

ABSTRACT

Title of Dissertation: CONCEPTS: TAKING PSYCHOLOGICAL EXPLANATION SERIOUSLY

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What do we need a theory of concepts for? Two answers to this ‘meta-level’ question about concepts figure prominently in the recent philosophical literature, namely, that concepts are needed primarily for the purposes of psychological explanation, and that concepts are needed primarily for the purposes of normative epistemology. I argue that the psychological perspective leads to what I call ‘Judgment Pragmatism’, which is a version of conceptual/inferential role semantics according to which concepts are not constitutively tied to rationality and knowledge.

I begin in Chapter 1 by distinguishing two uses of the term ‘concept’ found in the literature, and laying out some constraints on any adequate theory of concepts. In Chapter 2, I articulate the two meta-level approaches under consideration, and explain how the work of Jerry Fodor and Christopher Peacocke is representative of the psychological and epistemological perspectives, respectively. I also show that the meta-level question is distinct from the object-level question of whether Fodor’s Informational Atomism or Peacocke’s ‘Concept Pragmatism’ is correct.

In Chapter 3, I distinguish two versions of Concept Pragmatism: Judgment Pragmatism, which individuates concepts in terms of mere judgment, and Knowledge Pragmatism, which individuates some concepts in terms of knowledge. I argue against Peacocke's claim that the former leads to the latter, and show that the perspective of psychological explanation provides us with reasons to resist Knowledge Pragmatism. I then consider, in Chapter 4, one of Peacocke's arguments for Judgment Pragmatism, and articulate the Quinean Challenge it faces. In Chapter 5, I argue that Quine's arguments against the analytic/synthetic distinction are inadequate, and that Concept Pragmatism is not vulnerable to Fodor's empirical case against the analytic.

I then make the empirical case for Judgment Pragmatism, in Chapter 6, by defending the view that positing the analytic/synthetic distinction is a piece of explanatory psychology. In Chapter 7, I consider the dialectical role of Frege cases, and argue that adopting the psychological perspective allows us to stake out a middle ground between Fodor's 'syntactic' treatment and Peacocke's claim that concepts are constitutively tied to reasons and rationality. Chapter 8 offers some concluding thoughts.

CONCEPTS: TAKING PSYCHOLOGICAL EXPLANATION SERIOUSLY

by

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We are in the midst of a major interdisciplinary attempt to understand the mental process by which human behavior accommodates to the world's demands—an attempt to understand human rationality, in short. Concepts are the pivot that this project turns on since they are what mediate between the mind and the world. Concepts connect with the world by representing it, and they connect with the mind by being the constituents of beliefs. If you get it wrong about what concepts are, almost certainly you will get the rest wrong too.

-Jerry Fodor

Asking a psychologist, a philosopher, or a linguist what a concept is is much like asking a physicist what mass is. An answer cannot be given in isolation. Rather, the term plays a certain role in a larger world view that includes the nature of language, of meaning, and of mind. Hence the notion of a concept cannot be explicated without at the same time sketching the background against which it is set; and the “correctness” of a particular notion of concept cannot be evaluated without at the same time evaluating the world view in which it plays a role.

-Ray Jackendoff

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CHAPTER 1

CONCEPTS: A DISTINCTION AND SOME ASSUMPTIONS

Introduction

Unlike other species on the planet, human beings think about and understand the world by using an incredibly rich conceptual repertoire. Of course we also represent things perceptually, and since we share some of our innate perceptual machinery with other species, presumably such perceptual representations are shared as well.¹ There is, moreover, evidence that we share at least some of our conceptual capacities with other species (Munakata *et al.* 2001, Hauser 2000). But the sheer number and diversity of concepts that humans exploit in thinking about the world is surely one feature of our minds that distinguishes it from the minds of other species. We can think about quarks, quasars, infinity, life, democracy, and disease, which, *prima facie*, are *not* represented perceptually. Thinking about these things requires concepts.²

Given that concepts are central to all aspects of human cognition, it's unsurprising that theorists approach the task of providing a theory of concepts from many perspectives. Developmental psychologists, for instance, are interested in characterizing the innate representations that children bring to experience, as well as the learning mechanisms that are required in order for them to construct concepts from those innate

¹ For instance, there are homologies between the low-level visual systems of human and non-human primates (Tootell *et al.* 1996).

² There is, of course, an empiricist tradition in philosophy and psychology according to which all of our concepts are constructed out of (and definable in terms of) perceptual primitives. In my view, there are powerful empirical and philosophical arguments against such empiricism. We'll discuss this further in Chapter 2.

representations (e.g., Carey 1985, Keil 1989). Others set aside issues of development and acquisition, and focus on characterizing the nature of the concepts that furnish the adult mind, which will be our concern. A brief look at the philosophical and psychological literature reveals a range of theories both about the nature of concepts and how they get their content.

There are so-called “classical” or “definitional” theories, according to which a concept specifies a set of necessary and sufficient conditions for something’s falling under it. The concept BROTHER³, on such a view, might specify that something falls under it *just in case* it’s a *male sibling*; and the concept BACHELOR might specify that something’s a bachelor *just in case* it’s an *unmarried adult male*. Although such views have a long, venerable history, they’ve come under attack in recent years, and from *many* different quarters (see e.g., Wittgenstein 1953/1958, Quine 1953a, Putnam 1970, Fodor *et al.* 1980, Smith and Medin 1981, Armstrong *et al.* 1983).

In light of the problems plaguing definitions, some theorists claim that a concept specifies necessary *but not* sufficient conditions for something’s falling under it (Jackendoff 1983, Pinker 1989). For example, the concept RED might specify that something can’t satisfy it *unless* it’s colored; the concept KILLING might specify the condition ‘causing to die’; and the concept PERSUADE might specify the condition ‘cause to believe’. Others claim, in a similar vein, that some of the arguments against definitions (e.g. Quine’s) are inconclusive, and that it is still possible to hold that our analytic intuitions are best explained by positing constitutive connections among concepts (Horwich 1992, Rey 1993a). Other approaches depart from the classical

³ I follow the convention of using words written in small capitals to refer to concepts. When quoting, I change the author’s notation to fit mine.

tradition altogether, claiming that conceptual structure consists in probabilistically weighted lists of features (Rosch and Mervis 1975, Rosch 1978). While still others claim that conceptual structure consists in relations to other concepts as specified by a mental theory of some kind (Murphy and Medin 1985).

The nature of conceptual *content* is an issue that is addressed almost exclusively by philosophers, and as one might expect, this literature is no less diverse. Some claim that concepts have the content they do in virtue of law-like connections they bear to the external (extra-conceptual) world (Dretske 1981, Fodor 1987, 1990). Others claim that conceptual content is determined by conceptual/inferential role, or perhaps a mix of internal role and external environment, or truth-conditions (Field 1977, Loar 1981, Peacocke 1992a, Block 1986). And still others claim that conceptual content is determined by certain information-carrying functions, which are the product of evolution by natural selection (Papineau 1987, Millikan 1984). Each of these ambitious proposals has problems of its own, and in my view it's fair to say that none of them qualifies as an adequate (counterexample-free) theory of what concepts are, or how they get their content.

At this point, what's important to notice about these theoretical disagreements is that they're all, as it were, *at the object level*. That is, they're disagreements about what concepts are and how conceptual content is determined. Let's take a step back from these disagreements and consider what we might think of as a "meta-level" question about concepts, namely: *What is a theory of concepts for?* As the epigraph from Jackendoff makes clear, theorists approach the task of providing a theory of concepts from what are often quite different perspectives, and thus invoke concepts for quite different purposes.

The question I want to consider is whether a theory of concepts is needed primarily for the purposes of *psychological explanation* or *normative epistemology*, two purposes for which concepts have been recruited that figure prominently in the recent philosophical literature on concepts.

As Jackendoff points out, understanding the ‘background’ against which some theorist’s view of concepts is set is crucial for understanding the theory itself. The reason for this is straightforward. In the case at hand, a theorist who approaches the task of providing a theory of concepts from the perspective of normative epistemology will have an explanatory agenda that is quite different from the agenda of one who approaches the task from the perspective of psychological explanation. These different explanatory agendas will, in turn, determine the shape of the respective object-level theories in quite distinctive ways.

Of course, a theorist’s stance on the meta-level question—What is a theory of concepts for?—is often left largely implicit. But, since one’s stance on the meta-level question affects the outcome of one’s theoretical investigations at the object level, it’s worth sharply distinguishing these different ways of answering it. As it happens, two theorists whose work is at the forefront of the recent philosophical literature on concepts, Jerry Fodor and Christopher Peacocke, approach the task of providing a theory of concepts in just these different ways. Fodor approaches the task from the perspective of psychological explanation, while Peacocke approaches the task largely from the perspective of normative epistemology. Their work will thus provide an important focal point in our consideration of how one’s meta-level stance affects one’s object-level theory.

Speaking of the difference between these meta-level approaches, Fodor has recently said the following:

It may be that one has eventually to choose between getting a theory of concepts that is epistemologically useful and getting one that's useful for understanding how the mind works. That would hardly be surprising; epistemology really is a normative discipline, and psychology really isn't. (2004b, p.106)

If Fodor is right about this, then the meta-level question is very important indeed. For the implication is that an ecumenical approach to the meta-level question is not an option. In the remainder of this chapter, I spell out some necessary background. In Chapter 2, I consider these different meta-level perspectives in greater detail.

It's worth emphasizing at the outset, however, that this dissertation is not merely (or even mostly) about methodological, or metaphilosophical questions about concepts. There will be some of that, of course. But my main aim is to make clear what sorts of object-level claims about concepts can be defended by taking seriously the perspective of psychological explanation. Although I won't be arguing that the psychological perspective in fact *has* priority, I will argue that adopting the psychological perspective ultimately leads to a view that effectively splits the difference between the object-level theories of Fodor and Peacocke.

A Distinction and Some Assumptions

Throughout the dissertation I'll be making some assumptions about concepts. In what follows I make these clear, and note a distinction between two different uses of 'concept' that often get run together in the literature. I'll start with the latter.

In the psychological literature, concepts are often identified with mental representations, whereas in the philosophical literature they're often taken to be abstract, "graspable" entities of some kind. However, I take it to be untendentious in both philosophy and psychology that concepts are the constituents of thoughts, e.g. beliefs, desires, and the like. The belief that it's raining, for instance, has as an ingredient the concept RAINING. If you don't possess the concept you can't entertain the belief. While this should be uncontroversial, it doesn't clarify matters much. For there's a notorious state/content ambiguity in propositional attitude terms. For instance, one might take the sentence "John believes that it's raining" to be about *what* John believes: he believes *that it's raining*, and not that it's snowing or hailing. Alternatively, one might take the sentence "John believes that it's raining" to be about the *state* that John is in: he *believes*, rather than hopes or fears that it's raining. Claiming that concepts are the constituents of thoughts is thus ambiguous between claiming that they're the constituents of mental *states* and claiming that they're the constituents of mental state *contents*.

For those who identify concepts with mental representations, it's natural to think of concepts as the constituents of *mental states*. Fodor, for instance, says that he uses the term 'thought' to pick out mental representations, which on his view are what *express* the objects of the propositional attitudes (1998a, p. 25). Since concepts are the constituents of thoughts, concepts are mental representations that *express* properties, the constituents of the objects of the propositional attitudes. The concept CAT is thus a mental representation that expresses the property of *being a cat*. And to believe that the cat is on the mat is to stand in a (computational) relation to a mental representation that expresses *the cat's being on the mat*.

But it's tendentious to claim that concepts *are* mental representations. For some theorists claim that concepts are individuated differently than their corresponding mental representations. Peacocke, for instance, claims that "[i]t is possible for one and the same concept to receive different mental representations in different individuals" (1992a, p.3). Georges Rey (1998d) agrees, pointing out that one person's concept CITY might be associated with a mental representation corresponding to the predicate 'is a city', while another's might be associated with a mental representation corresponding to the predicate 'is a metropolis'. Rey adds, moreover, that concepts and mental representations come apart in the other direction as well: "one person might use an image of bustling boulevards to express CITY, another to express POLLUTION" (1998d, p. 507).

There are, of course, those who don't think that such arguments establish that concepts can't be identified with mental representations (e.g., Laurence and Margolis 1999). We'll consider these arguments further in Chapter 7. At this point, however, I merely want to draw attention to the distinction between the claim that concepts are mental representations, and the claim that they're the constituents of the *contents* of mental representations. For it's one thing to say that concepts are, as it were, the representational *vehicles* of thoughts (e.g., predicates in the language of thought) and it's quite another to say that they're *what's expressed* by such vehicles.

Luckily for us, most of the discussion in what follows will be sufficiently abstract so that this vexing ontological issue will not be particularly important. When nothing of importance hangs on it, I'll thus be quite loose with my use of 'concept'. When it matters, though, the discussion will keep careful track of the distinction. (It will matter, for instance, when we discuss Frege cases in Chapter 7.)

Now, on to the assumptions that I'll be making about concepts. I take the following to be widely accepted among philosophers and psychologists⁴:

(A) *Concepts are representational, or intentional.* The concept DOG, for example, has the intentional property of being *about* dogs. As Fodor says:

[A]pplications of concepts are susceptible of ‘semantic evaluation’; claims, or thoughts, that a certain concept applies to a certain thing are always susceptible of evaluation in such semantical terms as satisfied/unsatisfied, true/false, correct/incorrect, and the like. (1998, p. 24)⁵

It's important to notice that this assumption is compatible with the existence of empty, or vacuous concepts, i.e., concepts that have no referents. The concept ELF, for example, is not robbed of its intentionality merely because there are no elves. For there's a sense in which it is still *about* elves, even though (strictly speaking, and somewhat paradoxically) there are no things that it is about. In other words, a concept can be *intentional* without referring to anything *in the world*. Or, to put the point slightly differently still, a concept can have an intentional content, even if the object specified in the content doesn't exist.⁶

(B) *Concepts are individuated more finely than reference.* Two kinds of considerations have led philosophers to claim that reference isn't all there is to concept individuation. The first comes from Gottlob Frege (1892), who famously points out that true identity-

⁴ The list is loosely drawn from Fodor (1998a, pp. 23-39) and Prinz (2002, pp. 3-23).

⁵ Jackendoff (2002) is one notable theorist who denies that concepts are intentional.

⁶ This raises a whole host of subtle and complex issues. Some content externalists will deny that a concept can *have* content *unless* there exists a mind-independent object to which it refers. To such philosophers, all I can say is that my intuition that we have *genuine* thoughts about non-existent entities is far from outweighed by the admittedly plausible arguments that motivate content externalism. For a discussion of empty concepts in the context of content externalism, see Rey (forthcoming b, c).

statements can be informative. Thus, someone who possesses the concept MORNING STAR and the concept EVENING STAR might be surprised to find out that the morning star *is* the evening star. If possession of MORNING STAR and EVENING STAR *consisted in* knowing their satisfaction conditions, then this fact would be genuinely puzzling. Indeed, it would be inexplicable. For MORNING STAR and EVENING STAR have the same satisfaction conditions.

A second, related fact that Frege draws our attention to is that in certain contexts coreferential expressions can't be substituted for one another *salva veritate*. Propositional attitude ascriptions are the paradigm. The sentence "John believes that the morning star is beautiful" may be true, while the sentence "John believes that the evening star is beautiful" is false, even though "the morning star" and "the evening star" are coreferential. But if reference were all there is to the individuation of MORNING STAR and EVENING STAR, then we apparently couldn't account for such facts.

Frege's famous solution to these puzzles is to introduce the notion of *sense*, which is, in effect, an aspect or layer of meaning that goes beyond mere reference. There's a sense in which "the morning star" and "the evening star" have the same meaning, since they have the same referent. But there's *another* sense in which they differ in meaning, since they differ in what Frege calls "cognitive significance". The claim that "the morning star" and "the evening star" have different senses is meant to capture this latter sense of meaning.

Positing senses is thus meant to do explanatory work. That one can grasp the sense of "the morning star" without grasping the sense of "the evening star" explains why "the morning star is the evening star" can be informative. It also explains certain sorts of

normative facts, e.g., why one can *rationally* believe that the morning star is beautiful while failing to believe that the evening star is. Moreover, it's a difference in sense that explains why coreferential terms can't be substituted for one another in opaque contexts *salva veritate*. (On Frege's view, the referents of "the morning star" and "the evening star" shift in such contexts. Rather than taking their standard referent, *Venus*, they refer to the *sense* of "the morning star" and the *sense* of "the evening star".)

While Fregean senses can do explanatory work, it's not always clear what Fregeans mean by "sense". What are senses? We'll address this question in Chapter 7. At the moment, though, it's worth noting that one need not posit *Fregean* senses in order to claim that concepts are individuated more finely than reference. For while it's clear that a concept must have *some* property that individuates it more finely than reference, this property needn't be a Frege sense. Indeed, as we'll see in Chapter 7, satisfying this requirement does not even entail that this property be a *semantic* property.

Putnam (1975a) famously draws our attention to examples that illustrate what is essentially the same point as Frege's examples, but from the opposite direction. Putnam imagines a place called "Twin-Earth", which is identical to Earth, except that the watery stuff on Twin-Earth has a different chemical composition than the watery stuff on Earth. On Twin Earth, the liquid that fills the rivers and flows from taps is XYZ, not H₂O. On Earth, my concept WATER refers to H₂O even if I don't know anything about hydrogen or oxygen, whereas my doppelganger's concept WATER (on Earth, we might refer to it as 'TWATER') refers to XYZ even if he doesn't know anything about X, Y, or Z. Despite this difference in reference, there's still a sense in which our WATER concepts have the *same* content. For they're both concepts of the clear, colorless, liquid that fills the rivers

and comes out of the taps (they have the same ‘narrow content’). Thus, whereas Frege’s examples putatively show that identity of reference need not imply identity of (narrow) conceptual content, Putnam’s examples illustrate that identity of (narrow) conceptual content need not imply identity of reference (wide content). Both show that concepts must be more finely individuated than reference.

(C) *Concepts are shareable*. Whatever else concepts turn out to be, they must be the kind of thing that different people (and different time-slices of the same person) can *share*.

Fodor puts the point very nicely; I quote at length:

It seems pretty clear that all sorts of concepts (for example, DOG, FATHER, TRIANGLE, HOUSE, TREE, AND, RED, and, surely, lots of others) are ones that all sorts of people, under all sorts of circumstances, have had and continue to have. A theory of concepts should set the conditions for concept possession in such a way as not to violate this intuition. Barring very pressing considerations to the contrary, it should turn out that people who live in very different cultures and/or at very different times (me and Aristotle, for example) both have the concept FOOD; and that people who are possessed of very different amounts of mathematical sophistication (me and Einstein, for example) both have the concept TRIANGLE; and the people who have had very different kinds of learning experiences (me and Helen Keller, for example) both have the concept TREE; and that people with very different amounts of knowledge (me and a four-year old, for example) both have the concept HOUSE. Accordingly, if a theory or an experimental procedure distinguishes between my concept DOG and Aristotle’s, or

between my concept TRIANGLE and Einstein's, or between my concept TREE and Helen Keller's, etc. that is a very strong *prima facie* reason to doubt that the theory has got it right about concept individuation or that the experimental procedure is really a measure of concept possession. (1998a, p. 29)

The motivation behind this ‘Shareability Constraint’ (as I’ll call it) has to do with the very thing that motivates at least Fodor’s interest in a theory of concepts in the first place, namely, intentional explanation. What’s important about such explanations is that they aim for generality. For just like any other respectable science, intentional psychology aims to provide law-like generalizations, in this case about the causal relations *both* among various mental states *and* between mental states and behavior. But in order for different instances of a given relation between mental states to be subsumed under the same intentional generalization, it must be the case that the states have the same content. And, to a first approximation anyway, intentional contents are shareable *only if* concepts are shareable.⁷

For example, two people share the belief that there’s water in the cup only if they share the concept WATER. If they don’t literally share the concept WATER, then they don’t literally share the belief that there’s water in the cup. But if they don’t share that belief—or any other water-beliefs for that matter—then their water-directed behavior can’t be subsumed under the same intentional generalizations. Giving up on the Shareability Constraint thus apparently requires giving up on intentional explanations, which is why theories that are incompatible with it ought to be abandoned.

⁷ In Chapter 7 we’ll discuss the relation between the Shareability Constraint and intentional explanations in a bit more detail, and consider a possible exception to this claim.

(D) *Concepts are Compositional.* One of the most incredible features of the human mind is its capacity to entertain endlessly many thoughts. There simply seems to be no upper bound to the number of beliefs we can entertain, desires we can form, and sentences we can understand. Consider the last of these. Despite the fact that almost any sentence we read or hear uttered is *entirely novel*, we understand its meaning almost instantaneously. Of course, we're finite beings with finite memories. So there are certainly grammatical sentences whose *mere length* precludes us from understanding them. But the counterfactuals are important here: we *would* understand such sentences if our memories and other cognitive capacities weren't limited in the way that they are.

The same goes for thought. We can think that the dog is on the mat; that the dog, which chased the cat, is on the mat; that the dog, which chased the cat, which chased the rat, is on the mat; and so on. Again, there are of course thoughts whose contents are so long and complicated that our finite cognitive capacities preclude us from entertaining them. But abstracting away from such 'performance limitations', it seems that a theory of our grammatical and conceptual *competence* must account for the so-called 'productivity' of language and thought.

A second, less striking but still important feature of thought is that it appears to be *systematic*, in the following sense: a mind that is capable of entertaining a certain thought, P , is also capable of entertaining logical permutations of P . For example, minds that can entertain the thought that the book is to the left of the cup can also entertain the thought that the cup is to the left of the book. Although it seems to be conceptually

possible that there could be minds that didn't exhibit such systematicity, as a matter of empirical fact it seems that all minds do.⁸

The productivity and systematicity of thought cry out for explanation, and it appears that the only available explanation is that thoughts and concepts are *compositional*. Roughly, this means that the content of complex concepts is determined by the content of their constituents, and how those constituents are put together. Given a finite base of primitive concepts, our capacity to entertain endlessly many thoughts can be explained by positing a finite number of rules for combining concepts, which can be applied endlessly many times in the course of constructing a complex thought (or concept).

Similarly, compositionality explains systematicity because the stock of primitive concepts and rules for combining them are such that if a mind can entertain a certain thought P , then it can entertain thoughts that are logical permutations of P . The reason that a mind that can entertain the thought that the book is to the left of the cup can also entertain the thought that the cup is to the left of the book, is that these thoughts are built up out of the same constituents, using the same rules of combination. Since compositionality apparently provides the only hope of explaining the productivity and systematicity of thought, any theory that is incompatible with it ought to be rejected.

⁸ In putting it this way, I side with Fodor (1987), Fodor and Lepore (1992), and Fodor and Pylyshyn (1988) in regarding the systematicity/productivity of thought as an empirical fact, and not an *a priori*, conceptual truth about thought itself, as in Peacocke (1992a), who reads Evan's (1982) 'Generality Constraint' in this fashion.

CHAPTER 2

THE META-LEVEL QUESTION: PSYCHOLOGICAL EXPLANATION, NORMATIVE EPISTEMOLOGY, AND CONCEPTS

Introduction

In Chapter 1 I laid out what I take to be constraints on any adequate theory of concepts.

In this chapter, I consider in more detail the meta-level question broached at the beginning of Chapter 1, namely, whether concepts are needed primarily for the purposes of psychological explanation or normative epistemology. My aim is to outline each meta-level position, articulate the background motivation for each, and discuss the issue of which has priority. Along the way we'll also consider the object-level theories of Fodor and Peacocke, and see that the meta-level and object-level questions about concepts need to be sharply distinguished.

Concepts and Psychological Explanation

Perhaps more than any other philosopher of mind, Fodor has vigorously defended intentional explanations of human behavior, and it's important to understand his theory of concepts in this context. Whereas behaviorists and eliminativists argue that the intentional states invoked by folk-psychological explanations of human behavior will *not* be part of a serious scientific psychology, Fodor defends the view that they'll in fact be an *indispensable* part of such a psychology. The kind of intentional psychology Fodor envisions and defends thus vindicates (at least to some extent) our pre-theoretic beliefs about how humans get around in and manipulate their environments. On this view, a

scientific psychology that aims to articulate generalizations concerning human behavior will postulate mental entities that common sense takes to exist.

What are the commitments of folk psychology, and how are they (or will they be) related to those of a scientific psychology? Both questions are controversial. At a minimum, folk psychology is committed to two kinds of mental state: belief-like states, which represent the environment as being a certain way and *guide* one's behavior, and desire-like states, which represent one's goals and *motivate* behavior. Moreover, I take it that common sense has it that such states are *causally efficacious*. The practical syllogism captures some of these relations: if an agent desires x, and believes that performing action y would achieve x, then (*ceteris paribus*) the agent will form an intention to y and engage in y-directed behavior. We appear to exploit our knowledge of (something like) the practical syllogism in explaining peoples' actions. For instance, we explain Mary's walking to the music store in terms of her *desire* to buy her favorite band's new CD and her *belief* that she can purchase the CD at the store. It's also part of our common-sense psychology that states of the environment causally interact with mental states: Mary's belief that the CD is on the store's shelf is explained by her *seeing* the full-page advertisement in today's paper. Such explanations presuppose that mental states causally produce behavior, as well as stand in causal relations both with each other and the environment.

Must a scientific psychology respect our folk-classifications of mental states, and our common-sense beliefs about how they're causally related to each other, in order to count as a *vindication* of folk psychology? Rather than taking a stand at the outset on exactly how this question will be answered, Fodor reasonably says:

[T]he generalizations that are recognized by the vindicating theory mustn't be crazy from the point of view of common sense ... After all, common-sense psychology won't be vindicated unless it turns out to be at least approximately true. (1987, p.15, original emphasis)

Of course this doesn't mean that common sense, belief-desire explanations are in any way sacrosanct. For surely some of our common-sense beliefs will have to be given up in light of the empirical findings of a scientific psychology. As Fodor says, "a lot of what common sense believes about the attitudes must surely be false (a lot of what common sense believes about *anything* must surely be false)" (1987, p. 15). Perhaps even the folk-notions of 'belief' and 'desire' won't survive.¹ In any case, psychology has arguably already ousted some common-sense beliefs, for example that much of what's in the mind is learned through experience, or that much of what's in the mind is accessible to introspection. Nevertheless, in order to count as a *vindication* of folk psychology, an intentional psychology must share many of the commitments of the folk. It mustn't be "crazy" from the folk-perspective.

Perhaps the most important of these commitments is that folk psychology postulates states that have the property of *being intentional*. In other words, whatever *other* properties the entities that a scientific psychology quantifies over have, in order to count as a vindication of folk psychology they must have intentional ones. Beliefs are

¹ With respect to the issue of whether the attitudes themselves will be vindicated by a mature psychological theory, Fodor makes clear that he is a 'conservative' realist about such folk-notions: "I ... want to distinguish between two versions of intentional realism, one of which is merely conservative, and the other of which is die-hard. The merely conservative view is that the best hope for psychology is the exploitation of intentional categories, just as Granny has always said. The die-hard line, by contrast, is that the intentional categories that we want for science ought to include belief, desire, and the other taxa of commonsense propositional attitudinizing. ... A conservative intentional realist who is not a diehard can contemplate with equanimity the abandonment of belief/desire psychology strictly socalled, so long as the apparatus of *intentional* explanation is left intact" (1990, p. 175, original emphasis).

true or false, and desires have satisfaction conditions, and such semantic evaliability is one of the essential components of successful folk-psychological explanations. Mary believes *that the CD is in the store*, and desires (*that she own*) *a copy*. These states have content (specified by the ‘that-clauses’), and it’s because they have the content that they do that they can be invoked in a successful causal explanation of Mary’s going to the music store. The kind of scientific psychology Fodor and many others envision is thus a thoroughly *intentional* one. Its generalizations will quantify over mental states and their contents.

It bears emphasis that a defense of the intentional idiom in psychology does not necessarily depend upon the success of the practical syllogism. It’s true that Fodor himself often relies on cases involving practical reason and rationality, and rightly emphasizes the remarkable predictive power of folk psychology. For instance, if someone utters ‘I’ll be at the airport at 3pm on Tuesday’, then we take it that he intends to be there at that time. If he doesn’t turn up, then, as Fodor says, “it’s less likely that the [folk-psychological] theory has failed than that something went wrong with the airline” (1987, p. 3). This sort of predictive success certainly needs to be accounted for, and I agree with Fodor that the best of way of doing so is to accept that there are states with intentional content. But I would like to call attention to the fact that there are *intentional* psychological generalizations that have nothing particularly to do with the practical syllogism.

In psychophysics, for instance, there are generalizations that relate the intensity of physical stimuli to sensory magnitudes, or “just-noticeable differences”. In a typical experiment, subjects are asked to estimate the perceived magnitude of a stimulus (e.g.,

the brightness of a light source, or the loudness of a beep) with respect to a reference stimulus, the latter of which is assigned an arbitrary number. For example, if the subject perceives a light to be twice as bright (or a beep twice as loud) as the reference stimulus, then the subject assigns it a number twice that of the reference stimulus. Stevens (1957) formulated the so-called ‘Psychophysical Law’ as an exponential relationship between the stimulus and the perceived magnitude:

$$\Psi = KS^n$$

where Ψ is the perceived magnitude, K is a constant, S is the physical magnitude, and n is a modality-dependent exponent. What’s important to note here is that such psychophysical generalizations apparently quantify over mental states with intentional content. For such laws claim that states that *represent* a stimulus as having a certain brightness, or a certain loudness, are nomologically related to stimuli with certain physical properties. The laws thus pick out the mental states by reference to their contents.

Standard cognitive illusions provide another nice example. Consider, for instance, the famous moon illusion (Ross and Plug 2002). It’s apparently a psychological law that when we see the moon near the horizon, it appears much larger than it does when we see it in the zenith. Of course the moon itself doesn’t get bigger, nor does it cover areas of different size on our retinas depending on its position in the sky. The apparent difference in size is an illusion, and like the Müller-Lyer and other illusions, the moon illusion is quite striking in that it is (to use Pylyshyn’s nice phrase) *cognitively impenetrable*: knowing that moon isn’t larger on the horizon doesn’t change the fact that our experience *represents* it as such, just as knowledge that the lines in the Müller-Lyer

illusion are the same length doesn't affect our *representing* one line as longer than the other. Again, the important point is that the generalizations here quantify over mental states and their intentional contents.

It is, of course, an empirical question as to *exactly which* generalizations are such that their explanatory power depends upon exploiting the intentional idiom. I cite the cases of psychophysics and cognitive illusions because they're particularly vivid and plausible examples. For it's not at all clear how one could state psychophysical generalizations, or laws about illusions, without mentioning mental states and their contents. But other examples could be given, for instance from theories of vision (Marr 1982, Biederman 1995) or language (Fodor 1975, Fodor, Bever, and Garrett 1974, Jackendoff 1992), which arguably quantify over states with content. There are of course lively debates about *whether* such theories in fact quantify over states with content, and if they do, what *kind* of content it is.² The important point here is that while intentionality may be necessary for the scientific vindication of the practical syllogism, it's arguably necessary for *other* psychological generalizations too.

Now, there are philosophers who deny that there *are* any psychological generalizations, and, *a fortiori*, that there are psychological generalizations that quantify over mental states and their contents. For example, eliminativists claim that folk psychology, with its ontology of propositional attitudes, is explanatorily bankrupt and

² In the case of vision, for instance, some theorists argue that Marr's (1982) theory requires *wide* content (Burge 1986), others argue that what's needed is narrow content (Segal 1989), while still others argue that content plays no individuative role in the theory at all (Egan 1995). In the case of language, the disputes are not so easily characterized. For instance, in motivating his 'internalist' approach to language, Chomsky (2000) sometimes seems to suggest that he takes theories in linguistics—e.g., phonology and semantics—to invoke some notion of 'narrow' content. But sometimes Chomsky appears to deny that linguistic theories invoke representations *at all*. Jackendoff's (1992) discussions have a similar flavor, in that he stresses the need for mental representations in linguistic theories, but at the same time denies that the representations have intentional content (see especially chapters 2 and 8). See Rey (2003a, 2003b) and Chomsky (2003) for an interesting exchange on this issue.

false, and will eventually be replaced with a more scientifically respectable and *non-intentional* neuroscience (Churchland 1981). Presumably they would say the same for intentional, *non-folk-psychological* generalizations. Near the beginning of *Concepts: Where Cognitive Science Went Wrong*, Fodor says that “there isn’t any reason in the world to take [eliminativism] seriously and, in what follows, I don’t” (1998a, p.7).³ Although, *pace* Fodor, I think there *are* reasons to take the eliminativist challenge seriously, in what follows I follow his lead and ignore it.⁴

There are also *non*-eliminativists who deny the existence of psychological laws. For instance, in his monumental essay “Mental Events,” Donald Davidson claims that there can be no psychological or psychophysical laws “because of the disparate commitments of the mental and physical schemes” (1970/2002, p. 123). Roughly, the idea is that psychology (or our “mental scheme”) is inherently normative, since principles of charity and norms of rationality are constitutive of intentional ascription, whereas physical states and events—i.e., events picked out with the predicates of physical science—do not have this normative character. These “disparate commitments” of the mental and physical schemes, Davidson argues, rule out the existence of strict psychological laws.

Davidson’s argument has generated an enormous literature, and a full-fledged discussion of it would take us too far afield. I merely want to draw attention to a key premise of his overall argument for Anomalous Monism, namely, his “Principle of the

³ Elsewhere, Fodor says: “That people (and, surely, other higher organisms) act out of their beliefs and desires, and that, in the course of deciding how to act, they often do a lot of thinking and planning, strikes me as maybe empirical in principle but surely not negotiable in practice” (1994, pp.3-4). This seems right. But it’s easy to imagine sensible eliminativists—i.e., eliminativists who *don’t* think we’ll eventually be communicating and explaining each others’ actions in neurospeak—who wouldn’t deny the *practical* non-negotiability of folk-psychology. They would simply deny that it’s being practical is a matter of its being *true*. Theories can be practically useful, even though they’re strictly speaking false.

⁴ For detailed rebuttals of eliminativists’ arguments, see Horgan and Woodward (1985) and Rey (1997).

Nomological Character of Causality,” according to which “events related as cause and effect must fall under strict deterministic laws” (1970/2002, p.116). Not only is this an unargued premise, but it’s far from plausible. Perhaps the causal relations between the fundamental entities of physics are subsumed under strict laws (but see Cartwright 1983), but there is no reason to take this as a model for causal relations generally. For it’s plausible that there are genuine causal relations between entities and properties that occur at higher levels of organization in nature—i.e., those studied by the so-called “special sciences” such as geology, meteorology, biology, and psychology. And Davidson’s argument, if correct, would establish in a purely *a priori* fashion that any *non-microphysical* events that stand in causal relations must be *identical* to microphysical events. Perhaps such a strong form of physicalism is ultimately correct. But most philosophers who are physicalists would deny it. And in any case, it’s surely dubious that it can be established by Davidson’s Principle of the Nomological Character of Causality *alone*.

I mention this because, as a number of philosophers have argued, causal explanation in science is plausibly a matter of subsuming events under laws, *even if* the relevant laws are *not* strict. This is particularly plausible in the case of causal relations between events that occur at higher levels of organization, which one would expect to be subsumed under *ceteris paribus* laws. For the special sciences are concerned with incredibly complex systems to which exceptionless generalizations are unlikely to apply. For instance, the biological laws governing gene frequencies in populations are subject to exceptions given migration, recombination, genetic drift, and mutation (Sober 2000a). One would expect generalizations in psychology to have exceptions as well.

Generalizations in linguistics, for instance, are subject to apparent exceptions given speakers' finite capacities. This is why linguists (in the Chomskyan tradition, anyway) regard their theories as claims about speaker's underlying grammatical competence, which abstract away from the relevant performance limitations, such as memory, saturation effects, mortality, etc. (Larson and Segal 1995, Pietroski and Rey 1995).

Or, consider again the generalization captured by the practical syllogism: "If an agent desires x, and believes that performing action y will achieve x, then the agent will form an intention to y and engage in y-directed behavior". Clearly such a generalization needs a *ceteris paribus* clause, since there are circumstances that could interfere with the agent's forming the intention; he might get distracted, hit over the head with a bat, or struck by lightning. Of course, spelling out the nature of such *ceteris paribus* laws in any detail would require much more discussion (see, e.g., Fodor 1991a, and Pietroski and Rey 1995). In what follows I simply assume that genuinely causal relations between events can be subsumed under the *ceteris paribus* laws of the special sciences.

To sum up the discussion so far: On the sort of view under consideration, and which I endorse, folk psychology will (more or less) be vindicated by a mature, scientific psychology, which aims to articulate generalizations concerning human behavior that will quantify over belief (-like) states and desire (-like) states. Psychological laws are thoroughly intentional, in that they describe causal relations among mental states that are specified using intentional vocabulary (i.e., the laws quantify over mental states *and their contents*). Moreover, psychological generalizations are *ceteris paribus*, and this needn't threaten the status of intentional states as causally efficacious.

According to Fodor, some or other version of the Representational Theory of Mind (RTM) offers the best hope of carrying out this project. The key claim of RTM is that mental states have both *representational* and *causal* properties. The former are sensitive to, or carry information about, states of the environment, while the latter are supposed to be the properties in virtue of which mental states enter into mental processes, and thus eventuate in behavior. Both properties are essential. For instance, my belief that Washington D.C. is the capital of the U.S. is really *about* Washington D.C., and some state of my visual system is really *about* an edge. Moreover, such mental states clearly causally interact with each other, and at least sometimes eventuate in behavior. Mental states with both kinds of properties are typically called ‘mental representations’, and the key claim of RTM is that it’s *only* by postulating mental representations that a scientific psychology will be able to articulate generalizations concerning human behavior (Fodor 1975, 1987, Jackendoff 1992).

There are, to be sure, different versions of RTM, and the theory, as Fodor says, is “really a loose confederation of theses” (1998a, p. 6). The following five theses are central to Fodor’s version of RTM:

- (1) Psychological explanation is typically nomic and is intentional through and through.
 - (2) Mental representations are the primitive bearers of intentional content.
 - (3) Thinking is computation.
 - (4) Meaning is information (more or less).
 - (5) Whatever distinguishes coextensive concepts is ‘in the head’.
- (1998a, pp. 7-15)

For Fodor, interest in a theory of concepts arises in the context of RTM, for the latter provides the best hope for providing a theory of mental states, and “the natural home of a theory of concepts is a part of a theory of mental states” (1998a, p.6). I endorse this view, and in what follows I will not question the truth of (1), (2), (3), or (5).⁵ However, one my central claims, which will emerge in Chapters 5, 6, and 7, is that taking concepts to be needed primarily for the purposes of psychological explanation provides a powerful reason for rejecting thesis (4).

At this point, though, we need to consider the following question: Why think that concepts are an indispensable piece of RTM? To begin, consider the following remarkable fact about the human mind: it’s sensitive to, and capable of having thoughts about, *indefinitely* many properties: being a tiny folded piece of paper, an object that weighs more than one pound, an oval-shaped canteen, a beautiful artwork, an odd number, a muon, a supernova, and so on, indefinitely. Although this might seem rather mundane, it it’s in fact astonishing. For in virtue of what fact about us, or the organization of our nervous system, do we exhibit this remarkable kind of sensitivity? Certainly nothing else in the known universe is so organized.

One suggestion is that, to a first approximation anyway, it’s *because we have concepts*. It’s because we have the concepts TINY FOLDED PIECE OF PAPER, OBJECT THAT WEIGHS MORE THAN ONE POUND, ODDLY-SHAPED CANTEEN, BEAUTIFUL ARTWORK, QUARK, and SUPERNOVA, that we’re able to *think about* tiny folded pieces of paper, objects that weigh more than one pound, oddly-shaped canteens, beautiful artworks, quarks, and supernovas. Fodor (1986) argues in particular that what’s characteristic about organisms with minds like ours is not *just* that they’re sensitive to

⁵ In Chapter 6, though, we’ll consider a reason why some theorists are skeptical of (2).

indefinitely many properties, but that they’re sensitive to indefinitely many *non-nomic* properties, i.e. properties that *don’t* figure in any laws of nature. To use Fodor’s example, consider the property of *being a crumpled shirt*. There isn’t a law of nature that relates *being a crumpled shirt* to any other property, including any of the properties instantiated by our minds or our nervous systems. Now, it’s true that since laws govern all objects, laws govern crumpled shirts. But crumpled shirts aren’t subsumed under any laws *in virtue of being crumpled shirts*. They’re subsumed under laws in virtue of instantiating nomic properties, e.g. being a particular mass measure. This is important because despite the fact that *being a crumpled shirt* does not figure in any laws of nature, we’re nevertheless capable of responding selectively to it. Indeed, as we noted above, we respond selectively to all manner of such properties—*being an oddly-shaped canteen*, *being a beautiful painting*, etc. And Fodor (1986) suggests that this fact is, in part, what motivates RTM’s need for concepts.

Consider the following everyday sort of scenario: I turn my head in the direction of a crumpled shirt, notice it, and say ‘there’s the crumpled shirt’. What explains my behavior, i.e. my uttering the words ‘there’s a crumpled shirt’? Well, one explanation is that I noticed that the object is a crumpled shirt and (for whatever reason) wanted to draw attention to it. This would be a case, then, in which my behavior is *explained* by my being sensitive to a non-nomic property. But since *being a crumpled shirt* is non-nomic, there’s no law that relates it to the property of my saying ‘that’s a crumpled shirt’, or to my *noticing* that it’s a crumpled shirt. How, then, are we to make sense of explanations of human behavior that invoke such non-nomic properties? For isn’t it true that good

explanations involve subsuming events under *law-like* generalizations, i.e. generalizations that describe relations among *nomic* properties?

The key, here, is that such non-nomic properties of objects are nevertheless properties that we can *represent* objects as having. Although the property of *being a crumpled shirt* does not itself enter into any law-like relations, it's nonetheless a property of the object that we can represent it as having, and such mental representations *are* (or have) nomic properties. There is, for example, presumably a nomic relation between my representing something as a crumpled shirt and my coming to say ‘that’s a crumpled shirt’. (The generalization here will clearly be complex. For such a nomic relation will only hold *via* some other mental states, e.g., noticing that it’s a crumpled shirt, wanting to remark on it, and so forth.)

Of course, our being sensitive to a non-nomic property such as *being a crumpled shirt* is clearly dependent on the shirt’s instantiating some properties that *are* nomic. For we couldn’t represent objects as having non-nomic properties unless they also instantiated properties that are detectable by transducers, the systems at the periphery of the nervous system that have evolved to respond selectively to certain nomic properties in the world, e.g., light waves, sounds waves, chemicals, motion, pressure, electrical fields, magnetic fields, and temperature (Churchland and Sejnowski 1992, p. 142). Examples of such transducer systems include the rods and cones in our retinas, the hair cells in our cochlea, the taste buds on our tongues, and the stretch receptors in our muscles. The point is that when I turn toward the shirt, certain of its nomic properties enter into law-like relations with certain of my retinal states, which is necessary for my eventually coming to *represent* the object as a crumpled shirt.

Now, the precise way in which such processes work—i.e., how we (or our nervous systems) make so-called ‘perceptual inferences’—is no doubt incredibly complicated.⁶ The important point, as far as Fodor is concerned, is my *representing* something as a crumpled shirt *is* a nomic property, even though *being a crumpled shirt* is not. This sequence of events is represented in Figure 1.

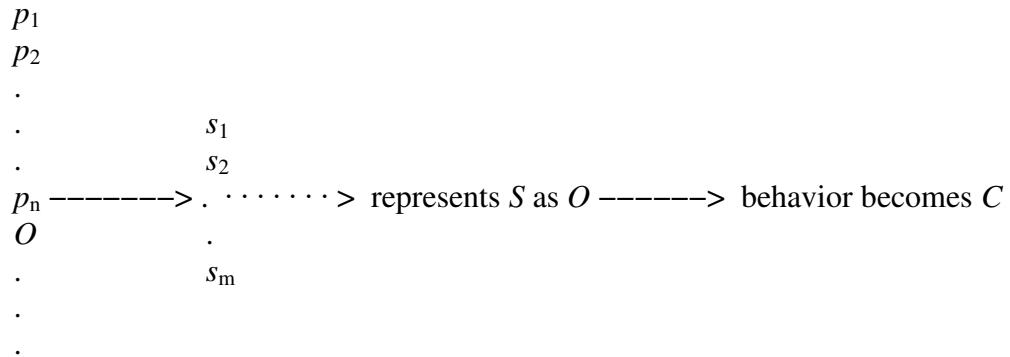


Figure 1

Figure 1. RTM explains how *nomic properties of the object of perception* can figure in determining the behavioral consequences of perceptual encounters: they occur as properties that the distal object both has and is represented as having. Thus *S* has psychophysical (hence nomic) properties $p_1 \dots p_n$ as well as (nonnomic) property *O*. In virtue of psychophysical law, causal interaction between *S* and organism *A* eventuates in (nonbehavioral) psychological states $s_1 \dots s_m$. In effect, states $s_1 \dots s_m$ carry the information that $p_1 \dots p_n$, and this information serves as the ‘premise’ of a perceptual inference of which the ‘conclusion’ is an attribution of *O* to *S*. Drawing this inference leads to behavioral consequences as illustrated. Roughly, horizontal connections indicated by dashed lines are nomologically necessary; those indicated by dotted lines are inferential (Fodor 1986, p. 15, original emphasis).

⁶ Speaking of such inferences, Fodor says: “That, I suppose, is the problem of perception insofar as the problem of perception is a problem in psychology. For though the information provided by causal interaction between the environment and the organism is information about physical properties in the *first* instance, in the *last* instance it may (of course) be information about any property the organism can perceive the environment to have. To a first approximation, the outputs of sensory mechanisms are appropriately viewed as physical descriptions, but perceptual judgments need not be articulated in the vocabulary of such descriptions. Typically they *are* not: A paradigm perceptual judgment is, ‘There’s a robin on the lawn’ or ‘I see by the clock that it’s time for tea’” (1975, p. 47, original emphasis).

Now, I don't want to place too much weight on the suggestion that it's our sensitivity to *non-nomic* properties that demands the postulation of concepts. For it's not entirely clear that the nomic/non-nomic distinction is what matters here. After all, couldn't one construct a machine that responded selectively to patches of green, and that had a timer on it so that it registered when the date reached January 1, 2006?⁷ If so, then the machine would be responding selectively to a grue-like property, the property of *being green and detected before January 1, 2006 or otherwise blue*, which is famously non-nomic (Goodman 1956). The machine's capacity to respond selectively to such a non-nomic property would plausibly not justify our attributing the concept GRUE to it.

But if it's not our sensitivity to non-nomic properties, then what *does* justify the need for concepts? Suppose there's a range of properties that are transducer-detectable, and the outputs of transducer systems provide the 'data' upon which perceptual inferences are made. Perhaps the need for concepts can thus be put by saying that certain organisms respond selectively to properties that are *non-transducible*, properties like *being a crumpled shirt* or *being a beautiful artwork*. Fodor doesn't like this proposal:

[T]he only difference between a transducer and anything else that responds selectively to proximal stimulation is that transducers are devices whose outputs, taken under their computationally relevant descriptions, are lawfully related to corresponding properties of their inputs; in short, the point about transducers is that they respond selectively only to nomic properties. If there were a way of defining "transducer" independently of notions like nomicity, I would jump at it. But I don't know of any, and there's independent reason to suppose that

⁷ Georges Rey raised this possibility in conversation.

nomicness is the more fundamental notion: unlike transduction, nomicness is a concept that we need outside the information sciences. (1986, pp. 15-16)

That the distinction between law-like and accidental properties is needed throughout the sciences is, all else being equal, a reason for preferring it to the distinction between transducer-detectable and non-transducer detectable properties. But it doesn't seem that all else is equal. For as the possibility of a grue-detector machine illustrates, it's not clear that transducers do respond only to nomic properties. This suggests that our capacity for responding to properties that are non-transducer detectable may be what matters after all.

This becomes particularly plausible when one considers the fact that we're sensitive to indefinitely many non-local, non-physical properties (Rey 1997). For instance, we can detect such non-local properties as *being the footprint of a triceratops* and *being a consequence of the big bang*, and such non-physical properties as *being a sentence* and *being a noun phrase*. Why think that this capacity is underwritten by our having the *concepts* TRICERATOPS FOOTPRINT, CONSEQUENCE OF THE BIG BANG, SENTENCE, and NOUN PHRASE? Consider the non-local property of *being a triceratops footprint*. When a paleontologist looks at a footprint, his transducer systems are sensitive only to the *local* properties of the footprint, e.g. the properties that determine its size and shape. Nevertheless, the paleontologist can come to represent it as a *triceratops footprint*. How? Well, presumably the paleontologist is *smart*: he knows a range of facts about the distinguishing characteristics of triceratops footprints, fossilization, etc., and is able to exploit this information in making a complicated inference that begins with the output of his transducer systems and ends with his *thinking* ‘there’s a triceratops footprint’.

Indeed, such non-demonstrative inferences provide the basis for a powerful argument for the existence of an internal representational system in which such inferences are carried out:

Perception must involve hypothesis formation and confirmation because the organism must somehow manage to infer the appropriate task-relevant description of the environment *from* its physical description together with whatever background information about the structure of the environment it has available. ... If one accepts ... [this] kind of approach to perception ... then one is committed to the view that perceptual processes involve computing a series of redescriptions of impinging environmental stimuli. But this is to acknowledge that perception presupposes a representational system; indeed, a representational system rich enough to distinguish between the members of sets of properties all of which are exhibited by the same event. (Fodor, 1975, pp. 50-51, original emphasis)

There's thus a real sense in which the paleontologist goes 'beyond the data' in representing something as a triceratops footprint. For it's clear that TRICERATOPS FOOTPRINT is not *merely* the concept of something that has certain transducer-detectable properties. That is, for something to fall under TRICERATOPS FOOTPRINT it's not enough for it to have, as it were, all of the appearance properties of a triceratops footprint.⁸ Rather, for something to be a triceratops footprint it must be fossilized in a particular geological layer, bear a causal relationship to the foot of an organism with a very particular evolutionary history, and so forth. This is why determining whether something

⁸ In a way, this illustrates what is perhaps the main problem with the logical empiricists' attempt to "reduce" the content of all non-sensory concepts to that of sensory ones (i.e. concepts whose content is fixed by transducer systems). For if our concepts go beyond the evidential data delivered by our sensory systems, it's hard to see how their content could be reduced to sensory content. We touch on some other problems with verificationism below.

is indeed a triceratops footprint involves making complicated inferences that only experts are in a position to make—they’re the only people with the appropriate background knowledge.

The present point is simply that paleontologists couldn’t make such inferences unless they were manipulating *representations* involving, e.g. the physics of dating techniques, the geology of certain layers of rock, the evolutionary history of triceratops, etc., in quite complex and subtle ways. In general, the “smarter” an organism is, the more complicated the mechanisms that mediate the input/output relations will be, which in turn increases the need for positing concepts.⁹ Thus, from both Fodor’s perspective and mine, what underlies our capacity to think thoughts about indefinitely many properties is that we have the corresponding concepts. Moreover, if our minds realize a set of principles for combining mental states and their constituent concepts, then we can explain why thought is both productive and systematic. In short, since concepts are the constituents of mental states, providing a theory of concepts is of a piece with providing a theory of such states and the psychological explanations that invoke them. For concepts are the constituents of the very things—mental states—that intentional psychology quantifies over when it provides generalizations concerning human thought and behavior.

⁹ The available evidence suggests that what we normally think of as quite simple (or “not so smart”) organisms in fact have quite complicated systems of internal representations (Gallistel 1990, Carruthers 2004). It’s worth noting, moreover, that while the need for concepts rests in part on the need for understanding the possibility of non-demonstrative inference, this is not to claim that there’s an adequate theory of the mechanisms that *mediate* such inferences. See Fodor (1983, 2000a) for pessimistic arguments about the prospect of our ever understanding such processes, and Carruthers (2003) for an optimistic rejoinder.

The Personal/Subpersonal Distinction

At this point I'd like to briefly discuss a very sticky issue, namely the distinction between personal and subpersonal explanations. My own suspicion is that quite a few of the ‘Big Picture’ disagreements among philosophers of mind are ultimately due to their having different attitudes about the importance of this distinction. But let me simply say a few words about the distinction, why it might seem to matter for our purposes, and how I plan to essentially bypass it in what follows.

The issue arises for us because each of the two meta-level perspectives we're considering—concepts as needed for psychological explanation, and concepts as needed for normative epistemology—is often accompanied by a corresponding stance on the personal/subpersonal distinction. Those who view the task of providing a theory of concepts from the perspective of psychological explanation will most likely *deny* that the personal/subpersonal distinction matters very much, whereas this is not so for those who view the task from the perspective of normative epistemology. The latter approach is part of a tradition in the philosophy of mind that takes intentional notions in general to be inextricably tied up with normative aspects of reason and rationality, and thus, as we'll see, places heavy emphasis on the role of concepts in reason and rationality. This is where the personal/subpersonal distinction comes in. For it's often stressed that reasons and rationality are “personal-level” notions: reasons are had by *people*, and it's *people* who *act* for reasons. The rationalizing explanations of folk psychology are thus personal-level explanations, since they render people's actions intelligible by citing the their beliefs and desires.

It's worth noting that a theorist who takes this view is not thereby committed to any particular claims about the *metaphysical status* of persons. One might take the “personal-level” to be like other “macro-levels” in the sciences, to be understood on a par with the chemical, geological, and biological levels. On such a view, persons and their properties reside at higher-levels of organization in nature, in the same way that hearts and their properties do, as entities that are fully “composed” by microphysical entities and properties that are fully “realized” by microphysical properties.¹⁰ Or one might take the view that persons are metaphysically fundamental, primitive entities of some kind. On this sort of view, persons and their properties are not understood as macro-objects and macro-properties, and intentional psychology is not viewed as just one among many of the special sciences.¹¹ Either of these views is open to those who take intentional properties to be inextricably tied up with reasons and rationality, and the latter to be notions understood at the personal-level.

The important point here is that if one combines this sort of view with the view that concepts are constructs needed for rationalizing explanations, then it's a short step to the claim that concepts have their home in person-level explanations. In *A Study of Concepts* we thus find Peacocke making the following claim:

All the specific possession conditions [for concepts] I gave ... were in fact at the personal level, and so are all those to follow. ... Indeed, this *must* be so, because all possession conditions speak in one way or another of a speaker's reasons for forming beliefs (positive or negative). The statement that someone forms a belief

¹⁰ This is a physicalist version of the view, anyway. Jaegwon Kim (1993) appears to hold a view of this kind, as he stresses *both* the importance of reason and rationality in understanding the propositional attitudes, *and* the fact that mental states and properties (and presumably the particulars that have them, namely people) are fully physically composed.

¹¹ Paul Pietroski (2000b) defends this view.

for a particular reason is at the personal level, not the subpersonal. (1992a, p.55,
my emphasis)

Why restrict an account of what it is to possess a certain concept to the goings-on at the personal-level? It's true that people have beliefs and desires, and that people act for reasons, but couldn't it be that an account of what it is to possess certain concepts will nevertheless appeal to goings-on at the *subpersonal* level? Peacocke rejects this, apparently because he takes the *very concept* CONCEPT to be constitutively tied to what *people* do:

[W]hatever is involved in the individuation of a concept needs to be at the surface of our psychology, on pain of otherwise *leaving the subject-matter of concepts behind*. The notion of a concept has its home within the domain of reason-based explanation of thought and action. Nothing not having to do with reason-based explanation should be included in an account of what individuates a particular concept. (1996/1999, p.348, my emphasis)¹²

Now, I don't know whether Peacocke takes it to be *analytic* that CONCEPT is tied to the personal-level, or whether he takes it to be a theoretical, constitutive claim about concepts themselves.

Either way, it's clear that Peacocke views a theory of concepts as in some sense *insulated* from the empirical findings of psychology, which will presumably appeal to events at the subpersonal level in its explanations. Indeed, Peacocke even goes so far as to say that "for any particular concept, the task for the psychologist is not fully formulated until the philosopher has supplied an adequate possession condition for it"

¹² In a recent article on the topic, Peacocke says: "The notion of a concept that is individuated by its possession condition at the level of constitutive role in rational explanation is at a level of generality that is above that of subpersonal explanation" (2005, p. 168).

(1992a, p. 190). For a theorist who views the task of providing a theory of concepts from the perspective of psychological explanation, such claims will appear *grossly* misguided. It's not that such a theorist must deny that there's a personal/subpersonal distinction. For there surely is such a distinction to be drawn: roughly, it's the distinction between a person and person's parts, e.g., a person's nervous system (Dennett 1969). What such a theorist will claim, rather, is that while the distinction may be real and important, it does not pose any substantive constraints on a theory of concepts.

For instance, the distinction is clearly very important when it comes to assessing legal and moral responsibility. For people are held responsible their actions, *not* their nervous systems, and this is true even though certain nervous disorders might exculpate the accused. The issues here—concerning freedom, autonomy, people's self-conceptions, etc.—are important, subtle, and complex.¹³ But that the personal/subpersonal distinction is relevant for *these* purposes does not imply that it's relevant for *all* purposes. In particular, it doesn't imply that the distinction is relevant for the purposes of psychological explanation. And if concepts are viewed as constructs needed primarily for the purposes of psychological explanation, then the personal/subpersonal distinction will not be seen as relevant for the purposes of a providing a theory of concepts either.

Why isn't the personal/subpersonal distinction relevant for the purposes of psychological explanation? As we've seen, the primary aim of an explanatory psychology is to articulate generalizations concerning human behavior, where the latter is construed broadly to include reasoning, remembering, seeing, attending, acting, comprehending a sentence, and so on. If one endorses RTM, then these generalizations

¹³ See, e.g., Dennett (1984).

will be spelled out in terms of various computational relations that hold among the representations involved in such mental processes. What's important to notice here is that while some of these processes will indeed be happily characterized at the personal-level, others will not.

Consider vision, for instance. It's true that *people* see objects. But it's also true that the early visual system performs a whole range of computations that underwrite this personal-level capacity. On Marr's (1982) theory, the visual system constructs 'raw primal sketches' (2-D representations) from retinal images, 'primal sketches' (richer 2-D representations) from 'raw primal sketches', '2 ½-D sketches' from 'raw primal sketches', and so on and so forth. These computations are clearly carried out *in* people. But are they also carried out *by* people? Most people of course don't know anything about Laplacian operators or zero-crossings in second derivatives, which (according to Marr) are used in carrying out the computations that allow for, say, the detection of light intensity changes across time. It would thus at least seem odd to say that people are carrying out these computations. But of course it's people who see objects, and it's clear that they couldn't do so unless their visual systems were carrying out some such computations.¹⁴ Similar points could be made about speech perception (e.g., representations of speech signals specify formant relations that are clearly at the subpersonal level), language comprehension, and memory.

¹⁴ Similarly, if Milner and Goodales's (1995) "two systems" hypothesis is correct, then the perceptual states that guide my actions when I simply grasp a cup are non-conscious and unavailable for personal-level explanations. Nevertheless grasping cups is surely something that *people* do. Thanks to Peter Carruthers for the example.

What this suggests is that the personal/subpersonal distinction is not particularly relevant for the purposes of psychological explanation. Fodor makes this point very clearly and vividly:

[T]he ordinary distinction between what the organism does, knows, thinks, and dreams, and what happens to and in its nervous system, does not seem to be frightfully important. The natural kinds, for purposes of theory construction, appear to include some things that the organism does, some things that happen in the nervous system of the organism, and some things that happen in its environment. ... [T]he states of the organism postulated in theories of cognition would not count as states of the organism for purposes of, say, a theory of legal or moral responsibility. But so what? What matters is that they should count as states of the organism for *some* useful purpose. In particular, what matters is that they should count as states of the organism for purposes of constructing psychological theories that are true. (1975, p. 52, original emphasis)

From this perspective, there is thus no reason for a theory of concepts to be *confined* to the personal-level. Of course some concepts do indeed have possession conditions that are formulated in personal terms. Recognitional concepts, which are individuated in terms of recognitional judgments, are presumably good candidates. For *people* make judgments. But the possession conditions for other concepts—e.g. those invoked in theories of vision, speech perception, and language comprehension—will presumably involve subpersonal events and states. Thus, while one may need to posit such concepts in order to characterize certain psychological generalizations, their possession conditions will generally not be spelled out (at least entirely) at the personal-level. Therefore, until

Peacocke provides a good reason for thinking that in order for something to *be* a concept it *must* be individuated at the personal-level, it seems reasonable on explanatory grounds to allow for the possibility of concepts with subpersonal possession conditions. In my view, the personal/subpersonal distinction is thus irrelevant for the purposes of providing a theory of concepts.

Now, I take it that it's an empirical issue whether or not the concepts that figure in personal-level psychological explanations belong to the same natural kind as concepts that figure in subpersonal psychological explanations. If it turns out that they play similar roles in psychological explanations, and figure in similar kinds of psychological laws, then that would provide the basis for an argument that they're of the same natural kind. If not, then there's an argument to the contrary. Either way, I think it's a mistake to place the *a priori* constraint on a theory of concepts that it be articulated at the personal-level.

Although I agree with Fodor that we ought to downplay the importance of the personal/subpersonal distinction for a theory of concepts, I don't think I'm begging any questions against Peacocke in doing so. For the conflict between their respective meta-level approaches can be adequately characterized *without* presupposing that some concepts do *in fact* have subpersonal possession conditions. Indeed, the discussions that follow will focus on concepts that are putatively individuated in terms of beliefs and judgments that are happily characterized at the personal-level. My arguments will thus not presuppose anything with respect to the personal/subpersonal distinction that Peacocke would find objectionable.

Concepts, Normativity, and Normative Epistemology

In my view, there's nothing *essentially* normative about concepts and concept possession. I take it as given that there's a range of descriptive facts about the human mind, and that it's the job of an intentional psychology to articulate generalizations that concern them. Since concepts are the constituents of these mental states, a theory of concepts simply falls into place as one (particularly crucial) component of a descriptive intentional psychology.

Now, one might object to this on the grounds that *any* theory of concepts and content must account for certain normative facts about the mind. After all, humans are fallible and sometimes make mistakes, and to make a mistake it to make an error, and making errors is something we *shouldn't* do. Suppose I apply SQUIRREL when I hear the sound of rustling leaves under the park bench. I know there are lots of squirrels in the park, and on numerous occasions I've heard the same sound from under the bench and later seen that it was a squirrel. So I infer that it's a squirrel that's making the sound. But I'm wrong. It turns out to be a chipmunk. This is a case of representational error.

Note that my erroneous judgment here isn't necessarily irrational, for I had (what I took to be a) good reason to apply SQUIRREL. Compare this with a case in which I'm face-to-face with a chipmunk in broad daylight, and have no reason to think that there are any squirrels-made-to-look-like-chipmunks in the area, or that I've been given a drug that makes squirrels look like chipmunks, etc. In this case, it would presumably be irrational for me to apply SQUIRREL. But it's plausible that I didn't violate any norms of rationality by applying SQUIRREL when I heard the rustling under the bench. Thus, *if* the possibility of representational error shows that concept-application has an essentially normative

component, then it's a weaker sort of normativity than that which is tied up with rationality.

But in fact, there's good reason to deny the antecedent of that conditional. For misrepresentation can plausibly be understood in the same way that we understand deviations from *ceteris paribus* laws in science generally (Pietroski and Rey 1995). As we saw earlier, in order to hit upon explanatorily interesting generalizations governing a given system, one must *abstract away* from certain of its features. For instance, although some samples gas will not obey Boyle's Ideal Gas Law ($PV=nRT$), it's understood that there are other factors responsible for this, factors that are *ignored* for the purposes of stating the generalization (e.g., electrical attraction between the gas molecules). The suggestion would be that the same goes for the factors responsible for representational error. They're simply factors that one abstracts away from for the purposes of stating certain explanatory psychological generalizations. On this model, my possession of the concept SQUIRREL doesn't imply that I *ought* to apply it to squirrels in any interestingly normative sense, any more than it's true that the temperature of a certain sample of gas *ought* to obey the Ideal Gas Law in any interestingly normative sense. The mere possibility of error thus need not imply that concept possession is essentially normative.

I admit that there's room to quibble here. One might claim that there's still a sense in which a theory of concepts and content is a normative enterprise, even if representational error can be understood in the way we understand deviations from *ceteris paribus* laws. For the possibility of error alone, one might claim, shows that concepts and content are normative notions. For naturalists, the hope is that even if this is the case, we will ultimately be able to provide a purely descriptive (i.e. reductive)

account of both representation and misrepresentation, and thus account for any normativity they may involve.¹⁵

For some theorists, however, the normative component to concepts goes much deeper than this. John McDowell is one such theorist, who claims that in order to “make sense of the idea of a mental state’s or episode’s being directed towards the world … we need to put the state or episode in a normative context” (1994, xi). What it is to ‘put a state in a normative context’? To use McDowell’s phrase, it’s to place the state in the “space of reasons,” which is a personal-level domain that (on his view) includes the properties of minds, meaning, rationality, and justification. According to McDowell, all of these have essential normative properties, which are “different in kind” (1994, xv) from the properties that characterize what McDowell calls the “realm of law,” the domain that’s happily described in impersonal, causal-nomological terms. For McDowell, this means that the normativity involved in concept possession is *sui generis*: “we must sharply distinguish natural-scientific intelligibility from the kind of intelligibility something acquires when we situate it in the logical space of reasons” (1994, xix).

Again, the hope of many contemporary philosophers is that we will eventually understand, or make intelligible, how the goings-on in the space of reasons can be (re-) described as (or explained in terms of) goings-on in the realm of law. Hence the so-called “naturalization” projects in philosophy of mind and epistemology (and ethics),

¹⁵ Fodor’s (1987, 1990) ‘asymmetric dependency’ account is an attempt to do just this. It’s constructed so as to guarantee that in cases of misrepresentation concepts don’t end up with disjunctive contents, e.g., that my concept SQUIRREL means *squirrel*, and not *squirrel or chipmunk or* Roughly, Fodor’s idea is that my *misapplication* of SQUIRREL to chipmunks (or any other non-squirrel) depends upon my *correct* application of SQUIRREL to squirrels, *but not vice versa*. The claim, in short, is that the mechanisms in virtue of which *non-squirrels* cause tokenings of SQUIRREL are asymmetrically dependent on the mechanisms in virtue of which squirrels cause tokenings of SQUIRREL. There’s been plenty of discussion in the literature of the so-called ‘disjunction problem’, as well as Fodor’s solution to it. See, for example, the papers collected in Loewer and Rey (1991).

projects that McDowell collectively calls “bald naturalism”. For various reasons McDowell rejects such bald naturalism. On his view, a purely descriptive psychology is thus not even in the offing.

Now, one might think that a rejection of bald naturalism would bring along with it a kind of Platonism about the space of reasons. As McDowell says:

Setting our faces against bald naturalism ... [i]t can seem that we must be picturing the space of reasons as an autonomous structure—autonomous in that it is constituted independently of anything specifically human, since what is specifically human is surely natural, and we are refusing to naturalize the requirements of reason. (1994, p. 78)

It is indeed very difficult to see how such a “rampant platonism” (1994, p. 78) about the space of reasons could be avoided once bald naturalism is rejected. For surely a *minimum* commitment of a bald naturalist¹⁶ about some domain in the space of reasons is that the entities and properties of that domain are *supervenient on* the entities and properties of a domain that’s in the realm of law, and are thus ultimately subject to (re-)description in causal-nomological terms.¹⁷ To deny that the goings-on in the space of reasons supervene on the goings-on in the realm of law would thus seem to be

¹⁶ Or, a “hairy realist”, as Fodor prefers to think of himself (1998e, p. 5).

¹⁷ For physicalists, the supervenience base will be constructed from the entities and properties of fundamental microphysics. I should note that there is a growing trend away from the thought that supervenience is the key to understanding how higher-level domains are related to microphysics. A supervenience thesis, it is suggested, is best seen as a statement of the *problem* that naturalists face, not a *solution* to it; see, e.g., Horgan (1993) and Kim (1998).

Speaking of McDowell’s proposed division between the “space of reasons” and the “realm of law”, Fodor rightly remarks: “Though I do agree that the problems about the mind and world are a lot harder than reductionists have sometimes supposed, I also think that an adequate and complete empirical psychology would, ipso facto, tell the whole, literal truth about the essence of the mental. Science discovers essences, as Kripke once remarked. So, if it’s literally true that rationality, intentionality, normativity, and the like belong to the mind essentially, then they must all be phenomena within the natural realm that scientists explore” (1998e, p. 5).

tantamount to claiming that the space of reasons is an “autonomous structure,” floating free from the rest of the natural world.

Nevertheless, one of McDowell’s (1994) goals is to show that rejecting bald naturalism need not lead to rampant Platonism. I agree with Fodor (1998e), Pietroski (1996), and Wright (2002) that McDowell’s defense of a proposed middle ground between bald naturalism and rampant Platonism is deeply unsatisfying. But there’s no need to consider his proposal here. For our purposes, the important point to note is that McDowell sees a theory of concepts as an essentially normative enterprise that has its home in the space of reasons, where this is conceived of as being incompatible with its being part of a descriptive, explanatory psychology.

But philosophers who suspect that concepts and concept possession have a normative dimension to them need not adopt McDowell’s irreducible “space of reasons”. For example, rather than seeing a theory of concepts *merely* as a fundamental component of an intentional psychology, Peacocke takes a theory of concepts to be inextricably tied up with normative epistemology, while at the same time allowing that the theory be naturalistically kosher. In a series of densely-argued books—*A Study of Concepts*, *Being Known*, and most recently, *The Realm of Reason*—Peacocke has proposed one of the leading philosophical theories of concepts, and articulated its relation to questions about how we ought to conceive of reasons and truth-conditions (/epistemology and metaphysics) in various domains, as well as the nature of reason-giving relations in general.

Throughout his work, Peacocke implicitly adopts the following meta-level view: providing a theory of concepts is of a piece with answering very general normative,

epistemological questions about reasons and rationality. What sorts of questions? Here are some examples:

- (1) In virtue of what are some reasons for judging a content *good*, and others *bad*?
- (2) In virtue of what are there *a priori* ways of coming to know certain contents?
- (3) How does perceptual experience *rationalize*, or *entitle* one to make, perceptual judgments?
- (4) Why, in certain circumstances, do some perceptual judgments count as *knowledge*, and others not?

On Peacocke's view, concepts are constitutively tied to a whole host of related normative notions—e.g., reasons, rationality, entitlement, and knowledge—and he claims that recognizing the ways in which they are so tied will illuminate interesting concept-based answer to normative epistemological questions such as (1)-(4). It's worth briefly mentioning the sorts of answers Peacocke gives to these questions, so as to get a feel for the way in which he takes a theory of concepts to be related to normative epistemology.

With regard to question (1), for instance, Peacocke claims the following:

Certain circumstances in which a thinker may find himself can give him good reasons for taking particular attitudes to thoughts built up from given constituent concepts. In some cases the status of the reasons as good reasons is dependent upon the identity of one of the constituents of the complete content in question.

When it is, we can count the triple of circumstance, attitude, and thought as among the normative liaisons of the constituent in question. (1992a, p. 126)

For instance, your seeing a certain individual *as a bald man* may give you a good reason for judging *that man is bald*, but not a good reason for judging that *that spy is bald*

(Peacocke 1992a, p. 126). That your experience gives you a *good* reason for judging the one content but not the other, Peacocke claims, is *constitutively* tied to identity of the concepts involved.

Peacocke gives a similar answer to (3). Only certain experiences will *rationalize*, or *entitle* one to judge a given content, and Peacocke claims that this too is a matter of the identity of the concepts involved:

[I]t seems *obvious* that the identity of an observational concept is relevant to the issue of why it is that a thinker's perceptual experience entitles her to make a perceptual, empirical judgement. (2000c, p. 257, my emphasis)

Peacocke's answer to question (2) falls out of his thoroughgoing, yet 'moderate' rationalist epistemology, which he calls 'Moderate Rationalism':

[F]or any a priori way of coming to know a given content, there is a substantive explanation of why it is a way of coming to know that has a priori status, an explanation which involves the nature of the concepts in the given content.
(2000c, p. 260)

A priori knowledge is thus explained, *not* by appeal to a special faculty of 'rational insight' or 'rational intuition', which many have found mysterious and naturalistically inexplicable, but rather in terms of the possession conditions of the relevant concepts.

Finally, Peacocke's answer to (4) is given in his so-called 'Linking Thesis':

[T]here is a class of concepts each member of which can be individuated, partly or wholly, in terms of the conditions for a thinker's knowing certain contents containing those concepts. (1999, p. 13)

Peacocke's motivation for the Linking Thesis has a number of sources, not least of which is that he thinks that a theory of concepts ought to provide the resources for meeting what he calls the 'integration challenge', the challenge of providing a metaphysics and an epistemology that are simultaneously sustainable. We'll explore the motivations for the Linking Thesis further in Chapter 3.

Of course, whether other theorists look favorably on Peacocke's approach to these questions will depend on whether they too view concepts as constructs needed primarily for the purposes of normative epistemology. In our discussion of the personal/subpersonal distinction, we noted one of the constraints that come along with this meta-level view:

The notion of a concept has its home within the domain of reason-based explanation of thought and action. Nothing not having to do with reason-based explanation should be included in an account of what individuates a particular concept. (Peacocke 1996/1999, p. 348)

But as I suggested, theorists who approach concepts from the perspective of psychological explanation will fail to see the reason for placing such strictures on a theory of concepts. In any case, they certainly need not find it (for example) as *obvious* as Peacocke does that the identity of certain concepts are, as a constitutive matter, relevant to normative questions about entitlement, or questions about how to integrate our metaphysics and epistemology for some domain. Indeed, in Chapter 3, I argue that adopting the psychological perspective provides us with a reason to resist Peacocke's claims here.

At this point, the important thing to note is that Peacocke's theory of concepts, at the object-level, is apparently determined by his stance with respect the meta-level question. For it's only because Peacocke takes the task of providing a theory of concepts to be of a piece with the task of providing a theory of normative epistemology that he's led to his particular claims about the possession conditions for certain concepts, and how concepts are constitutively linked to rationality, entitlement, and knowledge. His claims about the nature of concepts are thus driven by his stance on the meta-level question. However, a theme that will emerge in Chapter 3, and recur throughout the rest of the dissertation, is that one can draw upon a theory of concepts in answering such normative questions *without* taking concepts themselves to be constitutively tied to normative reasons, rationality, knowledge, etc.

Putting Peacocke's Meta-Level Approach in Context: Verificationism and its Failure

Peacocke is not the first philosopher to claim that a theory of concepts is inextricably connected to epistemology. In fact, Peacocke's approach is continuous with much of what goes under the heading of the 'linguistic turn' in philosophy, and it's important to see it in this light. Initially, the linguistic turn consisted in the idea that in order to give a philosophical account of thought, we should *first* give a philosophical account of language. Under the assumption that the structure of natural language mirrors the structure of thought, the hope was thus that one can give an account of the latter *by giving* an account of the former (Dummett 1993). Indeed, perhaps, as Dummett claims, what's

characteristic about analytic philosophy is its claim that this is the *only* way to give such an account.¹⁸

As Williamson (2004) points out, in recent years the philosophy of mind has displaced the philosophy of language at the center of much of the debate. Williamson says:

[T]he notion of a mental representation is central to the new philosophy of mind. A concept is a mental representation in this sense, whether or not it corresponds to an expression in a language of thought. One might therefore classify both thought and language together under the more general category of representation, and argue that the linguistic turn was just the first phase of the *representational turn*, on which the goal of philosophy is the analysis (in a generous sense) of representation. (2004, p. 108, original emphasis)

Both Peacocke and Fodor would count as philosophers of the ‘representational turn’. But what distinguishes Peacocke is his connection, presumably through his teacher Michael Dummett, to the verificationist tradition associated with the positivists, which arose out of the linguistic turn (or, in Williamson’s terms, the first phase of the representational turn). For along with the shift of focus from thought to language came a corresponding shift from theories of justification to theories of meaning. Descartes’ preoccupation with *justifying* the whole of science was thus replaced with the positivists’ interest in

¹⁸ The way Dummett tells analytic philosophy’s story, it might seem that philosophers who take thought to be *explanatorily prior* to language (e.g., Evans, Fodor, and Peacocke), don’t count as analytic philosophers. Indeed, Dummett suggests just this, at least about Evans (1993, p. 4). But he also goes on to say that one needn’t subscribe to the particular claims of those who founded the analytic tradition in order to be a member of the ‘analytic school’. It seems to me, though, that if what matters for membership in the school is “adopting a certain philosophical style” and “appealing to certain writers rather than to certain others” then this would also determine who counts as an analytic philosopher. After all, why should non-analytic philosophers be allowed in the analytic school? I don’t suppose that anything substantive hangs on the terminology, though.

providing an account of the *semantics* of scientific claims. The hope, of course, was that a theory of meaning could provide the means with which to explain how scientific claims are justified. Peacocke approaches the task of providing a theory of concepts with a similar hope.

Verificationism is *one* way of providing a theory of meaning that can do such epistemological work. On this view, the meaning of a statement is *identical* with its confirming/disconfirming conditions, and thus knowing the meaning of a statement *just is* knowing what it would take to verify that the statement is true, i.e. to be justified in believing it. For instance, a verificationist might *define* claims about something's being an acid or a base in terms of its turning litmus paper pink or blue. At the level of concepts, the claim would be that the concept ACID is constitutively tied to *turning litmus paper pink*, and the concept BASE is tied to *turning litmus paper blue*. The motivation behind such claims was clearly epistemological, for the verificationists hoped to provide a theory of meaning according to which *knowing* the meaning of a concept is tantamount to knowing what it would take to be *justified* in applying it to something. To possess ACID, on this view, is to know when one is justified in believing that something's an acid. There's thus no real gap between specifying the meaning of a concept and saying what it would take to be justified in applying it.¹⁹

¹⁹ Another motivation of the positivists was to demarcate science from pseudo-science. Statements that are consistent with all possible evidence—i.e., statements that *have* no confirming/disconfirming conditions—are rendered meaningless and thus unscientific. The positivists had in much such statements as Heidegger's "Das nich nichts" and Bradley's "The Absolute enters into, but is itself incapable of, evolution and progress" (Ayer 1952). But the claims of contemporary metaphysicians about the nature of universals, tropes, states of affairs, and so on, would arguably also be ruled out. As some metaphysicians themselves admit, if two systems are equal with respect to the standard theoretical virtues (simplicity, explanatory power, etc.), then there's simply no way of deciding which system more accurately describes the world. David Armstrong, for instance, takes it to be a live possibility that a theory of universals and some form of trope theory are mere "alternative languages" (1989, p.139). See Oliver (1996) for discussion.

Fodor is thus correct when he says that “the idea that semantics might underwrite justification, thereby doing for us what God wasn’t able to do for Descartes, is most of what is called philosophy’s ‘linguistic turn’ (2001b, p. 4). But it’s important to see that verificationism is one particularly *extreme* way of carrying out this turn, and thus getting a theory of concepts to do epistemological work. It’s thus worth briefly rehearsing some of the well-known difficulties facing verificationism. Since Peacocke’s theory of concepts attempts to do much of the same work while avoiding the difficulties, it’ll be useful to have these difficulties in mind when we consider his view in more detail.

For starters, and as some positivists were well aware, it turns out to be incredibly difficult to formulate a *non-vacuous* version of the verifiability theory of meaning (Hempel 1965). Moreover, the possibility of verification-transcendent truth is a notorious difficulty for verificationists. For it arguably leads to anti-realism, e.g. about the past (Peacocke 1999). More importantly, and perhaps more intuitively, we seem to *understand* the meaning of certain concepts well *before* we’ve worked out what would count as evidence for believing that they apply. String theory provides a particularly vivid contemporary example of this. Would a verificationist really be prepared to claim that string theorists do not *understand* what they’re claiming about the world *merely* because they haven’t yet figured out the conditions under which the theory would be empirically confirmed or disconfirmed? Of course, if verificationists are prepared to say that what matters for meaning is not what’s presently verifiable but what’s verifiable *in principle* (as Ayer (1952) does), then there’s no problem here. For according to current estimates, an accelerator the size of the *entire universe* would allow us to literally see

individual strings (early estimates had it that an accelerator the *mere* size of the galaxy would do!) (Greene 2000, p. 215).

But it's important to see that the point isn't whether it's logical, practical, or nomological possibility that's relevant to determining the meaning of 'string'. Rather, the point is that string theorists make these and other calculations concerning how truths about strings could be confirmed or disconfirmed *well after* they formulate their claims about strings. *Contra* Quine, then, it's implausible that the "associations between observable events and theoretical vocabulary" are the means by which "we learn the theoretical vocabulary in the first place" (1974, p.38). For theorists must surely acquire the theoretical concepts in order to *work out* their relation to observable events. It thus can't be that the content of the concept STRING, for instance, *consists in* its verification conditions, since theorists exploit the very concept STRING in determining what those conditions are. One simple way of putting this point is that it often takes quite a bit of *ingenuity* (and not *mere* conceptual competence) to come up with experiments that confirm or disconfirm theoretical claims. Indeed, it's often only the very best of scientists that are able to construct the elaborate experimental designs that will do the trick, which is why they're the ones who win prestigious prizes.

Note, too, that verification conditions often *change* as a theory progresses. For instance, some string theorists claim that strings might be under much less tension than the standard theory supposes, which would make them substantially larger. For some this is an exciting possibility, since if it's true it means that string theory may be confirmed or disconfirmed by the next generation of particle accelerators (Greene, 2000, p.398n8). This would be a case, then, in which physicists' beliefs about the conditions under which

string theory can be tested would change as their *beliefs about strings* change. Surely any theory must allow for such changes without *thereby* demanding a change in the content of STRING. Imagine a string theorist who rejects this possibility, claiming that what *he* means by ‘string’ is tied to the observations one would make in a universe-sized particle accelerator! Perhaps this is far-fetched, but there is a real problem for verificationism here, which concerns *which* conditions are to be held constitutive of meaning and which aren’t.

As Quine (1953a) famously points out, and philosophers of science since Duhem have emphasized, hypotheses simply can’t be tested *in isolation*. Doing the experiments to test for strings, for example, presupposes a large body of hypotheses about the world—from all of the relatively ‘low-level’ claims about the inner workings of a particle accelerator, to the ‘high-level’ theoretical claims about the strings themselves. Indeed, when one thinks of actual cases of confirmation in science in this way, Quine’s “confirmation holism”—embodied in his famous claim that “statements about the external world face the tribunal of sense experience not individually but only as corporate body” (1953a, p. 41)—is very hard to resist.²⁰ And it suggests that verificationists do not have a principled way of deciding which conditions are supposed to be constitutive of a given concept and which aren’t. For given confirmation holism, it might seem arbitrary to take a particular condition as constitutive of any given hypothesis (or concept).

Now, some philosophers argue that one can endorse what’s *right* in Quine’s confirmation holism while at the same time holding out for some kinds of *local* confirmation, which Quine and others wrongly take such holism to rule out. One might

²⁰ In Chapter 5, we’ll see that confirmation holism itself should be kept distinct from this claim, at least with the ‘only’ included.

agree that not *all* concepts are constituted by their confirmation conditions, but argue that this needn't imply that *no* concepts are so constituted. After all, it's *prima facie* plausible that some concepts *are* tied to their confirmation conditions. Dispositional concepts such as FRAGILE, FLEXIBLE, SOLUBLE, POISONOUS (etc.) are good candidates. For possessing a dispositional concept plausibly requires knowing under what conditions a disposition will manifest itself. Possessing FRAGILE, for instance, arguably implies knowing that objects that fall under it will (*ceteris paribus*) break if dropped, and possessing SOLUBLE arguably implies knowing that things that fall under it will (*ceteris paribus*) dissolve in water.

Even so, the Quinean will insist that it's not entirely clear how we're supposed to decide *a priori* which conditions are constitutive of a given concept and which aren't. Perhaps it's *analytic* that fragile objects tend to break when dropped, and that soluble substances tend to dissolve in water (Mumford 1998). But this requires an analytic/synthetic distinction, which is a cost that many Quineans will not be willing to incur. They will insist, rather, that the ways in which we confirm or disconfirm hypotheses always depend upon our empirical theory of the world. As Fodor and Lepore put it, "it is *a posteriori* (rather than *a priori*) what confirms what" (1992, p. 39). On this view, empirical beliefs such as those concerning the size of strings, or about how objects with certain molecular and crystalline structures will behave under certain circumstances, will determine the relevant confirmation conditions, and these *can't* be known *a priori*.²¹

²¹ Even if confirmation conditions depend upon our empirical beliefs of the world, this doesn't *entail* that they're thereby only *contingently* related to the property in question. On 'dispositionalist' or 'causal' theories of properties, for example, the causal powers a property bestows on a particular are *essential* to the identity of the property (Shoemaker 2003, Swoyer 1982, Ellis 2001). On such views, the causal powers that individuate *being a string* would include those a string has in virtue of *being a certain length*, or *being under a certain amount of pressure*. The causal powers that individuate *being a string* must be *discovered*, but they are nonetheless powers that strings have in all possible worlds.

Of course, someone who insists on at least some concepts being constituted by their confirmation conditions may deny Fodor and Lepore's claim that "it is a posteriori what confirms what". Where one comes down on this issue will depend on where one thinks the burden lies with respect to the analytic/synthetic distinction, confirmation holism, and constitutive conceptual connections. In Chapters 5 and 6, I argue not only that conformational holism is perfectly compatible with an analytic/synthetic distinction, but also that our best empirical theory of the world may in fact require such a distinction.

It's worth mentioning one last, related objection to verificationism, namely, that when coupled with confirmation holism, verificationism leads to *semantic* holism. The idea is that if the content of a concept is identified with its confirming/disconfirming conditions, and no concept has confirming/disconfirming conditions of its own, then the content of a concept must be identified with the confirming/disconfirming conditions of our *entire* theory, or belief system. In other words, verificationism leads to the claim that the content of a concept is not something one can specify *independently* of *all* of the relations it bears to other concepts. In my view, the price of semantic holism is very high indeed, for it appears to be incompatible with the existence of genuine disagreements among people, genuine changes of mind within a person, scientific realism, and the reality of intentional laws (Fodor and Lepore 1992). I thus take it that semantic holism is not a genuine option. If confirmation holism is independently plausible, this counts as yet another reason to deny a verificationist account of meaning.

It bears emphasis that a rejection of verificationism is *not* tantamount to giving up on the hope that a theory of concepts might underwrite a theory of justification. We've already seen that Peacocke takes the task of providing a theory of concepts to be of a

piece with providing a theory of normative epistemology. But, while Peacocke's theory of concepts does have a verificationist element to it—in that he argues it rules out certain concepts as spurious, or beyond the limits of intelligibility (1992a, ch.8)—he argues that it carries no commitment to a thoroughgoing verificationism, e.g. like that found in the work of Dummett (1978). For one needn't claim that the meaning of a concept is *identical* with its confirming/disconfirming conditions in order to claim that a theory of concepts can do work in normative epistemology. It's sufficient to formulate one's theory of concepts so that it *explains* the normative and epistemic properties of certain judgments, e.g., why certain judgments are rational, warranted, or count as knowledge under certain conditions. As we saw above, Peacocke goes even further, claiming, for example, that what counts as a good reason for a judgment depends upon the identity conditions of the concepts that are the constituents of the judgment's content.

Why not leave questions about rationality, knowledge, entitlement, etc. to the epistemologists? For Peacocke, the very notion of a concept can't be separated from these normative notions. As he has recently put it:

My own view is that there is a large circle of interrelated notions, including entitlement, knowledge, and even intentional content itself, each of whose elucidations ultimately involves the others. (Peacocke 2004a, p. 11)

If this is right, then what motivates a theory of concepts can't be separated from epistemological questions about knowledge and entitlement. As we'll see in Chapter 3, not only are there reasons to resist this claim, but it's doubtful that Peacocke himself needs to endorse it in order to carry out his epistemological project.

The Two Meta-Level Perspectives and the Issue of Priority

Consider the following question: Which is explanatorily prior, concept *possession*, or concept *individuation*? For Peacocke, a theory of concepts *just is* a theory of concept possession, and thus there is nothing more to the identity conditions for a concept than an account of what it is to possess the concept. Fodor, on the other hand, claims that an explanation of concept possession is ultimately *parasitic* on an explanation of concept individuation: “First you say what it is for something *to be* the concept X ... and then *having* the concept X is just *having whatever the concept X turns out to be*” (1998a, p. 2, original emphasis).

Why does this question of priority matter? According to Fodor, it matters because the claim that concept possession is prior to concept individuation “frequently manifests a preference for an ontology of mental dispositions rather than an ontology of mental particulars” (1998a, pp. 3-4), which he apparently takes to be incompatible with the claim that concepts have causal powers.²² But, as Peacocke (2000a) rightly points out, accepting the priority of concept possession is fully compatible with “the thesis that concepts considered as particulars are have causes and effects and cannot be reduced to anything dispositional” (p. 328). For if the priority claim is taken as a claim about types (as in Peacocke 1992a), then it is fully consistent with the claim that the particular tokens are causally efficacious.

²² Fodor’s first ‘non-negotiable’ condition on a theory of concepts is that “concepts are mental particulars; specifically, they satisfy whatever ontological conditions have to be met by things that function as mental causes and effects” (1998a, p. 23). And he says elsewhere: “[I]f concept possession is a dispositional state, the causation of behavior by thoughts isn’t ‘event causation’; it’s not to be construed on the analogy of billiard balls colliding. Rather, thought causes behavior in the way that fragility might cause the glass to break” (2004a, pp. 29-30). Unfortunately Fodor does not make clear precisely what this way is, or why he thinks it’s problematic. Needless to say, the issue of the causal efficacy of dispositions is controversial. See, e.g., Mumford (1998) and Rives (2005).

Even so, the priority issue is relevant to our concerns for the simple reason that *starting* with the question of what it is to possess a concept leads very quickly into epistemological considerations. For by having concepts we have all sorts of epistemic capacities. Having concepts allows us to devise plans, draw inferences, make judgments, and so on. If we take concept possession to be explanatorily prior to concept individuation, then we might be led to think that some of these epistemic capacities are constitutive of concept possession. And if there's nothing more to the individuation of a concept than an account of what it is to possess it, then such capacities will be concept-constitutive.

However, according to Fodor, the only capacity that's constitutive of having a concept is the capacity to *think thoughts with it*. Of course, the capacity to think thoughts about what falls under a given concept brings along with it a whole host of other epistemic capacities. A thinker who didn't possess DOG, for instance, couldn't plan to take his dog for a walk, judge that the barking next door is coming from a dog, infer that since John owns a dog John owns an animal, and so on. But it's a mistake, on Fodor's view, to take these *or any other* epistemic capacities to be *constitutive* of concept possession. Although Fodor doesn't talk in terms of possession conditions, we might characterize his position as follows: the only possession condition on a given concept is having the corresponding mental representation, which bears the appropriate content-determining relation to the relevant property.

Since Fodor starts with the question of what it is to be a concept, and he takes concepts to be mental representations, he seems to identify the task of providing a theory of concepts with the task of providing a theory of mental representation. For those who

think that a theory of concepts is needed primarily for the purposes of psychological explanation, this identification might be seen as a virtue. As we saw above, psychological explanations require laws that quantify over mental representations, and concepts seem to be the simplest mental units that have both causal and representational properties. Since psychology needs both a theory of mental representation and a theory of concepts, identifying these tasks would be economical.

As we noted in Chapter 1, there may be good reasons, which are *independent* of our meta-level question, to deny that concepts are identical with mental representations. In Chapter 7, I consider Fodor's positive reasons for insisting on the identification, and argue that there are good reasons to resist it. At this point, though, what's important to notice is that Fodor's taking the task of providing a theory of concepts to be identical with the task of providing a theory of mental representation stands in stark contrast to Peacocke, who views the task of providing a theory of concepts as *much* wider than this.

Now, one of my aims is to show that the meta-level issue of whether concepts are needed primarily for the purposes of normative epistemology or explanatory psychology ought to be sharply distinguished from the question of whether Peacocke's or Fodor's particular object-level theory is correct. Embedding a theory of concepts in a theory of psychological explanation needn't lead to Fodor's object-level theory, and embedding a theory of concepts in a normative epistemology needn't lead to Peacocke's object-level theory. The position I ultimately argue for is that Fodor's meta-level approach ought to lead us to *a version* of the object-level theory Peacocke endorses.

But before we consider the object-level dispute further, I need to consider the issue of whether one meta-level perspective ought to have priority over the other. One

might reasonably worry whether it's even possible to argue for a particular answer to the meta-level question. For is it really possible to argue, for instance, that when theorizing about concepts, the perspective of psychological explanation ought to have *priority* over the normative, epistemological perspective? Can't a theorist interested in concepts approach the issue from whatever perspective they see fit? Indeed, isn't it plausible that for *any* given area of inquiry, there will be a number of distinct approaches to the phenomena in question, each of which is perfectly legitimate? Consider, for instance, bats, which can be studied from any number of different perspectives: ecology, population genetics, molecular genetics, physiology, evolutionary theory, or some combination thereof. Would it even make sense for someone to claim that one of these perspectives has priority over any of the others? Surely the mere fact that there are interesting questions to ask about bats from a variety of perspectives itself legitimizes the practice of approaching them from those perspectives, without giving priority to any one of them.

Why can't the same be said for concepts? Part of what makes bats different from concepts in this regard is that, in the case of bats, I take it that there is no serious question as to *what* is being studied from these different perspectives. Unlike 'bat', 'concept' is a term of art, whose meaning, as Jackendoff says, will be determined by its "role in a larger world view that includes the nature of language, of meaning, and of mind" (1989/1999, p. 305). If one approaches concepts from the psychological perspective then one may end up with a theory of *what concepts are* that is different from the theory of one who approaches concepts from the perspective of normative epistemology. This is clearly not the case with theorizing about bats from different perspectives. For instance, an

ecologist's and a molecular geneticist's theories about a particular species of bat will differ only to the extent that they are making different *kinds of claims* about bats. Both theories can be true since, again, there is no serious question as to what bats are. Unlike in the case of bats, what one takes concepts *to be* may be determined by the perspective one approaches the issue from.

How might one argue that the psychological perspective has priority over the epistemological one? One might start by pointing out that concepts are the constituents of thoughts, and that any theorist who begins from a perspective that assumes otherwise is *ipso facto* not theorizing about concepts. I take it that all relevant parties can agree with this. Those who approach concepts from the perspective of psychological explanation will obviously agree, since they take a theory of concepts to have its proper home in an explanatory theory of the mind, and according to the best such theory (i.e., RTM) thoughts have concepts as constituents. Those who approach concepts from the perspective of epistemology should also agree. For they're interested in questions concerning rationality, entitlement, knowledge, and justification, all of which are properties of thoughts. Peacocke, for instance, is interested in explaining the normative properties of particular kinds of thoughts (i.e. judgments) in terms of the nature of their constituent concepts. In order for the nature of concepts to explain the normative features of such thoughts, it's plausible that concepts must be their constituents.

Suppose, then, that all parties must begin with the claim that concepts are the constituents of thoughts firmly in place. From here, one might think that it's a short step to the claim that the psychological perspective has priority over the epistemological one. For thoughts themselves, one might think, are logically prior to whatever normative,

epistemological properties thoughts have. For instance, one might think that there *couldn't be* knowledge, justified belief, or rational judgment *unless* there were thoughts, in the same way, perhaps, that there couldn't be good or right actions unless there were actions.²³ If the psychological perspective has priority with respect to thoughts, then the fact that explanatory psychology has priority when it comes to concepts might be parasitic on the fact that it has priority when it comes to thoughts, since concepts are the constituents of thoughts. And one might argue that explanatory psychology *does* have priority with respect to thought, since we don't posit thoughts for the purposes of doing epistemology. Rather, one might argue, we posit thoughts (and their contents) because they're necessary to explain generalizations concerning human behavior.

Consider, in this regard, an eliminativist or a behaviorist who challenges the existence of thoughts and their constituent concepts. One might claim that even those who take a theory of concepts to be needed primarily for normative epistemology will have to defend concepts on explanatory, psychological grounds. For suppose they say to the eliminativist/behaviorist: “But if there weren’t concepts, then we couldn’t understand how justification is possible, why some judgments and not others are rational in certain circumstances, or how *a priori* knowledge is possible!” The eliminativist/behaviorist will clearly *not* be moved by this. For if there *are* no thoughts, then there *can’t be* justified beliefs, rational judgments, or *a priori* knowledge. One might argue, then, that defending the existence of the former by insisting on the existence of the latter won’t work.

²³ Louise Antony says: “no device can be said to have *epistemic* access to any aspect of its environment unless it is a device that *represents* its environment. (Of course there may be other conditions necessary for epistemic access—but this much at least is necessary.)” (1995, p. 74, original emphasis). Similarly, one might think, one can’t know something about the environment, rationally judge something about the environment, or be entitled to form a belief about the environment, unless one can *think* about the environment.

Although I'm sympathetic with this line of reasoning, I don't think it's sufficient to show that the psychological perspective has priority over the normative, epistemological perspective. The reason is that one can't *non-question-beggingly* claim that thoughts are logically and explanatorily prior to the normative features of thought. For it's part of the very normative, epistemological perspective in question that thoughts themselves are constitutively connected to reasons and rationality. Thus, while a theorist who approaches concepts from the perspective of explanatory psychology may think that thought is explanatorily prior to rational, or entitled thought, the theorist who approaches concepts from the perspective of normative epistemology will not. Since the above argument turns on this premise, it's thus apparently question-begging.

The flip side of this, of course, is that philosophers can't argue for the priority of the normative, epistemological perspective by *assuming* that rational, entitled thought is logically or explanatorily prior to thought. In what follows I simply assume for the nonce that concepts have their proper home in an explanatory psychology. I hope the reader does not find this disconcerting. My aim is to argue that adopting this perspective can shed interesting light on some recent philosophical disputes over concepts. In particular, I'll be arguing that adopting the psychological perspective allows us to steer a middle course between the object-level theories of Peacocke and Fodor. In order to show this, I don't *also* need to take on a large-scale defense of the psychological perspective itself. Let me conclude this chapter, then, by saying a bit more about the differences between the object-level theories of Fodor and Peacocke.

Distinguishing the Object-Level Theories

For the better part of twenty years now, Fodor has been engaged in a sustained attack on what's known as conceptual, or inferential role semantics. The idea behind this sort of view, which has its roots in Wittgenstein's claim that the "meaning of a word is its use in a language" (1953/1958, §43), is that the identity of a concept is determined by its *role* in a thinker's cognitive life, e.g. in thought and inference. The view is perhaps most plausible in the case of logical concepts. If you possess the logical concept AND, for instance, then it's *prima facie* plausible that from the conjunctive thought JOHN RAN AND MARY SWAM, you must be willing to infer *both* the thought MARY SWAM *and* the thought JOHN RAN. If you're unwilling to draw these inferences—i.e., if you're unwilling to infer both P and Q from thoughts of the form 'P and Q'—then presumably you lack the concept AND.

Many philosophers have thought that something like this ought to work for non-logical concepts too. A thought containing the concept BROTHER, for example, plausibly has certain of its entailments *in virtue* of its containing BROTHER. The thought JOHN HAS A BROTHER entails the thought JOHN HAS A MALE SIBLING, and that it does so is plausibly a matter of its containing the concept BROTHER. Similarly, the thought JOHN KILLED HIS BROTHER entails the thought JOHN'S BROTHER DIED, and it's plausible that it does so *in virtue* of its containing the concept KILL. To possess these concepts is thus partly a matter of being able to draw certain inferences.

Fodor loathes conceptual/inferential role semantics, and has pushed the following line of reasoning very hard: If conceptual/inferential semantics is true, then, for any given concept, either *all* of the inferences and judgments that it figures in are concept-

constitutive, or only *some* of them are. The former option is semantic holism, which (as we've seen) faces many problems, most notably that it's incompatible with the Shareability Constraint. As Fodor recently put it, "since practically everybody has some eccentric beliefs about practically everything, holism has it that nobody shares any concepts with anybody else" (2004, p. 35). Since the viability of intentional psychology depends upon people at least sometimes sharing concepts with each other (as the Shareability Constraint requires), and the viability of intentional psychology is not in dispute here, semantic holism faces a serious problem.²⁴

Presumably, then, conceptual/inferential role theorists ought to claim that only *some* inferences and judgments are concept-constitutive. But if they go this route, then they owe some account of what distinguishes those inferences and judgments that are constitutive from those that aren't. And Fodor argues that the only way to provide such an account is to endorse an analytic/synthetic distinction, and claim, in effect, that the concept-constitutive inferences and judgments are the analytic ones. But, the argument continues, there are good philosophical and empirical arguments for the claim that there is no principled analytic/synthetic distinction. Thus, Fodor claims, the conceptual/inferential role theorist in fact has no way of distinguishing the constitutive from non-constitutive roles.

I argue in Chapter 5 that this line of reasoning fails. But Fodor suggests that in light of it conceptual/inferential role semantics ought to be abandoned, and along with it the view that lexical concepts are structurally complex. In its place, Fodor proposes what

²⁴ I'm assuming here that holistic accounts that give up on a notion of 'content identity', and invoke instead some notion of 'content similarity', don't work. I won't discuss this issue, since it's orthogonal to our primary concerns. See Block (1986), Harman (1999), Fodor (1998a), and Fodor and Lepore (1992) for discussion.

he calls ‘Informational Atomism’, according to which lexical concepts are unstructured atoms, which have their content in virtue of law-like relations that hold between them and the properties they express. On this view, the concept BROTHER means *brother* in virtue of a law-like relation that holds between instances of *being a brother* and BROTHER, and *not* in virtue of the conceptual/inferential relations BROTHER bears to other concepts, e.g. MALE. Similarly, KILL means *kill* in virtue of a nomic relation it bears to *being a killing*, and not in virtue of its relation to CAUSE TO DIE.²⁵

Now, although Informational Atomism has it that none of the epistemic properties of a concept are constitutive, it does allow epistemic properties a role in fixing content. And rightly so, for it’s plausible that a concept’s epistemic properties are what *sustain* the laws in virtue of which it has its content. Consider, for instance, the law in virtue of which the concept DESK has *being a desk* as its content. This law can be mediated by a variety of DESK’s epistemic properties. Perceptual mechanisms are the paradigm: put a thinker in a well-lit room full of desks, then the thinker will *recognize* that the objects are desks, and a tokening of DESK will ensue. The mechanisms that underwrite a thinker’s ability to recognize desks might involve prototype or stereotype-like representations,

²⁵ Now, exactly what this law-like relation comes to is far from clear. As we noted in our brief discussion of representational error, Fodor himself has argued that it’s an asymmetric dependence relation (Fodor 1987, 1990). Given the numerous objections to his account found in the literature (see, for example, the papers in Loewer and Rey (1991)), however, Fodor rightly wishes to distance the plausibility of intentional psychology *per se* from the plausibility of the asymmetric dependence account: “I don’t want anybody to think that if [asymmetric dependence] is wrong, then the practical syllogism goes too. That said, I think [asymmetric dependence] does what it promised to do: it suggests that there is no *principled* reason why you can’t embed a theory of misrepresentation in an informational semantics. I think this claim is interesting (if true) because so many non-naturalists in semantics have denied it” (2004b, p.110). For Fodor, then, what the asymmetric dependence account shows is that there is no *in principle* barrier to constructing a theory that accounts for the normative properties of concepts and content in purely non-normative, descriptive vocabulary. Fodor makes clear, however, that *whatever* the nature of the actual content-making relation, it will *not* treat as constitutive any of a concept’s epistemic properties.

images of exemplars, or other epistemic properties that psychologists have shown we exploit in a variety of categorization tasks.

According to Informational Atomism, such properties and mechanisms sustain the laws in virtue of which concepts have their content, even though they are not constitutive of the concepts themselves. Rather, possessing DESK is a matter of possessing a mental representation that stands in the appropriate nomic relation to desks, *however the relation is mediated*. Perceptions of desks that lead to tokenings of DESK are the paradigm, but the law could also be sustained by hand-held devices that light up when and only when a desk is present, or by anything else could sustain a reliable connection between desks and DESK. To paraphrase Fodor (1998a, p. 76), it's *that* a thinker's mental representations contrive to resonate to *being a desk*, not *how* they contrive to resonate to *being a desk*, that is constitutive of the concept DESK.

However, conceptual/inferential role semanticists, such as Peacocke, take the epistemic properties of some concepts to be constitutive. The concept RED, for instance, is reliably connected to the property of *being red* via perceptual mechanisms. Thinkers experiencing objects as red is what typically mediates the connection between RED and *being red*, for experiencing something as red tends to lead to tokenings of RED, e.g. judgments that *that's red*. This epistemic property of RED, on such views, *is* concept-constitutive. Similarly, Peacocke draws our attention to cases in which experiences make available certain perceptual-demonstrative concepts, like THAT DESK, and argues that the fact that experiences mediate the connection between the concepts and their referents is constitutive of the concepts.

Fodor has recently dubbed conceptual/inferential role approaches to concepts ‘Concept Pragmatism’, and his alternative approach ‘Concept Cartesianism’ (2003, 2004a). On his view, Concept Pragmatism is not only where “cognitive science went wrong” (as the subtitle of his (1998a) proclaims), but also the whole of 20th century philosophy of mind (2004a)! Fodor says:

[T]he characteristic doctrine of 20th century philosophy of mind/language [is] that *concept possession is some sort of dispositional, epistemic condition*. Maybe it’s some sort of ‘knowing that’; or maybe it’s some sort of ‘knowing how’; or maybe it’s a bit of both. In any case, ‘knowing’, ‘believing’ and the like must come into the story somewhere, and what you have to know in order to have a concept ipso facto constitutes the concept’s content. (2004a, p. 29, original emphasis)

On Fodor’s alternative Cartesian account, “it’s not what you know (-how or –that) that determines what concepts you have; *it’s what you are able to think about*” (2004a, p. 31, original emphasis). In order to possess TREE, for instance, you don’t have to know that trees are plants, or that they typically have leaves; you don’t even have to know how to recognize good instances of trees when you see them. To possess TREE, on this view, is simply to have a bit of mental apparatus in your head that ‘resonates’ with trees—a mental representation that enables you to *think about* trees. According to the Cartesian, concept possession is thus an intentional state, but it’s not in any way an epistemic one.

It bears emphasis that the object-level dispute between Cartesians and Pragmatists *crosscuts* the dispute between those who think that concepts are needed primarily for the purpose of psychological explanation and those who think they’re needed primarily for normative epistemology. The meta-level question is thus distinct from the question of

whether Informational Atomism or conceptual/inferential role theories are correct.

Indeed, I argue in later chapters that approaching the task of providing a theory of concepts from the perspective of psychological explanation provides us with good reason to reject Informational Atomism and adopt instead some kind of Concept Pragmatism.

However, as we'll see in the next chapter, adopting the psychological perspective provides us with reasons to reject Peacocke's version of Pragmatism, according to which there are constitutive connections between concepts, and rationality and knowledge.

CHAPTER 3

PEACOCKE'S KNOWLEDGE PRAGMATISM: FOUNDATIONALIST MOTIVATIONS AND REASONS FOR RESISTANCE

Introduction

This chapter proceeds as follows. I begin by saying a bit about the foundationalist motivations that motivate Peacocke's Pragmatism, as well as the kind of theoretical work that his kind of Pragmatism can do. Then, I explore Peacocke's argument that concepts individuated in terms of judgment can *also* be individuated in terms of knowledge. This argument is particularly important for Peacocke, since it's meant to establish a constitutive connection between concepts, on the one hand, and normative notions such as rationality and knowledge, on the other. I argue that it fails, and that theorists who approach the task of providing a theory of concepts from the perspective of psychological explanation should not endorse it. But, on behalf of Peacocke, I also suggest that my arguments are not necessarily devastating to his epistemological aims.

One of the important morals that will emerge in this chapter is that ‘Concept Pragmatism’ is an umbrella term that fails to distinguish two distinct doctrines. According to *Peacocke’s* Pragmatism, concepts are constitutively tied to rationality and knowledge, with all of their normative dimensions. I call this view ‘Knowledge Pragmatism’. But there is a weaker kind of Pragmatism, which Fodor’s term ‘Concept Pragmatism’ fails to distinguish from Peacocke’s, according to which concepts are constitutively tied to judgment, but *lack* any constitutive connections to rationality and

knowledge. I call this weaker view ‘Judgment Pragmatism’. Although Peacocke’s meta-level approach leads him to adopt the stronger kind of Pragmatism, I suggest that Judgment Pragmatism will suffice even for those who think a theory of concepts ought to do epistemological work. I argue, moreover, that there are good reasons for theorists who approach concepts from the perspective of psychological explanation to resist Knowledge Pragmatism.

Foundationalist Motivations and Epistemically Individuated Concepts

As we saw in Chapter 2, Peacocke has recruited his theory of concepts to do heavy philosophical work. It’s meant to provide the foundation for a thoroughgoing, yet ‘moderate’ rationalist epistemology, which explains *a priori* knowledge by appeal to the possession conditions of the relevant concepts (Peacocke 1998a, 1998b, 2000c, 2004a). Moreover, it’s meant to deliver the means with which to solve a perennial philosophical problem, namely, the problem of providing a metaphysics and an epistemology that are simultaneously sustainable. Peacocke (1999, 2000b) argues that, at least for a range of domains, his theory of concepts provides the key with which to meet such integration challenges.¹

¹ This project is carried out most fully in his recent book *Being Known*, where he considers the integration challenge for a range of domains. Different kinds of solution are offered for each. For example, in the case of the past Peacocke maintains a robust realism, which requires revisions to our theory about the way we come to know past-tense truths (e.g., it requires externalism about memory). He goes the other way around with necessity, however. Many philosophers want to be realists about necessity, but the most developed account of modal truths is found in Lewis (1986), who takes other possible worlds to be on a par with the actual world. Arguably, this makes knowledge of modal truths difficult to come by. In the case of necessity, Peacocke thus reins in the metaphysics in order to bring it in line with an appropriate epistemology. Still other solutions are available, as in the case of reconciling an externalist metaphysics of intentional content with our knowledge of our own mental states. As we’ll see, I’m focusing on what Peacocke claims is a condition for solving them at all, namely, that the concepts in question are epistemically individuated.

These projects are tremendously interesting, and deserve the careful attention of philosophers. But for our purposes, the important thing to note is that they apparently rely upon Peacocke’s Pragmatism, his view that there’s a tight connection between epistemology and a theory of concepts. This connection is put forward in Peacocke’s so-called “Linking Thesis”:

[T]here is a class of concepts each member of which can be individuated, partly or wholly, in terms of the conditions for a thinker’s knowing certain contents containing those concepts; and ... every concept is either such a concept, or is individuated ultimately in part by its relations to such concepts. (1999, p. 13)

I’ll follow Peacocke in calling such concepts “epistemically individuated”. While several approaches individuate concepts in terms of judgment, the Linking Thesis is unique in that it goes further and individuates concepts in terms of knowledge.² In fact, Peacocke argues that any concept that can be individuated in terms of judgment is *also* an epistemically individuated concept. We’ll go into his reasons for claiming this in the next section. But first, we should consider the foundationalist motivations underlying the Linking Thesis, an example of the *kind* of concept that Peacocke claims is epistemically individuated, and Peacocke’s reason for saying that any *non*-epistemically individuated concept will ultimately be individuated by its relations to epistemically individuated ones.

In order to understand the motivation behind the Linking Thesis, we must first see that even fans of epistemically individuated concepts must admit that not *all* concepts are epistemically individuated. Here, one must only consider the incredibly intricate inferences that physicists must make to arrive at truths in physics, e.g., truths about

² Peacocke has hinted at this conception in earlier writings (1992a, pp. 157-158; 1992b, pp. 807-811), but as far I know it doesn’t get a full treatment until Peacocke (1999, chapter 2).

quarks. The ways in which physicists come to know things about quarks are clearly *not* individuated by the concept QUARK.³ For physicists must first learn, and then exploit the concept QUARK in order to *work out* the conditions under which claims about quarks can be confirmed or disconfirmed. Theoretical concepts in the physical sciences are thus clear examples of *non*-epistemically individuated concepts. Since scientists arrive at knowledge involving such concepts by making abductive inferences, the concepts in question are clearly not individuated in terms of knowledge. Indeed, in general, concepts that figure in contents that are *inferentially* known will *not* be epistemically individuated.

There is, however, a famous argument in epistemology according to which inferential knowledge is possible only if there's some *non*-inferential knowledge. What I have in mind, of course, is the traditional 'Regress Argument' for epistemological foundationalism. This argument is important for our purposes because it is, in large part, what *motivates* Peacocke's claim that some concepts are epistemically individuated. Anthony Quinton provides an exceptionally clear statement of the Regress Argument:

If any beliefs are to be justified at all ... there must be some terminal beliefs that do not owe their ... credibility to others. For a belief to be justified it is not enough for it to be accepted, let alone merely entertained: there must also be good reason for accepting it. Furthermore, for an inferential belief to be justified the beliefs that support it must be justified themselves. There must, therefore, be a kind of belief that does not owe its justification to the support provided by the others. Unless this were so no belief would be justified at all, for to justify any

³ In this respect, the concept QUARK is no different from STRING, our example from Chapter 2. I use QUARK here for the simple reason that the existence of quarks (as far as I know) is not in dispute, and this is obviously not the case with strings.

belief would require the antecedent justification of an infinite series of beliefs.
(1973, p. 119)

Knowledge is thus threatened with a vicious regress. While many of our beliefs are clearly justified by inference, the Regress Argument suggests that at some point this kind of inferential justification must come to an end. Otherwise, each justified belief would require an infinite series of beliefs, which is *prima facie* absurd.⁴

The Regress Argument has been tremendously important in shaping both historical and contemporary debates in epistemology. As Laurence BonJour says:

[T]he [epistemic regress] problem ... is perhaps the most crucial in the entire theory of knowledge. The stand which a philosopher takes here will decisively shape the whole structure of his epistemological account. (1985, p. 18)

The foundationalist's way out of the regress, of course, is to claim that there are some beliefs that are *non-inferentially* known, and that all inferential knowledge ultimately depends for its justification upon these non-inferentially justified beliefs. Not surprisingly, much of the resistance to foundationalism comes in the form of skepticism about the possibility of spelling out *how* a claim can be non-inferentially known.

For Peacocke, this is where epistemically individuated concepts come in. For he claims that the Regress Argument provides a kind of transcendental rationale for thinking that some concepts *must be* epistemically individuated. Peacocke argues, in Kantian fashion, that the existence of such concepts is a condition on the *very possibility* of our having knowledge at all. While he focuses on abductive inference, the point is perfectly general:

⁴ This is not to say that some philosophers haven't embraced the regress. Peter Klein (1998), for instance, defends a kind of 'Infinitism', according to which justification can actually result from an infinite regress. For detailed arguments against Klein's position, see Gillett (2003), and Klein (2003) for a reply.

[I]f knowledge is to be possible at all, not everything can be known by inference to the best explanation. An inference to the best explanation can yield knowledge only if the propositions to be explained in the explanation are themselves already known. If they are not already known, the explanatory hypothesis in question, however impressive, cannot by that means acquire the status of knowledge. It follows that if knowledge is to be attainable in some cases by inference to the best explanation, there must be some knowledge which is not so attained. ... It is knowledge of contents containing epistemically individuated concepts which ultimately makes possible knowledge attained by inference to the best explanation. (1999, pp. 15-16)

More recently, Peacocke has framed the argument in terms of ‘entitlement’:

There is an abstract, structural argument that if rational, entitled thought is to be possible at all some concepts must be such that one is default-entitled to presume that one is in the circumstances in which they are individuated. ... Now could it always be that inference, or some other entitled transition, has to made before we are entitled to apply a concept? It seems that this could not be so if entitled application is ever to get started. (2004a, p. 72)

Finally, to round off our sample of foundationalist quotes, here’s one that makes clear that Peacocke takes the existence of non-inferential, *a priori* knowledge to be a condition on the very existence of empirical science:

[I]t seem to me incoherent to suppose that the empirical ways of knowing employed in reaching empirical theories, including our theory of the layout of the observable world around us, could exhaust the ways of coming to know

propositions. Any case of knowledge of an empirical theory exists only because some a priori entitlements also exist. Empirical knowledge is not merely inextricably entwined with the a priori. A better metaphor would be that the a priori provides the girders without which empirical entitlement would collapse (2004a, p. 194).

We can now see that the Regress Argument is used as a kind of ‘Ur-argument’, in both the theory of knowledge *and* the theory of concepts. For one’s stance on the Regress Argument shapes not only the structure of one’s epistemology, as BonJour says, but it can also shape one’s views on the nature of concepts. For Peacocke uses the Regress Argument to motivate his claim that a theory of concepts must take at least some concepts to be epistemically individuated.

Indeed, it’s striking to note how the traditional foundationalist claims about knowledge are mirrored, at the conceptual level, in Peacocke’s Linking Thesis. Consider the following characterization of foundationalism, provided by BonJour:

[E]pistemological foundationalism...is the twofold thesis: (a) that some empirical beliefs possess a measure of epistemic justification which is somehow immediate or intrinsic to them, at least in the sense of not being dependent, inferentially or otherwise, on the epistemic justification of empirical beliefs; and (b) that it is these “basic beliefs”, as they are sometimes called, which are the ultimate source of justification for all of *empirical* knowledge. All other empirical beliefs, on this view, derive whatever justification they possess from standing in appropriate inferential or evidential relations to the members of this epistemically privileged class. (1985, p. 17, original emphasis)

These two claims present the foundationalist with two challenges. First, one needs to show *how* such immediate, intrinsic justification is possible. As BonJour says, “a basic empirical belief is in effect an epistemological unmoved mover”; and he asks, “How can a contingent, empirical belief impart epistemic ‘motion’ to other empirical beliefs unless it is itself in ‘motion’? (Or, even more paradoxically, how can such a belief epistemically ‘move’ itself?)” (1985, p. 30). Second, one needs to show how it is that all other beliefs are ultimately justified by their *relations* to non-inferentially justified beliefs.

Peacocke’s Linking Thesis apparently offers an answer to both of these challenges. If the first part of the Linking Thesis is true, then we have an explanation, *at the level of concepts*, for how the justification of a belief could possibly be *intrinsic* to it. For if the content of the belief has epistemically individuated concepts as constituents, then, assuming certain conditions are met (see below), the belief is *thereby* justified. Moreover, the Linking Thesis, if true, explains why all of our inferential beliefs ultimately depend for their justification upon non-inferential beliefs. For the second part of the Thesis claims that non-epistemically individuated concepts, which are the constituents of the contents of inferentially held beliefs, are individuated ultimately by their *relations* to epistemically individuated concepts. Abstract, theoretical concepts—such as QUARK—are thus presumably individuated ultimately by their relations to epistemically individuated ones. This provides the foundationalist with an answer to the question of *how* the justification of our theoretical beliefs, which are based on non-demonstrative forms of inference, depends upon the justification of our basic, non-inferential beliefs. As Peacocke admits at one point, “this is actually a form of a classical

rationalist principle, to the effect that all *a posteriori* reason-giving relations rest ultimately on *a priori* reason-giving relations” (2000a, p. 334). Since epistemically individuated concepts are the ingredients of *non-inferentially* known contents, they’re thus supposed to be what make knowledge by inference possible.

Now, one obvious way for an opponent of epistemically individuated concepts to proceed would be to argue against the foundationalist motivation for them, and instead defend an alternative, coherentist picture of knowledge and justification. For if it could be shown that ‘*a posteriori* reason-giving’ does *not* need to be grounded in ‘*a priori* reason giving’, then we wouldn’t need epistemically individuated concepts to underwrite all of our knowledge by inference. I note this merely in passing, for a full-scale defense of a general theory in epistemology, foundationalist or coherentist, is (fortunately) *well* beyond the scope of this dissertation. I’m highlighting Peacocke’s foundationalism for two reasons: (1) I think Peacocke’s Pragmatism can be better understood in the context of a foundationalist theory of knowledge; and (2) while foundationalist claims are scattered throughout Peacocke’s corpus, he rarely makes explicit how foundationalism motivates his theory of concepts.

What kind of concept is epistemically individuated? Recognitional concepts are the paradigm. If RED is recognitional, then, in optimal conditions, thinkers who possess RED will judge that an object is red when they’re perceiving it *as red*, and taking their experience at “face value”.⁵ When does an experience of an object count as an experience of the object *as red*? Peacocke says that it’s when the experience “presents its object in a red’ region of the subject’s visual field,” where “red’ is fixed as that property

⁵ To say that a thinker is taking an experience at face value means, roughly, that she isn’t worried about skeptical counterpossibilities, whether the lighting is good, etc. For more on this, see Peacocke (1999, pp. 51-55).

characteristically instantiated in regions of the visual field in which red objects are properly perceived” (1992a, p. 7). Given this characterization, one can experience an object *as red* without possessing the concept RED. For to experience an object as red is to enjoy an experience that presents the object in a red’ region of the visual field, and *being red'* is a sensational property. This comports with our everyday talk, for in ordinary English we say things like the following: “The object *looks* red to him, but of course he would never *say* (or *think*) that it looks red, since he doesn’t have the concept RED; he’s only two-years old, etc.” But, thinkers who *do* possess RED, according to Peacocke, must be willing to judge that an object is red when they experience it as red and they’re taking their experience at face value.

An important thing to notice about such judgments is that they’re in accordance with the norms of rationality. For it’s surely rational to judge that something’s red if you see it *as red*, and are taking your experience at face value. Moreover, it’s *prima facie* plausible that it’s *irrational* to be unwilling to judge that something’s red in such circumstances. For this reason, Peacocke calls the judgments that figure in the possession conditions for epistemically individuated concepts “rationally non-discretionary”. On his view, if thinkers possess RED, then they’re not rationally permitted to withhold the judgment, say, *that’s red*, when they’re experiencing an object as red, and the relevant conditions are optimal. To say that RED is *epistemically* individuated is to say that the fact that such rationally non-discretionary judgments count as *knowledge* in appropriate circumstances is (at least partly) individuative of the concept RED.

Why should we believe that such judgments count as *knowledge*? Because, Peacocke says, “there is … a general connection between the conceptual, the epistemic,

and the metaphysical” (Peacocke 1999, p. 39). In this case, there’s a close relation between the way the property of *being red* is individuated, and the way the concept RED is individuated: “The relations to shades which contribute to the individuation of the colour are precisely those to which one who grasps the colour concept must be sensitive when making perceptually based judgments involving the concept” (2000c, p. 268). This provides the basis for Peacocke’s concept-based explanation of our putative *a priori* knowledge of color-incompatibilities. Perhaps more interestingly, Peacocke argues that our concept METAPHYSICAL NECESSITY involves tacit knowledge of the conditions under which putative possible-world descriptions represent genuine possibilities, so that the possession conditions for the concept are tied to the very conditions that *make* something possible (1999, chapter 4). Again, my aim isn’t to evaluate the details of Peacocke’s account of particular cases, but to draw attention to the fact that it’s his Knowledge Pragmatism that’s carrying most of the philosophical load here. For *both* Peacocke’s account of *a priori* knowledge *and* his solutions to various integration challenges depend upon the relevant concepts being epistemically individuated.

From Judgment Pragmatism to Knowledge Pragmatism?

It should be clear by now that Peacocke’s theory of concepts is both imbedded in and motivated by a larger project, which, at the most general level, is that of constructing a theory of what both the world and our minds must be like, such that the latter can *know about* the former. One such condition, as we saw, is imposed by the Regress Argument: we clearly have some inferential knowledge; but our having some *non*-inferential knowledge is a condition on the very possibility of our having inferential knowledge; so

we must have some non-inferential knowledge. For a theorist who accepts the conclusion of the Regress Argument, like Peacocke, a natural question to ask is: What must our minds be like in order for such non-inferential knowledge to be possible? According to Peacocke, an adequate answer to this question requires the postulation of epistemically individuated concepts.

Now, I suspect that a foundationalist epistemologist, who hasn't much thought about the nature of concepts, will find Peacocke's claims plausible. But consider the reverse possibility, namely, a theorist who *has* thought about the nature of concepts, but hasn't thought much (or at all) about the nature and structure of human knowledge. Consider, in particular, a theorist who approaches concepts from the perspective of psychological explanation, and agrees with Peacocke that concepts are individuated by their roles in judgment. Are there any good reasons for such a Judgment Pragmatist, who approaches concepts from the perspective of psychological explanation, to make the *further* claim that some concepts are individuated in terms of *knowledge*?

Peacocke thinks there is, for as I mentioned above he argues that any concept that can be individuated in terms of judgment is *also* an *epistemically* individuated concept. It's worth going through Peacocke's argument for this claim, in part because it will serve to highlight what Peacocke takes to be the essential connections between concepts, on the one hand, and rationality and knowledge on the other. I argue that Peacocke's defense of the claim that Judgment Pragmatism leads to Knowledge Pragmatism fails. But after spelling out his argument, and noting the problems with each of its steps, I suggest that this is far from devastating to Peacocke's aims. For he does not in fact *need* concepts to

be epistemically individuated in order to carry out his project. Concepts individuated in terms of judgment will suffice.

Peacocke's argument consists of the following four steps:

- (1) Take a target concept which is individuated in terms of its role in outright judgement. Then consider the judgements mentioned in the concept's possession condition, as judgements a thinker must be willing to make in specified circumstances. These judgements must be ones which are rationally required of the thinker, in those circumstances, if she makes any judgements on the matter at all. They must be rationally non-discretionary judgments.
- (2) Rationally non-discretionary judgments aim at knowledge.
- (3) So, if the suitably attained presuppositions of the thinker when making a rationally non-discretionary judgment are fulfilled, and any beliefs on which she is relying in making it are knowledge, and any faculties on which she is relying are operating properly, the rationally non-discretionary judgments will be knowledge.
- (4) Hence the target concept could be individuated in part in the following way: as that concept which, when certain judgments involving it are made by specified methods, and the properly made presuppositions are fulfilled, and any beliefs on which the thinker is relying are knowledge, and any faculties on which she is relying are operating properly, then the judgements so reached involving the concept are knowledge. (1999, pp. 17-18)

At first glance, the argument appears straightforward. For the idea behind it is apparently simple: a Judgment Pragmatist ought to accept the existence of epistemically

individuated concepts since the judgments that figure in the possession conditions for concepts, *in the right circumstances*, will also count as knowledge. The concepts individuated in terms of judgment, the thought continues, could thus just as well be individuated in terms of knowledge. As Peacocke puts it:

The point is not that the formulation of the possession condition in terms of judgements is in some way incorrect. It is not. The point is rather that the formulation in terms of knowledge is equally correct. (1999, p. 28)

But I wonder whether this deceptively simple argument is actually flawed. In fact, I think the argument is faulty at every step. Let's start with premise (1).

The claim in (1), so far as I can see, is that if it's true that a thinker must judge that such and such in certain specified circumstances in order to possess a given concept, then the judgment must be rationally non-discretionary for the thinker in those circumstances. Consider RED, again. If, in order to possess RED, thinkers must be willing to judge, say, *that's red*, when they're having an experience of an object as red and taking their experience at face value, then the judgment that *that's red* must be rationally non-discretionary. What's the *argument* for (1)? Somewhat unhelpfully, Peacocke says that the claim in (1) is "part of what is involved in the idea of a possession condition" (1999, p. 18), at least by a Judgment Pragmatist's lights. Perhaps this explains why his first argument for (1) seems to be little more than a restatement of it:

If, in given circumstances, a content is one on which a rational thinker can intelligibly withhold judgement, consistently with her possession of the concepts involved, then judging that content in those circumstances cannot be part of the possession condition for any of the concepts composing the content. (1999, p. 18)

One question that immediately arises is the following: *which* judgments are the ones that a thinker must be willing to make, *consistent* with possessing the concept in question? I take this to be a question that *all* Pragmatists must ultimately address, for it underlies much of the Cartesian's skepticism about the plausibility of Pragmatism. But since this question applies to both Judgment and Knowledge Pragmatism, we'll put it to one side for now. We'll consider it in more detail in later chapters.

At this point, I'd like to draw attention to a rather different concern with Peacocke's claim here. Note, first, that the skeptic will deny it. For if, as the skeptic claims, we are not rationally justified in judging that *anything* is red, it *can't* be that we are justified in believing that some particular object is red, in certain circumstances, in virtue of the possession conditions for the concept RED. Now, one might think that Peacocke intends his claim here to *refute* the skeptic.⁶ The idea would be that given the nature of the concept RED, having experiences of objects as red *entails* that the concept applies. It thus becomes a sort of conceptual necessity that red objects exist. But this won't do. For there's no good reason, as far as I can tell, for the skeptic to deny that we *think thoughts* about redness. That is, the skeptic needn't deny that we *have* the concept RED. Rather, what the skeptic denies is that (we're rationally justified in believing that) the concept applies to anything in the world. This requires denying that possession of the concept RED is constitutively tied to making certain rationally non-discretionary judgments, as Peacocke claims. So in tying the possession of RED to such judgments, Peacocke is setting his face against skepticism.

⁶ The skeptic does not figure in *Being Known*. In fact, near the beginning of the book Peacocke says that he "will not be tackling scepticism head-on in this work" (1999, p. 11). Regardless of Peacocke's intentions, however, it does seem that his claims here are in tension with skepticism.

On its own, this is more of an observation than a criticism. But notice that Peacocke isn't merely committed to denying skepticism. His claim here is stronger than that, as it's stated in terms of the *intelligibility* of making or withholding certain judgments. Even if we're convinced that skepticism is false, the counterpossibilities that the skeptic makes so vivid are surely intelligible. Indeed, they couldn't be so vivid if they *weren't* intelligible. So long as we find it intelligible for the skeptic to deny certain judgments, while still possessing the relevant concepts, then such judgments can't be rationally non-discretionary, and the above claim is false.

It seems to me that the skeptic is intelligible. For the skeptic essentially makes two claims: (a) there are certain counterpossibilities (of the evil-demon or brain-in-a-vat variety, say) such that *if* they obtain, then we have no knowledge; and (b) we're not in an epistemic position to *rule out* such counterpossibilities. Is there anything in either (a) or (b) such that the skeptic becomes unintelligible, or irrational, in endorsing them? I submit that there isn't. Given this point, the skeptic will claim to have undermined the rationality of the judgments that Peacocke takes to be rationally non-discretionary. Like Peacocke, I'm not a skeptic. But I do think that, insofar as the argument for (1) requires a denial of the very *intelligibility* of skepticism, it is implausible.⁷

⁷ In his more recent work, *The Realm of Reason*, Peacocke admits that his entire project in *Being Known* would collapse without an answer to skepticism, and for this reason attempts to take on the skeptic. He offers what is essentially a Russellian response, arguing that good explanations are “complexity reducing” and that skeptical hypotheses do not reduce complexity but increase it (2004a, Chapter 3). He adds to this a Darwinian claim about what justifies us in believing in the accuracy of our perceptual systems, and even goes so far as to say that it “is a relatively *a priori* truth” that since subjects rely substantially on their perceptual systems in the formation of belief there will be selection for roughly accurate perceptual systems” (2004a, p. 88, my emphasis). But surely it is a substantive, empirical claim that *selection* is responsible for the existence of *any* trait. The contingency of the properties of evolved systems thus makes it very unlikely that we can discover *a priori* how they came to have those properties. Indeed, there are lively debates in both biology and psychology about the extent to which selection is responsible for evolutionary change. See, for example, Fodor (2000a), Gould (1997), Gould and Lewontin (1978), and Lewontin (1990) for skepticism about the power of natural selection, and Maynard Smith (1978), Barkow, Cosmides, and Tooby (1992), Dennett (1995), and Pinker (1997) for more ‘pro-selectionist’ views.

Peacocke's second argument for (1) makes explicit use of what he calls "the unexceptionable assumption that a thinker can do what rationality permits" (1999, p. 19).

Peacocke says:

Spelled out more fully, the case for (1) would run as follows. If rationality permits a thinker to withhold judgement on a content containing a given concept, in specified circumstances, while continuing to possess that concept, then it is possible for a thinker to withhold such judgement while possessing the concept. It follows that willingness to make such a judgement cannot be part of the possession condition for the concept in question. The judgement cannot, in the specified circumstances, be rationally non-discretionary. By contraposition, any outright judgements mentioned in the possession condition for a concept must, in the circumstances mentioned in the possession condition, be rationally non-discretionary. (1999, p. 19)

Notice that the point about skepticism applies here as well. For if the skeptic is right, and rationality permits someone to withhold the relevant judgment, then this passage could actually be taken as an argument *against* premise (1).

But setting skepticism aside, I wonder whether Peacocke's 'unexceptionable' claim that thinkers can do what rationality permits is really so unexceptionable. As Peacocke notes, the "case for premise (1) connects something normative—the rationally non-discretionary—with a descriptive condition which states what it is for a thinker to possess a given concept (1999, p. 18). But as the extensive psychological literature on human rationality suggests, the connection between normative rationality and descriptive psychological claims about how people *actually* reason is not that tight. For instance,

there's an extensive literature concerning people's performance on various versions of the so-called 'Wason selection task', which suggests that our reasoning about conditionals is far from optimal. There's also ample evidence that we're poor reasoners when it comes to evaluating probabilities: people fail to take into account relevant base rates, even when they're readily available; and, given the same background information, people will take certain conjunctive statements ("John is a dentist and a political activist") to be more probable than one of their conjuncts ("John is a dentist"), which of course the laws of probability don't allow. Results like these have led psychologists Richard Nisbett and Lee Ross to claim that humans depart "from normative standards of inference", and are subject to "profound systematic, and fundamental errors in judgments and inferences" (1980, p. 6).⁸

Now, perhaps one could argue that such conclusions do not necessarily rule out Peacocke's 'unexceptionable' claim, since people can *come to see* the ways in which they reason badly, and make the appropriate corrections. The idea would be that even if rationality requires that we make certain judgments and not others, the fact that people often *do not* make the judgments that rationality requires, does not by itself show that people *can't* make such judgments. For people can acquire good reasoning skills, and hone the ones they already possess, and thus get better at bringing their actual judgments in line with the judgments that rationality permits, or requires.

Although this reply is *prima facie* plausible, it seems to me that there could be cases in which people simply can't make the appropriate corrections. I don't have in

⁸ For a review of this kind of literature, in addition to Nisbett and Ross (1980), see Goldman (1986, chapters 13-16), and Tversky and Kahneman (1983). For interesting discussions of how this literature bears on the philosophical literature on rationality, see Stich (1990), Stein (1996), and Botterill and Carruthers (1999).

mind here the possibility of people who are *constitutionally bad* at reasoning, but rather cases in which people, for whatever reason, can't bring themselves to make certain judgments in accordance with what rationality permits, or requires. Suppose John is a gambler, who often makes inferences and judgments that don't comport with what rationality permits. For John frequently commits the gambler's fallacy: he often infers that the likelihood of rolling a two in a game of craps increases as the number of past rolls that don't come up two increases; or, he sticks to his number in a game of roulette, judging that the odds of hitting the number increase as the number of spins in which the number fails to win increases. As Stich and Nisbett point out, it's plausible that in games of chance, John is *in fact the norm*; that is, it's plausible that people frequently follow a principle according to which the probability of some outcome occurring after $n + 1$ consecutive instances of non-occurrence is greater than the probability of its occurrence after n consecutive instances of non-occurrence (1980, p. 192). This is probably true of even some highly reflective people. Indeed, Stich and Nisbett draw our attention to a 19th century *logic text* in which the gambler's fallacy is actually endorsed as good reasoning!⁹

There's no doubt that after having the fallacy pointed out to them, *some* of these people will correct their reasoning, and thus bring their judgments in line with the judgments rationality permits. Perhaps most will. But what are we to make of *incorrigible* gamblers who simply refuse to give up the fallacy? As Stich and Nisbett point out, there's reason to think that, at least for some people, the principles that are guiding their inferences and judgments are in *reflective equilibrium* for them. Perhaps

⁹ The text reads: "Thus, in throwing dice, we cannot be sure that any face or combination of faces will appear; but if, in very many throws, some particular face has not appeared, the chances of its coming up are stronger and stronger, until they approach very near to certainty. It must come; and as each throw is made and it fails to appear, the certainty of its coming draws nearer and nearer" (Quoted in Stich and Nisbett (1980), p. 193).

after having the faulty principles that are guiding their inferences and judgments articulated clearly for them, and after having time to reflect on them and the inferences and judgments themselves, they will *continue* to believe that there's nothing wrong with either. Pointing out the fallacy, for such people, will thus *not* bring their judgments in line with what rationality permits. If such people are even a possibility, then Peacocke's 'unexceptionable' claim is thrown into doubt.

Or, consider a different example. After teaching the traditional arguments for and against the existence of God, and having discussions with students about the role of faith in religious belief, it's clear to me that some students, for whatever reason, are simply more open to doubt than others. It's true that *most* believers appear to have no problem arriving at the following judgment: "God *might not* exist, but, all things considered, I believe that He does". But some of my students seem to believe so strongly that even contemplating the *possibility* that God does not exist is not really an option for them. It seems that they're simply incapable of genuine doubt. But surely rationality at least *permits* thinkers to doubt whether God exists; indeed, rationality arguably requires it. If such doubt is not open to some people, as the anecdotal evidence from my classes suggest, then Peacocke's 'unexceptionable' principle is again off the mark.

But let's put these worries about (1) to one side for now, and consider the next step in Peacocke's argument. Premise (2) says that rationally non-discretionary judgments aim at knowledge. Peacocke begins his defense of (2) by pointing out that *all* judgments, both rationally discretionary and rationally non-discretionary, aim at truth. As he recently put it, "it is a constitutive aim of judgement that one tries to judge that *p* only if it is true that *p*" (2004a, p. 13). I take the claim here to be that the very nature of

judging (or believing) is such that one can't judge (or believe) that p unless one is at least attempting to judge only what's true. Peacocke doesn't tell us whether he thinks aiming at the truth is part of the very concept JUDGMENT, or whether he thinks it's merely constitutive of judgment that it aim at the truth. But regardless of whether it's meant as a bit of conceptual analysis, or a theoretical, constitutive truth about judgment itself, the claim is clearly meant to rule out what we might call "random" or "epistemically irresponsible" judgments—e.g., judging that the number of stars is odd. If it's actually possible to get yourself to assent to the proposition that the number of stars is odd, Peacocke's claim would rule that out as counting as a genuine judgment. For assuming that you have no good reason to believe that the number of stars is odd (you haven't talked to God, or an alien superscientist), you can't be *aiming at the truth* by assenting to the proposition.

Before going any further into Peacocke's argument for (2), I should express my reservations about Peacocke's claim that it's constitutive of judgment that it aim at the truth. It is true that, *in general*, people form beliefs and make judgments in the context of a background desire to get things right. For nobody goes around with a background desire to have false beliefs, and for good reason: it would be incredibly imprudent to do so. But it's not clear that this observation is enough to ground Peacocke's constitutive claim about judgment. For imagine someone who, for whatever reason, deliberately aims to have false beliefs, not in general, but about something specific. For instance, suppose John, our gambler, doesn't *want* to have true beliefs about the principles that guide his judgments in the casino. Why not? Well, he might believe that if he comes to believe the truth about the gambler's fallacy, he won't get as much pleasure out of going to the

casino. Perhaps his desire to have a certain amount of pleasure at the casino is stronger than his desire not to throw away his money. And he suspects that if he comes to believe the truth about the gambler's fallacy he'll gamble away less money, but at the expense of the pleasure he'd otherwise have. The point is that while John may have merely pragmatic reasons for not wanting to believe the truth, it would seem odd to deny that he *literally judges* that that the likelihood of rolling a two in a game of craps increases as the number of past rolls that don't come up two increases, or that the chances of hitting a number on the roulette wheel increases as the number of spins in which the number fails to win increases.

Or consider Pascal's Wager. Pascal claims that there are insufficient *evidential* reasons for belief in God, but nonetheless thinks that one ought to believe for *prudential* reasons. Truth is clearly not the *aim* of Pascal's judgment here. For his judgment is aimed, not at truth, but at satisfying his *desire* not to burn for eternity! Is Peacocke prepared to claim that Pascal didn't really come to *believe* that God exists because his reasons for belief were merely prudential, and not evidential? If Peacocke's claim is intended as a bit of conceptual analysis, then it seems off the mark. For I suspect that many people would have no problem with the *intelligibility* of the following sentence:

Pascal believes that God exists, but also believes that there is insufficient evidence for God's existence.

Of course, people might think that Pascal is being epistemically irresponsible, and perhaps, following Clifford (1877/1999), even doing something that's *morally* wrong. However, it's doubtful that they'll think there's something conceptually confused, or

unintelligible about the sentence.¹⁰ And if Peacocke doesn't intend the claim as an analysis, then what's the *argument* for the theoretical claim that it's constitutive of judgment that it aims at truth? Peacocke doesn't offer one, and as we've just seen the claim is *prima facie* implausible in any case.¹¹

Putting this worry about the constitutive aim of judgment to one side, why think that rationally non-discretionary judgments aim at knowledge? Peacocke points to the simple fact that rationally non-discretionary judgments will be revised in light of evidence that suggests they're not knowledge. To use Peacocke's example, suppose you see a coin and judge *that's a coin*. Suppose, moreover, that given the circumstances, the judgment is rationally non-discretionary for you. Now suppose you're told that there are many coin holograms in area. What's the rational thing to do? Of course the rational thing to do is to think twice about your judgment, and consider whether what you're experiencing is really a coin, or merely a coin hologram. It is, to use Peacocke's terminology, no longer rational for you to take your experience at face value:

The rationality of taking perceptual experience at face value is undermined if the thinker comes to accept that something in these same circumstances, something at which she is not in fact looking, is not a coin even though it would produce the same subjective type of perceptual experience. (1999, p. 21)

¹⁰ There's an issue here about whether it's psychologically possible for Pascal to put himself in a position to believe that God exists, given that he also *believes* he lacks evidential reasons to believe that God exists. This is what makes this case slightly different from the case of John. For in John's case, he deliberately *avoids* putting himself in a position to gather evidence about whether his beliefs about gambling are true. But note that this is an issue about nomological possibility, not conceptual possibility. Some people might think that given empirical facts about human psychology, Pascal can't put himself in a position to genuinely believe that God exists. But this judgment plausibly does *not* reflect the nature of the concept JUDGMENT, or BELIEF, but rather reflects people's *empirical beliefs about* judgments and beliefs. If this is right, then the intelligibility claim above still stands.

¹¹ See Papineau (1999) for a rich discussion of the normativity of judgment, and for an argument that all such normativity in fact belongs *outside* of a theory of conceptual thought.

The presence of coin holograms in the vicinity thus undermines the judgment's counting as knowledge. And Peacocke is apparently suggesting that since it's rational for you to reassess the judgment given the information that it's not knowledge, this shows that rationally non-discretionary judgments *aim* at knowledge.

If this is right, and rationally non-discretionary judgments aim at knowledge, then how does (3) follow? That is, how does it follow that when the presuppositions of a judgment are fulfilled, the relevant faculties and mechanisms are working properly, and the beliefs relied upon in making the judgment are knowledge, that the judgment will not just *aim* at, but *be* knowledge? Peacocke's claim is apparently that such a judgment couldn't fail to be knowledge: "if [the presuppositions, etc.] could all be fulfilled and yet the judgement still not be knowledge, then the judgement would not after all be rationally non-discretionary" (1999, p. 22). So it looks as if the relevant difference between such a judgment that is knowledge and a judgment that isn't, is that the former is rationally non-discretionary and the latter isn't. *Being rationally non-discretionary* is thus quite an important property, for a judgment that figures in the possession conditions for a concept counts as knowledge *only if* it's rationally non-discretionary.

But now recall what it takes for a judgment to have the property of *being rationally non-discretionary*: it must be a judgment that rationality requires a thinker to make in certain circumstances, namely the ones mentioned in the possession conditions for the concept in question. So, on Peacocke's view, there's apparently no real gap between the circumstances in which it's *rational* to make a judgment, given the possession conditions for the concept, and the circumstances in which a judgment will count as *knowledge*. Since a judgment can't be knowledge unless it's true, this

connection between rationality and knowledge implies a corresponding connection between rationality and truth. As Peacocke has recently put it:

Suppose that the possession conditions for concepts composing a given conceptual content p are such that they jointly imply that in given circumstances a thinker will be willing to judge outright that p If the judgement could be false in these circumstances, then it could not rationally be required of thinkers that they be willing to make the judgement in these circumstances, and a formulation of the possession conditions for [a] concept that says they should be so willing would be incorrect. (2004b, p. 88)

We can thus see that Peacocke's view is that there are very tight, constitutive connections between truth, rationality, knowledge, and concepts. Given the nature of rationally non-discretionary judgments, the rationality of a judgment that figures in the possession conditions for a concept is tied to the judgment's counting as knowledge. For if the judgment *weren't* knowledge in those circumstances, then the judgment *wouldn't be* rationally non-discretionary. Given this, the conditions that make such a judgment rational will be the *very conditions* that make the judgment true, which in turn implies that there will be no real gap between the rationality of such a judgment and the truth of the judgment.

Peacocke sums all of this up with his 'Kiplingesque Conditional'. I quote at length:

Let us fix on a judgement of a given content, the content $\Sigma(F)$, say, mentioned in F 's possession condition. In the general case, the circumstances mentioned in this possession condition may include requirements such as these: that the rationally

non-discretionary judgement be made when the thinker is operating in a certain mode M (such as that of taking perceptual experience at face value); that it be made with certain presuppositions, Prsp; that it is inferred from certain premises, Prem; and the judgement is made because the thinker is in some mental state, or is enjoying a certain mental event, E. We can now write the required knowledge-involving clause for a possession condition for the target concept *F* as follows, using these materials from the possession condition involving judgement.

Take the informational conditions, call them Inf, corresponding to the mode M in which the thinker is operating. In the case in which, for instance, the target concept is an observational [i.e., recognitional] concept, the informational conditions Inf would be the conditions for an experience to be a genuine perception. If what I have argued is correct, then the following conditional holds for any arbitrary concept *F*, where Prsp, Prem, and the rest are taken from the judgement-involving possession condition.

If:

the informational conditions Inf are fulfilled;

everything in the presupposition set Prsp is adequately reached and true,

and all the premises in Prem are known;

the thinker is in the mental state or enjoying the mental event E, and

judges that $\Sigma(F)$ because he is so, and because he accepts the members of

Prem, and because he is making the presuppositions Prsp, and because of

the mode in which he is operating;

then:

his judgement that $\Sigma(F)$ is knowledge.

This Kiplingesque formulation is just the generalization of the point I was making when I said that rationally non-discretionary judgements are knowledge when their various properly attained presuppositions are fulfilled, their premises are knowledge, and the relevant faculties or mechanisms are functioning properly.
(1999, pp. 27-28)

The Kiplingesque Conditional makes it clear that, on Peacocke's view, there's virtually no gap between rationality, on the one hand, and truth and knowledge, on the other. An obvious worry is that the Conditional ends up being vacuous. For it looks as if the conditions under which rationality requires us to make the judgments that figure in the possession conditions for the relevant concepts are *identical* to the conditions under which such judgments are both true and knowledge. In other words, Peacocke's 'optimal conditions' seem to collapse into 'whatever conditions turn a judgment into knowledge'. For the Kiplingesque Conditional says: If a thinker judges that $\Sigma(F)$ in optimal conditions, then the judgment that $\Sigma(F)$ is knowledge. But if 'optimal conditions' *just means* 'whatever conditions turn a judgment into knowledge', then the conditional becomes: If a thinker judges that $\Sigma(F)$ in whatever conditions turn a judgment into knowledge, then the judgment that $\Sigma(F)$ is knowledge.

But perhaps this is not a bad thing. After all, Peacocke might say, what *else* would you expect 'optimal conditions' to turn out to be, if not the conditions that turn a judgment into knowledge? If the conditions could obtain, and yet the judgment fail to be knowledge, he might continue, then they wouldn't be optimal conditions after all, would they? The problem with this reply, however, is that the Kiplingesque Conditional is

supposed to be a summary of Peacocke's argument for the claim that judgment-individuation *implies* epistemic-individuation. But if Peacocke is willing to accept the Conditional as trivial, then it follows that the claim that judgment-individuation implies epistemic-individuation is not a substantive one. In fact, Peacocke sometimes seems to suggest that this is how he intends the claim:

We need to be clear on the conclusion of the reasoning so far. The conclusion is not that there is, for concepts individuated in terms of outright judgement, a layer of epistemic requirements in addition to those formulated in terms of judgements. The argument is rather that when we appreciate the rationally non-discretionary nature of the judgements mentioned in such possession conditions, we are in a position to develop an argument that there are requirements for the possession of such concepts which can equally be formulated in terms of knowledge. (1999, p. 28)

This is very odd. For consider again the Judgment Pragmatist, who does *not* take a theory of concepts to be one piece of a larger, philosophical theory about what the world and our minds must be like in order for the latter to *know about* the former, but rather simply a piece of explanatory psychology. For such a Judgment Pragmatist, questions about whether such judgments are rationally non-discretionary, or the conditions under which such judgments count as knowledge will simply be beside the point. Most psychologists are no doubt agnostic about epistemology in general. But even Judgment Pragmatists who are philosophers (like myself) might explicitly deny that concepts are individuated in terms of knowledge. Why? One reason is that they may think that, *pace* Peacocke, knowledge *does* introduce "a layer of epistemic requirements in addition" to

those of judgment. Of course, Peacocke will reply that such Judgment Pragmatists simply haven't appreciated "the rationally non-discretionary nature of the judgments mentioned in the possession conditions". Once they do, Peacocke will continue, they'll see that a judgment's being rationally non-discretionary, in the circumstances mentioned in the possession conditions for the concept, entails that the judgment is both *true* and *knowledge*.

But now we come to what I take to be the real worry about the Kiplingesque Conditional. We noted earlier that not *all* concepts are epistemically individuated. The concept RED may be, but surely QUARK is not. But it seems that in attempting to show that any concept that can individuated in terms of judgment is *also* an epistemically individuated concept, Peacocke has made the Kiplingesque Conditional far too strong. For he claims that the "Kiplingesque conditional can ... function as one clause of a possession condition for the target concept *F*" (1999, p. 28). But suppose we take the target concept '*F*' to be QUARK. It will be true that if all the conditions mentioned in the Kiplingesque Conditional are fulfilled, the judgment $\Sigma(\text{QUARK})$ will be knowledge. But if *QUARK* is epistemically individuated, then which concepts are *not* epistemically individuated? Surely our knowledge of quarks is as theoretical and inferential as knowledge can get! Something has gone wrong.

My diagnosis is that, in attempting to argue that judgment-individuation is sufficient for knowledge-individuation, Peacocke has tried to close what is a *necessary* gap between rationality and truth. *Prima facie*, the conditions under which a judgment is *rational* are one thing, the conditions under which it's *true* are quite another. Even if we grant Peacocke's claim that truth is a constitutive aim of judgment (which I argued above

we shouldn't), why can't a *rational* judgment *aim* at the truth and *miss*? Indeed, it's tempting to read the history of science as one long, extended argument against the Kiplingesque Conditional. Hypotheses that are rational to hold at one point in time are shown to be false at another. Perhaps Peacocke will say that, in hindsight, endorsing such hypotheses was not rational after all. But for scientific realists, at least, there is a strong presumption in favor of the view that there's a significant *gap* between what's rational and what's true.

I've argued that Peacocke's argument for the claim that any concept individuated in terms of judgment is *also* an epistemically individuated concept fails. But I want to end this section with a half-sympathetic conclusion. Even if the considerations I've offered against Peacocke's claim are correct, they're not necessarily devastating to Peacocke's aims. As long as *some* concepts are individuated in terms of judgment, Peacocke can still meet the demands of the Regress Argument, which is his primary motivation for introducing epistemically individuated concept in the first place. All that Peacocke would need to show is that the judgments that figure in the possession conditions for some concepts do *in fact* meet the conditions necessary for knowledge. He doesn't need the additional—and as we've seen, implausible—claim that such concepts can also be *individuated* in terms of knowledge. Of course it would take some work to show that such conditions are indeed met by the judgments that figure in the possession for the relevant concepts. But nothing in what I said above implies that Peacocke (or some epistemologist) couldn't carry out such work.

Similarly, meeting the integration challenge does not *require* that any concepts be epistemically individuated.¹² As long as the judgments mentioned in the possession conditions for the concepts in some domain meet the conditions for knowledge (whatever they turn out to be), then Peacocke can hold onto his claim that the integration challenge is to be solved, at least in part, by supplying a theory of concepts for that domain. Again, the real work is to show that the judgments do in fact meet the conditions for knowledge, not that the concepts are also individuated in terms of knowledge.

Conclusion

The interim morals here are twofold: (1) the motivation behind Peacocke's Knowledge Pragmatism does not suffice to show that we ought to adopt it; and (2) Peacocke's argument that Judgment Pragmatists ought to be Knowledge Pragmatists fails. With respect to (1), *both* the transcendental rationale underlying Knowledge Pragmatism *and* the integration challenge for various domains can be met by a *mere* Judgment Pragmatism. This bears emphasis. For while Peacocke's Knowledge Pragmatism can perhaps *both* explain how some contents can be known non-inferentially *and* provide the means with which to integrate our metaphysics and epistemology for some domains, this is not alone sufficient to show that it ought to be adopted on grounds of explanatory power. It's certainly insufficient to justify Peacocke's claim that "there is a large circle of interrelated notions, including entitlement, knowledge, and even intentional content itself, each of whose elucidations ultimately involves the others" (2004a, p. 11). For a normative, epistemological theory can *draw upon* a theory of concepts, without concepts themselves being *constitutively* tied to entitlement or knowledge, as Peacocke claims. As

¹² As Alvin Goldman (2001) suggests in his review of *Being Known*.

we saw in Chapter 2, a theorist who approaches the task of providing a theory of concepts from the perspective of psychological explanation may view the task as a purely descriptive enterprise. We can now see that this *needn't* preclude a theorist who adopts Peacocke's meta-level approach from drawing upon such a descriptive theory in order to provide answers to normative, epistemological questions.

Consider, by way of analogy, someone interested in answering questions about how one ought to perform heart surgery of some kind. Someone interested in such questions can clearly draw upon a purely descriptive theory of how the heart in fact works. Indeed, such a theory is plausibly *indispensable* in formulating a theory about how a certain kind of heart surgery ought to be performed. But this does not require that a theory of the heart and how it works must *itself* be normative. Just so, a purely descriptive theory of concepts may be of indispensable use to someone, such as Peacocke, who is interested in normative epistemology. But this does not *require* that concepts themselves must be elucidated in terms of normative notions such as entitlement and knowledge. That is, a theory of concepts may have normative import, without its being the case that concepts themselves are essentially normative.¹³

I've argued that there are good reasons for Judgment Pragmatists, who approach the task of providing a theory of concepts from the perspective of psychological explanation, to resist Peacocke's Knowledge Pragmatism. Moreover, I've argued that virtually every step of Peacocke's argument for the claim that Judgment Pragmatism leads to Knowledge Pragmatism is flawed in some way. It's clear that what motivates Peacocke's Knowledge Pragmatism is his meta-level approach. But Peacocke has not shown that a theorist who shares his object-level Judgment Pragmatism, but not his meta-

¹³ I've been influenced here by Horwich's (1998, chapter 8) discussion of the normativity of language.

level approach, must endorse his *further* object-level claim that concepts are constitutively tied to rationality and knowledge. Moreover, as I just suggested, even theorists who *share* his meta-level approach needn't adopt his object-level theory.

CHAPTER 4

THE APPEAL TO EXAMPLES AND THE QUINEAN CHALLENGE

Introduction

In the Chapter 3, I argued that some of the considerations that Peacocke offers in favor of *his* kind of Pragmatism—Knowledge Pragmatism—fail. These considerations, however, presupposed the truth of Judgment Pragmatism, and will thus be of little interest to a Concept Cartesian who denies the truth of *any* form of Pragmatism. Indeed, the Cartesian no doubt agrees with Fodor that if “one asks to hear some serious arguments for [Concept Pragmatism], one discovers, a bit disconcertingly, that they are very thin upon the ground” (1998a, p. 36). This complaint applies to both sorts of Concept Pragmatism.

So too, moreover, do some of Fodor’s arguments against Pragmatism. Over the years, Fodor has leveled many such arguments: that Concept Pragmatism either leads to semantic holism, which violates the Shareability Constraint, or presupposes an analytic/synthetic distinction, which violates Quinean scruples (Fodor 1998a, Fodor and Lepore 1992); that it disrespects compositionality, and is thus unable to explain the systematicity and productivity of thought (Fodor 1998a, 1998c, 1998d, 2001b, Fodor and Lepore 2002); and most recently, that it can’t be formulated without vicious circularity (Fodor 2004a). My aim in this chapter is to consider the first of these arguments, and in particular, the recurring Quinean theme in Fodor’s work that there’s no principled way to distinguish those inferences and judgments that are constitutive of a given concept from

those that are merely ‘collateral’. *If* this Quinean claim is correct, *and* semantic holism is not a viable option, then it looks as if we must admit that *none* of the inferences or judgments that a concept figures in are constitutive. But to admit this would be to simply *give up* on Pragmatism altogether. This line of reasoning threatens both Judgment Pragmatism and Knowledge Pragmatism, since the argument will apply regardless of whether one takes constitutive roles to include knowledge or mere judgments and inferences.

Peacocke has attempted to tackle head-on Fodor’s charge that there are no good arguments in support of Pragmatism, and that there’s no way to distinguish constitutive from non-constitutive roles. His arguments fall into two classes: there are arguments from specific examples, and there are arguments from more general considerations having to do with concepts and reasons. The first class of arguments supports a mere Judgment Pragmatism, whereas the more general considerations are meant to support Peacocke’s Knowledge Pragmatism. In this brief chapter, I restrict my attention to the former sorts of arguments. I consider the more general arguments in Chapter 7.

After spelling out the kind of example Peacocke has in mind, I articulate the Quinean Challenge that appeals to such examples face. I should note at the outset, however, that as pressing as this Challenge is, it’s far from decisive. In Chapter 5, I consider the other side of this dialectic, and argue that there are in fact good reasons to doubt the position with respect to the analytic/synthetic distinction that *motivates* the Quinean Challenge. Then, in Chapter 6, I defend a particular strategy for meeting the Challenge on empirical grounds. In this chapter, though, my aim is to make the Quinean

challenge to Concept Pragmatism as vivid as possible, which I propose to do by considering Peacocke's recent attempt to meet it, and showing why it fails.

The Examples

If we focus on perceptual-demonstrative ways of thinking, Peacocke claims, then we'll agree that, *contra* Fodor, it *is* possible to say which roles are concept-constitutive. Here's the kind of example Peacocke has in mind:

Having a perception of a desk straight ahead of one seems to give grounds, in everyday circumstances, for the content 'that desk is straight ahead', and that it does so seems to be partially constitutive of the identity of the concepts in the content. (2004a, p. 34)

Or consider someone who sees a blue mug, and subsequently judges that the mug is blue. The concepts involved in this case, Peacocke suggests, also seem to be individuated by their role in making judgments. Peacocke says:

The following is constitutive of this perceptual-demonstrative way of thinking: that when the subject is taking perceptual experience at face value, and perceives the object, which is seen as a mug and which is presented as *that mug*, to be blue, then the subject is willing to judge *That mug is blue*. How could one abandon such a sensitivity and replace it with another and still be employing the same concept? (2000a, p. 333)

The question is rhetorical. It's supposed to be obvious, or at least *prima facie* plausible, that one who lacked such sensitivity would *thereby* lack the concept. Peacocke's suggestion is that the plausibility of these kinds of examples illustrates the implausibility

of Cartesianism. For the Cartesian denies that there's a constitutive connection between the concepts THAT MUG, and BLUE, on the one hand, and perceptions of mugs that represent them as blue, on the other. (Ditto for DESK STRAIGHT AHEAD, and perceptions of desks as straight ahead.) That a perception of a mug as blue leads to (and perhaps grounds) the judgment that the mug is blue, according to the Cartesian, is thus not *constitutive* of the concepts involved. Since Cartesianism denies that such relations are concept-constitutive, Peacocke claims that it flies in the face of what seem to be *prima facie* plausible facts about perceptual-demonstrative cases.

I submit that, at this point in the debate, asking a rhetorical question such as 'How *could* one abandon such a sensitivity and replace it with another and still be employing the same concept?' is out of place. Sometimes Peacocke avoids the rhetorical question by appealing to what he takes to be obvious:

[I]t seems *obvious* that the identity of an observational concept is relevant to the issue of why it is that a thinker's perceptual experience entitles her to make a perceptual, empirical judgement. (2000c, p. 257, my emphasis)

And, in specifying the roles he takes to be constitutive of the concept PLUS, he simply appeals to what *seems plausible*:

The additional premise is that finding instances of [the clauses specifying the addition function] primitively compelling and doing so because they are of [those forms] ... are constitutive of a grasp of PLUS. This additional premise *seems plausible*. ... [I]t *seems* that a thinker is not thinking of the addition function in the way of *plus* unless he finds those instances compelling and does so in part because of their form. (1992a, p. 137, my emphasis)

For logical concepts, such as CONJUNCTION, Peacocke appeals to inferences that are “primitively compelling”:

Conjunction is that concept C to possess which a thinker must find transitions that are instances of the following forms primitively compelling, and must do so because they are of these forms:

$$\begin{array}{ccc} p & \underline{q} & \underline{p C q} \\ p \quad C \quad q & p & q \end{array} \quad (1992a, p. 6)^1$$

But, like asking the rhetorical question above, appealing to what’s obvious, plausible, or primitively compelling is no way to argue against a Cartesian who denies that possessing a concept is constitutively tied to making certain judgments or inferences. The reason for this is that the Cartesian agrees with Quine that’s there’s no *principled* way to distinguish between those roles that are concept-constitutive and those that aren’t. Pointing to properties such as *being obvious*, *plausible*, or *primitively compelling* will thus not cut any ice with a Cartesian, like Fodor, who agrees with Quine that such properties do not provide us with a principled distinction between matters of meaning and matters of fact.

To feel the force of the Quinean challenge, consider the current controversy over whether laws that restrict marriage to heterosexual couples violate the equal-protection clause of the constitution.² Many think that this issue is in part conceptual, in that it

¹ Peacocke characterizes the notion of a “primitively compelling” transition as follows: “To say that the thinker finds such transitions primitively compelling is to say this: (1) he finds them compelling; (2) he does not find them compelling because he has inferred them from other premises and/or principles; and (3) for possession of the concept C in question he does not need to take the correctness of the transitions as answerable to anything else” (1992a, p. 6). See Rey (1996/1999) for a discussion of this notion, and for doubts that it’s naturalistically kosher or can successfully meet Quinean demands. Much of the discussion in the present chapter is in the same spirit.

² I owe this example to Georges Rey, who raises it in his *Stanford Encyclopedia* article “The Analytic/Synthetic Distinction”.

depends on the *meaning* of ‘marriage’. Some opponents of gay marriage, for instance, claim that it’s part of the very concept (or definition of) MARRIAGE that it’s a union between a male and a female. Others claim that they find nothing unintelligible or contradictory about the concept SAME-SEX MARRIAGE. I suspect that there are people on each side of the dispute who find their own inferential connections *obvious, plausible*, and perhaps even *primitively compelling*. Of course they can’t both be right, assuming that both share the concept MARRIAGE. For either the inference from MARRIED→HETEROSEXUAL is constitutive of MARRIAGE or it isn’t. Quine’s challenge is to say exactly what would settle the dispute. That is, Quine demands an answer to the following question: What fact about thinkers could possibly make it true that the inference from MARRIED→HETEROSEXUAL is a matter of meaning, and not a matter of perhaps deeply held empirical belief?

Now, if it were *analytic* that marriage implies heterosexuality, then opponents of gay marriage would be correct in saying that gay-rights activists are not talking *about marriage* when they claim that same-sex marriage ought to be legal. Indeed, if MARRIAGE → HETEROSEXUALITY were analytic, then those who failed to make the inference would thereby lack the concept MARRIAGE. For analytic inferences are concept-constitutive. Peacocke himself admits that accepting “the theory of possession conditions as an account of concepts and content does indeed involve commitment to some analogue at the level of thought of the analytic/synthetic distinction” (1992a, p.244n7).

Given this, it seems Peacocke owes the Cartesian an answer to the Quinean challenge. For as Fodor says:

[T]here seems to be no way to distinguish the inferences that *constitute* concepts from other kinds of inferences that concepts enter into. ... Quine shocked [philosophers'] faith that 'defining inference' is well defined, and hence their faith in such related notions as analyticity, propositions true in virtue of meaning alone, and so forth. We have, as things now stand, no account of what makes an inference a defining one, and no idea how such an account might be devised.

(1998a, p. 45)

Quine's challenge, then, for those who think that certain inferences or judgments are concept-constitutive while others are merely 'collateral', is to specify *in a principled way* the properties in virtue of what the constitutive roles *are* constitutive.

Those who speak of constitutive properties of concepts without addressing the Quinean challenge, run the risk of falling prey to the following criticism of Stephen Stich:

In reading the books and articles of those who invoke the notion of a constitutive property, it is easy to get the feeling that one has fallen into a time warp. These philosophers write as though the notion of constitutive properties were entirely unproblematic, and they give no indication that they have ever heard of Quine and his assault on the analytic/synthetic distinction. (1996, p. 88n)

Some prominent Concept Pragmatists are willing to admit that they don't know how to answer Quine's challenge. Consider Paul Boghossian's claims in the following passage (he talks of constitutive inferences, but the point applies equally well to judgments):

If expressions mean what they do by virtue of the inferences they participate in, then some inferences are *constitutive* of an expression's meaning what it does,

and others aren't. And the pressing question is: Which are which? What property does an inference have to have, if it is to be meaning-constitutive?

All the participants to the present debate agree that to this day no one has succeeded in providing a systematic answer to these sorts of questions. As yet, there are no plausible accounts out there of what properties an inference must have if it is to be meaning-constitutive. (1994b, pp. 110-111)

Boghossian wrote that over ten years ago, and it will hardly come as a surprise to Cartesians like Fodor if it turns out that Pragmatists have made little progress on this issue in the meantime. For Fodor claims that *if* Quine is right about there being no principled analytic/synthetic distinction, then there *is* no way to distinguish constitutive from non-constitutive roles. Such a lack of progress will be viewed by Cartesians as evidence that there is no such distinction to be drawn. In fact, Fodor has recently begun treating the lack of a principled analytic/synthetic distinction as a *datum* to be explained, taking it to be a virtue of Informational Atomism that it *explains why* Quine was right that there's no such distinction (1998a, p.71).

As we'll see in Chapter 5, there are reasons for thinking that neither the philosophical nor the empirical arguments against the analytic/synthetic distinction are convincing, and thus that Fodor should *not* take the lack of the analytic/synthetic distinction as a datum to be explained by a theory of concepts. On the other hand, it's clear that Peacocke's appeal to what's obvious or plausible isn't sufficient to show that the Cartesian is wrong to deny that there is such a distinction. In the remainder of the chapter, I explain why this is so.

Why the Appeal to Examples Fails

Consider Peacocke's claim that it's *plausible* that thinkers who see a blue mug in broad daylight will be willing to judge that it's a blue mug, and that failure to do so would be grounds for denying that they had the concept BLUE MUG. This is insufficient to refute the Cartesian, but *not* because the claim itself is implausible. (Who could deny *that*?) Rather, it's insufficient because what's needed is an explanatory basis for the claim that failing to judge that the mug is blue *entails* a lack of the concepts, and neither plausibility nor obviousness provides this.

Consider what Fodor says about the relation between seeing a rabbit and judging that there's a rabbit, on the one hand, and possessing the concept RABBIT on the other:

No landscape is so empty, or so well lit ... that your failure to recognize that it contains a rabbit *entails* that you haven't got the concept RABBIT. So, it couldn't be that your having the concept RABBIT requires that there are circumstances in which you couldn't but recognize a rabbit as such. (1995, p. 36, original emphasis)

The same point holds for seeing a dog, judging there's a dog, and possessing DOG:

[I]f you try to list the sorts of perceptual environments in which *dog*-thoughts *must* arise if a creature has the concept DOG, you will find that there aren't any: no landscape is either so barren, or so well lit, that it is *metaphysically* impossible to fail to notice whether it contains a dog. (1998a, p. 79, original emphasis)

And Fodor will say the same, of course, for BLUE MUG, or DESK STRAIGHT AHEAD.

What's driving Fodor here is his agreement with Quine that's there's no principled way to distinguish between those inferences/judgments that are reliable *because* they're

concept-constitutive and those that are reliable even though they're *not* concept-constitutive. Fodor will thus demand an answer to the Quinean question: What could serve as a *truthmaker* for the claim that a thinker who is unwilling to judge 'that's a rabbit' or 'that's a blue mug', in certain circumstances, lacks the concept RABBIT or BLUE MUG, where such a truthmaker would *rule out* the thinker's merely having an idiosyncratic *belief* about rabbits or blue mugs?

Possible Replies

Although Peacocke doesn't attempt to answer this question, one can imagine a Concept Pragmatist replying as follows: "Granted, I don't know what the truthmaker for this claim is, but that's OK. I'm actually not sure what the truthmaker for lots of claims are, but I can nevertheless know that they are true. For instance, I don't know the ultimate truthmaker for the sentence "The smell of the curry made Eric ill". But I can nonetheless know that there must *be* a truthmaker, since I can *know* that the claim is true. Similarly, I don't know what fact about thinkers makes it true that failing to judge 'that's a rabbit' or 'that's a blue mug' in certain circumstances entails a lack of the concept RABBIT or BLUE MUG. But that's not a problem, since I know it's true that failing to make such judgments manifests their lack of RABBIT and BLUE MUG. Since we can have knowledge of the truth of a claim without having knowledge of the truthmaker in virtue of which it is true, what's the problem?"

But this reply misses the point. Perhaps we don't know the ultimate truthmaker for "The smell of the curry made Eric feel ill". But we do have *some* idea, and, more importantly, we can have non-question begging *evidence* that there must be such a

truthmaker. That is, we can have evidence that Eric feels ill, and that smelling the curry put him in that state. Perhaps “knock-down” evidence that this is so is unavailable, but surely strong non-demonstrative evidence *is* available. The Cartesian is asking for similar evidence that thinkers failing to judge ‘that’s a blue mug’ or ‘that’s a rabbit’ in certain circumstances *lack* the concepts BLUE MUG or RABBIT, again, where this evidence *rules out* the hypothesis that they merely have idiosyncratic beliefs about blue mugs or rabbits. In a certain sense, the Quinean/Cartesian demand for such a truthmaker *just is* a demand for evidence that some connections to judgment are concept-constitutive while others are not. Unlike in the case of the curry’s making Eric feel ill, the Quinean/Cartesian will insist, the Concept Pragmatist has not provided any such evidence.

Second possible reply: “But surely we *do* have evidence that *some* divergences in judgment manifest a difference at the level of concepts. Suppose two people are having a discussion about rabbits, and one thinks that rabbits are the fastest land animals, while the other disagrees. Surely *this* divergence in judgment does not count as evidence of a conceptual difference. To insist that it did would be to insist that whether thinkers have the concept RABBIT depends upon *all* of their rabbit-beliefs. For it’s hard to see how a theorist could accept that thinkers lack the concept RABBIT if they believe that rabbits are the fastest land animals *without* accepting that the content of RABBIT is determined by the *all* of one’s rabbit-beliefs. But to endorse the latter claim is to endorse semantic holism, which leads to the claim that people can’t actually have genuine disagreements about rabbits, or anything else for that matter, since holism violates the Shareability Constraint. So Cartesians and Pragmatists alike ought to reject it. Let’s agree, then, that

disagreement over whether rabbits are the fastest land animals would clearly not count as evidence of a conceptual difference.

“But suppose, in the course of the discussion, a rabbit appears in the vicinity. One person says “Look, there’s a rabbit,” while the other person, for whatever reason, withholds judgment on whether it’s in fact a rabbit. Suppose, moreover, that the latter person is not withholding judgment because of poor lighting, etc. If *this* kind of divergence in judgment occurred, then surely it *would* count as decisive evidence that the thinker who failed to make the judgment lacked the concept RABBIT. If the conversation were to continue, then it would be clear, as Davidson says another context, that the thinker had “changed the subject” (1970/2002, p. 120). There thus *can* be evidence that some judgments are constitutively tied to the possession of certain concepts after all. I just described it.”

Again, the Cartesian will insist that this won’t do. For the Cartesian can admit that someone’s failing to judge that something’s a rabbit in certain conditions would count as *prima facie* evidence that they lacked the concept RABBIT. That is, the Cartesian can agree that such circumscribed would count as *prima facie* evidence that the person had “changed the subject,” or was no longer talking *about* rabbits. (“If you don’t agree that *that’s* a rabbit, then we’re obviously talking past each other,” one might say.) However, in the context of a debate between a Cartesian and a Pragmatist about concept possession, such evidence is apparently question-begging. For the Cartesian will simply deny that such *prima facie* evidence in fact counts in favor of a *constitutive* link between concepts and judgment. For it could also be taken as evidence that the two people have divergent *beliefs about* rabbits. And, the Cartesian will insist, Quine taught us that

there's apparently no principled way of deciding which of these two views is right. As Fodor says:

[P]eople can have radically false theories and really crazy views, consonant with our understanding perfectly well, thank you, which false views they have and what radically crazy things it is that they believe. Berkeley thought that chairs are mental, for Heaven's sake! Which are we to say that he lacked, the concept MENTAL or the concept CHAIR?" (1987, p. 125)

On Fodor's Cartesian view, it's true that people's beliefs and theories *mediate* the mind-world relations that determine conceptual content. But it's possible for people (especially philosophers!) to believe almost anything, including, as Fodor points out, that rabbits are mental. Some of these differences in beliefs may lead to divergent perceptual judgments about whether a given object falls under RABBIT. For the Cartesian, this is perfectly consistent with both thinkers possessing RABBIT, however, so long as the mediation between RABBIT and rabbits is reliable. And perhaps there are good, explanatory reasons for believing that this is so, despite the divergence in belief.

We can now see that the Cartesian has a ready reply to Peacocke's appeal to specific examples. For while it's *prima facie* plausible that if thinkers "abandon such a sensitivity and replace it with another" they will not "be employing the same concept," the question is whether such *prima facie* plausibility tells of anything *more* than an intimate connection between perception and judgment. To return to Peacocke's example, it's certainly true that if our experience represents something *as a blue mug*, and we're taking our experience at face value, then we can't help but judge that *that's a blue mug*.

That's a datum that needs explaining. Peacocke is right that one explanation is that such judgments figure in the possession conditions for the concepts involved.

But it seems to me that if that's right, it needs to be true that we make such judgments *in virtue* of the constitutive connection between them and the concepts. The appeal to examples does not establish that this is so. In demanding that Peacocke show that such judgments hold in virtue of their constitutive connections to concepts, I'm *not* demanding that he show that we exploit an explicit *theory* of concepts when we make perceptual judgments. For people clearly do not make perceptual judgments by appeal to a theory of concepts—e.g., people don't justify their perceptual judgments about blue mugs by appealing to the possession conditions for BLUE MUG, and any theory that demanded that they do so would be absurd. Rather, the point I'm pressing is that, for his argument to be compelling, Peacocke needs to point to a datum that can be explained *only* by appealing to constitutive connections between concepts and judgments. That is, Peacocke needs to point to a fact such that in order to explain it we, *as theorists*, must say that it's *in virtue* of the connection between perception and judgment that a thinker possesses BLUE MUG, which in my view he hasn't done.

Until he comes up with such a fact, the Cartesian has a simpler explanation of the datum that Peacocke *has* pointed to, namely, that there are causal relations between perceptions and judgments. For on *any* plausible view, perceiving something as a blue mug is causally related to judging or believing that it's a blue mug. Indeed, this connection follows from standard functionalist claims about the individuation of mental states, and this is presumably true *regardless* of which brand of functionalism one endorses. For instance, the intimate causal connection between perception and

belief/judgment would surely be part of what Lewis (1972) would ramsify over when defining ‘belief’ (and ‘perception’), if he ever bothered to collect the folk-platitudes about mental states. The same goes, I take it, for functionalists who prefer to ramsify over whatever can be discovered by *a priori* philosophical analysis, such as Shoemaker (2003). For *if* we can know about the nature of mental states from the armchair, then surely that perceptual states are intimately related to judgment/belief states is one of the things we can know. The point also applies to those who accept instead some kind of *psychofunctionalism*, which ramsifies over the deliverances of the best empirical psychological theory (Lycan 1987, 1988, and Rey 1997). For it’s part of our best empirical theory of mental architecture that the outputs of the perceptual system are inputs to the belief/judgment system, and that states of the latter are caused by the former. It thus seems that functionalists of all stripes must apparently accept that any account of perception must respect its intimate connection with belief and judgment. Given this, functionalism alone can explain the relevant data.

For our purposes, the important point here is that nothing *immediately* follows about concepts or concept-possession from such claims about the relation between perceptual states and beliefs/judgments. It can be true that judging that something is a blue mug is related to perceiving it as a blue mug *without* its being the case that there’s a constitutive connection between the concepts BLUE MUG and the judgment *that’s a blue mug*. The data might simply reflect something about perception and judgment, or our concepts PERCEPTION and JUDGMENT, along functionalist lines. In Chapter 6, I claim that such examples might provide an explanatory basis for constitutive connections if the causal relations were subsumed under psychological laws. But taken by itself,

Peacocke's appeal to examples will not convince Cartesians like Fodor who deny that concepts are constitutively connected to judgment.

CHAPTER 5

SALVAGING CONCEPT PRAGMATISM FROM QUINE'S AND FODOR'S ARGUMENTS AGAINST THE ANALYTIC

Introduction

In Chapter 4, Peacocke's appeals to examples were met with essentially Quinean replies. For I argued that such examples do not provide an explanatory basis for the claim that some concepts are constitutively tied to making certain judgments, and as a result that they are question-begging against Cartesians, who deny that there are any such connections. Philosophers who are either thoroughgoing Quineans themselves, or sensitive to the force of the Quinean challenge, will find the arguments of the last chapter unsurprising. For skepticism about specifying concept-constitutive roles in thought and judgment goes hand in hand with skepticism about the prospects of drawing a principled analytic/synthetic distinction. As Fodor puts it:

[I]f ... there isn't an epistemic analytic/synthetic distinction, then the notion of a possession condition is infirm and you can't identify grasping a concept with being disposed to draw the inference by which its possession conditions are constituted. (1998a, p. 33)

In fact, I suspect that many philosophers—Quineans and non-Quineans alike—accept the following conditional: If Quine is right about the analytic/synthetic distinction, then there

can be no principled distinction between the concept-constitutive and non-concept-constitutive inferences and judgments.¹

As we saw in Chapter 2, accepting this conditional *can* lead to semantic holism. Ned Block, for instance, says that he accepts holism because “no one has provided a convincing reason for including some inferences and excluding others” (1991, p. 40). Given the problems with semantic holism, though, some philosophers claim that *since* Quine is right about the analytic/synthetic distinction, then Concept Pragmatism must be given up. At the end of their discussion of Block’s account, for instance, Fodor and Lepore say:

If, as we suspect, Quine is right about the a/s distinction, then the moral of our discussion is that [Concept Pragmatism] is false. (1992, p. 186)

Stich, a fellow Quinean, is a bit more cautious. He says:

[Concept Pragmatists] must either make it plausible that the arguments against the existence of the analytic/synthetic distinction are mistaken or that the notion of a conceptually necessary property can be made sense of without presupposing or entailing the existence of analytic sentences. (1996, p. 62)

¹ Stich says: “[I]f some properties are constitutive for a concept, and others are not, and if...claims about concepts are interchangeable with claims about what our words mean, then it seems there must also be some sentences that are true entirely in virtue of meaning and others whose truth or falsity depends in part on the way the world is. That is, there must be some sentences that are *analytic* and others that are *synthetic*. ... Starting more than forty years ago, however, Quine and others offered some enormously influential arguments aimed at showing that the analytic/synthetic distinction is untenable. On Quine’s view, and on the view of many other philosophers as well, there are no sentences that are true solely in virtue of their meaning. If this is right, then there are no constitutive or conceptually necessary properties” (1996, p. 62). Rey has the same worry: “[I]f there is no principled basis for distinguishing matters of meaning from mere matter of factual belief, then there can be no basis for selecting some and not other conditions as constitutive of possession of a particular concept” (1996/1999, p. 339).

Questions ensue: What exactly *are* the arguments that are supposed to show that there is no principled analytic/synthetic distinction? And how, exactly, do they bear on the question of whether some roles in inference/judgment are concept-constitutive?

This chapter pursues these questions, and proceeds as follows. First, I spell out Quine's arguments against the analytic, and show that the analytic/synthetic distinction is perfectly compatible with both his Confirmation Holism and his claims about empirical revisability. I then turn to Fodor's empirical arguments against the analytic, and focus in particular on his arguments against lexical decomposition and definitions. I argue, first, that there are reasons to doubt that these arguments establish their intended conclusion, namely, that lexical items lack structure 'at the semantic level' and that the corresponding lexical concepts are internally unstructured. Second, and more importantly, I argue that *even if* Fodor's arguments against lexical decomposition and definition are successful, they do *not* constitute a convincing argument against Concept Pragmatism. For Concept Pragmatism needn't endorse the view that the conceptual/inferential role of a lexical concept is specified by its internal structure. I then outline what I take to be viable version of Concept Pragmatism, according to which meaning relations are captured by inference rules, or 'meaning-postulates'. This view, it seems to me, deserves to be resurrected and reconsidered. For it allows one to *accept* the empirical arguments in favor of Concept Atomism, while at the same time *deny* that such arguments imply that there's no analytic/synthetic distinction. I conclude by answering Fodor's objection that such a view has unhappy consequences concerning the relation between concept constitution and concept possession.

Quine's Arguments and their Failure

Some philosophers are rightly puzzled by Fodor's and other philosophers' heavy reliance on claims about "what Quine showed" regarding the analytic/synthetic distinction. As they see it, while it's true that Quine indeed led an assault on the analytic/synthetic distinction, it's far from clear that his arguments establish that there *is* no such distinction. Perhaps his arguments are best regarded, not as establishing that there is no analytic/synthetic distinction, but rather as presenting us with a sort of *puzzle*: on the one hand, there seems to be an undeniable and interesting difference between, say, "bachelors are unmarried" and "bachelors are frustrated"—one can see that the former is true merely by *understanding the meaning* of the sentence, while this is not the case with the latter; on the other hand, as Quine arguably shows, there has been little success when it comes to spelling out a principled distinction between analytic and synthetic sentences.²

Convinced Quineans, of course, will not see matters this way. As they see it, there's a different sort of puzzle, which is generated by two facts: (1) there *is* no analytic/synthetic distinction; and (2) it *seems to speakers/thinkers* that certain sentences/thoughts and not others can be seen to be true merely by understanding their meaning. In an attempt to meet this puzzle, Quine (1953a) appeals to what's "central" in the web of belief, and Fodor (1998a) adds to this an appeal to the existence of so-called "one-criterion concepts". These strategies rely on the idea that we easily confuse epistemic facts with semantic ones. What we naively think of as our semantic intuitions, according to Quine and Fodor, are actually intuitions of facts that are merely epistemic. In Chapter 6, I consider in detail these attempts to "explain away" (2) so as to hold onto

² Thanks to Paul Pietroski for the suggestion that Quine's arguments are perhaps best viewed as presenting a puzzle, and for a discussion that prompted much of the material in this chapter.

(1), and argue that they fail. At this point, however, let's merely note that those skeptical of Quine's arguments against the analytic/synthetic distinction will reject (1).

What are Quine's arguments? To answer this, we need to have clearly in mind Quine's target, which is Frege's notion of analyticity: truth based on the laws of logic and definitions. A statement is analytic, in Frege's sense, just in case it is transformable into a logical truth by substituting synonyms for synonyms. I'll follow Boghossian (1996) and refer to this notion as 'Frege-analyticity'. It's clear at the outset of "Two Dogmas" that Frege-analyticity is Quine's target. Quine says:

Statements which are analytic by general philosophical acclaim ... fall into two classes. Those of the first class, which may be called *logically true*, are typified by:

- (1) No unmarried man is married.

The relevant feature of this example is that it not merely is true as it stands, but remains true under any and all reinterpretations of 'man' and 'married'. If we suppose a prior inventory of *logical* particles, comprising 'no', 'un-', 'not', 'if', 'then', 'and', etc., then in general a logical truth is a statement which is true and remains true under all reinterpretations of its components other than the logical particles.

But there is also a second class of analytic statements, typified by:

- (2) No bachelor is married.

The characteristic of such a statement is that it can be turned into a logical truth by putting synonyms for synonyms; thus (2) can be turned into (1) by putting 'unmarried man' for its synonym 'bachelor'. (1953a, pp. 22-23)

The members of this second class of analytic statements are thus putative examples of Frege-analyticity. Quine goes on to say that “[o]ur problem is … analyticity; and here the major difficulty lies not in the first class of analytic statement, the logical truths, but rather in the second class” (1953a, p. 24).

Famously, Quine goes on to argue that all extant attempts to define ‘analyticity’ in this second sense ultimately involve other, related terms—‘synonymy’, ‘definition’, ‘semantic rule’, and ‘contradiction in terms’—which are, in his view, equally in need of explication. Indeed, Quine points out that these other notions could just as easily be defined in terms of ‘analytic’. They form a “family circle” of intensional notions, and Quine’s complaint is that no one has successfully broken out of the circle. For this reason, his argument here is sometimes referred to as the “circle of terms argument”. Quine concludes the argument as follows:

[O]ne is tempted to suppose in general that the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith. (1953a, pp. 36-37)

As many philosophers—following H.P. Grice and P.F. Strawson (1956)—have pointed out, the circle of terms argument, *all by itself*, is hardly a reason to deny that there’s an analytic/synthetic distinction. For there are plenty of examples of groups of

notions in which each member of the group is defined (only) by mentioning other members of the group. Grice and Strawson mention “morally wrong”, “blameworthy”, and “breach of moral rules” (1956, p. 148), and there are lots of examples from scientific discourse: the terms in “ $E = \frac{1}{2} mv^2$ ”, “ $F = ma$ ”, and “ $F = (gm_1m_2)/d^2$ ” are perhaps examples. But the fact that members of a group of expressions are inter-defined in this way is not normally a reason for thinking that the expressions are senseless, or that belief in their application is, as Quine says, “an article of faith”. Indeed, one wonders why this argument worries Quine so much. After all, if there *are* no analyses, as he claims, then surely we shouldn’t reject the analytic merely because we can’t provide an *analysis* of “analytic”!

Perhaps the circle of intensional notions is different. Harman (1999), for example, suggests that in other cases, some of the terms will be *antecedently* understood, and that this is not the case with the circle of intensional notions. But one might think that there’s a perfectly ordinary notion of ‘sameness of meaning’, and that agreement upon use in this case is legitimate justification of the notion (Grice and Strawson 1956). And if synonymy can be explained in terms of this ordinary notion of ‘sameness of meaning’, then analyticity can be explained in terms of synonymy, as proponents of Frege-analyticity claim. Putnam apparently thinks this, and claims that “*where there is agreement on the use of the expressions involved with respect to an open class, there must necessarily be some kind of distinction present*” (1975b, p. 35, original emphasis). Needless to say, Harman disagrees.

But I don’t want to enter further into this dispute. For the circle of terms argument is certainly not the most powerful of Quine’s arguments. It leaves open *both*

the question of how one should view the relationship between the ‘ordinary’ and ‘technical’ uses of such intensional notions (as the dispute between Putnam, Grice and Strawson, on the one hand, and Harman, on the other, illustrates), *and* the possibility that further research will provide some way of breaking out of the circle. In this latter regard, for instance, philosophers attempting to *naturalize* content can be seen as attempting to break out of the circle, by stating in *non-semantic* terms what it is for a symbol to have a meaning. If the circle of terms argument were the only argument Quine offered against the analytic/synthetic distinction, then it’s doubtful that “Two Dogmas” would’ve been so influential.

Quine’s better argument against analyticity appeals to his epistemological claims—in particular, his confirmation holism and his claims about empirical revisability. According to Quine’s famous picture, our beliefs about the world form a seamless web, and none of our beliefs are immune to revision. With enough recalcitrant experience, even logical and mathematical beliefs could rationally be given up, if doing so would preserve the overall coherence, simplicity, etc., of the entire web. Quine’s epistemology thus poses a threat to the traditional picture of Frege, Carnap, and others, according to which logic, mathematics (and philosophy) were different sorts of enquiry than physics, chemistry, geology, biology, and the other sciences. The former were taken to be *a priori* disciplines, which deliver necessary truths, whereas the latter were taken to be *a posteriori* disciplines, which deliver contingent truths. But if Quine is right, what distinguishes logic and mathematics from physics and biology is *merely* that the former are more “central” in the web of belief than the latter. As Quine puts it:

[N]o statement is immune to revision. Revision even of the logical law of excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and a shift whereby Kepler superceded Ptolemy, or Einstein Newton, or Darwin Aristotle? (1953a, p. 43)

There's thus no fundamental epistemological difference between logic and mathematics, on the one hand, and all of our other beliefs, on the other. Logical and mathematical beliefs can be given up, on Quine's view, it's just that it would take a massive amount of pressure from the experiential periphery of the web in order for us to be *rational* in doing so.

This epistemological picture is widely taken to have undermined the traditional motivation for positing an analytic/synthetic distinction. For the reason that Frege, Carnap, and others posited analyticity is that they thought it provided a respectable way of accounting for the apriority and necessity of logic and mathematics. As Harman says, “the ultimate defence of the full-blooded theory of analytic truth rests on the claim that some truths are either necessarily true or knowable *a priori*” (1999, p. 121). The idea, recall, was that if our *a priori* justification of mathematics and logic could be explained by the relevant terms/concepts being analytically related to one another, then there would be no need for what many regard as dubious rationalistic explanations, which posit a faculty of the mind (“rational insight” or “rational intuition”) that somehow gives us direct—non-experiential—access to necessary truths. Appealing to analyticities provides a much more mundane explanation: if, say, ‘ $2 + 2 = 4$ ’ were analytic, then if you understood its meaning, you couldn’t fail to see that it’s true. Given this conception of

analyticity and the work it is meant to do, it might seem that if Quine is right that no beliefs are immune to revision, then we no longer have a reason for believing that there are any such analyticities. For analyticities, it's often said, are supposed to have just such immunity.

Now, before discussing this line of reasoning any further, I should note that the direction of Quine's argument in "Two Dogmas" is not entirely clear. In the paper itself, Quine's arguments against the analytic/synthetic distinction come *before* his rejection of localism about confirmation. Given this, it's natural to read Quine as arguing *from* a rejection of the analytic/synthetic distinction *to* confirmation holism. Fodor reads Quine that way:

Quine rejects the Positivist account of confirmation because it assumes that there are 'local' semantic connections (between 'data sentences' and 'theory sentences'). He rejects local semantic connections because they would imply that there are urevisable statements. And he rejects the claim that there are unrevisable statements because it is false to scientific practice. In short, Quine's tactic is to infer Confirmation Holism from the refutation of semantic localism, and *not the other way round*. (1987, p. 65, original emphasis)

I'm not so sure about this. If Quine denies that there are analytic statements (or 'local semantic connections') because they imply that there are unrevisable statements, and he rejects the latter because scientists typically reason holistically, then hasn't Quine argued *from* confirmation holism *to* a rejection of the analytic?³ In any case, regardless of how Quine takes the direction of the argument to go, it's clear that there's *an* argument from

³ In *Holism: A Shopper's Guide*, it appears that Fodor has changed his mind on this issue: "[W]hat underlies Quine's rejection of analyticity [is] a recognition of the holistic, nonlocal character of empirical inference" (Fodor and Lepore 1992, p. 183).

confirmation holism to a denial of the analytic/synthetic distinction. Quine himself says that “the two dogmas are, indeed, at root identical” (1953a, p. 41).

Is this epistemological argument against the analytic convincing? In an important early discussion, Putnam (1975b) agrees with Quine’s confirmation holism, but says that if you deny the existence of the analytic/synthetic distinction, “you will not be wrong in connection with any philosophical issues not having to do specifically with the distinction” (p. 36). It’s only when philosophers “attempt to use [the distinction] as a weapon in philosophical discussion” (p. 36) that they go wrong. Putnam assumes, then, that confirmation holism is *consistent* with the existence of an analytic/synthetic distinction. More recently, a number of philosophers have pointed out the shortcomings of Quine’s epistemological arguments against the existence of analyticities (and the *a priori*) on just these grounds (BonJour 1998, Horwich 1992, Rey 1993, 1998a, Sober 2000b). It’s true that scientists (and the rest of us) do, and ought, to consider how accepting a belief affects the rest of our beliefs—our Quinean web—in terms of coherence, simplicity, and the like. But this fact alone doesn’t rule out the analytic. For it may be that upon reflection on the totality of the web, it will turn out that it is in fact rational, *by Quine’s own empiricist standards*, to accept the existence of the analytic and the *a priori*.

For our purposes, the crucial point to notice here is that contemporary defenders of the analytic needn’t be committed to the traditional (positivistic) motivations mentioned above. For it may be that we ought to believe in analyticity, *not* because it affords a nice way of accounting for our apparent *a priori* knowledge of logic and mathematics, but rather because our best empirical theory of the world requires it, e.g.,

our best linguistic theory. Similarly, it may be that our belief in the *a priori* is not *itself a priori*, but rather depends upon our best empirical theory of the abstract structure of the mind (Rey 1998a, 2001). That there's an analytic/synthetic distinction and *a priori* knowledge, on these sorts of views, are empirical facts and thus ones that a good Quinean ought to accept. Of course, *defending* the analytic and the *a priori* on empirical grounds requires work, and I want to stress that it's not my aim to carry out such work. My point is simple and modest: Regardless of whether these empirical defenses of the analytic and the *a priori* succeed, it's clear that Quine's epistemology *alone* does not rule them out.⁴

Given that Quine's arguments against the analytic/synthetic distinction are apparently inadequate, it's worth pursuing the question of exactly *which* arguments are supposed to rule out it out. In Chapter 4, I argued against Peacocke's attempt to answer the Quinean challenge by appeal to specific examples. But, one might wonder, if Quine's own arguments against the analytic/synthetic distinction aren't altogether convincing, then why should Peacocke and other Concept Pragmatists care about committing themselves to it? After all, if we regard Quine's assault on the distinction as presenting us with a puzzle and not a refutation, as I suggested above, then it's no objection to Peacocke or anyone else to point out that a non-holistic Pragmatism runs afoul of "what Quine showed".

⁴ As Georges Rey has emphasized to me, many philosophers bought into Quine's confirmation holism because it's very plausible when understood as the claim that "our beliefs confront the tribunal of experience as a corporate body". What people apparently failed to notice, however, is that in arguing against the analytic and the *a priori* Quine adds an 'only' to this formulation: "our statements about the external world face the tribunal of sense experience not individually but *only* as a corporate body" (1953a, p. 41, emphasis added). But you can't get to *that* claim by pointing out that scientists and the rest of us reason holistically.

Fodor's Arguments Against Lexical Decomposition, Definitions, etc.

Although Fodor often relies on Quine, in some moods he'll admit that Quine's arguments are not decisive. For instance, at one point he says:

It is often, and rightly, said that Quine didn't prove that you can't make sense of analyticity, definition, and the like.

Then he adds:

But so what? Cognitive science doesn't do proofs; it does empirical, non-demonstrative inferences. (1998a, p. 46)

It is these non-demonstrative inferences that thus deserve our attention. For if there's a strong but non-demonstrative argument to the effect that there's no analytic/synthetic distinction—and not just that we don't know how to draw it in a principled way—then Peacocke and other Concept Pragmatists are presumably in trouble.

There are a number of strands to Fodor's empirical, non-demonstrative argument against the analytic/synthetic distinction, but foremost among them are his arguments against the lexical semantics tradition in linguistics. My aim in this section and the next is to spell out in detail the structure of these arguments, and to show why they're ineffective as arguments against Concept Pragmatism.

To get a grip on these arguments, let's begin by supposing, with Fodor and many others (including his opponents, e.g. Jackendoff 1992, 2002), that there's a close parallel between the lexical and conceptual systems of the mind. I use the vague phrase "close parallel" since the systems can come apart. Most obviously, non-linguistic animals might possess concepts. Less obviously, there can be differences between the number of lexical items that languages attach to a single conceptual domain: English has one verb for the

‘putting on clothes’ domain, whereas Japanese has at least four distinct verbs for different aspects of that domain (one verb for putting on headgear, a different verb for putting clothes on lower parts of the body, etc.). English speakers can obviously make a *conceptual* distinction between these different aspects, even though such distinctions do not have lexical counterparts (Clark 1983, Keil 1989).

In what follows, I’ll put to one side concerns about whether concepts can be *identified* with word meanings. Our concern is with linguistic humans, and the fact that the lexical and conceptual systems do not always coincide will not affect the arguments that follow. I’ll thus simply assume for the nonce that *words express concepts*: the lexical item ‘red’ expresses the lexical concept RED, the phrase ‘fast runner’ express the phrasal concept FAST RUNNER, and so on. On this assumption, the distinction between primitive and defined lexical items suggests a corresponding distinction between complex and primitive concepts (Fodor *et al.* 1980; Fodor 1981b).

According to the lexical semantics tradition, many items in the lexicon exhibit structure ‘at the semantic level’. Two classic examples of lexical decomposition are ‘bachelor’, which putatively gets decomposed into ‘unmarried adult male’ (Katz and Fodor 1963), and ‘kill’, which putatively gets decomposed into ‘cause to die’ (McCawley 1968). Although on the surface the lexical items ‘bachelor’ and ‘kill’ appear to be primitive, the claim is that at the semantic level they’re in fact structurally complex. Given the assumption above, this implies a corresponding claim about the structure of the concepts BACHELOR and KILL. Although they appear on the surface to be primitive, they’re in fact structurally complex: BACHELOR has the structure exhibited by ‘UNMARRIED ADULT MALE’, and KILL has the structure exhibited by ‘CAUSE TO DIE’.

Part of what motivates the lexical semanticist is the desire to reduce the number of primitives posited in the lexicon. For it's plausible that what's primitive must somehow be encoded in the genome. As Jackendoff says:

Nearly everyone thinks that learning *anything* consists of constructing it from previously known parts, using previously known means of combination. If we trace the learning process back and ask where the previously known parts came from, and *their* previously known parts came from, eventually we have to arrive at a point where the most basic parts are *not* learned: they are given to the learner genetically, by virtue of the character of brain development. (2002, p. 334, original emphasis)

If both lexical items and the corresponding lexical concepts are structurally complex, then we can at least imagine a story about how learning them takes place. Learning the concept BACHELOR might consist in constructing (somehow) the concept from its constituents, UNMARRIED, ADULT, MALE. If BACHELOR lacked internal structure, and thus didn't *have* any constituents, then there obviously wouldn't be any way of constructing the concept *from* its constituents. Lexical decomposition is thus seen as a strategy for reducing the number of primitive lexical items, which is desirable because primitives are *ipso facto* unlearned.

It's worth pointing out that while empiricists assume that the primitives are sensory concepts, and that the rest of our concepts are constructed from them via association or some other general-purpose learning mechanism (Cowie 1999, Prinz 2002), modern lexical semanticists rightly (in my view) don't commit themselves to any such claims. Rather, they simply assume that there are *very few* lexical items that are *not*

decomposable, and deal with the issue of primitives on a case by case basis (Jackendoff 2002, ch.11).⁵

Part of what leads Fodor to his Atomism is that he thinks that lexical items do not have decompositions, and thus that the corresponding concepts do not have structure. Fodor assumes that lexical decompositions are definitions, i.e., that decomposing the meaning of a lexical item is tantamount to providing its definition. And since he doesn't think that there *are* any definitions—or if there are, there are very few—he denies that there are any lexical decompositions and that the corresponding lexical concepts have internal structure.

To get a feel for the argument, it's worth going through an example of a putative decomposition, and to have a look at why Fodor claims it fails. In his (1981b), Fodor considers a proposal of George Miller (1978), who makes the following claim: "When nouns of Type *M* are used as verbs, the meaning of *x Ms y* is to be construed as 'x covers the surface of y with *M*'" (p. 104; quoted in Fodor 1981b, p. 285). Such verbs include 'paint', 'butter', 'color', 'dye', and 'grease'. Using 'paint' as our example (as Miller and Fodor do), the proposal is that 'x paints y' is to be decomposed into 'x covers the surface of y with paint'. The transitive verb 'paint_{tr}' is thus given an analysis in terms of the noun 'paint_n', along with 'cover', 'surface', and 'with'. On the conceptual side, the proposal is that the concept X PAINTS_{tr} Y has the structure exhibited by 'X COVERS THE SURFACE OF Y WITH PAINT_n'.

Under the assumption that decompositions are definitions, Fodor argues that this analysis fails. For covering the surface of something with paint is necessary, but not

⁵ Fodor's Concept Atomism famously commits him to an incredibly large number of unlearned concepts (Fodor 1975, 1981b). See Fodor (1998a) for some qualifications.

sufficient for painting the surface. Consider a paint factory that explodes and covers some spectators with paint. As Fodor says, “this may be good fun, but it is not a case of the paint factory (or the explosion) painting the spectators” (1981b, p. 286). The reason for this seems obvious: painting is an activity carried out by agents of some kind, and factories aren’t agents. But now consider the improved analysis of ‘ x paints_{tr} y ’ as ‘ x is an agent and x covers the surface of y with paint_n’. Fodor points out that this doesn’t work either, for an agent who knocks over a bucket and covers the floor with paint does not thereby paint the floor. Painting is an *intentional* action, but covering something with paint need not be. Perhaps, then, ‘ x paints_{tr} y ’ ought to be analyzed as ‘ x is an agent and x *intentionally* covers the surface of y with paint_n’. But again, Fodor says, this won’t work:

It’s got to be that when you cover the y with paint, what you primarily have in mind to do (the description, as it were, under which you intend your act) is that y should be covered with paint in consequence of your activity. If, like Michelangelo, what you primarily have in mind is not (just) that the surface should become covered with paint in consequence of your activity, but that there should be a picture on the surface in consequence of your activity, then what you’re doing when you cover the surface with paint doesn’t count as painting the surface. (1981b, p. 287)

So, while Michelangelo intentionally covered the ceiling with paint, what he *primarily* intended to do was paint a picture on the ceiling. Intentionally covering a surface with paint, Fodor claims, is thus *not* sufficient for painting the surface.

Perhaps a better analysis of ‘x paints_{tr} y’ is ‘x is an agent and x intentionally covers the surface of y with paint_n and x’s primary intention in covering the surface of y with paint_n is that the surface of y should be covered with paint_n in consequence of x’s having so acted upon it’. However, when Michelangelo dips his brush into the paint, he is intentionally covering the surface of the brush with paint, and it is his primary intention that the surface of the brush be covered in paint as a result of his dipping it in the paint. But, Fodor claims, surely Michelangelo is not *painting* his brush when he dips it in the paint. Rather, what he’s doing is putting paint *on* his brush. After considering and rejecting these proposed improvements on Miller’s account, Fodor concludes:

I don’t know where we go from here. For all I know—for all *anybody* knows—‘paints_{tr}’ is undefinable; for all I know, you can’t eliminate it even in terms of such a very closely related term as paints_n'. Or perhaps it *is* definable, but only in a reduction base that includes ‘dinasour’ and ‘Chlorodent’. Either way, the present point is that Miller’s examples don’t work. That’s not surprising; when it comes to definitions, the examples almost always don’t work. (1981b, p. 288, original emphasis)

Now, I don’t find Fodor’s argument here all that persuasive. For instance, in some contexts at least, it’s not at all clear that there’s something semantically wrong with ‘Michelangelo painted the ceiling of the Sistine Chapel’. Imagine the following context: you’re in the Sistine Chapel and someone asks “Remind me again, who painted the ceiling?” You reply: “Michelangelo painted the ceiling.” In this context, what you said seems perfectly acceptable and true. Imagine the look you’d get if you instead said: “Do you mean ‘Who painted *the picture* on the ceiling?’ Michelangelo did.” Or, take the

dipping of a brush into a bucket of paint. Perhaps in most contexts that doesn't count as a painting of the brush. But what about the artist who dips entire brushes into paint and uses them in a piece? The curator might say, describing how the piece came to be: "she painted the brushes and arranged them ...". Now, I don't think that any of this is decisive. But, at the very least, it suggests that Fodor moves through the putative counterexamples a bit too quickly.

Fodor has also offered arguments against other proposals of lexical semanticists. He argues against analyzing 'kill' as 'cause to die' (Fodor 1970); against Jackendoff's (1992) analysis of the polysemous verb 'keep' as 'cause a state that endures over time' (Fodor 1998a, pp. 49-56); against Pustejovsky's (1995) analysis of 'bake', 'begin' ('/finish'), and 'enjoy', as well as his more general arguments for lexical decomposition (Fodor and Lepore 2002, ch.5); against Pinker's (1984, 1989) arguments that positing lexical structure is a necessary component of a theory of how children acquire certain aspects of syntax (1998a, pp. 56-68); and against Hale and Keyser's (1993) arguments for analyzing 'denominal' verbs as phrases that contain the corresponding nouns (e.g. analyzing 'sing' as 'do a song', 'shelve' as 'put on a shelf', etc.) (Fodor and Lepore 2002, ch.6).

On top of all that, the experimental work of Fodor and others suggests that comprehending a sentence does not involve recovering the decompositions of the lexical items it contains, which suggests that such structures are not 'psychologically real' (Fodor *et al.* 1980). Indeed, Fodor has recently quipped: "[i]t's an iron law of cognitive science that, in experimental environments, definitions always behave exactly as though

they weren't there" (1998a, p. 46). This is all supposed to be part of the empirical, non-demonstrative case against analyticity.

Now, a lexical semanticist might point out that while Fodor has perhaps shown that lexical items can't be decomposed *definitionally*, he overlooks the possibility that there are, as Jackendoff puts it, "non-definitional forms of decomposition" (2002, p. 336). Indeed, one wonders exactly what Fodor takes himself to be doing when he's providing counterexamples and suggesting improvements to proposed analyses of 'paint'. It's surely implausible that he's exploiting an *empirical theory of painting* when he denies that Michelangelo paints his brush when he dips it into the paint. Could it really be a mere empirical fact about painting that dippings of brushes into paint are not paintings of brushes? This is apparently exactly what Fodor's Concept Atomism implies. For on his view, *even if* all paintings must in fact be a kind of covering of a surface with paint, this does *not* imply that 'paints_{tr}' must have some structure at the semantic level involving 'paint_n' and 'surface', or that the concept PAINTS_{tr} is constitutively connected to PAINTS_n, SURFACE, etc. Rather, it merely implies that the property of *being a painting_{tr}* is metaphysically connected to the properties of *being paint, being a surface*, etc.

However, Fodor's own methodology does not sit happily with this position. For in arguing against lexical decompositions and definitions, Fodor himself seems to be engaged in just the sort of traditional, conceptual analysis that he's attempting to undermine. One might argue that what he's actually doing when he's proposing and rejecting putative analyses of 'paints_{tr}' is exploiting the analytic rules that govern his use of 'paints_{tr}'. And even if we *generously* grant that Fodor picks a good point at which to stop the analysis ("I don't know where we go from here..."), it seems to me that his

suggestion that PAINTS_{tr} bears no constitutive relations to other concepts is entirely unwarranted. For, by his own admission, he's provided some *necessary conditions* on the application of PAINTS_{tr}. So even if PAINTS_{tr} lacks a full-fledged *analysis* (i.e., necessary and sufficient application conditions), there at least appear to be some semantically interesting connections it bears to other concepts. Perhaps such relations are what Jackendoff is suggesting with his “non-definitional forms of decomposition”.

How is Fodor's relentless attack on lexical decomposition and definitions related to his attack on Peacocke and other Concept Pragmatists? Here's how I think Fodor sees the connection. As noted above, the claim that lexical items have structure ‘at the semantic level’ implies a corresponding claim about the structure of lexical concepts. If ‘bachelor’ and ‘kill’ have structure at the semantic level, then BACHELOR and KILL have internal structure that mirrors the lexical decomposition. On this view, lexical semanticists are engaged in conceptual analysis of the concepts corresponding to the putatively decomposable lexical items.⁶ Fodor's claim, then, is that arguments against lexical decompositions are also arguments against the claim that lexical concepts have internal structure that can be illuminated by philosophical, or conceptual analysis.

If lexical semanticists are committed to lexical concepts with internal structure, then, the line of thought continues, Concept Pragmatists are committed to lexical items having structure at the semantic level. A claim about the semantic structure of lexical items implies a claim about the conceptual structure of lexical concepts, *and vice versa*.

As Fodor and Lepore put it:

⁶ See Katz (1988, chapter 10) for a defense of the claim that lexical semanticists are effectively engaged in Moore-style conceptual analysis.

[Concept Pragmatism] constrains grammatical theories since, on standard versions of the view, the semantic lexicon of a language is supposed to be the component of a grammar that makes explicit whatever one has to learn/know to understand the lexical expressions of the language. [It] thus implies that meaning-constitutive inferences are part of the semantic lexical entries for items that have them. Lexical entries are therefore typically complex objects ('bundles of inferences') according to standard interpretations of [Concept Pragmatism].

(2002, p. 90)

Given this, Pragmatists are committed to the claim that lexical entries specify meaning-constitutive conceptual/inferential roles, which implies that lexical concepts also have internal structure that specifies the conceptual/inferential roles that are concept-constitutive.

In order to illustrate what Fodor sees as the contrast between him and the typical Concept Pragmatist, consider the following conceptually necessary inference about arctic foxes: WHITE VIXEN → WHITE.⁷ This inference ought to count as analytic *on any account*. Since the concept WHITE is literally a part of the complex concept WHITE VIXEN, then the inference will be guaranteed by the structure of the concept alone. The only way such an inference could *fail* to be analytic is if conceptual structure failed to be compositional. For if the content of WHITE VIXEN is determined by the content of WHITE, the content of VIXEN, and the way the concepts are put together (i.e., 'AN' construction), then the inference WHITE VIXEN → WHITE will hold in virtue of this fact alone. In other

⁷ Fodor and Lepore's favorite example is BROWN COW → BROWN, which they contrast with BROWN COW → ANIMAL, arguing that only the former is analytic (Fodor and Lepore 2002). I prefer to contrast WHITE VIXEN → WHITE and WHITE VIXEN → FEMALE, for the simple reason that 'vixens are female' is a better candidate for being analytic than 'cows are animals', as Putnam's (1970, 1975a) radio-controlled robot cats example suggests.

words, compositionality alone underwrites certain analyticities, and since compositionality is a non-negotiable constraint on a theory of concepts (see Chapter 1), then *any* account of concepts must allow for the existence of *some* analytic inferences.

As Fodor and Lepore (2002, pp. 20-22) point out, though, it's possible that compositionality underwrites *only* those analytic inferences that hold in virtue of the relation between complex concepts and their syntactic constituents. On this view, which Fodor and Lepore endorse, compositionality entails an analytic/synthetic distinction that can distinguish WHITE VIXEN → WHITE from WHITE VIXEN → FAST, but *not* WHITE VIXEN → FAST from WHITE VIXEN → FEMALE. If neither WHITE VIXEN → FAST nor WHITE VIXEN → FEMALE turns on the compositional structure of WHITE VIXEN, the thought goes, then neither is an analytic inference.

Of course, given Fodor's assumptions, if lexical semanticists and Concept Pragmatists are right, then lexical concepts *themselves* have compositional structure. If, say, 'vixen' is lexically decomposed into a structure that contains 'female', and VIXEN has internal structure that guarantees the inference from VIXEN → FEMALE, then compositionality *does* entail an analytic/synthetic distinction that distinguishes VIXEN → FEMALE and VIXEN → FAST. (This assumes, of course, that 'female', but not 'fast', is part of the decompositional structure of 'vixen', and VIXEN → FEMALE but not VIXEN → FAST is guaranteed by the structure of VIXEN). Lexical semanticists and Concept Pragmatists thus effectively assimilate inferences like WHITE VIXEN → FEMALE to inferences like WHITE VIXEN → WHITE. On their view, both are conceptually necessary, analytic inferences, which are underwritten by the compositional structure of concepts.

Fodor will turn this argument on its head, and claim that *since* there is no lexical decomposition, and *since* lexical concepts do not have internal structure, compositionality does *not* underwrite an analytic/synthetic distinction that can distinguish WHITE VIXEN → FEMALE and WHITE VIXEN → FAST. On his view, “the only thing a lexical entry specifies is the denotation for the item it describes” (Fodor and Lepore 2002, p. 90). Since lexical items do not have decompositions, and lexical concepts do not have structure, then compositionality only entails analyticities that hold between complex concepts and their syntactic constituents. Notice, though, that the way that Fodor sets out the terrain here *presupposes* that the structure of lexical concepts is what underwrites concept-constitutive inferences. In other words, Fodor’s (and Lepore’s) claim that the arguments against lexical decomposition are also arguments against Concept Pragmatism presupposes that both lexical semanticists and Pragmatists are committed to internally structured lexical concepts.

Why Fodor’s Arguments Fail.

I’ve already suggested some reasons to be skeptical of Fodor’s arguments. But there’s a more fundamental reason for skepticism. As we’ve just seen, Fodor assumes that lexical decompositions are definitions, and that providing a definition of a lexical item is tantamount to providing an analysis of the corresponding lexical concept, which in turn requires that lexical concepts have internal structure. This assumption underlies Fodor’s claim that arguments against lexical semantics are also arguments against Concept Pragmatism. But if Fodor’s attack relies on this assumption, then Pragmatists are not in fact vulnerable to it. Or so I will argue. In particular, I argue that Peacocke and other

Concept Pragmatists need not commit themselves to the claim that lexical concepts have internal structure, and that the above line of reasoning is thus insufficient to support the claim that their analyses suffer the same fate as those of lexical semanticists.

Let's begin with the observation that in stating the possession condition for certain concepts, Peacocke certainly doesn't seem to be providing definitions. Rather, he appears to be stating conditions that must obtain in order for thinkers to *possess* certain concepts, and, *prima facie*, these need not be taken as definitional claims about the structure of the concepts themselves. This is especially plausible in the case of recognitional concepts. Possessing such concepts requires that under optimal judging conditions, thinkers recognize instances of the concepts as such. The concept DESK, for instance, is supposed to be individuated in this fashion. But to say that such conditions *individuate* the concept DESK is not to say that such conditions *define* DESK, or that the lexical item 'desk' has definitional structure at the semantic level. For presumably, a definition of 'desk' would look something like the following: 'x is a desk iff x is an object with a flat surface used for writing, etc.'. Such a definition, however it's ultimately spelled out, will apparently make no mention of the sorts of recognitional capacities that Peacocke and other Concept Pragmatists invoke in stating the possession conditions for DESK.

Of course, Fodor and Quine will deny that we can provide any such definition of 'desk'. Fair enough. But it doesn't beg any questions to point out that if there *were* a definition of 'desk' it *wouldn't* have on the right-hand side of the biconditional any of the specifications that Peacocke mentions in stating the possession conditions for DESK. Indeed, Peacocke's claims about the possession conditions for recognitional concepts

seem *entirely independent* of issues concerning their lexical decomposition, as well as those concerning whether lexical concepts have internal structure, definitional or otherwise.

There's thus apparently no reason to think that if DESK is constitutively tied to perceptual judgment in the way Peacocke suggests, then it must have internal structure that specifies that it is so tied. In fact, there's no reason why Peacocke must deny that DESK lacks structure altogether, as the Concept Atomist claims, merely because he is committed to DESK's identity being tied to a thinker's making certain perceptual judgments containing it in certain circumstances. If this is right, then Fodor's arguments against lexical decomposition and definitions do not apply to proposals about how perceptual judgments figure in the possession conditions for certain concepts.

This issue, it seems to me, may be obscured by the fact that the methodology of some Concept Pragmatists—including Peacocke—falls squarely in the tradition of *a priori*, philosophical or conceptual analysis. For it's often supposed that philosophers who work in this tradition are implicitly committed to a certain view about the nature of concepts that, perhaps unbeknownst to them, the arguments of Quine and others have undermined. The idea is that if there's to be such a thing as philosophical or conceptual analysis, then concepts must *have* structure, which is, as it were, *there to be analyzed*. On this sort of view, Fodor's complaints about lexical semantics, definitions, and the like would have some force against Peacocke's proposals, since Peacocke's possession conditions would be taken to illuminate conceptual structure. However, *contra* the above view, a methodology of philosophical, or conceptual analysis does not necessarily

presuppose that lexical items have structure at the semantic level, or that lexical concepts are internally structured.

In fact, if one looks at recent attempts by philosophers to defend conceptual analysis—e.g., Frank Jackson’s (1998) *From Metaphysics to Ethics* and George Bealer’s (1987) “The Philosophical Limits of Scientific Essentialism”—one will not find defenses of the claim that lexical concepts have internal structure, or claims that such projects depend on such a defense. Rather, what you’ll find is a defense of the claim that conceptual analysis is a crucial component of doing ‘serious metaphysics’ (Jackson 1998), or that philosophy is an ‘autonomous’ discipline, which ‘empirical science cannot eclipse’ (Bealer 1987). To the best of my knowledge, prominent defenders of conceptual analysis do not defend the claim that lexical concepts are internally structured.

We’ve seen that Peacocke views the task of providing a theory of concepts in much the way that Jackson and Bealer view philosophy in general. Indeed, Peacocke claims not only that empirical psychology will not trump philosophical analyses of concepts, but that the empirical investigation of a concept *cannot even begin* until the philosopher provides their possession conditions (!): “For any particular concept, the task for the psychologist is not fully formulated until the philosopher has supplied an adequate possession condition for it” (Peacocke, 1992a, p. 190). Peacocke’s aim is to provide some of these, by working out in an *a priori* fashion the conditions under which thinkers can be said to possess certain concepts.

But, again, it’s not clear that a methodology of this kind presupposes that lexical concepts have internal structure. It seems to be an underappreciated possibility that such a methodology could illuminate necessary conditions on the possession of certain

concepts *without* those conditions reflecting the structure of the concepts themselves. Of course, pointing out that *a priori*, conceptual analysis is *consistent* with Concept Atomism does not tell us what sort of activity *a priori*, conceptual analysis *is*. If philosophers who engage in such an activity are not illuminating the structure of concepts, then what *are* they doing? How *can* one arrive at the possession conditions for a lexical concept *unless* those conditions illuminate its structure? These are interesting, and difficult questions. One possibility that needs to be taken seriously is that conceptual analysis is a way of accessing the analytic rules that govern our conceptual competence, where these rules do *not* hold in virtue of structural relations among concepts. The point I'm making is that the claim that such a methodology of analysis *does* presuppose that lexical concepts are internally structured is at least in need of an argument. And, as far as I can tell, neither Fodor nor anyone else has provided one.

One source of difficulty here is that it's not clear how to understand claims about conceptual structure. In the literature, debates abound as to the structure of certain concepts—e.g., in philosophy there are long-standing debates about FREEDOM, MORAL RESPONSIBILITY, TRUTH, JUSTICE, EXPLANATION, CAUSATION, GOOD, and PERSON (among others), and in psychology there are debates about the structure of less (philosophically) interesting concepts such as EVEN (/ODD) NUMBER, APPLE, FEMALE, FRUIT, and SPORT (among many others). But what it is for a concept to *have* internal structure? It might seem that we must first get straight about this question before we can make progress on the question of whether the claims of Peacocke and other Concept Pragmatists carry with them an implicit commitment to lexical concepts having internal structure.

In Fodor's work, he seems to presuppose that a concept is structurally complex just in case it *literally* has other concepts as constituents. The idea is clearest in the case of concepts corresponding to phrases of natural language, e.g. WHITE VIXEN. This concept is structurally complex since WHITE and VIXEN are quite literally *constituent parts of* WHITE VIXEN. It follows that a thinker can't think WHITE VIXEN without thinking WHITE and VIXEN, just as one can't inscribe the words 'white vixen' on a page without inscribing the words 'white' and 'vixen', or move a table without moving its constituents.

But what about lexical concepts? What would it be for them to have internal structure? Fodor's assumption is that if lexical concepts have structure, then it will be just like the structure that phrasal concepts have, except that lexical concepts will not, so to speak, wear their structure on their sleeves. For instance, if BACHELOR has definitional structure, then the assumption is that it literally has the concepts UNMARRIED, ADULT, and MALE as constituents. In other words, if the traditional analysis is correct, then 'UNMARRIED ADULT MALE' is a *structural description* of BACHELOR. This assumption has the consequence that the thought JOHN IS A BACHELOR is more complex than the thought JOHN IS UNMARRIED, since the structure of UNMARRIED (whatever it is) is literally a part of the structure of BACHELOR. As I noted above, the experiments of Fodor *et al.* (1980) and others suggest that this consequence is false. Indeed, Fodor says that this ought to be obvious to untutored intuition: "Does anybody present really think that thinking BACHELOR is harder than thinking UNMARRIED? Or that thinking FATHER is harder than thinking PARENT?" (1998a, p. 47). Of course, the force of the experimental evidence and this appeal to intuition against

lexical semantics and definitions depends upon the claim that if lexical concepts have structure, then they have other concepts as constituents.

This understanding of what conceptual structure consists in has a long, venerable history. It is, in effect, the way that Kant (1781/1998) thought of conceptual structure, with his “concept containment” model of analyticity, whereby if ‘As are Bs’ is analytic, then “the predicate B belongs to the subject A, as something which is (covertly) contained in this concept A” (A6-7). With phrasal concepts, the constituent structure is overt, since the names of such concepts are also patently their structural descriptions: ‘WHITE VIXEN’ serves as *both* a name *and* a structural description of WHITE VIXEN. With lexical concepts, as we noted above (and as Kant notes), the constituent structure is covert, and thus must be provided by a lexical decomposition, or conceptual analysis.

But understanding conceptual structure according to the “concept containment” model clearly will not help Fodor’s case. For again, suppose it’s a possession condition for RED that one be willing to judge, *that’s red*, if one has an experience of an object as red in optimal conditions, and that it’s a possession condition for DESK that one be willing to judge, *that’s a desk*, in optimal conditions. These conditions on concept possession do not *require* that thinkers possess concepts other than those for which they are conditions. Since possessing an internally structured lexical concept—in the containment sense of structure—would require possessing more than one concept, such possession conditions thus don’t require that the lexical concepts in question have internal structure.

The upshot of all of this is that Concept Pragmatists will be vulnerable to Fodor’s arguments against definitions and lexical decomposition, and the concept-containment

construal of conceptual structure such arguments depend on, only if they endorse the claim that lexical concepts are internally structured. But since it's open to Pragmatists to deny this claim, Fodor's arguments against lexical decompositions, definitions, and the like do not necessarily apply to them. In other words, the existence of analyticity and constitutive connections among concepts does *not* imply the existence of internally structured lexical concepts. Given this, arguments against the latter needn't count as arguments against the former.

Keeping Options Open

Of course, many Concept Pragmatists do believe that lexical concepts are internally structured. Perhaps this belief, when coupled with the belief in conceptual/inferential role semantics, constitutes a “Sort of Consensus about concepts in cognitive science” (Fodor, 2000b, p. 350). Even so, I take the above considerations to show that the question of whether or not lexical concepts have internal structure is *distinct* from the question of whether Concept Atomism or Concept Pragmatism is correct. Arguments against lexical decomposition, definitions, and the like will thus count as arguments *for* Atomism and *against* Pragmatism, I submit, *only* under the assumption that the conceptual/inferential role of a lexical concept is specified by its internal structure. Although I raised some doubts about Fodor's arguments against definitions and lexical decomposition, Concept Pragmatists can consistently accept these arguments, so long as they deny this assumption.

Putting (what seems to be) the same point slightly differently, the ‘informational’ and ‘atomistic’ components of Informational Atomism can come apart. That is, it doesn't

follow from the fact that lexical concepts are atoms that their content is determined in the covariant way that an informational semantics says it is. Conversely, that content is determined by informational relations does not entail that lexical concepts are atoms. For one might hold a view according to which concepts are structured entities of some kind (e.g., that they have prototype or stereotype structure), whose content is nevertheless determined by informational relations (Prinz 2002). Denying Atomism is thus perfectly compatible with accepting informational semantics. (Figure 2 illustrates these various options.)

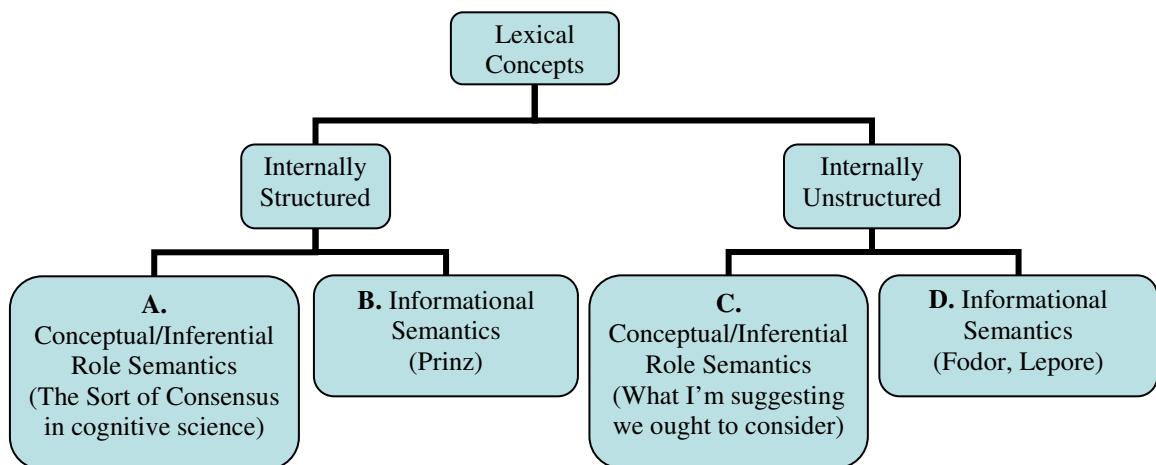


Figure 2. Four Live Options

It seems clear that Options A, B, C, and D are all open. The issue of whether lexical concepts are internally structured is, *prima facie*, *entirely independent* of the issue of whether informational or conceptual/inferential semantics is correct. However, if one reads Fodor on these issues, then one might get a different impression of the landscape. For he claims that the arguments in support of Atomism about lexical concepts *also*

support informational semantics. Indeed, he seems to think that the stand one takes on the question of whether concepts are internally structured *determines* one's stand on the question of how content is determined:

[W]hat settles the metaphysical issue between informational theories of meaning and inferential role theories of meaning is that the former, but not the latter, are compatible with an atomistic account of concepts. (Fodor, 1998a, p. 15)

Fodor is surely wrong about this. For, as the above discussion shows, a Concept Pragmatist can *accept* Fodor's case for Atomism by *denying* that the conceptual/inferential role of lexical concepts is specified by their structure. In other words, Option C is a live one.

Now, it's a bit odd having to point this out, since Fodor himself once favored a position very much like Option C. For in *The Language of Thought* he endorses 'meaning postulates' as a way of capturing meaning relations *without* postulating any lexical decomposition (Fodor 1975).⁸ Here's how Fodor introduces them:

If entailments that derive from terms in the 'nonlogical' vocabulary of a natural language do not depend on a process of definition, how are they determined? A standard proposal (since Carnap 1956) is that if we want F to entail G (where one or both are morphologically simple expressions of the object language) we should simply *say* that F entails G ; i.e., we should add $F \rightarrow G$ to the inference rules. Such nonstandard rules of inference have come to be called 'meaning postulates', so the present proposal is that it is meaning postulates that do the work that definitions have usually been supposed to do. (1975, p. 149, original emphasis)

⁸ See also J.D. Fodor, J.A. Fodor, and M.F. Garrett (1975).

There are number of benefits to this position. By replacing definitions with meaning postulates, one can respect the empirical data that suggests that understanding a sentence does not involve recovering the decompositions of the lexical items it contains. Moreover, one can respect the intuition that thinking BACHELOR is not harder than thinking UNMARRIED, and that entertaining the thought JOHN IS A BACHELOR needn't involve thinking UNMARRIED. All this could be the case even if the inference from BACHELOR to UNMARRIED MALE is analytic, i.e. even if it's an 'entailment that derives from the nonlogical vocabulary'. As Fodor puts it:

*[B]achelor gets into the internal language as an abbreviation for a complex expression of the internal language: viz., as an abbreviation for *unmarried man*.*

The abbreviatory convention is stored as a principle of the logic (i.e., as *bachelor* ↔ *unmarried man*). Since, in the course of learning English, 'bachelor' gets hooked onto *bachelor* and 'unmarried man' gets hooked onto *unmarried man*, *bachelor* ↔ *unmarried man* can be used to mediate such inferential relations as the one between 'x is a bachelor' and 'x is an unmarried man'. (1975, p. 152. original emphasis)

Positing meaning postulates also allows for a solution to the so-called 'residuum problem'. Consider what Option A has to say about RED. In order to capture the inference from RED to COLORED, it supposes that the latter is one of the constituents of the former. But what are the *other* constituents of RED? It's hard to see that there could be any. As Janet Fodor says:

*[T]o say that something is red is to say more than that it is colored. So now at least one other component of the meaning of *red* must be identified. Let us*

assume, just for convenience, that there is only one, i.e., that no further analysis is possible. For this component to be independent of the coloredness component, it must consist of the concept of redness-but-not-necessarily-coloredness. But there surely is no such concept (or even percept). There is no property R which does not itself contain the property of being colored, such that being red = being colored + R . (1977, p. 150)

The concept RED is thus *unlike* the concept VIXEN, or BACHELOR on this score. A fox is exactly like a vixen *except* that it is not necessarily female, and a male is exactly like a bachelor *except* not necessarily unmarried. So, the property of *being a female* is that property R such that *being a vixen* = *being a fox* + R ; and the property of being unmarried is that property R such that *being a bachelor* = *being a male* + R . In other words, when you “subtract” ‘FEMALE’ from the definition of VIXEN what you have left is ‘FOX’, and when you subtract ‘UNMARRIED’ from the definition of BACHELOR what you have left is ‘MALE’. But if you subtract ‘COLORED’ from the definition of RED, it doesn’t seem that you’re left with *anything*. Or, as (Jerry) Fodor puts it, “it looks like the only thing that could combine with ‘COLOURED’ to mean *red* is ‘RED’” (1998a, p. 109).

Meaning postulates (Option C) provide a nice solution to this problem. For they can accommodate constitutive inferences that do *not* stem from definitions. That the inference from RED to COLORED is constitutive of RED, on this treatment, does not imply that RED has decompositional, or definitional structure. The inference can hold in virtue of an inference rule, or meaning postulate, which effectively specify a one-way entailment relation that holds between RED and COLORED. Given the evidence against definitions and lexical decompositions, one could then extend this account to cover those

inferences that were traditionally thought to stem from definitional structure. For even the best examples of inferences arising out of definitional structure are arguably faulty in some way.⁹ Rather than insisting on entailments that hold in *both* directions, one could thus give up on definitions and opt instead for meaning postulates that specify one-way entailment relations, i.e. necessary but *not* sufficient conditions.

It bears emphasis that this approach will have both a simplifying and complicating effect on one's overall theory. It complicates things because it greatly increases the number of inference types in the logic. For an inference rule is needed for every analytic inference connecting "non-logical" concepts. Rather than inflating the logic in this way, some philosophers (e.g., Jerrold Katz (1988)) claim that it's better to take analytic inferences to hold in virtue of complex lexical structure. But this is where the simplifying benefits of meaning-postulates come in. For positing complex lexical structure makes it much more difficult to understand sentence comprehension, and in particular how people do it so *fast*. As Fodor says:

[W]e mitigate the mystery [of sentence understanding] insofar as we assume a 'shallow' theory of messages, since the more structural similarity there is between what gets uttered and its internal representation, the less computing the sentence understander will have to do. The interest of meaning postulates is that they provide a general procedure for complicating the logic in ways that reduce the strain on sentence comprehension. That is, they let us do what psychological

⁹ Bachelors aren't just *any* unmarried males. For five-year old boys aren't bachelors. But bachelors aren't just any unmarried *adult* males either. For the Pope isn't a bachelor. And so on, and so forth. Note that even if BACHELOR *can* ultimately be given a successful definition, it's doubtful that very many of our concepts can.

theories need to do: simplify the representation of computations that must be carried out on-line. (1975, p. 152)

Fodor, Fodor, and Garrett echo this line of argument:

[B]arring decisive evidence to the contrary, we should assume that the semantic representation of a sentence is as much like the surface form of the sentence as we can. For, in doing so, we reduce the load on the processes that must be assumed to be performed on-line. In particular, then, given a choice between assigning a process to the comprehension system and assigning it to the inferential system, all other things being equal we should choose the latter option. That is precisely what hypothesizing meaning postulates in place of eliminative definitions permits us to do. (1975, p. 526)

The cost of meaning postulates, this line of reasoning suggests, is worth their theoretical payoff.

Given that Fodor himself once endorsed something like Option C above, it's thus strange to find him claiming that what "settles" the debate between informational semantics and a conceptual/inferential role view is that only the former are compatible with Atomism. For the considerations that lead to Atomism are not only perfectly compatible with Option C, but can also be used to *motivate* it. In any case, Fodor devotes an appendix of *Concepts* to meaning postulates (1998a, pp. 108-112), in which he offers an argument against them that's *independent* of the issues concerning Atomism.¹⁰

Whereas he once claimed that the theoretical benefits of meaning postulates are worth

¹⁰ If he really believed that Atomism "settles" this issue, then wouldn't such an appendix be entirely unnecessary?

their cost, he now seems to think that there are additional theoretical costs, which apparently went unnoticed in 1975 and that outweigh the benefits.

The most important difference between Option C (meaning postulates) and Option A (the Sort of Consensus) is that the latter, but not the former, takes analytic inferences that link lexical concepts to hold in virtue of their internal structure. Suppose the inference from C to C1 is a constitutive, analytic inference. According to Option A, this inference holds because C is internally structured and has C1 as a constituent. Option C claims no such thing, since it says that the inference is guaranteed by a meaning postulate, or inference rule, that links C and C1. On this latter view, there's a one-way entailment relation that holds between C and C1, which is consistent with their being primitives.

So far, so good. But a problem arises for Option C, Fodor claims, when we consider what it must say about the relation between concept *constitution* and concept *possession*. Note that on *any* kind of conceptual/inferential role view, the inferences and judgments that *constitute* a concept C must be coextensive with the ones that a thinker must accept in order to *possess* C. This constraint doesn't have anything in particular to do with concepts. Rather, it's simply a consequence of the nature of the constitution and possession relations, or (if you prefer) the meanings of 'constitution' and 'possession'. If one entity *a* (partly) constitutes another entity *b*, then you can't possess the latter without possessing the former. You can't possess this particular dollar bill, for instance, without possessing its constituents.¹¹ This holds for constitutive properties as well. You can't

¹¹ I'm ignoring delicate metaphysical issues here. Strictly speaking, you *can* possess a dollar bill without possessing its constituents, since a dollar bill can survive the loss of some of its constituents. There are thus counterfactual scenarios in which you possess the dollar bill even though it's constituted by a different set of constituents. Such issues concerning artifact identity will not affect the point in the text.

possess the dollar bill without possessing something that has its constitutive properties. If one of its constitutive properties is that it was produced by the Federal Bureau of Engraving and Printing, then you can't possess the dollar bill without possessing something that was made by the Federal Bureau of Engraving and Printing.

The relation that must hold between concept constitution and concept possession is just an instance of this more general truth: the entities and properties that are constitutive of a thing must be mentioned in the possession conditions for that thing. On Option A, this relation is guaranteed by the fact that constitutive inferences relate a concept to its constituents. If the inference from C to C1 is constitutive in virtue of the latter being a constituent of the former, then it *immediately* follows that a thinker can't possess C without possessing C1. On this view, the relation that C bears to C1 is analogous to the relation that a dollar bill bears to its constituents. Given what it is to be a constituent, you can't possess a concept (or a dollar bill) without possessing its constituents. But, Fodor claims, if we adopt Option C then there is no such guarantee, which is an unhappy theoretical cost: “[meaning postulates] weaken the architecture of your overall theory [by breaking] the connection between the structure of a concept and its possession conditions” (1998a, p. 111).

In my view, this is unconvincing. It's true that if certain properties are concept-constitutive, then a thinker must satisfy (/possess) those properties in order to possess the concept. However, we must sharply distinguish between what's constitutive of a concept and what holds in virtue of conceptual structure. Fodor slides from talk about the relation between *concept constitution* and concept possession to talk about the relation between *conceptual structure* and concept possession. But there's an obvious distinction between

concept constitution and conceptual structure. For, as we've seen, properties that have nothing to do with conceptual structure might nevertheless be concept-constitutive.

Again, this point doesn't have anything in particular to do with concepts. A dollar bill's being made by the Federal Bureau of Engraving and Printing may be one of its constitutive properties, even though (*prima facie*) this is *not* a property that relates the dollar bill to its constituents. Just so, certain relational properties might be constitutive of a concept even though they don't hold between the concept and its constituents. This is, in effect, precisely what Option C claims: certain inferences are concept-constitutive even though they don't hold in virtue of conceptual structure.

There's thus no reason to think that positing concept-constitutive inferences that hold in virtue of inference rules, or meaning postulates, in any way 'weakens the architecture of your overall theory'. If it did, then my guess is that Fodor's own theory would have an architecture that is *just* as weak. To see this, note that Fodor's Informational Atomism *also* posits the existence of analyticities that do not turn on conceptual structure, just as Option C does. For it falls out of Informational Atomism that it's analytic that, for example, Mark Twain is Sam Clemens, water is H₂O, and Hesperus is Phosphorus. The reason for this is as follows: the content of MARK TWAIN is Mark Twain, and the content of SAM CLEMENS is Sam Clemens; 'Mark Twain is Sam Clemens' is thus conceptually necessary in the sense that it's guaranteed by the content of MARK TWAIN and SAM CLEMENS, together with the fact that Twain *is* Clemens. (The same goes for 'water is H₂O' and 'Hesperus is Phosphorus'.) Of course, we can't know *a priori* that Twain is Clemens (or that water is H₂O, or that Hesperus is Phosphorus), and it's surely a virtue of Informational Atomism that it distinguishes between what's analytic

and what's *a priori* in this way. However, the present point is that Fodor's architectural point against Option C would, if sound, apparently hold against his own view as well, since it too posits analyticities that don't hold in virtue of conceptual structure.¹²

In any case, I don't think that the architectural point holds against Option C. Meaning postulates constrain theories of concept possession just as decompositional theories do, they just do so in a different manner. If the inference from C to C1 is concept-constitutive and holds in virtue of a meaning postulate or inference rule, then in order to possess C, thinkers must be willing to make the inference from C to C1. And, just as you can't possess C without possessing C1 if the latter is a constituent of the former, *you can't be willing to infer C1 from C unless you possess C1*. Option C thus places constraints on concept possession just as Option A does. As far as I can tell, Fodor's architectural point does not give us a reason to think that a theory that links concept constitution to concept possession via meaning postulates is in any way weaker than a theory that links them via conceptual structure. Meaning postulates are still apparently a viable option.

It's reasonably clear what the Quinean/Fodorean response to this will be: if what's constitutive of a concept does not hold in virtue of its structure, then there's nothing to

¹² In case the reader is worried that I'm being uncharitable in saying that it follows from Informational Atomism that 'water is H₂O', 'Hesperus is Phosphorus', and the like are *analytic*, here's Fodor himself admitting as much: "Informational semantics may well have to say that 'Cicero' and 'Tully' carry the same information, hence that they have the same content, hence that they mean the same. So, it's true both that 'Cicero' means *Tully* and that 'Tully' means *Cicero*. It also follows that 'Cicero was Cicero' and 'Cicero was Tully' are both analytic (though, of course, the second isn't knowable *a priori*)" (1994, p. 110). If 'Cicero was Tully' is analytic because 'Cicero' and 'Tully' carry the same information, then 'water is H₂O' is analytic too, since 'water' and 'H₂O' carry the same information. It's worth pointing out that this does *not* sit happily with some of Fodor's claims elsewhere. As we saw above, in his discussion of the constraints that compositionality places on a theories of meaning and concepts (e.g. Fodor and Lepore 2002), Fodor is at pains to point out that analyticity and compositionality come to pretty much the same thing: that the analytic inferences are those that turn on the compositional structure of concepts, and that the inferences that turn on the compositional structure of concepts are the analytic inferences. Of course, if 'water is H₂O' is analytic, then compositionality and analyticity come apart, since the inference from 'water' to 'H₂O' does not turn on the compositional structure of WATER.

constrain a theory of concept constitution.¹³ In other words, the objection will be that there's no substance to my simply *saying* that such a theory *is* constrained because the constitutive relations that hold between concepts are a matter of them being linked by meaning postulates. The Quinean/Fodorean rhetorical refrain will thus be: what distinguishes the relations that hold in virtue of meaning postulates or inferences rules, and those hold as a matter of mere empirical belief? Concept Cartesianism thus raises a challenge against the sort of Concept Pragmatism I've been defending that is precisely analogous to the challenge that Quine raised against Carnap. In the next chapter, I argue that the Pragmatist has the resources to meet at least part of this challenge.

Conclusion

In this chapter we've seen that Quine's epistemological arguments against the analytic/synthetic distinction are far from convincing. Contemporary defenders of the distinction needn't endorse the traditional, positivistic motivations behind it, and giving up on these motivations opens up the possibility of an empirical defense of the analytic that is fully compatible with Quine's Confirmation Holism and claims about empirical revisability. Moreover, we've seen that Fodor's empirical arguments against the analytic have problems. First, the methodology of his arguments against lexical decomposition

¹³ One might take this to be in the spirit of both Hume and Kripke. Hume tells us that there are no necessary relations between distinct existences, and many contemporary metaphysicians construct their theories around this. (Lewis (1983, 1986) is perhaps the most famous modern Humean; see Ellis (2001) for a nice critical discussion of the role of the "Humean Supervenience Thesis" in contemporary metaphysics.) Necessary connections between concepts that do *not* hold in virtue of conceptual structure thus might violate some philosophers' metaphysical scruples, since they would count as necessary connections between distinct existences. (A concept and its constituents, however, are arguably *not* distinct existences. 'Constitution' is generally taken to pick out a metaphysical relation that is weaker than identity, but nevertheless stronger than relations that imply distinctness.) One might take such constitutive connections among concepts to go against the spirit of Kripke, too, at least if one reads Kripke in a way that invites the following claim: all (metaphysical) necessities are structural in nature. In any case, a full defense of Option C would certainly sort out the metaphysics more than I've done here.

and definitions appears to be at odds with the very view he is attempting to motivate. Second, and more importantly, the success of his arguments does not spell disaster for Concept Pragmatism. Whereas Fodor assumes that Concept Pragmatism is committed to the view that concept-constitutive inferences and judgments are specified by conceptual structure, we've seen that it is committed to no such thing. Concept-constitutive roles and the analyticities they engender do not *imply* that lexical concepts are internally structured. Arguments against such internal structure thus count as arguments against analyticity and concept-constitutive roles only if the latter are taken to hold as a matter of conceptual structure. If we don't take concept-constitutive roles to be specified by conceptual structure, as I've suggested, then one can accept Fodor's arguments for Concept Atomism without giving up on Concept Pragmatism.

CHAPTER 6

PROSPECTS FOR MEETING THE QUINEAN CHALLENGE

Introduction

Let's take stock. In Chapter 4, we saw that Peacocke's examples are consistent with a Cartesian denial of a constitutive connection between concepts and perceptual judgments. Perceiving something *as red* plausibly requires that, in certain conditions, one judges that (say) *that's red*. Peacocke takes this to support the claim that there's a constitutive connection between RED and the corresponding recognitional judgment. But as we've seen, the Cartesian has an alternative explanation. That judgments with certain contents tend to follow perceptions with certain contents can be explained *merely* by the functional nature of judgment and perception. For 'perception' is plausibly defined functionally in terms of 'belief' or 'judgment'. Rather than showing anything about the metaphysics of concepts, then, the Cartesian can insist that Peacocke's examples merely illustrate the functional nature of mental states themselves. And, as Fodor has long insisted, one can be a functionalist about mental states *without* being a functionalist about their contents.

Moreover, we've seen that the Cartesian will insist that the Pragmatist's claim that some judgments and not others are concept-constitutive commits him to an analytic/synthetic distinction, which the Pragmatist hasn't told us how to draw. Peacocke's perceptual-demonstrative examples, for instance, apparently do not provide an explanatory basis for the claim that some divergences in judgment manifest a

difference in concepts, while others merely indicate a difference in empirically held belief. Peacocke is right to point out that some examples seem obvious or plausible, and that thinkers find some inferences/judgments and not others primitively compelling. But we've seen that this is not sufficient to meet the Quinean challenge, and that such examples thus leave this challenge intact and unanswered.

In Chapter 5, however, we had a look at the other side of this dialectic. We saw there that Quine's best arguments against the analytic/synthetic distinction are unconvincing, and that confirmation holism and empirical revisability are perfectly compatible with the existence of a principled analytic/synthetic distinction (and even the *a priori*). Given the failure of Quine's arguments, we turned to Fodor's "empirical, non-demonstrative" case against analyticity and conceptual connections: his arguments against lexical decomposition and definitions. I argued that the force of these arguments depends upon presupposing the 'concept-containment' notion of conceptual structure, and that, for precisely this reason, Concept Pragmatism is not necessarily vulnerable to them. For, *contra* Fodor (in at least some of his moods), the question of whether lexical concepts have constitutive roles in thought and judgment is *independent* of the question of whether those concepts are internally structured. Lexical concepts can be relationally individuated in terms of their roles *even if* they lack structure altogether. In addition, we saw that Fodor is wrong to suppose that such a view does not have the resources to explain the relation between an account of what's constitutive of a concept and its possession conditions.

Now, all of this leaves us in a somewhat puzzling dialectical situation. Chapter 4 acknowledges the force of the Quinean challenge and the Concept Pragmatist's apparent

failure to meet it, and Chapter 5 acknowledges that both Quine's and Fodor's arguments against the analytic are far from convincing. My aim in what follows is to take some preliminary steps towards resolving the apparent tension here. I consider, in particular, two things: (1) the prospects for meeting the Quinean challenge, noting both the promise and limitations of a recent strategy that attempts to do so; and (2) Peacocke's (2004a, 2004b) recent remarks concerning the relation between Quine's attack on the analytic/synthetic distinction and his own project. I argue that the Quinean challenge can be met for constitutive connections among concepts, and briefly consider a promising strategy for meeting it in the case of constitutive connections between certain concepts and perceptual judgments. I conclude the chapter by showing that Peacocke's most recent attempt to meet the Quinean challenge fails.

The Quinean Challenge Revisited.

Recall that the Quinean challenge is to provide some data, the best explanation of which is that there are constitutive, analytic connections among concepts. Given the arguments of Chapter 5, the existence of such data would not *require* the hypothesis that lexical concepts are internally structured. For one natural way of spelling out such a hypothesis would be to say that meaning relations are captured by inference rules, or meaning postulates, which hold between lexical items that lack structure at the semantic level. Explaining the data in this fashion would thus render the hypothesis *immune* to Fodor's empirical arguments for Concept Atomism.

Of course, pointing this out is insufficient to meet the Quinean challenge. For while the arguments against lexical decomposition and definitions are certainly Quinean

in flavor, they're nevertheless distinct from the Quinean challenge itself. The challenge arises for *all* theorists who claim that there are meaning relations, or constitutive connections among concepts, *regardless* of whether or not they make the additional claim that such connections hold in virtue of conceptual structure. For Quine challenges the very existence of meaning relations, and this challenge is independent of the issue of whether such relations are structural in nature, or instead hold in virtue of inference rules, or meaning postulates. In other words, a Concept Pragmatist can reply to Fodor's arguments against lexical decompositions and definitions by denying that analyticities hold in virtue of conceptual structure, but this is not yet to reply to the Quinean Challenge.

Indeed, Quine's attack was originally leveled against Carnap's meaning postulates. Fodor expresses the Quinean challenge with some rhetorical questions that echo Quine's original complaint:

Imagine two minds that differ in that ‘whale → mammal’ is a meaning postulate for one but is ‘general knowledge’ for the other [an example from Partee (1995)].
Are any further differences between these minds entailed? If so, which ones? Is this wheel attached to anything at all? (1998a, p. 112)

The worry is that meaning postulates merely provide us with an *idle label* for those connections and inferences we wish to treat as constitutive.¹ But it's the burden of those who endorse meaning postulates to spell out a principled reason for treating some connections among concepts as constitutive in the first place, and thus answer Fodor's rhetorical questions. Can defenders of such connections show that the wheel *is* attached

¹ See Quine (1966a).

to something after all? Providing some data the best explanation of which is that there *are* analytic connections would, I take it, do just that.

In attempting to provide such data, philosophers have found inspiration in the Chomskyan revolution in linguistics. Paul Horwich (1992) and Georges Rey (1993a, forthcoming a, b), for instance, argue that an explanatory basis for the analytic can be provided by the same sort of data that linguists use to support their hypotheses about syntax. Linguists take speakers' judgments about whether certain strings of words are "acceptable" or "unacceptable" as data to be explained by hypotheses about the structure of the language faculty. Of course, this isn't to say that linguists' theoretical claims about syntax can *always* be overridden by the judgments of speakers. Indeed, when one bears in mind Chomsky's competence/performance distinction, one would *expect* certain strings to be judged ungrammatical by speakers, despite the fact that they nevertheless count as grammatical according to linguistic theory. For the gap between judgments of acceptability and grammaticality can plausibly be explained by appeal to performance limitations, e.g. processing effects (Larson and Segal 1995, p.560n.17; Pietroski and Rey 1995).

Just as speakers' intuitions about grammaticality can be used as data by the linguist, the thought goes, speakers' semantic intuitions can be used as data for the concept theorist. In particular, people make judgments of synonymy, antonymy, redundancy, entailment, and contradictoriness, all of which can be used in constructing a semantic theory (Katz 1972). Convergences in peoples' judgments provide a potentially powerful explanatory basis for constitutive connections among concepts. As we noted in Chapter 5, it certainly *seems* that there's an interesting difference between "bachelors are

unmarried” and “bachelors are frustrated”—one can see that the former is true *merely* by understanding the meaning of ‘bachelor’, while this is not true of the latter. I take it that all competent speakers of English agree with this. Correspondingly, thinkers who possess BACHELOR will find the thought JOHN IS A MARRIED BACHELOR to be (in some sense) unintelligible, or unthinkable. This would certainly seem to count as *prima facie* evidence that “bachelors are unmarried” is analytic, and correspondingly, that BACHELOR is constitutively connected to UNMARRIED (i.e., it’s a condition on possessing BACHELOR that you must be willing to infer from BACHELOR to UNMARRIED).²

Many philosophers agree, including convinced Quineans. Fodor, for example, admits that such analytic intuitions at least count as *prima facie* evidence for constitutive conceptual connections:

... I want to concentrate on the argument that the very fact that we *have* intuitions of analyticity makes a formidable case for there being intrinsic conceptual connections. I’m sympathetic to the tactics of this argument. First blush, it surely does seem plausible that *bachelors are unmarried* is a different kind of truth from, as it might be, *it often rains in January*; and it’s not implausible, again first blush, that the difference is that the first truth, but not the second, is purely conceptual. I agree, in short, that assuming they can’t be otherwise accounted for, the standard

² In what follows, I don’t wish to place much weight on the term ‘intuition’. It seems unlikely to me that the term picks out a unified class of states (or perhaps even a natural kind appropriate for psychological theorizing). I use it, in a very rough manner, to cover a range: those peculiarly compelling judgments that thinkers make relatively quickly after considering a possible state of affairs (“an object *couldn’t* be both red and not colored”), considered judgments that require a bit more reflection (“oh right, I see, justified true belief *couldn’t* be sufficient for knowledge”), as well as judgments that fall somewhere else on the spectrum. It’s worth pointing out that the locution “intuition of analyticity” is a *theoretical* way of describing what’s going on in the head of a thinker. Ordinary thinkers don’t have the intuition that *it’s analytic* that, say, red things are colored. Rather, they have the intuition that nothing red could fail to be colored. Perhaps they even have intuitions that they can see that “red things are colored” is true *merely* by knowing the meanings of ‘red’ and ‘colored’. The theorist who has the concept ANALYTIC describes such intuitions as “intuitions of analyticity”. Perhaps once you possess ANALYTIC, the concept itself can figure in the content of the intuition, so that you *can* have intuitions of analyticity.

intuitions offer respectable evidence for there being cases of intrinsic conceptual connectedness. (1998a, pp. 71-72, original emphasis)

Even Quine himself admits that if there were widespread agreement among speakers/thinkers about a range of cases, then that would give him pause. He says:

The fact remains that, if evidence should accumulate to suggest that there is an impressively broad range of sentences which nearly all informants would put into list A [which includes such intuitively “analytic” statements as ‘no bachelors are married’ ‘black swans are black’, etc.], this would be a uniformity worth studying. A study of common traits of such sentences, and of psychological mechanisms behind them, might help us on some semantical points which have been ill served by the uncritically posited dichotomy between analytic and synthetic. (1967, p. 54)

It’s not implausible that there *is* an impressively broad range of cases, which goes well beyond ‘bachelors are unmarried’ and the like. Consider the following list, which includes some hoary examples but also some not-so-hoary ones:

- (1) John is unmarried if he’s a bachelor.
- (2) Vixens are female foxes.
- (3) Desks are furniture.
- (4) John is a male sibling if he’s a brother.
- (5) Nightmares are dreams.
- (6) The car is colored if it is red.
- (7) John has lots of money if he is rich.
- (8) If John runs, then John moves.

- (9) John believes that P if he knows that P.
- (10) If the door is closed then it is not open.
- (11) John is an adult if he is 40 years old.
- (12) John is naked if he is nude.
- (13) If John boiled the water, then the water boiled.
- (14) If John persuaded Bill to leave, then Bill intended to leave.
- (15) John tried to get Bill to decide to leave if John encouraged Bill to leave.
- (16) If John lifted the cup then the cup rose.
- (17) Bill died if John killed him.

I assume that all competent speakers of English would assent to (1)–(17), and would agree that determining their truth requires no more than understanding their meaning. Moreover, I take it that speakers would find the *denials* of (1)–(17) to be in some sense *unintelligible*. If peoples' judgments were to converge on these and other such cases, then, as both Quine and Fodor admit, that would count as *prima facie* evidence that (1)–(17) are analytic, and that the relevant concepts are constitutively connected. That is, the hypothesis that there are analyticities offers a *prima facie* plausible *explanation* of the fact that speakers' judgments about these and other such cases converge in the ways that they do.

Again, on the sort of view being suggested, that (1)–(17) are analytic *needn't* be taken to imply that the relevant lexical concepts are internally structured. For we can understand the Chomskyan defense of analyticity as claiming that the analyticities that are posited to explain the analytic data hold in virtue of facts that are entirely internal to the language faculty, in the same way that the rules of grammar hold in virtue of facts

internal to the language faculty. On a standard cognitivist understanding of linguistic theory, the rules of grammar are represented—either explicitly or implicitly—in the language faculty (Larson and Segal 1995). And many linguists claim that certain inferences hold simply in virtue of our being competent speakers of a language. The inference in (13), for instance, arguably holds in virtue of our linguistic competence (Pietroski 2003b), and we can understand the other inferences in (1)-(17) in a similar fashion. Rather than taking the inference in (17) to hold in virtue of the internal structure of KILL, one may take it to hold in virtue of a rule, internal to the language faculty, which guarantees the validity of the inference from ‘x killed y’ to ‘y died’. The Chomskyan analogy could thus plausibly be developed along the same lines with which I defended Concept Pragmatism in Chapter 5.

Now, as both Horwich and Rey stress, if analyticities reside in the abstract structure of the mind in the way that the Chomskyan analogy suggests, then they may not play the epistemological role for which they were historically invoked. For whereas analyticities were traditionally taken to be obvious and readily available to introspection, the analytic rules that govern our conceptual competence need not be obvious to untutored intuition, any more than the rules of grammar are. The examples above happen to be relatively obvious. But think of the subtlety of the scenarios concerning painting we considered in Chapter 5, which, on this sort of view, can plausibly be taken to reveal the analytic rules governing PAINT_{TR}.³ Positing analyticities for broadly explanatory

³ Think, too, of the range of scenarios that philosophers come up with in order to reveal the constitutive connections among our concepts: Gettier-style cases as evidence against the traditional analysis of knowledge; Putnam’s Twin-Earth and Burge’s arthritis cases as evidence for an externalist treatment of meaning and mental content; Block’s Inverted-Earth case as evidence against representationalist theories of consciousness; Davidson’s Swampman as evidence against teleological theories of content; and so on and so forth.

reasons thus requires giving up on the claim that we can know *prior* to empirical investigation exactly which conceptual connections are analytic. Not only that, but analyticities so conceived will not have the privileged epistemic status they were once given. After all, that the rules governing our conceptual competence have certain contents and structure does not *guarantee* that the things in the world to which our concepts apply will cooperate with such rules.

Unlike the behavioristic conception of analyticity shared by the Positivists and Quine, the Chomskyan analogy thus suggests that analyticities need *not* be unrevisable. As Horwich puts it:

[T]here might be evidence that under certain circumstance certain contents of the language faculty (in particular the ‘meaning postulates’) are revised, constituting a change in language. We might find out that such changes are *pragmatically* (as opposed to *epistemologically*) driven—that they result, unlike normal changes in belief, from a practical desire to achieve an overall simplification in our network of beliefs-representations. (1992, pp. 100-101, original emphasis)

Such changes could also be theory-driven. For instance, noting a suggestion made by Wittgenstein (1953/1958) and Ziff (1959), Rey remarks that it might turn out that, upon investigation, the inference from THINKING → ALIVE is analytic (forthcoming a, p. 17). Nevertheless, there may be strong theoretical reasons for supposing that *being alive* is *not* a necessary condition on *being a thinking thing*, in which case it could turn out that according to our best theory it is *analytic, but false* that thinking things are alive. If this were the case, the concept THINKING (or THOUGHT) would be used scientifically in a way that *forges* some of its constitutive connections to other concepts.

This idea is rather Kripkean in spirit, since it takes (some of) the analytic rules that govern our concepts to be mere reference fixers. Thus, in the case above, it may turn out to be analytic that thinking things are alive. On the Chomskyan view under discussion, this would amount to there being a constitutive connection between THINKING and ALIVE. However, if upon empirical investigation of the world, it turns out that *what we’re getting at* with our term ‘thought’ (or concept THOUGHT) *isn’t* necessarily connected to the property of being alive, then our best empirical theory would tell us *both* that it’s analytic that thinking things are alive *and* that it’s false that thinking things are necessarily alive. The analytic connections would thus be used merely to fix the reference of ‘thought’, and philosophers and scientists could then *use* the term ‘thought’ while ignoring those connections.⁴

Alternative Accounts

Given that the Chomskyan strategy posits analyticities and constitutive connections for broadly empirical/explanatory reasons, Quineans are forced to take such data seriously. In order to avoid postulating analyticities, they must give some alternative explanation of the data that does not posit genuine constitutive connections. The general Quinean strategy for offering such an alternative explanation is to, first, grant that it *seems to us* that certain statements can be seen to be true *merely* by understanding the relevant terms, or possessing the relevant concepts, and second, argue that our intuitions of analyticity

⁴ Another potentially promising possibility can be found in the internalist conception of semantics found in Chomsky (2000) and Pietroski (2003a, forthcoming), according to which meanings “constrain without determining” reference and truth conditions. This sort of view divorces meaning and truth-conditions in a way that sits nicely with the psychological notion of analyticity under discussion, and would seem to allow for the possibility of false analyticities. But working out the details is well beyond the scope of this dissertation (and, *ipso facto*, this chapter!).

are in fact the result of something *other* than analyticity itself. In this section, I consider the alternative accounts of Quine, Fodor, and Margolis and Laurence, and argue that each of them fails.

Quine provides two alternative explanations of the analytic data, one that invokes the *origin* of some of our beliefs, and one that invokes the *centrality* of some of our beliefs. Let's start with the first.

The way in which we learn certain terms/concepts, Quine says, leads to the mistaken sense that some claims can be seen to be true solely by understanding what they mean:

[O]ur intuitive semantics rates ‘bachelor’ and ‘unmarried man’ as synonymous.

... But now what can have been the cause of those intuitive ratings themselves?

Not, I think ... an implicit sociological guess that under extraordinary stimulation most people would hold ‘bachelor’ and ‘unmarried man’ coextensive.

A likelier place to seek the cause is in ... how we whose mother tongue is English learn ‘bachelor’. We learn ‘bachelor’ by learning appropriate associations of words with words. ... One looks to ‘unmarried man’ as semantically anchoring ‘bachelor’ because there is no socially constant stimulus meaning to govern the use of the word; sever its tie with ‘unmarried man’ and you leave it no very evident social determination, hence no utility in communication.

(1960, p. 56)

Putting aside worries about the behavioristic conception of word learning Quine is assuming here, it's clear that this won't work as a *general* explanation of our analytic intuitions. For there can surely be convergences in people's judgments about cases that

are *not* a result of how they learned the relevant terms. Even if Quine's explanation works for BACHELOR and the like, it clearly won't work for analytic data that concerns concepts for which it is quite difficult to come up with constitutive connections (e.g., more abstract concepts like DEMOCRACY, or FREEDOM). Moreover, even for cases that *do* fit Quine's story about word learning, people have no problem at all dissociating the relevant terms. Kripke's (1972) treatment of proper names illustrates this (Rey 1993a). The concepts COLUMBUS and DISCOVERER OF AMERICA, for instance, no doubt fit Quine's story for many people. But people have no problem imagining that someone else discovered America, e.g., the Chinese. Since there is analytic data for cases that *don't* fit Quine's story, and cases that fit Quine's story for which there's *no* analytic data, his explanation is clearly insufficient.

Quine's other, more well-known, alternative explanation appeals to a different epistemic property of concepts, namely, *being central* in the web of belief. The idea here is that thinkers confuse the *epistemic* property of centrality for the *semantic* property of analyticity. It only seems to us that certain statements can be seen to be true solely in virtue of their meaning, so the explanation goes, because those statements are at (or near) the center of our belief systems. As Fodor puts it: "Quine's point (utterly convincing in my view) is that what pass for intuitions of analyticity are in fact intuitions of centrality; and centrality is an *epistemic* relation, not a *semantical* one" (1990, x-xi, original emphasis).

The appeal of this explanation is clear enough. Since the analytic/synthetic distinction was traditionally invoked to explain the puzzling epistemic and modal status of logic and mathematics, Quine's confirmation holism was thought to have undermined

the need for analyticity. For confirmation holism implies that with enough recalcitrant experience even the claims of logic and mathematics can rationally be given up. Of course, it might *seem to us* that such claims (“ $2 + 2 = 4$ ”, “ P or not P ”, etc.) *are* immune to revision. But, Quine says, that’s just because such claims occupy a central place in our overall theory of the world. The idea, then, is that the reason we have such a hard time imagining that so-called ‘analytic’ claims could turn out to be false is that giving up on them would require a *massive* amount of revision to rest of our belief system.

While this explanation is perhaps plausible in the case of logic and mathematics, like Quine’s first explanation, it is hardly sufficient as a *general* explanation of the data. For a belief’s having the property of centrality is neither necessary nor sufficient for it’s seeming analytic. Consider, for instance, Russell’s (1921) famous hypothesis that the world has existed for only five minutes. Although the hypothesis is compatible with all of our present experiences (and, according to Russell, our memories) we all believe that it is false, and this belief surely occupies a central place in our web of belief (think of the amount of revision required to give *that* up!). Nevertheless it doesn’t at all seem analytic (Rey 1993a). For unlike the examples in (1) – (17) above, it’s very easy for us to imagine that the belief is false. In other words, however implausible the five-minute hypothesis may be, it is nevertheless perfectly *intelligible*. This suggests that being central is not sufficient for seeming analytic.

Moreover, the examples in (1) – (17) illustrate that being central is not even a necessary condition for seeming analytic. For our beliefs in (1) – (17) seem as analytic as any belief could, yet they aren’t the least bit central to our overall theory of the world. Giving up on them would hardly have a massive effect on the rest of our beliefs. Indeed,

the only beliefs affected would seem to be those concerning what the relevant terms mean (Rey 1993a). If I were to give up my belief that ‘if x killed y , then y died’ and instead decided to use ‘kill’ to cover cases that I would now refer to as ‘attempted killings’, I wouldn’t need to change any beliefs other than those that concern what ‘kill’ means. Being central is thus not even necessary for seeming analytic. Quine’s appeal to centrality won’t suffice as a plausible alternative explanation of the analytic data.

While Fodor apparently used to believe that centrality is all that is needed for a Quinean explanation of our intuitions of analyticity (see the above quote from Fodor 1990), his most recent favored alternative explanation involves an appeal to Putnam’s (1983) “one-criterion” concepts. As we saw earlier, Putnam endorses confirmation holism and is in general sympathetic with Quine’s attack on the analytic and the *a priori*, in the sense that he does not think that there are analyticities that can do interesting philosophical work. However, he doesn’t think that agreeing with Quine about *this* requires giving up on *all* analyticities. In particular, Putnam claims that the putative analyticities that reside out at the periphery of our web of belief—those analyticities that, as we just saw, *can’t* be explained away by appeal to centrality—remain intact even after Quine’s confirmation holism is swallowed whole. The hoary examples of analyticities involving bachelors, vixens, killings, brothers, etc. all fit this category, and Putnam’s proposal is that these analyticities arise because the corresponding concepts are one-criterion concepts. Putnam explains the proposal as follows:

The idea, in a nutshell, is that there is an exceptionless ‘law’ associated with the noun ‘bachelor’, namely, that someone is a bachelor *if and only if* he has never been married; an exceptionless law associated with the noun ‘vixen’, namely that

something is a vixen *if and only if* it is a female fox; etc. Moreover, this exceptionless law has, in each case, two important characteristics: (1) that no other exceptionless ‘if and only if’ statement is associated with the noun by speakers; and (2) that the exceptionless ‘if and only if’ in question is a *criterion*, i.e., speakers can and do tell whether or not something is a bachelor by seeing whether or not it is an unmarried man; whether or not something is a vixen by seeing whether or not it is a female fox; etc. (... I contend that only a few hundred words in a natural language have this ‘one-criterion’ character: most words are either associated with no exceptionless criterion, or with more than one.). (1983, p. 89)

So, analyticities will involve concepts for which there is only one criterion, i.e., only one ‘way of telling’ whether or not the concepts apply to a given object.

The problem with Putnam’s proposal as an account of the analytic is that there appears to be no way of individuating criteria that does not itself appeal to analyticity. One could count criteria by counting synonyms (e.g., ‘unmarried man’ and ‘not married man’ are the *same* criterion since they’re synonymous), but this won’t help since, as Quine famously points out, analyticity can just as well be defined in terms of synonymy. As Fodor says: “it looks as though Putnam’s construal of *analytic connection* in terms of *one-criterion concept* leaves us back where we started; in a tight circle of interdefined semantic-cum-conceptual vocabulary” (1998a, p. 82).

This doesn’t worry Fodor, though, since unlike Putnam he is *not* out to defend analyticity for a small handful of beliefs out at the periphery of our web. Rather, he is merely interested in *explaining away* our intuitions of analyticity. And he argues that

Putnam's one-criterion concepts can be put to use for *this* purpose, even if it will not work as an account of analyticity itself. The idea is that if a concept has just one way of telling whether or not it applies, then this will give rise to an intuition of analyticity.

Fodor explains his account as follows:

[S]uppose you think the only epistemic route from the concept C to the property that it expresses depends on drawing inferences that involve the concept C^* . Then you find it intuitively plausible that the relation between C and C^* is conceptual; specifically, that you can't have C unless you also have C^* . And the more you think that it is *counterfactual supporting* that the only epistemic route from C to the property it expresses depends on drawing inferences that involve the concept C^* , the stronger your intuition that C and C^* are conceptually connected will be. (1998a, p. 83, original emphasis)

Fodor considers a couple of examples to illustrate how this is supposed to work. He says, for instance, that on his account DOGS ARE ANIMALS comes out as a “relatively poorish” candidate for analyticity, and that this is as it should be. The reason is that there are many ways of telling whether something is a dog that do *not* rely on deploying the concept ANIMAL. As Fodor puts it, “there are lots of plausible scenarios where your thoughts achieve semantic access to doghood but not via your performing inferences that deploy the concept ANIMAL” (1998a, p. 84). The most common way of telling whether something's a dog is by the way it looks, but we also apply DOG on the basis of barks and jingling collars. The point is that none of these ways of telling involve inferences containing ANIMAL. When I see a dog and then apply DOG (e.g., judge *that's a dog*) I don't do this *via* a deployment of ANIMAL, and this would seem to be true *even if*

perception is always inferential. Since DOG is not a one-criterion concept, we shouldn't have any strong analytic intuitions concerning it. And, Fodor says, we don't. (But see below.)

The same point applies to other metaphysical necessities, such as "water is H₂O". There are lots of ways of determining whether something is water—its look, taste, location, etc.—that do *not* rely upon deploying the concept H₂O. So even though Informational Atomism has it that WATER and H₂O are *synonymous* (since they express the same property), Fodor's account explains why we don't think that there's a constitutive, analytic connection between the concepts themselves: WATER is not a one-criterion concept. But, Fodor says:

I can't imagine how I might determine whether John is a bachelor except by determining that he's male and un- (viz. not) married. ... Hence the intuitive analyticity of *bachelors are unmarried*. ... I'm suggesting that it's the epistemic property of being a one-criterion concept—not a modal property, and certainly not a semantic property—that putative intuitions of analyticity detect. (1998a, p. 84)

There are a few points I want to make about Fodor's account of our analytic intuitions. First, it's not entirely clear why Fodor's complaint against Putnam's account doesn't *also* apply to his own account. That is, it's not clear why *Fodor* gets to treat UNMARRIED MALE and NOT MARRIED MALE as the same criterion ("un – (viz. not) married") *without* specifying a principle of individuation for criteria, whereas Putnam's account fails precisely because he doesn't provide such a principle. Fodor might reply that failing to provide a way of counting criteria isn't a problem for his account, since he isn't interested in giving an account of analyticity itself, but rather just some faulty

intuitions. But it's not clear why this should matter. Since Fodor's claim is that intuitions of analyticity arise for those concepts that have *one*-criterion, it's *prima facie* plausible that he too needs to provide some way of counting criteria.

Moreover, it's not entirely clear why Fodor thinks he's warranted in claiming that BACHELOR has only one criterion. Note that the reason that DOG and WATER are *not* one-criterion concepts is that people associate with each concept a lot of contingent information—"dogs bark, have a certain look to them, etc., " "water is found in the lakes and streams, comes out of the taps, quenches thirst, etc."—that can be used as (rough and ready) ways of telling whether the concept applies. And Fodor uses this point in motivating his account. But I take it that the same holds for BACHELOR. Bachelors are unmarried men, but they're also people who tend to lead certain lifestyles. For instance, I can tell whether John is a bachelor by finding out whether he spends a lot of time at singles bars. I might also check to see if he wears a wedding ring, or whether he lives alone. Or I might just ask one of John's friends or relatives whether he's a bachelor. Of course, none of these are *surefire* ways of determining whether John is a bachelor. But this is *also* true of the multiple criteria Fodor cites in the case of WATER and DOG. Not everything that looks like a dog *is* a dog, and not everything that looks and tastes like water *is* water. But if this doesn't disqualify *those* ways of telling as genuine criteria for WATER and DOG, it's not clear why the above tests for BACHELOR shouldn't count as genuine criteria too, in which case *even* BACHELOR won't count as a one-criterion concept and Fodor's alternative explanation fails.

Now, Fodor seems to be aware of this criticism, and in the following passage is apparently attempting to preempt it:

What Putnam must have had in mind, and what I too propose to assume, is that some ways of telling pretty clearly depend on others. It's the latter—the pretty clearly *independent* ones—that you are supposed to count when you decide whether something's a one-criterion concept (1998a, pp. 82-83, original emphasis)

So perhaps for each concept there will be many ways of telling whether it applies. But, Fodor says, this is not a problem for his account since some ways of telling depend upon others. I can tell whether John is a bachelor by finding out whether he spends a lot of time at singles bars, or by finding out whether he is unmarried and male. But that the former is a way of telling, Fodor will say, *depends upon* the latter's being a way of telling, *but not vice versa*. That is, finding out that John is at the singles bars every night *wouldn't count* as a way of telling that John is a bachelor if it weren't for the fact that finding out that John is an unmarried male counts as a way of telling that he's a bachelor. But finding out that John is unmarried and male would be a way of telling whether John is a bachelor *even if* finding out that John spends every night in the singles bars *wasn't* (e.g., even if there weren't any singles bars). Similarly, asking John's friends whether he's a bachelor is a way of telling that depends upon finding out his gender and marriage status as a way of telling, but not vice versa.

Although this is an interesting suggestion, I'm not sure it works. Note, first, that we seem to have analytic intuitions concerning concepts that aren't one-criterion concepts. Again, consider DOG. Fodor is surely right that DOGS ARE ANIMALS is a "relatively poorish" candidate for analyticity. For we have (relatively) no problem *imagining* that dogs are radio-controlled robots from outer space (Putnam 1970, 1975a).

But what about DOGS ARE OBJECTS? This thought seems to have all the characteristics of analyticity: we think that understanding the meaning of “if x is a dog, then x is an object” is sufficient for determining its truth; we find the denial of “if x is a dog, then x is an object” to be unintelligible; and so on. But these analytic intuitions can’t be accounted for by Fodor’s one-criterion-concept story, since DOG is *not* a one-criterion concept. So it looks like being a one-criterion concept is not a necessary condition for giving rise to analytic intuitions.

Perhaps, though, DOGS ARE OBJECTS does have the property of *being central in our web of belief*, in which case such analytic intuitions can be accounted for by Quine’s strategy. But it’s not clear whether Fodor takes his explanation to replace, or supplement, Quine’s explanation. He says, for instance: “[W]hat I say about analyticity intuitions is, of course, a lot like what Quine says; *except* that he takes the epistemic property to be centrality *whereas* I think it’s one-criterionhood” (1998a, p. 86, my emphasis). If we take Fodor at his word here, then DOGS ARE OBJECTS *will* be a problem since it’s intuitively analytic but doesn’t have the right epistemic property, namely one-criterionhood. But Fodor goes on to say: “I doubt that either story covers all the cases, and there’s no obvious reason why they shouldn’t both be true” (1998a, p. 86). If this is right, then Fodor can perhaps appeal to centrality to explain away our intuitions concerning DOGS ARE OBJECTS, and the like, and then appeal to one-criterionhood to explain away the others.⁵

⁵ Another option available to Fodor is to appeal to metaphysical necessities. For perhaps some of our intuitions of analyticities are a result of our taking certain claims to be metaphysically necessary. For instance, Fodor could make such an appeal in explaining away Katzian intuitions about Putnam’s robot cats, for it’s plausible that “cats are animals” is *metaphysically* necessary, even if it’s not conceptually necessary. Perhaps the same goes for “cats are objects”.

In any case, there are other problems with Fodor's account. Consider again the concepts WATER and H₂O. Fodor's story about why we don't have intuitions that WATER is constitutively connected to H₂O is that WATER is not a one-criterion concept. But don't all the ways of telling whether some stuff is water depend upon telling whether it's H₂O? The reliability of seeing how some watery stuff looks, tastes, etc. as ways of telling whether the stuff is water would seem to depend upon the reliability of checking whether the stuff is H₂O as a way of telling whether it's water. That is, seeing what the stuff looks like and tastes like *wouldn't be* a good way of telling whether the stuff is water if checking on the stuff's chemical composition wasn't a good way of telling whether it's water, but not vice versa. If this is right, then all the ways of telling whether some stuff is water *do* asymmetrically depend upon checking its chemical composition as a way of telling, in which case *even* WATER is a one-criterion concept. However, if WATER is a one-criterion concept, then on Fodor's account we should have the intuition that it's constitutively connected to H₂O. But we don't. In particular, we don't think that accepting the inference from WATER to H₂O is necessary for possessing WATER. "Water is H₂O" may express a *metaphysical* necessity (Kripke 1980), but it surely doesn't express a *conceptual* one. (The ancients didn't lack the concept WATER, for instance, just because they didn't know any modern chemistry.)

However, perhaps this is unfair to Fodor. For maybe there are different kinds of asymmetric-dependency relation that can hold between criteria, only some of which will be relevant to determining whether a concept has just one criterion. In the case of WATER, for instance, the plethora of ways of telling whether it applies to some stuff will depend upon checking the stuff's chemical composition as a way of telling, but in a

metaphysical sense of dependence. In other words, the reliability of telling that some stuff is water by seeing that it's clear and potable depends upon checking its chemical composition as a way of telling, but the latter is reliable because *water's being H₂O* is a *metaphysical* necessity. And perhaps the sort of reliability Fodor has in mind is epistemic, not metaphysical. Indeed, Fodor seems to suggest just this in the following passage:

You can tell, pretty reliably, whether stuff is water by, for example, how it looks, how it tastes, where it's located, its specific heat, its specific gravity, what it says on the bottle, which tap it came from, and so on and on. No doubt, the fact that all these ways of telling work depends on a bundle of metaphysical and nomic necessities; but your employing the tests doesn't depend on, and isn't usually rationalized by, your knowing that this is so; pretty clearly, the various tests for *being water* are largely epistemically independent. (1998a, p. 83)

But it's not clear why this should matter. Suppose that someone determines that John is a bachelor because he's been give a list of bachelors and John's name appears on the list. I don't see any reason for thinking that this way of telling isn't 'epistemically independent' of checking John's gender and marriage status, just as checking what it says on a bottle of some watery stuff is 'epistemically independent' of checking the stuff's chemical composition. What exactly is the contrast here? Fodor can say that applying BACHELOR to John on the basis of his appearing on a list of bachelors is ultimately 'rationalized' by one's *knowing* that appearing on the list is correlated with gender and marriage status. But why say this? Because the concepts UNMARRIED and MALE must *actually be deployed* when one infers that John is a bachelor because John's on the list

and everyone on the list is a bachelor? Surely not. But it's not clear what else Fodor could mean by saying that all the ways of telling whether BACHELOR applies are epistemically dependent on checking gender and marriage status. It seems plausible that what rationalizes the application of BACHELOR to John in the above scenario is that appearing on the list is *actually correlated in the world* with being unmarried and male. As far as I can tell, Fodor hasn't said anything that rules this out. But then there's no real contrast between the sorts of dependencies exhibited by BACHELOR's criteria and WATER's criteria, and the objection above stands.

Moreover, as Rey (forthcoming b) points out, there are one-criterion concepts that don't seem to give rise to analytic intuitions. For example, I might only know of one way to determine whether ACID applies to something, and that is to see whether it turns litmus paper pink. All the other ways of telling—e.g. sending the stuff over to the chemistry building—may depend in the right sorts of ways on that way of telling. But the thought ACIDS TURN LITMUS PAPER PINK doesn't seem to be analytic, since the content of ACID isn't plausibly determined by the way we happen to test for acidity. This doesn't seem to be the case with BACHELOR, however. As Rey puts it:

‘[B]achelor’ marks a superficial kind, whose nature is pretty much exhausted by the linguistics of the matter: unlike the case of litmus paper and acidity, *the reason that gender and marriage status are the best way to tell whether someone’s a bachelor is that that’s just what ‘bachelor’ means!* Indeed, should a chemist propose revising the test for acids in the light of better theory—perhaps reversing the dependency of certain tests—this would not *per se* constitute a change in the meaning; by contrast, should, say, a feminist propose, in the light of

better politics, revising ‘bachelor’ to include women, this obviously would.
(forthcoming b, p. 30, original emphasis)

Part of Rey’s point here is not only that one-criterionhood isn’t sufficient for *seeming* analytic, but also that the analytic would seem to be required for *explaining why* some ways of telling asymmetrically depend upon others. One wants to know *why* asking John’s friend as a way of telling depends upon checking John’s gender and marriage status as a way of telling, and the most plausible explanation is that the content of BACHELOR *just is unmarried male*. If this is right, then Fodor can’t help himself to the idea that some criteria are asymmetrically dependent on others in explaining away our analytic intuitions, since analyticity itself is needed to explain just such asymmetric dependence.

Rey also points out that both relatively abstract concepts such as FREEDOM and KNOWLEDGE, and empty concepts like DEMON pose a *prima facie* problem for Fodor’s account. For although we clearly have analytic intuitions concerning these concepts, they don’t seem to be one-criterion concepts. We have analytic intuitions concerning FREEDOM and KNOWLEDGE, yet we just as clearly don’t have *even one* adequate criterion for either of them. Perhaps we have some necessary conditions on their application, but nothing that’s criterial. Similarly, we have analytic intuitions concerning DEMON, yet there isn’t obviously any way to tell whether something is a demon.

I want to conclude this section by considering one more attempt to explain away our analytic intuitions, which is found in Margolis and Laurence’s (2003) recent paper “Should We Trust Our Intuitions? Deflationary Accounts of the Analytic Data”. For not only are Margolis and Laurence notable Quineans about concepts, but theirs is (to my

knowledge) the only other extant attempt to explain away our analytic intuitions, i.e. to provide what they call a “deflationary” as opposed to a “face-value” account of the analytic data. If we can show that their account is inadequate, then we will have shown that the alternative explanation that posits genuine constitutive connections among concepts is still in the running. Indeed, if I’m right that none of the extant Quinean accounts of the seeming analytic—Quine’s, Fodor’s, or Margolis and Laurence’s—is adequate, then, assuming there are no other Quinean alternatives, then the hypothesis of constitutive connections is the only other available alternative. Showing that these accounts are inadequate thus provides powerful evidence in favor of the analytic, and thus an empirically motivated Judgment Pragmatism.

Let’s begin by noting, with Margolis and Laurence, that face-value theorists must posit some psychological mechanism whereby the putative analyticities *generate* our analytic intuitions. I suspect that some philosophers who are enthusiastic about the role of intuitions in philosophical enquiry don’t realize that they’re committed to the existence of some mechanism that links our intuitions and the concepts that they putatively reveal the nature of. For in many areas of philosophy it seems to be simply taken for granted that consulting our intuitions reveals the nature of our concepts (and presumably the things in the world they pick out). But, someone might ask, why think that our intuitions reveal *anything* about the nature of concepts? Alvin Goldman and Joel Pust (1998) would reply that it’s “almost a matter of *definition* … [that] a concept tends to be manifested by intuitions that reflect or express its content” (p. 188, original emphasis).

But to their credit, Goldman and Pust don’t posit a mysterious “faculty of intuition” (as some recent conceptual analysts have done) reminiscent of the claims of

some traditional rationalists. For they go on to say that there's good reason to posit some psychological mechanism that links our concepts and intuitions:

[A]lthough we do not currently know the precise causal route that connects concept structures with their conscious manifestations, it is extremely plausible, from any reasonable cognitive-science perspective, that there should be such a causal route. (Goldman and Pust 1998, p. 188)

This point is worth bearing in mind, since it's an empirical commitment of the explanatory argument for analyticity that I'm defending. The argument that our analytic intuitions provide data the best explanation of which is that there are genuine constitutive connections among our concepts, requires that there be a mechanism of some sort that links the analyticities themselves with our intuitions of analyticity.

The key idea of Margolis and Laurence's paper, however, is that once a psychological mechanism is posited to explain our intuitions, deflationary accounts, which attempt to *explain away* the analytic data, can co-opt the very same explanatory mechanism, *but without positing any analyticities!* Their strategy is thus to make explicit an empirical commitment of face-value accounts, and then to argue that that very commitment shows that the analyticities themselves are not doing any explanatory work in face-value theories. While I think Margolis and Laurence are absolutely correct in drawing attention to the explanatory burden face-value theorists face, I do not think they have succeeded in showing that analyticities themselves are explanatorily useless. In the remainder of this section I explain why.

Margolis and Laurence consider two possible mechanisms that might generate our analytic intuitions, one 'theory-based' and one 'similarity-based'. Since the difference

between these is irrelevant for our purposes, I'll focus on the theory-based account.⁶ On this view, our analytic intuitions are generated by what they call an "implicit theory of meaning". Such a theory might be developed along the lines of Jackson (1998), who takes our intuitions to reflect a "folk theory" of the relevant kinds, in much the way that "folk psychology" is taken to underwrite our inferences and judgments concerning the mental states of others.⁷ In my view, Jackson is motivated by exactly the right reason:

[O]ur classifications of things into categories ... is not done at random and is not a miracle. There are patterns underlying our conceptual competence. They are often hard to find ... but they must be there to be found. (1998, p. 64)

Of course, one needn't agree with Jackson that the implicit theory is a folk theory, but the difficulty of coming up with analyses does at least suggest that the theory is in some sense implicit. Recall from Chapter 5, for instance, the difficulty in providing an analysis of the concept PAINTS_{tr} (!); and when it comes to concepts such as KNOWLEDGE or FREEDOM it notoriously becomes even more difficult to provide analyses. However we understand it, though, Margolis and Laurence tell us that the implicit theory "takes categorization judgments as input and generates, among other things, judgments that certain claims have a special status owing to the meaning of their constituent terms" (2003, p. 15). When we reflect on the patterns we find in our judgments, the principles of the implicit theory thus serve to generate what we take to be intuitions of analyticity.

⁶ They describe the similarity-based account as follows: "[I]nstead of an implicit theory that generates intuitions of analyticity, there is a psychological mechanism embodying a similarity metric that classifies categorization judgments as being more or less alike. According to this classification, claims that are analytic constitute a natural similarity class. For example, *Bachelors are unmarried* would be judged to be more like *vixens are female* than either would be judged to be like *grass is green*. This gives rise to the intuition that claims that are analytic constitute a special class" (Margolis and Laurence, 2003, p. 16).

⁷ Jackson explains his view as follows: "My intuitions about which possible cases to describe as cases of *K*-hood, to describe using the term 'K', reveal my theory of *K*-hood (remembering, but suppressing in the interests of keeping things simple, that this 'revelation' may be far from straightforward). In as much as my intuitions are shared by the folk, they reveal the folk theory" (1998, p. 37).

Once such a theory is posited, however, Margolis and Laurence claim that the analyticities themselves become unnecessary. They argue as follows:

[D]eflationists can simply mimic the face value accounts without committing themselves to any real analyticities. The models canvassed in the previous section [i.e. the ‘implicit theory’ and ‘similarity-based’ models] are all *psychological* accounts of the genesis of intuitions; they make *no* essential appeal to the existence of analyticities. As a result, deflationists are free to co-opt those models more or less wholesale. (2003, p. 17, original emphasis)

Note the oddity of Margolis and Laurence’s remark here. Since the mechanisms that generate our intuitions are *psychological* in nature, they claim, such mechanisms do *not* involve an ‘essential appeal to the existence of analyticities’. But why the contrast between psychology and analyticity? The *whole point* of the Chomskyan analogy and the explanatory argument for the existence of analyticity is to put the analytic/synthetic distinction *on a par* with the other posits of our best psychological theory. There are, of course, those who don’t take Chomskyan linguistics to be part of cognitive psychology (Devitt and Sterelny 1989, Devitt forthcoming), and these theorists would perhaps cut off the Chomskyan strategy before it could even begin. But surely Margolis and Laurence do not want to rest their argument against the empirical case for the analytic on *that*, especially since Laurence has argued to the contrary elsewhere (Laurence 2003). One immediately wonders, then, whether Margolis and Laurence have taken to heart the implication of the explanatory argument they’re criticizing, namely, that *analyticity be psychologized.*

They continue their argument by considering how the deflationary theorist could co-opt the face-value theorist's implicit theory:

On these models, intuitions of analyticity are the product of an implicit theory which takes sets of categorization judgments as input and delivers as output intuitions of analyticity. Nowhere in these models is an appeal made to actual analyticities. So a deflationist can happily adopt such a model, while denying that there are any analyticities. The implicit theory may imply that certain claims are analytic, but who's to say that it's right? The theory *doesn't have to be true* in order to be explanatory; people would have the very same intuitions either way. In other words, *analyticity per se does no explanatory work in accounting for our intuitions of the seeming analytic.* (2003, p. 17, first emphasis added)

Again, it seems to me that Margolis and Laurence have not realized an important implication of the view they're attempting to undermine. Recall from our earlier discussion that the Chomskyan analogy allows for an interesting divergence between what's analytic and what's true. For what's analytic, on this view, is a matter of what the empirical facts about the structure and contents of our minds turn out to be. There's thus no reason for thinking that analyticities, so conceived, *must* be true. (Recall Rey's nice example of "thinking things are alive".) Margolis and Laurence, then, are absolutely right that an implicit theory that explains the genesis of our analytic intuitions wouldn't have to be true in order to be explanatory. But this is not to the point. For the fact that a false theory would be just as explanatory as a true theory *doesn't* show that analyticities themselves are unnecessary in explaining the analytic data, *except* under the assumption that analyticities are 'true in virtue of meaning'. But although this is perhaps a traditional

conception of analyticity, it is precisely what the explanatory defense of the analytic is suggesting that we abandon. To *psychologize* analyticity is to give up on its necessary connection to *truth*. Of course, our best theory of the world might have it that some claims are *both* analytic *and* true, but they don't, as it were, 'inherit' their truth from their analyticity. Contra Margolis and Laurence, then, the mere fact that the principles of an implicit theory may turn out to be false does not show that such principles can't engender analyticities.

Concepts, Perceptual Judgments, and Analyticity Revisited

I've been defending the explanatory argument for the existence of constitutive connections among concepts. We've seen that the fact that there are convergences in peoples' judgments about statements such as (1)-(17) provides a powerful explanatory basis for the claim that there are analytic rules governing our competence with the relevant concepts. Moreover, we've seen that the alternative explanations of this data—Quine's, Fodor's, and Margolis and Laurence's—are all inadequate in some way. In this section, I want to consider whether this explanatory strategy will be of any help to Concept Pragmatists who believe that there are constitutive connections between some concepts and perceptual judgments. In other words, I'm interested in whether we're now in a position to meet the Quinean Challenge with respect to recognitional concepts that we considered in Chapter 4.

To begin, note that the analytic connections that we've been discussing so far are such that there will plausibly be plenty of data that is clearly relevant to the question of whether there are such connections. For people clearly *have* intuitions about (1)-(17) and

the like. But is there similar evidence in the case of the putative constitutive-connections between certain concepts and perceptual judgments? Consider the claim that there's a constitutive connection between the concept RED and judging *that's red* in certain conditions. As we saw in Chapter 4, Peacocke is surely correct in saying that it's *plausible* that a thinker who is staring at a red object (in good lighting conditions, with a properly functioning perceptual system, etc.) but is unwilling to judge that the object is red, lacks the concept RED. However, I argued in Chapter 4 that such plausibility is of no use in attempting to convince a Cartesian that there's a constitutive link between the concept and the perceptual judgment. What we're after now is some data that can provide an explanatory basis for postulating such a constitutive connection, which, unlike appeals to what's plausible or obvious, the theorist interested in concepts for the purposes of psychological explanation has reason to take seriously.

Now, I suspect that most people share Peacocke's intuitions that in order to possess certain concepts (e.g. RED, DESK, etc.), one must be willing to make certain perceptual judgments (e.g. *that's red*, *that's a desk*, etc.) when having experiences of certain kind (e.g. of a red object, of a desk, etc.). That is, it's plausible that people's judgments will converge on the claim that certain concepts are such that you can tell whether they apply *just by looking*. But they obviously won't have such intuitions for certain other concepts. I take it that no one thinks that if you possess BACHELOR, then you can tell just by looking whether someone's a bachelor. Is the fact that thinkers will converge on such judgments enough to provide an explanatory basis for the existence of a certain class of recognitional concepts? One might think so. For it's *prima facie* plausible that if we can provide an explanatory basis for the existence of constitutive

connections *among* concepts (e.g., BACHELOR's being constitutively connected to UNMARRIED MALE, etc.), then a similar basis can be provided for the existence of constitutive connections among certain concepts and perceptual judgments.

Paul Horwich, for instance, begins a recent article with some examples of the sort of thing the Concept Pragmatist takes to concept-constitutive, and recognitional concepts are included as a matter of course:

For the sake of concreteness (and to a very first approximation) here are some examples of the sort of meaning-constitution claims that may issue from [the use theory of meaning/Concept Pragmatism]:

“true” means what it does to us in virtue of our provisional underived acceptance of instances of the schema, “ $\langle p \rangle$ is true $\leftrightarrow p$ ”

“bachelors”’s meaning is engendered by our underived acceptance of the sentence “The bachelors are the unmarried men”

“red”’s meaning stems from our underived propensity to accept “That is red” in response to the sort of visual experience normally provoked by observing a clearly red surface

...

“and” means what it does in virtue of our underived acceptance of the two-way argument schema, “ $p, q // p \text{ and } q$ ”. (2004, pp. 351-352)

Many Concept Pragmatists will no doubt join Horwich in taking the concept-constitutive roles for recognitional concepts to be *on a par* with the constitutive roles for non-recognitional concepts, at least to the extent that they think that the same *kind* of evidence

that exists for one exists for the other. Thus if there's an explanatory basis for one, the idea would go, there ought to be an explanatory basis for the other.

While I feel the pull of this, it seems to me that there's a striking difference between the sort of data that exists for the concept-constitutive roles of recognitional concepts like RED, and the data that exists for the roles of non-recognitional concepts like GOOD or BACHELOR. While thinkers clearly *do* have intuitions about what's constitutive of something's being good, or something's being a bachelor, it's not clear that they have intuitions concerning what's constitutive of the concept RED. For that simply doesn't seem to be the kind of thing that thinkers have intuitions about. Ask people whether something *could be* red without being colored, and most will find it unintelligible. But ask them whether something *could be* the *concept* RED if it didn't bear any relation to perceptual judgments or beliefs, and I suspect that most people won't know quite what to say. Speaking for myself, I simply don't have intuitions about this.

It's thus important to distinguish two questions here, the conflation of which I suspect leads Concept Pragmatists to treat the evidential basis of recognitional concepts as on a par with the evidential basis of non-recognitional ones:

- (A) Could a thinker *possess* RED without being willing (or disposed) to judge *that's red* when having an experience of an object as red?
- (B) Could something *be* the concept RED if it weren't tied to perceptual judgments?

People plausibly have intuitions about (A), and will say "no". This is exactly the kind of data that Peacocke invokes in his argument that appeals to examples. As we've seen, it's intuitive that someone who (in good lighting conditions, etc.) is having an experience of

an object as red, but is not willing to judge *that's red*, does not possess the concept RED. Peacocke's theoretical claim that such perceptual judgments figure in the possession conditions for recognitional concepts thus has intuitive support. But again, as we saw in Chapter 4, these intuitions can plausibly be given an alternative Quinean/Cartesian explanation, which does *not* involve positing a constitutive connection between RED and the perceptual judgment. For these intuitions might just as well be taken to reflect the nature of our concept PERCEIVE AS—e.g., that it is constitutively tied to JUDGE THAT, or BELIEVE THAT, as I take it standard functionalism about mental states implies. Or, instead of taking them to reflect something about our mental-state concepts, they might be taken to reflect our best guesses as to the metaphysics of *perceiving as*, or *judging that*.

It seems to me, then, that in order for Concept Pragmatist to meet the Quinean/Cartesian challenge for recognitional concepts on explanatory grounds, it must be the case that people have intuitions about (B), and in particular that they converge on “no”. This would constitute data that would bear on the question of whether something *could be* the concept RED if it weren’t constitutively connected to perceptual judgments, and thus on the very issue that divides Cartesians and Pragmatists with respect to recognitional concepts. For that issue is the *metaphysical* one of whether it’s constitutive of the concept *itself* that it’s connected to perceptual judgment. Both Cartesians and Pragmatists agree that it’s intuitively plausible that one who fails to make certain perceptual judgments does not *possess* the concept RED. But appealing to data concerning people’s intuitions about whether someone could *possess* RED if they weren’t

willing to make certain perceptual judgments can't settle the issue of what constitutes the concept itself. Data concerning peoples' judgments (B) would seem to be required.

After all, the Cartesian claims that RED does not *have* any possession conditions—or, if you prefer, that the only condition on thinkers' possessing RED is that they have the corresponding mental representation that allows them to think thoughts about red—and thus that it's metaphysically possible not only that a mind could possess RED *and no other concepts*, but also that a mind could possess RED *without having any perceptual apparatus whatsoever*. A mind, according to this metaphysical view, thus *could* possess RED and yet be incapable of making a perceptual judgment that something is red. Recall here Fodor's point about "Informational Theology": God's thoughts could have *immediate* semantic access to *being red* (Fodor 1998a, p. 79). That is, God's thoughts about red might be unmediated by epistemic capacities of *any* kind. While I do not find such extreme Cartesian Atomism plausible, my finding it so is plausibly *not* a result of consulting my intuitions. My nervous system doesn't tell me that there's "something odd" about Fodor's claim, in the way it tells me there's something odd about the possibility of an object that's red but not colored, or a married bachelor. I suspect that other thinkers' intuitions are the same on this score.

The point here is that while the Chomskyan, explanatory strategy for defending constitutive connections among concepts has a fighting chance against the skepticism of Quine and Fodor, it's far from clear that strategy will work in defending constitutive connections between concepts and perceptual judgments. This will come as no surprise to Fodor. For he argues not only that there isn't any empirical evidence in favor of recognitional concepts, but that there's in fact quite strong empirical evidence *against*

them. In particular, Fodor thinks that “it’s sort of provable that there aren’t any recognitional concepts” (1998c, p. 36), since they’re existence, he thinks, is incompatible with the fact that concepts are productive and systematic.⁸ I don’t wish to engage with Fodor over the compositionality of recognitional concepts.⁹ For even if it can be shown that recognitional concepts are compatible with a principle of compositionality that’s strong enough to explain the productivity and systematicity of thought, there’s still the prior question of whether such concepts exist. If the above considerations are correct, then the Quinean challenge that Peacocke and other Concept Pragmatists face with respect to recognitional concepts can’t be met on the kind of explanatory grounds suggested by the Chomskyan analogy.

There’s a further promising move the defender of recognitional concepts could make at this point, which is consistent with the broadly Quinean strategy we’ve been considering.¹⁰ This move depends on an assumption concerning the relation between laws of nature and properties, namely, that the genuine properties and relations that exist are those that figure in laws of nature. On this view, there is not a property corresponding to every predicate of natural language, nor even most predicates. Rather, the number of genuine properties is *much smaller* than the number of predicates, and is determined by the laws that exist at a world.¹¹ In the case of mental properties, then, the idea would be that the mental properties that exist—or, if you prefer, the mental predicates that pick out genuine natural kinds—are those that figure in genuine psychological laws.

⁸ See also Fodor (1998d) and Fodor and Lepore (2002).

⁹ It seems to me that there are plausible moves that Concept Pragmatists can make here, though. See, for example, Grandy (1998), Horgan (1998), Carruthers (2000), Prinz (2002), and Robbins (2002).

¹⁰ Thanks to Peter Carruthers for the suggestion, and for a discussion that prompted the next few paragraphs.

¹¹ See Lewis (1983) and Armstrong (1997) for a defense of so-called “sparse” conceptions of properties. I once heard Barry Loewer (at a talk given to the UMD philosophy department) say that properties and laws are a “package deal”. This nicely describes the view I have in mind.

Now, one might make use of this idea in defending the existence of recognitional concepts on empirical grounds. For if there are psychological laws that relate certain concepts to percepts, then, given the assumption above, there are plausibly constitutive connections between the two. And concepts that bear constitutive connections to percepts are, of course, recognitional concepts. For instance, suppose it's a psychological law that (*ceteris paribus*) in a normal functioning human, the concept RED will come to be entokened when one has an experience that represents something *as red*. Since the law picks out the states by reference to their contents, the law could be taken to describe a constitutive connection between experiences of that type and judgments of that type. We thus ought to believe that RED is a recognitional concept, the thought goes, because there's a psychological law that describes the relation it bears to percepts. Rather than appealing to empirical facts about the intuitions and considered judgments of thinkers, this strategy appeals to the empirical fact that there are psychological laws that describe how certain concepts are related to perception.

As I said, I think this is a promising move. I'm not entirely sure, though, that it will convince a Cartesian. Note, first, that Cartesians can happily admit that the genuine mental properties are those that figure in laws of nature. Moreover, Cartesians can admit that when someone judges *that's red* as a result of perceiving something *as red*, this event is subsumed by a genuine psychological law. However, one *prima facie* worry with this way of defending recognitional concepts is that it apparently requires the existence of so *many* psychological laws. For I suspect that if there are any recognitional concepts, then there are lots of them. The putative class of recognitional concepts presumably includes not only color concepts, but the other response-dependent concepts, as well as some

artifact concepts too. Are there really so many such laws, one for each recognitional concept and the corresponding percept?

The Cartesian might insist that what's going on is that the instances of all of these distinct (putative) laws are actually instances of a more general law that holds between perception and judgment, which does *not* pick out the respective states in terms of their contents. Rather, they might claim that the law simply describes a relation that holds between perceptual states and judgment states, *not* their contents.¹² The psychological law would thus describe how the perceptual system and judgment/belief system are causally related: the outputs of the former are received as inputs to the latter, and cause states in such a way that *respect* their contents (e.g., if you experience the apple *as red*, then you come to believe *that it's red*, etc.), even though it doesn't type the states in terms of their content. The law, so conceived, thus wouldn't warrant the claim that there are constitutive connections between some concepts and percepts, i.e. that there are recognitional concepts.

Not only does this view have the virtue of simplicity, but one might claim that there's no loss of explanatory power either. For perhaps the instances of the diverse (putative) laws can all be explained by positing the general law that holds between the states. The law could be put as follows: If one perceives something as an F, then (*ceteris paribus*) one will (be willing to) judge that it's an F. Any particular instance of a perceptual state leading to a judgment state, in a content-respecting way, could thus be subsumed under this general law.

¹² This reply is reminiscent of the reply to Peacocke's appeal to examples that I offered on behalf of the Cartesian in Chapter 4.

A potential problem here, though, is that there must be some way of *delimiting* the range of the general law, since not *all* concepts are such that one is willing to *non-inferentially* apply them on the basis of having certain experiences. The concepts QUARK and ELECTRON, for instance, are obviously not recognitional, since scientists apply them on the basis of experience by presupposing massive amounts of theory. Such judgments are thus clearly inferential. And the defender of recognitional concepts may argue that in order to capture the distinction between these sorts of concepts and concepts like RED, one must posit more specific laws that *do* make essential reference to the contents of the states they subsume. If an argument along these lines can be developed and defended, then perhaps there is an explanatory basis for recognitional concepts after all, although one of a different kind than that suggested by the Chomskyan analogy.

Peacocke's Recent Remarks on Analyticity

In his recent paper “Three Principles of Rationalism” and also in his book *The Realm of Reason*, Peacocke argues that the Quinean challenge does *not* threaten the kind of Concept Pragmatism he favors. But his arguments are quite different from those I offered in Chapter 5. For Peacocke makes the surprising claim that his views *carry no commitment* to an analytic/synthetic distinction that either Quine or Fodor ought to find objectionable. Since this may come as a bit of a shock given everything I’ve said up to this point (and rightly so!), I want to conclude this chapter by considering Peacocke’s recent claims in a bit more detail.

Consider the following passage, in which Peacocke is responding to a recent essay of Fodor's, in which he (Fodor) makes his usual points about Concept Pragmatism presupposing an analytic/synthetic distinction it doesn't know how to draw:

Fodor may be surprised by the extent of my agreement with his points about the analytic/synthetic distinction. The reason that this agreement can exist without incoherence is that we ought to distinguish very sharply between a theory of the analytic and a theory of the a priori. An attack on the applicability and the very intelligibility of the notion of the analytic, *the idea of truth purely in virtue of meaning*, is not thereby an attack on the applicability and intelligibility of the a priori. (Peacocke 2004b, p. 92, my emphasis)

This passage is telling, for it illustrates that Peacocke takes Quine's arguments against analyticity to be directed at what Boghossian calls the “metaphysical notion of analyticity,” according to which a proposition S is analytic if and only if S owes its truth value *completely* to its meaning, and not at all to the “facts” (1996, p. 363). Peacocke makes clear that he joins Quine in rejecting the metaphysical notion of analyticity:

Fourteen years before ‘Two Dogmas’ Quine had already in ‘Truth by Convention’ argued against the idea that any sentence could be true by convention, or indeed true purely in virtue of meaning alone. In my judgement, the arguments Quine gave there, and especially those given later in ‘Carnap and Logical Truth’, constitute one of his most enduring contributions to philosophy. To make clear what it is that I will later be opposing to Quine’s vision I should say that my own view ... is that Quine’s arguments in ‘Truth by Convention’ and ‘Carnap and Logical Truth’ are decisive. ... Contemporary theorists ... should not be

involved with the uninstantiated and uninstantiable notion of ‘true purely in virtue of meaning’. (2004a, p. 27)

Indeed, I think it’s fair to say that the received view in philosophy—shared by Quineans and non-Quineans alike—is that Quine definitively showed that the idea of a proposition (or statement) that is true *solely* in virtue of its meaning ought to be abandoned.¹³

Why did Carnap and the other positivists to whom Quine was reacting accept the metaphysical notion of analyticity? As Boghossian (1996) points out, it’s because *in addition* to providing an analytic theory of *a priori* knowledge, they were *also* concerned to provide a linguistic theory of necessity. Necessary truths, they hoped to show, were the result of conventional decisions about how we use language. What drove them to this was their fear of admitting the existence of necessities *in the world*, which they regarded as metaphysical and mysterious. Nowadays, and perhaps somewhat ironically, it seems that the idea of truth solely in virtue of meaning is regarded as more mysterious than the necessity it was introduced to explain away.

Quite apart from Quine’s attack on convention in “Truth by Convention” and “Carnap and Logical Truth”, the linguistic theory of necessity, like the metaphysical notion analyticity it invokes, can seem *obviously* wrong. As Boghossian says:

¹³ Consider the following rhetorical questions of Boghossian’s: “What could it possibly mean to say that the truth of a statement is fixed exclusively by its meaning and not by the facts? Isn’t it in general true—indeed isn’t it in general a truism—that for any statement *S*, *S* is true iff for some *p*, *S* means that *p* and *p*? How could the *mere* fact that *S* means that *p* make it the case that *S* is true? Doesn’t it also have to be the case that *p*?” (1996, p. 364, original emphasis). Pietroski, another defender of analyticity, also rejects the metaphysical notion of analyticity: “If a sentence is true by virtue of anything it is true by virtue of how the world is. A sentence is true when the world is the way the sentence says it is” (2003b, p. 181). Horwich also rejects metaphysical analyticity: “the *truth* of any sentence will inevitably depend, not merely on what it means—on what proposition it expresses—but also on whether that proposition is true” (1998, p. 152, original emphasis). Some Quineans go on to make the further point that this is true not only of sentences like ‘bachelors are unmarried’ or ‘cats are animals’, but *logical* truths as well. Harman asks, rhetorically: “[W]hy doesn’t the truth expressed by “Copper is copper” depend in part on the general fact that everything is self-identical?” (1999, p. 119). Devitt concurs: “In virtue of what is ‘All unmarrieds are unmarried’ true? The localist could, and I think should, answer as follows: it is true partly in virtue of what it means and partly in virtue of the way the world is, the fact that all unmarrieds *are* unmarried” (1996, p. 22).

In general, I have no idea what would constitute a better answer to the question:

What is responsible for the truth of a given class of statements? than something bland like ‘the world’ or ‘the facts’; and ... I cannot see how a good answer might be framed in terms of meaning, or convention, in particular. (1996, p. 365)

Rey, too, thinks that the positivist appeal to convention was hopeless, right from the start.

Commenting on Carnap’s claim that the reason that *being married* and *being a bachelor* are necessarily incompatible properties is that it’s specified by the relevant meaning postulates, Rey says:

It’s one thing to think that the relations of words to reality is conventional, quite another to think this of the relations among the concepts or properties *themselves* (if it is up to us whether being a bachelor is compatible with being married, why not choose to be both?!). (1993a, p. 66, original emphasis)

Peacocke shares the view of Boghossian and Rey, and agrees that a defense of the analytic and *a priori* need have no truck with conventionalism, stressing that his alternative to Quine “is emphatically not a variant of a Carnapian approach” (2004a, p. 51).

How, then, *does* Peacocke respond to the Quinean challenge? As far as I can tell, his response to Quine in *The Realm of Reason* is two-pronged. First, he points out that Quine’s argument has a lacuna, and does not rule out his kind of Concept Pragmatism, with its attendant claims about the *a priori*. Second, he spells out a detailed rationalistic alternative to Quine’s picture, which he claims is “an elaboration and generalization of the classical rationalist tradition present in different forms in Leibniz, Frege, and some parts of Gödel” (2004a, p. 51). The first prong is meant to show that Quine’s arguments

don't rule out the kind of position Peacocke favors, and the second prong is meant to show that there is a viable, non-Quinean alternative when it comes to providing a theory of meaning and epistemology. For our purposes, the details of Peacocke's alternative do not matter. Rather, I want to say a few things about the first prong of his response to Quine.

We've already seen some of shortcomings of Quine's arguments, and Peacocke points out that Quine essentially assumes the verificationist claim that "an *a priori* content is one whose meaning-constituting evidential conditions are always confirmed by the evidence" (2004a, p. 33). Peacocke claims that a truth-conditional theory of content, which does not take content to be exhausted by evidential relations, is thus not vulnerable to Quine's attack. In fact, Peacocke argues in detail that truth-conditional theories of content have the resources to solve the problems that plague verificationism (2004a, pp. 34-51). Peacocke thus rightfully joins Quine in rejecting the metaphysical notion of analyticity, and goes on to claim that *a priori* contents are true, at least in part, in virtue of their truth-conditions obtaining:

[A]n outright *a priori* method of coming to accept a given content p is one with the following distinctive property. It is entailed by the possession-conditions for the concepts composing p , together with the way their semantic values are determined, their mode of combination in the content p , that use of the given method guarantees that p will be true in any world in which the method is applied. (2004a, p. 33)

Rejecting the metaphysical notion of analyticity, Peacocke is apparently claiming, leaves it open that there could be *a priori* contents, whose *a priori* status is explained by the

possession conditions of the concepts that make them up, and which are true in virtue of their truth-conditions holding. Peacocke thus says that “the Quinean critique of analyticity, both in its epistemological and its constitutive aspects, does not carry over to those approaches to the a priori which reject the notion of analyticity” (2004b, p. 93).

However, assuming I’ve understood him correctly, there are problems with Peacocke’s proposal. First off, note the somewhat odd character of the dialectic. At least since the publication of *A Study of Concepts*, critics have pointed out that Peacocke’s theory of concepts owes an answer to the Quinean challenge.¹⁴ And I argued in Chapter 4 that his most recent attempts to distinguish constitutive from non-constitutive conceptual roles—namely, his appeal to examples—is question-begging against Quine and Fodor. But now, Peacocke is apparently claiming that he in fact *agrees* with Quine and Fodor that there are no analyticities! So we have, on the one hand, Peacocke’s critics claiming that his proposed possession-conditions commit him to an analytic/synthetic distinction, which makes him vulnerable to the Quinean challenge. On the other hand, Peacocke himself is apparently claiming that his view is not vulnerable to the Quinean challenge *precisely because* he agrees with Fodor and Quine that there are no analyticities. What is going on?

A plausible diagnosis, it seems to me, is that Peacocke and his Quinean critics (including me) are, at least to some extent, talking past one another. What bothers Fodor

¹⁴ Even Boghossian says that Peacocke’s appeal to roles that thinkers find primitively compelling won’t work: “[T]hese conditions are insufficient for answering the Quinean challenge: a non-constitutive, though highly obvious, form of inference may also be found compelling because of its form, and not on the basis of inference from anything else. So these conditions cannot be what distinguish between a constitutive and a non-constitutive inference” (1996, p. 391n38). Later in the same paper, Boghossian says that he doesn’t have a fully thought-through proposal about how to distinguish constitutive from non-constitutive roles, but somewhat puzzlingly tells us in a footnote to see Peacocke’s *A Study of Concepts* “for a good start” (1996, p. 391n43, my emphasis). Needles to say, it’s not clear how to square these claims. I take this as further evidence that the issues in this neck of the woods are incredibly difficult and not all that well understood.

and other Quineans about Peacocke's proposal isn't that it's committed to the existence of analyticities, *understood in the metaphysical sense*. That is, Quineans do not claim that Peacocke's project founders because it's committed to the existence of truths that are true *in virtue of meaning alone*, i.e. metaphysical analyticities.¹⁵ Rather, what worries Quineans is that Peacocke is committed to an *epistemic* notion of analyticity, i.e. a notion of analyticity that is meant to do justificatory work.

Consider, for example, Peacocke's claim that certain contents can be known *a priori*, in part, because the concepts that make them up have the possession conditions that they do: "I think the a priori is possible only because certain principles and transitions are mentioned in the possession condition for a given concept" (2004b, p. 93). Indeed, according to the Moderate Rationalism defended in *The Realm of Reason*, "for any a priori way of coming to know a given content there is a substantive explanation of why it is an a priori way of coming to know that content, an explanation which involves the nature of the concepts in the given content" (2004a, p. 155). As we've seen, in taking certain principles and transitions (or roles) to be concept-constitutive, it seems that Peacocke is committed to an analytic/synthetic distinction. And the present point is that the distinction Peacocke needs here is presumably epistemic.

There are, I think, two dimensions to the epistemic nature of such analyticities. First, as we just noted, Peacocke takes the concept-constitutive roles to underwrite our *a priori* knowledge of certain contents. This is not to say that such concept-constitutive roles guarantee the *truth* of certain contents in which the concepts figure. I take this to be Peacocke's point in rejecting the notion of 'truth purely in virtue of meaning', i.e.

¹⁵ Well, perhaps some of them do claim this, but this is plausibly because the metaphysical and epistemic dimensions of analyticity are not distinguished as often as they should be.

metaphysical analyticity. But rejecting metaphysical analyticity, and coupling his claims about the possession conditions for certain concepts with a truth-conditional theory of content, is not enough to stave off Quinean attacks. For the analyticities that Peacocke is committed to do have epistemic import: they are, after all, part of what makes *a priori* knowledge possible! Given Peacocke's Moderate Rationalism, it's thus very odd to find him saying that "we ought to distinguish very sharply between a theory of the analytic and a theory of the *a priori*" (2004b, p. 92). For according to Moderate Rationalism, it's the constitutive possession-conditions on concepts, which presumably carry a commitment to the existence of analyticities, that explain why certain contents can be known *a priori*.

The second epistemic dimension has to do with the fact that Peacocke takes the specification of concept-constitutive roles to be the business of *a priori*, philosophical investigation of concepts. I argued above that, at least in certain cases, this need not commit him to the claim that the lexical concepts have internal structure, or that there are analyticities in the concept-containment sense. However, if certain principles and transitions are concept-constitutive, and which of them are so can be discovered *a priori*, then presumably this is a result of their being analytically connected to the concepts. That is, Peacocke seems to be committed to the traditional view of analyticities according to which they are readily available to competent users of concepts (or perhaps highly reflective philosophers).

But this epistemic notion of analyticity is precisely what Quine and Fodor will claim Peacocke can't presuppose. For Quine's challenge to defenders of such a notion is to specify a principled distinction between, for example, the transitions in thought that

one accepts as a result of the meaning of concepts, and those that one accepts as a result of one's empirical theory of the world. This is, of course, precisely why those who defend analyticities on Quinean, explanatory grounds *give up on* the traditional epistemic role that the analytic was often thought to play. For by doing so the analytic/synthetic distinction becomes just one more piece of our empirical theory of the world.

Conclusion.

In this chapter I've argued that there are good explanatory reasons for believing that there are constitutive connections among concepts, and that all of the extant alternative explanations of the analytic data are insufficient. These two arguments, if correct, constitute a powerful empirical case for Judgment Pragmatism. I also argued, however, that although there is not a *similar kind* of explanatory basis for the existence of recognitional concepts, there is a promising strategy for defending such concepts on explanatory grounds of a different kind, namely, by an appeal to psychological laws. Of course, this strategy needs to be articulated and defended further. But since neither Quine's nor Fodor's arguments against the analytic/synthetic distinction are decisive, and there are good reasons for positing constitutive connections among concepts in general, perhaps Pragmatists can assume for the nonce that some concepts are constitutively tied to perceptual judgments.

Furthermore, we've just seen that Peacocke's own recent reply to the Quinean challenge apparently misses the mark. While it's true that part of the attack on the analytic/synthetic distinction involved an attack on the notion of truth solely in virtue of meaning, Peacocke can't reply to Quine *merely* by pointing out that he avoids any

commitment to such a notion. For Quine challenged not only the metaphysical notion of analyticity, but *also* its epistemic counterpart. And, as I just suggested, since it appears that Peacocke is committed to an epistemic analytic/synthetic distinction, he is thus apparently still vulnerable to the Quinean challenge after all. One of the great virtues of taking a theory of concepts to have its proper home in a theory of psychological explanation is that it frees Concept Pragmatism from such vulnerability.

CHAPTER 7

RATIONALITY, CONTENT, AND FORM: THE DIALECTICAL ROLE OF FREGE CASES

The point of appealing to senses in semantic theories is to provide the extra degree of freedom—the ‘mode of presentation’ of a referent—that allows extensionally distinct belief states to be type-identical and allows type-distinct belief states to be extensionally identical. My guess is that, when all the dust has settled, it will be mental representations—syntactic structures in the Language of Thought—that play this semantical role.

-Jerry Fodor (1994, p. 24)

Introduction

Informational Atomism is a species of semantic theory according to which reference, or denotation is all there is to meaning. In the philosophy of mind and language, such theories have a variety of names: ‘Russellianism’, ‘Millianism’, ‘Direct Reference Theory’, or ‘the “Fido”-Fido theory’. What all these views have in common is the idea that reference is *exhaustive* of meaning.¹ Such theories claim, for instance, that the meaning of a proper name is its bearer, and that the meaning of a predicate is a property. For our purposes, this can be put in terms of the corresponding concepts: for instance, the content of the concept DANIEL BOONE is the individual, Daniel Boone, and the content of the concept PIONEER is the property, *being a pioneer*.

There are well known problems with any such purely referential theory of meaning. As we noted in Chapter 1, two problems stand out: those raised by Putnam’s Twin-Earth examples, which putatively show that concepts with (what we pre-

¹ This is true at the level of primitives, in any case. Also, it’s worth pointing out that such theorists may adopt an inferential role (i.e., non-referential) account of the logical constants, as indeed Fodor (1990) himself once did; but see Fodor (2004a).

theoretically think of as) the *same* content can *differ* in reference, and those raised by so-called ‘Frege cases’, which putatively show that concepts that *differ* in content (again, pre-theoretically) can nevertheless be *identical* in reference. As we noted, Putnam and Frege together apparently show that conceptual content and reference are independent *in both directions*. Any adequate theory of concepts must account for both Twin-Earth cases and Frege cases. In this chapter I put the former to one side and focus on the latter, and consider their dialectical role in the debate between Concept Cartesians and Concept Pragmatists. I ultimately argue against the Cartesian’s treatment of Frege cases, but suggest that Peacocke is also mistaken in thinking that they support Knowledge Pragmatism. My suggestion should by now be familiar: Embedding a theory of concepts in an explanatory psychology allows for a theory that splits the difference between Informational Atomism and Knowledge Pragmatism.

Peacocke’s ‘General Argument’ for Pragmatism

In Chapter 4, I considered the first of Peacocke’s two arguments for Concept Pragmatism—the appeal to particular examples—and argued that it would not convince a Cartesian. Whereas the appeal to examples is (I suggested) best viewed as an argument in favor of mere Judgment Pragmatism, Peacocke’s second, ‘general argument’ is meant to support the stronger form of Pragmatism, Knowledge Pragmatism. Recall that the distinction here is between a Pragmatism that individuates concepts in terms of mere judgment and a Pragmatism that individuates concepts in terms of knowledge (i.e., one that posits *epistemically* individuated concepts). I take as a starting point this general argument for Knowledge Pragmatism, as it will serve as a useful springboard into our

discussion of the dialectical role of Frege Cases. I argue that it presupposes a treatment of Frege cases that the Cartesian will deny.

Peacocke summarizes this argument as follows:

It is a general argument in support of Fregean IRS [Inferential Role Semantics] accounts that they can explain facts about the rationality of making given judgments in given circumstances, and thereby, also account for the epistemic status of those judgments. (2000a, pp. 333-334)

Peacocke's favorite example involves the concepts SQUARE and (REGULAR-) DIAMOND, which are distinct but coreferential recognitional concepts. It's *prima facie* plausible that perceiving something *as a square*, in optimal conditions, rationally requires (a willingness to make) the judgment that *that's a square*, but not the judgment that *that's a diamond*. Indeed, assuming the thinker doesn't know that *being a square* and *being a diamond* are identical properties, the latter judgment will be *irrational* in those circumstances. Moreover, the former judgment will plausibly be justified and count as knowledge, whereas the latter judgment, if made, won't.

If SQUARE and DIAMOND are epistemically individuated, Peacocke claims, then these differences in rationality and epistemic status can be explained. For, if it's constitutive of SQUARE that judgments that objects are square are rationally non-discretionary when accompanying perceptions of objects as squares, and constitutive of DIAMOND that judgments that objects are diamonds are rationally non-discretionary when accompanying perceptions of objects as diamonds, then only the former judgments will count as knowledge when thinkers are perceiving objects as squares and taking their experience at face value. Given such considerations, Peacocke claims the following:

[W]hen the rationality of some particular transition turns on the identity of the particular concepts it involves, the rationality of the transition is grounded ultimately in the IRS-involving possession conditions for those particular concepts. In fundamental, constitutive cases, the rationality of the transition can be founded in constitutive features which contribute to the individuation of the concept and are mentioned in its IRS. (2000a, p. 334)

Such an account thus “promotes an integration of the theory of concepts with epistemology” (200a, p. 334), which, Peacocke assumes, is a good thing.

In the context of a debate between a Pragmatist and a Cartesian about concept possession, however, Peacocke can’t simply *assume* that “an integration of the theory of concepts with epistemology” is a good thing. For as we’ve seen, those who approach concepts from the perspective of psychological explanation doubt that normative epistemology places substantive constraints on a theory of concepts, and will thus be reluctant to go in for such integration. Of course, they needn’t outright deny that epistemology places *any* constraints on a theory of concepts. If epistemologists discover that a judgment’s having some property P is necessary and/or sufficient for its being justified, or for a thinker to be entitled to make it (or for it to be rationally non-discretionary, etc.), then, *prima facie*, one’s theory of concepts ought not preclude too many of our judgments from having P. For instance, a theory of concepts that implies that we’re *rarely* justified in making perceptual judgments is (*ceteris paribus*) worse than a theory that doesn’t have this implication. But a theorist can allow this without allowing that some concepts are *essentially* tied to judgments with property P. That is, a theorist can admit that normative epistemology places some minimal constraints on a theory of

concepts without admitting the kind of ‘integration’ Peacocke has in mind. Peacocke thus can’t take integration (in his sense) with epistemology as *itself* a desideratum on a theory of concepts.

I am thus not impressed by Peacocke’s claim that Knowledge Pragmatism ought to be endorsed because it can explain epistemological facts about the rationality and epistemic status of certain judgments. But in saying this, someone might object, aren’t I implicitly adopting *a priori* views about which theories explain what facts? And doesn’t this run counter to the approach to the meta-level question about concepts I’ve been suggesting we ought to adopt, namely, that one take concepts to have their proper home in an explanatory psychology? After all, if I’m a good Quinean about how we ought to approach the task of providing a theory of concepts, then surely I should accept that (*ceteris paribus*) theories that explain more are better than theories that explain less.

To see how one might respond to this, recall the strategy I suggested for the Judgment Pragmatist in Chapter 3. I claimed that one can draw upon a theory of concepts in answering normative questions about the epistemological status of certain judgments, but that this needn’t involve a commitment to the claim that the concepts themselves are constitutively tied to rationality and knowledge, with all of their normative dimensions. The Judgment Pragmatist thus potentially has the resources available to do all of the explanatory work here that needs to be done. One might then make the further claim—implicit in some of Fodor’s discussions—that a theory of concepts isn’t the right kind of thing to provide explanations concerning the rationality and epistemic status of certain judgments.

What explains facts about the rationality and epistemic status of judgments on Fodor's view? Like his theory of content, Fodor's epistemology is externalist. Although he rarely discusses issues in epistemology, he hints at various places that he favors some or other version of reliabilism. For instance, consider the following passage, in which Fodor discusses the relation between semantic and epistemic access:

Informational semantics says that it's because the mediation between dogs and DOG-tokens is reliable that there is a community of *dog*-thinkers, creatures whose mental processes fall under the intentional laws about *dog*-thoughts. Just so, epistemologists (have been known to) say that it's the reliability of the mediation between dogs and one's *dog*-thoughts that *justifies* one's knowledge claims about dogs. This convergence of views is all to the good, of course; the requirements that epistemology places upon epistemic warrant ought to be ones that the theory of content allows many of one's beliefs to actually meet. (1998a, pp. 75-76, original emphasis)

Applying this to the square/diamond case, the rationality and epistemic status of one's judgment that *that's a square* or *that's a diamond* will depend upon whether the mediation between squares and diamonds and the respective judgments are reliable ones. Although one would of course like to see reliabilism worked out in detail, the question of whether or not this can be done successfully is moot.² The important point here is that such an account, if it can ultimately be spelled out, will *not* appeal to the possession conditions for concepts in explaining facts about rationality and justification.

² Debates about reliabilism concern foundational issues about the form an epistemological theory ought to take. For a representative sample of the large literature on the internalism/externalism debate in epistemology, see the papers in Kornblith (2001), and Sosa and BonJour (2003).

Now, one might think that it's a bad idea to rest the defense of one's theory of concepts on an epistemology that's yet to be worked out in full. Of course, one can't accuse Fodor here of holding his theory of concepts hostage to particular epistemological claims. But he is committed to claiming that the data Peacocke appeals to in his general argument for Knowledge Pragmatism ought to be explained by one's epistemology, and not one's theory of concepts. And this alternative explanation requires that there be a defensible reliabilist epistemology. Peacocke expresses his skepticism about the prospects of providing such an epistemology as follows:³

The property of a transition of tending to lead to true judgements (or to do so when its premises are true) is not by itself enough to make a transition entitling. Pure reliability of a transition is not by itself enough to make the transition entitling, as, in my judgement, many examples in the discussion of pure reliabilism over the years have shown. If I am wrong about this, and there is a form of pure reliabilism that can capture a rationality requirement, all well and good. For what it is worth, I myself am sceptical that any such form of reliabilism exists. (2004a, pp. 11-12)

Given the current context, though, Peacocke is surely wrong to suppose that all is 'well and good' if there's a form of reliabilism that explains the relevant facts about the rationality and epistemic status of judgments. For this would apparently undermine his general argument, since one *wouldn't* need to posit epistemically individuated concepts to explain the facts he points to. I take it, then, that the viability of Peacocke's general argument for Knowledge Pragmatism does depend (albeit indirectly) on facts about rationality and justification not being reconstructed in reliabilist terms.

³ Peacocke speaks here in terms of entitlement rather than justification or knowledge, but this is irrelevant.

Elsewhere, when writing in the context of the debate between Cartesianism and Pragmatism, Peacocke makes it clear that he does not think Cartesians like Fodor can give an explanation of these facts that is ‘outside’ the theory of concepts:

[N]ot all kinds of reliability yield knowledge, as epistemologists have long known, and as the *square/regular-diamond* example further illustrates. It seems to me that the Fregean IRS theorist has the resources to explain satisfactorily why not all kinds of reliability yield knowledge. I doubt that Fodor’s approach does.

(2000a, p. 334)

What, exactly, is the crucial fact here that needs explaining? What would it be for ‘not all kinds of reliability to yield knowledge’? Here’s what I think is at issue. Consider again the square/diamond case: the judgment that *that’s a diamond* will be reliably true given a perception that represents something *as a square*; this is true *even though*, in the given circumstances, the judgment that *that’s a square* will count as knowledge and the judgment that *that’s a diamond* won’t. It’s *this* fact—and presumably other facts like it—that Peacocke thinks gives Knowledge Pragmatism a leg up on Cartesianism. The question we face, then, is the following: Since Knowledge Pragmatism offers an explanation of this fact, should we adopt it on grounds of explanatory power?

The answer to this, I submit, will depend upon the answer to another question, namely, What determines whether a thought or judgment is rational? One answer to the latter question is that whether a thought or a judgment is rational turns on its content, and the identity conditions for the concepts that make up it up. If this is correct, then one *might* think that the crucial fact Peacocke points to provides *prima facie* support for

Knowledge Pragmatism.⁴ But Peacocke can't simply *assume* that issues about the rationality of thought turn on issues concerning content. For as we'll see, on Fodor's view issues about rationality turn on the form, not the content of thought. It seems to me, then, that we must get clear on this question *before* we can address the question of whether Peacocke's Knowledge Pragmatism is more explanatorily powerful than either Judgment Pragmatism or Concept Cartesianism. To do this, we must come to grips with the issues raised by Frege cases. Let's begin with the problem they pose for purely referential theories of meaning.

Substitution Failures, Senses, and MOPs

If a purely referential theory of meaning—e.g. Informational Atomism—is true, then we ought to be able to freely substitute coreferential concepts for one another *salva veritatae*. The reason for this is simple. If the content of a concept is exhausted by its referent, then two concepts with the same referent ought to make the same contribution to the content of an entire thought. For if there were something that one *but not the other* of two coreferential concepts contributed to the content of a thought, then their content wouldn't be exhausted by reference. In short, a purely referential theory of meaning implies that coreferential concepts are *synonyms*. On the assumption that synonyms have the same effect on the truth-value of thoughts containing them, it seems that the truth or falsity of a thought containing one concept ought to be unaffected by substituting another, coreferential concept in its place. It's for this reason that Fodor admits that “the status of

⁴ I argue below, though, that *even if* facts about rationality turn on facts about content, this needn't be taken as support for Knowledge Pragmatism.

conceptual atomism depends, rather directly, on whether coreference implies synonymy” (1998a, p. 14).⁵

But, *prima facie*, coreference does *not* imply synonymy. Consider the following sets of coreferential concepts: HESPERUS and PHOSPHORUS, WATER and H₂O, SQUARE and DIAMOND, HEAT and MEAN MOLECULAR KINETIC ENERGY, CORIANDER and CILANTRO, JOCASTA and OEDIPUS’ MOTHER. Despite the fact that the members of each pair are coreferential, it’s *prima facie* plausible that they differ in content, and it’s very easy to generate substitution failures involving such concepts that illustrate this. First, though, consider some sentences that do *not* generate a substitution failure:

- (1) Hesperus is Hesperus.
- (2) Hesperus is Phosphorus.

On the face of it, (1) and (2) differ in meaning. For it certainly seems that (2) might have been false, whereas (1) is necessarily true (since everything is necessarily identical with itself). One might take this apparent difference in modal status to account for the apparent difference in meaning. The idea would be that (1) and (2) can’t have the same meaning since (1) is *a priori* and necessary, and (2) is *a posteriori* and contingent. However, as Kripke (1980) persuasively argues, although (2) is known *a posteriori* it is nevertheless *necessarily* true, just as (1) is. I thus take it that if (1) and (2) differ in meaning, it’s *not* because of a difference in their modal status.

⁵ Fodor ought to say ‘Informational Atomism’ here, not ‘Conceptual Atomism’. Given the discussion in Chapter 5, it should come as no surprise to find Fodor writing under the assumption that the informational and atomistic components of Informational Atomism can’t come apart. But as I argued there, they can. It’s the informational component of Informational Atomism whose status depends on whether coreference implies synonymy. Atomism is *not* at issue here.

This is where Frege cases come in. For they present cases in which the putative difference in content does not turn on any modal differences. Consider the following sentences:

(3) John believes that Hesperus is Hesperus.

(4) John believes that Hesperus is Phosphorus.

Prima facie, the truth of (3) doesn't imply the truth of (4), since it's possible that John doesn't know what we know, namely, that Hesperus = Phosphorus = Venus. This creates a problem for purely referential theories of meaning. For since 'Hesperus' and 'Phosphorus' are coreferential, they ought to make the same semantic contribution to the sentences in which they occur, and thus be substitutable for one another *salva veritatae*. But this doesn't seem to be the case. For it's *prima facie* plausible that (3) can be true and (4) false. Since the only difference between (3) and (4) is a coreferential term, it thus seems that purely referential theories of meaning violate substitutivity.

Some referential theorists bite the bullet here, and claim that the thought expressed by (3) is *identical* to the thought expressed by (4). Prominent among them is Nathan Salmon (1986), who claims that "anyone who knows that Hesperus is Hesperus knows that Hesperus is Phosphorus, no matter how strongly he or she may deny the latter" (p. 83). Of course this is highly counter-intuitive. For I take it that most, if not all, speakers have a *very strong* intuition that the truth of (3) does not imply the truth of (4). Salmon thus owes us an explanation of our faulty intuitions; and he provides one. He argues, in effect, that when we think that (3) could be true and (4) false we're confusing pragmatics for semantics. Details aside, the basic idea is that while (3) and (4) have the same semantic content—i.e. they express the same thought (or proposition)—they

nevertheless differ in their pragmatic implications. For instance, (3) implies that John is thinking of Hesperus as ‘Hesperus’ and not as ‘Phosphorus’. Given this implication, which he claims is merely pragmatic, we come to think that (3) could be true and (4) false, when in fact they literally express the same thought and thus have the same truth condition.⁶

But one might complain that the intuition that (3) and (4) can differ in truth-value is *too* strong to be trumped by what may be an otherwise attractive theory.⁷ For it can seem obvious and undeniable that (3) and (4) can differ in truth-value, and some may feel it’s within their rights to claim that any theory that implies otherwise ought be rejected. Of course, it’s not at all obvious how to settle conflicts between our intuitions and our theories. As Fodor once quipped in another context, perhaps the best thing to do in such situations is to get your intuitions fixed! In any case, it’s examples like (3) and (4) that have traditionally led theorists away from purely referential theories and towards Fregean theories, which recognize an aspect or layer of meaning that *goes beyond* mere reference.

While there’s a sense in which coreferential terms have the same meaning, these theorists claims, there’s another sense in which they clearly differ in meaning, since they differ in what Frege called ‘cognitive significance’. Thus

(1) Hesperus is Hesperus

is *uninformative*, while

(2) Hesperus is Phosphorus

⁶ Soames (2002) also defends a view of this kind.

⁷ The bullet-biting view has been attacked by both proponents and opponents of a purely referential theory of meaning. See, e.g., Crimmins and Perry (1989), Braun (1998), Devitt (1996), Pietroski (2000), and Recanati (1993).

is potentially *informative*. Fregeans posit senses in an attempt to explain why coreferential terms can differ in such properties. On their view, it's because a thinker can grasp the sense of ‘Hesperus’ without grasping the sense of ‘Phosphorus’ that (1) is uninformative and (2) is potentially informative. Moreover, it’s a difference in sense that explains why it would be irrational for a thinker to deny (1), while it can be perfectly rational to deny (2). It also explains why (3) and (4) can differ in truth-value. Fregeans will claim that the thought (or proposition) expressed by (1) differs from the thought expressed by (2), and the thought expressed by (3) differs from the thought expressed by (4). Positing senses thus captures our intuitions about sentences like (1) and (2), and (3) and (4), and offers a potential explanation of relevant facts about the rationality and epistemic status of judgments.

What *are* senses? What, for instance, is the sense of ‘Phosphorus’? Frege says that a sense is a ‘mode of presenting’ a referent (a MOP), and many Fregeans follow Evans’ (1982) gloss on MOPs as ‘ways of thinking’ about a referent. A typical claim is that a sense or MOP of (say) ‘Phosphorus’ is a *description* that Venus satisfies, e.g. ‘the brightest star in the morning sky’. On this sort of view, it is natural to take senses to be determined by the inferential relations that ‘Phosphorus’ bears to other terms. Fregeans thus typically claim that the aspect of meaning that goes beyond reference will be provided by an conceptual/inferential role semantics. A less typical claim is that MOPs are *non-descriptive*, e.g., that they involve causal properties that determine reference (Devitt 1996). For our purposes, the important thing to note is that Fregeans propose that there’s a *semantic* difference between (1) and (2), and (3) and (4). Unlike many of those who endorse purely referential theories of meaning, Fregeans take each of these to

express distinct thoughts, and thus take MOPs to be identical with Fregean senses (/meanings/contents).

Peacocke's theory of concepts, of course, is guided by just this line of reasoning. For his theory of concepts put forward in *A Study of Concepts* is built around a criterion of concept individuation based on the Fregean notion of 'informativeness':

Concepts *C* and *D* are distinct if and only if there are two complete propositional contents that differ at most in that one contains *C* substituted in one or more places for *D*, and one of which is potentially informative while the other is not.

(1992a, p. 2)

Since (2) is potentially informative, it follows from Peacocke's informativeness criterion that HESPERUS and PHOSPHORUS must be distinct concepts, despite the fact that they're coreferential. He combines this with the standard Fregean view that what distinguishes coreferential concepts is conceptual/inferential role. Peacocke claims, moreover, that this combination implies that reasons and rationality are constitutively tied to concepts:

What all Fregean IRS theories, of whatever stripe, agree upon is the central place of reasons and rationality in the individuation of concepts. Reasons for making judgements are central in any Fregean theory, since the informativeness criterion appeals to what can be reasonably judged in given circumstances. (2000a, p. 332)

According to Peacocke, then, the informativeness criterion leads to the claim that there are constitutive connections between rationality and concepts. For the informativeness criterion individuates concepts at the level of Fregean sense, and sense properties are what explain the relevant facts about the rationality and epistemic status of certain judgments—e.g., why it can be rational to deny (2), but irrational to deny (1).

It should be clear by now that I don't think Peacocke is right about this implication of a Fregean conceptual/inferential role semantics. I argue below that one can accept the latter without making the *further* claim that concepts are constitutively tied to reasons and rationality. For now, though, let's just note that it's surely a virtue of the informativeness criterion that it gets some crucial facts right here: coreferential concepts—WATER and H₂O, SQUARE and DIAMOND, CORIANDER and CILANTRO, JOCASTA and OEDIPUS' MOTHER—are surely distinct, and psychological explanations require that they be treated as such. John is surprised to find out that coriander is cilantro, and Oedipus is disgusted when he discovers that Jocasta is his mother. Explaining and predicting such behavior requires that the concepts that are the constituents of the relevant mental states—i.e., CORIANDER and CILANTRO, and JOCASTA and MOTHER—be treated as distinct. I take it that any theory that implies otherwise ought to be rejected.

As we noted above, Informational Atomism implies that coreferential concepts are synonyms. If it's a law that tokenings of WATER covary with (instances of) the property *being water*, and (instances of) *being water* = (instances of) *being H₂O*, then it's a law that WATER covaries with (instances of) *being H₂O*. Informational Atomism thus implies that WATER and H₂O are synonyms. And the same goes for other coreferential concepts. Why does Fodor adopt a theory that has this patently unintuitive consequence? Because, if coreference is not sufficient for synonymy, then at least some aspect of meaning must be determined by the *internal* role of term. In other words, denying that coreference implies synonymy invites a conceptual/inferential role semantics. And Fodor would rather live with the counter-intuitive claim that coreferential concepts are

synonyms than adopt a theory of concepts that requires an appeal (however small) to conceptual/inferential role.

Of course, one can believe that there is water in the cup without believing that there is H₂O in the cup, since one can have the concept WATER without having the concept H₂O. Indeed, this is precisely the situation that the vast majority of humans have found themselves in. Even if coreferential concepts are synonyms, then, they are nevertheless distinct. Given this, Informational Atomism commits Fodor to the claim that “*content* individuation can’t be all there is to *concept* individuation” (1998a, p. 15, original emphasis). Or, as he puts it elsewhere, “if intentional contents are broad, then something other than content must be able to distinguish between propositional attitudes” (1994, p. 49). What *else* could be involved in concept individuation? If coreferential concepts are distinct, and they are distinct *not* in virtue of any differences in meaning or content, as Informational Atomism requires, then Fodor must appeal to *non-semantic* differences to distinguish them. But if there’s no semantic difference between

(5) John believes that there is water in the cup

and

(6) John believes that there is H₂O in the cup

then what explains the fact that the *truth* of (5) does not imply the *truth* of (6)? On Fodor’s view, the key is to understand that such substitution failures can indicate a difference in belief *state*, *without* indicating a difference in belief *content*.

There is widespread agreement that beliefs are to be analyzed as *relations*, e.g. between people and propositional contents. For an informational semantics, taking beliefs to be two-place relations will not do, though, since it says that the that-clauses in

(5) and (6) express the *same* proposition. That is, according to informational semantics, the proposition that there is water in the cup *just is* the proposition that there is H₂O in the cup. Suppose that beliefs are mere two-place relations. It follows that if John bears the belief-relation to the proposition that there is water in the cup, he *thereby* bears the belief-relation to the proposition that there is H₂O in the cup. *Prima facie*, this is the wrong result. So it looks as if informational semantics *can't* take beliefs to be mere two-place relations.

In light of this, Fodor suggests instead that we conceive of beliefs (and other propositional attitudes) as *mediated* relations between people and propositions. That is, he suggests that beliefs should be analyzed as *three*-place relations between people, propositions, *and* mediating representations. On this view, the truth of (5) still implies that John stands in the belief-relation to the proposition that there is water in cup. It's just that John stands in that relation *by standing* in a relation to an internal representation that expresses the proposition. The truth of (6) implies that John stands in the very same belief-relation to the proposition that there is water (/H₂O) in the cup. But in this case he stands in that relation by standing in a relation to a *different* internal representation that expresses the *very same* proposition. Distinct mediating representations can thus be identical in their content.

It's worth pointing out that Fodor has long held that propositional attitudes are mediated relations to propositions, even before he opted for a thoroughgoing externalist semantics (Fodor 1975, 1980). In these earlier days, however, the claim was motivated by his computationalism. The guiding idea is that if mental processes are computational, then it's plausible that mental states are relations to internally structured representations,

which are individuated by their formal, syntactic properties.⁸ For computational processes are sensitive only to the purely formal properties of the representations that they're defined over. Indeed, it's this fact that provides both the potential power *and* limitation of the computational theory of mind (Fodor 1983, 2000a; but see Carruthers 2003).

In the earlier days, though, Fodor combined his computationalism with an *internalist* theory of content, according to which the contents of states that figure in psychological laws are fixed by properties entirely within the skin of the organism (Fodor 1987, 1991b). On this view, the content that's necessary for psychological explanation supervenes on the internal properties of the organism's nervous system. This 'narrow' theory of content sits nicely with the computational theory of mind, since one can at least see how there could be computationally sufficient conditions for being in a mental state with a certain (narrow) content. But once Fodor gave up on the view that psychological explanation requires a notion of narrow content, and went in for a purely externalist (/informational) theory of content, he faced the difficult task of reconciling this with the computational theory of mind. For if content is determined by certain *external* (e.g., informational/causal/nomic, etc.) relations that an organism bears to its environment, then

⁸ In what follows I use 'formal' and 'syntactic' interchangeably. Strictly speaking, of course, these are not the same thing. For computational relations could be defined over representations that do not have a syntax. Rotating a mental image, for example, might be a computational process that is sensitive only to the formal properties of the representation. But mental images don't have a syntax. Being syntactic is thus one way of being formal, but representations needn't have syntactic descriptions in order for computational processes to be defined over them. As Fodor says: "What makes syntactic operations a species of formal operations is that being syntactic is a way of not being semantic. Formal operations are the ones that are specified without reference to such semantic properties of representations as, for example, truth, reference, and meaning. Since we don't know how to complete this list (since, that is, we don't know what semantic properties there are), I see no responsible way of saying what, in general, formality amounts to. The notion of formality will thus have to remain intuitive and metaphoric, at least for present purposes: formal operations apply in terms of the, as it were, shapes of the objects in their domains" (1980, p. 227).

it is far from clear how content and computation are supposed to hang together. As Fodor says:

[I]t is, to put it mildly, obscure how a thing could satisfy the conditions for having its *external* relations simply in virtue of having the *internal* relations that it does. ... But if internal relations don't guarantee external relations, then computational relations don't guarantee intentional relations. ... Computational mechanisms implement intentional laws only if computational properties can somehow guarantee intentional ones. But there seems to be no way they could do so on the assumption that the metaphysics of content is informational. (1994, pp. 14-15)

Since Fodor does not wish to give up on informational semantics, he has to show how the computational theory of mind is compatible with a purely externalist theory of content after all. In his Jean Nicod lectures—published as *The Elm and the Expert*—he attempts to do just this.

For our purposes, the interesting thing to see here is that the problem of reconciling computationalism with externalism about content *mirrors* the constraint that Frege (and Twin) cases impose on a theory of concepts. The truth of (5) does not imply the truth of (6); that John believes that there's water in the cup does not imply that John believes that there's H₂O in the cup. As we've seen, this fact is *prima facie* puzzling if informational semantics is true, since the latter implies that WATER and H₂O are synonyms. The point can be put in terms of the relation between computation and content: Frege cases are cases in which *distinct* computational mechanisms implement beliefs with *identical* contents. In other words (and roughly put), if Frege cases show that reference doesn't determine sense, then (assuming computationalism) they show that

content relations don't determine computational ones. (Twin cases, of course, illustrate the same point but from the other direction: the *same* computational mechanism can implement beliefs with distinct contents, where content is specified externalistically.)

We can now see how Fodor's treatment of Frege cases links up with his computational theory of mind. Since informational semantics precludes an appeal to a semantic difference to explain why the truth of (5) does not imply the truth of (6), mediating internal-representations are perfectly suited to task. As we noted above, computationalism *already* requires that such representations be individuated by their formal (i.e., non-semantic) properties. A computational theory of mind can thus happily be combined with an informational semantics that can handle Frege cases, so long as mediating internal-representations can play the role of Fregean MOPs. Fodor thus says:

It's really the basic idea of RTM that Turing's [computational] story about the nature of mental processes provides the very candidates for MOP-hood that Frege's story about the individuation of mental states independently requires. If that's true, it's about the nicest thing that ever happened to cognitive science.

(1998a, p. 22)

To sum up, then, the idea is this. While John's 'water-MOP' has the same *content* as his ' H_2O -MOP', they are nevertheless syntactically distinct. For they are, as it were, *spelled differently*: they have different formal properties, and will thus differ in the causal relations they enter into in virtue of having those properties. This is why John in (5) will behave differently than John in (6). Informational Atomism thus apparently forces Fodor to reject the Fregean claim that MOPs are identical with senses (/meanings/contents), and claim instead that MOPs are the representational vehicles of thoughts, which are

individuated by their purely formal properties. Fodor's burden is to make this claim plausible, for otherwise coreference will not be sufficient for synonymy and Informational Atomism will be in trouble. In what follows I consider Fodor's attempt to do just this, and argue that it fails. However, as we'll see, a defeat for Cartesianism here is *not* tantamount to a victory for Peacocke's Knowledge Pragmatism. Indeed, there's a plausible middle ground.

Mates, Kripke, and MOPs

Fregeans argue that substitution tests show that there's a semantic difference between coreferential but distinct concepts. Recall their argument: since coreferential concepts are not substitutable for one another *salva veritatae*—as (3) and (4), and (5) and (6) show—and synonyms *are* freely substitutable for one another *salva veritatae*, it follows that coreferential concepts are *not* synonyms. This putatively shows that there's a semantic difference between coreferential but distinct concepts, *contra* Informational Atomism.

Fodor rejects this argument because he thinks that *even synonyms* are not substitutable for one another *salva veritatae*. Following Mates (1950), he claims that synonyms can fail substitution tests, and thus that substitution tests are not tests for synonymy. Consider the concepts LAWYER and ATTORNEY, which would seem to be synonymous if any concepts are. Assuming that they are, the following sentences should *not* generate a substitution failure:

(7) John is an attorney.

(8) John is a lawyer.

And they don't. The truth of (7) guarantees the truth of (8). But now consider:

(9) John believes that all attorneys are attorneys.

(10) John believes that all attorneys are lawyers.

Perhaps (9) is true if and only if (10) is true. But while it doesn't seem possible to wonder whether (9) is true, it *does* seem possible to wonder whether (10) is true. What might lead us to doubt (10)? Well, we might wonder whether John acquired ATTORNEY and LAWYER in such a way that he believes that there *is* a difference, however slight, between attorneys and lawyers. For instance, he might think that lawyers are just like attorneys, except only the latter are members of the American Bar Association. Surely this is at least possible. But if it is, then the following sentences apparently *do* generate a substitution failure:

(11) Bill does not wonder whether John believes that all attorneys are attorneys.

(12) Bill does not wonder whether John believes that all attorneys are lawyers.

Such iterated attitude-contexts are known as ‘Mates cases’, and they at least *suggest* that substitution tests are *not* tests for synonymy. In this case, the truth of (11) does not guarantee the truth of (12), despite the fact that ATTORNEY and LAWYER are synonyms. Fodor thus says that Mates cases “[throw] doubt on the claim that failures of substitution salva veritate in belief contexts are ipso facto arguments for nonsynonymy” (1990, p. 165).

If Mates cases do indeed show that synonymous expressions can fail substitution tests, then this would apparently undermine the claim that Frege cases show that there must be a *semantic* difference between coreferential but distinct concepts. Indeed, Fodor uses Mates cases to motivate his claim that MOPs are not identical with Fregean senses. Why do Mates cases suggest this? Well, as Fodor points out, whatever else Fregean

senses are, synonymous expressions must share them. Assuming that ATTORNEY and LAWYER are synonyms, then, they must share a Fregean sense. But Mates cases apparently show that ATTORNEY and LAWYER are not freely substitutable for one another *salva veritatae*. Fodor draws the following conclusion: “if … MOPs *just are* whatever it is that the substitution test tests for, then it’s unlikely that MOPs are senses” (1998a, p. 16, original emphasis).

This argument is a potentially powerful weapon for those who wish to defend a purely referential theory of content against the standard Fregean considerations. It’s thus no surprise to find that Saul Kripke, who (along with Burge and Putnam) is in large part responsible for the spread of purely referential/externalist theories of meaning in philosophy of mind and language, offers a similar defense of such theories against the standard Fregean considerations. In his famous paper “A Puzzle about Belief,” Kripke argues that the opacity of belief contexts needn’t count in favor of Fregeanism over Millianism (about proper names). One of his arguments is that, sometimes at least, positing Fregean senses does *not* in fact explain failures of substitutivity. In the cases that putatively generate substitution failures, Kripke points out that people often don’t assign distinct senses to the relevant coreferential terms. For instance, to the extent that people do assign senses to ‘Cicero’ and ‘Tully’, they’re plausibly *identical*; a typical thinker might associate each name with the description ‘was an ancient Roman orator’. But if this is typical, then the standard Fregean explanation for substitution failures involving ‘Cicero’ and ‘Tully’ would seem to be undermined. Since ‘Cicero’ and ‘Tully’ clearly *can* generate substitution failures, it would seem that they must be associated with different MOPs. But if Kripke’s observation is correct, and the names are associated with

the same sense, then this suggests that MOPs are *not* Fregean senses. (Kripke himself does not draw this conclusion, at least in these terms).

Fodor offers what seems to be essentially the same argument, or at least a related argument for the same conclusion⁹:

Suppose I tell you that Jackson was a painter and that Pollock was a painter, and I tell you nothing else about Jackson or Pollock. Suppose, also, that you believe what I tell you. It looks like that fixes the sense of the names ‘Jackson’ and ‘Pollock’ if anything could; and it looks like it fixes them as both having the *same* sense: viz. a painter. (*Mutatis mutandis*, it looks as though I have fixed the same inferential role for both.) Yet, in the circumstances imagined, it’s perfectly OK—perfectly conceptually coherent—for you to wonder whether Jackson and Pollock were the *same* painter. (Contrast the peculiarity of your wondering, in such a case, whether Jackson was Jackson or whether Pollock was Pollock.) So, then, by Frege’s own test, JACKSON and POLLOCK count as different MOPs. But if concepts with the same sense can be different MOPs then, patently, MOPs can’t be senses. (1998a, p. 16)

What this Kripke-esque argument and the Mates cases suggest, Fodor claims, is that substitution failures do *not* show that coreferential concepts are not synonymous. Rather, they suggest that MOPs are syntactically, not semantically individuated:

[W]hat’s going on doesn’t seem to have to do with *meaning*. Rather, the governing principle is a piece of logical syntax: If ‘a’ and ‘b’ are different names, then the inference from ‘Fa’ to ‘Fb’ is never conceptually necessary [unless the

⁹ In a footnote, Fodor says: “By the way, I have the damnedest sense of *déjà vu* about the argument in the text; I simply can’t remember whether I read it somewhere or made it up” (1998a, p. 17n). Perhaps he read it in “A Puzzle About Belief”?

inference from ‘Fa’ or ‘Fb’ to $a=b$ is conceptually necessary, e.g. if ‘Fa’ is ‘a has the property of being identical to b’]. … It looks like the moral of this story about Jackson and Pollock is the same as the moral of Mates’s story about [lawyers and attorneys]. *Frege’s substitution test doesn’t identify senses.*

Correspondingly, if it is stipulated that MOPs are whatever substitution *salva veritate* turns on, then MOPs have to be sliced a good bit *thinner* than senses. Individuating MOPs is more like individuating forms of words than it is like individuating meanings. (Fodor 1998a, p. 17, original emphasis)

Supposing, then, that differences between the rationality of thoughts turn on the MOPs associated with them, it follows that on Fodor’s view such differences in rationality turn on the syntax, not the content of thoughts. This shows that, in this context, Peacocke’s assumption that the rationality of a thought depends upon its content is contentious. The Cartesian will thus reject Peacocke’s general argument for Knowledge Pragmatism, at least to the extent that it depends on this assumption.

In his recent book, *The Realm of Reason*, Peacocke offers the following argument for Pragmatism, which also turns on an assumption the Cartesian will deny:

[I]t cannot simply be that evidence is wholly irrelevant to meaning and content.

In fact, for anyone who employs a notion of content constrained by Fregean consideration of cognitive significance, it is a consequence of the very nature of intentional content that evidence cannot be completely irrelevant. If, when we hold background information constant, there is something that is evidence for p but not evidence for q , it follows that p and q are distinct Fregean Thoughts. If we apply Frege’s classical test, someone with that evidence could rationally

believe p but not believe q . Even if they do not exhaust meaning, and even if they are not fundamental, evidential factors cannot be excluded from any epistemically significant notion of content. (2004a, p. 34)

We can now see that this argument is unconvincing, for two reasons. First, it may be that evidence and rationality must figure in the individuation of any “*epistemically* significant notion of content”. But, at a certain level, the very issue at hand here is whether content *is* epistemically significant. In particular, for those who approach a theory of concepts and content from the perspective of psychological explanation, the normative, epistemological properties of content do *not* make an appearance at the level of content-ascription. If an epistemically significant notion of content is one that is constitutively connected to such normative properties, then it’s question-begging for Peacocke to argue that *since* concepts and content are epistemically significant, evidence and rationality *must* be involved in their individuation.

Second, it’s false that *any* theory constrained by Fregean considerations of cognitive significance must posit an epistemic dimension to the individuation of concepts and content, or admit that “reasons and rationality” occupy the “central place … in the individuation of concepts” (Peacocke 2000a, p. 332). Fregean considerations certainly don’t warrant Peacocke’s claim that “to be a concept is simply to have a certain role in rational transitions in thought, in the mind of a rational thinker” (2005, p. 169). For we’ve just seen that Fodor’s Cartesian account can claim to be constrained by Fregean considerations, despite the fact that it *denies* that there’s a constitutive connection between rationality and evidence, on the one hand, and concepts and content, on the

other.¹⁰ What such Fregean considerations show, according to Fodor, is that synonymous concepts have distinct MOPs, which are *syntactically* individuated. Since it's open for a Cartesian to deny the Fregean identification of MOPs with senses, it's question-begging in this context for Peacocke to assume that issues about rationality and the like turn on the *content* of thoughts. For they may just as well turn on their form, or syntax. The question of whether Peacocke's Pragmatism should be endorsed on grounds of explanatory power thus depends upon the antecedent question of whether issues about rationality turn on the content of thoughts. And it should now be clear that this question, in turn, depends upon the question of whether MOPs should be identified with senses. I argue that the cost of answering "no" to this latter question is quite high.

The Mates cases, Kripke-esque examples, and Informational Atomism each independently suggest that MOPs are sensitive to the syntactically individuated vehicles of thought. As we've seen, a purely informational semantics alone essentially forces this conclusion, since it requires that distinct coreferential concepts are synonyms. And if 'content individuation can't be all there is to concept individuation', then the syntactic or formal properties of concepts would seem to be the only other candidates for individuation. If this were the only consideration in favor of the claim that MOPs are syntactically individuated, then Fodor's case would be very weak indeed. For as we noted above, one may be inclined to take the fact that Informational Atomism implies that coreferential concepts are synonyms to be *prima facie* evidence *against* Informational Atomism. A Fregean thus might rightly be suspicious of Fodor's using this implication as *support* for the claim that MOPs are syntactically individuated. The Mates

¹⁰ We'll see below that Peacocke's claim is implausible for *other* reasons as well.

and Kripke-esque examples are thus an important dialectical tool, for they provide much needed *independent* support for Fodor's claim that MOPs are not Fregean senses.

This isn't to say, of course, that Fregeans will accept Fodor's claims about the implications of these examples. For Fregeans will surely have something to say in response to Fodor's claim that such examples show that substitution failures do not indicate that the relevant concepts differ in content. For instance, Fregeans can attempt to account for Mates cases by providing a semantics of 'that'-clauses in natural language. Of course, as Pietroski (2000a) points out, Fodor might deny that one can adequately reply to him by offering a particular Fregean theory of the semantics of attitude ascriptions (p. 344). For Fodor claims that "English *has no semantics*" (1998a, p. 9, original emphasis), which implies that it doesn't *have* a semantics of attitude ascriptions. In other words, if natural languages are not what have meaning "in the first instance" (as Fodor (2001b) puts it) and in fact have no compositional semantics at all, then one can't reply to Fodor's use of Mates cases by offering a semantics of attitude ascriptions. But, as Pietroski also points out, to say that natural languages don't have a compositional semantics is to make a *very* strong claim, which would require not only that semanticists are wrong about what their theories are theories of, but also that there be an *alternative* explanation for the successes that semanticists have had in providing detailed semantic theories:

[O]ne does not *account* for the successes of actual semantic theories simply by positing unspecified mechanisms that map (translate, or compile) the sentences we know about into allegedly distinct sentences whose structure and meaning remains unknown. One owes a case-by-case discussion of the facts apparently

explained by our best theories of adverbs, tense, etc. In the absence of a semantics textbook for Mentalese, it is reasonable to assume that English has a semantics partly described by the (actual) semantics textbooks for English.

(2000a, p. 345, original emphasis)

In the eyes of Fregeans, then, it might seem rash to insist that Mates cases support the claim MOPs aren't senses, if doing so requires that one deny that natural languages have a compositional semantics.

In any case, there isn't space here to consider in detail Fregean attempts to handle Mates cases, or to evaluate the basis of Fodor's claims that there's no compositional semantics for natural language. Nor is there space to consider possible Fregean replies to Kripke.¹¹ However, there are good reasons, which are *independent* of these technical issues in philosophy of language and linguistics, for thinking that Fodor's claim that MOPs aren't senses is implausible. So, rather than replying to Fodor by attempting to undermine his use of the Mates and Kripke-esque cases, I will instead argue that his position is implausible when considered on its own (i.e., *irrespective* of his justification for it).

There are three reasons to doubt the adequacy of Fodor's account. First, it's not clear that Fodor's account can handle cases in which the coreferential concepts in question are both distinct *and* primitive. Second, there are reasons for thinking that concepts and their corresponding internal representations do not stand in the tight relationship that Fodor's account seems to require. Third, taking MOPs to be the

¹¹ Fodor makes use not only of the Kripke-esque Jackson/Pollock example above, but also Kripke's (1979) puzzle about Pierre; see Fodor (1990, chapter 6).

representational vehicles of thought apparently requires that one give up on content-based explanations of people's behavior in Frege cases. Let's consider each of these in turn.

Coreferring, Primitive LOT expressions

If coreferential terms are synonyms, then they can nevertheless express distinct concepts *so long as* MOPs are syntactically individuated. I suspect that in many (perhaps most) cases there will be the needed structure at the level of internal representation to distinguish such concepts. Thus, although informational semantics has it that HEAT and MEAN MOLECULAR KINETIC ENERGY are synonyms, it nevertheless counts them as distinct concepts since the latter, but not the former, has KINETIC and ENERGY as constituents. Given the difference in the structure of the internal representations, Fodor's story thus offers a nice account of why HEAT and MEAN MOLECULAR KINETIC ENERGY are distinct concepts.

But what about cases in which there *is* no structure that can distinguish the coreferential concepts? Consider again SQUARE and DIAMOND. On Fodor's view, the propositional-attitude ascription "judges that's a square" attributes to someone a judgment-relation to the very same proposition that the ascription "judges that's a diamond" does. But despite the identity of the content of the ascribed judgments, they are *distinct judgment states*. On Fodor's account, this is because the ascriptions are also used to specify the vehicle of the judgment states, which they do by "displaying a formula that is, to one or another degree, structurally isomorphic to the vehicle" (1990, p. 169). But in this case, "judges that's a square" seems to be structurally isomorphic to "judges that's a diamond," in which case the corresponding vehicles will *not* be

structurally distinct. In other words, SQUARE and DIAMOND seem to be primitives, and this apparently spells trouble for Fodor's account. For if SQUARE and DIAMOND are primitives, then one can't appeal to differences in the *form* of the corresponding judgments to explain the differences in their rationality and epistemic status. Primitive concepts are, after all, *formally* identical. One would thus apparently have to appeal to the distinct *contents* of SQUARE and DIAMOND in order to explain the relevant facts about rationality and epistemic status. But informational semantics can't make any such appeal, since it implies that SQUARE and DIAMOND are synonyms.

One way for the informational semanticist to get out of this would be to claim that DIAMOND and SQUARE are *not* in fact coreferential. The idea would be to grant that coreferential concepts pick out identical properties, but claim that *being a diamond* and *being a square* are in fact distinct. For instance, one might claim that which of these properties is instantiated on a given occasion is tied to the way the object is positioned with respect to the perceiver. Indeed, if one goes in for Fodor's (1998a) view that most middle-level properties of objects are in fact mind-dependent, then it might fall out of one's theory that *being a diamond* and *being a square* are distinct properties. For *being a diamond*, on that view, *just is* the property that our minds lock to in consequence of having experiences of stereotypical diamonds. And, one might claim, the set of stereotypical diamonds is not identical to the set of stereotypical squares, since stereotypical diamonds are positioned in a very particular way with respect to perceivers, i.e. they're positioned in such a way that they *look like* diamonds and not squares. Hence the properties that thinkers lock onto as a result will be distinct. *Perhaps* a metaphysical response to the worry about primitive, coreferential concepts could be developed along

these lines. But even if it could, to respond in this way would be to rest the plausibility of one's theory on what I take to be a relatively tendentious metaphysics.¹²

Still another way out would be to deny that there could be genuinely primitive, coreferential predicates in the language of thought. For perhaps in every case in which two coreferential natural language terms *appear* to be primitive, at least one of the corresponding internal representations is in fact complex.¹³ The problem with this reply, however, is that I don't see how an informational semanticist could defend it with any plausibility. Perhaps *some* of the apparent cases of primitive coreferring terms will turn out to correspond to complex mental representations that can distinguish the relevant concepts. But what's the argument that *all* of them will? That a particular theory of content needs there to be such internal structure in order to explain all the relevant data is no argument at all. For surely the issue of which internal representations have structure is an empirical one, not to be decided by one's philosophical theory of content.

But perhaps it wouldn't bother Fodor if his theory of content determined his view about which representations are primitive and which are structured. He might reason as follows: (i) informational semantics is a well motivated theory, since (as Fodor (1994, 1998a) has claimed for some time now) it's the only theory that has any hope of working

¹² Fodor might also reply that he's only attempting to provide *sufficient* conditions for content, not necessary ones. As he puts it in *A Theory of Content*: "Suppose we had naturalistically sufficient for content. It wouldn't, of course, follow that any of our neural states, or any of our public symbols have the content that they do because they satisfy the conditions on offer. Indeed, it wouldn't follow from the mere existence of sufficient conditions for content that anything in the universe has actually got any. 'P implies Q' is neutral about Q. God can accept the consequents of any true hypotheticals whose antecedents He doesn't know to be false; but we can't" (1990, p. 131). But even if Fodor's asymmetric dependency theory is intended to supply mere sufficient conditions, presumably this is *not* the case for informational semantics generally. That is, I take it Fodor thinks that informational semantics is actually true of us, and will thus supply necessary and sufficient conditions for content.

¹³ Indeed, Fodor can object that this is true of the very case under consideration. After all, *being a diamond* isn't identical with *being square*, since some diamonds aren't regular-diamonds. The relevant concepts are thus *SQUARE* and *REGULAR-DIAMOND*, and the structure of the latter can serve to distinguish the concepts. This is surely a happy accident, however. In any event, I take it the case could be made with *CORIANDER* and *CILANTRO*.

as a *naturalistic* account of content; (ii) informational semantics requires that coreferential concepts be distinguished syntactically, not semantically; so, (iii) in all cases in which two concepts are coreferential, the corresponding representations must be syntactically distinct; (iv) but saying this doesn't require one to *deny* that the issue of which representations have structure and which don't is an empirical one; for (v) although informational semantics places constraints on one's view about the structure of mental representations, they aren't *a priori* constraints (or at least not *purely* *a priori*), since informational semantics itself is (at least in part) empirically motivated.

Now, I'm not sure whether Fodor would offer such a reply. He has made a similar *kind* of claim before, though. For instance, when it comes to the possibility of primitive concepts for which the corresponding properties are *uninstantiated* (e.g. GHOST or PHLOGISTON) Fodor bites the bullet and says that there can be “no *primitive* concept without a corresponding property for it to lock to” (1998a, p. 165, original emphasis). Fodor thus allows informational semantics to determine his ontology in a way that one might rightly find a bit disconcerting. Perhaps there are good metaphysical reasons for positing uninstantiated properties—e.g., some theorists argue that they're needed in an account of the metaphysics of laws.¹⁴ But one might reasonably balk at positing such properties *merely* because one's *theory of content* needs them.¹⁵ In any case, if Fodor is willing to believe that there's a property for every primitive concept merely because informational semantics requires it, then perhaps he's *also* willing to believe that, for every case of apparently primitive coreferential concepts, there will be structure to

¹⁴ See Tooley (1977) and Rives (under revision) for discussion.

¹⁵ Rey (forthcoming b, c) makes this point in his discussion of empty concepts in the context of content externalism.

distinguish them, again where the belief is driven by a commitment to informational semantics.

However, even if Fodor is willing to adopt a methodology of this kind, it bears emphasis that such a reply would not sit happily with his other commitments. For as an Informational Atomist, he denies that lexical concepts *have* internal structure. If lexical concepts are those concepts that correspond to lexical items in natural language, and two lexical items are apparently coreferential, then Fodor can't save his informational semantics by claiming that the corresponding lexical concepts are in fact structurally complex, on pain of having to give up on Concept Atomism. The possibility of primitive coreferential concepts thus poses a dilemma for the Informational Atomist. Either the Atomist must insist that there will *always* be structure at the level of internal representation to distinguish apparently primitive, coreferential lexical concepts, in which case he must admit that some lexical concepts have internal structure, *contra* Atomism.¹⁶ Or, the Informational Atomist can retain his Atomism in the face of primitive coreferential concepts, but must then give up on a non-semantic account of what distinguishes coreferential but distinct concepts. Either way, it seems, the Informational Atomist loses.

¹⁶ Now, it's true that Fodor often hedges when it comes to characterizing Atomism. In fact, to the best of my knowledge he nowhere claims that *all* lexical concepts are internally unstructured. Rather, he usually says that *most* lexical concepts lack structure, or that it's *approximately true* that all lexical concepts are atoms. Presumably, the reason he hedges is that it's an empirical issue as to whether a certain mental representation has internal structure. Who knows, perhaps it will turn out that there's even a small handful of definitions! The point is that the distinction between structured and unstructured lexical concepts must be *principled*, and positing structured lexical concepts on the basis of whether they happen to corefer would surely not provide a principled distinction.

Concepts and Mental Representations

A related worry has to do with the fact that Fodor's account appears to require that concepts be *identified* with the corresponding mental representations. For if MOPs are what distinguish coreferential but distinct concepts, and MOPs are internal mental representations, then presumably concepts themselves just are representations. Indeed, the first of Fodor's five 'non-negotiable conditions on a theory of concepts' is that "concepts are mental particulars; specifically, they satisfy whatever ontological conditions have to be met by things that function as mental causes and effects" (1998a, p. 23). And since MOPs, on Fodor's account, are both in the head and proximal mental causes (1998a, p. 20), one might think that Fodor takes concepts themselves to be identical with their corresponding mental representations.¹⁷

Before we get to what I take to be a legitimate worry about the claim that concepts are identical with mental representations (/MOPs), let's consider a recent argument Peacocke has put forward, which I think leaves that claim untouched:

If we accept that a thinker's possession of a concept must be realized by some subpersonal state involving a mental representation, why not say simply that the concept *is* the mental representation? ... [Because it can] be true that there are concepts human beings may never acquire, because of their intellectual limitations, or because the sun will expand to eradicate human life before humans reach a state at which they can acquire these concepts. 'There are concepts that will never be acquired' cannot mean or imply 'There are mental representations which are not mental representations in anyone's mind'. If concepts are

¹⁷ Fodor often moves freely between talk of MOPs, concepts, and mental representations. See especially (1998a, pp. 15-22).

individuated by their possession conditions, on the other hand, there is no problem about the existence of concepts that will never be acquired. They are simply concepts whose possession conditions will never be satisfied by thinkers. (2005, p. 169, original emphasis)

If concepts aren't mental representations, this argument certainly doesn't show it. The fact that there are concepts that no human will ever acquire (or thoughts that no human will ever entertain) is perfectly consistent with the claim that concepts (or thoughts) are mental representations. For the latter claim can be understood in terms of mental-representation *types*, in which case 'There are concepts that will never be acquired' can mean 'There are mental representations which are not mental representations in anyone's mind'. Of course, the sun may wipe us out before anyone *entokens* such a mental representation, but that surely doesn't show that the concept *couldn't be* the mental representation. Indeed, given that Peacocke himself thinks that there are concepts that may never be entokened (and thus apparently doesn't have a problem with abstract types in general), I don't see why he thinks the same can't be true for the corresponding mental representations.

In any case, there's another, more serious worry about the claim that concepts are mental representations, and that is that there are *prima facie* cases in which the two can come apart.¹⁸ There are really two worries here. One is that the same concept can

¹⁸ We touched on this issue in Chapter 1, when we discussed the different notions of 'concept' found in philosophy and psychology. It bears emphasis that the ontological dispute between those who take concepts to be mental representations and those who take them to be what's *expressed* by mental representations *crosscuts* the dispute between those who take concepts to be needed primarily for the purposes of psychological explanation and those who take it to be needed primarily for normative epistemology. As we noted in Chapter 1, Peacocke (1992a) and Rey (1994, 1998b) think there are good reasons for distinguishing concepts and mental representations, whereas Laurence and Margolis (1999) and Fodor do not (although see below).

correspond to different mental representations in different people (and perhaps different time-slices of the same person), and the other is that the same mental representation can correspond to different concepts in different people (and perhaps different time-slices of the same person). Rey provides the following examples to motivate each of these worries:

One person might express the concept CITY by the word “city,” another by the word “ville”; still another perhaps by a mental image of bustling boulevards; but, for all that, they might have the same concept CITY; one could believe and another doubt that cities are healthy places to live. Moreover, different people could employ the same representation to express different concepts: one person might use an image of Paris to express PARIS, another to express FRANCE. (1994, p. 186; quoted in Laurence and Margolis 1999, p. 76)

If this is right, then possessing a particular mental representation is neither necessary nor sufficient for possessing a particular concept.

Let's put the second case (where the same representation is used to express distinct concepts) to one side, and focus on the first kind of case. Note that it's essentially an application of the familiar claim that mental properties are multiply realized. If type-type identity theory is false with respect to the propositional attitudes, as most philosophers of psychology believe, then it's presumably also false with respect to concepts and their corresponding mental representations. The empirical and philosophical arguments in favor of multiple realizability thus also provide *prima facie* support for the claim that the same concept can correspond to distinct mental representations in different people (or different time-slices of the same person).

A defender of the view that concepts are mental representations might be unconvinced, however. For even if one grants that different mental-representation types can express the same concept, one might object, it doesn't follow that concepts can't be identified with mental representations. For it all depends on how one *types* the mental representations. If a token of mental-representation type M1, and a token of mental-representation type M2, each express the same concept C, then obviously C can't be identical with either M1 or M2. But, the objection continues, the claim that concepts aren't mental representations will follow from this *only* under the assumption that the token of M1 and the token of M2 aren't each *also* tokens of some broader mental-representation type, say, M3. For if they are, then C may very well be identical with M3, in which case the claim that concepts are mental representations survives.¹⁹

There's at least *something* right about this objection, for surely two tokens of distinct mental-representation types can nevertheless be tokens of the same mental-representation type, if the latter is typed more broadly. Indeed, this is true of two tokens of *anything*. On my desk there is a copy of *Concepts: Core Readings* and a copy of *The Elm and the Expert*. These are two tokens of distinct types, if we type according to *which* book the tokens are tokens of. But if we type the things on my desk more broadly, in terms of books, mugs, clocks, and phones, then these two tokens will be of the same type, namely, *book*. If we type more broadly still, then *all* of the tokens on my desk will be of the same type, say, *object*. The point is that the objection above is absolutely right that we can evaluate claims about whether concepts are identical with mental representations only if we know the appropriate fineness-of-grain with which to type mental-representations. But what is the appropriate fineness-of-grain? We know that, say,

¹⁹ Laurence and Margolis raise an objection of this sort (1999, p. 76).

mental representation is too broad, since surely there are *some* type-distinct mental representations. But what about a mental image of bustling boulevards and a predicate in the language of thought corresponding to “city”? Given that mental representations are posited as part of an explanatory psychology, the latter is clearly what we should look to in order to answer this question.

Now, given the Shareability Constraint, there are good explanatory reasons for thinking that two people can share the concept CITY, *regardless* of the character of the corresponding mental representations. Moreover, I take it that the question of whether the internal representations are type-identical or type-distinct is an empirical one. That said, there seem to be two relevant possibilities for typing the representations. According to one, the internal representations ought to be typed according to their purely formal properties. On this way of individuating representations, the token mental image and the token word-like representation will apparently be of *distinct* types. For formal properties are simply non-semantic properties, and these two representations clearly have distinct non-semantic properties. The image presumably has certain imagistic (/iconic) properties that are individuative, and the word-like representation presumably has certain orthographic (/discursive) properties that are individuative.

For what it’s worth, I take it that this way of individuating representations accords with our intuitions. Prima facie, an image of a house is a different type of representation than the predicate “house,” even though they may be identical in content (i.e., they’re both about houses). This way of individuating also sits well with some of the things that Fodor says when he’s summarizing the basic tenets of the Representational Theory of Mind. For instance, he says:

[Propositional attitudes] ... have their logical forms *intrinsically*. Which is to say not only that if x and y are propositional attitudes of different logical forms they are ipso facto different mental particulars, but also that they are ipso facto mental particulars of different types. (Fodor 2000a, p. 14, original emphasis)

Note that Fodor puts the point here in terms of *logical* form. But that's because he's only concerned with the propositional attitudes, whose relevant formal properties are their logical-form properties. So the mere fact that mental images *don't have* logical forms is beside the point. The claim is that what matters for the type-individuation of mental representations is their formal properties.²⁰ On this way of typing mental representations, the above example would constitute a case in which two tokens of distinct mental-representation types each correspond to the same concept. I take it, then, that on this way of individuating, such examples would show that concepts are not identical with mental representations.

However, there's another way to individuate the internal mental-representations that correspond to concepts, namely, in terms of their functional/causal roles. On this view, whether two token mental representations are of the same type is a matter of whether the representations have identical functional/causal roles. If they do, then they're of the same type, and if they don't, then they aren't. Fodor *also* seems to endorse this way of individuating internal representations:

If ... MOPs are in the head, then they can be proximal mental causes and are, to that extent, apt for functional individuation. If MOPs are both in the head and

²⁰ See also note 8 above.

functionally individuated, *then a MOP's identity can be constituted by what happens when you entertain it.* (1998a, p. 20, original emphasis).²¹

On this way of individuating mental representations, are a token mental image of bustling boulevards and a token predicate in Mentalese corresponding to “city” tokens of identical mental-representation types? Again, in the end it’s an empirical issue, of course. But it’s at least not *prima facie* implausible that those who entoken an image of bustling boulevards when they entertain CITY, and those who entoken a predicate in that language of thought when they entertain CITY, are entokening representations that have *distinct* functional/causal roles. It seems reasonable that a person who uses the image to express CITY will be entokening a representation that has a different causal profile than the representation entokened in someone who uses the predicate. If this is correct, then the above example does pose a threat to the claim that concepts are identical with mental representations.

But suppose it were to turn out that although the image and the predicate in the language of thought are formally distinct, they’re identical in their functional/causal roles. If this were the case, then individuating mental representations terms of their roles

²¹ The following passage makes essentially the same point, but in terms of machine states: “The characterization of the type/token relation for Mentalese is functional, recursive, and (what else?) highly-counterfactual. Suppose we have a machine that computes in Mentalese. For convenience, suppose it has an input tape and an output tape, and that Mentalese can be written on either. I assume that we can recognize the *numerical* identity of the machine’s tape states, hence that the notion of *numerically identical tape states* is available to us to use for the characterization of type identity for Mentalese symbols. Suppose that T1 and T2 are token inscriptions that do or can appear on the machine’s tapes. Then: 1. T1 and T2 are tokens of the same type if, for any machine process (i.e., for any operation compatible with the machine’s table), the numerically same output state that was (or would have been) produced by the machine when it is given T1 as input would have been produced if the input had been T2; and vice versa. 2. If Ti and Tj belong to the same symbol type by criterion 1, then Tm and Tn belong to the same symbol type if any machine process that yielded (or would have yielded) Ti as output given Tm as input would have issued in Tj as output if the input had been Tn. 3. The type/token relation for primitive expression is closed under 1 and 2. The basic idea is that two tokens are of the very same type if they would both cause the machine to be in the very same states; i.e., in the *numerically same states*” (Fodor 1994, pp.105-109, original emphasis).

would provide a potential reply to the sorts of cases that motivate the claim that concepts are distinct from their corresponding internal representations. For examples like the one above wouldn't be cases in which there are two tokens of distinct mental-representation types corresponding to the same concept. However, adopting this way of individuating representations would clearly create problems for Informational Atomism. For if MOPs (i.e., the internal representations) are individuated in terms of their functional/causal roles, *and MOPs = concepts*, then concepts are individuated in terms of their functional/causal roles. This implication will of course be anathema to Informational Atomists, who think that conceptual/inferential role semantics inevitably leads to semantic holism, which in turn violates the Shareability Constraint and has disastrous consequences for intentional psychology.²² Given that Informational Atomists deny that concepts can be functionally individuated such that they satisfy the Shareability Constraint, one wonders why the functional individuation of MOPs will be any less problematic, if *MOPs = concepts*.

Once again, the problem can be posed as a dilemma. The Informational Atomist can individuate the internal representations corresponding to concepts *either* in terms of their formal properties *or* in terms of their functional/causal roles. The former strategy allows for the possibility of tokens of distinct mental-representation types to correspond to the same concept, which threatens the claim that concepts are identical with their corresponding mental representations. But the latter strategy plausibly *also* allows for this possibility, in which case it too threatens the identification claim. (Of course if it turns out that the tokens are of the same mental-representation types because they have

²² This point is made in Aydede (1998). See Fodor and Lepore (1992) for an extensive discussion of the disastrous consequences of holism.

identical functional/causal roles, then the claim that concepts are internal representations is no longer threatened.) But it's not clear that Informational Atomists can adopt this way of individuating mental representations, since it's apparently at odds with their insistence that *concepts* cannot be individuated functionally. If this is right, then Informational Atomists must either give up on the claim that concepts are identical with mental representations, or allow that at least some concepts are individuated in terms of their roles.

One possible way out for the Informational Atomist would be to claim that this is a false dilemma. For perhaps one can maintain *both* that mental representations are individuated in terms of their formal properties *and* that they're individuated in terms of their functional/causal roles. The way to do so is to claim that the functional/causal role of a mental representation *supervenes on* its formal properties, or, to put it slightly differently, that a mental representation has its functional/causal role *in virtue of* (among other things) its formal properties. In fact, this claim is part and parcel of the very computationalism that Fodor champions (at least for the modular components of the mind; Fodor 1983, 2000a). Here are two of the three defining features of the computational theory of mind as laid out by Fodor:

- i. Thoughts have their causal roles in virtue of, inter alia, their logical form.
- ii. The logical form of a thought supervenes on the syntactic form of the corresponding mental representation. (2000a, p. 18)²³

Given this, a belief whose logical form is conjunctive will correspond to a mental representation whose syntactic form is conjunctive, and the syntax of the mental

²³ The third is the following: "iii. Mental processes (including, paradigmatically, thinking) are computations, that is, they are operations defined on the syntax of mental representations, and they are reliably truth preserving in indefinitely many cases" (Fodor, 2000a, pp. 18-19).

representation is what determines its functional/causal role. For instance, suppose I entertain the belief that John ran and Mary swam, and thereby entoken the corresponding conjunctive mental representation. Suppose further that my entertaining this belief causes me to entertain the belief that John ran. The former thought caused me to entertain the latter thought in virtue of its logical form, which, according to computationalism, supervenes on the syntax of the corresponding mental representation. In such a case, *both* the causal role of the representation *and* its formal properties are individuative, since they bear some sort of determination/dependency relation to one another.

While this is a promising move, note that it will only work for *complex* mental representations, i.e., representations that *have* internal structure upon which their causal role can supervene. It will thus work for the propositional attitudes, since their corresponding mental representations are presumably structured. However, the Informational Atomist can apparently make no such appeal in the case of *lexical* concepts that putatively correspond to distinct mental representations in different people. For the Atomist is committed to the claim that lexical concepts lack internal structure, and thus have no structure upon which their causal roles can supervene. In the case of lexical concepts, then, the Informational Atomist is apparently vulnerable to the above dilemma.

Intentional Explanations

In light of the these considerations, one may wonder why Fodor just doesn't *give up* on the Shareability Constraint, at least as applied to the *representational vehicles* of thought. For perhaps what's shared across people's heads are *not* MOPs, but conceptual *contents*,

where the latter are understood *broadly* (as informational semantics requires). Indeed, Fodor uses the fact that there can be interpersonal MOP variation to *motivate* the claim that psychological generalizations are stated in terms of broad content. If this is right, then giving up on the Shareability Constraint for the representational vehicles of thought will pose no threat to the viability of intentional psychology. Fodor explains his view as follows:

One can, I think, imagine a world where everything is delicately balanced in the following way: Content is broad, the metaphysics of content is externalist (e.g., causal/informational), and modes of presentation are sentences of Mentalese. Modes of presentation with similar causal histories (or nomic affiliations; anyhow with similar broad contents) overlap enough in their syntax to sustain robust psychological generalizations. But not enough to make the minds that these generalizations subsume homogenous under *syntactic* description. ... In such a world, the laws that a computational psychology implements might be *intractably and ineliminably* intentional precisely *because* they are laws about broad content; viz., laws about a kind of content that computationally heterogeneous minds can share in virtue of similarities in their *extrinsic* relations. (1994, p. 53, original emphasis)

This argument is an ingenious attempt to turn an apparent weakness of Fodor's treatment of Frege cases into a strength of Informational Atomism.

Do concepts themselves satisfy the Shareability Constraint on this view? Yes and No. If concepts are identical with their corresponding MOPs, as Fodor sometimes

claims,²⁴ then the answer is clearly “no”. For MOPs, he allows, don’t satisfy the Constraint. But if, as Fodor says elsewhere, “a concept is a MOP *together with a content*” (1998a, p. 20n.16, my emphasis), then concepts *do* satisfy the Constraint, at least along one of their dimensions. That is, although MOPs aren’t shared, conceptual *contents* are. And, again, if psychological explanations are broad, then the fact that concepts violate the Shareability Constraint along their MOP-dimension will not threaten the viability of intentional psychology. Although this is elegant, Fodor’s treatment here apparently comes at a high price, namely, that one must give up on intentional explanations of people’s behavior in Frege cases.

To see this, consider the hoary example of Oedipus, who didn’t want to marry his mother or kill his father, but ended up doing both all the same. The common-sense/folk-psychological explanation of Oedipus’ behavior, of course, is that Oedipus didn’t believe that Jocasta was his mother, or that the rude traveler was his father. Let’s take the truth of this explanation as given. The question is: how does Fodor’s account accommodate it? As we’ve seen, informational semantics forces Fodor to a *non-semantic* explanation of people’s behavior in Frege cases, for it has it that Oedipus’ desire to marry Jocasta is a mental state with the *same content* as the state Oedipus would have been in had he desired to marry his mother (which he didn’t). Given this constraint, an explanation of Oedipus’ behavior cannot appeal to the *contents* of Oedipus’ thoughts. Rather, the explanation must be pitched at the level of syntactically-individuated internal

²⁴ For instance: “I use ‘entertaining’ and ‘grasping’ a MOP (/concept) interchangeably” (Fodor, 1998a, p. 17). And: “For present purposes, it will do to think of thoughts as mental representations analogous to closed sentences, and concepts as mental representations analogous to the corresponding open ones” (Fodor, 1998a, p. 25).

representations—i.e., at the level of the mechanisms that *implement* intentional psychological laws.

However, one may think that this is the wrong level at which to pitch the explanation, at least if the explanation is to respect the folk-psychological explanation.²⁵ For one might claim that the folk are *not* doing implementation-level psychology when they explain Oedipus' behavior. Rather, they're simply appealing to *what* Oedipus believed and desired. Oedipus was disgusted *because* he came to believe *that he married his mother*, and not just that some new terms in the language of thought came to be entokened in his nervous system. Of course, if RTM is true, then Oedipus' coming to have this belief *just is* his coming to stand in the belief-relation to a sentence in the language of thought. And one might argue further that the folk-psychological explanation somehow makes implicit reference to *non-semantic* differences in the vehicles of Oedipus' thoughts before and after he realizes what he's done. (Recall Fodor's claim that attitude ascriptions specify the vehicle by displaying it.) But even if Fodor's account does not disrespect the explanatory practices of the folk, it does seem at odds with a content-based explanation of Oedipus' behavior.

One may wonder, though, whether this is actually such a high price to pay. After all, as we noted in Chapter 2, we shouldn't expect a mature scientific psychology to respect folk psychology *in its entirety*. Some bits of the latter may have to give way to the more sophisticated conception of ourselves that scientific psychology delivers. Indeed, some arguably already have. Perhaps the folk-psychological explanation of peoples' behavior in Frege cases is just one (more) example of this, in which case it's no objection to Fodor's account that it disrespects the folk-psychological explanation.

²⁵ I owe this objection to Peter Carruthers.

But this reply ignores an important distinction. It's true, of course, that the categories of a mature scientific psychology might replace some folk-psychological categories while nevertheless remaining (in some sense) a vindication of it. Exactly how many and which categories may be replaced is controversial. But what *isn't* controversial, I take it, is that if a developed scientific psychology fails to be intentional, then folk psychology will fail to be vindicated. For folk psychology is, as Fodor likes to say, *intentional through and through*. Thus, although folk psychology might survive the replacement of, say, the categories of belief and desire, it will not survive the replacement of intentional categories altogether.²⁶ Fodor's claim is not that an explanation of people's behavior in Frege cases will not appeal to the folk-psychological categories of belief and desire. But he does seem committed to the claim that such explanations will not appeal to the content of the relevant mental states, which does seem like a fairly high price.²⁷

Conclusion

Taken together, the previous three sections constitute what I take to be a convincing case against Fodor's treatment of Frege cases. Let's conclude, then, by supposing that Fodor

²⁶ Recall, in this regard, Fodor's distinction between 'conservative' and 'die-hard' intentional realists: "A conservative intentional realist who is not a diehard can contemplate with equanimity the abandonment of belief/desire psychology strictly socalled, so long as the apparatus of *intentional* explanation is itself left intact" (1990, pp. 174-175, original emphasis).

²⁷ Fodor (1994) seems to be aware of this consequence, since he claims not only that Frege cases are *rare* (*contra* Fodor 1980), but also that they are *exceptions* rather than *counterexamples* to psychological generalizations couched in terms of broad content. *Even if* these claims are correct, however, that there should be an asymmetry between the kind of explanation required in Frege and non-Frege cases (i.e., that one invokes content and the other doesn't) is itself a theoretically unattractive consequence of the view. For it seems odd to claim that a different *kind* of explanation is required *merely* because the person lacks some of the information upon which the success of his/her behavior depends. If content is invoked in cases in which people happen to have all of the relevant information, then why shouldn't it *also* be invoked in cases in which they don't? Because one's theory of content requires it? In any case, a full-fledged discussion of Fodor's claims in *The Elm and the Expert* would take us too far afield. As the literature on the book demonstrates, though, Fodor's arguments there are not only far from clear, but also controversial. See, e.g., Arjo (1996), Brook and Stainton (1997), Segal (1997), and Aydede and Robbins (2001).

is wrong that MOPs should not be identified with senses, and asking ourselves the following two questions: (1) Does this *also* show that Fodor is wrong to deny that the rationality of a transition in thought, or a judgment, turns on its content?; and (2) if so, does it follow that concepts are constitutively connected to reasons and rationality?

Fodor apparently thinks that the answer to question (1) is “no”. Consider the following passage:

I would've thought there was considerable *prima facie* evidence that chains of synonymous thoughts can differ in their rationality. For example, it's irrational to flout *modes* [sic] *ponens* in transparent cases, but it needn't be when the premises are syntactically involute, in which case, it may be only a mistake. If this is right some of the time, perhaps it is right all of the time. And if it's right all the time, then maybe *any* theory that can distinguish between the MOPs of equivalent thoughts *whether or not it supposes that MOPs are senses* can provide a reconstruction of rationality that's, anyhow, as good as Frege's. What I like so much about the computational theory of mind is that, unlike Frege's, it offers the prospect of a *nonssemantic* account of rationality; hence one that has a fighting chance of being mechanizable. (2000b, p. 369, original emphasis)

Note, first, that Fodor is helping himself to a distinction between what counts as an error of *reasoning* and what counts as a *mere* mistake. For I take it that this is what the difference between *irrationally* flouting a transparent instance of modus ponens and *accidentally* flouting a non-transparent instance amounts to. Now, I don't know whether Fodor thinks there's a principled distinction to be drawn here, and if so, how he thinks it should be drawn. One way to do so would be to ground the distinction in an *a priori/a*

posteriori distinction. But of course it's not clear that, *qua* good Quinean, Fodor could do this. In any case, even if there is a principled distinction between being irrational and merely making a mistake, I don't see Fodor's point here. For if the *reason* that thoughts containing synonymous concepts can differ in their rationality is that the synonyms have distinct MOPs, then it's unclear why the issue of *whether* MOPs are senses is irrelevant to providing a 'reconstruction of rationality'. If MOPs *are* senses, as Fregeans suppose, then it seems that whether or not a (transition in) thought is rational *will* depend upon its content; and if MOPs *aren't* senses, then it won't. I simply don't see how Fodor's example affects this.

It's *prima facie* plausible, then, that the answer to (1) is "yes". For if MOPs aren't the representational vehicles of thoughts, then presumably they're thought contents (i.e., senses). And if MOPs are posited (as they traditionally have been) in order to explain such facts as, say, why John can *rationally* think the morning star is beautiful and that the evening star is ugly, then facts about rationality *will* turn on the content of thought. Moreover, it seems to me that (*contra* Fodor's claim in the last sentence of the above quote) such a Fregean position is perfectly compatible with the computational theory of mind. Perhaps the latter is incompatible with Frege's own anti-mentalistic theory. But neo-Fregeans like Peacocke don't accept Frege's anti-mentalism, and are thus free to accept the fifth thesis of Fodor's RTM: "whatever distinguishes coextensive concepts is ipso facto 'in the head'" (1998a, p. 15).²⁸

²⁸ Oddly enough, Fodor himself seems to recognize this point (1998a, p. 15n9). It's worth pointing out, moreover, that Frege himself was concerned with deduction, which is surely the best candidate when it comes to the plausibility of capturing rational relations in computational terms. So I simply don't see Fodor's point about his nonsemantic account being better off than Frege's in terms of having a "fighting chance of being mechanizable". Indeed, Fodor (1983, 2000a) himself has argued at length that the rationality involved in *nondeductive* forms of inference are problematic for the computational theory of mind. Does he think we can give a nonsemantic account of *that* kind of rationality?

However, a positive answer to (1) needn't imply a positive answer to (2). We saw earlier that Peacocke assumes that what "all Fregean IRS theories, of whatever stripe, agree upon is the central place of reasons and rationality in the individuation of concepts" (2000a, p. 332). However, it seems to me that one can adopt a version of Concept Pragmatism for (*inter alia*) the familiar Fregean reasons, while at the same time deny that concepts are constitutively connected to reasons and rationality. For appealing to concepts in an explanation of facts about rationality does not require that one take the concepts themselves to be *individuated* in terms of rationality.

To return to a previous example, suppose someone is perceiving an object as a square, and we want explain the difference in the rationality of the judgment *that's a square* and the judgment *that's a diamond* in those circumstances. We may very well need to invoke content in the explanation. But this doesn't require that SQUARE and DIAMOND *themselves* be individuated in terms of what it's rational to judge given an experience of a certain kind. They may simply be individuated in terms of the corresponding judgments, in which case the question of their rationality and epistemic status would be a *further* question, whose answer is not, as it were, written into the identity of the concepts themselves.

I argued earlier that Peacocke's general argument for Knowledge Pragmatism fails because it presupposes a treatment of Frege cases that the Cartesian will deny. We can now see that the argument fails *even if* the Cartesian treatment of Frege is false. A defeat for Cartesianism here thus does *not* imply a victory for Knowledge Pragmatism. I conclude, then, that Judgment Pragmatism is not threatened by anything either the Cartesian or the Knowledge Pragmatist has to say about Frege cases.

CHAPTER 8

CONCLUDING THOUGHTS

We started with the meta-level question: *What do we need a theory of concepts for?* I hope to have shown that taking an explanatory psychology to be the proper home of concepts can shed light on the recent debate between Concept Cartesians and Concept Pragmatists. I've argued, in particular, that adopting the psychological perspective leads to a version of conceptual/inferential roles semantics—Judgment Pragmatism—according to which concepts are not constitutively tied to rationality and knowledge.

The analytic/synthetic loomed large in our discussion, and I'd like to conclude with some remarks on its role in the dialectic. As we've seen, the Quinean Challenge is the primary motivation for the Cartesian's denial of constitutive connections among concepts. My polemical strategy was to show the very meta-level approach adopted by Cartesians in fact provides powerful explanatory reasons for positing such a distinction. There's irony here. For while Cartesians claim that Pragmatists wrongly put epistemological and philosophical concerns in the driver's seat when they construct their theories, my arguments show that Pragmatists can defend their position on the very explanatory grounds that Cartesians (rightly, in my view) take to be primary. Once we see this, though, we can also see that Cartesians themselves ultimately adopt their position for primarily philosophical reasons (i.e., Quinean scruples), despite their own methodological moralizing.

Of course, Cartesians do argue against the analytic/synthetic distinction on empirical grounds. But if my arguments are correct, Pragmatists needn't worry about these arguments. For I argued not only that these arguments don't constitute a convincing case for Concept Atomism, but also that even if they *are* successful, they're entirely compatible with a Pragmatism that does not take the constitutive roles of lexical concepts to be specified by their internal structure. The upshot of this is that Fodor is wrong to claim that what "settles" the issue between informational semantics and conceptual/inferential role accounts is that only the former are compatible with Atomism (1998a, p. 15). *Contra* Fodor, the empirical case for Atomism thus does *not* show that an informational semantics should be a defining thesis of the Representational Theory of Mind. Rather, the latter can and should be built upon a version of conceptual/inferential role semantics.

I can't hope to have convinced the reader of all of the particular claims I defended above. But, if nothing else, I hope my arguments illustrate that distinguishing the psychological and epistemological perspectives on concepts shows that Cartesians and Pragmatists alike have underestimated the number of positions available.

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