FROM SEMANTICS TO METAPHYSICS

by

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My favorite argument for the existence of numbers.

To Bernard J. Ortcutt.

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ABSTRACT

It is widely assumed in philosophy that there is a tight connection between semantics and metaphysics. Semantic theories about the meanings of natural language terms and phrases are taken to provide evidence for and against various metaphysical theses about the nature of non-linguistic parts of the world. Call this view the *widespread thesis*. Versions of the widespread thesis can be seen at work in, among other places, Davidson's arguments for the existence of events, Lewis's arguments for the existence of possible worlds, Quine's worries about quantifying into intensional contexts, and Stanley and Williamson's argument that knowledge how is a species of knowledge that. In this dissertation, I argue that the widespread thesis is mistaken: semantic theories do not generally have robust metaphysical consequences.

I contend that the best argument for the widespread thesis turn on an interpretation of semantic theories according to which they are metaphysically illuminating: the metaphysical structure of the world is directly mirrored in the structure of the theory. Terms and existential quantification in the theory signal the existence of entities in the world, predicates signal the existence of properties, and the structures of set theoretic objects invoked by the theory directly correspond to structural features of the world. Such an interpretation is not the only one available, however, and it is a substantive philosophical (and linguistic) issue which interpretation is correct.

Accordingly, I develop and defend an alternative interpretation of semantic theories according to which the structural features of these theories and of their theorems reflect biological and computational constraints on the architecture of the language faculty and historical contingencies in its evolutionary development. Supposedly metaphysically committing features of the theory—including the appearance of quantification and (ostensibly) referring terms—may thus be arepresentational artifacts of these constraints, rather than representational features that reflect metaphysical reality.

To determine the metaphysical consequences of a semantic theory then, we must determine which of its features represent language-independent characteristics of the world and which are arepresentational consequences of the structure of the language faculty. To do this, however, we must have some prior idea what the language-independent world is like—that is, we must engage in prior metaphysical theorizing that is not beholden to the semantic theory itself.

Chapter 1

Meaning, Metaphysics, and the Non-Representational Dodge

§1 Language and the world. What can we learn about non-linguistic aspects of the world through the study of language? Many philosophers would answer: *a lot*. They hew to the *widespread thesis*:

> (W) Theories about language have metaphysical entailments; therefore, theories about language constrain the range of plausible metaphysical theories, and metaphysical theories constrain the range of plausible theories about language.

As Davidson (1977, p. 199) puts it: "[I]n making manifest the large features of our language, we make manifest the large features of reality. One way of pursuing metaphysics is therefore to study the general structure of our language." And more recently, Peter Ludlow, in the course of arguing that the linguistic semantics of tense supports presentism about the nature of time, claims, "concrete questions about the nature of reality can be illuminated by what we know about semantic theory, and … important question in semantic theory may be adjudicated by certain of our metaphysical intuitions about the constitution of reality" (Ludlow 1999, p. 5).

The paradigmatic example of inference from language to metaphysics occurs in the metaphysics of events. In a series of papers, Davidson (1967, 1969, 1970) urged that the logical forms of action sentences are best accounted for by viewing them as quantifying over a special class of particulars: events. So, for example,

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(1) Brutus stabbed Caesar

has not the logical form

(2) Stabbed(Brutus, Caesar),

but rather the form

(3) ($\exists$e) Stabbed(Brutus, Caesar, e).

'e' is a dedicated event-variable, and (3) may be glossed as *There is an event e such that e is a stabbing of Caesar by Brutus* (see Davidson 1967, pp. 118-119). Such an account of the logical forms of action sentences nicely captures various of their entailments. For example, if the logical form of

(4) Brutus stabbed Caesar with a knife

is

(5) ($\exists$e) (Stabbed(Brutus, Caesar, e) & ($\exists$x)(Knife(x) & With(e, x))),

then (4) entails (1), as expected.

As it stands, this account is fairly crude. However, current linguistic theory has extensively developed Davidson's basic insight. Covert event quantification has been incorporated into both truth-theoretic and model-theoretic semantic theories, and its applications have been generalized beyond the analysis of action sentences.[1]

Now, Davidson contends that there are good metaphysical reasons to believe in events: the existence of events (as individuals) is crucial to the best accounts of the metaphysics of action, of causality, and of the relation between the mental and the physical (Davidson 1969, pp. 164 − 165). He also contends, however, that there is

> a more direct consideration… in favour of an ontology of events, which is that without events it does not seem possible to give a natural and

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[1] See Pianesi and Varzi (2000) and Higginbotham (2000) for an overview.

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acceptable account of the logical form of certain sentences of the most common sorts; it does not seem possible, that is, to show how the meanings of such sentences depend upon their composition. (Davidson 1969, p. 166.)

According to Davidson, the occurrence of events in our best compositional semantic theories is, *by itself,* reason enough to believe in their existence. Indeed, Davidson takes the argument from semantics not only to be sufficient, but to be primary: the metaphysical considerations in favor of events are not independent, but are mere "symptoms" of the semantics (Davidson 1969, p. 166).

§2 **The direct argument**. The orthodox thesis gets much of its intuitive appeal from the *direct argument*. Here is a schematized version:

(DR-1) Discourse *d* has representation conditions [generally: truth conditions] ϕ.

(DR-2) *d* correctly represents [is true].

(DR-3) ϕ.

The argument is generally run by picking some positive sentence of *d*.[2] When we apply it to the Davidsonian semantics for adverbial discourse, we get the following argument that the world contains events:

(6) JOSH WALKS SLOWLY is true iff there is an event e, such that e is a walking, e is slow, and Josh is the agent of e.

(7a) JOSH WALKS SLOWLY is true iff Josh walks slowly.

(7b) Josh walks slowly.

(8) There is an event e, such that e is a walking, e is slow, and Josh is the agent of e.

(9) There are events.

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[2] A *positive sentence* of a discourse is one in which every bit of logical vocabulary occurs inside the scope of at least one vocabulary item characteristic of the discourse.

(6) is a theorem of empirical linguistics;[3] (7a), an instance of the T-schema, is an *a priori* constraint on truth; (7b) is an easily accessible fact about the world—it can be known just by looking. And so, from plausible premises, we conclude that there are events.

§3 **The non-representational dodge.** Davidson's event semantics is a *representational* semantic theory: it explains meaning of a sentence in terms of what that sentence represents the world as being like. 'Josh walks slowly' is true just in case Josh walks slowly—that is, the sentence represents the world as being such that Josh walks slowly. Representational semantic theories—truth theories like the Davidson's, along with Montagovian model theories—are the dominant theories of meaning in empirical linguistics. However, among philosophers, such theories compete for attention with accounts of meaning that are more or less non-representational. According to these theories, the purpose and use of language—communicative, expressive, representational, or what have you—outstrips its representational content. Linguistic norms of use and success thus diverge from truth conditions. And, it might be thought, if the representational content of a discourse is non-existent, or sufficiently attenuated, or normatively divorced from successful use, then the direct argument is unsound.

Different non-representational approaches target different premises of the argument. Some charge that the first premise gets the meaning of the discourse wrong, so that, although some sentences of the discourse are true, they don't have the truth conditions attributed to them in the argument. Rather, much of what the direct argument assigns to representational content is *non*-representational. Other approaches concede

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[3] The most natural reading of JOSH WALKS SLOWLY is as a generic. But it has a non-generic reading, too—imagine somebody contemporaneously narrating my day, as they might a sporting event: "Josh walks slowly into the room. He opens his computer and cracks his fingers purposefully. It looks like he means business, today," etc.

that the first premise correctly characterizes the truth conditions of the discourse, but contend that the positive claims of the discourse are in fact false (or, perhaps, truth-valueless). They avoid the pitfalls of an error theory for the discourse by arguing that the norms governing deployment of the discourse are indifferent to truth and falsity, and engage non-representational content, instead.[4] And, finally, some approaches reject both premises, claiming that the representational semantics both gets the truth conditions wrong and misconstrues their relevance to language use.

What all of the non-representational semantics have in common is the claim that the correct meaning theory doesn't traffic (exclusively) in truth. Truth provides the language/world connection needed to underwrite inferences from semantics to metaphysics, and so the correct, non-representational, meaning theory blocks such inferences. I'll argue that this line of thought is mistaken: to whatever extent representational theories of meaning are committing, non-representational theories are committing, too. Thus, even if the right semantic theory turns out to be non-representational, we're still faced with the question: what can we learn about the non-linguistic world from the study of language? And the *prima facie* answer is still: a lot.

§4 The commitments of non-representationalism. The direct argument appeals to the obvious truth of various everyday claims and takes representational semantic theories to lay out the metaphysical conditions under which those claims are true. But, for the purposes of the argument, there's nothing special about truth—*any* semantic property (or

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[4] An error theory is any semantic or metaphysical theory according to which we are systematically mistaken about a very large number of our ordinary judgments. The classic error theory is Mackie's normative error theory, according to which all of our positive ethical judgments—*e.g.*, that charity is good, that murder is wrong, *etc.*—are in fact false, since the universe contains no properties with the right sorts of features to be the referents of our ethical terms. See Mackie (1977).

even non-semantic) property will do. The representational version direct argument is simply an instance of a more general argument:

(D-1) Discourse *d* has semantic property *P* under conditions ϕ.

(D-2) *d* has semantic property *P*. _

(D-3) ϕ.

So we can adapt the direct argument to make use of *assertability*, the favored semantic property of non-representational meaning theories:

(DN-1) Discourse *d* has assertability conditions ϕ.

(DN-2) *d* is properly assertable. _

(DN-3) ϕ.

The adapted argument appeals to the obvious assertability of various everyday claims and takes non-representational semantic theories to lay out the metaphysical conditions under which those claims are assertable.

The availability of the direct argument for non-representational meaning theories is often overlooked, I think, because its instances can't be spelled out in quite the same way as for representational theories. For example, this argument is clearly unsound, given the divergence of truth and assertability posited by non-representationalists:

(10) JOSH WALKS SLOWLY is assertable iff ϕ.

(11a) JOSH WALKS SLOWLY is assertable iff Josh walks slowly.

(11b) Josh walks slowly. _

(12) ϕ.

(10) is just the non-representational meaning theorem, whatever it may be, and so the non-representationalist is bound to accept it. But every non-representational theory

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rejects (11a), and some further reject (11b). (10) – (12), then, can't be used to generate metaphysical commitments.

In the representational version of the direct argument, (6) – (9), we can invoke a *use* of JOSH WALKS SLOWLY—premise (7b)—because truth binds use and mention through the T-schema. Assertability doesn't bind use and mention, so we can't detour through use in the non-representational case. But we don't have to: instead of relying on a straightforward, difficult-to-deny judgment about what the (non-linguistic) world is like, we can rely on a straightforward, difficult-to-deny judgment about language use.

(13) JOSH WALKS SLOWLY is assertable iff ϕ.

(14) <u>JOSH WALKS SLOWLY is assertable.</u>

(15) ϕ.

(13) – (15) is valid; to deny that it is also sound, the non-representationalist would have to claim that we can *never* properly assert JOSH WALKS SLOWLY. This is just as implausible as claiming that Josh never walks slowly and leads just as directly to a sort of error theory for adverbial discourse. For we certainly do sometimes assert JOSH WALKS SLOWLY, and these assertions oftentimes seem proper. If they never are, then we're *systematically mistaken* in making them.[5]

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[5] There is a roundabout way we might use assertability conditions to connect use and mention, and so construct a non-representational argument that parallels (6) – (9). According to non-representational meaning theories, assertive uses of a sentence communicate something other than the truth-conditional content of the sentence. Let $\lceil <\alpha> \rceil$ denote whatever an utterance of α communicates, truth-conditional or otherwise. We then have:

    (16) JOSH WALKS SLOWLY is assertable iff ϕ.
    (17a) JOSH WALKS SLOWLY is assertable iff <JOSH WALKS SLOWLY>.
    (17b) <u><JOSH WALKS SLOWLY>.</u>
    (18) ϕ.

To block the argument then, the non-representationalist would have to either (a) deny that what is communicated is tied to assertability conditions or (b) deny whatever is communicated by JOSH WALKS SLOWLY. Neither option is attractive. The connection between assertability conditions and what is

Denying that the positive sentences of a discourse are ever properly assertable also undercuts the motivation for moving to a non-representational meaning theory in the first place. Non-representational semantics are attractive because there seem to be some situations in which a sentence is false but assertable, and other situations in which an assertion of a sentence communicates more than its truth-conditional content. The non-representational theories are meant to account for these facts about assertability. If, according to such a theory, the sentences of a discourse are systematically unassertable, then there are no such facts to account for.

As do representational theories, non-representational theories not only generate metaphysical commitments for the theorist, but also reveal commitments of the language users of whom the theory is true. In the representational case, the revelation is a consequence of the intensional equivalence between φ and the sentence mentioned on the left-hand side of the meaning theorem. An assertion that Josh walks slowly *just is* an assertion that there is a slow walking event with Josh as its agent. If an assertion is sincere, then the speaker believes what he asserts; and we are, trivially, committed to what we believe. Thus, a speaker who sincerely asserts that Josh walks slowly is committed to there being a slow walking event with Josh as its agent—and so is committed to there being events. This is so whether or not the speaker explicitly accepts the representational meaning theory, just so long as the theory is actually true of him.

Non-representational meaning theories expressly deny the intensional equivalence of a sentence and its assertability conditions. And because assertability conditions and truth conditions diverge, sincere assertion doesn't necessarily require belief of what is

communicated *explains why* assertions of JOSH WALKS SLOWLY communicate <JOSH WALKS SLOWLY>—as opposed to, say, that Josh walks slowly, the purely truth-conditional content. And systematically denying whatever JOSH WALKS SLOWLY communicates again mires us in error-theory.

asserted. Even so, a non-representational semantics will have some sort of sincerity norm: a speaker who sincerely asserts a sentence will believe that its assertability conditions are met. Thus, a speaker who sincerely assertively utters JOSH WALKS SLOWLY will believe ϕ, the assertability conditions posited by the non-representational semantics, and so will be committed to ϕ. Indeed, it seems that truth conditions are committing *only* insofar as they figure into assertability conditions. If there were no general norm to speak (what one believed to be) the truth, then we couldn't infer from assertion to belief, and so to commitment.[6]

The non-representationalist might try to resist this line of argument by retreating to meta-linguistic claims about the language of the meaning theory. The occurrence of ϕ on the right-hand side of the semantic theorem, the response goes, is itself to be understood non-representationally. A speaker thus need not believe that ϕ is *true* in order to meet the sincerity norm, but need only believe that ϕ is somehow appropriate. Since the speaker isn't committed the truth of ϕ, he bears no metaphysical commitments.

The trouble with this response is that it simply assumes that the non-representational content of ϕ isn't metaphysically committing. But an instance of the direct argument is available for any sentence that is sometimes assertable. If we're to avoid an error theory for the original discourse, ϕ better be assertable in some contexts. Thus, there is an instance of the direct argument that generates metaphysical commitments for ϕ. It's no help for the non-representationalist to retreat yet further to a

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[6] We can generalize this point to belief by treating belief as a sort of mental assertion. The truth-conditional content of a belief is committing only insofar as there is a norm to believe truly. A non-representational semantics for beliefs would deny that any such norm holds generally, there being instead a norm to believe *correctly*, where correctness does not (always) require truth.

non-representational semantics for the assertability conditions of φ, for we will again be able to run the direct argument.

§5 **Some toy examples.** So far, I've been discussing non-representational theories in the abstract. Now I'd like to illustrate the metaphysical commitments that various such theories incur by applying the direct argument to a few concrete examples. There are various flavors of non-representational semantic theory:

- Conventional paraphrase
- Pragmatic theories
  - Positive pragmatic theories
  - Negative pragmatic theories
- Pretense theories and fictionalism
- Use theories

I'll describe a toy example of each and compare its metaphysical commitments to those generated by a representational semantics for a simple discourse.

Consider a discourse, *d*, the characteristic vocabulary of which is the adjective FLAT. The other vocabulary items of the discourse are the names KANSAS and COLORADO, the definite description THE PANCAKE, the copula IS, and the various logical expressions of English. The discourse is the denumerable set of all grammatical sentences that can be formed from its vocabulary: $d = \{d_1, d_2, \ldots\}$. We'll be interested in the semantics of the characteristic vocabulary, and the contribution it makes to the semantics of the sentences of *d*; we'll take the semantics of all of the other vocabulary as given and unproblematic.

A semantics for *d* consists in:

(i) a statement of representational content—i.e., truth conditions—for each sentence of *d*;

(ii) a statement of assertability conditions for each sentence of *d*;

(iii) a statement of what is communicated by each sentence of *d*.

Given that *d* has infinitely many sentences, a proper semantics will give a recursive characterization of truth conditions, assertability conditions, and what is communicated. The metaphysical commitments of a discourse can be generated from just a few sentences, so I'll only describe a fragment of each semantic theory below. For the sake of the illustration I'm going to assume that Jason Stanley's extreme thesis about context-sensitivity is correct: any context sensitivity of content must be explicitly represented at the level of logical form—*i.e.*, it must appear in the representational content.[7]

Here are some *obvious facts*. The pancake is flatter than Kansas, and Kansas is flatter than Colorado.[8] There are some contexts in which 'The pancake is flat' is true, or at least assertable; and there are others in which it is not. In some, but not all, of the contexts in which THE PANCAKE IS FLAT is true and/or assertable, KANSAS IS FLAT is also true and/or assertable. In some contexts in which KANSAS IS FLAT is true and/or assertable, COLORADO IS FLAT is not true and/or assertable. Any adequate semantics for *d* must explain these facts about truth and assertability.[9]

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[7] See Stanley (2005).

[8] Note that these aren't facts we can state in *d*, since FLATTER isn't one of its vocabulary items.

[9] If you don't think all of these facts are obvious—perhaps because you don't know which pancake I'm talking about or because you think that pancakes are not very flat at all—don't fret: just take them as stipulations.

§5.1 The purely representational semantics, take 1 (RS1). The simplest representational

semantics for *d* takes the representational content of its sentences to be given by their

disquotational truth conditions—i.e., by instances of the T-schema.

Truth conditions

   (RS1-1) THE PANCAKE IS FLAT is true iff the pancake is flat.

   (RS1-2) KANSAS IS FLAT is true iff Kansas is flat.

   (RS1-3) COLORADO IS FLAT is true iff Colorado is flat.

Assertability conditions

   (RS1-4) A sentence α is assertable iff α is true.

What is communicated

   (RS1-5) A sentence α communicates its truth conditional content.

According to RS1, assertability conditions and truth conditions are co-extensive, and so

an assertion of KANSAS IS FLAT commits the speaker to Kansas being flat, to there being

flat things, etc. Since (RS1-1) – (RS1-5) aren't context sensitive, the semantics is

committed to there being a single (apparently non-relational) property that all flat things

share. Given the obvious facts, then, RS1 is clearly inadequate. There is some context in

which KANSAS IS FLAT is assertable and some other context in which it isn't. By (RS1-4),

it is assertable just in case it is true; and by (RS1-1), it takes the same truth value in every

context. But now we have a contradiction: in each context, KANSAS IS FLAT must be

both true and false. So either RS1 is wrong, or we must reject the obvious facts and

adopt an error theory.

§5.2 The purely representational semantics, take 2 (RS2). One way of salvaging the

representational semantics for *d* is *linguistic paraphrase*. According to a linguistic

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paraphrase account, the logical forms of the sentences of *d* are different than their surface forms. (Davidsonian event semantics, for example, is a linguistic paraphrase account of adverbial discourse.)

Truth conditions

(RS2-1) THE PANCAKE IS FLAT is true in context c iff the pancake is flatter than *f*(c), a contextually specified object.

(RS2-2) KANSAS IS FLAT is true iff Kansas is is flatter than *f*(c), a contextually specified object.

(RS2-3) COLORADO IS FLAT is true iff Colorado is is flatter than *f*(c), a contextually specified object.

Assertability conditions

(RS2-4) A sentence α is assertable iff α is true

What is communicated

(RS2-5) A sentence α communicates its truth conditional-content.

Linguistic paraphrase does not result in a non-representational semantics: RS2 is purely representational because, according to (RS2-4), it identifies assertability conditions and truth conditions and, according to (RS2-5), what a sentence communicates is its truth-conditional content. FLAT picks out different properties in different contexts, and so KANSAS IS FLAT can be true in some contexts and false in others. RS2 thus has no trouble accommodating the obvious facts.

Suppose that for context c, *f*(c) = Ohio. Then a speaker who utters KANSAS IS FLAT in c is committed to Kansas being flatter than Ohio, to there being things flatter than Ohio, etc. It seems that c is among the contexts in which KANSAS IS FLAT is obviously

assertable—it's one of those contexts quantified over in the obvious facts—and so from RS2 we can infer that not only does the speaker bear such commitments, but in fact Kansas is flatter than Ohio, there are things flatter than Ohio, etc.

§5.3 Conventional paraphrase (CP). Suppose that you think that RS1 gives the right account of the representational content of *d*. So you think that many uses of KANSAS IS FLAT are literally false, even though they appear perfectly natural. The conventional paraphraser claims that these uses are not, in fact, literal: they are simply eliminable short hand for a different claim, one that is actually true. According to the paraphraser, there is an explicit convention to systematically use the sentences of *d* in place of sentences of some other discourse *d**.

Let *d** be a discourse containing the following sentences, among others: THE PANCAKE IS FLATTER THAN OHIO, KANSAS IS FLATTER THAN OHIO, and COLORADO IS FLATTER THAN OHIO. $t^c(x)$ is a (context-dependent) function that takes sentences of *d* onto sentences of *d**; let $t^c(\text{KANSAS IS FLAT}) = \text{KANSAS IS FLATTER THAN OHIO}$, etc.

Truth conditions

    (CP-1) THE PANCAKE IS FLAT is true iff the pancake is flat.

    (CP-2) KANSAS IS FLAT is true iff Kansas is flat.

    (CP-3) COLORADO IS FLAT is true iff Colorado is flat.

Assertability conditions

    (CP-4) A sentence α is assertable in c iff $t^c(\alpha)$ is true.

What is communicated

    (CP-5) A sentence α communicates the truth conditional content of $t^c(\alpha)$.

So, when a speaker asserts KANSAS IS FLAT in c, he doesn't *mean* Kansas is flat—he means Kansas is flatter than Ohio.[10]

Thus, a speaker who utters KANSAS IS FLAT in c is committed to Kansas being flatter than Ohio, to there being things flatter than Ohio, etc. Context c is among the contexts in which KANSAS IS FLAT is obviously assertable—it's one of those contexts quantified over in the obvious facts—and so from RS2 we can infer that not only does the speaker bear such commitments, but in fact Kansas is flatter than Ohio, there are things flatter than Ohio, etc. Note that these are *exactly the same* commitments borne by RS2.[11]

§5.4 A positive pragmatic theory (P$^+$).[12] According to positive pragmatic theories, a discourse communicates its truth-conditional content and more besides. (The standard explanations of the temporal ordering effect of 'and' and of the apparent exclusivity of many occurrences of 'or' are both positive pragmatic theories.)[13] Suppose that (like the advocate of RS1 or CP) you think that that the representational content of FLAT is context insensitive. However, you also want to preserve the literal truth of utterances of KANSAS IS FLAT in contexts in which it appears true. One way to do this is to insist that *everything* except the least flat (possible) thing is literally flat—so that KANSAS IS FLAT is true in

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[10] Another way to think about conventional paraphrase is as speaking in code. When George Smiley says to his people, "The albatross flies at midnight," he isn't saying that the albatross flies at midnight: he's saying that Karla is crossing the East German border at two a.m.

[11] We can—and in the absence of an explicit convention, do—use the sentences of *d* literally. When we do so, we take on the commitments of RS1 instead of RS2. Incidentally, this answers Alston's (1958) complaint against Quine's (1953, especially pp. 1-19) paraphrase program that every deflationary paraphrase is equally an inflationary paraphrase in the other direction. Alston is free to use the deflated discourse as short hand for the inflated discourse if he wishes; but Quine is using the deflated discourse with its literal truth conditions.

[12] *Cf.* Cappelen and Lepore (2005).
[13] See, for example, Grice (1989).

every context—and then to account for the *un*assertability of KANSAS IS FLAT in some contexts *via* non-representational content.

Truth conditions

(P$^+$-1) THE PANCAKE IS FLAT is true iff the pancake is flat.

(P$^+$-2) KANSAS IS FLAT is true iff Kansas is flat.

(P$^+$-3) COLORADO IS FLAT is true iff Colorado is flat.

Assertability conditions

(P$^+$-4) A sentence $\alpha = \lceil$ x IS FLAT $\rceil$ is assertable in c iff α is true and x is flatter than

f(c), a contextually specified object.

What is communicated

(P$^+$-5) A sentence $\alpha = \lceil$ x IS FLAT $\rceil$ communicates that x is flatter than f(c), a

contextually specified object.

Note that the assertability conditions posited by P$^+$ are exactly those posited by RS2 and CP: *e.g.*, KANSAS IS FLAT is assertable in c just in case Kansas is flatter than Ohio.[14] RS2, CP, and P$^+$ differ only in how they take the assertability conditions to be generated. According to RS2, the assertability conditions are coextensive with the truth conditions— they are thus generated by semantics module of the linguistic faculty. According to CP, the assertability conditions stem from explicit convention.   According to P$^+$, the assertability conditions are the outcome of ordinary Gricean pragmatic reasoning.[15] Suppose that a speaker utters KANSAS IS FLAT. His interlocutor thinks, "Well, of course

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[14] Since everything except the least flat thing is flat, the second conjunct of the assertability conditions (x is flatter than f(c)) entails the first ($\lceil$ x IS FLAT $\rceil$ is true—i.e., x is flat).

[15] See Grice (1989).

Kansas is flat—nearly everything is flat. Why would he utter such an obvious truth? His utterance can be relevant to the conversation only if he's trying to communicate something else. What could this be? Ah, that Kansas is flatter than Ohio!"[16]

What, then, are the commitments of *d*, according to P[+]? A speaker who sincerely utters KANSAS IS FLAT is committed to Kansas being flat, to it being flatter than Ohio, to there being flat things and things flatter than Ohio, etc. So it appears that P[+] has all the commitments of RS2 and CP, plus *additional* commitments to non-relationally flat things (as in RS1).[17]

§5.5 *A negative pragmatic theory (P⁻).*[18] Negative pragmatic theories take the positive sentences of a discourse to be systematically false, and invoke pragmatic reasoning to explain why they are, in some contexts, nonetheless assertable. So, suppose that, like the advocate of CP, you think that RS1 gets the representational content of *d* correct, and you think that, in fact, *nothing* is flat. However, you don't find it plausible that there's an explicit convention to use *d* in place of *d*.

Truth conditions

(P⁻-1) THE PANCAKE IS FLAT is true iff the pancake is flat.

(P⁻-2) KANSAS IS FLAT is true iff Kansas is flat.

(P⁻-3) COLORADO IS FLAT is true iff Colorado is flat.

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[16] This, of course, is a only caricature of pragmatic reasoning. See the above-cited Grice for more nuanced and detailed account of such reasoning.

[17] I'll argue below that these apparently additional commitments don't really impose any additional metaphysical *costs* on P[+].

[18] *Cf.* Unger (1975, pp. 47-91).

Assertability conditions

(P⁻-4) A sentence $\alpha = \ulcorner x \text{ IS FLAT} \urcorner$ is assertable in c iff x is flatter than $f(c)$, a

contextually specified object.

What is communicated

(P⁻-5) A sentence $\alpha = \ulcorner x \text{ IS FLAT} \urcorner$ communicates that x is flatter than $f(c)$, a

contextually specified object.

Although both P⁺ and P⁻ assign the same truth conditions to *d*, P⁺ takes those truth

conditions to be by-and-large met, whereas P⁻ takes them to go systematically unmet.

The pragmatic reasoning that generates the assertability conditions is parallel to that

posited by P⁺. Suppose that a speaker utters KANSAS IS FLAT. His interlocutor thinks,

"Kansas isn't flat! Nothing is flat! Why would he utter such an obvious falsehood? His

utterance can be relevant to the conversation only if he's trying to communicate

something else. What could this be? Ah, that Kansas is flatter than Ohio!"

P⁻ generates exactly the same commitments as RS2 and CP. A speaker who utters

KANSAS IS FLAT in context c is committed to Kansas being flatter than Ohio, to there

being things flatter than Ohio, etc. Despite the representational content that P⁻ attributes

to *d*, P⁻ does not generate a commitment to there being non-relationally flat things, since

truth isn't a norm of assertion according to P⁻: the non-representational content of *d*

overrides its representational content.

§5.6 A short digression. RS2, CP, P⁺, and P⁻ all take the assertability conditions of

KANSAS IS FLAT in context c to be the same: that Kansas is flatter than Ohio. Thus to

some extent they all generate similar metaphysical commitments. However, they differ

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in that they give different answers to the question: are there, *strictly speaking,* any flat things? RS2 answers "yes" because FLAT just means *flatter than f(c)*. CP and P⁻ answer "no" (or "very few") because they take FLAT to mean *absolutely flat*. P⁺ answers "yes" because flat means something like *such that it is possible to be less flat*. This would seem to engender subtle differences in their metaphysical commitments: RS2 is committed to there being flat things, but no non-relationally flat things; CP and P⁻ aren't committed to there being flat things at all; and P⁺ is committed to there being non-relationally flat things.

But these don't represent real differences over what the world is like. Consider a model containing the pancake, Kansas, Colorado, and Ohio in its domain. The model also contains a relation, *flatter than*, and the properties *absolutely flat* and *such that it is possible for there to be something less flat.* The *flatter than* relation is such that the pancake > Kansas > Ohio > Colorado; no object in the domain is absolutely flat; every object in the domain is such that it is possible for there to be something less flat. RS2, CP, P⁺, and P⁻ are all compatible with this model and the obvious facts; they differ only in what they take the semantic value of FLAT to be—*i.e.*, which property in the model it picks out—and how the assertability conditions of various sentences are generated. The differences in how they distribute T's and F's, then, don't make for real differences in metaphysics.

§5.7 Fictionalism (FS). According to fictionalist and pretense theories, we do not assert or believe that the sentences of *d* are true. Rather, we assert that they are *fictional*—true according to an appropriate fiction—and *pretend* that they are true. Like CP and P⁻, FS

takes the positive sentences of *d* to be false in nearly all contexts, but nonetheless assertable because truth and assertability come apart. FS differs from CP and P⁻ in how the assertability conditions are generated.

Truth conditions

 (FS-1) THE PANCAKE IS FLAT is true iff the pancake is flat.

 (FS-2) KANSAS IS FLAT is true iff Kansas is flat.

 (FS-3) COLORADO IS FLAT is true iff Colorado is flat.

Assertability conditions

 (FS-4) A sentence α is assertable in c iff: if α is true iff ϕ, then according to a

   contextually appropriate fiction, ϕ.

What is communicated

 (FS-5) A sentence α communicates that according to a contextually appropriate

   fiction, ϕ.

FS, as given in (FS-1) – (FS-5), has very different commitments than any of the above semantics: an assertion of KANSAS IS FLAT in c commits the speaker to there being a contextually appropriate fiction according to which Kansas is flat, the existence of fictions, etc. It doesn't commit the speaker to there being flat things, or things flatter than Ohio.

But (FS-1) – (FS-5) is the only simplest possible fictionalist account: it posits *bare* facts about fictions—what fictions there are, what is the case according to them, which ones are contextually appropriate, etc.[19] Any plausible fictionalism, however, will

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[19] Bare from the semantic point of view, that is. We might think that these facts supervene on lower-level facts, but that knowledge of this metaphysical structure is semantically inert. (In the same way that knowing that water consists largely of H_2O molecules is irrelevant to knowing the meaning of WATER.)

have to give an independent characterization of the fictions and their contents--otherwise it will offer only very shallow explanations of the obvious facts. So, amend FS with

(FS-6) In every context c, there is an appropriate fiction $s(c)$, such that an object x is flat according to $s(c)$ iff x is flatter than a contextually salient object $f(c)$.

(FS-6) tells us under what conditions an appropriate fiction exists for a context and gives a systematic account of what is true in the fiction. Presumably, (FS-6) is not just true, but constitutes part of a speaker's tacit knowledge underwriting his competence with FLAT. Once we add (55), FS has the same assertability conditions, communicated content, and metaphysical commitments as CP and P^-.

§5.8 Two use theories (U1, U2). There is a nearly endless variety of use theories. Their common claim is that the meaning of a discourse is to be explained, not by how its sentences relate to the world, but by how its sentences are used. They generally deny that the discourse in question has any representational content at all. I'll consider two different use theories here: a social practice theory (U1) and a radical speech-act theory (U2).

According to the social practice theory, the assertability conditions of a discourse depend, not on truth, but upon (usually tacit) social norms that approve or disapprove of utterances.[20]

Truth Conditions

Null

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[20] Just what the social practice theory says about truth can vary wildly. A few options: (a) truth is a real, non-epistemically constrained property, but is semantically uninteresting because we can't have access to it; (b) there is no property truth, 'true' is simply a term that expresses approval of an utterance; (c) truth just is acceptability under the operative social norms.

Assertability Conditions

    (U1-1) A sentence α is assertable in context c iff there is a social norm that approves

        of utterances of α in c.

What is communicated

    (U1-2) A sentence α communicates that a social norm approves its utterance in c.[21]

The most obvious commitment of U1 is that there are social norms: a speaker who utters

KANSAS IS FLAT is committed to there being a social norm that approves his utterance.

As with the simple fictionalist account, the explanatory power of (U1-1) – (U1-2) is fairly

limited. Any plausible social practice theory must give an account of when there is a

social norm approving an utterance (even if this account is patchwork rather than

systematic.) But this account will fill out the assertability conditions and communicated

content—and thus the metaphysical commitments—of U1. So, for example, if we add

    (U1-3) For every context c, there is a social norm $n(c)$, such that $n(c)$ approves

        utterances of $\alpha = \ulcorner x \text{ IS FLAT} \urcorner$ iff x is flatter than $f(c)$, a contextually determined

        object.

to U1, U1 shares assertability conditions, communicated content, and metaphysical

commitments with CP, P⁻, and FS.[22]

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[21] A social practice theory may not explicitly traffic in "what is communicated," since the conversational practice that the norms characterize might not be one of communication. Nonetheless, a listener will be in a position to make certain inferences upon hearing an utterance of α, given its success (read assertability) conditions; so just take what is communicated to be what a listener is entitled to infer from an utterance of α.

[22] Commitment can't be avoided by insisting on analyzing the social norms of approval in terms of other norms. While such an approach may avoid a commitment to things flatter than Ohio, it fully embraces a commitment to a rich (and semantically fundamental) ontology of norms.

According to a radical speech-act theory of meaning, we don't represent the world with our assertions; rather, we use them to *do* various things, such as affect the attitudes and behavior of others.[23]

Truth Conditions

    Null

Assertability Conditions

    (U2-1) A sentence α is assertable by speaker s in context c iff s has an end, e, and s's

           utterance of α in c will contribute to the realization of e.

What is Communicated

    (U2-2) A sentence α communicates that a speaker, s, who utters it has an end, e, and

           s's utterance of α will contribute to the realization of e.[24]

The immediate commitments of U2 include ends and their realizability. But the contents of ends will themselves generate additional commitments[25]—the speaker will need to represent e to himself, have the expectation that his utterance of α will (help to) make the world such that e is realized, etc.

Now there may be no systematic theory of what utterances contribute to the realization of which ends. But in every *particular* case in which an utterance is appropriate, there will be some such relation between the utterance and an appropriate

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[23] Less radical speech-act theories allow that representation is one of the uses to which we put language. For a mainstream speech-act theory, see Austin (1975). To my knowledge, nobody actually advocates a radical speech-act theory.

[24] As with social practice theories, what is communicated is just what a listener is entitled to infer from an utterance of α.

[25] Much as the fictions of fictionalism and the norms of social practice theory generate additional commitments when laid out with any specificity.

end, and the speaker will be aware of the relation—otherwise U2 would fail to explain his linguistic competence. So, for example, suppose that you and I are discussing where I will go on vacation. You want me to go to Kansas, and believe that if I think Kansas is flat, I'll go there. Your end—that I go to Kansas—and your belief that an utterance of KANSAS IS FLAT will help bring that about make KANSAS IS FLAT assertable in this context. But you are then committed to there being a Kansas, to there being a Josh, to it being possible for Josh (and so, someone) to go to Kansas, etc.[26]

§6 **Revenge of the error theories.** So, non-representational theories of meaning are no more metaphysically neutral than representational truth theories. Many such theories—those that simply transform truth-conditions into pragmatic assertability conditions or conditions on the existence and content of an assertion-governing fiction, for example—will have *exactly the same* commitments as some nearby representational semantics. Even those non-representational theories whose metaphysical commitments diverge sharply from those of the best representational theory for a discourse will have no metaphysical advantage over a conventional paraphrase that takes the assertability conditions of the non-representational theory to be the reducing discourse.

One important corollary to this lack of metaphysical innocence is that non-representational semantics are, like their representational counterparts, susceptible to a sort of error theory. Non-representational semantics parry the threat of traditional error theories by claiming that the sentences of a discourse may be properly assertable even

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[26] What this shows is that any *apparent* metaphysical economy claimed by a radical speech-act theory is a consequence only of its generality. If the theory is to have any predictive or explanatory value when applied to individual cases, it must pay the metaphysical costs. Roughly the same point holds for fictionalist and social practice theories.

when systematically false, and that we (understandably) confuse assertability and truth. But whatever norms of assertion the non-representational semantics posits could likewise go *systematically* unmet. Suppose, for example, that—for whatever reason—nothing is really flatter than anything else.[27] This metaphysical fact would confound not only the representational semantics RS2, but also the non-representational CP, P^+, P^-, and FS. If one of the latter were the correct semantics for FLAT, we would be systematically mistaken in our judgments that sentences of *d* are assertable.

There are two moves the non-representationalist might make in response to this charge of error. The first is to offer a revised semantics, one that provides assertability conditions that don't advert to the flatter-than relation and so are satisfiable given the metaphysical facts. The second is to claim that, although the sentences of *d* really are systematically unassertable, many are nonetheless *hyperassertable*; we mistake the hyperassertability of these sentences for assertability.[28] Of course, hyperassertability conditions will themselves generate metaphysical commitments, and so any appeal to hyperassertability will itself be liable to pervasive error. Note these moves are exactly parallel to those available to the representationalist. The representationalist can provide truth conditions that don't appeal to the flatter-than relation, or he can invoke assertability conditions (and thus adopt a non-representational semantics). The *only* difference between the defenses of the representationalist and the non-representationalist against the threat of systematic error is the level of content at which the stand is made.[29]

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[27] Perhaps mereological nihilism is true—so that there are no composite objects—and the mereological simples don't stand in the flatter-than relation to each other.

[28] Hyperassertabililty is to assertability as assertability is to truth.

[29] Or from which the retreat is begun, I suppose.

§7 The upshot. Non-representational theories of meaning do not in general have any metaphysical advantage over representational theories. Particular non-representational theories may very well differ in their metaphysical commitments from particular representational theories; but this metaphysical difference arises simply from the differences in content between the theories. After all, even different purely representational semantics can have different metaphysical consequences.

If we could also show that non-representational theories in general carried greater metaphysical burdens than representational theories, then we would have a *ceteris paribus* argument against non-representational theories (insofar as ought to prefer metaphysically parsimonious theories). But it is hard to see how such an argument would go. The susceptibility of both representational and non-representational semantics to the direct argument turns simply on the *use* of the sentences that give the assertability (or truth) conditions of the discourse. Any strategy for eliminating or deflating this use would seem equally available to representationalists and non-representationalists.

Although these metaphysical considerations don't offer an argument against non-representational semantics, they do defeat an attitude of indifference—or "quizzicality," in Yablo's (1998, p. 231) phrase—the non-representationalist might have towards metaphysical questions. In evaluating the viability of a non-representational semantics, we must take account of both our metaphysical intuitions—as data that the semantics must respect—and of our best metaphysical theories—as autonomous theories with which the semantics must be reconciled. And if a non-representational meaning theory does turn out to be correct, semantics might still reveal something about "the large features of reality."

References

Alston, William. 1958. "Ontological Commitments," *Philosophical Studies* 9, 8-16.

Austin, J.L. 1975. *How to do Things with Words,* 2nd edn., ed. J.O. Urmson and Marina Sbisà (Cambridge, Mass.: Harvard University Press).

Cappelen, Herman and Ernie Lepore. 2005. *Insensitive Semantics: A Defense of Semantic Minimalism and Speech Act Pluralism* (Malden, Mass.: Blackwell Publishing).

Davidson, Donald. 1967. "The Logical Form of Action Sentences," in Donald Davidson, *Essays on Actions and Events* (New York: Clarendon Press, 2001), 105-122.

_____. 1969. "The Individuation of Events," in Donald Davidson, *Essays on Actions and Events* (New York: Clarendon Press, 2001), 163-180.

_____. 1970. "Events as Particulars," in Donald Davidson, *Essays on Actions and Events* (New York: Clarendon Press, 2001), 181-188.

_____. 1977. "The Method of Truth in Metaphysics," in Donald Davidson, *Inquiries into Truth and Inerpretation* (New York: Clarendon Press, 1984), 199-214.

Grice, Paul. 1989. *Studies in the Way of Words* (Cambridge, Mass.: Harvard University Press).

Higginbotham, James. 2000. "On Events in Linguistic Semantics," in James Higginbotham, Fabio Pianesi, and Achille C. Varzi (eds.), *Speaking of Events* (New York: Oxford University Press, 2000), 49-80.

Ludlow, Peter. 1999. *Semantics, Tense, and Time: An Essay in the Metaphysics of Natural Language* (Cambridge, Mass.: The MIT Press).

Mackie, J.L. 1977. *Ethics: Inventing Right and Wrong* (New York: Penguin Books).

Pianesi, Fabio and Achille Varzi. 2000. "Events and Event Talk: An Introduction," in James Higginbotham, Fabio Pianesi, and Achille C. Varzi (eds.), *Speaking of Events* (New York: Oxford University Press, 2000), 3-48.

Quine, W.V. 1953. *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press).

Stanley, Jason. 2005. "Semantics in Context," in Gerhard Preyer and Georg Peter (eds.), *Contextualism in Philosophy: Knowledge, Meaning, and Truth* (New York: Clarendon Press, 2005), 221-253.

Unger, Peter. 1975. *Ignorance* (New York: Clarendon Press).

Yablo, Steven. 1998. "Does Ontology Rest on a Mistake? I," *Aristotelian Society: Supplementary Volume* (no. 72, suppl.), 229-261.

Chapter 2

Deflating Representational Semantics

§1 The challenge. If the argument of Chapter 1 is right, the direct argument convicts representational and non-representational theories alike of metaphysical commitments. We can't, then, sever the link between semantics and metaphysics simply by adopting a non-representational meaning theory. Any attempt to disentangle questions of meaning from questions of metaphysics—and so to deny the widespread thesis—must confront the direct argument head-on. How, then, ought we deal with the direct argument?

§2 How not to deny the direct argument. Consider a slight variation on the Davidsonian instance of the direct argument in favor of the existence of events:[1]

(1) BRUTUS STABBED CAESAR WITH A KNIFE is true iff there is an event e, such that (i) e is a stabbing, (ii) Brutus is the agent of e, (iii) Caesar is the theme of e, and (iv) there is an x, such that x is a knife and x is the instrument of e.[2]

(2) BRUTUS STABBED CAESAR WITH A KNIFE is true.

(3) There are events.

(1) is a theorem of linguistic semantics; (2) is an ordinary judgment about the truth of a claim (and so, by proxy, an ordinary judgment about the way the world is). I've

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[1] See Chapter 1, p. 3.

[2] Throughout, I'm going to suppress issues to do with tense and slide between sentences that differ only in tense. Nothing in the current discussion hangs on this. The proper semantics of tense is controversial and raises its own metaphysical issues. For a discussion of both the semantics and the metaphysics, see Ludlow (1999).

suppressed the detour through the T-schema here for several reasons. First, instances of the T-schema are logical truths, and so are unlikely to be the weak links in instances of the direct argument. Second, the detour is inessential to the argument, since we are generally prepared to accede to ⌜S is true⌝ in any context in which we accede to S. Third, the semantic data are usually taken to be (judgments about) the truth-values of sentences in contexts, rather than the purported facts that the sentences report. (That is: the data *mention* the relevant sentences, they do not *use* them.)

§2.1   The premises are true.   Suppose we attack the soundness of the argument by denying (2). Of course, we might think that (2) is false because we think the historians have gotten it wrong. We might think that Brutus stabbed Caesar with a sword, not a knife; or that Cicero, instead of Brutus, stabbed Caesar; or perhaps Caesar wasn't stabbed at all, but died quietly in his sleep. None of these alternative hypotheses will rid us of the unwanted metaphysics, however; for each will support an instance of the direct argument with (3) as its conclusion.

To block the argument by denying (2), we must claim that (2) is false, not as a simple matter of historical fact, but because the truth-conditions of positive sentences whose semantics involve event quantification go systematically unmet. Neither Brutus, nor anybody else, killed Caesar in any manner; indeed, Caesar never died. (Nor, for that matter, was he ever born). Given the pervasive reliance on event quantification in contemporary semantics, if we wish to undermine the direct argument for events, we are forced to become wide-ranging antirealists: a very many ordinary and apparently obvious judgments about the world are mistaken, in a way that is not immediately corrigible. We must even deny that you are now reading this paper, and that I ever wrote

it. Clearly, then, the cost of denying (2) is too high. We might, of course, try to blunt the force of the looming anti-realism by adopting a non-representationalist semantics. As argued in Chapter I, however, such maneuvers only exchange one set of metaphysical costs for another, they do not eliminate them. Given that we must pay the metaphysical piper come what may, I think it is better to not saddle ourselves with the additional burden of claiming that so many of our ordinary judgments are false.

Suppose we instead target premise (1). As with (2), there are two possible motivations for denying (1). We might think that the semantic theory of which (1) is a part is the wrong theory of meaning, much in the way phlogiston theory is the wrong theory of combustion. If we take this line, however, we must both present an event-free semantics and show that it is better supported by the *linguistic* evidence than the standard semantics. But of course, event quantification pervades our (current) best semantic theories, the theories for which we have the best linguistic evidence.[3] Any attempt to evade the conclusion of the Davidsonian argument by denying that (1)—or something like it—is a part of the correct meaning theory, then, turns on overthrowing our best current science. This seems a rather Quixotic strategy. Furthermore, even were it successful, it would fail to disentangle metaphysics and semantics. For whatever successor theories replaced event semantics would also generate instances of the direct argument. We might, therefore, manage to rid ourselves of events, but we would still be saddled with some other metaphysics solely in virtue of our semantics.

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[3] And indeed, if our best theories didn't traffic in events, then the Davidsonian argument would hardly be problematic. For a smattering of the available event semantics, as well as discussions of the linguistic evidence, see Higginbotham, Pianesi, and Varzi (2000).

If we wish to undermine the direct argument schema—and not just this instance of it—by denying (1), we must claim that (1) is false—or, at least, we shouldn't believe it is true—*despite* being part of the "right" theory of meaning. That is: we must be *scientific anti-realists* about our semantic theories.[4] Scientific anti-realism is less implausible than anti-realism about common sense, but it still requires us to reject many beliefs that we seem loathe to discard: that water is H_2O; that atoms exist; that heat and pressure are manifestations of molecular motion; that bacteria and viruses can cause disease.[5] Certainly, such beliefs aren't as integrated into our daily lives as, say, the belief that the sun rose this morning.[6] We are, nonetheless, strongly attached to them, and so a general scientific anti-realism comes with significant cost.[7] Better to accept events and other creatures of semantics than to reject atoms, bacteria, etc.[8]

§2.2 The argument is valid. Instead of attacking the premises of the argument, we might contend that the argument is invalid due to an equivocation on the word 'true'. (1) is a claim of empirical linguistics, and 'true' as it is there used is a *theoretical* term: it is no

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[4] Gross (2006) appears to advocate a strategy of this sort.

[5] Exactly what we must reject, of course, depends on the variety of anti-realism.

[6] Though many of them are more central to our lives than beliefs about who killed Caesar.

[7] I tend to think that arguments for scientific anti-realism fall into the same trouble that Dogmatists diagnose for other skeptical and anti-realist arguments: the denials of their conclusions are much more plausible than their premises. We are thus inclined regard the arguments as a *modus tollens*, showing that the premises are false. See, *e.g.*, Moore (1944) and Lycan (2001).

[8] These worries aside, it isn't even clear that prominent contemporary forms of scientific anti-realism will relieve us of the most worrying metaphysical burdens that semantic theories threaten to impose. Van Fraassen's (1980) constructive empiricism denies that we have no warrant to believe the *unobservable* consequences of our theories; but it seems plausible that at least some events—if we're taking event ontology seriously—are observable. Cartwright's (1983) anti-realism admits that the theoretical entities posited by our theories are real, but insists that our theories make false claims about them. Neither van Fraassen's nor Cartwright's anti-realism will undermine the Davidsonian direct argument.

more a term of ordinary language than is 'force' as used in physics.[9] The intuitive

strength of (2), however, turns on the contained use of 'true' picking out our *common-sense* or *ordinary* notion of truth. For one thing, we simply don't have reliable intuitions

about theoretical notions. And so the fact that we judge that BRUTUS KILLED CAESAR

WITH A KNIFE is true couldn't straightforwardly be taken as evidence for (2) if the truth in

question were theoretical truth.[10] Furthermore, if the use of 'true' in (2) picked out

theoretical truth rather than common-sense truth, then denying (2) wouldn't mire us in

any sort of vicious anti-realism. What's worrying about anti-realism is that we're

systematically mistaken about a very many of our ordinary judgments. But we don't

generally make ordinary judgments about theoretical truth, and have little reason to

regard such judgments as we do make as reliable; thus, there is very little cost to denying

them.

The trouble with denying the validity of the direct argument in this way is that it

saps much of the explanatory power from our semantic theories. Semantic theories play

an important role in explaining our "semantic" judgments—judgments about whether or

not particular sentences are true or false in particular contexts, judgments about

entailment, etc. But these judgments concern the *ordinary* truth and falsity of sentences,

and so it is the possession of these properties by sentences that semantic theories must

explain. Take some sentence S and some context c, such that competent speakers judge

that S is true in c. Our judgment is to be explained by two factors: (i) our knowledge of

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[9] Indeed, in some theoretical frameworks this is made obvious by assigning sentences semantic values of 1 and 0 instead of true and false. The frameworks are inter-translatable, but the terminological difference makes clear that the semantic values are theoretical, rather than common-sense, properties of sentences.

[10] Compare: since the forces we are most familiar with are mechanical forces, we might have the intuition that forces can only act through contact; but, ever since Newton, physics assures us that that there are non-contact forces.

the meaning of S (in c), and (ii) our knowledge of what the world is like in context c. *I.e.*, our judgment that S is true in c is explained by our knowledge that that world is such-and-such a way and our knowledge that S is true when the world is this way. Since we're concerned with explaining ordinary truth here, theoretical truth must entail ordinary truth; otherwise knowledge of theoretical truth conditions wouldn't suffice for knowing that S is *ordinarily* true.

For the direct argument to go through, of course, ordinary truth must entail theoretical truth. But parallel reasoning establishes this entailment. Suppose we correctly judge that S is false in c. The judgment is explained by our knowledge that the world is such-and-such a way and our knowledge that S is false when the world is this way. For the explanation to work, theoretical falsity must entail ordinary falsity; but then ordinary truth entails theoretical truth.[11]

Thus, there is no compelling case against the validity of the direct argument, or against the truth of its premises. The direct argument is so compelling, I suggest, because it is sound.

§3 A way out. If the direct argument is sound, how can we avoid the metaphysical burdens imposed by its conclusion? The only strategy left is to *deflate* the conclusion by deflating the referential semantic theories that provide the key premise: we must explain the occurrence of apparently metaphysically committing features—*e.g.*, singular terms, quantification—in such a way as to rob them of their metaphysical significance.

Consider the conclusion of the Davidsonian instance of the direct argument:

(3) There are events.

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[11] We must assume bi-valence and excluded middle for both ordinary and theoretical truth. Denying either embroils us in a larger battle than that over the direct argument.

Davidson claims that this is a metaphysical discovery, that the proper analysis of adverbial discourse has shown us something new not just about language, but something about the non-linguistic world. But what, exactly, is this discovery supposed to be? It depends, I suggest, on how we interpret (3).

So far as I can see, there are two possibilities. We could, of course, follow Davidson in interpreting (3) as asserting the existence of a certain class of particulars—that is, as

(3') Events exist.

If the Davidsonian interpretation is the right one, then (3) tells us that in addition to tables and atoms and persons, the universe contains another sort of object, *events*. And, given the connection of (3) to the various theorems of the semantic theory, the theory also tells us that these objects are crucial participants of much of what goes on in the world. Brutus stabbing Caesar with a knife doesn't involve just Brutus, Caesar and the knife, it involves an event, too.

Now it might be objected that it's unfair to saddle the Davidsonian interpretation of (3) with the claim that the universe contains events *in addition to* everything else. After all, (3') makes no commitment as to the *nature* of events. Perhaps they are reducible to, or supervene upon, various ordinary objects (Brutus, Caesar, knives, etc.), or regions of space-time, or some other non-objectionable bit of our ontology. (3') doesn't necessarily commit us to events being something *over and above* everything else, in the same ontologically worrying sense that the dualist thinks the mental is something over and above the physical. All this I grant. But there is some sense in which the world contains tables in addition to atoms, even if tables reduce to or supervene upon

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collections of atoms. This is the sense in which the Davidsonian interpretation of (3) posits events in addition to everything else. But positing events, even in this sense, raises the question: *what is the metaphysical nature of events*? The various analyses of events in terms of less objectionable entities are attempts to answer this question. Events, as posited by (3), need not be ontologically funny; the crucial point is just that, ontologically funny or not, according to (3), they exist.

The alternative to the Davidsonian interpretation is to insist that all (3) says is

(3'') Stuff happens.

Viz., we don't live in the abjectly boring world that is completely uniform in time. This seems to be the most natural reading of (3), given the role that event quantification plays in the semantics. Consider what the intuitive gloss on the right hand side of (1) says:

(1') Something happened; it was a stabbing; Brutus did it; it was done to Caesar; and it was (done) with a knife.

The semantics correctly captures the entailment from

(1) Brutus stabbed Caesar with a knife

to

(4) Something happened

just because (1) entails (3)—that is, just because (1') entails (3'').

But, of course, (3'') is no metaphysical *discovery*! We didn't need a Davidsonian event semantics—or, indeed, any sort of semantics—to tell us that stuff happens. We knew that already, just by looking around. If (3'') is the right interpretation of (3), then, (3) doesn't tell us anything new about the non-linguistic world. (3'') is, therefore, a

deflationary interpretation. (3') on the other hand is *inflationary*. If (3') is the right interpretation, (3) not only tells that stuff happens, it tells us *what it is* for stuff to happen.

Note that (3'') being the right interpretation of (3) is *compatible* with the existence of events and a metaphysics that assigns events a crucial role in action, etc. All (3'') deprives us of is the *direct argument* for events. We may still decide that we need to posit the existence of events for our best metaphysical account of action, or change, or whatnot. But then we'll have discovered events via *metaphysics*, not semantics.

All I've done so far is suggest an alternative to the Davidsonian interpretation of (3) and argued that such an interpretation robs (3) of its metaphysical import. But is the deflationary interpretation possible? Can we explain the occurrence of quantification over events in our semantic theory in an ontologically neutral way? The rest of this chapter attempts to give such an explanation.

§4 **Deflating reference.** Remember that the target is not *just* Davidson's semantic argument for the existence of events. The ultimate aim is to show that *all* instances of the direct argument are problematic. And to do this, we need to explain away all of the supposedly committing features of our semantic theories. So, for example, Ludlow (1999, p. 71) speculates that the semantics of singular terms commits us to Pegasus. There are a number of ways to flesh out Ludlow's remarks into a full-blown argument that Pegasus exists.[12] One, of course, would be to detour through one or another apparently true sentence about Pegasus—e.g., PEGASUS IS A MYTHICAL BEAST. Ludlow,

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[12] Though Ludlow seems to want to distinguish between a commitment to Pegasus and a commitment to Pegasus existing—he claims that Pegasus is a non-existent object (1999, p. 71). But I don't understand what a commitment to Pegasus is, unless it's a commitment to Pegasus existing. To say that theory is committed to Pegasus is to say that the theory entails that Pegasus exists; to say that a speaker committed to Pegasus is to say that the speaker ought to believe that Pegasus exists. To call something a non-existent object is precisely to deny a commitment to it.

however, seems to take a short-cut: the commitment to Pegasus is not derived from the truth of various sentences, but from the lexical axiom for PEGASUS alone.

As best I can tell, Ludlow's argument runs as follows. He begins with the following lexical axiom:

(L-P) Val (x, PEGASUS) iff x = Pegasus

where 'Val (x, PEGASUS)' is to be read 'x is a semantic value of PEGASUS', and the variable is implicitly bound by a universal quantifier. (L-P), I take it, equivalent to

(L-P*) PEGASUS refers to Pegasus.

But even if they aren't equivalent, I'm willing to grant, for the sake of argument, that they carry the same ontological commitments. Now, PEGASUS is meaningful, so the implicit quantification in (L-P) cannot be vacuous.[13] Thus (L-P) and (L-P*) commit us to Pegasus. If Ludlow's line of argument is right, then any semantics with referential axioms for proper names will generate a host of unexpected commitments.[14]

It will help to have a toy semantic theory on hand, so consider the following Davidsonian theory for a simple subject-predicate fragment of English:[15]

Lexical Axioms

(L1) HARRY TRUMAN refers to Harry Truman.

(L2) HARRY POTTER refers to Harry Potter.

(L3) x satisfies LIVED ON PENNSYLVANIA AVENUE iff x lived on Pennsylvania Avenue.

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[13] See Ludlow (1999, pp. 220 – 221).

[14] Ludlow is not alone in the belief that existence and reference (or semantic value) are entangled; see, *e.g.*, Donnellan (1974) and Thomasson (2008). Frege's (1891) account of existence as a second-order concept is, perhaps, another view of this sort. For more, see below, pp. 47 – 48.

[15] Aside from various simplifications described below, the theory is along the lines of the Davidsonian theories presented by Larson and Segal (1995) and by Ludlow (1999).

(L4) x satisfies LIVED ON PRIVET DRIVE iff x lived on Privet Drive.

Compositional Axioms

(C1) [NP VP] is true iff there is an x such that (i) NP refers to x and (ii) x satisfies VP.

For ease of exposition, I've made a few simplifications here. As throughout the paper, I'm ignoring issues of tense. And I've suppressed the syntactic and semantic complexity of the predicates LIVED ON PENNSYLVANIA and LIVED ON PRIVET DRIVE. I've also assumed a simplified syntax for sentential phrases; this simplification carries through to the compositional axiom (C1). Nothing in what follows turns on these simplifications.

Using the axioms (L1) – (C1), we can derive truth-conditions for the various sentences of our fragment:

(T1) HARRY TRUMAN LIVED ON PENNSYLVANIA AVENUE is true iff Harry Truman lived on Pennsylvania Avenue.

(T2) HARRY POTTER LIVED ON PRIVET DRIVE is true iff Harry Potter lived on Privet Drive.

And so on. The trouble is that this axiom set also leads, *via* Ludlovian arguments and variations on the direct argument, to the metaphysical conclusion that Harry Potter exists. And none of the standard moves in reaction to this conclusion are palatable.

We could, of course, bite the bullet and simply accept that Harry Potter exists. And a good number of philosophers have argued that we ought to do just that: for contrary to what we might naively believe, fictional characters really *do* exist.[16] But

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[16] Van Inwagen (1977), though pursuing the Quinean project of regimenting natural language, rather than providing a semantic analysis for it, is motivated by a regimentalist analogue of the direct argument. Kripke (1973), Salmon (1998), and Thomasson (2003) argue for the existence of fictional characters on related grounds.

surely admitting fictional characters into our ontology should be a move of last resort. There is a strong and wide-spread intuition that Harry Potter doesn't exist; indeed, fictional characters seem to be *paradigm examples* of things that don't exist.[17] Russell's (1919) polemic against Meinong is still the most forceful articulation of this intuition:

> "There is only one world, the 'real' world: Shakespeare's imagination is part of it, and the thoughts that he had in writing Hamlet are real. So are the thoughts that we have in reading the play. But it is of the very essence of fiction that only the thoughts, feelings, etc., in Shakespeare and his readers are real, and that there is not, in addition to them, an objective Hamlet. When you have taken account of all the feelings roused by Napoleon in writers and readers of history, you have not touched the actual man; but in the case of Hamlet you have come to the end of him. If no one thought about Hamlet, there would be nothing left of him; if no one had thought about Napoleon, he would have soon seen to it that some one did." (pp. 170 – 171)

Common sense isn't sacrosanct, of course, and such intuitions can in some instances be overthrown by philosophical theorizing. But they must carry some evidential weight; if we can avoid massive revisions to our common-sense intuitions, we should.

Furthermore, even if we do eventually admit fictional characters into our ontology, it's unlikely that the direct argument should be the entry-point. Consider: according to the direct argument, the existence of Harry Potter can be derived straight-forwardly from the semantics of HARRY POTTER. Furthermore, this semantics is something that every competent user of HARRY POTTER tacitly *knows*—knowledge of (L2) is invoked to *explain* semantic competence. But then the strong and widespread intuition that Harry Potter does not exist conflicts with knowledge that forms the basis of semantic competence—semantic competence that is needed to even state the intuition. If the direct argument is right, then, expressions of non-existence are, if not outright

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[17] Indeed, it's not simply an unreflective intuition. In my experience, most people—at least non-philosophers over the age of six—make the *considered judgment* that Harry Potter doesn't exist.

contradictory, at least self-falsifying. How, then, can the intuition that Harry Potter doesn't exist be so strong and widespread? If such a belief really conflicted with our semantic knowledge, we might be expected to notice. Accordingly, admitting the existence of fictional characters on the basis of the direct argument, then, should be a move of last resort.

The other option is to deny that (L2) is the proper lexical axiom for HARRY POTTER and to provide another in its place. The most obvious way to provide a non-committing axiom would be to adopt a Russellian account on which natural language names are disguised definite descriptions. Indeed, one of Russell's motivations for analyzing ordinary names into definite descriptions was to avoid the trouble caused by empty names.[18] Quine (1948, p.12) also adopts such a strategy, regimenting natural language names into first order logic not as constants, but as predicates. The Russelian maneuver relieves us of the burden of Harry Potter; for, given that nothing satisfies the relevant description, the name fails to refer to anything. So (with the right semantics for EXISTS), we can preserve our intuition that

    (4) Harry Potter does not exist

is true. Unfortunately, without further complication of our semantics, we must now deny *other* apparently true sentences; *e.g.*:

    (5) Harry Potter lived on Privet Drive.[19]

    (6) Harry Potter is a fictional character.

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[18] See, *e.g.*, Russell (1919, pp. 179 – 180).

[19] The cost of denying the literal truth of (2) is, perhaps, low. Unlike judgments that (1), (3), (4), and (5) are true, the judgment that (2) is true is weak and changeable. I myself am tempted to a pretense account of statements that, like (2), seem to be true only "according to a fiction." See Walton (1983).

(7) Harry Potter was created by J.K. Rowling.

(8) Many eight year olds admire Harry Potter.[20]

If we want a straightforward response to the direct argument for Harry Potter, we need semantic axioms that paraphrase away reference to him, without upsetting (too many of) our ordinary judgments. But as Parsons (1979, p. 97) pointed out: "nobody knows how to produce the paraphrase. It hasn't been done. None of you know how to do it either." And this despite the fact that "some of the best minds have been trying for over fifty years now, without success" (p. 98). Things haven't changed, and by now it's been over eighty years.

It's also worth pointing out that not just any paraphrase—even any paraphrase that preserves the bulk of the relevant intuitions—will do. It has to be a paraphrase that can plausibly be attributed to ordinary speakers as a part of their semantic competence. So it ought not be overly complicated, or reliant on *recherché* entities or principles. Paraphrases along the lines of, for example, Field's (1980) nominalistic paraphrase of physics or Lewis and Lewis's (1970) physicalist paraphrase of discourse about holes won't do.

Note that all of these straightforward strategies take for granted that (L2) entails the existence of Harry Potter. According to strategy one, since HARRY POTTER refers to Harry Potter, Harry Potter must exist—since, if he didn't, HARRY POTTER wouldn't refer to anything. Strategy two runs a *modus tollens* to the *modus ponens* of strategy one: since Harry Potter doesn't exist, HARRY POTTER doesn't refer to anything. (L2) is the right *form* of axiom, but it's false. (Indeed *every* axiom of that form is false for HARRY

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[20] Of course, intensional transitives like ADMIRE raise issues—and require complications—of their own. See Montague (1974) and Thomason (1980).

POTTER.) Since Harry Potter doesn't refer to anything, many of the claims we are wont to make using HARRY POTTER are likewise false. The third strategy refuses the dilemma posed by (L2). If the lexical axiom for HARRY POTTER were to have the form of (L2), we would be forced to either admit that Harry Potter exists, or to deny various common-sense judgments. So, obviously, the correct lexical axiom does *not* have the form of (L2), but rather a form that traffics in the satisfaction of some open formula, rather than reference.

But since all of these strategies for dealing with the apparent metaphysical commitments of (L2) are unpalatable, I suggest we pursue a deflationary strategy that rejects the shared premise. (L2) is the correct semantic axiom for HARRY POTTER, I contend, but it does not demand that Harry Potter exists.

Call an instance of reference *Fregean* if and only if it is a relation between a lexical item and an existing object to which the lexical item refers.[21]   (L1) uncontroversially presents a case of Fregean reference: HARRY TRUMAN refers to an existing object, Harry Truman.   Proponents of the straightforward strategies explored above take (L1) to be exemplary; they, along with most other philosophers, subscribe to *the reference dogma*: all reference is Fregean.[22] And no wonder: the foundational systematic accounts of reference (Frege 1892, Russell 1905), as well as later competitors (*e.g.*, Kripke 1980), all have the reference dogma built in. On these accounts and their descendents, the relation between reference and existence is "platitudinous" (Thomasson 2008, p. 66), perhaps even analytic.

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[21] This leaves open just what *determines* reference—both description theorists and externalists can think that reference is Fregean.

[22] For a denial of the reference dogma, see Parsons (1979).

Nothing, however, forces us to adopt this constraint on the reference relation. There's no guarantee that the semantic property that our best semantic theories traffic in is Fregean reference. 'Refers' as it appears in our semantic axioms is a term of *empirical linguistics*. And we don't generally just stipulate the meanings and extensions of our theoretical terms—they get defined contextually and are open to re-interpretation on the basis of empirical investigation.[23] It is thus an empirical matter of theory interpretation whether or not 'refers' picks out Fregean reference in (L1) and (L2).

Accordingly, I suggest we reject the reference dogma, on the grounds that a non-Fregean account of reference allows us to adopt a simple, intuitive semantics of singular terms without untoward metaphysical consequences. Rejecting the dogma leads us to the following view: reference is *not* invariably a relation between a term and an existing entity to which the term refers. Reference isn't monolithic—there's no single, metaphysically uniform relation that holds between meaningful or successful terms and (bits of) the world. Rather, terms relate to the world in various ways, and there are myriad reference properties, with diverse metaphysical natures.

Now one natural objection to such a view is that it vitiates our semantics. For reference is supposed to provide the basis for the recursive computation of meaning (or semantic value, or truth value, or what have you). And for it to play this role, there has to be a single relation; otherwise our compositional axioms—*e.g.*, (C1)—won't be fully general.[24]

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[23] *Cf.* the history of the theoretical term 'acid' in chemistry. See also Chapter 3, below.

[24] Rich Thomason raised this objection in conversation.

This objection shows that we must be careful about how we describe the non-Fregean position. There is, we must maintain, a single reference relation; it's the relation defined by all true instances of the schema

(RS) τ refers to o.[25]

And this is *all there is* in common between instances of the reference relation. Instances of reference (may) have different metaphysical bases, but they are united by their satisfaction of the schema (RS).[26] One way of looking at this is to take reference to be a single relation, but a disjunctive one. Or we could take reference to be a real relation, just not a *natural* one—reference doesn't cleave the world at its inherent joints.[27]

(And really, why *should* we expect reference to be a natural relation? The claim that reference is a natural relation looks equivalent to the claim that our *language faculty* carves nature at the joints (or at least attempts to), since reference is the coin of semantics. But however much we may try to construct joint-carving theories in the pursuit of science, it seems unlikely that our language faculty—constrained as it is by our neurobiology and shaped as it is by evolution—is *by its very nature* joint-carving.)

So, in *some* cases, reference is a relation between a singular term and an existing entity to which the term refers. But in other cases, it is not. What, then, are these non-Fregean reference properties? What is it to refer to Harry Potter? Let's begin with a

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[25] Truth may not be enough. We may have to restrict it to all disquotational instances of (RS), or to all interpretive instances. On the latter, see Larson and Segal (1995, pp. 32 – 34 and 38 – 42).

[26] This treatment of reference is exactly parallel to some deflationary treatments truth. *Cf.* Horwich (1998).

[27] The relevant idea of naturalness appears in Lewis (1983) and has lately been developed by Sider (2002 and 2009).

broadly causal-historical account of reference, of the sort initially developed by Kripke (1980) and Putnam (1975).[28]

According to the causal-historical account, HARRY TRUMAN refers to Harry Truman because the term is causally connected in the right sort of way to Harry Truman.[29] In 1884, a baby was born to John and Ellen Truman, and they (we can imagine) said to one another "Let's call him Harry," thereby affixing the reference of HARRY TRUMAN to just this child. They then conveyed this name to others—"Meet our son, Harry"—who intended to use it with same reference, and who passed the name on to yet others. And so the name—along with its reference established by that initial dubbing—made its way throughout the linguistic community. A term, then, refers to Harry Truman, just in case it stands at one end of a causal-historical chain of this sort, the other end of which is grounded in Harry Truman himself.[30] Indeed, this is just what the property of referring to Harry Truman *is*.

If we try to tell a parallel story for HARRY POTTER, however, we run into trouble: Harry Potter doesn't exist, and so isn't available to anchor the causal chain. As Donnellan (1974) puts it, in tracing back the causal-historical chain in search of the referent for a fictional name, we run into a "block": the chain terminates, not in (a

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[28] Nothing hangs on this—a description theory of non-Fregean reference is possible, as well, though it would face all the problems confronting description theories generally. I here develop an externalist account of non-Fregean reference because it more clearly illustrates the difference between referring to Harry Potter and referring to Harry Truman, and because I have externalist sympathies.

[29] Although all versions of the causal theory will agree that Harry Truman himself anchors the causal chain, we have various choices for the second relatum: uses of HARRY TRUMAN, its entry in the mental lexicon of a speaker, etc.

[30] This is only the barest sketch of the causal account of reference, of course, and much work needs to be done to fill in exactly what sorts of causal chains result in transmission of a term (along with its reference), what is required for successful dubbing, and so on. There are various attempts to develop the causal account—see, e.g., Devitt (1981) and Soames (2002)—and nothing here hinges on exactly how the details are spelled out.

dubbing of) the object to which the name refers, but in an author making the name up in the course of writing a fiction! Advocates of the causal account of reference have generally displayed one of two reactions to such defective chains: claim that names at the end of such chains don't refer (Donnellan 1974, Kripke 1980), or claim that fictional characters in fact exist and so *can* anchor reference (Kripke 1973).[31] And, of course, if we hold onto the reference dogma, these are our only options.

Abandoning the reference dogma provides us a third option. The property of referring to Harry Potter just is the property of standing at the end of a causal-historical chain (of the right sort) that is anchored in J.K. Rowling making up the name HARRY POTTER.[32] And so a term can refer to Harry Potter even though Harry Potter doesn't exist—because referring to Harry Potter doesn't involve Harry Potter himself. Blocks thus do not, as Donnellan (1974) would have it, prevent terms from referring. Rather, they provide reference anchors when ordinary reference anchors—*existing* objects to which terms refer—are unavailable.

Now we might worry about what *exactly* counts as the reference-fixing—and so what counts as the anchor—in this case. Is the reference of HARRY POTTER fixed when Rowling first thinks of the name? When she first writes it down? When the first Harry Potter novel is substantially complete? What if Rowling had initially written a novel about a detective named Harry Potter, before burning the draft and appropriating the name for a new novel about a wizard? These are important issues, and in any full account they must be worked out. But they are *general* questions about reference-fixing

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[31] Though, of course, reference-fixing to fictional characters can't occur via ostension in the normal way.

[32] Assuming Rowling really is the author of the Harry Potter books. If, instead, Rowling provides a real-life instance of a Gödel-Schmidt case, the chain will be anchored in the creative acts of the real author.

and don't pose any special problem for names with non-Fregean reference. Consider: was the reference of HARRY TRUMAN fixed when one of Truman's parents first thought of it? When they first voiced the name? What if they had originally tried the name out on an earlier child? Whatever resources are brought to bear by the orthodox causal theorist can be adapted for non-Fregean cases as well.[33]

On a Fregean view of reference, terms always refer to their reference anchors. The innovation of the non-Fregean account of reference is to allow reference and reference anchor to come apart: some terms *do not* refer to their reference anchors. For only existing things can anchor reference, but some terms—HARRY POTTER, for one—refer to things that don't exist. So, HARRY TRUMAN refers to Harry Truman, and HARRY POTTER refers to Harry Potter; but these two reference facts are underwritten by very different metaphysical states of affairs. Nonetheless, these metaphysically diverse reference properties both serve to ground the recursive computation of meaning/truth-conditions. The compositional semantic module is "blind" to the metaphysical difference.

With this account of reference in hand, we can accept the semantics given by (L1) – (C1), without having to either admit that the inclusion of (L2) commits us to the existence of Harry Potter or reject our ordinary judgments about (4) – (8). For we can block the direct argument, as well as Ludlow's variation on it, by pointing out that reference does not, in general, require the existence of a thing referred to. And indeed,

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[33] See Devitt (1981) for some reflections on the complications of reference-fixing.

HARRY POTTER provides just such an instance of reference without a referent.[34] The combination of referential lexical axioms for singular terms and true positive statements using those terms does not commit us to any particular ontology of referents.

It might be objected that we can't get off so easily: for (L2) *uses* HARRY POTTER, and so no meta-theoretical account of the reference relation can rescue (L2) from a commitment to Harry Potter. But the objection is clearly question-begging. For if the non-Fregean account of reference is right, then the occurrence of HARRY POTTER on the right-hand side of (L2) itself can refer, even in the absence of an existing Harry Potter. Indeed, it's the very fact that HARRY POTTER is used in (L2) *with whatever reference property it actually has*, that divests (L2) of a commitment to Harry Potter. If HARRY POTTER can contribute to the generation of truth conditions for object language sentences, even if Harry Potter doesn't exist, it can just as well contribute to the truth conditions of (L2).

Note that we've begun to separate linguistic structure from metaphysical structure, for the syntactic and semantic form of (L2) does not reflect the metaphysical structure of the fact (L2) reports.[35] (L2), of course, is a sentence in the meta-language. But because of the role (L2) plays in the recursive computation of truth conditions for the object language, this has the effect of decoupling the linguistic form of object language sentences involving HARRY POTTER from metaphysical structure as well.

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[34] There is another available argument for the existence of Harry Potter, one that proceeds *via* existential generalization. The non-Fregean account of reference does not, by itself, disarm this argument. I deal with the issues presented by quantification directly below.

[35] I don't mean to rest any metaphysical weight on "fact" here.

§5 Deflating quantification. But the appearance of singular terms is not the only feature of our semantic theories that is sometimes taken to generate metaphysical commitments. To complete our deflationary account of semantics—and to undercut the direct argument for events—we must deal with quantification.

Quantification enters our semantic theory in four ways. First: since the lexical axioms for singular terms themselves use singular terms, they support existential generalization. So we can infer from (L2),

(9) HARRY POTTER refers to something.[36]

Second: lexical axioms for predicates, such as

(L4) x satisfies LIVED ON PRIVET DRIVE iff x lived on Privet Drive

take the form of universally quantified statements and lay down conditions under which an object satisfies the predicate. Third: the compositional axioms, such as

(C1) [NP VP] is true iff there is an x such that (i) NP refers to x and (ii) x satisfies
      VP.

involve existential quantification over those very things to which the singular terms of the object language refer, and by which the predicates of the language are satisfied.

These first three sources of quantification threaten to re-introduce the unwanted metaphysical commitments we hoped to eliminate by giving a non-Fregean account of reference. For HARRY POTTER refers to something; what is this thing? Why, Harry Potter, of course! And *this very object* to which HARRY POTTER refers—Harry Potter—

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[36] Or, if our meta-language has first-order quantifiers instead of natural language quantifiers,
    (9*) (∃x) HARRY POTTER refers to x.
Most contemporary metaphysical debates take (9) and (9*) to be, if not completely synonymous, at least equivalent in point of metaphysical baggage, and I'll follow them.

must satisfy LIVED ON PRIVET DRIVE if (5) is to come out true.[37] So even if the semantic theory doesn't commit to Harry Potter by using his name, we might argue, in the spirit of Quine (1948), that the theory nonetheless commits to Harry Potter by quantifying over him.

Satisfaction conditions provide the fourth entry-point for quantification, for some satisfaction conditions are explicitly quantificational. Take, for example, a Davidsonian lexical entry for STABBED:

(L5) For all x, x satisfies STABBED if and only if there is an event e such that (i) e is

stabbing, and x is the agent of e.[38]

According to (L5) part of what it is for an object to satisfy stabbed is for *there to be* an event. And this, of course, is what gets the Davidsonian direct argument off the ground.

One way to evade the apparent metaphysical consequences of (9), (L4), (C1), and (L5) is to offer an interpretation of the quantifiers of our theory according to which the offending entities—fictional characters, events, or whatnot—aren't in their domain, either by giving an alternate domain, or a non-standard semantics for the quantifiers. But the most straightforward ways of doing this are unsatisfactory.

We deflated our referential lexical axioms by offering an account of how a term could have a reference property, even in the absence of some existing entity to which the term refers. Given that, even in cases where we lack (existing) referents, we still have reference properties, we might attempt to deflate the quantifiers by letting them range over reference properties instead of the objects referred to. The trouble with this strategy

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[37] If you don't think that HARRY POTTER LIVED ON PRIVET DRIVE is true, substitute HARRY POTTER IS FICTIONAL and make appropriate emendations to semantic axioms.

[38] Continuing the usual simplifications and adopting the further expedient of suppressing the patient thematic role.

is that it conflicts with the content of the semantic axioms. Consider the role of the compositional axiom (C1) in determining the truth conditions of HARRY TRUMAN LIVED ON PENNSYLVANIA AVENUE. It requires that something in the range of the quantifier be referred to by HARRY TRUMAN and satisfy the predicate LIVED ON PENNSYLVANIA AVENUE. And to do the latter, according to (L3), that thing must live on Pennsylvania Avenue. But HARRY TRUMAN doesn't refer to the property of referring to Harry Truman, nor does the property of referring to Harry Truman live on Pennsylvania Avenue. If we simply let the quantifiers of the semantic axioms range over reference properties, then, we will systematically miscalculate the truth-values of our sentences.

Instead of searching for an alternative domain, we might try to take advantage of one of the non-standard semantics for the quantifiers developed, at least in part, to deal with the thorny problem of empty names. Suppose we interpret the quantifiers of our semantic theory as *substitutional* quantifiers, so that they are governed by the following semantics:

(Sub-∃) $\ulcorner(\exists\chi)A(\chi)\urcorner$ is true if and only if some substitution instance $A(\alpha/\chi)$ is true.

(Sub-∀) $\ulcorner(\forall\chi)A(\chi)\urcorner$ is true if and only if some every substitution instance $A(\alpha/\chi)$ is true.[39]

$A(\chi)$ indicates that variable χ is free in A, and $A(\alpha/\chi)$ is the sentence that results from replacing every free occurrence of χ with name α. (Sub-∃) and (Sub-∀) ground the truth of quantified sentences in the truth of certain non-quantified sentences, rather than in a domain of objects. We thus avoid a commitment to Harry Potter and other dubious

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[39] (Sub-∃) and (Sub-∀) are proposed as semantic clauses for the quantifiers of the semantic theory given in (L1) – (C1), not as semantic clauses governing the object language of which (L1) – (C1) treats. Thus, they are stated in the *meta*-meta-language, rather than in the meta-language, and they are not part of the theory (L1) – (C1) itself.

entities.[40] The substitutional strategy breaks down, however, because we don't have enough names. If we're to get the right results—the right truth-values for our object language sentences—the meta-language must have a name for every purported object. But there are un-named fictional characters (and ones that can't even be picked out by definite description!); events don't have names; etc. Furthermore, there are predicates we reasonably think are satisfied—and so have true existential closures—even though we can't refer to any particular object that satisfies them. (Take, for example, EXISTS BUT WE DON'T KNOW THAT IT EXISTS, or IS AN OBJECT OUTSIDE OUR LIGHT CONE.) So the quantifiers of the semantic theory cannot be substitutional.[41]

Nor can we interpret the quantifiers as free-logic quantifiers. Free logic allows for empty names and restricts existential generalization and universal instantiation to those that denote:

(FL-$\exists$-in) $A(\alpha/\chi)$, $\text{Exists}(\alpha) \vdash (\exists\chi)A(\chi)$

(FL-$\forall$-out) $(\forall\chi)A(\chi) \vdash (\text{Exists}(\alpha) \rightarrow A(\alpha/\chi))$

We can thus have the name 'Harry Potter' in our semantic theory without generating the problematic

(10) $(\exists x) x = \text{Harry Potter}$,

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[40] Ludlow (1982) points out that the substitutionalist is committed to a rich ontology of syntactic types. It's not clear that this is a problem in the present case, as our linguistic theory presumably already carries such commitments.

[41] Note that this is not a *general* problem about substitutional quantification. Kripke (1976) demonstrates that substitutional quantification is perfectly coherent, doesn't reduce to objectual quantification, and can be used in Davidsonian T-theories. But he also makes the point made here: substitutional and objectual quantification diverge in their truth-assignments when we have more (purported) objects than names. And the problem can't be solved by allowing a (countably infinite) substitution class of syntactically complex, constructible terms—e.g., definite descriptions—since the substitution class must be determined in advance.

which is generally taken as equivalent to

(10') Exists(Harry Potter).

And so the free logic strategy bars one of the entry points for quantification: generalization from lexical axioms. Quantifier clauses in the axioms themselves are left untouched, though such quantification might be thought metaphysically unproblematic given the inferential restrictions.

But as with the previous strategies involving re-interpreting the quantifiers, the free logic strategy can't deliver the right truth-values for object language sentences. Because of the inferential restrictions, that Harry Potter doesn't exist entails

(11) ~(∃x) HARRY POTTER refers to x,

despite the fact that (L2) is an axiom. By (C1), a subject predicate/sentence is true only if there's something to which the subject phrase refers. So no (positive) sentence of the form ⌈HARRY POTTER pred⌉—including HARRY POTTER LIVES ON PRIVET DRIVE, HARRY POTTER IS A FICTIONAL CHARACTER, *etc.*—can be true.

What do these attempts at analyzing the quantifiers of our semantic theory reveal? The quantifiers must range over the type of thing that satisfies the predicates of the object language—and so the type of thing that lives, stabs, is fictional, etc.; the quantifiers must allow for unnamed objects; and the singular terms of the meta-language—at least those used in reference clauses—must uniformly support existential generalization. And there is, of course, a semantics for the quantifiers that gives us all of this: the standard objectual interpretation. I suggest, therefore, that the meta-language quantifiers of our semantic theory are objectual quantifiers. And since objectual quantification is generally

taken to carry ontological commitment, we must offer a deflationary explanation of such quantification—just as we deflated reference.

Consider the semantic clauses for objectual quantification:

(Obj-∃) ⌜(∃χ)A(χ)⌝ is true if and only if some value of χ satisfies $A(χ)$;

(Obj-∀) ⌜(∀χ)A(χ)⌝ is true if and only if every value of χ satisfies $A(χ)$.

If we allow our variables to take only existing objects as values, then our meta-language quantification will, indeed, generate commitments to various dubious entities. But just as we explained how a lexical item might refer to a non-existent object, we can explain how a variable might take a non-existent object as a value.

To refer to Harry Potter is to bear a sort of causal-historical relationship to a particular object,[42] the reference anchor of HARRY POTTER. But this object *isn't Harry Potter*. Concomitantly, to take Harry Potter as a value is to bear a (logical?) relation to a particular object—but likewise an object that isn't Harry Potter. How might this work? It would be nice if we could take valuation just to be reference—a variable takes an object as its value by referring to it.[43] But our deflationary account of reference precludes this for two reasons. First, we've explained reference as a sort of causal/historical relation, and variables simply don't bear such a relation to Harry Truman, Harry Potter (or the reference anchor for HARRY POTTER), or anything else. Second, if we identified valuation and reference, our variables couldn't take as values things we can't refer to, since there are no reference properties grounded in these things.[44]

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[42] Or collection of objects, or event, or what have you.

[43] And such a strategy might be encouraged by various formulations of Quine's criterion in which he writes of variables "referring" to objects. See, *e.g.*, Quine (1948, pp. 13 – 14).

[44] The second problem is, perhaps, only a problem if we think there are no un-instantiated properties.

In fact, these problems don't go away even if we adopt a non-deflationary account of reference, for the trouble isn't with the reference anchors, but with the mechanism of reference. An orthodox causal theory, according to which the reference of a term is invariably grounded in the (existing) object to which it refers, still takes reference to be a causal/historical relation, a sort of relation that variables simply don't bear to objects.

Description theories of reference have it even worse. For one, variables quite clearly don't have descriptive content; if they did, they couldn't range over *all* objects—this is enough to distinguish valuation and reference. But suppose they did: perhaps variables function by taking on different descriptive contents on different occasions, and have objects as values by virtue of those objects satisfying the given descriptions. But on standard analyses, definite descriptions themselves contain variables. So suppose we tried to account for the reference of variable x in terms of definite description *The y such that F(y)*. We're then faced with providing the descriptive content of y, which introduces yet another variable; and so on. A descriptive theory of variable reference thus faces a regress.

The lesson, I take it, is that valuation is not reference—variables do not refer to their values. (At least not on any substantive understanding of reference.) What, then, *is* valuation? I suggest that valuation is what is left when we abstract away from (i) the specifics of reference determination, and (ii) the particular terms doing the referring. What's left is simply a "bare" association between a place-holder for (possible) terms—a variable—and the things that are candidates to be referred to—the things taken as values. On standard, Fregean, accounts of reference, only existing things can be referred to; so valuation is a bare association between a variable and an existing object. And in

abstracting away from both our actual linguistic resources and the mechanisms of reference determination, we ensure that variables can range over *all* the existing objects—even those we don't have terms for, or those that we're epistemically isolated from.

To deflate valuation, we don't need to change the basic analysis of valuation as bare association between a variable and an object; we need only to change the candidate values. In the deflationary account of reference given above, terms can refer to non-existent objects; they do so in virtue of having their reference anchored in some other *existing* object. So we can still treat valuation is still a bare association between a variable and an object—but sometimes it's an association with a non-existent object. And parallel to the explanation of reference to non-existent objects, we can explain association with a non-existent object in terms of association[45] with some other *existing* object. Which existing object? The one that *would* act as a reference anchor for a term referring to the non-existent object.[46]

Thus: to have Harry Potter as a value is to be associated with Harry Potter, which just *is* to be associated with the reference anchor of HARRY POTTER. Note that, at this level of description, having Harry Potter as a value is perfectly isomorphic to having Harry Truman as a value: to have Harry Truman as a value is to be associated with Harry Truman, which just is to be associated with the reference anchor of HARRY TRUMAN. The difference is simply that Harry Truman is the reference anchor of HARRY TRUMAN, but Harry Potter—since he doesn't exist—isn't the reference anchor of HARRY POTTER.

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[45] A slightly different, not-quite-so-bare association. See below.

[46] Were there such a term, and were any barriers to reference—such as epistemic isolation—overcome.

There's one serious objection that must be dealt with: variables must be able to take reference anchors themselves as values, since we can say things like "There are reference anchors," and "Every reference anchor exists." This is a problem when we try to explain the property of having Harry Potter as a value, since we explained *this* property as an association with the reference anchor of HARRY POTTER. If having the reference anchor of HARRY POTTER as a value is also an association with the reference anchor of HARRY POTTER, it looks like the two valuation properties must be identical. And so there's no difference between having Harry Potter as a value and having the reference anchor of HARRY POTTER as a value. But then Harry Potter is *identical* to the reference anchor of HARRY POTTER—a result that the non-Fregean account of reference explicitly denies.

To begin with, we must allow objects to anchor the reference of more than one term. So far this is no departure from the Fregean account of reference, since co-referring terms—*e.g.,* CICERO and TULLY—must on any account have their reference anchored in the same object (though the causal chains linking the terms to the object will differ). What we further need is for some objects to anchor the reference of multiple *non-*co-referring terms. Suppose, for example, that the reference of HARRY POTTER is anchored in a particular sentence token in one of J.K. Rowling's notebooks. Let's call that sentence token *the HP sentence token*.[47] Now (by hypothesis) the HP sentence token anchors the reference both of HARRY POTTER and of THE HP SENTENCE TOKEN. How can it do this, since Harry Potter and the HP sentence token aren't identical?

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[47] THE HP SENTENCE TOKEN is a Millian name, not a so-called "descriptive" name—that is, it rigidly designates whichever sentence token grounds the reference of HARRY POTTER in *this* (albeit hypothetical) world. On descriptive names, see Evans (1982, pp. 46 – 50).

The problem is, I take it, akin to the *qua* problem facing causal theories of reference generally.[48] In any given attempt at reference-fixing, we are faced with the question of just *what* we're fixing reference to: an individual object? *Which* object? (Goliath or Lumpl?) A kind? *Which* kind? (Zebras, animals, striped things?) Reference-fixing, then, cannot proceed purely ostensively, or solely on the basis of causal interaction. Any act of reference-fixing must also include some descriptive information to single out which potential referent is being dubbed.[49]

Just as dubbing must involve more than just the thing dubbed, so reference anchoring must involve more than just the reference anchor: it must involve the property in virtue of which the object is *eligible* to ground the reference of the given term. Any existing object is eligible to ground the reference of a term referring to that object. So, for example, Harry Truman exists, and so can ground the reference of terms referring to him—e.g., HARRY TRUMAN. Likewise, the HP sentence, in virtue of its existence, grounds the reference of THE HP SENTENCE. But the HP sentence has another property, a property in virtue of which it grounds the reference of HARRY POTTER: the HP sentence is the "block" for HARRY POTTER. So, though the HP sentence anchors the reference both of THE HP SENTENCE and of HARRY POTTER, it does so in virtue of different properties.[50]

We now have the resources to solve our problem about valuation. Valuation is a bare association between a variable and an object, where *this* association is to be explained in terms of another association, an association between the variable and a,

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[48] See, *e.g.*, Devitt (1981), Devitt and Sterelny (1987), and Stanford and Kitcher (2000).

[49] Though some or all of this information may be provided by context, rather than given explicitly. In some cases, the descriptive information may also impose necessary conditions for successful reference-fixing.

[50] Don't confuse the property in virtue of which an object anchors reference with the property used to pick the object out in an act of reference fixing. TULLY and CICERO were likely affixed to Cicero under different descriptions, but Cicero anchors the reference of both by virtue of his existence.

perhaps distinct, object *qua* potential reference anchor. If we wanted to spell this out formally, we might model objects *qua* potential reference anchors as ordered pairs of objects and properties in virtue of which they are eligible to anchor reference. Then: to take Harry Truman as a value is to be barely associated with Harry Truman, which is to be associated with <Harry Truman, existing>. Likewise: to take the HP sentence as a value is to be barely associated with the HP sentence, which is to be associated with <The HP sentence, existing>. And to take Harry Potter as a value is to be barely associated with Harry Potter, which is to be associated with <The HP sentence, being the block for HARRY POTTER>.[51]

The following general picture of reference and quantification emerges. Begin with everything that exists. In virtue of existing, each of these things is eligible to anchor the reference of a term referring to it.[52] Some of these things also have other properties—properties that make them potential "blocks" of Fregean reference—in virtue of which they are eligible to anchor the reference of other terms, terms that don't refer to the things themselves, but instead refer to things that don't exist. The quantifiers of our semantic theory range equally over things that exist and things that don't. Quantification is, nonetheless, ordinary objectual quantification, spelled out in terms of valuation. To take an object as a value is to be associated with whatever potentially anchors reference to that object, *qua* potential reference anchor.

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[51] It's important to think of this device only as a *model*: the deflationary position shouldn't be committed to sets, properties, and other metaphysically controversial entities. One way to handle this—indeed, I think, the right way—is simply to interpret singular terms for and quantification over such entities in the deflationary manner set out in this chapter. That is: the deflationary account is intended to be perfectly general, applying to the explanation of reference and valuation no less than to any other bit of language.

[52] Though some of them don't actually anchor reference. What is eligible to anchor Fregean reference is just a matter of what exists; what actually anchors Fregean reference is a matter both of what exists and what our terms are causally related to.

§6 Events Again. Let's return, at last, to the Davidsonian direct argument for events. Consider again the conclusion of the argument:

(3) There are events.

According to Davidson, (3) reveals not just something commonsensical about the world—that things happen—nor just something about the deductive structure of our language—that verbs have implicit quantificational structure. According to Davidson, (3) also reveals something about the deep metaphysical structure of our world, something about *what it is* for things to happen. Things happen whenever (and wherever) a certain sort of individual—an event—exists. So, for example, Brutus stabbing Caesar with a knife involves more individuals than Brutus, Caesar, and the knife; it also involves the individual that's the stabbing.[53] And this opens up all sorts of questions about the *nature* of this other individual: Is it a mereological sum of Brutus, Caesar, and the knife? Is it a region of space-time? Is this event identical to the murder—that is, the event that is a murder—involved in Brutus murdering Caesar?

Nothing I've argued above rules out the Davidsonian view;
but it does show that the Davidsonian view cannot be established solely on the basis that our best semantic theory entails (3). Davidson also needs to offer substantive, independent arguments for his metaphysically heavy interpretation of event quantification, since (3) itself is compatible with a deflationary interpretation.

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[53] Here's another way to see the contrast between the Davidsonian and non-Davidsonian positions. According the non-Davidsonian, (3) is about a ternary relation between Caesar, Brutus, and a knife; stabbing is a three-place relation. According to the Davidsonian, (3) is about a *quaternary* relation between Caesar, Brutus, a knife, and an event; stabbing is a four-place relation.

Consider how we interpret (3) in light of the account of quantification in our semantic theories offered above. Quantification is objectual, so (3) is true just in case some value of the variable—something in the domain—is an event.[54] But variables can take non-existent things as values; all that's needed is the right sort of (potential) reference anchor. So, the question is: what grounds variables taking events as values? Is event quantification like quantification over ordinary physical objects (such as Harry Truman), or is it like quantification over fictional characters (such as Harry Potter)?

The Davidsonian account of event valuation is metaphysically heavy: the valuation is grounded in the event itself, in virtue of the event's existence. What's the lightweight alternative? That the valuation is just grounded in Brutus stabbing Caesar with a knife, where this involves nothing but Brutus, Caesar, and the knife. To put it another way—in a turn of phrase common in debates between ontological realists and anti-realists—the valuation isn't grounded in the event of Brutus stabbing Caesar with a knife (since events don't exist, they can't ground valuation); rather, it's grounded in Brutus, Caesar and the knife, in virtue of their being arranged stabbing-wise.

But nothing in (3) itself, nor anything in the compositional semantic theory, determines which of these is the right account of event quantification. So if the Davidsonian wants to argue for the existence of events on the basis of (3), he needs to offer some *additional* argument in favor of the heavy-duty interpretation of event quantification. This argument can't simply be that events must exist to play the explanatory role they do in the semantic theory, since, as I've argued, the semantic theory itself makes no such demand. The Davidsonian argument must, then, proceed in the

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[54] If 'e' is a dedicated event variable, with its range implicitly restricted to events, this amounts to being able to value the variable at all.

following way: we ought to prefer the heavy-duty interpretation because we have ample reason, independent of our semantic theory, for thinking that events exist. And there may well be such reasons: perhaps we must take events to exist because events are the fundamental causal relata, and they can only be the fundamental causal relata if they exist.

Indeed, which account we ought to adopt seems to depend upon whether or not we have good reasons—*apart from our semantic theory*—for thinking that events exist. Consider why the lightweight interpretation of reference to Harry Potter is so attractive: we take ourselves to have very strong evidence that Harry Potter doesn't exist. Why, then, is the lightweight interpretation of reference to Harry Truman *un*attractive? Because we have very strong evidence that Harry Truman *does* exist. What account of a particular reference (or valuation) property we're motivated to accept, then, depends upon whether or not we take the referents (and values) exist. So: we must decide the metaphysical issues *before* we can give an account of the reference/valuation properties.

§7 Does deflation make metaphysics impossible? There's one last objection to be dealt with, an objection, not to the details of the account offered above, but to the very idea of a metaphysically deflationary approach to semantics. The objection is this: the deflationary interpretation of semantics undermines the very project of metaphysics, the philosophical project of figuring out what the world is like at some fundamental level. Now some philosophers might welcome this conclusion—so much the worse for metaphysics![55] But I take it to be a serious objection: I think we often do try to give theories about what the world is like—at all sorts of levels, some more fundamental,

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[55] See, *e.g.*, Wittgenstein (1953, *passim*), Rorty (1979, especially Ch. VIII), and Putnam (2005, Lecture 4).

some less—and I think we sometimes non-accidentally succeed in giving correct ones.[56]

Moreover, one of the *motivations* for adopting a deflationary interpretation of semantics is that, un-deflated, such theories often conflict with deeply held and well-supported metaphysical beliefs. We sought to deflate reference because *Harry Potter doesn't exist*. But if deflation shows metaphysics to be nonsense, then we lose the motivation for deflating in the first place; the deflationary project isn't incoherent, but it is self-undermining.

The worry that deflation undermines metaphysics comes in two forms, one semantic and the other epistemic. The semantic worry: if (3) does not commit to the existence of events, what could? To echo the complaint of Quine's McX, "Does *nothing* we say commit us to the assumption of … entities which we may find unwelcome?" (1948, p. 12).[57] Claims about what exists and what does not are central to metaphysics; if we cannot make them—literally and unequivocally—then metaphysics is indeed nonsense.

The answer, of course, is that we commit ourselves to the existence of events, not with (3), but with

(3') Events exist.

There is a perfectly commonsense, natural-language distinction between quantification and predications of existence, as evidenced by the willingness of all but philosophers (and some five-year-olds) to deny that Harry Potter exists, all the while quantifying over

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[56] Some substantive metaphysical claims I think are contentful and true: fictional characters don't exist, and neither do numbers or (merely) possible worlds; but atoms do. Objects are wholly present at every moment of their existence; they don't have temporal parts. Some mereologically complex objects exist, but the sum of my elbow and this piece of paper isn't one of them.

[57] Wyman and his close companion McX, of course, are concerned with the commitments imposed by names and by predicates. Quine's (1948, p. 12) response is that it is exactly with sentences like (3) that we commit ourselves.

him willy-nilly.[58] And of course, in some contexts we *can* commit to events by uttering (3): contexts in which the domain of quantification is implicitly or explicitly restricted to things that exist. Presumably, when we are concerned with fundamental ontology—when, as van Inwagen (XXXX) puts it, we are in the ontology room—we are in such a context. Perhaps, even, when we are attempting to give the best possible account of the natures of particular reference properties.   But the contexts in which we construct and deploy compositional semantic theories are not, in general, such contexts.[59]

The second worry about the deflationary approach to semantics is epistemic: if the deflationary interpretation is right, it deprives us of certain sorts of evidence for our metaphysical theories. Jason Stanley (2006) has put this worry most clearly:

> Those philosophers who are outside of philosophy of language and semantics look to philosophy of language for certain kinds of resources. It is a cost to a metaphysical theory that it results in an error theory about certain central regions of discourse. For example, it is a familiar cost to presentism about tense that it seems to falsify many statements that we regard as obviously true (*e.g.* "England has had several kings named 'George'")….   The [deflationary account of semantics] will never be developed in a form that can play the role that current semantic theories do in these ongoing philosophical projects.[60]

The deflationary account of semantics, then, doesn't turn metaphysical questions into nonsense, but it does leave us without the epistemic tools we need to answer them.

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[58] The bulk of the present chapter, of course, presents an account of non-committing objectual quantification for the quantifers of semantic theories. Such a story can be straightforwardly adopted for object-language quantifiers, as well. For some other recent accounts of how quantification and predications of existence come apart—or, at least, might—see Parsons (1979), Azzouni (2004), Hofweber (2005), and Yablo (2009).

[59] See Chapter 3, below, for an argument that (at least some) scientific contexts allow for non-committing quantification.

[60] It is perhaps unfair to attribute this view to Stanley—at least in this exact formulation—as the quoted passage is from a blog post, and not a published article. Regardless of whether this is Stanley's considered position, however, it is a reasonable objection. In the post, Stanley is primarily concerned with attempts to deflate semantics by adopting a use theory of meaning, but the objection must be dealt with by any deflationary account. For more on the evidential relevance of empirical linguistics for metaphysics, see Ludlow (1999, pp. 4 – 5), and Stanley and Szabo (2000, p. 246).

The first thing to note in response is that a consistent non-deflationary approach to semantics seems to lead all by itself to various error theories: unless we deflate reference, for example, we must either admit that Harry Potter exists or deny a whole host of other commonsense judgments. A non-prensentism in which we quantify over past and present entities—perhaps taken to follow from our best semantic theories of tense—falsifies our strong belief that things come into and go out of existence. And so on. Insofar as we wish to avoid error theories, deflationism is an ally.

Furthermore, we don't generally need a semantic theory to tell us when a particular metaphysical theory systematically undermines a bunch of our beliefs. Consider: we judge that Harry Potter doesn't exist—and so van Inwagen (1977) and Salmon (1998) are mistaken about fictional characters; that tables and chairs do exist—and so van Inwagen (1995) is mistaken about composite objects; that murder is morally wrong—and so Mackie (1977) is mistaken about ethics. Perhaps these judgments are incorrect, but we manage to make them with nary a semantic theory in sight. They are judgments in first order discourse, ordinary judgments about fictional characters, composite objects, and the morality of actions; we do not need to ascend to semantics to make them.

If we don't—and in fact can't—look to our compositional semantic theories to find evidence for metaphysical claims, what evidence *do* we have for such claims? There is, I suggest, no *special* epistemological problem here: we have exactly the evidential resources that we bring to bear in answering any other question. These resources include all the tools of empirical science,[61] as well as the strategies we have for finding out rather

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[61] For more on issues of evidence and metaphysics in science, see Chapter 3, below.

mundane facts about the world.[62] They may even include special philosophical methods, such as *a priori* reasoning and conceptual analysis.

We can do quite a bit of metaphysics with these resources. Lewis (1986, pp. 203 ff.) argues for perdurantism from the claim that things have temporary intrinsic properties. Willimson (1998) argues for the existence of merely possible objects (partly) on the basis of the greater simplicity of a logic and metaphysics that countenances such things. Benacerraf (1973) argues against mathematical Platonism on the grounds that Platonism can give no satisfactory account of mathematical knowledge. None of these metaphysical arguments rely on semantic evidence.[63] Denying that the compositional semantics of English tells us about the deep structure and nature of the world does not force us into skepticism about issues metaphysical.

§8 Semantics and metaphysics disentangled. Our semantic theories—both taken alone and when conjoined with ordinary, uncontroversial claims about the world—entail various claims involving quantification, talk about reference, and singular terms; *e.g.*,

(3) There are events.

(L2) HARRY POTTER refers to Harry Potter.

Since we are bound to accept both our semantic theories and the ordinary, uncontroversial claims, we are also bound to accept these consequences. According to the thought behind the orthodox thesis and the direct argument, these consequences reveal various—perhaps surprising—metaphysical facts about the world.

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[62] *E.g.*: whether or not there is milk in the refrigerator, whether or not Harry Truman was the 33rd President, whether or not Harry Potter is a fictional character.

[63] Though both Lewis and Williamson also claim support for various of their metaphysical views from semantics.

But above I've offered an explanation of these supposedly committing features according to which the theoretical roles they play don't require—though they do permit—the existence of the controversial entities. We can accept—even *believe*—our best compositional semantic theories without taking a stand on the natures of particular reference and valuation properties, and so, too, without taking a stand on metaphysical questions about the existence of fictional characters, events, or possible worlds, about the nature of time or mereological composition. Metaphysical questions simply aren't asked or answered in the course of giving a compositional semantic theory. They are asked and answered in the course of *metaphysical* investigation, in the course of investigating the nature of the non-linguistic world. The widespread thesis is false.

References

Azzouni, Jody. 2004. *Deflating Existential Consequence: A Case for Nominalism* (New York: Oxford University Press).

Benacerraf, Paul. 1973. "Mathematical Truth," *Journal of Philosophy* 70, 661 – 679.

Cartwright, Nancy. 1983. *How the Laws of Physics Lie* (New York: Oxford University Press).

Devitt, Michael. 1981. *Designation* (New York: Columbia University Press).

Devitt, Michael and Kim Sterelny. 1987. *Language and Reality: An Introduction to the Philosophy of Language* (Cambridge, Mass.: MIT Press).

Donnellan, Kieth. 1974. "Speaking of Nothing," *Philosophical Review* 83, 3 – 31.

Evans, Gareth. 1982. *The Varieties of Reference* (New York: Oxford University Press).

Field, Hartry. 1980. *Science without Numbers* (Princeton, NJ: Princeton University Press).

Frege, Gottlob. 1891. "Function and Concept," in Petter Geach and Max Black (eds.), *Translations from the Philosophical Writings of Gotlob Frege* (Oxford: Basil Balckwell, 1966).

_____. 1892. "On Sense and Reference," in Petter Geach and Max Black (eds.), *Translations from the Philosophical Writings of Gotlob Frege* (Oxford: Basil Balckwell, 1966).

Gross, Steven. 2006. "Can Empirical Theories of Semantic Competence Really Help Limn the Structure of Reality?," *Noûs* 40, 43 – 81.

Higginbotham, James, Fabio Pianesi, and Achille C. Varzi (eds.). 2000. *Speaking of Events* (New York: Oxford University Press).

Hofweber, Thomas. 2005. "A Puzzle about Ontology," *Noûs* 39, 256 – 283.

Horwich, Paul. 1998. *Truth*, 2nd edn. (New York: Oxford University Press).

Kripke, Saul. 1973. *Reference and Existence: The John Lock Lectures for 1973* (unpublished).

_____. 1976. "Is there a Problem about Substitutional Quantification?," in Gareth Evans and John McDowell (eds.), *Truth and Meaning* (New York: Oxford University Press, 1976).

_____. 1980. *Naming and Necessity* (Cambridge, Mass.: Harvard University Press).

Larson, Richard and Gabriel Segal. 1995. *Knowledge of Meaning* (Cambridge, Mass.: The MIT Press).

Lewis, David. 1983. "New Work for a Theory of Universals," *Australasian Journal of Philosophy* 61, 343 – 377.

_____. 1986. *On the Plurality of Worlds* (Oxford: Blackwell Publishing).

Lewis, David and Stephanie Lewis. 1970. "Holes," *Australasian Journal of Philosophy* 48, 206 – 212.

Ludlow, Peter. 1982. "Substitutional Quantification and the Problem of Expression Types," *Logique et Analyse* 25, 413 – 424.

_____. 1999. *Semantics, Tense, and Time: An Essay in the Metaphysics of Natural Language* (Cambridge, Mass.: The MIT Press).

Lycan, William. 2001. "Moore against the New Skeptics," *Philosophical Studies* 103, 35 – 53.

Mackie, J.L. 1977. *Ethics: Inventing Right and Wrong* (New York: Penguin Books).

Montague, Richard. 1973. "The Proper Treatment of Quantification in Ordinary English," in Jaakko Hintikka, Julius Moravcsik, and Patrick Suppes (eds.), *Approaches to Natural Language* (Dordrecht: Reidel), 221 – 242.

Moore, G.E. 1944. "Four Forms of Skepticism," in G.E. Moore, *Philsophical Papers* (London: Allen and Unwin, 1959), 193 – 222.

Parsons, Terence. 1979. "Referring to Nonexistent Objects," *Theory and Decision* 11, 95 – 110.

Putnam, Hilary. 1975. "The Meaning of 'Meaning'," in Hilary Putnam, *Mind, Language and Reality: Philosophical Papers, Volume 2* (New York: Cambridge University Press, 1975).

_____. 2005. *Ethics without Ontology* (Cambridge, Mass.: Harvard University Press).

Quine, W.V. 1948. "On what there is," in *From a Logical Point of View*, 2nd edn. rev. (Cambridge, MA: Harvard University Press, 1980), 1 – 19.

Rorty, Richard. 1979. *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press).

Russell, Bertrand. 1905. "On Denoting," *Mind* 14, 479 – 493.

_____. 1919. *Introduction to Mathematical Philosophy* (London: Allen and Unwin).

Salmon, Nathan. 1998. "Nonexistence," *Noûs* 32, 277 – 319.

Sider, Theodore. 2002. *Four-Dimensionalism: An Ontology of Persistence and Time* (New York: Oxford University Press).

_____. 2009. "Ontological Realism," in David Chalmers, David Manley, and Ryan Wasserman (eds.), *Metametaphysics: New Essays on the Foundations of Ontology* (New York: Oxford University Press), 384 – 423.

Soames, Scott. 2002. *Beyond Rigidity: The Unfinished Semantic Agenda of Naming and Necessity* (New York: Oxford University Press).

Stanford, P. Kyle and Phillip Kitcher. 2000. "Refining the Causal Theory of Reference for Natural Kind Terms," *Philosophical Studies* 97, 99 – 129.

Stanley, Jason. 2006. "The Use Theory of Meaning," http://leiterreports.typepad.com/blog/2006/03/the_use_theory_.html

Stanley, Jason and Zoltán Gendler Szabó. 2000. "On Quantifier Domain Restriction," *Mind and Language* 15, 219 – 261.

Thomason, Richmond. 1980. "A Model Theory for Propositional Attitudes," *Linguistics and Philosophy* 4, 47 – 70.

Thomasson, Amie. 2003. "Speaking of Fictional Characters," *Dialetica* 57, 205 – 223.

_____. 2008. "Existence Questions," *Philosophical Studies* 141, 63 – 78.

van Fraassen, Bas. 1980. *The Scientific Image* (New York: Oxford University Press).

van Inwagen, Peter. 1977. "Creatures of Fiction," in Peter van Inwagen, *Ontology, Identity, and Modality: Essays in Metaphysics* (New York: Cambridge University Press, 2001), 37 – 56.

_____. 1995. *Material Beings* (Ithaca: Cornell University Press).

Walton, Kendall. 1983. *Mimesis as Make-believe: On the Foundations of the Representational Arts* (Cambridge, Mass.: Harvard University Press).

Williamson, Timothy. 1998. "Bare Possibilia," *Erkenntnis* 48, 257 – 273.

Wittgenstein, Ludwig.   1953.   *Philosophical Investigations* (Oxford: Blackwell
    Publishers).

Yablo, Stephen. 2009. "Must Existence Questions have Answers?," in David Chalmers,
    David Manley, and Ryan Wasserman (eds.), *Metametaphysics: New Essays on
    the Foundations of Ontology* (New York: Oxford University Press), 507 – 526.

Chapter 3

Against Indispensability

§1 A twist on the direct argument. In Chapter 2, I argued that the viability of the direct argument rests on a particular interpretation of the theoretical apparatus of our compositional semantic theories. And I offered an alternative, deflationary interpretation, according to which such theories are independent of contentious metaphysical issues.

But it might be objected that this maneuver misses the point of the direct argument. Consider Ludlow's pre-emptive strike against deflation:

> I fail to see how we can stipulate T-theories to be deflationary…. One way to get a handle on this issue is to think of the axioms and theorems in a T-theory as akin to the laws in any other science. One may not want to reify talk about planets and quasars, but it is hard to see how, short of general scientific anti-realism, once can escape commitment to them. (Ludlow 1999, p. 218, n. 15)

The key premises of (instances of) the direct argument, the thought goes, are drawn from our best *scientific* theories of language. In those theories, we *have to* quantify over events, fictional objects, possible worlds, times, and other sorts of entities that metaphysicians argue over. We have no idea how to eliminate such quantification.[1] So, according to our best science, such things exist. And there's simply no room for a deflationary re-interpretation, as scientific theories *demand* metaphysically heavy interpretations.[2]

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[1] As, for example, Parsons (1979) has emphasized in the case of fictional characters.

[2] Ludlow has also made this argument to me in conversation, as has Jason Stanley.

This defense of the direct argument relies on a version of the *indispensability argument*, originally advanced by Quine (1948 and 1955) and Putnam (1971). Putnam (1971, p. 57) succinctly formulates the argument as follows:

> Quantification over mathematical entities is indispensable for science, both formal and physical; therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question.

Quine (1955) makes clear that there's nothing special about mathematical entities—indispensability is just what gives us warrant to believe in all manner of objects.[3] Accordingly, we can construct an exactly parallel indispensability argument for the creatures of semantics: quantification over events, fictional objects, *etc.* is indispensable for science—in this case, the special science of *semantics*; therefore we should accept such quantification; but this commits us to accepting the existence of the entities in question.[4]

§2 **Indispensability arguments and direct verification.** Given a particular theory, an indispensability argument relies on five substantive claims to saddle us with an ontological commitment:

(1) Theory T quantifies over certain entities, Fs, and such quantification is ineliminable.

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[3] And, indeed, not just theoretical objects, but common-sense ones, too.

[4] This indispensability argument could be run as a stand-alone argument for the entities of semantics, but I prefer to view it as an augmentation of the direct argument that (i) makes explicit the scientific *bona fides* of the first premise of the direct argument, and (ii) attempts to foreclose the deflationary strategy pursued in Chapter 2, above. Even in stand-alone versions, the indispensability argument may not be entirely distinguished from the direct argument. Much of the interesting quantification in our semantic theories is embedded in biconditionals, and so appeals to commonsense judgments—*e.g.*, that Brutus stabbed Caesar—may be necessary to generate existentially quantified consequences.

(2) All theories are confirmed or disconfirmed as a whole (i.e., whatever confirmation attaches to a theory attaches equally to *all* sentences of the theory.)

(3) Whatever confirmation attaches to a theory does so to the extent that T has the five theoretical virtues: simplicity, familiarity, scope, fecundity, empirical success.[5]

(4) T is our best theory with respect to the five virtues.[6]

(5) Quantification over entities is ontologically committing to those entities.

(I) Our best theory ontologically commits to Fs.

Together with the sentiment—implicit in Putnam's formulation of the argument—that we are rationally bound to accept our best theories, (I) forces us to accept an ontological commitment to Fs. (2), holism, (3), the Quinean account of confirmation, and (5), the criterion for ontological commitment, are general claims about how science works. (1) and (4), on the other hand, are claims about whatever particular theory and entities are at issue. Quantum mechanics and relativity, for example, provide instances of (1) and (4) that convict us of commitment to mathematical objects. Semantic theories provide instances that convict us of commitments to events, fictional characters, *etc.*

There are roughly three ways to deny a commitment when confronted by such an argument. The anti-realist about Fs can deny (1) and is then confronted with the task of providing an entity-free version of the theory.[7] Or he can deny (4) and either advocate an alternative theory or plead agnosticism. These are both particularist strategies; they grant

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[5] See Quine (1955, p. 247).

[6] It is probably not enough that T be the *best* theory; likely, it must also meet some minimum threshold of confirmation. That is: T must not be just the best of uniformly bad alternatives, it must be a *good* theory. In what follows, I will suppose that any theory that is the best is also good.

[7] See Field (1980) for an example of this strategy applied to the case of numbers.

the general legitimacy of the indispensability argument but protest that the theory in question fails to satisfy the premises.[8] The remaining strategy is global: deny the legitimacy of indispensability arguments by rejecting the Quinean view of science, and claim that existentially quantified statements in simple, familiar, broad, fecund, empirically adequate theories are not necessarily ontologically committing. The aim here is to sever (or at least weaken) the link binding confirmation and quantification to ontology.

Penelope Maddy (1997) pursues a strategy of this last sort. Focusing on the history of the atomic/molecular theory, she argues that a theory may be, by Quinean standards, the best available theory and yet not commit us to the entities over which it quantifies. According to Maddy, the atomic theory, at the end of the 19th century, outperformed all competitors with respect to the Quinean virtues; it was, quite simply, the best available theory of the structure of matter. Nonetheless, skepticism about the existence of atoms persisted until Perrin's experiments on Brownian motion in 1908. (Indeed, Duhem and Mach denied the reality of atoms until they died, both in 1916.) Maddy proposes that acceptance of the reality of atoms awaited a further sort of test of the theory—"'seeing' or 'observing'...[or] 'direct testing' or 'experimentally verifying'" the existence of the posited entities (1997, p. 142)—and that Perrin provided just such a test. This conclusion suggests that something is wrong with the Quinean program. Although their best theories said there were atoms, turn-of-the-century scientists did not unequivocally accept them as anything more than posits, *mere* theoretical objects, or useful fictions.

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[8] I effectively considered, and rejected, such strategies for dealing with the semantic indispensability argument above in Chapter 2, p. 30.

The best explication of Maddy's position seems to be:

(6) Confirmation does not attach to entire theories; some statements of a theory require additional "direct experimental verification."

(7) Until such statements are directly verified, they should be viewed as useful fictions.[9]

(8) Quantification in fictional contexts is not ontologically committing.

And thus,

(A) The indispensable occurrence of quantification in a well-confirmed theory is not (necessarily) ontologically committing.

(6) is the rejection of holism and (8) is a qualified rejection of the criterion for ontological commitment.[10]

Though Maddy rejects wholesale application of the indispensability argument, her own position doesn't necessarily offer refuge to the opponent of the theoretical entities of semantics. Directly verified existential claims are still ontologically committing. And depending what's required for direct verification, it might be argued that, *e.g.*,

(9) There are events

is directly verifiable. (Consider: every time you see something happen, you're just seeing an event!) Furthermore, those claims with ontological import that *haven't* been directly verified can't be considered *true*—we must treat them fictionally, or instrumentally. But this amounts to adopting a limited form of anti-realism towards the theory in question. So, for example, until we directly verify the existence of fictional

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[9] See Maddy (1997, pp. 143, 152).

[10] Maddy herself is circumspect about whether the trouble is with holism or the criterion; see (1997, pp. 142-143).

characters—and really, how could we ever?—we must admit that we don't have good evidence that

(L2) HARRY POTTER refers to Harry Potter

is true. The best we can say is that it's useful to *treat* (L2) as true.

So: If we want to undercut the semantic indispensability argument, while remaining thoroughgoingly realist about our semantic theories, we must show that both Quine and Maddy are wrong about the interplay of evidential and ontological issues in science.

§3 **Some motivating worries.** As evidence for (6) – (8), Maddy offers the following historical claim (1997, p. 142): at the beginning of the 20th century, the atomic/molecular theory was well-endowed with the Quine's five theoretical virtues, but atoms were not considered real (the theory "was not accepted as true" (1997, p. 142)) until Perrin's experiments on Brownian motion. This account of the atomic theory leaves open various questions, both historical and philosophical, crucial to the assessment of Quine's and Maddy's ontological strategies.   Maddy paints a picture of a fairly unified scientific community agreeing on the empirical success of atomic theory, but awaiting direct confirmation of the existence of atoms. However, it is unclear that the community *was* united in its attitude towards the theory, or that anti-atomist skepticism was based entirely on the supposed experimental inaccessibility of atoms.

The second edition of van't Hoff's research monograph on theoretical organic chemistry, *The Arrangements of Atoms in Space* (van't Hoff 1898), appeared in 1894, fourteen years before Perrin's observations. There van't Hoff accounts for various features of chemical isomerism—most notably the rotation of light by some isomers of a

compound—by tracing them to the spatial structures of molecules. He opens with a "Statement of the Fundamental Conception":

> **The molecule a stable system of material points.**—When we arrive at a system of atomic mechanics the molecule will appear as a stable system of material points; that is the fundamental idea which continually becomes clearer and clearer when one is treating stereochemistry; for what we are dealing with here is nothing else than the spatial—*i.e.* the real—positions of these points, the atoms. (van't Hoff 1898, p. 5)

The essay, continuing in similar vein, offers no hint that the author considers atoms mere theoretical posits. Indeed, much of van't Hoff's treatment makes little sense if atoms are to be regarded as unreal: he goes beyond the systematic and "calculational"[11] advantages of the atomic notation and assigns the spatial structure of molecules a *causal* role in the production of observable phenomena. Thus, it seems, at least segments of the chemical community accepted an ontological commitment to atoms—and even used that commitment to theoretical advantage—prior to Perrin's experiments.

The primary anti-atomist argument was the apparent impossibility of observing of atoms; Maddy (1997, p. 138) quotes Berthelot: "Who has ever seen a gas molecule or an atom?" But the unobservability of atoms was not the only concern of the anti-atomists. Duhem (1991, p. 108) acknowledges that in organic chemistry, the atomic theory "has produced ... innumerable shoots of extraordinary vigor;" but, "in the numerous chapters which make up inorganic chemistry ... the mathematical operations of the atomic notation are of quite restricted usage." Admittedly, this passage appears in *German Science*, a virulently anti-German polemic and not a considered scientific essay. Nonetheless, it suggests that Duhem considered the accomplishments of the atomic theory to be limited;

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[11] See Duhem (1991, pp. 31 – 32).

his criticism is easily seen as a complaint that the theory fell short in one or another of the Quinean virtues—scope, fecundity, or, perhaps, empirical success.

Complicating the issue are the philosophical views of many opponents of the atomic theory. Duhem and Mach were early positivists; Ostwald, a later Nobel laureate, was Mach's student. What role did any parochial philosophical views of the anti-atomists play in their attitudes towards the theory? The answer to this question is vital to Maddy's evaluation of the indispensability argument. If the anti-atomists were motivated largely by philosophical positions not shared by the wider scientific community—and so rejected the atomic theory on other than purely *scientific* grounds—it softens Maddy's case that "the actual behavior of the scientific community in this case does not square with the Quinean account of confirmation" (Maddy 1997, p.142).

The foregoing suggests that attitudes towards the atomic theory were more nuanced than Maddy supposes, and that philosophical as well as scientific considerations were at play in the debate. In what follows I will offer a close reading of two stages in the history of the atomic theory in an attempt to assess Quine's and Maddy's accounts of confirmation and ontological commitment. Neither account is entirely accurate. The early acceptance of the atomic theory by chemists and the positivism of turn-of-the-century anti-atomists tell against any general scientific demand for "direct observation"— and so, too, against Maddy's postulation of a sixth theoretical virtue. Nonetheless, I agree with Maddy that "the case of atoms makes it clear that the indispensable appearance of an entity in our best scientific theory is not generally enough to convince scientists that it is real" (Maddy 1997, p. 143). For the history of the atomic theory speaks against the Quinean indispensability argument, as well. In the middle of the 19th

century, it was considered an open question just what the ontology of the atomic theory *was*. During this period, the attitudes of chemists towards the atomic theory only weakly correlated with their opinions on the ultimate structure of matter. This independence of metaphysics from theory—even if partial—undermines Quine's criterion for ontological commitment. And at the end of the 19th century (before Perrin's experiments), although chemists nearly universally believed in the discrete structure of matter, the debate over atomism persisted in physics. The difference in prevailing attitudes in chemistry and physics raises worries about Quinean holism.

Thus, the history of the atomic theory undermines both the indispensability argument and Maddy's account of scientific ontology. Proponents of the direct argument must, therefore, look elsewhere to defend against the deflationary interpretation of semantics offered in Chapter 2.

§4 **An episodic history of the atomic theory.** At the end of the 18th century, commonsense held that matter—substance—was continuous and infinitely divisible. When mixed, two substances combined such that they created an entirely new substance, and did not themselves continue to exist as component parts in the product. In some cases, of course, the product could be decomposed; but this process was viewed as generation of new instances of the reactants, and not merely recovery of substances persisting in the product. Generally, no distinction between chemical and physical combination—and so between true compounds and simple mixtures—was drawn.[12]

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[12] This view of matter and chemical combination is later mentioned by Wurtz (1898, p. 307) and discussed at length—indeed *advocated*—by Duhem (1902). A version of it can be seen at work in Richter (1792). For an overview of pre-Daltonian chemistry, see the first three chapters of Ihde (1964).

These opinions were not universal. Deriving from Boyle and Newton, crude strains of "corpuscularism," the view that matter is ultimately discrete, had survived the century. And William Higgins (1789) sketched an atomic theory and applied it problems of chemical combination, but it died without influencing the chemical community. Nonetheless, speculations on the structure of matter and the nature of chemical combination—whether finding discrete or continuous substances—played little role in the development of the chemical sciences prior to Dalton. Into the beginning of the 19th century, chemistry was largely an effort in cataloguing and characterizing substances and their reactions.

Dalton was first led to the atomic hypothesis by his researches into atmospheric composition and the behavior of gasses; only later did he carry atomism into chemistry. The first hint of his theory came in an 1803 lecture, wherein he explained the solubility properties of mixed gasses as resulting from "the weight and number of the ultimate particles of the several gasses" (quoted in Ihde (1964, p. 105)), and presented a table of the relative weights of these particles. Dalton's path from atmospheric physics to chemistry is obscure, but by 1807 his atomic theory was fully mature and was included in the third edition of Thomas Thomson's *System of Chemistry* (T. Thomson 1807).

Dalton published his own account, giving a much more detailed treatment, a year later in *A New System of Chemical Philosophy* (Dalton 1808). The key elements of Dalton's theory concern the discrete constitution of matter, the properties and physical behavior of atoms, and the manner of their chemical combination. Dalton writes: "[A]ll bodies of sensible magnitude, whether liquid or solid, are constituted of a vast number of extremely small particles, or atoms of matter bound together by a force of attraction"

(1808, p. 141). This force of attraction, which also accounts for condensation and freezing, is distinct from the forces that result in chemical combination. (So Dalton distinguishes mixtures and chemical compounds.) In the gaseous state, like particles repel each other; particles of different gasses do not affect each other. (These last hypotheses account for the law of partial pressures and the solubility properties of gas mixtures.) "[T]*he ultimate particles of all homogenous bodies are perfectly alike in weight, figure, &c.* In other words, every particle of water is like every other particle of water; every particle of hydrogen is like every other particle of hydrogen" (1808, p. 143; emphasis original). The particles of different chemical substances differ from each other in weight and volume. In chemical combinations, the identities of the combining atoms are preserved: "No new creation or destruction of matter is within the reach of chemical agency.... All the changes we can produce, consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance" (1808, p. 212). Atoms of elements—non-decomposable substances—are indivisible. Dalton also proposed rules for determining the numbers of constitutive atoms in compound substances, hypothesizing that the most common combinations of substances are the simplest: "When only one combination of two bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary.... When two combinations are observed, they must be presumed to be a *binary* and a *ternary*," and so on (1808, p. 214).

Dalton's atomism stood apart from earlier corpuscular hypotheses on the strength of three interdependent features. (i) It was more systematic and extensively developed than any previous such hypothesis; it not only posited discrete particles of matter, but

also assigned their law-like behavior a central role in the production of the chemical and physical phenomena that were of central interest to chemists. (ii) The atomic theory found evidential support in Dalton's own observations on the behavior of gasses; in the law of definite proportions, firmly established by Joseph Louis Proust; and in the law of multiple proportions, demonstrated by Dalton, Thomas Thomson, and William Hyde Wollaston. (The law of definite proportions states that substances combine chemically only in fixed ratios (by weight)—chemical composition does not vary continuously; the law of multiple proportions states that substances may combine in several different ratios, each ratio being an integer multiple of the smallest.) And (iii) Dalton had a clear vision that atomism was of theoretical and experimental use. He writes, in *A New System*:

> Now it is the one great object of this work, to shew the importance and advantage of ascertaining *the relative weights of the ultimate particles, both of simple and compound bodies, the number of simple elementary particles which constitute one compound particles which constitute one compound particle, and the number of less compound particles which enter into the formation of one more compound particle.* (Dalton 1808, p. 213; emphasis original)

A New System of Chemical Philosophy not only laid out the atomic theory, but founded a research program upon it. This single sentence set a fair portion of the agenda of chemistry for the next century, and is yet a reasonable description of much current chemical research.

Several of Dalton's theses, however, hindered the project of determining atomic weights and the formulas of compounds. His rules for determining formulas—privileging diatomic compounds—led him to mistake the composition of many substances. For example, he assumed that a molecule of water was composed of one

atom of oxygen and one atom of hydrogen, and assigned it the formula OH.[13] Since, at the time, it was measured that oxygen and hydrogen combine in a ratio of 7:1, this formula led to relative atomic weights of 7 and 1 for the two elements. We now know that Dalton's assumptions about the simplicity of chemical constitution are mistaken; a water molecule comprises two atoms of hydrogen, and the relative atomic weights of oxygen and hydrogen are roughly 16 and 1. (The constitution of water—one hydrogen or two—was a matter of much controversy for the first half of the 19th century.)

Two other shortcomings of Dalton's theory also bear mentioning. Dalton failed to distinguish clearly between *atoms* of elemental substances and *molecules* of compound substances; the ultimate particle of any substance, simple or compound, was called an 'atom'. This caused much confusion—especially in the discussion of combinations of gaseous substances—until Stanislao Cannizzaro drew the distinction in 1858. And Dalton's opinion that atoms of different substances differed in volume led him to reject the hypothesis, reached independently by Amadeo Avagadro and André Marie Ampère, that equal volumes of gas contained equal numbers of particles, as well as Joseph Louis Gay-Lussac's observation that gasses combined in fixed volume ratios. (Had Dalton accepted Gay-Lussac's results, he could have counted them as further evidence for his theory.) The Avagadro-Ampère hypothesis was revived by Cannizzaro in 1858 as well. Both of Cannizzaro's contributions finally led to the establishment of accurate weights and formulas.[14]

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[13] Though Dalton used different chemical symbols.
[14] See Cannizzaro (1858).

§4.1   The atomic notation and the ontology of atomism.   A half-century after Dalton introduced his theory, the debate over atomism was taken up in earnest at an international congress in Karlsruhe. The participants recognized a distinction between "two kinds of questions—those that concern the very root of things, and others that are questions of form" (Wurtz 1860, p.27). The first concerned the physical reality of atoms—and were considered by Adolphe Wurtz to be "not yet ripe enough" for resolution (Wurtz 1860, p. 27). The second concerned the relative merits of the atomic notation and the equivalent notation.

The problem of determining chemical formulas and atomic/equivalent weights requires solving for numerous variables. So, for example, hydrogen and oxygen combine in a ratio of 1:8 (by weight) to give water.   If a and b are the atoms/equivalents of hydrogen and oxygen in water, and H and O the atomic/equivalent weights, the equation

(1) $aH / bO = 1 / 8$

must be solved for all four variables. The chemical formula for water is then H_aO_b.

The atomic notation presupposes that there is a uniquely correct solution to the equation, and thus a uniquely correct formula for water. According to the atomists—who answered the ontological question about atoms affirmatively—in the atomic notation H and O express the relative weights of the fundamental particles of hydrogen and oxygen,[15] and a and b are the number of atoms of each element in one molecule of water. Weights and formulas are determined from combining proportions, chemical analogies (e.g., that lead and antimony exhibit similar chemical behavior), combining volumes (with the aid of Avagadro's hypothesis and Gay-Lussac's law), and various physical data

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[15] A standard—e.g., $H = 1$, or $O = 10$, or $C = 12$ (the modern standard)—must be conventionally determined.

	H:O:C	H=1, O=8, C=3	H=1, O=8, C=4	H=1, O=8, C=6
water (H_2O)	1:8:0	HO	HO	HO
methane (CH_4)	1:0:3	HC	H_4C_3	H_2C
ethane (C_2H_6)	1:0:4	H_3C_4	HC	H_3C_2
carbon monoxide (CO)	0:4:3	OC_2	O_2C_3	OC
carbon dioxide (CO_2)	0:8:3	OC	O_4C_3	O_2C
carbonic acid (H_2CO_3)	1:24:6	HO_3C_2	$H_2O_6C_3$	HO_3C

Table 3-1: Alternative Chemical Equivalents

and laws (e.g., specific heats and the law of Petit and Dulong, which states that the product of an element's atomic weight and its specific heat is a constant).

In the equivalent notation, by contrast, one of the solutions to equation (1) is chosen by explicit convention, thus setting the formula for water as well as the "equivalent" weights of hydrogen and oxygen. Taking different reactions as the standards for equivalent weights—setting different conventions—results in different chemical formulas. Take as an example, the following series of compounds: water, methane, ethane, carbon monoxide, carbon dioxide, and carbonic acid. Table 3-1 shows the formulas that result from taking different bases for the determination of equivalents. The first column shows the combining ratio of the elements in each compound (given in the row heading with it's modern chemical formula); the second column shows the formulas that result from taking the standards to be water and methane (i.e., water = HO and methane = HC); the third column shows the formulas when water and ethane are the standards, and the fourth when water and carbon monoxide are the standards. From the point of view of the equivalent notation, the above chemical formulas (taken column by column) are equally correct, and differ only in the equivalent weight *conventionally assigned* to each element.

In practice, equivalents were chosen so as to give simple formulas for particularly important or common substances, and so that chemically similar substances were represented by similar formulas; thus, water was almost always assigned the formula HO, giving equivalent weights $H = 1$ and $O = 8$. And considerations deriving from the combining volumes of gaseous reactants and from series of reactions with molecular reactants often led to molecular formulas that were integer multiples of the empirical formula.

Marcellin Berthelot, a prominent anti-atomist and advocate of the equivalent notation, explains, "Equivalents express...the ratios of weight according to which bodies combine or substitute themselves for one another" (Berthelot 1877, p. 244). The coefficients in chemical formulas (e.g., a and b in the formula H_aO_b) indicate the combining proportions of the elements relative to the given equivalents.

Atomists naturally preferred the atomic notation; but, surprisingly, some anti-atomists argued on its behalf as well. Berthelot and Charles Marignac carried the notational debate from the Karlsruhe Congress into the journals. Leaving aside his antipathy towards the atomistic hypothesis, Berthelot objected to the atomic notation on two grounds. Most importantly, he argued that there was in fact no consistent and generally accepted system of atomic weights: "The agreement of the numbers adopted by the partisans of atomic weights is then more apparent than real" (Berthelot 1877, p.246). In part, the confusion was a remnant of Dalton's initial assumptions (see above). But also, the physical methods employed in determining atomic weights did not always agree, and even when consistent weights could be determined, they often required complicating established chemical formulas and reaction schemes based on equivalents.

Berthelot also objected to the great weight proponents of the atomic notation placed on physical data. Equivalents, he insisted, "may be determined from purely chemical considerations, which are never wanting..." (Berthelot 1877, p. 244); the physical properties of substances enter into the determination of equivalent weights "only in a subordinate way, and for the purpose of giving greater precision to chemical analogies" (Berthelot 1877, p. 244).

Marignac (1877) argued that the equivalent notation had its own confusions and inconsistencies: the equivalent commonly given for nitrogen, for example, corresponded by volume, but not by chemical value, to an equivalent of hydrogen or chlorine; it corresponded by chemical value, but not by volume, to an equivalent of phosphorous or arsenic; and it corresponded not at all to equivalents of oxygen and sulphur (p. 233). The atomic notation, in Marignac's opinion, held various advantages over the equivalent notation. Atomic weight is directly proportional to specific heat for simple gasses;[16] molecular heats are proportional to the number of atoms in the molecule. The atomic weights of gaseous elements correspond to equal volumes, so that "their ratios of combination in volumes are directly expressed by atomic formulas" (Marignac 1877, p. 237); likewise, the molecular weights of most organic substances correspond to equal volumes. The atomic notation simplified the formulas of numerous compounds, essentially dividing through by two (e.g., $C_4H_8O_4$ in the equivalent notation became $C_2H_4O_2$ in the atomic notation). The atomic notation also accounted for various chemical isomorphisms (e.g., $AgCl$ and Ag_2S compared to $CuCl$ and Cu_2S) known experimentally but "incomprehensible with the notation based on equivalents" (Marignac 1877, p. 238).

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[16] Berthelot denied this, but even if he hadn't, it is not clear that he would have recognized it as an advantage for the atomic notation—he and Marignac disagreed fundamentally on the proper role of physical properties in chemistry.

Despite championing the atomic notation, Marignac did not embrace the existence of atoms. He agreed with Berthelot that "[t]he atomic weights rest on an hypothesis which has never been, and, in fact, can never be demonstrated...that of the existence of atoms" (Marignac 1877, p. 236). Nonetheless, he argued, "the existence of atoms is only useful in justifying the name of *atomic weights*" (Marignac 1877, p. 236); the weights themselves he regarded "as being only equivalents, in the determination of which arbitrary conventions have been replaced by scientific considerations, based on the study of physical properties" (Marignac 1877, p. 237). Thus, although he embraced the atomic notation and the physical methods that the atomists used to determine the relative weights of the chemically fundamental particles, Marignac insisted on interpreting the notation in terms of equivalents, without reference to physical atoms.[17] He did not, however, abandon the term 'atom'.

This attitude—adopting the nomenclature of atomism, while tacitly reinterpreting it in terms of equivalents or combining proportions—seems to have been fairly common. So William Odling, although he also employed the atomic notation, insisted that "all [chemists'] modes of thought, and all the government of their actions...are based upon the observed fact that certain bodies combine in certain proportions;" and that the "laws of chemical combination are general expressions of observed facts upon which the atomic hypothesis is superinduced" (Brodie 1869, p.144).

The empiricist worry of Berthelot and Marignac—that the existence of atoms could never be demonstrated—and the distinction between hypotheses and (observable) facts motivated much of the anti-atomism of this period. Some scientists, however, also

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[17] Duhem advocates the same course in his (1892). Such a strategy abandons the prospect of an *explanation* of why physical properties consistently correspond to a single system of equivalents. This was no bother to Duhem, of course, since he denied that explanation was a proper goal of science.

questioned the scope or utility of the atomic theory. Edward Frankland doubted that the atomic theory could be reconciled with the acceleration of gas-phase reactions by heat (Brodie 1869, p. 143); and Benjamin Brodie argued that the lack of an explanation provided by the atomic theory for the law of even numbers (that the sum of volumes of "dyad elements" entering into reaction is even) "indicate[d] some profound defect in chemical theory" (Brodie 1866, p. 786). Brodie, Odling, and Berthelot all questioned the contribution of atomistic notions to the progress of their science and believed that such could, if necessary, be eliminated without inhibiting the study of chemical combination, the topic they took as definitive of chemistry.[18]

In an 1869 lecture to the Chemical Society of London (Williamson 1869), A.W. Williamson presented an extended argument for atomism.[19] Williamson (1869, p. 113-115) suggests that the law of multiple proportions does not by itself give evidence for atomism, but does so only in conjunction with considerations of molecular composition and combination. So we find that, as predicted by the atomic theory, compounds never enter into reactions in a proportion less than the sum of the weights of their constituent atoms. (*E.g.*, ferric oxide, Fe_2O_3, with a molecular weight of 160 never enters into a reaction as less than 160 parts by weight.) According to Williamson (1869, p. 121), "not one of the enormous number [of reactions] that we know accurately and with certainty, has shown combining proportions of molecules at variance with the atomic theory, and corresponding to the idea that matter is infinitely divisible." The type-theory of chemical classification and the radical theory of chemical combination—according to

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[18] For Odling's opinions, see Brodie (1869, pp. 143-144); for Brodie's opinions see his (1866) and (1869). Berthelot questions the importance of the atomic theory in his (1877, p. 246).

[19] Brodie (1869) is an account of the subsequent discussion.

which certain whole groups of atoms retain their identities through chemical change and determine the general properties of the compounds—in Williamson's eyes, also provided chemical evidence for the atomic theory. Apart from these considerations, Williamson also found confirmation of the atomic theory in various physical properties of substances: the proportionality of gaseous and vapor densities to atomic and molecular weights;[20] the independence of gaseous volume (under like conditions) from chemical identity; the periodicity of boiling and melting points; and the dependence of diffusion rates on atomic and molecular weights.

Williamson could find nothing to account for these facts other than the existence of atoms. He challenged the anti-atomists:

> The opponents of the atomic theory are bound to explain, in some other way, the facts which point so distinctly to the existence of molecules, if they wish to advance from the position of mere contradictors to that of chemists.
> Hitherto they have not done so, and the case stands thus: on the one hand, we have a simple theory which explains in a consistent manner the most general results of accurate observation in chemistry, and is daily being extended and consolidated by the discovery of new facts which range themselves naturally under it. On the other hand we have a mere negation: for the statements of those who say that our evidence of the existence of atoms is not conclusive, and yet omit to show any alternative are nothing more. (Williamson 1869, 125-126).

Williamson was correct: there was no serious competitor to the atomic theory. No other view on the structure of matter played a significant role in guiding the direction of chemical research or in the explanation of observed facts. For the anti-atomists, the proposition that matter is continuous was not a theoretical tool, as the proposition that matter is discrete was for the atomists. This is, perhaps, why many anti-atomists still made some sort of use of the atomic theory—it was the only theoretical guide they had.

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[20] Note that Marignac freely used this proportionality to set "atomic" wieghts, but did *not* admit it as evidence of the existence of atoms.

The atomists of this period began to take advantage of their ontology. If atoms were real, then they could be a causal component in the production of observable phenomena. This provided the atomists with a powerful explanatory tool, and even, in some cases, the ability to manipulate and engineer physical properties of substances. William Thomson presented his theory of vortex atoms, according to which atoms are vortices in the ether, in 1867. This theory of the constitution and structure of atoms was ultimately fruitless; but Thomson sought to obtain from it explanations of the physical behavior of the phases of matter, and of the spectral characteristics of elementary substances (W. Thomson 1867). Louis Pasteur (1861) attributed the rotation of light by some crystals to an asymmetric arrangement of the molecules, and the rotation of light by some solutions to an asymmetry in the solute molecules themselves. Soon after, Jacobus van't Hoff began to develop a theory of the spatial arrangement of atoms in molecules that showed how molecular asymmetry could arise (Van't Hoff 1874). Van't Hoff's theory, stereochemistry, allowed chemists to predict the number of isomers of a substance as well as the optical activity of substances. It also allowed chemists to *design* syntheses to obtain optically active products, often from optically inactive reactants.[21] Van't Hoff's theory made possible (perhaps the first) structural design of molecules—and so engineering of substances based on considerations about the substances themselves, rather than about chemical analogues.

§4.2 The triumph of atomism. By the beginning of the 20th century the atomic notation had won a decisive victory over the equivalent notation. In part, this was because a consistent set of atomic weights was finally developed and had gained general acceptance

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[21] Victor Meyer proposed two such syntheses—of isomers of bromochloronitroethane—in 1876, although it is unknown whether he carried them out; see Ramberg (2003, p. 160).

over preceding third of a century (thus eliminating one serious complaint against the notation). Too, some of the staunchest advocates of equivalents—notably Henri Saint Claire Deville—had died; and the survivors were perhaps swept up in the march towards standardization that had begun at the Karlsruhe Congress. In 1897 even Berthelot converted to atomic formulas.[22]

Among chemists, the ontological debate also seems to have swung in favor of atoms. The dissembling justifications of the atomic notation prevalent in the 1860's and 1870's (*e.g.*, Marignac's) are almost completely absent from turn-of-the-century chemical literature,[23] and protestations that atoms were only mis-named equivalents disappeared. The notation had become entirely unremarkable. And, too by this time, atoms and molecules were *routinely* invoked in contexts divorced from stoichiometry and notational formalisms, in contexts in which they could not be just combining proportions. The following passage is typical:

> We can think of the benzene molecule forming a molecular compound with the bromine molecule, in which system the individual atoms can exert forces on each other, so that an internal re-arrangement of atoms in the additive molecule can take place, resulting in an unstable molecule which breaks down into the final products. This conception gives us a reasonable picture of substitution. We do not consider the atoms in a molecule as rigidly attached at certain definite positions with regard to the molecule, but whatever the forces between atoms may be, in the molecule all atoms can act on each other. Thus if a bromine molecule as a whole is brought into close enough proximity to the benzene molecule, then a bromine atom and a hydrogen atom form, at first as part of the molecule, a system without much effect external to itself and so without much connection with the molecule, in other words, the complex additive molecule is unstable and the HBr molecule splits off. (Bevan 1904, p.111)

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[22] See Nye (1984, p. 242).

[23] The notable exceptions are penned by Duhem. See, for example, his (1900) and (1902).

These atoms and molecules, with spatial positions and causal powers and instabilities, cannot be notational fictions—they must be real physical entities. And there is no hint in the literature that chemists distinguished these physical atoms from the chemical atoms referenced in chemical formulas. The word 'atom' took the same meaning whether the combining properties of substances or their microscopic behavior was at issue. The atomic notation—although independent of ontological atomism 40 years earlier—was, in the early 1900's, tied intimately to the discrete structure of matter.

As the atomists of the mid-1800's had, chemists (and physicists) at the turn of the 20th century put atoms and molecules to work in explanation and prediction. Van't Hoff had further developed his ideas on stereochemistry, and they were widely applied in organic chemistry to predict and explain isomerism and optical activity.[24] Alfred Werner extended stereochemistry from organic chemistry into inorganic chemistry and used it, in conjunction with his theory of coordination (chemical bonding), to account for reactivities and physical properties of metal compounds.[25] Johannes Thiele (1899) made crucial use of structural theories of molecules in explaining the properties of benzene and related compounds. J. J. Thomson (1904) proposed an internal structure for the atom to account for his observation of cathode rays. Ernest Rutherford proposed that radioactive change by the emission of a-particles was, in fact, the disintegration of atoms, and attributed it to "some peculiarity of atomic constitution" (Rutherford 1905, p. 219). These were all among the most significant applications of atomic theory, but they were not extraordinary. The idea of the physical atom pervaded *fin de siécle* chemical research.

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[24] See van't Hoff (1898).

[25] See Werner (1893) and (1898).

(In 1901, Van't Hoff won the inaugural Nobel Prize in Chemistry for his development of stereochemistry and his discovery that Avagadro's law applies to the osmotic pressure of solutions. The presenter, C. T. Odhner, remarked of his work, "As a result of this the concept of the molecule in chemistry was found to be definite and universally valid to a degree hitherto undreamed-of" (Odhner 1901). Rutherford won the chemistry Nobel in 1908, and Werner won in 1913. J. J. Thomson won the physics prize in 1906 for his work on electricity; J. P. Klason presented the prize and claimed, "even if Thomson has not actually beheld the atoms, he has nevertheless achieved work commensurable therewith, by having directly observed the quantity of electricity carried by each atom" (Klason 1906).)

William Noyes assessed the general opinion of organic chemists in 1909: "there is, among these, a practically universal belief that atoms and molecules actually exist and that there is something in the structure of the molecules which actually corresponds to our formulas" (Noyes 1909, p. 1369).[26] Even Ostwald (speaking some years earlier) was forced to admit that the atom was widely reckoned to be real:

> Every scientifically thinking man, if called upon to express his opinion as to the 'inner structure' of the universe, would sum up his ideas in the conception that things consisted of atoms ... and that these atoms and their mutual forces were the final realities underlying all phenomena. (Ostwald 1896, p. 337)

F. W. Clarke, speaking to the Manchester Philosophical Society one hundred years after Dalton had given the first intimation of his theory before the same body, delineated the evidence that shaped the opinion of chemists: Avagadro's law and it's extension to solutions by van't Hoff, the law of Dulong and Petit, Faraday's laws of electrolysis

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[26] Noyes delivered this remark at an address in September, 1909—the same month Perrin's pamphlet announcing his results on Brownian motion appeared in French, and many months before it was translated into English (Perrin 1910).

(relating the amount of substance created at an electrode to the charge passed), the periodic law of Mendeleev, and "the multitude of relations connecting the physical constants of bodies with their chemical character" (Clarke 1903, p. 523). Many of these same considerations were cited in defense of the atomic theory 35 years earlier by Williamson (1869); and with the additional evidence that had accrued, the support for the atoms was nearly unshakeable. Clarke (1903, p. 523) writes, "The salient facts ... make, at least for chemists, an exceedingly strong case. The convergence of testimony is remarkable, and when we add to the chemical evidence that which is offered by physics, the theory becomes overwhelmingly strong .... The atomic theory has had no better vindication."

Vocal opposition to atomism in this period came, not from chemists, but from physicists pursuing the energeticist program championed by Mach, Duhem, and Ostwald.[27] The energeticists aligned themselves against Boltzmann's kinetic-statistical account of thermodynamic phenomena, preferring instead to take macroscopic thermodynamic parameters as fundamental. Ostwald went so far as to insist that energy—and not matter—was the only basic constituent of the universe: "undoubtedly ... the predicate of reality can be affirmed of Energy only" (Ostwald 1896, p. 349). And Duhem attempted to derive chemical mechanics—indeed, all mechanics—from (energeticist) thermodynamics; he declared, "Rational Mechanics is no more than the application to a particular problem of *locomotion* of this general Thermodynamics, of this

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[27] Ostwald was, of course, both physicist and chemist—he won the chemistry Nobel in 1909 for his work on catalysis. He and Berthelot were the most notable anti-atomistic chemists.

Energetics whose principles encompass all the transformations of the inorganic world" (Duhem 1902, 109).[28]

The energeticists and kinteticists engaged in conflict on methodological, theoretical, and evidential issues. The energeticists abjured the use of mechanical models, dismissing them as mere analogies, and declaring that they inevitably lead to contradiction.[29] (Boltzmann's theory, of course, was the epitome of a mechanical theory.) At the theoretical level, besides rejecting the atomic hypothesis (and so too the role it played in kinetics), the energeticists decried the statistical interpretation of Carnot's version of the second law of thermodynamics. The energeticists insisted that Carnot's principle was absolute and ruled out reversible processes; the kineticists viewed the principle as an expression of (im)probability (of, say, all the molecules of a gas rushing to one side of its container) and allowed for reversible processes at the microscopic level (e.g., molecular collisions). And the energeticists disputed the success of the kinetic theory; so Nye (1972, p. 36) reports that Henri Poincaré "conclud[ed] that it could not account for all the known experimental facts, that it could neither be deemed true nor false, and that the question of its sterility or fecundity as an aid in discovery remained wide open."[30]

The energeticist program was driven, at least in part, by the philosophical positivism of its advocates. Duhem and Mach were, of course, the most prominent and influential positivists of the era. Following Comte, Duhem rejected the idea that

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[28] Duhem relates thermodynamics to chemical mechanics in chapter 10 of his (1902).

[29] See, for example, Ostwald (1896, pp. 342-343).

[30] For a rich account of the controversy over energetics and kinetics and its relation to the dispute over atoms, see Nye (1972).

explanation is a goal of science and declared, "A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and exactly as possible a set of experimental laws" (Duhem 1954, p.19). For Duhem, 'experimental laws' encompassed only statements of observable regularities, free of "hypotheses."[31] And accordance with these laws is the ultimate measure of theory: *"Agreement with experiment is the sole criterion of truth for a physical theory"* (Duhem 1954, p. 21; emphasis original). Indeed, Duhem's insistence on this criterion was so staunch—and his interpretation of it so narrow—that it led him to cast aspersions on the empirical basis of the law of multiple proportions. He argued, "No procedure of chemical analysis, however subtle it might be supposed to be, can give us the exact relation between the mass of carbon and the mass of hydrogen in a chemical compound. It brings to our knowledge only two numbers A and B between which the proportions lie" (Duhem 1902, p. 92). Because of this limit imposed by experimental error, the law of multiple proportions "transcends experience" and is ultimately justified only by the "power and fecundity" of the chemical system that it undergirds (Duhem 1902, p. 93).[32]

Ostwald, who had been Mach's student, was not as philosophically sophisticated or influential as Mach and Duhem, but he was no less of a positivist. On one occasion, after pressing objections to the kinetic theory—and mechanistic theories in general—Ostwald exhorted:

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[31] So, for example, Avagadro's law—since it pronounces on the number of (unobservable) particles in a given sample of gas—is not an experimental law.

[32] Note that in Duhem's view (*contra* Quine) this power and fecundity do not speak to the *truth* of the system.

> Thou shalt not make unto thyself any image or likeness. Our task is not to view the world in a more or less bedimmed and crooked mirror, but as directly as the nature of our minds will permit. To co-ordinate realities, *i.e.*, definite and measurable quantities, so that when certain of them are given the others can be deduced, is the problem set before science, and this problem cannot be solved by assuming as substratum any hypothetical analogue, but only by the determination of the mutual relations existing between measurable magnitudes. (Ostwald 1896, p. 346)

Thus, according to Ostwald, it the aim of science to relate *observable* facts to each other (in service of prediction)—and this can be properly done only through reference to observable properties (e.g., macroscopic thermodynamic properties).

Other chemists and physicists recognized and rejected the positivist foundations underlying criticisms of atomism and the kinetic theory. So Einstein wrote, retrospectively,

> The antipathy of these scholars [Ostwald, Mach] towards atomic theory can indubitably be traced back to their positivistic philosophical attitude. This is an interesting example of the fact that even scholars of audacious spirit and fine instinct can be obstructed in the interpretation of facts by philosophical prejudices. The prejudice...consists in the faith that facts by themselves can and should yield scientific knowledge without free conceptual construction. (Einstein 1949, p. 49)

And Dimitri Mendeleev—refering particularly to Ostwald's brand of energeticism and its positivist connotations—declared, "This (to my mind) scholastic view reminds me of the philosophy according to which nothing exists except 'I' because everything comes through my consciousness. Such notions are unlikely to be retained in healthy minds."[33] He later objected to the anti-atomists: "If you take away from modern chemistry atomic concepts of the structure of matter, there will be no understanding of the multitude of firmly established facts, and a rough empiricism will follow."[34]

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[33] Quoted in Solov'ev and Petrov (1968, p. 305).

[34] Quoted in Solov'ev and Petrov (1968, pp. 305-306).

The kinetic theory finally found vindication in Perrin's experiments on Brownian motion.[35] In a series of papers from 1905 and 1906, Einstein made a theoretical study, from the standpoint of the kinetic-molecular theory, of the behavior of colloids and suspensions.[36] Einstein offered predictions about the translational and rotational motions of suspended particles, their diffusion, and the molecular dimensions. He also suggested in the initial paper (Einstein 1905) that the study of colloids could very well provide a crucial experiment for kinetics and energetics; for kinetic theory and energetics lead to different predictions about the behavior of suspended particles.   After a number of smaller and less decisive papers on Brownian motion—none of which directly addressed Einstein's theoretical work—Perrin's key article (translated into English as Perrin 1910) appeared in 1909. Perrin there offered three independent determinations of Avagadro's number, N, which agreed reasonably well with each other and with the results obtained by other methods,[37] and displayed experimental results confirming Einstein's theoretical predictions. Perrin declared, "The molecular kinetic theory of Brownian movement has been verified to such a point in all its consequences that, whatever prepossession may exist against Atomism, it becomes difficult to reject the theory" (Perrin 1910, p. 74).

Other physicists agreed, and the energeticists began to give in. Poincare admitted, "Atoms are no longer a useful fiction; things seem to us in favor of saying that we see them because we know how to count them.... The brilliant determinations of the number of atoms made by M. Perrin have completed the triumph of atomism.... The atom of the

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[35] Maddy, following Nye (1972), gives an excellent account of this portion of the history; see Maddy (1997), pp.139-142. See Nye (1972) for a more detailed historical account, and Perrin (1910) and (1913) for the primary scientific expositions.

[36] Einstein (1905), (1906a), and (1906b); see Nye (1972, pp. 112-118) for an overview of Einstein's results.

[37] See Perrin (1910, p. 90) for a tabular summary.

chemist is now a reality."[38] Even Ostwald capitulated. He wrote, in the preface to the

fourth edition of his textbook, *Grundriss der physikalischen Chemie*, "The agreement of

Brownian movement with the demands of the kinetic hypothesis... which [has] been

proved through a series of researches and at last most completely by J. Perrin, entitles

even the cautious scientist to speak of an experimental proof for the constitution of space-

filled matter."[39]

Ostwald aside, Perrin's results garnered comparatively little attention from

chemists—for chemists, by and large, already believed in atoms. *Brownian Movement*

and Molecular Reality (Perrin 1910) left no discernible mark on the German journals

Berichte der Deutschen Chemische Gesselschaft, *Monatshefte für Chemie*, and Liebig's

Annalen der Chemie, nor on the *American Journal of Chemistry*.[40] The *Journal of the*

Chemical Society (London) abstracted the German translation of Perrin's pamphlet, but

the proof of molecular reality had little, if any, noticeable impact on the published

articles. The review of the German version in the *Journal of Physical Chemistry* reads, in

its entirety:

> In this little volume we have an admirable presentation of the facts in
> regard to the Brownian movements. The author makes out a very good
> case and the book can be recommended to everybody. The reviewer was
> especially interested in the experiments, p. 43, on the distribution of the
> particles of mastic under the influence of gravity. The one weak point in
> the treatment is the transition from an emulsion to a true solution. If the
> change is a continuous one, we are confronted by the problem why he
> solute does not coagulate under certain conditions. (Bancroft 1911)

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[38] Quoted in Nye (1972, p. 157).

[39] Quoted in Nye (1972, p. 151).

[40] Although the German journals neither reviewed nor abstracted new books.

The review does not even mention atoms or molecules. In the *Journal of the American Chemical Society*, the reviewer agrees with Perrin that "the actual existence of atoms and molecules must be regarded as conclusively proved" (Washburn 1911, p. 603); he takes the decisive evidence to be "the striking agreement among so many methods of such widely different character" for determining Avagadro's number (Washburn 1911, p. 603), not Perrin's confirmation of Einstein's predictions. Perrin presents fifteen different determinations of N, three of which came out of his own investigations—and which could not be considered of widely different character from each other. Again, Perrin's impact seems to be limited. Among chemists, Perrin's experiments were seen primarily as a contribution to knowledge of colloids. Atomism was already rampant in the chemical community, and so the arguments that silenced the energeticists had few targets there. Chemists simply did not need to be convinced that atoms were real.

§5 Against direct observation and useful fictions. Relying on Nye (1972), Maddy makes the historical claim that, although atoms were indispensable to chemical and physical theories at the end of the 19th century, the scientific community refused to embrace atoms as real until Perrin's experiments allowed them to be "observed." Nye (1972) gives the impression that the controversy over atomism was coextensive with the controversy over thermodynamics, and Maddy interprets the skepticism about atoms in physics as motivated by a general scientific demand for direct confirmation. But an examination of other aspects of the history—presented above—a should dispel the impression and correct the interpretation.

The clash over the proper formulation of thermodynamics and the clash over atomism were certainly linked: kineticists, in general, accepted the reality of atoms,

energeticists, in general, did not. (And evidence in favor of the kinetic theory was, indeed, evidence for atoms.) But, around 1900, these debates were both parochial to *physics*—the debate about thermodynamic theory because it was primarily a physical, not chemical, theory, and the debate about atomism because *chemists had already committed themselves to the discrete nature of matter*. In chemistry, atoms and molecules were treated as real entities, with spatial properties, causal powers, and internal structures; and they were widely called upon in explanations of observable phenomena, as well as in theoretical descriptions of unobservable phenomena.[41] Contemporary sources (*e.g.*, Noyes (1909), Ostwald (1896)) make clear that chemists' doxastic attitudes towards atoms tracked their practical application of the concept in these contexts: chemists didn't just use atoms, they believed in them. Thus, questions over the foundations of thermodynamics were relevant to atomism only for a portion of scientific community. And, consequently, the impact Perrin's "proof" of atomism was limited to physics, where the issue was still considered live.

Maddy's position that atomism was the best available theory, but that scientists (or at least physicists) awaited whatever sort of proof Perrin provided, belies the countours of the debate (in physics) over kinetics and energetics. The anti-atomists did not view atoms, in the context of kinetic theory, simply as (supposedly) useful fictions; they recognized the metaphysical role atoms played in that theory. If the theory were truly ontologically neutral, if fictive atoms could serve kinetics just as well as real atoms, then energeticist complaints that it was based on an unproven hypothesis would have been pointless. So, at the very least, the energeticists deemed that their opponents intended kinetic theory to carry an ontology of atoms. A large portion of the energeticist

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[41] For an example of the latter, see the passage—quoted above, p. 93—from Bevan (1904, p. 111).

project, then, was to show that thermodynamics could be done—and, indeed, done better—without recourse to atoms.

The energeticists wielded two general sorts of complaints against atomism. The first attacked its scientific merits—its fruitfulness, its ability to account for known phenomena, etc. The energeticists *did not* view kinetics as "well endowed with the five theoretical virtues,"[42] only lacking of direct proof. Poincaré felt that the kinetic theory failed to cover various experimental facts and doubted its fecundity;[43] Duhem found phenomenal thermodynamics much more familiar than the mechanistic kinetic theory;[44] Ostwald (1896, p. 338) claimed the kinetic theory was "inconsistent with undoubted and generally known and recognised truths." These scientists believed that energetics provided a better theory—by roughly Quinean standards—than did kinetics. The second type of complaint was typified by Berthelot's question, "Who has ever seen a gas molecule or an atom?"[45] But this demand for observation, a demand to *see* atoms—as understood on some narrow and perhaps untenable model of seeing—seems bound to the positivist doctrines the energeticists espoused: their opposition to mechanistic models as mere analogies, their distrust of hypothesis, their insistence that the only responsibility of theory is to observational facts. These attitudes were generally rejected by the wider community of scientists.

Maddy uses her claims about the history of the atomic theory to support the philosophical position that the confirmation of theories (or their parts)—and so real

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[42] See Maddy (1997, p. 142).

[43] See Nye (1972, p. 36).

[44] See, for example, his (1954, pp. 69-72).

[45] Quoted in Nye (1972, p. 7).

commitment to the entities of theories—requires "observation," "direct testing," or "experimental verification."[46] But the history does not, in fact, fully support this picture of confirmation. The overwhelming majority of chemists, and some number of physicists, embraced some form of the atomic theory even before Perrin's experiments. The opponents of atomism did, indeed, demand observation; but such a demand was driven, at least in part, by a parochial philosophy. And, lack of observation aside, they found the atomic theory (in its particular manifestation in kinetics) empirically and practically inadequate. So, although the experimental verification of atomism by Perrin was apparently decisive, it was not universally deemed necessary.

§6 **Against holism and Quine's criterion.** Maddy's criticism of the indispensability argument thus misses the mark.[47] But the history of the atomic theory is no kinder to the Quinean position—although it affords no evidence decisive against Quine's account of confirmation as tied to the five theoretical virtues, it calls into question both his criterion for ontological commitment and his holism. The criterion, of course, is the route from theories to entities. It functions in the indispensability argument to tell us what objects a theory says exist. Holism binds apparently disparate theories into a single conceptual scheme. We draw consequences—including ontological consequences—from the system as a whole, rather than from individual statements or theories. Likewise, we confirm the system as a whole, rather than statement by statement or theory by theory. This prevents us from designating some "ontological" bit of the scheme, some one theory that tells us

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[46] See Maddy (1997, p. 142).

[47] At least *this* criticism does. Maddy (1997, pp. 143-152) also argues—persuasively—that the practice of idealization causes trouble for indispensability arguments.

what there "really" is, and disowning the ontological consequences of other theories. In particular, holism prevents us from favoring the observable sub-theories of our scheme over the unobservable sub-theories (on the grounds that the observable sub-theories are better confirmed), and thus prevents favoring an ontology of observables simply on the grounds that they are observable.[48]

Recall Quine's criterion for ontological commitment: "A theory is committed to those and only those entities to which the bound variables of the theory must be capable of referring in order that the affirmations made in the theory be true" (Quine 1948, pp. 13-14). So, if a theory quantifies over—says there are—tables, then it is committed to tables; if it says there are numbers, it is committed to numbers; if it says there are atoms, it is committed to atoms.

But, fifty years after the genesis of Dalton's theory, both atomists and (some) anti-atomists quantified over atoms in using the atomic notation. Take the chemical reaction scheme

$$2H + O \rightarrow H_2O$$
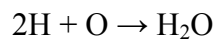
Roughly, this has the quantificational structure of the sentence 'two atoms of hydrogen and one atom of oxygen combine to give one molecule of water, which consists of two atoms of hydrogen and one atom of oxygen.'[49] The atomic notation thus commits to atoms and molecules (here, of hydrogen, oxygen, and water in particular). But *what* are the atoms referred to here? According to A. W. Williamson they are discrete pieces of matter, the smallest possible bits of elemental substances; according to Charles Marignac

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[48] See Quine (1955), especially pp. 252-254. *Cf.* chapters 2 and 3 of van Fraassen (1980).

[49] 'H₂O' is a complex symbol that not only refers to a molecule of water, but gives information about the composition of that molecule.

they are the definite masses of substances that enter into reactions—they are equivalent proportions. (Of course, on the continuum view of matter, *any* definite mass of a substance can enter into the reaction given an appropriate mass of the other substance; talk of equivalents, unlike talk of atoms, only makes sense in the context of a system of relative combining weights. Indeed, even given a system of weights, it is not clear what type of "entity" and equivalent is: it is just a ratio.) Only on Williamson's interpretation does the atomic notation ontologically commit to atoms in any interesting sense.

Now, the anti-atomist proponents of the atomic notation were, partly, engaged in a project of paraphrase; they eliminated (if only tacitly) talk of atoms in favor of talk of equivalent proportions. Quine, of course, explicitly allows for this sort of strategy: eliminating (apparent) quantification over some entities by translating the theory into a form that forgoes the quantification—by replacing it with quantification over different entities, or with some other device (e.g., predication) altogether. Quantification over the offending entities might then be considered a mere notational convenience. But acknowledging that this was, indeed, the strategy of the anti-atomists does not ease the trouble with Quine's criterion. To begin with, the paraphrase of the anti-atomists effects a systematic replacement of 'atom' with 'equivalent proportion'—the quantificational *structure* of the theory is untouched. We thus still need semantic information—that is, we need to know how the theory is to be interpreted—to know that the paraphrase makes an ontological difference.[50] And though some anti-atomists advocated an ontologically frugal interpretation of the atomic notation, this interpretation was not universally adopted. The atomists embraced a richer ontology and saw the atomic notation—at least

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[50] Imagine a paraphrase that replaces each occurrence of 'atom' with an occurrence of '**atom**'. Is there a clear ontological difference between the original theory and the paraphrase?

their uses of it—as embodying ontological claims. Thus any particular use of the atomic notation might commit to atoms, or it might commit only to equivalent proportions.

The notational debate and the ontological debate could be carried on separately, and atomists and anti-atomists alike quantified over atoms: these facts caution against rote applications of Quine's criterion. To draw meaningful conclusions about the ontology of an application of the atomic theory, it seems we need more than just the quantificational structure of the theory—we also need "semantic" information about the term 'atom'. Does 'atom' pick out discrete bits of matter whose properties are responsible for various macro-level chemical phenomena? Or is it simply a device for recording those phenomena, as sort of artefact of chemical accounting? This semantic information is not given on the face of the theory, but only in an interpretation, an account of how the terms of the theory relate to the underlying physical reality.[51]

Maddy (1997, p. 133) characterizes Quine's holism thus: "the confirmation resulting from a successful test adheres not to individual statements but to large bodies of theory." Ultimately, his holism is even stronger than this—it unites, not just statements into large bodies of theory, but bodies of theory into *entire* conceptual schemes. And, according to Quine (1995, p. 252), "Our one serious conceptual scheme is the inclusive, evolving one of science." (We are, of course, to draw our ontology from this serious conceptual scheme.) Just as confirmation attaches to the entire scheme, so does ontological commitment.

The history of the atomic theory undercuts any such claim that science is holistically unified. At the beginning of the 20th century, chemists took themselves to

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[51] Why consider this *semantic* information? Because what we need, I think, is to give an account of the reference property—in the very sense of "reference property" laid out in Chapter 2—*referring to atoms*. See also below, pp. 110 ff.

have very good evidence for the existence of atoms—evidence that staunched all serious debate; and it was impossible at this stage to disentangle chemical theory from atomism. In physics, however, the reality of atoms was still under question; atomism faced serious challenges that were not put down until Perrin confirmed Einstein's predictions. The widespread utility of atoms in chemistry, the chemical evidence for atoms, and the conviction of chemists did nothing to quell the controversy in physics. And the skeptical arguments of the energeticists, the atom-free thermodynamic theories, and the desire in physics for direct evidence of discrete matter did nothing to shake the place of the atom in chemistry.

We have a scientific community that does not behave as it would if holism were correct. Individual scientific disciplines are largely autonomous; they gather and evaluate evidence and issue judgments for the most part independently of each other. Scientific theories are fragmented; theoretical units smaller than whole conceptual schemes can, indeed, be confirmed or disconfirmed on their own.[52] Confirmation and ontological attitude are both parochial: in chemistry, the atomic theory was well-confirmed and an ontology of atoms was unreservedly adopted; in physics, the atomic theory and its ontology were more suspect.[53] It is thus no simple matter to read ontological commitments off our science. Did the science of 1900 commit to atoms? How strongly? Chemistry counseled one attitude, physics another.

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[52] So, for example, the chemical evidence for the existence of atoms did not work to confirm the entire conceptual scheme that incorporated kinetic theory. The debate over thermodynamics proceeded largely independently of the chemical evidence.

[53] No individual seems to have adopted atomism in chemistry but rejected it in physics. Such a position seems logically possible: although a discrete structure of matter accounts for the phenomena of chemical combination, there are purely energetic (rather than kinetic) explanations of thermodynamic phenomena. Ostwald rejected atomism in both fields. But his position put him outside the mainstream in chemistry; in physics, his anti-atomism, although perhaps a minority view, was still mainstream.

§7 From atoms to events. Without the criterion for ontological commitment and holism, Quine's account of how we divine our commitments from science comes apart. The quantificational form of a theory is not a sure guide to its ontology, and our science may not issue an unequivocal verdict on the existence of some entity because science is not univocal. But neither can we tell what things science commits us to by looking only to those existential statements that have been "directly tested," as Maddy claims. Both of these procedures—brute application of indispensability, and strict reliance on observation—miss subtleties in the actual practice of science, subtleties to which any plausible account of ontological commitment and its relation to confirmation must pay heed.

So, after this long detour through the history of chemistry, where are we with respect to semantics? I suggested that proponents may want to defend against the deflationary interpretation of semantic theories offered in Chapter 2 by appealing to indispensability. And to thwart this defense, I've argued that indispensable quantification over a class of entities, even in scientific theories we take to be true, does not necessarily generate metaphysical commitments. This is no special fact about semantics: even the metaphysics of the physical sciences can be difficult to determine.

It's instructive to reflect on exactly how Quine's and Maddy's accounts of the metaphysics of science founder on the atomic theory. In the middle of the 19th century both realists and some anti-realists about atoms adopted the atomic/molecular theory as the best available theory of chemical combination. The realists and anti-realists could agree, for example, that a molecule of water consisted of an atom of oxygen and two atoms of hydrogen, and in contexts concerned primarily with facts about chemical

combination, their pronouncements are indiscernible. The anti-realists, no less than the realists, took such statements to be *true* in these contexts. They disagreed only over the *metaphysics* of the theory: yes, water is H_2O, but do atoms and molecules really exist? Is the atomic molecular theory a true theory only about chemical combination, or does it tell us more—does it tell us about the metaphysical structure of matter? These questions were ultimately resolved in favor of the existence of atoms. But the resolution had nothing to do with the mere occurrence of atom-talk in the theory. Rather, atoms and molecules came to fill explanatory roles—not demanded by the initial theory of chemical combination—that they could not fill *unless* they existed.

Compare this account to the strategy I pursued in Chapter 2 against the direct argument. There I argued that there are (at least) two ways to interpret the theoretical terms of our semantic theories, and that the two interpretations diverge in the metaphysical commitments with which they endow the theories. According to the first interpretation, preferred by the proponent of the direct argument, reference is uniformly Fregean; terms refer to existing entities, or they refer to nothing at all. On this interpretation, semantic theories have a great many metaphysical consequences, some quite surprising: fictional characters really exist, you reading this paper involves more things than just you and the paper, *etc.* If this first interpretation is right, compositional semantic theories aren't just theories of semantic competence; they are, in some sense, theories of *everything*. According to the second interpretation of semantic theories, reference is does not have a uniform metaphysical nature: in some instances it is Fregean, in others, not. Some terms refer to existing entities, some terms refer to non-existent entities. And the semantic theory itself doesn't resolve which reference

properties are Fregean and which aren't, since the explanatory demands of the theory don't require that Harry Potter (for example) exists. On this deflationary interpretation, the metaphysical consequences of semantics are rather thin, amounting to nothing much beyond the commonsense judgments recorded in the utterances taken as data. If this second interpretation is right, compositional semantic theories are simply theories of semantic competence, not also theories of the wider world.

Since both interpretations are compatible with our semantic theories, we must look elsewhere to decide which to adopt. The deflationary interpretation is, of course, more parsimonious. (Though we pay for that parsimony with the complications to reference and valuation.) But more importantly, it allows us to avoid a clash between semantics and other information we have about the world—*e.g.*, that Harry Potter doesn't exist.

Even once we've adopted the deflationary interpretation, however, there's still interpretive work to be done. Since reference and valuation properties are multifarious, we face the task of giving an individual account of the nature of each. And the compositional semantics itself doesn't tell us what these natures are. Instead, we must investigate what wider explanatory roles Harry Potter, Harry Truman, events, *etc.* play, and whether or not those roles require the things filling them to exist.

And so: linguistic semantic theories are, indeed, scientific theories like any other. And like other theories, they do not openly display their metaphysical consequences.

References[54]

Bancroft, Wilder D. 1911. Review of *Die Brown'sche Bewegung und die wahre Existenz der Moleküle*, in *Journal of Physical Chemistry* 15, 708.

Berthelot, Marcellin. 1877. "On Systems of Chemical Notation," letter to Charles Marignac, in *Moniteur Scientifique*, December; translated in Nye (1984), 243-246.

Bevan, P. V. 1904. "The Combination of Hydrogen and Chlorine under the Influence of Light," in *Philosophical Transactions of the Royal Society of London. Series A, Containing Papers of a Mathematical or Physical Character* 202, 71-121.

Brodie, Benjamin. 1866. "The Calculus of Chemical Operations; being a Method for the Investigation, by means of Symbols, of the Laws of the Distribution of Weight in Chemical Change.—Part I. On the Construction of Chemical Symbols," in *Philosophical Transactions of the Royal Society of London* 156, 781-859.

_____. 1869. "Discussion on Dr. Williamson's Lecture on the Atomic Theory, November 4th, 1869," in *Journal of the Chemical Society* 22, 433-441; reprinted in Nye (1984), 141-149.

Cannizzaro, Stanislao. 1858. "Sunto di Un Corso Filosofia Chimica," *Il Nuovo Cimento*, 7, 321-366; excerpted and translated in Leicester and Klickstein (1952).

Clarke, F. W. 1903. "The Atomic Theory," in *Science* 18, 513-529.

Dalton, John. 1808. *A New System of Chemical Philosophy. Part I* (London: R. Bickerstaff).

Duhem, Pierre. 1892. "Notation atomique et hypothéses atomistiques," in *Revue des questions scientifiques* 42, 395-454.

_____. 1900. "The Work of J. H. van't Hoff, in Connection with a Recent Book," in Duhem (2002), 277-290.

_____. 1902. "Mixture and Chemical Combination: An Essay on the Evolution of an Idea," in Duhem (2002), 2-118.

_____. 1954. *The Aim and Structure of Physical Theory*, trans. Philip P. Wiener (Princeton: Princeton University Press).

_____. 1991. *German Science*, ed. and trans. John Lyon (La Salle, IL: Open Court).

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[54] Where a translation, reprint, or excerpt is indicated, all references in the text are thereto.

_____. 2002. *Mixture and Chemical Combination and Related Essays*, ed. and trans. Paul Needham (Boston: Kluwer Academic Publishers).

Einstein, Albert. 1905. "Über die von der molekular-kinetischen Theorie der Wärme Bewgungen von in ruhenden Flüssigkeiten suspendierten Teilchen," in *Annalen der Physik* 17, 549-560.

_____. 1906a. "Theorie der Brownische Bewegung," in *Annalen der Physik* 19, 371-381.

_____. 1906b. "Eine neue Bestimmung der Molekul-dimensionen," in *Annalen der Physik* 19, 289-306.

_____. 1949. "Autobiographical Notes," in *Albert Einstein: Philosopher-Scientist*, 3rd edn., ed. Paul Arthur Schilpp (La Salle, IL: Open Court, 1970).

Field, Hartry. 1980. *Science without Numbers* (Princeton, NJ: Princeton University Press).

Higgins, William, 1789. *A Comparative View of the Phlogistic and Antiphlogistic Theories* (London: J. Murray).

Ihde, Aaron J. 1964. *The Development of Modern Chemistry* (New York: Harper and Row, Publishers).

Klason, J. P. 1906. "The Nobel Prize in Physics 1906," http://www.nobel.se/physics/laureates/1906/press.html.

Leicester, Henry M. and Herbert S. Klickstein (eds.). 1952. *A Source Book in Chemistry, 1400-1900* (Cambridge, Mass.: Harvard University Press).

Ludlow, Peter. 1999. *Semantics, Tense, and Time: An Essay in the Metaphysics of Natural Language* (Cambridge, Mass.: The MIT Press).

Marignac, Jean Charles. 1877. "Chemical Equivalents and Atomic Weights considered as bases of a system of Notation," in *Moniteur Scientifique*, September; translated in Nye (1984), 230-242.

Maddy, Penelope. 1997. *Naturalism in Mathematics* (Oxford: Clarendon Press).

Noyes, William. 1909. "Molecular Rearrangements," in *Journal of the American Chemical Society* 31 (12), 1368-1374.

Nye, Mary Jo. 1972. *Molecular Reality: A Perspective on the Scientific Work of Jean Perrin* (New York: American Elsevier Inc).

_____ (ed.). 1984. *The Question of the Atom: From the Karlsruhe Congress to the First Solvay Conference, 1860-1911* (Los Angeles: Tomash Publishers).

Odhner, C. T. 1901. "The Nobel Prize in Chemistry 1901," http://www.nobel.se/ chemistry/laureates/1901/press.html.

Ostwald, Wilhelm. 1896. "Emancipation from Scientific Materialism," in *Science Progress: A Quarterly Review of Scientific Investigation* 4, 419-436; reprinted in Nye (1984), 337-354.

Parsons, Terence. 1979. "Referring to Nonexistent Objects," *Theory and Decision* 11, 95 – 110.

Pasteur, Louis. 1861. "Researches on the Molecular Asymmetry of Natural Organic Products by Louis Pasteur, (1860)," *Alembic Club Reprints*, No. 14 (Edinburgh: Alembic Club, 1905); excerpted in Leicester and Klickstein (1952), 375-379.

Perrin, Jean. 1910. *Brownian Movement and Molecular Reality*, trans. Frederick Soddy (London: Taylor and Francis).

_____. 1913. *Atoms*, trans. D. Ll. Hammick (Woodbridge, CT: Ox Bow Press, 1990).

Quine, W. V. 1948. "On what there is," in *From a Logical Point of View*, 2nd edn. rev. (Cambridge, MA: Harvard University Press, 1980), 1-19.

_____. 1955. "Posits and Reality," in *The Ways of Paradox and other Essays*, rev. edn. (Cambridge, MA: Harvard University Press, 1976), 246-254.

Ramberg, Peter J., 2003. *Chemical Structure, Spatial Arrangement: The Early History of Stereochemistry, 1874-1914* (Burlilngton, VT: Ashgate Publishing Company).

Richter, Jeremias Benjamin. 1792. "Anfangsgründe der Stöchyometrie oder Messkunst chymischer Elements;" excerpted and translated in Leicester and Klickstein (1952), 206-208.

Rutherford, Ernest. 1905. "Bakerian Lecture: The Succession of Change in Radioactive Bodies," in *Philosophical Transactions of the Royal Society of London. Series A, Containing Papers of a Mathematical or Physical Character* 204, 169-219.

Solov'ev, Yu I. and L. P. Petrov. 1968. "Russian Scientists on Dalton's Atomic Theory," in *John Dalton and the Progress of Science*, ed. D. S. L. Cardwell (New York: Manchester University Press, 1968), 300-308.

Thiele, Johannes. 1899. "Zur Kenntniss der ungesättigten Verbindungen," in *Annalen der Chemie* 306, 87-142; translated and excerpted in Leicester and Klickstein (1952), 510-511.

Thomson, J. J. 1904. "On the Structure of the Atom: An Investigation of the Stability and Periods of Oscillation of a Number of Corpuscles Arranged at Equal Intervals around the Circumference of a Circle; with Application of the Results to the Theory of Atomic Structure," in *Philosophical Magazine* 7, 237-265.

Thomson, Thomas. 1807. *System of Chemistry*, 3rd edn., vol. III (Edinburgh: Bell and Bradfute, and E. Balfour).

Thomson, William. 1867. "On Vortex Atoms," in *Proceedings of the Royal Society of Edinburgh* 6, 94-105; reprinted in Nye (1984), 91-100.

Van Fraassen, Bas C. 1980. *The Scientific Image* (Oxford: Clarendon Press).

Van't Hoff, Jacobus. 1874. "Voorstel tot Uitbreiding der Structuur-Formules in de Ruimte;" translated and excerpted in Leiceseter and Klickstein (1952), 445-453.

_____. 1898. *The Arrangement of Atoms in Space*, 2nd edn., trans. and ed. Arnold Eiloart (New York: Longmans, Green, and Co.); reprinted as *The Development of Modern Chemistry 1789-1914*, vol. X, ed. David Knight (New York: Routledge, 1998).

Washburn, E. W. 1911. Review of *Die Brown'sche Bewegung und die wahre Existenz der Moleküle*, in *Journal of the American Chemical Society* 33, 601-603.

Werner, Alfred. 1893. "Beitrag zur Konstitution anorganischer Verbindungen," in *Zeitschrift fur Anorganischer Chemie* 3, 267-330; translated and excertpted in Leicester and Klickstein (1952), 516-520.

_____. 1898. "Stereochemical Isomerism of Inorganic Compounds," appendix to van't Hoff (1898), 185-199.

Williamson, A. W. 1869. "On the Atomic Theory," in *Journal of the Chemical Society* 22, 328-365; reprinted in Nye (1984), 103-140.

Wurtz, Adolphe. 1860. "Comptes rendu des seances du Congres international des chimistes reuni a Carlsruhe le 3, 4 et 5 septembre 1860;" translated in Nye (1984), 5-28.

_____. 1898. *The Atomic Theory*, 7th edn., trans. E. Cleminshaw (London: Kegan Paul, Trench, Trübner and Co., Ltd.); reprinted as *The Development of Modern Chemistry 1789-1914*, vol. II, ed. David Knight (New York: Routledge, 1998).