

Conceptual and Linguistic Analysis: A Two-Step Program

ANDREW MELNYK
University of Missouri

Many philosophers have a bad habit, and this paper offers a two-step program to help them quit. The bad habit is conceptual or linguistic analysis, understood as the use of a certain *method*—the method of hypothetical cases—to achieve a certain *end*. The method consists in settling into an armchair and asking oneself, for a variety of hypothetical situations, whether one *would* apply a given concept, or word, to something in that hypothetical situation; and the end to which the method is taken to be, together with further reflection, a sufficient means is the discovery a priori of necessary truths that can be formulated by using the given concept or word, e.g., truths expressing necessary and sufficient conditions for the applicability of the given concept or word. So the bad habit I'm deploring isn't the method of hypothetical cases as such; it's just the method when it's aimed at the a priori discovery of necessary truths. I'll call the use of the method of hypothetical cases when aimed at the a priori discovery of necessary truths *conceptual analysis* if one asks about the applicability to hypothetical circumstances of a *concept*, and *linguistic analysis* if one asks about the applicability to hypothetical circumstances of a *word*.

The first step in quitting a bad habit is to acknowledge there's a problem. In a nutshell, the problem with conceptual analysis and linguistic analysis is that, although they presuppose that the method of hypothetical cases *can* yield a priori knowledge of necessary truths, it's very hard to see *how* it could; and the more you wonder about how it could, the more you suspect that it couldn't. In philosophy, no less than in science, methodological practice presupposes substantive theoretical commitment; in order for a given methodological practice to be appropriate, whether in science or philosophy,

the world has to cooperate by being a certain way. To ground conceptual or linguistic analysis, then, there has to *be* (even if practitioners don't have to *know*) some true account (psychological, linguistic, semantic, or whatever) of the conditions that make conceptual or linguistic analysis possible. The trouble is that no such account seems to exist. Section One and Section Two argue for this claim, first for conceptual analysis and then for linguistic analysis.

The second step in quitting something is to see clearly that life can go on without it. It's tempting to think that, if conceptual and linguistic analysis are abandoned, then philosophers are left with no way at all of discovering the necessary truths they've traditionally wanted to discover—about knowledge, causation, freedom, or whatever. As a corollary, it's also tempting to think that conceptual and linguistic analysis, being indispensable, must *have* a theoretical underpinning, even if at present we can't quite say what it is. The good news is that both these temptations can be resisted. The key is to see that the knowledge of necessary truths that philosophers have traditionally wanted can be achieved without conceptual or linguistic analysis. Section Three shows how, against two recent doubters. Section Four suggests that, even if you give up conceptual and linguistic analysis, you don't have to give up the method of hypothetical cases—only one false image of what it is. That should also give comfort.

I

Let's begin with conceptual analysis.¹ If conceptual analysis is a viable philosophical methodology, then there must be some account, even if it's very abstract, of what's going on in our minds when we use the method of hypothetical cases, some account which explains how in principle the method is capable of yielding a priori knowledge of necessary truths. But what account? Since conceptual analysis can't plausibly be claimed to involve the exercise of a *sui generis* faculty of insight into a platonic realm of essences, we have to assume that any such account will construe the necessary truths discovered by the method of hypothetical cases as *semantic* in origin, and then insist on the *a priori knowability* of the relevant semantic facts (see, for example, Peacocke 1993; Boghossian 1996; Henderson and Horgan, 2000 and 2001; Chalmers and Jackson 2001, especially 320–28; Gertler 2002). Specifically, we must take the viability of conceptual analysis to require the truth of the following claims:

(Prerequisites for conceptual analysis)

C1. The necessary truths that the method of hypothetical cases (together with further reflection) uncovers are *conceptually necessary*, i.e., true in virtue of the contents of concepts.

C2. Anyone who possesses a given concept *automatically* (i.e., necessarily, in virtue only of their possession of the concept) has some sort of cognitive access to

the content of that concept; and since this cognitive access is automatic and hence achieved without further empirical inquiry or dependence on claims grounded in earlier empirical inquiry but not required for concept-acquisition, it counts as a priori.

C3. This cognitive access to concept-content manifests itself in, because it *guides*, the person's judgments about whether a given concept would apply to something in a specified hypothetical situation, thus enabling the discovery a priori, through further reflection, of conceptually necessary truths.²

In conceptual analysis, then, the method of hypothetical cases is capable of yielding a priori knowledge of necessary truths only if claims C1, C2, and C3 are true. Now some philosophers who have cautioned against conceptual analysis have done so because, on Quinean grounds, they don't believe in conceptually necessary truths; they would say that the problem with conceptual analysis is that it requires the truth of C1, which is false. But that's not the real problem, in my view, since we can (arguably) make good sense of conceptually necessary truths.³ The real problem for conceptual analysis is this: all available accounts of what it is to possess a concept with a given content either entail that C2 is false or fail to provide an adequate explanation of how it could be true. Even if the necessary truths allegedly discernible a priori via the method of hypothetical cases are conceptually necessary, that won't make them discernible a priori unless conceptual content itself is discernible a priori.

Let me divide available accounts of what it is to possess a concept with a given content into two categories, first arguing my claim for those accounts that tell an *externalist* story about how the content of concepts is metaphysically determined. Such accounts, while allowing that an internal state of a person may qualify as a concept only if it plays (or has the job of playing) a suitable internal role, insist nevertheless that what determines the content of the concept are the external and perhaps historical relations (e.g., causal or nomological relations) in which it stands to the substances or properties that the concept refers to. Such theories may also posit modes of presentation for concepts—in order to explain, for example, the difference between believing that Cicero was an orator and believing that Tully was an orator; but these modes of presentation will be non-semantic, understood perhaps as the narrow causal roles of the concepts that have them. Both Jerry Fodor and Ruth Millikan, among several others, have developed accounts of concept-possession and content-determination that fall into this first category, and no doubt the space of such accounts has yet to be mapped exhaustively (Fodor 1998 and 2004; Millikan 1984, 1993, and 2000).

Let's see first how Fodor's account entails the falsity of C2. According to Fodor, to possess a concept of something (e.g., water) is to be able to think thoughts about it (Fodor 2004, 31). To think a thought is to host a sentence-like structure (in humans, realized neurally) that possesses a specific

truth-conditional content and that plays a distinctive computational role within the overall economy of the mind, with different roles explaining the difference between different kinds of thoughts, e.g., between beliefs and desires. Such a hosted structure is sentence-like in having its semantic and syntactic features determined by the semantic and syntactic features of its constituents; but the semantic features of these constituents are not determined by anything in the head of the thinker (e.g., by their computational relations to other such constituents) but rather by their standing in a complex causal-nomological relation to the properties in the external world that they represent (Fodor 1990). It is these syntactic and semantic constituents of thoughts—these words in sentences of the language of thought—that are concepts, content-bearing mental particulars that can function as causes and effects (Fodor 1998, 23). So to possess a concept of water, for example, is merely to be capable of hosting sentences in the language of thought that have as a syntactic and semantic constituent an element in fact representing water.

Now the crucial point about Fodor's account is how *little* it demands of a thinker in order for the thinker to possess a concept with a given content. To possess a concept of water, for instance, it's not required by the account that the thinker know or believe anything about water; or about the concept WATER (e.g., that it has so-and-so content).⁴ Nor, even though the concept owes its content entirely to its bearing a certain relation to a certain portion of the external world, is the thinker required to be at all aware *that* it bears that relation to the world; or even that *if* it bears that relation to that portion of the world, *then* it refers to water (*pace* Chalmers and Jackson 2001, 325, who insist that thinkers competent with a given concept thereby know the concept's "application conditionals"). Such awareness could, of course, be acquired by a thinker, but it would take empirical inquiry, first, in order to discover *which* causal-nomological relation is the reference-constituting one, and secondly, to discover which external things actually stand in that relation to one's internal states. On Fodor's account of concept-possession and content-determination, then, those who possess a given concept don't automatically have some sort of cognitive access to the content of that concept; C2 is false.

Millikan's account of what it is to possess a concept with a given content has the same implication. According to her account of substance concepts (i.e., concepts of the referents of subject-terms), to possess a substance concept of a certain thing is (roughly) to have an ability to gather and use information about the thing, this ability made possible by the proper functioning of a hosted mental representation of the thing; and the content of this mental representation (together with the character of the ability which it makes possible) is metaphysically determined by facts about the ontogenetic or phylogenetic history of its acquisition, facts that are generally not known to the possessor of the concept and that could only be learnt by empirical inquiry (Millikan 2000). Millikan's account, then, like Fodor's, demands little

from the possessors of a concept. In particular, it imposes no requirement that, simply as such, they have cognitive access of any sort to the content of the concept; not even to the way in which a concept's content depends on how the world turns out (*pace* Chalmers and Jackson 2001, 325). So if Millikan's account of concept-possession and content-determination is true, C2 is false.

So much, then, for accounts of what it is to possess a concept with a given content that tell an externalist story about content-determination. Perhaps it's no surprise that accounts of this kind prove inhospitable to conceptual analysis, and perhaps no conceptual analyst would wish to appeal to them in order to explain how conceptual analysis is possible.⁵ But the result still matters, because, for all that anyone can seriously claim to know now, these accounts—or accounts relevantly like them—might yet turn out to be true. Yet conceptual analysts must assume they're all false. Perhaps they're feeling lucky.

Let's turn next to the second category of accounts of concept-possession and concept-content, those that tell a wholly or partially *internalist* story about how the content of concepts is metaphysically determined.⁶ Such accounts seem at first sight to be good candidates *not* to entail the falsity of C2, since they hold that the content of a concept is determined by facts *internal* to the head of the thinker. In fact, however, as I'll eventually argue, accounts of concept-possession that tell a wholly or partially internalist story about concept-content are hardly more hospitable to conceptual analysis than the externalist accounts we've just considered. Internalist accounts may actually entail that C2 is false; but at best they leave it a mystery how C2 could be true. Either way, they fail to provide a theoretical underpinning for conceptual analysis.

All such accounts known to me are variations on the generic idea that to possess a concept with a given content is (at least in part) to have a certain set of mental dispositions to *use* the concept, i.e., dispositions to *apply* the concept under certain circumstances, or to make certain *inferences* using it, or to do some combination of both.⁷ Thus it might be claimed that to possess a concept of a swan one must be disposed, for example, to think of a thing as a swan if it's an elegant, long-necked, water bird, to infer from the belief that there's a swan over there the further belief that there's a bird over there, and so on. Let's call accounts of concepts that implement this generic idea that concept-possession is the having of certain dispositions to mental use *dispositionalist* accounts of concept-possession and concept-content. Now there are also accounts of concepts that claim that possession of a given concept requires its possessor to hold certain *beliefs* involving the concept; such accounts might claim, for example, that to possess a concept of a swan one must *believe* that all swans are birds. I shall count these accounts as dispositionalist too, because for the purposes of my argument the difference between (i) believing that all swans are birds and (ii) having a disposition to

infer from the belief that something is a swan the further belief that it's a bird won't matter. Similarly, I shall also count as dispositionalist those *descriptivist* accounts of concepts that view the content of a concept as determined by some privileged *definite description* (reference-fixing or meaning-giving) that the thinker in some way *associates* with the concept. For descriptivists must explain what psychological relation they intend by their talk of association, and a natural way to do so is to say that to associate a concept of a swan with, say, "the (actual) elegant, long-necked, water bird" is to be disposed to make the sort of inferences with, and/or applications of, the concept of a swan that I mentioned above.

Dispositionalist accounts of concept-possession and concept-content—perhaps under the name “use theories of meaning”—are surely what some practitioners of conceptual analysis have assumed as the foundation of their practice (see, e.g., Peacocke 1993; Gertler 2002; and presumably Chalmers and Jackson 2001, given their doubt that conceptual analyses are mentally represented explicitly). And they certainly yield a picture that's attractive in one way. In general, you can have a disposition and yet be in no better epistemic position to specify its character than an external observer is; for example, you surely possess various dispositions to react to pathogens, thanks to the particular condition of your immune system, but the only way you have of discovering the character of these dispositions is to make inferences from observations of your own past reactions. Moreover, your judgments, no matter how confident, about how you *would* react, *were* you to confront such-and-such a pathogen, would have no special status, at best reflecting an unconsciously formulated hypothesis, based on past experience, about the nature of your dispositions to react to pathogens. The method of hypothetical cases, however, promises to overcome these difficulties with respect to the dispositions identified with concept-possession. For we seem able to probe the character of our own dispositions to inference and application by asking ourselves to consider merely hypothetical circumstances, thereby *simulating* inference concerning, and application to, the actual world. We have no such ability to simulate in the case of our dispositions to react to pathogens.

However, the ability to probe the character of our own dispositions to inference and application is not enough. Dispositionalist accounts of concept-possession and content-determination either entail C2's falsity or else fail to provide an adequate explanation of how it could be true.⁸ Let me now embark on an extended argument for this claim. We need first to notice a non-obvious constraint on the plausibility of dispositionalist accounts. Clearly such accounts identify the possession of a given concept with the having of a certain set of dispositions to apply the concept and/or make inferences with it. But *which* set of dispositions? It's tempting to answer that possession of a concept by a given thinker can be identified with possession of the *totality* of that thinker's dispositions to apply and/or make inferences with

the concept, so that the thinker's *actual* dispositions with the concept are individually necessary and jointly sufficient for his or her possession of the concept. But possession of a concept by a given thinker can't be identified with the totality of that thinker's dispositions with the concept—and for two reasons. First, identifying the possession of a given concept by a thinker with the totality of the thinker's dispositions to apply the concept and/or make inferences with it implies that two people who don't have the *same* totality of dispositions with regard to any of their respective internal states don't possess the same concepts—contrary to our pre-theoretical judgment that they do possess the same concepts. For example, suppose we claim that a physics professor possesses the concept ELECTRON in virtue of her total disposition to inference and use in regard to a certain internal state; then we will have to say that all physics majors who lack such a total disposition in regard to any internal state don't possess the concept ELECTRON—even if we would ordinarily insist that they do. So identifying the possession of a given concept by a given thinker with the totality of that thinker's dispositions with regard to some internal state individuates concept-possession too finely to accommodate the fact that concepts are *public*, i.e., shared by many different people.

The second reason why possession of a concept by a given thinker can't be identified with the totality of that thinker's dispositions with the concept is that, for most (if not all) thinkers, the totality of the thinker's mental dispositions with regard to a given internal state includes dispositions that are *erroneous* in the sense of failing to correspond to genuine conceptual truths. (A disposition to apply *C* to something whenever it is *F* is erroneous in this sense if it isn't conceptually necessary that all *F*s are things to which *C* applies; a disposition to infer that something is *G* from the premise that *C* applies to it is erroneous if it isn't conceptually necessary that all things to which *C* applies are *G*s.) For example, non-mathematicians still possess simple geometrical concepts (e.g., a concept of parallel lines) despite having erroneous dispositions to use those concepts to deduce geometrical falsehoods from geometrical truths if presented with subtly fallacious proofs; many children, and doubtless many adults, possess the concept SWAN despite having an erroneous disposition to *deduce* that something is white given that it's a swan (or: despite mistakenly thinking that swans *have* to be white); many people have the concepts TURTLE and TORTOISE despite having erroneous dispositions to apply both concepts; and so on. But it can hardly be (partly) constitutive of, and hence a necessary condition for, possession of a concept to have a disposition that is erroneous in this sense, or else we will have to refuse to credit people with possessing the concept on the grounds that they make no mistakes with it!

So a plausible dispositionalist account of concept-possession and content-determination has to hold that the possession of a given concept by a typical



thinker who possesses the concept should be identified with her possession of some *proper subset* of the totality of her dispositions.⁹ But *which* proper subset? With regard to one's possession of (e.g.) SWAN, which of one's actual dispositions are in and which are out? Now Fodor has pressed such a question against dispositionalist accounts, suggesting that no principled answer can be given, because doing so would be equivalent to making out a principled distinction between analytic and synthetic truths, the possibility of which was put into serious doubt by Quine (e.g., Fodor 1998, 37 and 45–6). But let's temporarily set aside Fodor's *metaphysical* question about what *makes* a given disposition partially concept-constituting or not. In order to continue my case that dispositionalist accounts of concept-possession and content-determination either entail C2's falsity or else fail to explain how it could be true, I want to press a distinct though related *epistemological* question: regardless of what metaphysically determines the composition of the proper subset of dispositions possession of which by a thinker constitutes her possession of a given concept, is any thinker who possesses the concept *guaranteed to be able to discover this composition a priori*—i.e., without further empirical inquiry or dependence on claims grounded in earlier empirical inquiry but not required for concept-acquisition?

Here's why this epistemological question is so crucial. Prerequisite C2 of conceptual analysis requires any thinker who possesses a given concept to be able to discover its *content* a priori in the sense just given. And a dispositionalist account of concept-possession and content-determination seems to enable a thinker to do so, since it says that a concept's content is determined by the thinker's mental dispositions, and a thinker can probe the character of his own mental dispositions a priori by considering hypothetical cases. However, as we've seen, a plausible dispositionalism about concepts must hold that not all of these dispositions are constitutive of possession of the concept whose content the thinker is investigating. So, before the thinker can discover a priori the content of any concept that he possesses by probing his own dispositions, he must first be able to discover a priori the *composition* of the proper subset of his actual dispositions possession of which (subset) constitutes his possession of the concept in question; if he's not able first to discover a priori which of his dispositions are constitutive of possession of the concept whose content he is investigating and which are not, then he can't know which of his dispositions to note and which to disregard. Consequently, conceptual analysts who look to dispositionalism about concepts to underpin conceptual analysis need an *affirmative* answer to the epistemological question: they need it to be true that any thinker who possesses a concept *is* thereby guaranteed to be able to discover a priori the composition of the set of dispositions possession of which constitutes his possession of that concept.

That's what they need. But can they have it? I don't think so. To begin with, here's an argument for thinking that, if (plausible) dispositionalism



about concepts is true, then a thinker who possesses a given concept *can't* discover a priori the composition of the set of dispositions possession of which constitutes his possession of that concept. The argument has three premises. The first premise says that if dispositionalism about concepts is true, then some satisfactory answer to Fodor's metaphysical demand (for a principled account of what determines which mental dispositions are included in the set whose possession constitutes possession of a given concept) is true. In effect, this premise merely says that Fodor's demand requires some answer. The second premise says that the only satisfactory answer to Fodor's demand is that there are true a posteriori identity claims to the effect that possession of SWAN = possession of just *this* set of mental dispositions, that possession of ELECTRON = possession of just *that* set of mental dispositions, and so forth, so that which mental dispositions are required for possession of a given concept is determined by whatever it is that possession of that concept turns out—a posteriori—to *be*. Such an answer seems quite satisfactory, since an analogous answer is all that we could give if someone demanded a principled reason why, say, oxygen is in and chlorine is out in our account of what water is: we would just have to say that water turns out to be H₂O, a compound that contains no chlorine. And the absence of obvious rivals to this answer is some evidence that it's the *only* satisfactory answer. The third premise is that if this answer to Fodor's demand is true, then a thinker who possesses a given concept *can't* discover a priori the composition of the proper subset of his dispositions possession of which (subset) constitutes his possession of that concept. This premise follows from the observation that if the suggested answer to Fodor's demand is true, then discovering the composition of the proper subset in question requires determining the truth of an a posteriori identity claim.¹⁰ Now premises one and two entail that if dispositionalism about concepts is true, then the answer to Fodor's demand that's mentioned in premise two is true. This subconclusion and premise three entail the promised conclusion that, if dispositionalism about concepts is true, a thinker who possesses a given concept can't discover a priori the composition of the proper subset of his dispositions possession of which (subset) constitutes his possession of that concept. However, I don't want to rest too much weight on this argument, because I offer no real proof of premises one and two, though they're surely plausible. But I do see the argument as a challenge that any dispositionalist defender of conceptual analysis must take up.

Now I'm going to argue that, even if dispositionalism about concepts is granted, it's very difficult to see how a thinker who possesses a given concept *could* discover a priori which mental dispositions are required for possession of that concept. Suppose, then, that (plausible) dispositionalism is true; how might a thinker who possesses a given concept discover a priori which of her mental dispositions are required for possession of that concept? Not by introspecting her phenomenal states, since there seems to be no distinctive phenomenology—nothing special that it's like—associated with



those mental dispositions that are required for possession of a concept. So how else? The only suggestion I've ever heard is that one can distinguish concept-constituting mental dispositions from other mental dispositions by the acceptably a priori procedure of attending to what one can *conceive*.¹¹ Presumably one can do so in this way because a concept-constituting mental disposition just *is* a mental disposition that stands in a certain relation to something that one can't *conceive*. Corresponding to each mental disposition is a generalization, and a disposition is a concept-constituting one iff you can't conceive a counterexample to its corresponding generalization. Thus, imagine you're disposed to apply SWAN only to birds. Because (say) you *can't* conceive of a swan that's not a bird, your disposition *is* a concept-constituting disposition. Conversely, imagine you're disposed to apply SOMETIME BARKER to all dogs. Because you *can* conceive of—make something of the idea of—a dog that never barks, your disposition *isn't* a concept-constituting one.

Unfortunately for this suggestion, however, having a mental disposition while being unable to conceive a counterexample to its corresponding generalization doesn't guarantee that the disposition is a concept-constituting one (or, of course, that its corresponding generalization is necessarily true). Here's a case that shows this. Consider a chef, with much experience and practical knowledge of vinegar, who reads an ill-informed website on the chemistry of cooking and as a result becomes fully (but erroneously) convinced that vinegar is the very same stuff that chemists call "formic acid". So the chef gains a disposition to apply VINEGAR only to stuff that's formic acid. Furthermore, he can't conceive a counterexample to the generalization that corresponds to this new mental disposition: he can make nothing at all of the idea that a bottle is full of vinegar and yet contains no formic acid. (Similarly, because you and I are fully convinced that water is H₂O, we can make nothing at all of the idea that a bottle is full of water and yet contains no H₂O.) Now, if having a mental disposition while being unable to conceive a counterexample to its corresponding generalization guarantees that the disposition is a concept-constituting one, it follows that the chef's disposition to apply VINEGAR only to formic acid is concept-constituting. But in fact this disposition *isn't* concept-constituting, because vinegar isn't formic acid, and hence doesn't have to be; and it can hardly be a requirement on possessing VINEGAR that one think vinegar *must* be something that in fact it isn't. So the notion that having a mental disposition while being unable to conceive a counterexample to its corresponding generalization guarantees that the disposition is a concept-constituting one yields the wrong result.¹² It yields the wrong result in many other cases too: think of a child who mistakenly supposes that swans just are a certain kind of white bird and hence must be white, or a molecular biologist who accepts a false hypothesis as to the chemical formula of a newly discovered enzyme, or (perhaps)



a non-mathematician who mistakenly supposes that parallel lines can't ever meet.¹³

It might be objected that, although having a mental disposition while being unable to conceive a counterexample to its corresponding generalization doesn't guarantee under all conditions that the disposition is concept-constituting, it does guarantee it under *ideal* conditions. But since conditions aren't always ideal, a guarantee that's only good when they are ideal is no help unless one can somehow tell when they are; and it's not obvious how telling when they are could be a priori, as it needs to be. A second objection is that, although having a mental disposition while being unable to conceive a counterexample to its corresponding generalization doesn't guarantee that the disposition is concept-constituting, it's still a reliable sign that the disposition is concept-constituting. But this second objection is hand-waving. It doesn't say what makes a disposition concept-constituting, nor does it say how an inability to conceive a counterexample to a disposition's corresponding generalization comes to be a reliable sign that the disposition is concept-constituting. So it doesn't begin to show how dispositionalism about concepts can explain how a thinker who possesses a given concept could discover a priori which of his mental dispositions constitute possession of that concept.

Why would anyone think in the first place that one can distinguish concept-constituting mental dispositions from non-concept-constituting mental dispositions by attending to what one can conceive? The answer, I suspect, is a widespread assumption about the nature of conceiving, namely, that what one can and can't conceive *must* reflect facts of *some* kind about the contents of one's concepts; perhaps it's thought that conceiving is plainly not just capricious, and that nothing else could regulate it. There is, however, an alternative to this view of conceiving, which is that, math and logic aside, conceiving is regulated by the a posteriori identity claims of which one is convinced; to a first approximation, one can conceive of something's being F but not G (e.g., being table salt but not NaCl) iff one isn't convinced a posteriori that being F = being G. If this alternative view is correct, then, math and logic aside, attending to what one can and can't conceive is informative only about what one does and doesn't *take* to be necessary. And this alternative view might well be true; certainly it makes good sense of the chef case above, where, plausibly, what the chef can conceive changes while nothing in the content of his concepts changes.

I therefore conclude that, even if dispositionalism about concepts is granted, it's very difficult to see how a thinker who possesses a given concept could thereby discover a priori which mental dispositions are required for possession of that concept, and hence how such a thinker could discover a priori the concept's content. Dispositionalism about concepts, then, seems to provide no explanation of how prerequisite C2 of conceptual analysis



could be true. A theoretical grounding for conceptual analysis is not easy to find.

II

The problem with linguistic analysis arises in much the same way as the problem with conceptual analysis. In order for linguistic analysis to be a viable philosophical methodology, there must be some account of what's going on in our minds when we use the method of hypothetical cases, some account which explains how the method can in principle yield a priori knowledge of necessary truths. And, as with conceptual analysis, any such account must presumably construe the necessary truths discovered by the method of hypothetical cases as semantic in origin, insisting on the a priori knowability of the relevant semantic facts. Specifically, we must take the viability of linguistic analysis to require the truth of the following claims:

(Prerequisites for linguistic analysis)

A1. The necessary truths that the method of hypothetical cases (together with further reflection) uncovers are *analytically necessary*, i.e., true in virtue of the contents of words.

A2. Anyone who is semantically competent with a given word *automatically* (i.e., necessarily, in virtue only of their semantic competence with the word) has some sort of cognitive access to the content of that word; and since this cognitive access is automatic and hence achieved without further empirical inquiry or dependence on claims grounded in earlier empirical inquiry but not required for acquiring semantic competence, it counts as a priori.

A3. This cognitive access to word-content manifests itself in, because it *guides*, the person's judgments about whether a given word would apply to something in a specified hypothetical situation, thus enabling the discovery a priori, through further reflection, of analytically necessary truths.

In linguistic analysis, then, the method of hypothetical cases is capable of yielding a priori knowledge of necessary truths only if claims A1, A2, and A3 are true. But I have no quarrel with A1, just as I had no quarrel with C1. The problem for linguistic analysis is this: every currently favored theory of semantic competence with public words *either* entails that A2 is false *or* fails to justify any expectation that A3 is true. The foundations of linguistic analysis are shaky either way.

Theories of semantic competence with the words of a public language aim to characterize the underlying state of speaker-hearers that explains their understanding of words of the language and their production of sentences composed of words in their standard senses. Since all such theories known to me fall into three categories, I'll take each category in turn and argue that theories in that category either entail the falsity of A2 or fail to predict A3.

Theories in the first category claim that semantic competence is a kind of ability or know-how. They claim, in particular, that to be semantically competent with a public word is to know how to translate back and forth from public words to concepts with the same content (see Millikan 1984, 147–8; Devitt and Sterelny 1999, 187–190). But one could presumably have such knowledge-how with regard to a particular public word without having any knowledge-that of the meaning of the public word—for example, without being able to define the word or specify a definite description equivalent to it. In fact, having such knowledge with regard to a particular public word doesn't seem to require *any* sort of cognitive access to the public word's meaning (unless it's assumed, contrary to Section One, that possession of the concept that translates the public word entails cognitive access to *its* content and thereby to the word's content). If so, then A2 is false given a theory of semantic competence belonging to the first category. Now someone might disagree, insisting that underlying every state of knowledge-how there has to be some sort of knowledge-that, so that semantic competence is at bottom a kind of knowledge-that. Perhaps so; but to disagree in this fashion is to adopt a theory of semantic competence belonging to the *third* category, which'll be discussed in due course.

Theories of semantic competence in the second category claim that semantic competence with a given public word is a particular set of dispositions to *use* the word—dispositions to apply the word under certain circumstances, or to make certain inferences using it, or to do some combination of both—where the content of the word is determined by the character of these dispositions. So what makes it the case, on these theories, that you're semantically competent with the word “swan” is your dispositions to apply this word (e.g., to elegant, white, long-necked, water birds) and to infer with it (e.g., from “There's a swan over there” to “There's a bird over there”). I should add that theories of semantic competence in this second category might well be taken up by *descriptivists*. Since descriptivists hold that the meaning of a given word, as used by a speaker, is determined by a certain definite description that the speaker *associates* with the word, they obviously need to say something about the psychological relation they mean by “associates.” Accordingly, they might suggest that to associate a certain definite description with a given word is simply to possess a particular disposition to apply the word and to infer with it.

Theories of semantic competence in this second category—we can call them “dispositionalist theories”—are obviously just the natural-language analogs of the dispositionalist accounts of concepts discussed in Section One. Unsurprisingly, then, they do no better theoretically grounding linguistic analysis than dispositionalist accounts of concepts do grounding conceptual analysis. Since this can be shown by essentially the same reasoning used against dispositionalist accounts of concepts in Section One, I shall be very brief here. To be plausible, dispositionalist theories of semantic



competence must say that a speaker's semantic competence with a given word is her possession of some proper subset of the totality of her actual dispositions to apply the word and/or infer with it. In that case, however, a speaker's semantic competence with a word can yield what A2 requires, viz., a priori access (in the sense of A2) to the word's content only if the speaker can somehow tell a priori (in the same sense) which of her linguistic dispositions are competence-constituting and which aren't. But nothing phenomenological distinguishes the two kinds of linguistic dispositions; and although an appeal to what the speaker can and can't conceive promises to allow the speaker to identify the competence-constituting dispositions, on closer examination it fails.

Theories of semantic competence in the third category claim that semantic competence with a given word in a public language is a kind of knowledge—that, presumably tacit knowledge that *W* means *so-and-so*. Now since such theories *identify* semantic competence with propositional knowledge of word-meaning, such theories obviously entail the truth of A2—that anyone semantically competent with a given word automatically enjoys cognitive access to the content of that word. But the foundations of linguistic analysis are not thereby secured. For not all ways of having propositional knowledge of meaning provide any reason to expect that A3 is true—that this knowledge guides its possessor's judgments about whether a given word would apply to something in a specified hypothetical situation so as to enable the discovery a priori, through further reflection, of analytically necessary truths. Suppose, for example, that semantic competence with “swan” were tacit knowledge that “swan” means swan, that semantic competence with “electron” were tacit knowledge that “electron” means electron, and so forth, where the meaning of each English word was mentally represented in the head of the speaker by its semantic equivalent in whatever system of mental representation the speaker's mind uses. This tacit knowledge of word-meaning obviously *couldn't* guide judgments about the applicability of a word under hypothetical conditions. For knowing that (e.g.) “swan” means swan doesn't entail knowing anything non-trivial about the applicability conditions of “swan” (unless it's assumed, again contrary to Section One, that possession of the concept SWAN entails cognitive access to *its* content, and thereby to the word's content). In order for propositional knowledge of a particular word's meaning to guide judgments about the applicability of the word under hypothetical circumstances, then, this knowledge must be of non-trivial conditions for the applicability of the word. For example, it could be knowledge of a non-trivial definition of the form, “Necessarily, *W* refers to *x* iff *x* is . . .”; or knowledge of a definite description (meaning-giving *or* reference fixing) of the form, “*W* refers to the *F*.”

So let's see whether the view that semantic competence with a word is propositional knowledge of non-trivial conditions for the applicability of the word provides any reason to expect that A3 is true. On this view, whenever

a speaker makes a verbal judgment about a hypothetical circumstance, the speaker must be manifesting her semantic competence with the words used, and hence she must be manifesting her propositional knowledge of non-trivial conditions for the applicability of the words. On this view, then, semantic competence with a word is at least *one* factor influencing a speaker's judgments about the applicability of the word under hypothetical circumstances. The problem, however, as we'll see, is that it won't be the *only* such factor, and the speaker will have no a priori means of disentangling its contribution from that of other factors that have nothing to do with semantic competence.

It's consistent with the current view of semantic competence, and in any case true, that some people are semantically competent with "swan" whilst falsely believing that swans must be white (because they believe that swans have turned out—a posteriori—just to *be* white birds of a certain sort); compare the chef in Section One who, we can assume, was competent with the word "vinegar" whilst falsely believing that vinegar must be formic acid (because he believed that vinegar had turned out—a posteriori—just to *be* formic acid). And for all that we can tell a priori, all of us have some false beliefs of this kind. Yet surely people with false beliefs of this kind are influenced by those beliefs when asked whether a given word would apply were certain hypothetical circumstances to obtain; we don't expect someone who holds that swans must be white to say that, if every other bird species were black, swans wouldn't be white. Accordingly, our verbal judgments about hypothetical circumstances respond to at least two influences: (1) our semantic competences, i.e., given the current view of semantic competence, our tacit knowledge of non-trivial conditions for the applicability of words; and (2) false metaphysical beliefs about what things have turned out—a posteriori—to be, and hence what conditions those things must meet in order for certain words to apply to them.

Since the second of these two influences is a source of error, our verbal judgments about hypothetical circumstances must themselves be liable to error. Now that's not a problem in itself. What is a problem, however, is that the different contributions to our judgments about hypothetical cases made by semantic competence, on the one hand, and by false metaphysical beliefs, on the other, can't be disentangled from one another a priori, and so we can't tell a priori *which* of our particular judgments about hypothetical cases are infected with error and which, since they reflect semantic knowledge, are not. Thus, even the theory that semantic competence is propositional knowledge of non-trivial conditions for the applicability of words provides no reason to expect that A3 is true, i.e., that this semantic knowledge can yield a priori knowledge of analytic truths by guiding linguistic judgments about hypothetical cases.¹⁴ Why can't the different contributions to our judgments about hypothetical cases made by semantic competence, on the one hand, and by false metaphysical beliefs, on the other, be disentangled from one another a priori? Because true beliefs that (allegedly) reflect semantic competence

aren't introspectively recognizable *as* reflecting semantic competence, and hence aren't introspectively distinguishable from false metaphysical beliefs that don't reflect semantic competence; neither can the two kinds of belief be distinguished by appeal to conceivability, since their negations are equally inconceivable to their holders; and no other a priori means of distinguishing between the two kinds of belief suggests itself.

I'll end this section by explaining how the problem for linguistic analysis that I've raised afflicts linguistic analysis as recently defended by Frank Jackson (Jackson 1998). Here's the Jackson story of how linguistic analysis is possible. Terms in natural languages have, as one part of their meaning, a so-called *A-intension*, a function from possible worlds, considered as actual, to referents: had w_1 turned out to be actual, the referent of term T would have been so-and-so; had w_2 turned out to be actual, the referent of T would have been such-and-such; and so forth. Intuitively, the A-intension of a term can be thought of as a rigidified definite description that's formed by Ramsifying the conjunction of the folk platitudes expressible by using the term; and this rigidified description fixes the reference, rather than gives the meaning, of the term. Because terms with A-intensions form sentences, any indicative sentence also has an A-intension, but in the slightly different sense of a function from possible worlds, considered as actual, to truth or falsehood. Some sentences, however, because they have an A-intension that maps *all* possible worlds, considered as actual, onto truth, express what amount to analytic truths; and these analytic truths are the necessary truths that linguistic analysis can discover. What makes them susceptible of a priori discovery is the a priori accessibility of the A-intension of any term, and hence of any sentence, that you understand. These a priori accessed A-intensions can then guide judgments about hypothetical cases. And how is the a priori accessibility of A-intensions meant to arise? Presumably from one's semantic competence with terms and sentences: **semantic competence with a term or sentence just *is* grasping the A-intension of the term or sentence** (e.g., Jackson 2001, 623).

As this sketch makes clear, Jackson's story exemplifies the general strategy for defending linguistic analysis with which this section began, and hence requires for its success the truth of A2 and A3. In that case, however, it's open to the objection that every extant theory of semantic competence with public words either entails that A2 is false or fails to justify any expectation that A3 is true. Admittedly, it's not clear which of the three kinds of theory of semantic competence examined above Jackson would prefer, though the second and third kinds perhaps mesh best with what he says; but the question doesn't much matter. What matters is that there's no sign that he favors some *fourth* kind of theory of semantic competence that might avoid the objection.

The claim that the A-intensions of terms and sentences that one understands should be accessible a priori is something Jackson often seems to treat as obvious, and as independent of contested accounts of semantic



competence. Why? One possible part of the reason is explored at the very end of this paper, but another possible part is the reasoning embodied in the following passage:

...the A-extension of *T* at a world *w* is the extension of *T* at *w* given *w* is the actual world, and so does not depend on whether or not *w* is in fact the actual world. Or, in other words, knowledge of the A-intension of *T* does not require knowledge of the nature of the actual world. (Jackson 1998, 50)

But the inference here (“in other words”) is a non-sequitur. Given that term *T* has a particular A-intension, the A-extension of *T* in each world is fixed, and hence someone who *already knows* *T*’s A-intension doesn’t need to learn whether world *w* is actual in order to know *T*’s A-extension at *w*. If that’s what Jackson’s first sentence is saying, then it’s true. But how did the prior knowledge of *T*’s A-intension arise in the first place? It can’t be a brute fact that *T* has this particular A-intension; something has to make it the case that *T*’s A-intension is what it is, and that something must be the nature of the reference-determining relation. If the reference-determining relation turns out to be one thing, then terms will have certain A-intensions; but if the reference-determining relation turns out to be another thing, they will have others. For example, suppose a term turns out to refer to whatever it is conventionally used to communicate thoughts of¹⁵; then had the actual world turned out to be one in which “water” was conventionally used to communicate thoughts of butter, “water” would’ve referred to butter; had the actual world turned out to be one in which “water” was conventionally used to communicate thoughts of steel, “water” would’ve referred to steel; and so forth. Or suppose a term turns out to refer to whatever its ancestor-tokenings had to correlate with in order to explain the subsequent proliferation of the term¹⁶; then had the actual world turned out to be one in which the ancestor-tokenings of “water” correlated with XYZ, “water” would’ve referred to XYZ; had the actual world turned out to be one in which the ancestor-tokenings of “water” correlated with milk, “water” would’ve referred to milk; and so forth. However, learning the nature of the reference-determining relation is learning an a posteriori identity claim, and hence requires a posteriori investigation of the actual world. So, Jackson’s second sentence is false: knowing the A-intension of *T* *does* require knowing the nature of the actual world.¹⁷

Any residual sense that linguistic meaning just *has* to be accessible a priori can be attributed to tacit endorsement of what we might call the Humpty Dumpty theory of meaning, the idea that words mean what they do as a result of someone’s sufficiently authoritative or powerful say-so—which, given atheism, means the sufficiently authoritative or powerful say-so of *us*. For if we get to *decide* what means what, surely we must automatically *know* what means what.¹⁸ Against Humpty Dumpty, suffice it to note that the possession by concepts and words of semantic properties can’t in the general



case be determined by someone's say-so, for saying-so (or thinking-so) is intentional and hence *presupposes* semantic properties.

III

Conceptual and linguistic analysis, then, seem to lack a theoretical grounding. But can philosophers live without them? I think so. Conceptual and linguistic analysis aim to use the method of hypothetical cases to gain a priori knowledge of necessary truths construed as conceptual or analytic. Let's keep the goal of gaining knowledge of necessary truths (leaving moot the question of their status as conceptual or analytic), but abandon the requirement that this knowledge be a priori. We can do so by construing the desired necessary truths as a posteriori identity claims, paradigms of which are the claims that table salt is NaCl, that genes are segments of DNA, and that having AIDS is being infected by an HIV virus. Then, instead of aiming to discover a priori the conceptually necessary and sufficient conditions for it to be true that x knows that p , we can aim to discover a posteriori some true informative identity claim of the form, "knowing = ...", and likewise for other philosophically interesting properties such as rationality, freedom, or personhood.

However, that we can replace the traditional goal of conceptual or linguistic analysis with knowledge of a posteriori identity claims doesn't entail that philosophers can live without conceptual or linguistic analysis. It's possible that validating a posteriori identity claims is a posteriori—it requires empirical investigation—but that it also requires conceptual or linguistic analysis. And Frank Jackson takes this possibility to be actual. He holds that in order to validate the claim that water = H₂O, one must learn via linguistic analysis that water = the (actual) F , next discover empirically that the (actual) F = H₂O, and then deduce, since identity is transitive, that water = H₂O (Jackson 1998, 57–60).

However, there's another way to validate a posteriori identity claims that doesn't require conceptual or linguistic analysis at any stage. It works by exhibiting a posteriori identity claims as the best explanation for some set of facts. A concrete illustration concerning table salt will make such reasoning clear. We learn, by standard empirical means, such facts as that salt readily dissolves in water, and that it forms crystals of a certain size, shape, and color. Independently, we learn that the chemical compound NaCl has a certain molecular composition and structure. We then realize that, given physical chemistry, any substance that has this molecular composition and structure could be *expected* to dissolve as readily in water as salt in fact does, and to form crystals of just the size, shape, and color that salt in fact forms. Perhaps we also learn, by standard empirical means, that salt and NaCl always co-occur. We're now in a position to explain *why* salt dissolves in water as readily as it does, and forms crystals of the size, shape, and color that it does: salt

just is NaCl, and therefore has a molecular composition and structure that, on independent grounds, would lead you to expect it to have these observed properties. Other explanations of why salt has these properties (and co-occurs with NaCl) can be imagined, but they're less parsimonious and hence less good than the one hypothesizing that salt = NaCl. Moreover, salt has no properties that definitely *can't* be explained on the supposition that it's the same stuff as NaCl. So, by inference to the best explanation, the fact that salt dissolves in water as readily as it does and forms crystals of a certain size, shape, and color provides evidence that salt = NaCl (for an extended treatment, see Melnyk 2003, 240–56).

The crucial point about this way of validating a posteriori identity claims is that it doesn't require conceptual or linguistic analysis; that is, at no stage does it use the method of hypothetical cases, aimed at gaining a priori knowledge of necessary truths. It certainly requires being able to *recognize* samples of salt and NaCl, otherwise we couldn't find out about salt's crystals or the structure of NaCl. But being able to recognize samples of some stuff *S* doesn't require using the method of hypothetical cases. We recognize things well enough for both everyday and scientific purposes by using a huge variety of contingent but sufficiently reliable signs of them (e.g., their textures, colors, smells, and tastes)—not by using the method of hypothetical cases to discover analytically sufficient conditions for “*S*” to apply and then examining candidate stuffs to see whether they meet any of those conditions. Again, the above way of validating a posteriori identity claims certainly requires being able to discover the *actual* properties of salt and NaCl. But it requires no ability to judge what properties salt and NaCl *would* have, if specified hypothetical conditions *were* to obtain (even though we probably have such an ability; see Section Four).

Brie Gertler also holds that the validation of a posteriori identity claims inevitably requires conceptual analysis as well as empirical inquiry (Gertler 2002). Her central claim is that “evidence for a reduction [i.e., an a posteriori identity claim] must be deemed as such by the concept of the reduced kind”, so that being justified in accepting an a posteriori identity claim requires performing conceptual analysis on the concept of the reduced kind (Gertler 2002, 23). You might wonder how the status of an empirical discovery as evidence for an a posteriori identity claim even *could* be determined by the concept of the reduced kind, but the answer becomes clear when you realize that Gertler endorses Jackson's account of validating a posteriori identity claims. Suppose we discover a posteriori that the actual watery stuff = H₂O; so what? What is the relevance of such a discovery to any proposed identity claim? Gertler would answer that it has no relevance at all *until* we learn a priori, by conceptual analysis, that water = the actual watery stuff. Of course, once joined with the a priori premise that water = the actual watery stuff, the empirical discovery that the actual watery stuff = H₂O permits us to deduce that water = H₂O. Thus, conceptual analysis tells us whether an empirical discovery is evidence for an a posteriori identity claim by telling us whether

the discovery can contribute to a Jacksonian derivation of the identity claim.

But Gertler's central claim doesn't show that conceptual analysis is required *simpliciter* to validate a posteriori identity claims. It only shows that conceptual analysis is required if the validation takes the Jacksonian form. But the validation could instead take the form of an inference to the best explanation, as presented above, in which conceptual analysis plays no role. In this alternative form of validation, an empirical discovery counts as evidentially relevant to an a posteriori identity claim for the same reason that *any* empirical discovery counts as evidentially relevant to a hypothesis that it supports: the discovery is (very crudely) something you would reasonably expect on the assumption that the claim is true. So conceptual analysis isn't required for validating a posteriori identity claims if the validation takes the alternative form presented above.

Gertler would disagree. While acknowledging that "we might well be disposed to accept [an] increase in explanatory force as evidence for a reduction [i.e., an a posteriori identity claim]", she still insists that "this disposition reflects our concept of the target: it is a conceptual truth that water is a natural kind, individuated by whatever it is that explains the macro properties of the watery stuff around here" (Gertler 2002, 37). But Gertler gives no argument that a disposition to accept an increase in explanatory power as evidence for an a posteriori identity claim *must* arise in her way; for all that she's said, the disposition could arise in the same way in which a disposition to treat explanatory power as evidential arises in those many inferences to the best explanation where the conclusions aren't a posteriori identity claims and hence there's no "concept of the target" to be conceptually analyzed. And if it does arise in this way, then conceptual analysis still hasn't been shown to be necessary for the validation of a posteriori identity claims.

IV

Conceptual and linguistic analysis, then, are philosophically dispensable. It doesn't follow, however, that the method of hypothetical cases is useless. In conceptual and linguistic analysis, the method is aimed at a particular target: the discovery a priori of necessary truths. But what if it's aimed at another target? In particular, can it play a role in validating a posteriori identity claims? The question is hard, because the answer depends on what's actually going on in our minds when we use the method—something we don't yet know. But I suggest the answer is yes. I begin with an account of what's going on psychologically when we use the method of hypothetical cases.¹⁹

We possess various capacities to reach warranted conclusions about the non-hypothetical world; call these our *epistemic capacities*, since although they presuppose, they don't constitute, our capacities to *think* about the world. Our epistemic capacities include capacities to recognize individuals

and kinds, and to engage in deductive and non-deductive inference of many types; ordinarily, therefore, these capacities take as inputs mental representations of the *actual* world, yielding further such representations as outputs. However, I suggest, when we ask ourselves whether a given concept or word would apply were certain hypothetical circumstances to obtain, we form mental representations of *hypothetical* circumstances and allow our epistemic capacities to take *those* representations as inputs; we can then answer questions about whether a given concept or word would apply under the represented hypothetical circumstances by consulting the output representations of our epistemic capacities.

These epistemic capacities, however, embody substantive a posteriori assumptions, assumptions that inevitably affect the outputs of our epistemic capacities when they take representations of hypothetical circumstances as inputs. One familiar kind of a posteriori assumption concerns which features of the world are reliable indicators of which other features. Thus, in judging whether there would be gold under certain hypothetical circumstances, we exercise epistemic capacities that embody assumptions about what signs reliably indicate gold. A second and unremarked kind of a posteriori assumption, however, concerns which a posteriori identity claims are true; for example, a capacity to infer the presence of formic acid from that of vinegar might assume not that vinegar is a reliable sign of formic acid but that it *is* formic acid. Assumptions of this second kind are crucially important when our epistemic capacities take representations of hypothetical circumstances as inputs. They explain why sometimes we consider a hypothetical case and judge that it just *must*—as a matter of more than nomological necessity—be a case of so-and-so; the explanation is that we assume that $F = G$, so that any case of F must—metaphysically—be a case of G , and thus we can make nothing of the idea of an F that isn't a G . Such an explanation, of course, makes no appeal to analyticity or conceptual necessity (construed as knowable a priori); it can be given by philosophers who doubt either the existence or the a priori accessibility of analyticity and conceptual necessity.

Epistemic capacities embodying either of these two kinds of a posteriori assumptions are fallible, since we can be and often are mistaken concerning what is a reliable sign of what else, and which a posteriori identity claims are true. So our answers to questions about whether a given concept or word would apply under hypothetical conditions are also fallible. They will be at best accidentally correct if the assumptions they embody are incorrect. Moreover, even a true assumption about what is a reliable sign of what in the actual world might lead to error if applied to a sufficiently different hypothetical world.

So can our answers to questions about hypothetical cases be used to validate a posteriori identity claims? To the extent that these answers are guided by assumptions concerning a posteriori identity claims, I think not, since they merely manifest the respondent's prior identity beliefs; they provide no

evidence that these beliefs are true. To the extent that our answers to questions about hypothetical cases are guided by assumptions concerning reliable signs, I'm more positive. Though fallible, these assumptions are the very assumptions embodied by the epistemic capacities we use in judging the applicability of concepts or words to the *actual* world. So, provided that the hypothetical circumstances we consider aren't so different from actuality as to render the assumptions concerning reliable signs inapplicable, they'll be as reliable when applied to hypothetical as when applied to actual circumstances. Thus, merely considering a not-too-far-fetched *hypothetical* Gettier case might be no less valuable evidentially than observing an *actual* case of the very same type. And whether the hypothetical circumstances we consider *are* so different from actuality as to render the assumptions concerning reliable signs inapplicable is a question that could be sensibly discussed. Ironically, then, if we're asked whether a given concept or word applies in a hypothetical situation and we want to answer that it *must* apply, then our answer has likely been guided by an a posteriori identity assumption for which no new evidence has been provided. But if we want to answer that the concept or word *probably* applies in the hypothetical situation, then our answer has likely been guided by an assumption about reliable signs, and our giving the answer may well be some evidence that it's correct; the answer can then be used as evidence in the validation of an a posteriori identity claim.

Even if conceptual and linguistic analysis are abandoned, then, the method of hypothetical cases needn't be. However, it has its limitations: the answers we give to questions about the applicability of concepts or terms in hypothetical circumstances don't always provide evidence relevant to validation of an a posteriori identity claim; such evidence as they do provide must go into the pot, democratically, along with possibly conflicting evidence obtained from investigation of the actual world; and when our judgments about hypothetical cases do provide evidence, it's because they result from our exercise of epistemic capacities off-line, not because they reflect some special insight into conceptual or semantic reality.

I've suggested that, when we use the method of hypothetical cases, we host mental representations of hypothetical circumstances and then exercise epistemic capacities that embody substantive and fallible empirical assumptions. And this account obviously differs from the account assumed by believers in conceptual or linguistic analysis, the account partially given by prerequisites C1 through C3 and A1 through A3. But I see no way that by *introspecting* while using the method of hypothetical cases you could tell which account (if either) was correct: both accounts predict the same introspectible phenomena. So the very existence of my alternative account shows that we can't just *assume* that what's going on when people make judgments about whether a given concept or word would apply under hypothetical circumstances is what conceptual or linguistic analysts think is going on. And yet conceptual and linguistic analysts typically assume exactly this. Chalmers

and Jackson, for example, take our reaction to a standard Gettier case to be an incontestable example of conceptual analysis, as if taking it to be anything else would be strained and unnatural (Chalmers and Jackson 2001, 321 and 337).²⁰ Likewise, Brie Gertler, in considering people who after reflection say under what conditions they would acknowledge that their term “cat” was empty, just assumes that these people are engaging in conceptual analysis—rather than applying their tacit assumptions about the nature of content-determination for natural languages (Gertler 2002, 24). Both conceptual and linguistic analysis are taken too much for granted, and they shouldn’t be.²¹

Notes

¹ In my terminology, Frank Jackson’s recent book, despite its title, defends linguistic, not conceptual, analysis (see his 1998 and especially his 2001); it will therefore be treated in Section Two.

² I shall say no more about the character of the reflection alluded to here, but it’s a central focus of recent work by David Henderson and Terry Horgan, who argue that such reflection amounts to inference to the best explanation that draws upon indisputably empirical data (see their 2000 and 2001).

³ Perhaps like this (*pace* Boghossian 1996, 59–60). Suppose that concept C1 rigidly designates the property of being brown and a dog, and that concept C2 rigidly designates the property of being a dog. Now consider the claim that all things picked out by C1 are things picked out by C2. Such a claim is true if the set of items picked out by its subject term is a subset of the set of items picked out by its predicate. But the claim meets this perfectly standard condition for the truth of a universal claim, and meets it regardless of which world is the actual world, solely in virtue of the *references* of C1 and C2. We might therefore consider it a conceptual truth. A similar story yields the result that identity claims formed using rigid designators are also conceptual truths.

⁴ I take no stand, and need to take no stand, on the plausibility of this implication of Fodor’s account.

⁵ There’s a way in which even accounts of concept-possession that tell an externalist story about content-determination might seem hospitable to conceptual analysis. For such accounts can allow the existence of *non-primitive* concepts (i.e., those identical with syntactically well-formed complexes of primitive concepts); and the content of these concepts would be *internally* determined, given that the content of their primitive constituents has already been (externally) determined (see, e.g., Fodor 1998, 28). However, the appearance of friendliness to conceptual analysis here is illusory. For users of the method of hypothetical cases have no a priori way of telling whether the answers they give concerning the applicability of a given concept to hypothetical cases reflect the successful analysis of a complex concept (if it is a complex concept) or something quite different (see Section Four for what that might be).

⁶ Henderson and Horgan, for example, seem to favor a hybrid view in which some elements of a concept’s content are not externalistically determined (see their 2000, 61–66).

⁷ Well, nearly all such accounts. Paul Boghossian suggests that competence with a concept is a matter of following a rule, where rule-following can’t, however, be reduced to having dispositions (Boghossian 1996, 382). But if rule-following *isn’t* reduced to dispositional facts, then it’s hard to see how you can tell a priori what rules you are following and hence the content of your concepts.

⁸ A similar argument is used by Henderson and Horgan (2001, 236–7) against the views of Christopher Peacocke in particular.

⁹ If a dispositionalist account is descriptivist, therefore, it must hold that the possession of a given concept should be identified with the thinker's association of the concept with some proper subset of the totality of the descriptions that the thinker in fact associates with the concept. With this point recognized, the arguments in the text below can easily be seen to apply to descriptivist accounts.

¹⁰ Of course, if the Chalmers and Jackson account of conceptual analysis is true, then even identity hypotheses regarded by the recent tradition as a posteriori are a priori for someone who only possesses enough empirical information (Chalmers and Jackson 2001). But to object on these grounds to the claim in the text would beg the question, simply assuming the viability of conceptual analysis when that is precisely the matter at issue. Also, it wouldn't help, since whatever exactly "enough empirical information" amounts to, merely possessing the concept of possessing concept *C* wouldn't ensure that you had it; so discovering that possessing *C* = having just this set of dispositions wouldn't be an automatic consequence of possession of the concept of possessing *C*, and hence wouldn't be a priori in the sense under discussion.

¹¹ Thanks for this suggestion to David Papineau.

¹² It might be objected to my reasoning that when the chef becomes convinced that vinegar is formic acid he ceases to possess VINEGAR and comes to possess a new concept, SCHVINEGAR instead—and that schvinegar really must be formic acid. As I've told the story, however, the chef has so much practical and theoretical knowledge about vinegar that to suppose him to have ceased to possess VINEGAR in the circumstances described would be highly implausible.

¹³ Conversely, having a mental disposition while being *able* to conceive a counterexample to its corresponding generalization doesn't rule out that the generalization is necessarily true. For example, someone with a very modest chemical education, who knows that all diamonds contain carbon atoms but who doesn't know that diamonds are arrangements of carbon atoms and hence must contain carbon atoms, has a disposition to apply DIAMOND only to things containing carbon atoms, but is quite able to conceive that some diamond contains no carbon atoms.

¹⁴ Henderson and Horgan (2001) introduce a notion of the "low grade a priori" that seems to imply that knowledge of analytic truths could still be a priori despite the point made in this paragraph. Of course, the a priori isn't terribly well-defined, and there may be no substantive disagreement between them and me. But I do wonder what exactly is known a priori in their view. Not analytic truths, the alleged final product of linguistic analysis, since according to Henderson and Horgan a posteriori considerations are required to achieve this product. Certain particular judgments about hypothetical cases, those that don't *in fact* reflect the influence of false beliefs? Perhaps, but even when these particular judgments are true and reliably produced, they are still alarmingly fluky, given their a priori indistinguishability from judgments that do reflect the influence of false beliefs; so they may not count as knowledge. In any case, the conclusions of this paper are consistent with allowing that such particular judgments are known a priori.

¹⁵ Here I hint at a Gricean account of the semantics of natural languages.

¹⁶ Here I hint at Ruth Millikan's rich account of the semantics of natural languages (Millikan 1984).

¹⁷ See again note 10. Also, essentially the same fallacy as Jackson's is committed by David Chalmers when he writes of concepts:

The primary intension [= A-intension] of a concept. . . specifies how reference depends on the way the external world turns out, so it does not itself depend on the way the external world turns out. (Chalmers 1996, 57)

The claim introduced by "so" does not follow, at least if it's taken to imply that primary intensions are knowable a priori. The primary intension of a concept depends precisely on what the reference-determining relation for concepts turns out to be, and that's an a posteriori matter.

¹⁸ There's a whiff of Humpty Dumpty in the penultimate sentence of Brie Gertler's defense of conceptual analysis (Gertler 2002, 45) when she writes that "We, the concept-possessing folk, are the ultimate authorities regarding what falls under our concepts."



¹⁹ An interesting alternative answer is presented by Stephen Laurence and Eric Margolis in their critique of Jackson's defense of linguistic analysis (Laurence and Margolis 2003).

²⁰ Note also their confident reference to "the *observation* that we have an a priori grasp of how our concepts apply to specific epistemic possibilities, when these are described in sufficient detail" (Chalmers and Jackson 2001, 341; italics added).

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