**Experimental Philosophy and the Methods of Ontology[[1]](#endnote-1)\***

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Those working in experimental philosophy have raised a number of arguments against the use of conceptual analysis in philosophical inquiries. But they have typically focused on a model that pursues conceptual analysis by taking intuitions as a kind of (defeasible) evidence for philosophical hypotheses. Little attention has been given to the constitutivist alternative, which sees metaphysical modal facts as reflections of constitutive semantic rules. I begin with a brief overview of the constitutivist approach and argue that we can defend a role for conceptual analysis, so understood, in ontological disputes against both the general skepticism about the relevance of intuitions, and against the specific worries raised by experimental results. Finally, I argue that even if the constitutivist view is adopted, experimental philosophy may still have quite a useful role to play, though purely empirical inquiries cannot in principle do the ontological work alone.

What are the proper methods for attempting to resolve ontological and modal disputes—taken as disputes about what ontological sorts of thing entities of various kinds (properties, numbers, statues, symphonies) are, what their existence and persistence conditions are, and so forth? To what extent are these questions resolvable at all?

One venerable method of approaching such questions—that pursued in ordinary language philosophy and (arguably) phenomenology—is to go by way of conceptual analysis, considering, e.g., whether a variety of actual and hypothetical cases would be situations in which a symphony was created or the same person person survived.[[2]](#endnote-2) Although conceptual analysis became less popular in neo-Quinean metaphysics, there have been several recent attempts at reviving and defending conceptual analysis as a method for ontology (Jackson 1998, Chalmers and Jackson 2001, Thomasson 2007, McGinn 2011). But conceptual analysis, as a method for doing work in philosophy, has come under fire. Some have raised a sort of blanket skepticism that our intuitions—whether we find out about them through introspective means, or through the empirical methods pursued by ‘positive’ programs in experimental philosophy—can tell us anything about mind-external features of the world (see Pust and Goldman 1998, 179; Knobe and Nichols 2008, 6). More specifically, those engaged in a ‘negative’ program in experimental philosophy have found evidence of certain types of variations in people’s intuitive judgments which, they argue, undermines the idea that conceptual analysis may play a useful role in philosophical inquiry—or at least (what is often taken to be the same idea) undermines the idea that intuitions can count as evidence in philosophical debates.

In this paper I address a number of objections to the idea that conceptual analysis may give us philosophical knowledge. For simplicity, I will focus just on disputes that arise in ontology, regarding the natures, existence, identity, and persistence conditions of things of various sorts. Those arguing against conceptual analysis have typically focused on a model that pursues conceptual analysis by taking intuitions as a kind of (defeasible) evidence for philosophical hypotheses. Little attention has been given to an alternative model of conceptual analysis, which we might call the ‘constitutivist approach’, which sees constitutive semantic rules as *determining* the modal features of the objects, if any, to which we refer. I have argued for and defended the constitutivist view elsewhere (though not under that name)[[3]](#endnote-3) and I will not have space to argue for it again here. Instead, the current project is a defensive one.

In section 1 I will give a brief overview of the constitutivist approach and its consequences for methodology in ontology. In sections 2-4 I argue that we can defend a role for conceptual analysis, so understood, in specifically *ontological* disputes against both the general skepticism about the relevance of intuitions, and against the specific worries raised by experimental results. Indeed these results may be readily accommodated by the constitutivist. Finally, in section 5 I address the broader question of what role might be available for experimental philosophy in doing ontology, if the constitutivist view is adopted. I will argue that experimental philosophy may still have quite a useful role to play, though (if the constitutivist view is correct) purely empirical inquiries cannot in principle do the ontological work alone.

1. **Ontology and Conceptual Analysis**

It will be useful to begin with a brief overview of the constitutivist approach (for a fuller discussion see my 2007a and 2007b). The central argument for the constitutivist view comes from considerations about reference. Causal theories of reference have been popular for the last several decades, but they face two crucial problems that suggest that causal chains alone cannot determine whether, and if so to what, our terms refer. The issues apply to both singular terms such as names and general terms such as natural kind terms. First, as those raising the *qua* problem have argued, even given causal and contextual connections, it remains radically ambiguous which kind or thing, if any, our noun terms refer to.[[4]](#endnote-4) For there are always *many* things (and kinds) with which those who (aim to) ground the reference of a term are in causal contact (e.g. they may be causally related to a statue, a lump, a surface, many property instances, a time slice, etc.).[[5]](#endnote-5) Without some way of disambiguating, it remains totally unclear what the term refers to. Similarly, as I have argued (2007a, 38-44), we need some way to distinguish *whether* a term is or is not properly grounded. For there is always *something* (and some kind of thing) with which the would-be grounders are in causal contact (at least the surrounding air molecules, the ground, their clothes), and yet we want to allow that in some cases the attempt to ground the reference of a term fails.

As I have argued elsewhere (2007a, 38-44), for general nouns, we can solve both problems if we allow that sortal nouns are associated with certain kinds of rule. The first sort of rule needed comes from being associated with ‘application conditions’: conceptually relevant conditions that must be fulfilled if the term N is to refer (or be grounded) at all, which distinguish cases in which a term does and does not refer. The second sort of rule needed involves what I (2007, 40-44) call ‘co-application conditions’. Co-application conditions give conceptually-relevant conditions under which the term (if successfully grounded) may be used again to refer to the same thing, enabling us to say that ‘this N is the same as that N’. It is differences in co-application conditions that enable us to disambiguate, e.g., whether a term is to refer to a lump or a statue by *establishing* which sort of thing we are talking about. Even names, I have argued, must be associated with these rules, which establish what sort of thing the term is to refer to if it is to refer at all. For names, this is usually done by associating the singular term with a high-level sortal term determining what category of entity the name is to refer to, should it succeed in referring at all. In short, reference to individuals is ontologically disambiguated only to the extent that the terms we use (in attempting to refer to individuals) are associated with such disambiguating rules.

Together, these semantic rules for use fix the basic modal facts for the thing(s) the term is to refer to (should it succeed in referring at all). Since the application conditions establish when the term ‘N’ refers at all, they also establish under what conditions it’s true to say that *there is a N,* thus establishing existence conditions for Ns. Since the co-application conditions fix those conditions under which a referring term ‘N’ may be used again to refer to the same thing, they also fix the most basic identity conditions for Ns, and the two sorts of condition together fix the most basic persistence conditions for Ns. In short, then, the application and co-application conditions associated with a term fix the most basic modal properties of the object(s) to be referred to (should the term succeed in referring): their existence, identity, and persistence conditions. In doing so, they also establish the ontological category of entity the term is to refer to.

The approach has a number of important and controversial consequences. One is that since the associated conditions fix what ontological sort of entity we are to refer to, competent speakers (those who have mastered the relevant rules) cannot turn out to be radically and systematically mistaken in their judgments about whether or not, in various hypothetical circumstances, a symphony exists, a statue persists, and so on—for the semantic rules they have mastered are *constitutive* of the basic modal facts in question.[[6]](#endnote-6) This arguably makes some views, like a pure musical Platonism or the view that a statue can’t survive even minor restorative changes, implausible, as they apparently conflict with the rules of use for applying and re-applying terms for ‘musical work’ (which is not done before the act of composition) or ‘statue’ (which is reapplied after minor restoration) (see my 2004, 2005). Another consequence is that some ontological questions about, say, the precise conditions under which a statue survives, or how many notes may be changed while the same symphony remains, may be unanswerable if our concepts are not fine-grained enough to fix those issues (see my 2009 and 2007b).

But what’s most important for the present paper is the consequences this view has for modal epistemology and for *methodology* in ontology. For this view gives a strong role to (something like) conceptual analysis in doing ontology—at least in working out the existence, identity, and persistence conditions of entities of different kinds. For talk about what (metaphysical) ‘modal properties’ an object has, or what the ‘modal facts’ regarding it are, on this view, involves hypostatization out of modal truths about it. We can, for example, move from “this statue couldn’t survive a crushing” to “this statue has the modal property of not possibly surviving a crushing”. We may come to know metaphysically necessary truths, in turn, by beginning from linguistic competence—having mastered the constitutive semantic rules. (Though note that you may not be able to recite the rules even if you have mastered them, just as one may master grammatical rules without being able to recite them). Having mastered the rules, one may consider a range of actual and hypothetical cases, making use of that competence to determine whether the term would be properly applied (or co-applied) in cases like those. By using that sort of thought experiment one may gain a more systematic and explicit grasp of what the rules are. Working out the application conditions for our sortal terms gives us the means for knowing the most basic (conceptually but not empirically relevant) existence conditions for members of the kind. Discerning the co-application conditions for our sortal terms gives us the means for reading off the most basic identity and persistence conditions of members of the kind. In short, on this view (which I have elsewhere called “Modal Normativism” (2007b)), basic truths about what is metaphysically necessary are object-language expressions of constitutive semantic rules.[[7]](#endnote-7) As a result, we may come to know about an object’s modal properties, its ‘ontological status’, by starting from knowing the constitutive semantic rules governing the relevant term.

While this is the way of coming to know the most basic truths about what is metaphysically necessary, it is not the only way to come to know metaphysical modal truths. One may also consider what follows from the rules, either as combined with each other or with empirical facts. Empirical discoveries, moreover, may contribute to knowledge of more detailed, *derivative* modal facts. For example, it may be a conceptual truth (knowable by the above sort of conceptual analysis) that a painting cannot survive the destruction of the canvas and paint, but a derivative, empirically discoverable truth that it thus cannot survive temperatures above 451 degrees Fahrenheit.[[8]](#endnote-8) But the modal force of this claim still derives from the more basic modal truth that, *whatever* physical conditions destroy a canvas and paint are conditions that a painting cannot survive.

This approach to basic metaphysical modal knowledge sounds very like the methods of conceptual analysis employed by ordinary language philosophers and by phenomenologists engaged in ‘eidetic variation’. But the term ‘conceptual analysis’ must be understood carefully: possession of the concept (in virtue of mastery of the rules) is crucial to being able to undertake analysis of this sort. But all of this work may be—and by metaphysicians, typically is—done in the *object* language. We consider which cases would be cases in which *the same statue* would remain (we don’t think of it in meta-linguistic terms as considering: in which cases could we apply the term “same statue as that”). To put it in a slogan, as long as the work is done in the object-language, it is analysis *with* concepts (done by making use of our conceptual competence), but not *of* concepts. (The term ‘conceptual’ in ‘conceptual analysis’ functions more like ‘laser’ in ‘laser surgery’ than ‘heart’ in ‘heart surgery’). Since it is conducted in the object-language, it is world-oriented and can justly be thought to result in knowledge about the modal features of objects.

This gives us a non-mysterious way of understanding how we can come to know those basic metaphysical modal facts that figure in ontological debates, by moving from mastery of the rules to more explicit knowledge of what the rules are and an ability to convey the rules and their consequences in object-language indicatives. Indeed, I have argued elsewhere (2007b) that a view along these lines seems to be about our only hope for overcoming the long-standing and vexed problems of how we can acquire modal knowledge, given that we cannot apparently perceive or otherwise empirically detect what is necessary or merely possible—but only what *is.* To the extent that a great many ontological questions are tacitly (metaphysical) modal questions, it also follows that the primary methodology suitable for addressing questions in the ontology involves some form of *conceptual analysis*.[[9]](#endnote-9)

With that model of conceptual analysis on the table, let us now turn to look at the objections commonly raised against the idea that conceptual analysis may play a useful role in philosophical inquiry. Once we focus on this constitutivist model, the issues show up rather differently.

1. **Concepts versus Objects**

The first and most general sort of objection against using conceptual analysis in any sort of philosophical inquiry is this: why should we think that our concepts, the meanings of our terms, or our intuitions, provide any guide at all to (or evidence about) how things *really stand* with respect to the philosophical issue at hand? Why, for example, should our intuitions about what counts as ‘knowledge’, or our use of the term ‘knowledge’, be thought to tell us anything about when someone *really* knows something? Why should people’s concept of responsibility, and their willingness to apply and refuse it in various circumstances, be thought to tell us about the circumstances under which someone is really *responsible* for their actions? The relevant form of the objection for ontological issues is: why should people’s intuitive conceptual judgments about whether a ship has survived or perished, or whether songs are identical or distinct, be thought to tell us anything about the existence, identity, or persistence conditions of ships or songs?

It’s worth noticing that this is a line of worry that applies not only to conceptual analysts, but also to experimental philosophy: at least to that branch of experimental philosophy that is engaged in a ‘positive’ ‘extramentalist’ program (to adopt terminology in Alexander, Mallon and Weinberg (2010)) of using experimental data about subject’s intuitions to help provide evidence for or against philosophical positions about mind-external matters (e.g. whether we have free will, whether acts of a certain sort are right, whether A caused B, etc.). As Knobe and Nichols express the worry for the positive experimental philosopher: “How on earth could information about the statistical distribution of intuitions ever give us reason to accept or reject a particular philosophical view?” (2008, 6). So, positive experimental philosophers and defenders of conceptual analysis alike owe an answer to this sort of question.

But these discussions of the viability of conceptual analysis as a tool in philosophy tend to focus on one particular model: one that takes our *intuitions* about various cases to play an *evidential role,* providing (at least some) evidence for or against substantive philosophical views about, e.g., incompatibilism versus compatibilism, the nature of causation, or which moral theory is correct. Thus, as Pust and Goldman put it, “The question facing a philosopher… is how and why to treat spontaneous mental judgments… as evidence for a philosophical hypothesis” (1998, 179). Critics attack the idea that anything about us—our intuitions, concepts, or language use—could possibly be relevant evidence about any of our *extra-mental, extra-linguistic* targets of knowledge.

Even defenders of the method of conceptual analysis tend to take on board the idea that intuitions are supposed to serve as some sort of evidence or warrant—merely insisting that they provide (at least some) evidence or warrant regarding extra-mental facts, just as our perceptions are thought to provide warrant regarding how things stand in the extra-mental world. So, for example, Ernest Sosa takes the core question regarding the viability of conceptual analysis to be “Can intuition enjoy relative to philosophy an evidential status analogous to that enjoyed by perception relative to empirical science?” (2008, 237), and defends the view that intuition at least plays an ‘indispensible’ role, in which “intuitions are supposed to function like observations” (2008, 239).

But the idea that we have some faculty of ‘intuition’ analogous to perception that can provide evidence about extra-mental facts raises difficult questions about just what this quasi-observational faculty is, and how it is supposed to work. Certainly untutored intuitions about, e.g., what bridges will and won’t stand, at what rate objects will fall, or what one’s chances are of winning a bet, are notoriously inaccurate about these issues in the world; why think they are any better about specifically philosophical issues?

Some defenders of conceptual analysis expressly limit the evidential role of intuitions to modal issues—Sosa, for example, restricts the subject matter where intuition can be granted a role as evidence to *modal* propositions (2008, 233). But while this may avoid the problems for intuitions regarding non-modal matters of fact, it raises further difficulties of its own. First, what are these modal facts, and how are we supposed to detect them through intuition? Second, why should we think that intuition provides knowledge *only* of modal propositions? Sosa himself punts on the second question, writing “One might quite properly wonder why we should restrict ourselves to modal propositions. And there is no very deep reason. It’s just that this seems the proper domain for philosophical uses of intuition” (2008, 233)).

On my view the problem is not with the use of conceptual analysis, but rather with the whole model assumed above by both critics and defenders: with the idea that our intuitions provide some sort of ‘evidence’ or ‘data’ (maybe imperfect and defeasible) about some deep, extra-mental philosophical facts. This is far from the constitutivist view, and once the alternative is made clear, the issues show up quite differently.

On the constitutivist view explicated in section 1 above, we are not to take ‘intuitions’ as a source of ‘evidence’ or ‘data’ about ontological facts. Instead, on this view, the rules governing when a concept is to be applied, refused, and reapplied (to one and the same thing) are *constitutive* of what ontological sort of thing we are talking about or thinking about. Talk of what it takes for Ks to exist is just the object-language correlate of talk about when the concept or term ‘K’ applies, and talk of when a K persists is the object-language shadow of talk of when ‘K’ may be reapplied. So there is no worry that there might be slippage or inaccuracy between the rules of use for our term and the basic modal features of the objects (if any) referred to.

This provides a straightforward way to answer the general question: why should people’s intuitive conceptual judgments about whether a ship has survived or perished, or whether songs are identical or distinct, be thought to tell us anything about the existence, identity, or persistence conditions of the entities in question? To the extent that these judgments are reflections of people’s competence in wielding the relevant concept, they are also reflections of the rules of use for the relevant concept and term. These rules of use can ‘tell us’ about the conditions under which a ship (say) persists, or a song is identical to a(nother) song, etc. not because they provide some kind of relevant *evidence* or *data* based on some special insight into the ontological facts*,* but rather because they fix what ontological sort of thing, if any, we are talking about.

A position like this is in many ways attractive. One attraction is that it can provide a more robust answer to the question Sosa raised: why the proper role of intuitions should be restricted to *modal* propositions. The short answer is that modal facts are not to be understood on analogy with facts about color, size, charge, or other empirical matters of fact (about which intuitions might be worryingly fallible). Instead, the modal facts that metaphysics seeks to uncover are hypostatizations out of modal truths: to say ‘it is a modal fact that paintings cannot survive the destruction of the canvas’ is just a hypostatization out of the modal truth ‘paintings cannot survive the destruction of the canvas’ (for further discussion and defense of the idea that talk of modal facts involves hypostatization out of modal truths, see my 2007b). And stating modal truths is just a way of making these semantic rules (or their consequences) explicit, while using the object language—in this case, the rule that a painting name (or term like ‘same painting’) is not to be re-applied after the destruction of the canvas (even if, say, a similar pattern of colors appears on a poster). Speakers master these rules insofar as they are capable of judging correctly (in accord with these rules) whether a given noun term is to be applied or refused, reapplied or not, in various actual and imagined circumstances.

So we have reason to think that conceptual analysis, so understood, has relevance to discerning modal facts—particularly those facts about existence, identity, and persistence conditions that are central to issues about ‘ontological status’—that it lacks for empirical facts. This gives a much cleaner and more compelling answer to the question of why we should care about our concepts or thought about the subject: there’s no appeal to a special ‘faculty’ providing special ‘evidence’ about a special realm of properties; instead, talk about the relevant properties and facts involves hypostatizations out of modal facts, which are expressions of constitutive semantic rules made in the object language, using semantic descent (see my 2007b). Thus on this view it is no great mystery how our concepts or intuitive judgments guided by those concepts may provide a guide to how things stand modally and ontologically.

Moreover, we can show the relevance of conceptual analysis to ontology without having to explain what this special ‘faculty’ providing modal knowledge is, or how it manages to ‘detect’ the modal facts. For as long as we can come to grasp the semantic rules by engaging in conceptual analysis (making use of our mastery of a concept and considering whether or not it is properly applied in a variety of hypothetical circumstances), their relevance to the metaphysical modal issues is clear and guaranteed.

1. **Cultural Variation**

That provides a general defense of the relevance of conceptual analysis—at least to specifically modal inquiries in ontology. But those pursuing the ‘negative’ program of experimental philosophy have not just raised a blanket skepticism about whether intuitions count as evidence. Instead, they have given specific evidence to undermine the view that our intuitions provide warrant for beliefs about the relevant philosophical issues. As Weinberg and Crowley put it, negative experimental philosophers look for “evidence that the intuitions of the philosophers themselves may well be problematically sensitive to factors that will lead them away from the truths they aim for” (2009, 178).

The first and best known of these is the challenge raised by documented cultural variations in intuitions. So, for example, there has been observed to be a strong cultural variation in intuitions about whether (in a Gettier case) a subject has knowledge (Weinberg, Nichols and Stich 2001), or what (in a Gödel/Schmidt case) a name refers to (Machery, Mallon, Nichols and Stich 2004). Negative experimental philosophers have taken this cultural variation to undermine the idea that our intuitions provide warrant for our beliefs about these philosophical issues. As Knobe and Nichols put it: “Since it’s in some sense an accident that I had the cultural upbringing that I did, I am forced to wonder whether my intuitions are superior at tracking the nature of the world, the mind, and the good” (2008, 11).

Responding to this specific objection is tricky, as work in experimental philosophy to date has by and large not focused on ontological issues, so we simply do not have the data here to see whether our ontological judgments are culturally variable. Nonetheless, there may be good reason for suspecting that at least some ontological judgments would be found to exhibit cultural variation, if the appropriate studies were done. Dominic McIver Lopes (2007) provides a very detailed study suggesting that the concept of a work of architecture varies between Western and Japanese cultures. His focus is on Ise Jingu, a Shinto shrine about 100 kilometers southeast of Kyoto, which includes a structure, the *goshoden,* which has been rebuilt on an adjacent site (alternating extant building/future site) roughly every twenty years since the eighth century (2007, 79). This particular site, Lopes also argues, is not unique, but rather a manifestation of a rather distinct Japanese concept of architectural continuity, a “tendency in Japanese architecture to perpetuate architectural form without much concern for the actual building itself”, as part of a culture in which “craftsmanship is valued more than uniqueness and antiquity in an object” (2007, 80).

If asked about the architectural work before them, it seems plausible, a casual Western visitor (not internalizing the local conceptual scheme) might say that it is less than twenty years old, and will not survive the next cycle of rebuilding. But, according to at least one strand of discourse in Japan, the shrines “are identical with the ones that stood there at least as early as 685” (Burchard, p.9), and so are far far older, and will continue to survive the destruction of the current assemblage of beams and walls.[[10]](#endnote-10)

So, for the sake of argument, suppose there were such evidence, say, that English-speakers of Japanese and Western cultures had different intuitions about when a work of architecture persists (or that similar cultural variations were found regarding when a painting survived, when two musical works were identical, etc.). Would the presence of such cross-cultural variation undermine the idea that conceptual analysis may provide knowledge of the relevant ontological facts?

Not at all. Once we restrict our attention to modal/ontological issues, and give up thinking of intuitions as attempting to ‘track’ certain features of the world, matters show up very differently. For on the constitutivist view it’s not a matter of intuitions of one culture or another being superior at tracking deep modal boundaries; nor does the variation undermine the idea that both intuitions may be accurate. Instead, for each group, the relevant rules of use for the concept or term *determine* what it is that they are talking about. Different groups of people may construct different languages, with terms with different rules of use, according to their own distinct interests and practices,[[11]](#endnote-11) and these may entail that they are talking about entities with different existence and persistence conditions—even if they use the same syllables or phonemes. So, Westerners may use the term ‘work of architecture’ in such a way that it may not be reapplied after the original structure is dismantled; those of Japanese culture may use ‘work\* of architecture’ in a way that permits reapplication given a continuity of form and history in a sanctioned rebuilding practice (cf. Lopes 2008, 196). If so, then apparent disagreements between these two groups of speakers about the age of the work/work\* before them may be merely verbal disagreements, not substantive disagreements in which we must conclude that at least one set of intuitions fails to capture the ‘ontological facts’. Once we see the rules governing application of our concepts as determining what we are talking about, we can see why cultural variation in ‘intuitions’ about various cases is only to be expected, and see that apparent disagreements may not show the intuitions to be unreliable measures of the facts—for the disagreements may be merely apparent or verbal disagreements resulting from differences in our conceptual schemes. Such variations in conceptual schemes may appear across cultural divides even where speakers of both cultures would normally be said to speak ‘the same language’.[[12]](#endnote-12)

Now it is true that differences in intuitions about whether entities of specified sorts are identical or not, persist or not, may still be unsettling, and lead to a diminished confidence in our own judgments. This, however, shouldn’t be seen as a matter of taking the cross-cultural variations to undermine the idea that our ‘intuitions’ provide ‘warrant’ for our beliefs about such things. Instead, the differences revealed in our conceptual schemes may reveal the arbitrariness of our own conceptual scheme, prompt us to reexamine it and whether it indeed best serves our practical interests (of composing or enforcing copyright law, or assigning praise, blame, or punishment over time, of preservation and restoration of historical artifacts, etc.…), and may thus prompt revisions in our own conceptual scheme. But to say that evidence about how conceptual schemes vary may prompt us to *revise* our own is of course a very different matter from suggesting that such evidence must suggest that at least one group is mistaken, or that neither group’s intuitions provide ‘warrant’ for ontological conclusions.

Weinberg and Crowley consider a constitutivist approach to the use of intuitions in philosophy, describing it as an approach according to which “Rather than treating intuitions as more-or-less good sources of evidence about some intuition-independent philosophical truths, this alternative approach construes the intuitions as in part *constitutive* of the relevant truths, ” (2009, 180). But they take the fact that such an approach allows for accepting that there may be variation in our concepts to be a cost of the view:

If intuitions are both strictly constitutive *and* various (as indicated by the experimental findings), then there must be a similar variety of philosophical concepts. For example those who disagree about the attribution of knowledge in a Gettier case must thereby mean different things by ‘know’. In short, a strict constitutivism guarantees the accuracy of intuitions at the cost of considerable additional complexity in our philosophical theories. Instead of a single account of knowledge, reference, etc., we will have several, indeed perhaps a great many. (2009, 180-181).[[13]](#endnote-13)

If this is the only objection to constitutivism, we need not be too worried. That a view might entail that matters are complex hardly seems a reason for thinking it’s not true—on the contrary, we have every reason to think that things *are* complicated, and that our conceptual schemes vary (bringing with them variation in what—if anything—we are talking about), and the diversity of experimental results strongly suggests this.

A related threat behind this worry is this, however: that the conceptual variety is not only complicating, but might interfere with allowing that speakers with different conceptual schemes may communicate or talk ‘about the same things’. Thus, some might say, it is obvious that we and our Japanese hosts may communicate about the architectural work before us, and that when we discuss it, we are discussing *the same thing.* But if we must explain away disagreement by appealing to ambiguity in the term (or concept), we are forced to deny this, treating them as talking past each other and about different things just as those who speak of going to the (river) bank and to the (financial) bank do*.*

But the above account does not require that we treat our terms as systematically *ambiguous* (as in ‘bank/bank’). To the extent that terms vary in their rules of use, the modal features of the objects (if any) referred to may differ—whether slightly or radically, depending on how great the difference of rules is. So, to account for conceptual variation and the presence of verbal disagreements we don’t have to say that there are just lots of different, unrelated, concepts of works of architecture, painting, or music. We also need not deny that Easterners and Westerners can ever communicate about architecture, paintings or music, even though where the rules of use for terms vary, so will the ontological truths about the things referred to by those terms. The rules of American football and Canadian football differ. Nonetheless, we can in some sense say that they play ‘the same game’ North of the border: there are many commonalities as well as a common historical source (in rugby), and adherents of different rules may communicate effectively about many issues (e.g. about whether football is a more exciting game than baseball). Yet some disputes, e.g. about whether a team ‘really’ has three or four downs in which to advance ten yards, will turn out to be merely verbal, pseudo-disputes depending on which sort of football is under discussion. Similarly, if there is a culture in which competent speakers will gladly say that architectural works A and B are the same work even if B involves completely different materials assembled at a different time and location from those that compose(d) A, we needn’t say that the term ‘architectural work’ is ambiguous in the way that (river) bank/(financial) bank is. Nor need we deny that members of the two cultures often communicate perfectly well (about the work’s architectural grace and simplicity, exhibiting fine craftsmanship…), in order to accept that certain disputes (e.g. about whether this work is more than twenty years old) may be merely verbal, and that each may be correct about its own subject of discussion—simply because rules like these fix the modal facts for the objects to be discussed.

But here another worry might raise its head: Is the constitutivist committed to saying that the disputants are really ‘talking about different things’ if their concepts vary in these ways? If so, the objector might say, that’s implausible, for it’s just *obvious* that all are talking about *the same thing* (that shrine over there). This is a tricky issue, for (as I have argued elsewhere (2009)) we count ‘things’ in different ways. Where philosophical considerations are at the forefront, we have good reason to say that if objects A and B have different persistence conditions or other modal properties, they cannot be strictly identical—for otherwise circumstances might arise in which, e.g. the individual both could and could not survive. In that strict sense, we should deny that the referent of the thought and discourse of the Japanese and Western individuals are identical. Nonetheless, our everyday counting practices more typically count using a single sortal, e.g. *physical lump*, in which case, we generally say there is but one ‘thing’ on the site. This is no different from the issues surrounding the problem of material constitution: multi-thingers will deny the identity of the statue and lump (given their different persistence conditions), and yet can perfectly well explain why we say there is but one ‘thing’ there (cf. my 2007a, Chapter 4), given our counting practices and the fact that there is but one physical lump. The multi-thinger can also perfectly well allow that the individual speaking of the statue and the one speaking of the lump may nonetheless seem like they’re talking about the same thing (‘that one over there’—said without a sortal) and communicate perfectly well about many issues (e.g. weight, location, melting point…).[[14]](#endnote-14)

1. **Other suspect sources of variation**

Beyond the cultural variations in intuitions, experimental philosophers have shown evidence that other apparently extraneous factors influence intuitive judgments about philosophical cases, which is thought to undermine the idea that at least some of those intuitive judgments could really be thought to provide warrant. As Alexander, Mallon and Weinberg put it:

philosophical intuitions are sensitive to facts about who is considering the hypothetical case, the presence or absence of certain kinds of content (e.g. abstract vs. concrete; affectively neutral vs. affectively engaging), or the context in which the hypothetical case is being considered. This sensitivity is problematic because such facts have not traditionally been thought to be relevant to the truth or falsity of the claims for which philosophical intuitions are supposed to provide evidence or data. (2010, 7)

So, for example, Knobe (2003) found that subjects were far more likely to say an act was done intentionally if the behavior was bad rather than good. Nahmias et. al. (forthcoming ) found that intuitions about responsibility were highly subject to framing effects about the way the question was put: respondents asked abstract moral questions were far more likely to favor incompatibilism than were those presented with concrete cases.[[15]](#endnote-15)

I will be briefer and more general in responding to these concerns, since I don’t know that any similar studies have been done for ontological intuitions, so we lack a concrete case to work on. But notice that, on the constitutivist model, even if there are arbitrary causal factors at work behind our judgments about a particular case, that does not necessarily show these intuitions to be *faulty* or misleading about the ontology. There may be arbitrary causal factors lying behind the development of some sets of rules rather than others, but that doesn’t undermine the idea that the rules fix what is and is not permitted in a game or social enterprise. If the regulation size of a basketball court arose due to the size constraints in a certain 19th century YMCA gym in Massachusetts, that does not undermine the idea that whatever the rules say is the regulation size is the regulation size. If we discover that the two-party Westminster system of government arose largely by historical accident given the architecture of the British Parliament building (with its sets of opposing benches), that does not undermine our warrant for our beliefs about what counts as electing MPs, passing legislation, and the like—these remain fixed by the rules even if the rules are, to some extent, products of a historical accident. So similarly, discoveries of fairly arbitrary causal processes behind the rules need not undermine the idea that the rules of use for our terms still determine what ontological sort of thing we are talking about, so that a good way of getting at the latter is via the former.[[16]](#endnote-16) Our ontological categories themselves may be fairly arbitrary, unsystematic, and so on in ways that reflect these features of the rules.

Of course discoveries of arbitrary causal processes or historical accidents like these lying behind our semantic (or other) rules may undermine our confidence in these rules in a different way: they may undermine our feeling that our rules are natural, inevitable, or the best possible to serve our purposes, and so may prompt us to consider revision: to adopt a larger regulation court size if it would better suit the size and skills of today’s players, or to adopt different political rules if it would better serve the interests of the electorate. So similarly, if we discover fairly arbitrary causal processes at work behind the semantic rules we employ, which fix which ontological sorts of thing we are talking about, that might prompt us to suggest adopting other rules instead, which might be more systematic, better suited to our interests, etc., and lead to a more systematic and reasoned set of ontological categories. But this is not a matter of finding that our semantic rules don’t provide ‘evidence’ about ontological ‘facts’, or of discovering the ‘true’ modal boundaries that our original concepts failed to track. Instead, it is a matter of deciding to revise the rules on pragmatic grounds, and thereby also revising what (ontological) sorts of thing we’re talking about.

Still, some causal factors might lead not to the adoption of certain rules, but instead interfere with people *following* those rules there are. Thus we still face the problem of determining which intuitive judgments do, and which do not, manifest mastery of the rules. I turn to this difficult issue below.

1. **The Role of Experimental Philosophy**

I have so far aimed to show that the criticisms experimental philosophers have leveled against the use of intuitions in philosophy do not undermine a constitutivist view of the role conceptual analysis plays in addressing ontological questions. The differences become clear once we emphasize that the view is not that our intuitions provide evidence about some deep ontological facts in the world, but rather that the semantic rules governing the terms and concepts we employ determine what ontological sort of thing, if any, we refer to. Conceptual analysis on this model is not a matter of mining intuitions for data, but rather of attempting to make explicit the rules of use for the concepts and terms we employ, enabling us to express our results (under semantic descent) in the object language to make claims about the conditions under which entities of various types would exist, persist, or be identical.

While I have focused on the kinds of modal inquiry that arise in ontology, as long as modal normativism is the right approach to metaphysical modality, the above results in principle should carry over wherever the philosophical inquiry is a (metaphysically) modal one, e.g. about under what conditions someone *would have* knowledge, when an act *would be* free, and so on. A full exposition and defense of that view, however, would require an analysis of how adjectival terms like ‘knows’ and ‘free’ work, of their rules of use and the relation between these and modal claims made involving those terms, parallel to the one I discuss for noun terms above—and that goes far beyond what can be done here.

But even if we just accept the suggested role for conceptual analysis in addressing ontological questions, the question remains: what, if anything, might be the role for empirical methods—particularly the methods of experimental philosophy—in doing work in ontology? I actually think there can be quite a useful role—I’m not at all against experimental philosophy; I think it is interesting, and can potentially be an aid in doing ontology. One use we have already seen: experimental philosophy, to the extent that it reveals cultural variation in our rules of use for our terms, or influence of those rules by arbitrary causal factors, may prompt conceptual *revision*, encouraging us to develop a new set of concepts (or revise the old) in ways that may make them more systematic, or able to better serve our interests.

Experimental results may also be helpful, however, even if we aim to do only descriptive, not revisionary, ontology. A first useful role is in providing a *corrective* influence on our attempts to acquire knowledge in ontology. I have suggested that we can move from mastery of the rules of use for a noun term to knowledge of the most basic modal features of its referents (if any). If we want to talk about the ontological status of *symphonies* as we know them, as they are covered in copyright law, as we care about them in our acts of aesthetic creation, criticism, or appreciation, we must be concerned with the *common* rules of use for the term in a public language. Finding that one’s own use is highly idiosyncratic may give some reason for thinking it is incorrect,[[17]](#endnote-17) and so experimental results may provide a useful corrective to the uses of a lone philosopher, whose use of key terms may not reflect mastery of the rules but instead be idiosyncratic, theory influenced, or just out of date. (That philosopher might still provide a true analysis of what *symphonies\** are, using her own idiosyncratic concept, and she might be right about what things, if any, *her* concept refers to. The only trouble is: no one will care, or need care, if she’s not talking about *symphonies,* the objects of common discourse by musicians, conductors, fans, and the like). It might in particular be useful if such studies were focused on the uses of the relevant experts who might be most expected to exhibit mastery: the composers, conductors, musicians, etc.

But doesn’t the possibility that the metaphysician turns out not to have mastered the rules undermine the idea that conceptual analysis (as described above) can give us ontological knowledge? As I initially told the story, competent usersof a sortal term ‘N’ can acquire modal knowledge about Ns by making use of their competence to consider hypothetical cases, to acquire a general and more explicit understanding of what the rules are, and (by expressing these in the object-language) thereby acquire the kind of modal knowledge about the Ns (if there are any) that is relevant to determining their ontological status, identity and persistence conditions, and so on. On this picture, speakers who have mastered the rules of use for a term are in a position to acquire an explicit grasp of the rules, and with it to acquire modal and ontological knowledge. They may of course not know *that* they have mastered the rules of use for a term—experimental results may undermine that assumption. If they don’t know *that* they have mastered the rules of use for a term, they may not know *that* they have modal knowledge—but that is a separate issue. As long as we are willing to allow that one may know something without knowing *that one* knows it, we may still allow that competent speakers who *have* mastered the rules may make use of that mastery to acquire modal knowledge—even if they don’t yet know *that* they are competent speakers, or even if the belief that they are competent could potentially be undermined by empirical results.

But what about speakers who have not mastered the rules of use for ‘N’ (or a term with the same rules of use): can they not acquire modal knowledge about Ns? Here also, experimental philosophy may be of some use, for experimental data may help speakers who have *not* mastered the rules of use for a given term to figure out the rules, putting them also in a position to acquire modal knowledge. We must be careful here, however, for it seems that experimental work alone isn’t sufficient. What we needed as the basis for modal metaphysical knowledge was knowledge of the relevant semantic *rules* (though this may be expressed in the object language). But empirical work that reveals what intuitions there are (in various circumstances) or how people actually use terms does not in itself distinguish which uses are reflections of the rules (exhibiting what linguists call ‘competence’), and which are mistakes (‘performance errors’). As Alexander, Mallon and Weinberg suggest, “semantic mentalists [who seek an account of the meanings of terms based on experimental data] owe a way of distinguishing meaning-constituting facts from non-meaning-constituting ones” (Alexander et. al. 2010, 11). Indeed they take this to be a formidable problem facing all varieties of *positive* experimental philosophy, along with traditional ‘armchair’ philosophy:

Since all of the positive programs are committed to the view that intuitions are trustworthy and shared, each faces the challenge of figuring out what to do with the results of negative experimental philosophy. In particular, the positive programs owe us the same kind of story that traditional armchair philosophers owe: how do we discern which intuitions count? (Alexander et. al. 2010, 25)

Some partial suggestions come to mind: for example, we could follow Strawson and Grice in suggesting that we attend not just to first-order linguistic behavior (intuitions about whether a term should be applied or refused) but also to *reactions to* those uses: e.g. violations of the rules are often met with bewilderment or failure to understand, rather than a mere refusal to believe the content of what’s said (1956, 150-51). We might also appeal to what is taught and corrected, who is counted or not counted as an authority, etc., rather than simply aggregating first-order linguistic judgments, to gain a little more traction in using other kinds of empirical data to distinguish uses that reflect mastery of the rules from those that are performance errors.

Nonetheless, if we think of Alexander, Mallon and Weinberg as challenging us to give a *purely empirical* way of determining which uses reflect mastery of the relevant concepts, it is indeed a formidable problem. But it is hardly a surprising problem, especially given the understanding of concepts we’ve been employing in terms of *rules* or *norms* of usefor employing the terms in question. As Quine noted, it’s not clear how to discern what are the genuine rules or conventions in force merely by observing linguistic behavior (1966/1976, 105-6).[[18]](#endnote-18) And the rule-following arguments of Wittgenstein (1953/2001, 201a), and Kripke (1982) make clear that no amount of purely empirical enquiry alone will enable us to uniquely determine a set of rules –not even if we could observe ‘inner’ mental states as well as linguistic behaviors. The demand to find “a way of distinguishing meaning-constituting facts from non-meaning-constituting ones”, at least if we take this to be a demand for discerning *normative* meaning facts via a purely *empirical* method, is an inappropriate and impossible demand.

So it seems that experimental work alone cannot tell us what the linguistic rules are, and thereby enable us to acquire the relevant ontological knowledge. But surely it can help. Similar problems in discerning rules from behavior arise for linguists attempting to discern the rules of obscure languages by observing the verbal behavior of native speakers, and for cultural anthropologists in trying to explicitly codify the rules governing behavior in various cultures in various circumstances. Yet the data is nonetheless useful, and progress is made in each of these fields. A crucial idea seems to be to allow not just observation of facts, but also a *mastery* of deeper *normative practices*, to play a role in working out the *rules* on the basis of facts about use (the same goes for speakers working out the *proper* way to go on in applying the rules learned for a term (cf. Brandom 1994, 20-27)). By making use of these normatively governed skills along with experimental results, we may hope to make use of experimental data in determining the rules of expressions we hadn’t previously mastered, and so in acquiring further modal and ontological knowledge.

So although the constitutivist view entails that there may be no purely empirical method of getting answers to modal/ontological questions, I don’t think that’s a problem for the view. The method suggested for acquiring modal knowledge doesn’t involve anything mysterious—it simply moves from semantic competence to the ability to generalize and reason and engage in semantic descent. And there does not seem to be any alternative story that *would* make the sort of modal knowledge at issue in ontological debates knowable by purely empirical methods. For the idea that we could discern these ontological facts purely empirically by investigating the world (rather than our employment of concepts) relies on the implausible idea that we can empirically detect modal facts. But Hume long ago noted that we cannot be thought to perceive any necessary connections in the world—while we may perceive what *does* happen, we do not perceive what *has to* happen. Recent attempts to suggest how we can empirically detect essences (e.g. Elder 2004) have predictably encountered troubles (see my 2006).[[19]](#endnote-19) In sum, that there is no purely empirical method for acquiring modal knowledge is not a regrettable consequence of the constitutivist approach, but rather an unavoidable situation.

I have argued that the constitutivist approach survives the various attacks on conceptual analysis, giving us a non-mysterious model of how we may acquire the kind of modal knowledge that plays a role in metaphysics, and justifying the use of thought experiments and the like in undertaking metaphysical inquiries. The approach also, I have argued, is perfectly compatible with acknowledging a useful role for experimental philosophy as a corrective against biases of individuals or of philosophers taken as a group, and in helping us work out what the semantic rules are that govern the key expressions used in our philosophical theories. While experimental work cannot in principle do the ontological job alone, it also doesn’t undermine the method of doing the job by way of conceptual analysis—taken as an attempt to make explicit (in the object language) the rules that govern the use of our terms and concepts, by whatever means we can do that best.

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1. \* A prior version of this paper was presented at the “Analysis and Explanation in Aesthetics” conference in Nottingham, UK, June 2010. Many thanks to those present for very helpful discussion. Thanks also to three anonymous referees of this journal for detailed and insightful comments. [↑](#endnote-ref-1)
2. For arguments that the methods of phenomenology share much in common with those of ordinary language philosophy, see my 2007c. [↑](#endnote-ref-2)
3. The most extended treatment is in my 2007a, and the detailed application to metaphysical modal issues is in 2007b. Also see my 2004, 2005, 2009. [↑](#endnote-ref-3)
4. Versions of the *qua* problem are discussed in Papineau (1979, 158-68), Devitt (1981), Sterelny (1983), Devitt and Sterelny (1999), Dupre (1981), Kitcher (1982), and Stanford and Kitcher (2000). I discuss it and the related problem of determining whether a term refers at all in (2007a, 38-44). [↑](#endnote-ref-4)
5. Ontologists who would deny the existence of all but one of these still face the problem of disambiguating *whether or not* the term refers (as well as problems with the standard arguments for eliminativism). See my 2007a for discussion. [↑](#endnote-ref-5)
6. They can of course go more wrong where derivative modal facts are concerned, since these also rely on empirical facts. [↑](#endnote-ref-6)
7. Note that this view of modality is only intended to handle so-called ‘metaphysical necessities’, not logical or physical/nomological necessities, which must be considered separately. [↑](#endnote-ref-7)
8. For more discussion of how the view handles *a posteriori* modal truths, see my (2007b), which draws on work in Sidelle (1989). [↑](#endnote-ref-8)
9. For clarification and further discussion of the relevant ‘normativist’ view of metaphysical modality and its consequences for modal epistemology, see my (2007b). [↑](#endnote-ref-9)
10. I will use the general term ‘architectural work’ rather than the name ‘Ise Jingu’ in the discussion that follows, for there is reason to think that the application and coapplication conditions associated with the name by those who ground and reground its reference (the Japanese) are those that fix the modal status of the referent of the name, so that Westerners might end up being wrong about Ise Jingu, even if they are right about ‘that architectural work’, using their own concept associated with that general term. There may also be difficulties to the extent that different general terms are in play, e.g. ‘architectural work’, ‘building’, ‘shrine’… I attempt to abstract from those above by simplifying and assuming both are using a term something like ‘architectural work’. [↑](#endnote-ref-10)
11. This of course is not to deny that there may be important commonalities across conceptual systems, perhaps even some basic concepts that may be biologically hard-wired in creatures like us. [↑](#endnote-ref-11)
12. Other defenders of conceptual analysis have appealed to the possibility that the apparent variations experimental philosophers point to are merely verbal disagreements, and insisted that verbal disagreement needn’t reveal substantive disagreement ‘if ambiguity and context might account for the verbal divergence’ (Sosa 2008, 234). And if it’s only verbal disagreement, then “the supposed problem for philosophical intuition evaporates” (ibid 2008, 235). Given the above account, we can easily see how many such disagreements may turn out to be merely verbal disagreements resulting from speakers employing differing concepts. [↑](#endnote-ref-12)
13. Alexander et. al. have even less to say against the idea that “maybe people with different intuitions just have different concepts, so everyone is still correct”, commenting only “But, we are skeptical about just how attractive, or how helpful, such a move towards relativism is.” (2010, 25). [↑](#endnote-ref-13)
14. One final sort of objection is often raised to this way of handling cultural diversity: if we say that the Western and Japanese individuals are each employing their own concepts, and (when discussing the age of the object) may each be right about the object of their own discourse, are we stuck with an implausible view that leads to “the odd state of affairs in which we turn out to be infallible about our own conceptual maps”? (Weinberg/Crowley 2009, 181) (Though they attribute this criticism to Weatherson (2003, 10), that doesn’t seem to be precisely what Weatherson is saying. And at any rate, Weinberg and Crowley endorse it, so the criticism may be more properly attributed to them.) No: even if the rules of use that competent speakers master are tied to the modal/ontological facts about the objects (if any) referred to, that is far from saying that we are infallible *about our own conceptual maps.* One may be competent in finding one’s way around the countryside without being able to articulate one’s own mental map, still less being infallible about what that mental map is like; and competent speakers’ use may be constitutive of the rules of grammar without their being infallible authorities able to state what rules they are following. The sort of protection from error we get is that competent speakers can’t be massively wrong in their basic modal judgments about when the object they are discussing would exist, persist, be identical to another, etc.—not that anyone is infallible about their own conceptual schemes. [↑](#endnote-ref-14)
15. In some cases, the goal of identifying these influences on judgments seems to be to ferret out which of various competing intuitions should be trusted, and which may be discarded. So, for example, Robert Northcott (manuscript) argues that experimental philosophy can help with metaphysical theorizing (e.g. about causation) by not only tracking intuitions, but licensing us to disregard some of them, making it possible to get a consistent analysis (or defend a prominent one) where otherwise the diversity of intuitions might make it look impossible. In other cases, the goal seems to be the more purely negative one of undermining the idea that intuitions may be trusted at all. [↑](#endnote-ref-15)
16. That is not to say, however, that we can never have grounds for disregarding certain reported intuitive judgments even about ontological issues. There may be cases in which, if we took all the judgments seriously and at face value, we would have to suggest that there is an inconsistency in the concept—giving reason to reexamine whether or not all the judgments really are reflections of mastery of the rules rather than some arising from simple mistakes grounded in other causal factors. [↑](#endnote-ref-16)
17. Though it cannot show this definitively, for reasons discussed below. [↑](#endnote-ref-17)
18. This is merely an instance of the same sort of problem noted by Kant in saying that one can’t derive an ‘ought’ from an ‘is’, and by critics of psychologism (such as Husserl and Frege) who argued that one can’t derive the *norms* for reasoning or calculation (and thus the rules for logic and mathematics) from *facts* about psychological behaviors and processes. [↑](#endnote-ref-18)
19. Kripkean cases in which empirical inquiry plays a role in discovering essences (as Sidelle (1989) and I (2007b) both argue) are best understood for as cases in which we empirically discover facts that may be plugged in to schematic modal truths known by other means. Thus again on that model, empirical inquiry alone cannot lead to modal knowledge. [↑](#endnote-ref-19)