those things). This position would appear to violate not only common sense (insofar as common sense would appear to dictate that there is nothing composed of my left ear and Mount Rushmore), but seems to require that one abandon the sort of *de re* grounding intuition that might be used

[323] For instance, according to a recent survey, the percentage of philosophers who favor Platonism is around 39.3 %, while about 37.7% favor Nominalism, and 23.0% prefer some other option (Bourget & Chalmers Forthcoming, 11). The survey also revealed that only about 26.3% and 15.5% of philosophers favoring B-Theory and A-Theory of time respectively (Bourget & Chalmers Forthcoming, 13). However, these results might be somewhat deceptive insofar as they concern the judgments of the field as a whole, whereas we are more directly interested in the opinions of specialists.

to back (Extensionality) on pain of inconsistency, given that nearly the exact same intuitions can be used to support mereological nihilism. Why should intuitions lose their status in this particular debate while maintaining it in the other debates? We have also seen that the apparent appeal to semantic intuitions to justify particular interpretations of natural language conflicts with practice in the philosophy of language.

One of the primary problems with Individualism in the case of intuition is that it allows philosophers to overlook the importance of disagreements over the *strength* of evidence. The binary dialectic between rationalists and skeptics restricts the dispute to whether intuitions have *prima facie* justification. Those rationalists who bother to defend the role of intuition in philosophy generally do not provide any means of determining *the degree of evidence* provided by an intuition in favor of a theory, nor do they elaborate how this evidence is supposed to be weighted in regard to other forms of evidence. Calculating the evidence provided by types of argument is straightforward in principle, because inference is essentially a formal matter.[324] In contrast, there seems to be no obvious general principles for determining the evidence provided by intuitions.[325] Indeed, there is no *prima facie* reason why different individuals can't treat the same intuition as

[324] These calculations tell one how the evidential weight of the premises is related to the conclusion in a way that determines how one should revise one's total evidence. Because arguments are inferential they can be understood in terms of conditional probabilities. Of course, it is important to realize that in these terms the arguments themselves should have a probability which relates to the probability of both the premises and the conclusion. If one can show deductively that an incredibly well-confirmed theory leads to a certain conclusion, this should either override the evidence one has against that conclusion or, if one's evidence against the conclusion is even more well-confirmed than the theory that leads to it, this should override the evidence that one has in favor of the theory. I take it for granted that something along these lines can be captured in probabilistic terms.

[325] On a first-order epistemological reading intuitions just seem to directly determine the evidential weight of individual propositions, much like perceptual experiences. But unlike perceptual experiences the evidence provided by intuitions is not repeatable: having the same intuition over again should not affect one's evidence. Moreover, one generally only gets a handful of distinct relevant intuitions in any case. Because of this, there cannot be any obvious inductive rules governing the evidential weight provided by intuitions. Intuitions are also unlike perceptual experiences in that they vary wildly in their content (e.g., some are about parthood relation, some are about reference, etc.).

having very different evidential strength.[326] There is no unified account of how intuitions are to be weighted in regard to other forms of evidence. Do they universally override empirical evidence? Nor is there any generally accepted account of how intuitions should be weighted other theoretical considerations such as simplicity. How much theoretical simplicity should we be willing to sacrifice to sustain our intuitions?

There is even internal inconsistency in how appeals to intuition are used as evidence, due to the fact that there are often competing intuitions between abstract principles and judgments about particular cases. Let's call the position that intuitions about general principles should take priority over intuitions about particular cases *Generalism*. For instance, a mereological Universalist will place greater emphasis on the intuitiveness of the axiom of unrestricted composition than she will on the supposedly counterintuitive results, such as the existence of trout-turkeys. Sider explicitly embraced this sort of Generalism in arguing for Universalism when he claims that "judgments about cases are often infused with irrelevant linguistic intuitions" and that a "more secure foundation [for mereology] comes from unabashedly metaphysical insight" (2007c, 88). (Note that this was before he became a nihilist.)

In contrast, Daniel Korman argues *against* Universalism by defending a form of *Particularism* about intuitions: i.e., the claim that intuitions about particular cases override intuitions about general principles.[327] Korman seems to think that the only explanation for why people like Sider favor Universalism must be because they endorse

[326] Given the non-inferential status of intuitions, it is reasonable to treat them simply as subjective probabilities on their propositional contents. This allows one to represent the variable evidential weight that individuals give to intuitions straightforwardly as differing probability distributions.

[327] "Particularism about a given domain of inquiry is the view that our intuitive judgments about cases in the domain are largely correct and that, when intuitive judgments about cases conflict with compelling general principles, the cases should in general be treated as counterexamples to those principles" (Korman 2010, 119-120).

some form of skepticism about particular intuitions about cases that is particular to the metaphysics of material objects.[328] However, Korman overlooks the possibility that Universalists like Sider have presupposed a background theory on which intuitions about general principles are privileged over ordinary judgments.[329] As far as I am aware, no one has produced a developed argument for either Generalism or Particularism.

The Universalist's intuition that unrestricted composition is true is clearly in conflict with particular intuitions about whether trout-turkeys exist. The intuition that a statue is destroyed when it is squashed down into an amorphous lump of clay is in conflict with the intuition that two objects cannot occupy the same place at the same time. The intuition that only the present moment exists clearly conflicts with the intuition that claims about dinosaurs must be made true in part because dinosaurs possess the properties these claims ascribe to them. The list goes on. While it is certainly possible to argue that—despite appearances—one of each of these contrary claims is not in fact intuitive, one can only get this argument by making substantial assumptions about the nature of intuition. The exact theory of intuition one uses to do this will obviously be motivated in large part by the results one wishes to get (i.e., by one's prior metaphysical commitments). Someone who favors Universalism over a theory on which composition is restricted will favor a Generalist theory of intuition. Someone who thinks the statue does not survive being squashed will favor a Particularist theory of intuition.

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[328] Korman points out that while philosophers in most domains (e.g., ethics, philosophy of language, philosophy of mind, etc.) happen to be particularists, this does not seem to be the case for the metaphysics of material objects (2010, 120). He then goes on to consider two different defeaters one might cite against particular judgments.

[329] Korman does not consider the fact that rationalists like Bealer and Chudnoff who defend intuition in metaphysics typically defend intuitions about abstract principles such as the axiom of choice in set theory. Formal mereology happens to be much closer to set theory than to a theory of commonsense objects, so one should expect it to be backed by similar intuitions.

Even if we put aside the fact that different researchers place different evidential weight on intuitions, we would still need a general account in order to evaluate individual intuitions. In particular, we need to be able to establish three things: (1) the proposition that serves as the content of the intuition in question; (2) whether the particular phenomenology of the mental state in question should count as an intuition, and (3) how much evidential weight this proposition should be given in virtue of the intuition that backs it. These questions can only be answered once we have a background theory of intuition. In particular, we saw in §2.3.1 that different background theories individuate intuitions according to different phenomenological traits. However, we also saw that these theories face different difficulties in accounting for intuitions in metaphysics.

The current use of intuitions can only support one conclusion: metaphysics is suffering from a bad case of schismatic complacency. These different parties all argue with one another, but their arguments are never settled because they continue to take very different things as evidence. Moreover, the current theories of how intuitions function either threaten to trivialize metaphysics or hold that intuitive evidence is too weak to legitimately serve its current role. Taken together, these various problems demonstrate that the use of intuitions in metaphysics faces significant methodological hurdles. The lack of objective standards regarding the use of intuitions is a problem for rationalist metaphysicians, regardless of whether they are personally justified in believing that their theories are true. If individuals vary in what they consider to be a valid method, they will build their theories in different ways. Assumptions will be piled upon assumptions, and eventually the resulting theories will not even be capable of making significant contact with one another, even if they want to.

Given these facts, there is only one reasonable conclusion: metaphysicians must stop appealing to intuitions. That is, at least until there is a good unified account that can be used to evaluate them. This has nothing to do with skepticism about intuition. Rather, it is simply a demand for methodological rigor. Research is not funded in order to help individuals in their pursuit of knowledge. It is a collective enterprise. Progress in metaphysics can only be made if the field remains disciplined. Progress can be made slowly but surely as long as metaphysicians continue to use rigorous methods. The problem is that the current use of intuitions is not rigorous, and has thus become a hindrance. This is not to say that intuitions can *never* be evidence, but rather that more work must be done before this can happen. The good news is that metaphysics already has plenty of evidence to work with aside from intuitions. Insight cannot supplant hard work.

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