

Response

HERMAN CAPPELEN AND ERNEST LEPORE

1. Introductory Remarks

Reading these excellent commentaries we already wish we had written another book—a more comprehensive, clearer, and better defended one than what we have. We are, however, quite fond of the book we ended up with, and so we've decided that, rather than to yield, we'll clarify. These contributions have helped us do that, and for that we are grateful to our critics.

We're lucky in that many (so far about twenty)¹ extremely able philosophers have read and commented on our work in print. A slightly discouraging fact is that *all* these commentators seem to think we are completely, utterly mistaken. On the positive side: Our critics seem to disagree about *what* we're completely wrong about. On the one hand, radical contextualists (e.g. Travis) find our objections against them off the mark, but our objections to moderate contextualism dead-on. On the other hand, the moderate contextualists (e.g. Szabo) think that our objections against them fail, but our objections to radical contextualism are strong (Szabo, concludes that we 'present strong arguments against radical contextualism, but only a weak case against moderate contextualism'). This means we've got our work cut out for us—defending the middle ground from every which way—something we are more than pleased to do.

We start with general points of clarification, points it will be useful to reference from time to time when discussing each commentary. (General Comment #4 is the most important, and we will make reference to it repeatedly in what follows.)

General Comment #1: Our View isn't Radical

Some philosophers, braver than we are, set out to defend extremely surprising conclusions (like the epistemic view of vagueness or realism about possible worlds). They are prepared to defend such views against what Lewis called the 'incredulous

Address for correspondence: Center for Cognitive Science, Rutgers University, New Brunswick, Psych. Bldg. Addition, Busch Campus, 152 Frelinghuysen Road, Piscataway, NJ 08854-8020, USA; Herman Cappelen, Philosophy Department, University of Oslo, Niels Henrik Abels v36, 0313, Oslo, Norway.

Email: Lepore@rucss.rutgers.edu; h.w.cappelen@filosofi.uio.no

¹ 'Reply to Critics', *Philosophy and Phenomenological Research* (2006), and 'Reply to Critics', *ProtoSociology* (2006).

stare'. Some, like Lewis and Williamson, do so with great success. The way some of our critics characterize our book makes it seem as if we set out to do something similar (but less successfully): defend an extreme view of 'ready', 'enough', comparative adjectives or context sensitivity more generally. That is a misreading of *Insensitive Semantics* (*IS*). Our goal is not to defend or push an extreme view of any particular class of expression, much less of context sensitivity in general. *IS* is, instead, about methodology in semantics. What we care about, throughout, is the interplay between certain basic methodological assumptions. One we call MA. It concerns the connection between semantic content and speech act content—we disconnect these in a way that much traditional work in semantics does not (our work here is along the lines of Salmon, 2005). A related principle concerns the use of a kind of thought experiment (what we call context shifting arguments), and their significance. (If you reject MA, these arguments do not on their own support any semantic conclusion.) A third methodological claim is that semanticists interested in context sensitivity need to accommodate a rather wide range of data about content sharing across contexts. (These concern the indirect reporting tests from Chapter 7.) Viewed this way, it is hard to characterize any central claim in *IS* as 'extreme' or even as worthy of an incredulous stare.

General Comment #2: Tests as Evidence

In Chapter 7 of *IS* we discuss various tests for context sensitivity. We never claim passing these tests is either necessary or sufficient for context sensitivity: We describe the tests as providing 'evidence' that sentences or expressions are context sensitive (see, for example, p. 89, where we introduce the first test);² 'the challenge', we say, 'is to explain away or challenge this data (or show that the test is no good)' (p. 96; also p. 99).

There were four reasons for denying that the tests provide necessary or sufficient conditions for context sensitivity:

1. The tests appeal to intuitions and intuitions are susceptible to all kinds of biases.
2. It is bad methodology to hold that one source of evidence overrides all other and larger theoretical considerations.
3. There are other related tests we don't take up in this reply—absent a complete gathering of the results of these nothing should have the status of a necessary or a sufficient condition. (See our reply to Hawthorne (2006a).)
4. Finally, there is the possibility that the tests deliver conflicting results. We know of no a priori proof this cannot happen. (Hawthorne (2006) argues that it does.)

² Pagination is to *Insensitive Semantics* unless otherwise noted.

We're not the only ones who exploit these tests; see, for example, Hawthorne (2004), Richard (2004), MacFarlane (2005), Egan, Hawthorne and Weatherston (2005), Stanley (2005), Lasersohn (2005) and Almog (2006), among others. Like us, none of these authors takes these tests to be necessary or sufficient—they see them as providing important considerations, data any adequate theory must explain.

General Comment #3: Tests and Relevantly Different Contexts

Here is a formulation of two of the tests we invoke:

Test #1: Inter-Contextual Disquotational Indirect Report: Take an utterance u of $S(e)$ ³ in context C by speaker A . If, in a relevantly different context C' , we can indirectly report u with 'A said that $S(e)$ ', then that's evidence e is semantically stable, i.e. that its semantic value doesn't vary across contexts of utterance.

Test #2: Collective Inter-Contextual Disquotational Indirect Reports: Take several utterances, u_1 – u_n , of $S(e)$ in contexts C_1 – C_n , all of which are relevantly different. If, in a C' , we can indirectly report u_1 – u_n with 'They said that $S(e)$ ' (where 'they' refers to the speakers of u_1 – u_n), then that's evidence e is semantically stable, i.e. that its semantic value doesn't vary between contexts of utterance.

Note that both tests appeal to the notion of *relevantly different contexts*. We don't take a stand on what counts as relevantly different (that's for a proponent of semantic variability to say). Our strategy is: if someone tells us e is semantically unstable and that its semantic value varies with features F_1 – F_n , then we run the tests with F_1 – F_n as the relevantly different features. What this means is that in order to use these kinds of tests, you must first make sure that the contexts you invoke are *relevantly different*, i.e. that they vary with respect to F_1 – F_n . If, for example, you want to test whether 'I' is context sensitive, you have to make sure that you don't have the same speaker in the imagined contexts; if you test 'that', make sure the same object isn't demonstrated; if you test the claim that 'know' is sensitive to contextually salient standards, make sure those are different. The easiest way to run these tests and guarantee relevant differences is to exploit what we call 'reports under ignorance' (p. 93). These are reports where the reporter is ignorant of the relevant features of the reportee's context, and therefore, the truth of the indirect report cannot be underwritten by any presumed overlap in such features.

General Comment #4: Test and Reply #4

In Reply #4 (p. 99), we consider a reply to our first test (the disquotational indirect reporting test); we wish we had elaborated on our response since the point is

³ Where ' $S(e)$ ' stands for a sentence S with an expression e in it.

overlooked almost invariably. We imagine someone reacting as follows to our appeal to the disquotational indirect reporting test:

Whether or not such indirect reports are true depends on the context of the report. In some contexts, a disquotational report of an utterance *u* might be acceptable, but in other contexts it might not. It all depends on the context of the report.

We claim, for example, that there are relevantly different contexts *C* and *C'* such that an utterance of 'A knows that *p*' by Ted in *C* can be reported by 'Ted said that A knows that *p*' in *C'*. We also claim (along with Richard (2004)), that an utterance of 'Naomi is rich' by A in *C* can be reported by 'A said that Naomi is rich' in a relevantly different context *C'*. We take this to be evidence that 'know' and 'rich' have a level of content that's context insensitive (as do others who use this test; see references above). What we're imagining in Reply #4 is a response along the following lines:

But I can imagine a relevantly different context *C''* in which that report *would* seem inappropriate, i.e. a context *C''* in which it would be unacceptable to report the original utterance with 'Ted said that A knows that *p*' or 'A said that Naomi is rich'.

Suppose such contexts exist (our speech act pluralism predicts they do). What are we to conclude? In some relevantly different contexts, e.g. *C'*, the disquotational indirect report is acceptable, while in others, such as *C''*, it is not. Do we then have an inconclusive result? We think not. Here's what we say in *IS* (p. 99):

This might be true (we argued for a view like this (1997) and discuss it further in Chapter 13), but even if it is, it has no bearing on the present issue. All we are claiming is that Anne said that the apple was red. If what we said by the previous sentence is true, and it is, then that settles the matter. Suppose there are contexts in which 'Anne said that the apple was red' are false (we are not saying there are such contexts, but suppose). All we need is that it is true that Anne said that the apple was red. That's it.

In other words, if a disquotational indirect report is true (in a relevantly different context *C'*), then you have evidence that the expression in question has a stable semantic value. You have something that needs to be explained if you want to hold that the sentence in question is context sensitive. Even if it's your intuition that in some other context *C''* such a report is not true (or inappropriate), that doesn't explain the true report in *C'*. Nor does it undermine the truth of the report in *C'*. At most what it establishes is that 'says' is context sensitive (or, as we argue in Chapter 13, that speech act pluralism is true).

Here's a perceptual analogy to help clarify how we see the situation: imagine A observing two objects O1 and O2 from perspective P. From P it appears that O1 and O2 are red, hence the observer thinks and says, 'Both O1 and O2 are red'. Now imagine B observing O1 and O2 from another perspective, P*. From P*, O1's redness isn't salient. The observer thinks and says: 'O2 is red, but O1 isn't'. We should not conclude from this that the description made from P is false. It is (minimally) in need of an explanation: It is data a theory has to take into account. The same goes for conflicting descriptions of speech acts. Suppose from one context of interpretation, CI1, it appears to A that both Naomi's and Mary's utterances, u1 and u2 of sentence S, said that p. Suppose that from a different context of interpretation, CI2, it doesn't appear that u1 and u2 both say that p. We should not infer that u1 and u2 didn't both say that p. We should not conclude from this that the description made from CI1 false. It is (minimally) in need of an explanation: it is data a theory has to take into account. Our central claim in Chapter 7 is that contextualists lack an explanation of our intuitions about shared content. No such explanation is given by drawing our attention to contexts where the shared content is not salient. Of course, we owe an explanation of what goes on in these other contexts (that's what our speech act pluralism does – see also Cappelen & Lepore, 2006b for elaboration).

Below we will refer to such alleged counter examples to our tests as BP-Examples (Besides the Point Examples). Travis, Bezuidenhout, and Szabo all appeal to such examples and in response we will refer back to this comment. We should mention that though our response to Reply #4 (p. 99) still strikes us as sound, we wish someone had addressed it directly since it raises complex issues that limitations of space prevent us from exploring in this reply.

Before we move on to the replies we would like to point out the importance of one particular passage in what follows: The last paragraph of S#3 includes an important elaboration on the tests in Chapter Seven *not* include in IS. It is a point we wish we had made clearer in *IS*—we elaborate on it in Cappelen and Lepore, 2006b.

2. Reply to Charles Travis

It should be obvious to readers of *Insensitive Semantics* that we have an affinity for Travis' position. Since his early work he has argued vigorously, originally, and influentially for one of the central contentions in our book: namely, that the kinds of arguments that support moderate contextualism (i.e. the kinds of arguments that make philosophers think that 'know' (Derose), 'might' (Lewis), 'good' (Drier), 'reindeer' (Stanley), 'red' (Szabo), 'rain' (Perry), etc. are context sensitive) generalize. If you think those kinds of arguments (we call them context shifting arguments and incompleteness arguments) are sufficient for establishing that an expression is context sensitive, then you're led to radical contextualism, i.e. to the view that all expressions are context sensitive. Travis' work is filled with ingenious examples to

this effect. The way we put this: Moderate contextualism is an unstable position. There is no stopping the slide from moderate to radical contextualism. (This is the point where our central disagreement lies with Szabo. See below.)

That said, we continue to think that:

- Travis' positive view fails to take into account important data (the kind of data elicited by the tests we outline in Chapter 7).
- Travis' positive view fails to explain how speakers can share content across contexts—how speakers differently situated can make the same claim, the same assertion, and investigate the same idea.
- Travis' positive view is internally inconsistent.

So, as things stand, the world in which we endorse radical contextualism is pretty far away, and Travis' reply doesn't render it any closer. We address his reply in five steps.

T#1: Central Disagreement with Travis: Neat Answers

Travis says:

Suppose one asked, 'Just what is this thing, being coloured blue? What way for a thing to be is that?' A neat answer might be: 'It is that way which a thing is iff C', where 'C' stated a condition which was met by (and only by) some determinate class of things.

Travis says no neat answer is available. There's no substitute for 'C' that would make that formula true. This is a point of disagreement because we think there is a reply, a very simple one, namely:

(NA1) Being blue is that way which a thing is iff it is blue.

(NA2) Being blue on the outside is that way which a thing is iff it is blue on the outside.

or in our preferred terminology:

(NA3) An object satisfies 'x is blue' iff it is blue.

(NA4) An object satisfies 'x is blue on the outside' iff it is blue on the outside.

Not particularly informative, but who can deny their truth? Here we would like to add a ground rule to those that Travis introduces: To refute NA1 and NA3 you need to convince us that the bi-conditionals are false. How would you do that? You need to find an object that satisfies 'being blue' but isn't blue (or the other way around). The same goes for NA2 and NA4: you need to find an object that satisfies 'x is blue on the outside' but isn't blue on the outside. We have seen no such counter-example from Travis. That is the *only* way to refute NA1–4.

It is useful here to consider Travis' claim that any neat solution would be reductionist. A neat answer, he says, would amount to:

... a sort of reduction of being blue, or being a grunter, to something else ... I see no more hope for such a reduction than there is generally for reductions of anything to anything else. Read that way, Bishop Butler had the right idea.

We don't think Travis is on the side of Bishop Butler here. It is Travis who assumes a neat answer would have to be 'a sort of reduction'. He assumes *that* because he doesn't even consider NA1–4 as possible; he doesn't take seriously the possibility that we can use the term in question to specify the conditions under which that term can be applied. That is what a non-reductionist solution would amount to—it would not be one that tried to specify C without using the terms 'blue' or 'blue on the outside'.

In the final part of his comments, Travis considers our non-reductionist answer and rejects it because we don't tell him what it is to be blue (or blue on the outside or a grunter). Our only strategy, he says, is 'studied silence':

They utter certain forms of words—'The sentence 'Sid grunts' expresses the proposition that Sid grunts, which is true iff Sid grunts' will do as illustration. But they emphatically refuse any explanation as to what it is they thus said. What they pretend to have said is what proposition 'Sid grunts' expresses, and when it would be true. But if one feels left in the dark by that, they have no help to offer.

Warning: No semanticist should come to Travis' aid here. We'll borrow a formulation of Travis' (he uses it as a response to a move he imagines us making):

... [our] only answer is that if you are one of us (say, a speaker of some language enough like English), then that is something you are prepared to recognize. If you are not, then [we] have no formula—as it were, a cognitive prosthetic—to offer you with which to replace such competence.

That is, if Travis claims not to understand 'blue' or 'blue on the outside', no one can help him. That's something we each know *qua* English speakers. Ask yourself: do you really think you don't understand, for example, the use of 'blue on the outside' in NA2 and NA4? Isn't it simply a part of being competent in our language that you grasp such fundamental truths? Note: understanding, of course, does not require recognition.

At this point we're afraid our readers might get the sense that what we have reached a sort of stand off. We claim to understand 'blue on the outside' in NA2 and NA4, Travis claims not to (or if he understands it, then only 'in a certain way' (which would make the formula false)). Is there a way to resolve our apparent

stand off? We suspect Travis' strategy will be to construct a range of cases that exhibit different understandings of 'blue on the outside'. We, in response, will insist that such stories do not constitute counter-examples to NA2/4 *unless* they are cases where 'blue on the outside' clearly applies, even though the object in question isn't blue on the outside (or the other way around). We've seen no such case, but we're open-minded.

T#2: On How to Understand Travis' Use of the Predicate 'Being Blue'

Before we leave the above topic, we'll briefly point out a potentially problematic feature of Travis' view (though this might just be our not fully understanding what he has in mind). Here's a kind of sentence that can be found throughout Travis' reply:

(T) 'is blue' speaks of being (coloured) blue.

How are we to understand the words 'being (coloured) blue' in that sentence? We know that Travis thinks (T) is meaningful (if it isn't, then huge chunks of his reply aren't meaningful). So 'being (coloured) blue' must have a semantic value (understood as loosely as you like). If we assume that (T) expresses a proposition, 'being (coloured) blue' must contribute something to that proposition. Call what it contributes a property, and call this property BCB. But then for (T) to be meaningful there must be a property, BCB, that all blue things instantiate. Now, we know that Travis doesn't think this, so there will be some way he thinks that expression can contribute meaningfully to his text without assuming that being (coloured) blue is a property. We would be curious to understand how that's supposed to work.

T#3: Travis on Communication

Our most fundamental disagreement with Travis is over what it is for people to understand each other. We claim radical contextualists are stuck with a kind of 'contextual content solipsism' where contents cannot be carried from one context of utterance to another. We claim radical contextualism implies that the same proposition can't be expressed by people differently situated. If Travis were right, it shouldn't, for example, be possible for us to say what Travis said, and Travis shouldn't be able to say what we said.

He describes our view as follows:

What communication requires, C&L suppose, is that from the fact that Max said, on some occasion, 'Sid grunts', of Sid (at time *t*), those words meaning in Max's mouth what they do mean, one can extract what it is that Max thus said to be so.

If this means that in order to communicate, an audience must (typically) understand *what the speaker said*, this certainly is a view we endorse. We endorse a number of other views we find fairly obvious, like the following:

- It is possible for you, our reader, to understand what we say and assert in this reply.
- It is possible for you to agree or disagree with what we said and assert in this reply.
- It is possible for you, wherever you may be, to tell someone else what we said in this reply.

It is not true that the best you can do is get close to what we say in this reply, or retrieve a thought merely similar to what we say in this reply. You can actually understand what we say. This might be a wild and radical view, but it's our view. It appears not to be Travis'.

Travis' positive view of communication is not presented in this reply, but we get a flavor of it from the following passage:

On the [radical contextualism] view, though, communication just isn't like that. True, Max's words are worth little to me if I know *nothing* of the circumstances in which he spoke. But knowing *something* of them may be enough to know something of what is to be expected if things are as he said. Which may, if things go well, make them quite useful to me.

Accordingly, we can never grasp exactly what another says; we can only muster something similar; something that'll do for whatever purpose we're up to. We are then to reject the model of successful communication as requiring sharing thoughts; rather, successful communication is just getting along in the right kind of way.

In *IS*, we advance a battery of arguments for why we abhor this way of construing communication. Our forthcoming book, *Shared Content and Semantic Spin*, is devoted solely to this important topic. Limitations of space prohibit a full presentation of our arguments (along with the different moves a radical contextualist might invoke in order to mitigate contextual content solipsism), but here are some important considerations.

It is essential to our linguistic practices that others can say what we say:

- *Responsibility*: we hold people responsible for what they said, not for what they almost or 'sort of' or partly said.
- *Reasons for Actions*: what other people say can provide reasons for actions and enter into our practical reasoning – what they almost said or something similar to (or part of) what they said can't play this role.
- *Justifying knowledge*: what others say often justifies our beliefs and provides the basis for knowledge—something relevantly similar to (or part of) what they said can't serve this same function.

- *Coordinated Action*: language can be used to tell many people what it is they all need to do in order to achieve some common goal—this requires the participants to receive and to follow identical instructions, not merely similar ones.
- *Intra-Personal Deliberation*: as we deliberate over time, we presuppose that contents are preserved from one context of deliberation to another.

We find it peculiar that a theorist who professes to be so devoted to close attention to linguistic practices (in the sense that Travis is Wittgensteinian) is willing to relinquish these most foundational features of our communicative and social practices; in particular, we find it shocking that Travis is willing to reject that component of our self-understanding as linguistic creatures that requires that we are capable of understanding each other across contexts. Of course, it's possible that Travis has a reply to all such worries. We'd like to see it.

T#4: Travis on MA

One issue should be quite easy to clear up: We claim that Travis, with other radical contextualists, endorses a version of the thesis we call (MA) (see Travis' contribution and *IS*, pp. 53–58). About this 'accusation', Travis says: 'Incredibly, C&L accuse me specifically of holding MA. I cannot imagine why.' In the very next sentence he goes on to imagine why, and he gets it basically right:

Perhaps it is because I think that if a *sentence* were true under such-and-such conditions, and you spoke it where those conditions obtained, you would say something true. That sounds about right. But of course I do not think that, in that sense, any sentence does have a truth condition.

The sense in which Travis endorses MA is this: a central claim throughout his work is that a semantic theory, understood as a theory about the properties of sentences (the abstract objects) will not generate something that can be true or false. Nor is there anything general that can be said about how a sentence in a context ends up saying what it says (semanticists won't be able to articulate the function that takes us from a sentence and a context of utterance to the proposition expressed by that utterance). Semantics, according to Travis, cannot deliver truth conditions. Travis uses that conclusion against, for example, Davidson's truth conditional theory of meaning, Grice's account of what is said, and what he calls the Carnapian and Montagovian tradition in philosophy of language. It is clear, therefore, that he thinks of this as a criticism of these semantic projects. Were they to be successful, they should deliver something truth-evaluable. If Travis didn't think this, his argument (conclusion) sketched above, wouldn't constitute a criticism. Hence, we place him as endorsing MA (and of course, that's perfectly compatible with his saying, 'I do not think that, in that sense, any sentence does have a truth condition').

T#5: Travis on Tests for Context Sensitivity

In §4 of his reply, Travis attempts to show that all the tests we invoke as objections to radical contextualism fail. We assume limitations of space must have prevented Travis from elaborating, but as they stand all of his arguments are instances of BP-Examples (discussed in General Comments #4 above). Like Recanati (below), he might object to our reply on p. 99, but the debate cannot advance unless we're told what the objection might be.

3. Reply to Zoltan Szabo**#1: Szabo's Reservations about the Slippery Slope from Moderate Contextualism to Radical Contextualism**

One of Szabo's central objections is his 'reservations about the alleged slide from moderate to radical contextualism'. First, some background: the argument Szabo expresses doubt about is essential both to the critical part of our book and to its positive part. Our argument against what we call *moderate contextualism* depends on the assumption that it collapses into radical contextualism. Our positive view depends on the assumption that for any utterance, we can trigger the intuition that many different propositions are said (this is at the core of our speech act pluralism). So Szabo is attacking one of the key assumptions in *IS*—if his 'reservations' are well justified, most of what we have to say collapses.

To establish these claims, we rely on a tradition in the philosophy of language dating back at least as far as Wittgenstein, continuing through Austin and the ordinary language movement of the 50's and 60's, and is very much alive today in a range of positions that can be roughly labeled as 'neo-Wittgensteinian', including Searle, Sperber/Wilson, Travis, Recanati, Carston, etc. (See Chapter 1 of Carston, 2002 and Travis, 1985; 1989; 1996, and further references in Chapter 2 of *IS*.) These writers are also among our central targets in *IS* because we disagree with their conclusion, namely, that the kinds of data they focus on show semantics to be impossible. What we don't dispute is their data—in fact, we take it to be important and correctly described (in that sense our sympathies are much more aligned with the radical than with the moderate contextualists). Where we balk is with the claim that this data imply the radical (semantic) conclusions these authors draw.

We mention this to point out that our case for the collapse of moderate into radical contextualism is *not* meant to be original—we rely largely on examples (and procedures for constructing examples) used by others, though in Chapter 13 we supply various further examples, not used by others. This long tradition is itself an important chunk of empirical data: it shows that the intuitions we appeal to are widely shared, over a rather long period of time, by a wide range of philosophers. Our goal, much like Grice's, is to show how a theory could accommodate both the intuitions of widespread context sensitivity and the view that there is such a thing as a stable, compositional semantic content. We tried to achieve that goal by

sharply distinguishing semantic content from speech act content (what speakers say, assert, claim, etc.).

An obvious feature of the examples, intended to demonstrate the slide from moderate to radical contextualism, is that they all require space: they involve rather elaborate stage setting. You need to get the theorist to imagine she is in a certain kind of setting with certain background assumptions and then she projects herself into those settings and recovers intuitions about what's said. Here we concur with Travis, who, in a related context, says: 'stories which show what we really would say under given circumstances tend to be space consuming, and even at that, more suggestive than specifying' (1985, p. 198).⁴

Szabo quickly runs through a number of cases in his reply. Each of his comments merits more discussion than we can provide here. Instead we'll first reply briefly to some of Szabo's comments (without much argument). For those we ignore, it will be obvious how to extend our discussion. We then present two additional context shifting arguments of the kind we discuss in Chapter 3. We do this to show that even if Szabo were correct about his 'reservations' about the specific examples we appeal to, others can be constructed that make the same point.

Szabo claims that three of the examples we use are instances of ambiguity—if he were right, that would make them useless for our purposes. But he's not right. What he counts as ambiguity is variability in what is said based on a core meaning. On the view we are recommending, there's a core meaning of 'philosopher', and it can, for example, be used to talk about people who are philosophical and people in a certain profession. From our point of view, it is question begging to assume that this is an instance of ambiguity. The same goes for 'have': we claim there is a core meaning and that it can be used to talk about what people were eating. Szabo's appeal to dictionaries is inconclusive because they are not in the business of making the kinds of theoretical distinctions that concern us in this part of *IS*.

The real problem, however, is not who is right or wrong about ambiguity; even if Szabo were right in *all* his reservations about our examples, it is easy to generate new ones of the kind we rely on for any case that he devises. *Nothing* in our argument depends on specific claims about which expressions are ambiguous. Our strategy is this: when we encounter a sophisticated and ingenious moderate contextualist, such as Szabo, we ask him for an expression he is confident is not ambiguous (or a disambiguation of an ambiguous expression); we then run our examples on that expression (or on that meaning). Here are two such illustrations:

CASE 1: Suppose for the purposes of discussion that 'philosopher' is ambiguous. Take the non-professional sense (Szabo seems to grant there is one). Here is a context shifting argument for that sense of 'philosopher': Take a group of professional philosophers (so there's no doubt that they are

⁴ For further examples, see Travis' 'grunt' example, this volume and instructions for how to generate these kinds of examples in Travis (1989).

philosophers in Szabo's 'professional' sense); we're all familiar with sincere utterances of the form:

Naomi is not a philosopher; she works in a philosophy department, but she is not a philosopher,

(said of a colleague specializing in, say, set theory) where what we mean to communicate is that she doesn't engage in the kind of activity the speaker in that context takes to be philosophical; what kind of activity that is will vary from one context to another. So someone else, with other philosophical values (and in a context where those values are salient), can utter 'Naomi is a philosopher' and say something true. The non-professional, shared concept of a *philosopher* is not enough to explain (away) our intuitions about such cases.

CASE 2: Second, take 'dangerous'. Szabo claims he can't get the intuition that there are different ways of being dangerous, different understandings of 'dangerous' (as Travis puts it in his reply). Consider these contrasting cases: (BACKGROUND) A piece of paper with information on it: 'Mr. Smith was born on June 7, 1892.' Let's assume also that were this information made public, it would have great implications for a certain trust, hundreds of millions of dollars would be lost by company C. (CONTEXT ONE) One of C's lawyers says: 'That's dangerous' (demonstrating the piece of paper (or the information on it). Intuition: this utterance is true. (CONTEXT TWO) Mr. Smith's friend, with no interest in company C, says (at the same time), 'That's dangerous', again demonstrating the piece of paper or the information on it. Intuition: this utterance is not true. Difference in truth-value indicates difference in content.

Our claim is that for any example of a non-ambiguous expression (or a single meaning of an ambiguous expression) we can conjure examples like this. Szabo, presumably, thinks we can't. Limitations of space prevent us from settling that debate here.

S#2: Szabo's 'Core Disagreement' and Minimal Content

We're not crystal clear on how to understand Szabo's 'core' disagreement. He says:

... let me move on to the core of my disagreement with *Insensitive Semantics*.
... it is hard to see why a speech act pluralist would deny that there is also some proposition that all utterances of 'I have a headache' express—perhaps the proposition that someone has a headache. Why does this not count as the semantic content of 'I have a headache'?

In this passage, it looks like Szabo wants to know why the proposition *that someone has a headache* isn't the semantic content of 'I have a headache'. If semantic content is supposed to be that which all utterances of S have in common, then this would seem to follow on the assumption that it is a proposition expressed by all utterances

of 'I have a headache'. Szabo assumes that would be an absurd conclusion. So, we take it, this argument is meant as a kind of *reductio* of semantic minimalism (it would also make our position internally inconsistent, since we explicitly say this isn't the semantic content).

We reply, first, we would need to see an argument for why speech act pluralism commits us to the view that all utterances of 'I have a headache' express the proposition *that someone has a headache*. We know of no such argument. Second, the result would follow only if we thought a proposition *p*, by virtue of being a common content for all utterances of *S*, would be sufficient for *p* being the semantic content of *S*. But that's *not* our view. On pp. 144–145, we describe a procedure for determining semantic content of a sentence. Being the common content is not sufficient. The first two are these:

- a) Specify the meaning (or semantic value) of every expression in *S* (doing so in accordance with your favorite semantic theory, i.e. we want Semantic Minimalism to be neutral between the different accounts of how best to assign semantic values to linguistic expression (e.g. objects, sets, properties, functions, conceptual roles, stereotypes, or whatever));
- b) Specify all the relevant compositional meanings rules for English (doing so also in accordance with your favorite semantic theory; again, we insist upon Semantic Minimalism being neutral between different accounts of how best to respect compositionality).

So, the view that the proposition *that someone has a headache* is the semantic content of 'I have a headache' simply does *not* follow from the assumption that this proposition gets expressed by every utterance of that sentence. That implication would have to be derived as part of a compositional meaning theory. We see no way this could happen. (Of course, Szabo might have an argument here that we are not aware of.)

We suspect there's deeper issue here connected to Szabo's follow up point. He imagines us saying that the reason why 'I have a headache' doesn't semantically express the proposition *that someone has a headache* is because it contains 'I', and this is a context sensitive expression, that the semantic theory will pick up on this, and hence, block the undesirable attribution of semantic content.

If this were our strategy, the question arises of how we are to identify the context sensitive expressions, and there, in Chapter 7, we appeal to various tests. Szabo thinks none of those tests is acceptable and that's the issue we take up next.

S#3: Szabo on Tests for Context Sensitivity, Proper Names and Binding

Szabo says that we hold the view that 'the only words that pass serious tests of context-sensitivity are the ones on Kaplan's list.' About this claim he says: 'This would move me if it were true. But I think it is not.'

Szabo makes a number of very quick points about our tests, again, we wish we had space for a fuller discussion. Our responses will have to be as brief as Szabo's comments:

Szabo on Proper Names

We didn't mean to take a stand on whether or not proper names are ambiguous (no argument in the book depends on it). If Szabo is right and they are not ambiguous (and not context sensitive), we would have to see the positive theory before we decide whether a serious problem lurks here. Absent a positive theory of how to treat proper names, it is hard to see exactly how this provides a challenge to the use we put our tests. As we pointed out in General Comments #1, we don't think passing the tests is either necessary nor sufficient for being context sensitive, so it is perfectly compatible with our theory that some special story would have to be told about proper names. This is just to say that we would have to see Szabo's positive proposal in order to evaluate the result of running the tests on proper names.

Szabo on Binding, Embedding and Indirect Reporting Tests

Szabo says:

As Cappelen and Lepore are well aware, a number of moderate contextualists believe that at least some context-sensitivity is due to a bindable variable in logical form. Expressions containing such variables obviously don't have their semantic values fixed under every embedding. Consequently, there is no compelling reason to believe that their semantic values don't change when they occur within a clausal complement of 'said', and they may well fail the inter-contextual disquotational indirect report despite being context-sensitive.

Not only are we well aware of this, Chapter 5 of *IS* is, in its entirety, devoted to a discussion of this idea (focusing on Stanley and Szabo, 2000). Two points (we wish we had more space to elaborate, especially on the second point, which adds something to our discussion in the book):

1. For reasons we spell out in Chapter 5, we think the idea of variables hidden in logical form (at least in the relevant cases) has counter-intuitive consequences and should be rejected.
2. Even if Szabo rejects our arguments to that effect (which he presumably does, though he doesn't tell us why), it would not help to establish his objection against our use of the indirect reporting tests. In *IS*, we formulate the tests by appeal to intuitions about sentences containing 'said that'. But we didn't need to do that. This is not a point we emphasized in *IS*, but we have become clearer on it later. All we need appeal to is intuitions about unembedded sentences in different contexts saying the same thing. So, we can, in this paper, say what Szabo said, by just saying it (without saying that we say it). Here's an example (what follows is a speech act performed by us): *We are aware that moderate contextualists believe that some context*

sensitivity is due to a bindable variable in logical form. In the previous sentence we just said, in this context, what Szabo said in another. To do that, and to trigger the intuition we did, we didn't need to embed that sentence within the scope of 'said that'. This is analogous to Kripke's response to Dummett's wide-scope account of rigidity. Kripke pointed out that we don't need to embed sentences in modal contexts to trigger intuitions about rigidity. We say the same about content sharing across contexts: We need only think about unembedded sentences and whether or not they say the same. The point we've just made, we take to be important and obviously in need of further elaboration and defense. We will have to leave that for another occasion.

4. Reply to François Recanati

Recanati's *Literal Meaning* (2004) has one central objection to the kind of view we call semantic minimalism (what Stanley, in a recent review of Recanati's book, calls *propositional syncretism*): Recanati argues, along with Carston (2002) and Stanley and King (2005), that the minimal proposition will, in Stanley's terms, be 'an idle wheel in an explanation of linguistic practice'. In Chapter 12, we respond to that challenge (we focus on the original version of the argument, found in early papers by Carston), and end with a reversal of that charge: it is Recanati's view that lacks psychological reality. Chapters 8 and 9 also directly object to the view Recanati advocates. In Chapter 8 we argue his view makes communication impossible and in Chapter 9 we argue it is internally inconsistent. So, there are a number of disagreements between Recanati and us that it would be interesting to explore.

These issues are not, however, what Recanati chose to focus on in his reply. His central argument concerns tests we present in Chapter 7. He has, unfortunately, misunderstood their status and nature. He has also misunderstood their role in our overall argument. We're afraid most of our reply will take the form 'This isn't our view, see page ...'. Given the substantive disagreements between Recanati and us, and given his interesting arguments for his view, we wish this exchange had been more substantive, but as it is, we're forced to reiterate our position and refer readers to relevant passages.

R#1: 'Cappelen and Lepore's Book is Addressed to their Fellow Minimalists'

Recanati correctly outlines our argument to the effect that there is a slippery slope from moderate to radical contextualism (in Chapters 3–5). In response to this argument he says '... the argument has force only for someone who antecedently rejects the contextualist conclusion. Cappelen and Lepore's book is addressed to their fellow minimalists, ... *C&L merely presuppose that contextualism is wrong*' (our emphasis).

In replying, we are befuddled. The middle part of our book is called ‘Part III: Refutation of Radical Contextualism’. It includes three chapters, each of which develops various objections to contextualism. Chapter 9 argues radical contextualism is internally inconsistent (something you should care about even if, or, rather, especially if, you’re a contextualist). Chapter 8 argues contextualism cannot account for communication across contexts (again, a point not only minimalists care about, we hope). Chapter 7 argues there’s a great deal of linguistic data (about disquotational reports, collective reports, reports under ignorance, etc.) that contextualism can’t account for. These are all points that should move anyone, regardless of their theoretical biases. So, in no way do we concede that our book presupposes contextualism is wrong—on the contrary, the central part of *IS* tries to show it is wrong, based on assumptions all participants in this debate can and should accept. If Recanati thinks our argument against radical contextualism is inconclusive, he should tell us why. If he thinks communication across contexts is impossible, he shouldn’t write a reply to us.

R#2: Recanati’s Distinctiveness Suggestion

Since Recanati ignores Part III of our book, he tries to help us ‘by providing an argument against the contextualist conclusion’. As noted above, our view is that the generalization is unacceptable for the reason we give in Chapters 7–9. We do not want or need to fill in any gaps with ‘Distinctiveness’. However, this might be as good a place as any to register comments on the idea that there are many kinds of context sensitive expressions. Some readers, including Recanati, seem to think we deny this. We don’t. All we assume is the following truism: *if an expression e is semantically context sensitive, its semantic value changes from one context of utterance to another*. That’s just what it is to be semantically context sensitive. Within the class of semantically context sensitive expressions, there are many different kinds (as we point out (p. 13)). Some are automatic, some are not. Some appeal to speaker’s intentions, some don’t. Some require demonstrations, others don’t. As far as we can tell, these are all differences with respect to how the semantic value is fixed in a context of utterance. We are completely neutral on these matters (see pp. 147–150). Nothing we say in *IS* depends on how these issues get settled.

R#3: Recanati’s Description of Our Tests

Recanati does discuss some of the data from Chapter 7. He says: ‘Let us now turn to C&L’s new criterion for context-sensitivity.’ It is clear Recanati takes ‘criterion’ to be something both necessary and sufficient for being context sensitive. So, an expression is context sensitive, on Recanati’s reading of Chapter 7, just in case it passes each and every test there. This is not our view. As we made clear in General Comment #2 above, we construe these tests as sources of evidence. We would never dream of using any one of these as a ‘criterion’ for context sensitivity—that would be confused methodology

R#4: Recanati On Our So-called ‘Master Argument’

Recanati claims that in §VI he has presented our ‘Master Argument’ for semantic minimalism. He thinks our master argument is that only terms in the Basic Set block homophonic reports across contexts. We wish it were not possible to read *IS* in this way. We don’t have a master argument. We don’t even use the argument Recanati calls our master argument (because we don’t treat the tests as criteria, see point R#3 above).

There is a section in the book called ‘Semantic Minimalism: Arguments’ (pp. 150–57) where we summarize all our arguments for semantic minimalism. We divide the arguments into two categories: ‘Argument by Elimination’ and ‘Argument from Explanatory Force’. The latter has seven sub-categories. The first of these seven has five sub-categories. What Recanati calls our ‘Master Argument’ is a version of one of these five sub-categories. So, it should be clear from that section that we do not treat the appeal to homophonic reports as a ‘Master Argument’.

R#5: Recanati’s ‘Counter-Example’

We claim that there are cases where we can give a disquotational indirect report of an utterance of a sentence with a term like ‘ready’. We claim that this provides evidence for the view that the term is semantically context insensitive. Again, in so doing we’re not alone: that assumption underlines many of the arguments in Hawthorne, 2004, Richard, 2004, MacFarlane, 2005, Egan, Hawthorne and Weatherson, 2005.

Recanati, in response, constructs a context in which such a report seems unacceptable, he says:

Whether or not a homophonic report is possible depends upon a number of factors, and, in particular, it depends upon the storytelling context (the context in which the report is made). . . . The trivial observation that the possibility or impossibility of homophonic reports depends upon the context in which the report is made is sufficient to dispose of Cappelen and Lepore’s theory.

We reply that the objection fails for two reasons, both of which should be obvious by now:

1. Recanati presents us with an instance of what we in General Comments #4 (see above) called a *BP-Example*. We refer the reader to that section and the discussion of these examples on p. 99 of *IS*. As we argue there, BP-examples are *beside the point*. Recanati might have an objection to our reply, but since he doesn’t present one, we don’t know how to advance the debate.
2. Even if Recanati has a knock-down objection to our use of the Inter-Contextual Disquotational Report Test, that would not be ‘sufficient to dispose of C&L’s theory’. As we noted in R#3 above, we don’t treat this test as ‘a criterion’. It is but one version of one argument out of five arguments that constitute one half of our arguments for semantic minimalism (again, see

pp. 150–157). (That said, we think it is a very strong argument, and we are in no way prepared to give it up. We also think further development and understanding of it and related tests is one of the central challenges in semantics and pragmatics.)

3. Finally: Recanati is seeking a single objection, a silver bullet, that will ‘dispose of’ semantic minimalism. We’re quite sure that’s not how this debate will play out, and we think it is unproductive to look for arguments that aspire to ‘dispose of’ one of the positions in logical space (and semantic minimalism is at least that). The issues raised are exceedingly complex—questions about the set of intuitions we should invoke, the interpretations of those intuitions, the best way of describing them, and the best theoretical framework for them. Simple knock down arguments will be hard to come by in this terrain. We’re more likely to gradually refine the sense of what counts as evidence, how to access that evidence, and the competing theoretical framework for explaining those intuitions.

R#6: Recanati on Contextual Salience Absorption

In connection with our discussion of contextual salience absorption Recanati says:

... the contextualist can have her cake and eat it: she can invoke Contextual Salience Absorption to account for the homophonic cases, and she can simultaneously invoke the other cases (the Leslie cases) to argue against the literalist schema.

Since we are not denying that there could be cases where the disquotational reports seem unnatural (see General Comment #4) we have no principled objection to this more limited appeal to contextual salience absorption (it is, of course, not an objection to the argument we run in the book, since we were arguing against someone who denied that there could be *any* reports of the problematic kind). What remains to be explained, for the contextualist, is *what* the shared content is in the cases where these reports are possible. Finally, what we call *collective reports* and *reports under ignorance* are unexplained by this strategy.

5. Reply to Anne Bezuidenhout

B#1: Theoretical Use of ‘Red’

IS argues that moderate contextualism collapses into radical contextualism and that radical contextualism is self-defeating. The latter part of the argument is presented in Chapters 7–9, and the last of these chapters argues that the way radical contextualists present their views is inconsistent with radical contextualism. In so doing we use examples from a paper by Bezuidenhout—not because we think her paper is particularly guilty of the point we’re trying to make, but because it exemplifies a general feature of the contextualist literature. Our argument goes as follows: the way Bezuidenhout uses the expression ‘red’ when she talks about the

kinds of examples that are meant to establish that 'red' is context sensitive is incompatible with the claim that 'red' is context sensitive. She is using 'red' in a context insensitive way to establish that 'red' is context sensitive. We claim that this is general problem—not specific to Bezuidenhout's particular formulation of radical contextualism.

In response to this line of criticism, Bezuidenhout says:

Now the word 'red' and other color terms like 'brown' and 'green' have been both used and mentioned frequently in this paper. So how did I understand these terms? What were the conditions of application for these terms that were operative in this context? I don't think this question makes much sense, because nowhere in this paper did I use the term 'red' to make a genuine color attribution to any real object, such as a real apple. . . . So the question as to what counts as red in the context of my paper is moot, as is the question of whether I was using 'red' in a self-defeating way.

The claim is that our question about the conditions for application for 'red' in her paper doesn't 'make much sense'. Why not? Because 'nowhere in this paper did I use the term 'red' to make a genuine color attribution to any real object'. If this were a sound reply it would undermine one of the central contentions in our book.

We're unconvinced. In her paper we're asked to imagine various scenarios involving apples, their colors and talk about their colors. These examples are supposed to trigger (or elicit) intuitions about the properties of the word 'red'. We are assuming that those intuitions are not specifically about 'red' when used to ascribe properties to *imaginary* apples—they are supposed to generalize. The fact that these are imaginary examples should make no difference—we treat these cases as if they were genuine, as if they exemplified genuine linguistic exchanges. It is when so interpreted, i.e. as making genuine color attributions, that the examples are self-refuting. If they examples do not extend to the application of 'red' to real apples we are owed a story about what the value of these imaginary examples is supposed to be.

In sum, if you use the word 'red' while speaking English, then you are talking about the color red—whether you like it or not. We are not Humpty Dumpty.

B#2: Bezuidenhout's 'Rebuttal' of our Examples in Chapter 7

Bezuidenhout claims our intuitions about the tests we run in Chapter 7 are wrong. She imagines two utterances by Nina of 'John is ready'. We claim these are cases where someone in a relevantly different context can say 'Nina said that John is ready' or 'In both cases, she said that John is ready'. Bezuidenhout says '... it will simply be false to say that what Nina said in both contexts was the same thing'.

If we put aside the concern that the way she sets this up makes it an instance of a BP-Example (see General Comment #4 above), the main charge is that the kind of

reports we are focusing on are false. What do we say to someone who claims all such reports are false? In short, we know of no argument for that claim. These reports are not only intuitive and ubiquitous, they also play an important role in our communicative interactions. They are, we have suggested, the bedrock of our conception of shared content. The idea that we can share content is of fundamental importance (for elaboration on this theme, see Cappelen and Lepore, 2006b). Of course, Bezuidenhout makes it easier for herself than she is entitled to by focusing on examples of the form 'John is ready'. Bezuidenhout is a radical contextualist, so she thinks 'John is ready for an exam' is context sensitive (the sentence itself doesn't express a proposition). So to test her view (in a way that makes it clearly distinct from a more moderate contextualist position), consider two utterances of 'John is ready for an exam' and ask yourself: Can they be reported (in relevantly different contexts) by 'They both said that John is ready for an exam' (similarly for reports under ignorance, and the other tests we exploit). Ask yourself whether you think this would be false, and you'll be able to settle the dispute between Bezuidenhout and us. (The same kind of comment applies to her discussion of our use of Travis' example involving 'weighs 80 kg'.)

B#3: Bezuidenhout on VP Ellipsis

The above point connects to Bezuidenhout's remark about VP ellipsis. She says:

If we have already interpreted the antecedent clause 'Bill weighs 80 kg' in such a way as to recover the property *weighing x while fully clad on an unknown planet*, then we can't recover the property *weighing x while naked on Earth* which is needed to resolve the ellipsis in the way C&L suggest is permissible.

If you assume two properties were expressed, then, of course, there can't be VP ellipsis. But our point was to elicit the possibility (argue for the possibility) that the property expressed in both cases is the property of weighing 80 kg. Of course, if her intuitions clash with ours with respect to that possibility, she is right. You can't get VP ellipsis. But we're quite confident intuitions of non-biased speakers are not on her side in this debate. We claim there are cases where we she would predict that VP ellipsis is impossible, but where it seems perfectly natural. Of course, these will not be cases where the example is set up in such a way that we specify that the properties in question are different. They will be examples (like those used in the book) where we have two utterances of, e.g. 'Smith weighs 80 kg naked on the moon', and where VP ellipsis seems natural (i.e. it seems natural to say 'A said that Smith weighs 80 kg naked on the moon, and B said that too') and where it turns out, that on further investigation, Bezuidenhout's version of radical contextualism implies that different properties were expressed by the two utterances of 'weighs 80 kg naked on the moon', and hence, that, by her own standards, VP ellipsis should not be possible.

6. Reply to Steven Gross

Gross on an 'Implicit Principle' in IS

At the very heart of Gross' commentary is the following principle which he attributes to us:

Implicit in C&L's presentation of cases is something like the following principle: if (1) A utters 'P,' (2) B utters about A's utterance 'A said that Q,' and (3) speakers judge B's report accurate, then the proposition semantically expressed by 'Q' is part of the speech act content of A's utterance.

He then presents us with two options for how to interpret 'B's report' in (3):

- a. Speakers judge some salient component of the speech act content of the report to be accurate.
- b. Speakers judge the semantic content of that report to be true.

Both options lead us into difficulty, Gross argues. If we choose (a), the implicit principle (i.e. (1)–(3)) just doesn't seem plausible, for several reasons; the most important of which is that if we've judged some non-semantic component of the report true, why would we infer that the semantic content is something the speaker said? If we choose (b), things also look bad for us, Gross says:

When condition (3) is understood this way, however, it is much less obvious when condition (3) is satisfied and whether speakers are in fact sufficiently good judges in the sense required. In particular, C&L do not provide us with reason for confidence in the cases used to support the Controversial Aspect (CA-cases).

Gross here runs an internal argument against us, appealing to what we say about speakers' access to the semantic content of sentences.

C&L point out that, indeed, the semantic content of most utterances is 'not even the most important proposition the speaker intended to communicate to her audience' (p. 205, punctuation adjusted). Similarly from the audience's perspective: in 'most regular-life contexts, the semantic content is not what is focused upon' (p. 207). If indirect speech reports are no exception to these claims (C&L provide no reason to think otherwise), then 'nontheoretic beliefs and intuitions about what people say,' as expressed in indirect speech reports and judgments thereof, are more likely keyed to some aspect of the reports' speech act content other than its semantic content.

He goes on to consider various ways in which we could reply to this dilemma. He says, for example, that we can't appeal to the context of our book being one in

which semantic content is salient—because these examples are supposed to be ubiquitous, not just confined to philosophy books.

This is a very clever objection: one that exhibits genuine understanding of fundamental features of our position. These are issues we should have addressed in more detail, and issues we wish we were clearer on. Gross' reply is also a sustained argument, rather than a collection of various objections. For this reason, our reply to him will be relatively brief (simply because all we will do is point out where in the argument we disagree with him, not because the point merits little discussion).

There is a point rather early in this argument where we would like to get off. If you look back at (1)–(3), we object to a part Gross does not discuss. Having presented (1)–(3), he adds the consequence, i.e. '... then the proposition semantically expressed by 'Q' is part of the speech act content of A's utterance.' This is the part we object to. Before we tell you how we object, note that this is independent of how to understand the expression 'B's report' in (3). No matter how that is understood, there's an independent assumption made by Gross about the effect of (1)–(3), namely, that it is the proposition semantically expressed by 'Q' should (on the basis of (1)–(3)) be included in the speech act content of A's utterance.

The possibility Gross does not consider is *that it is the proposition saliently asserted by 'Q' that should be included in A's speech act content*. We want to run the entire set of intuitions on what is saliently asserted. So understood, it is unproblematic to understand 'B's report' as referring to the speech act content of that report. In other words, the principle we would like to endorse goes something like this:

If (1) A utters 'P,' (2) B utters about A's utterance 'A said that Q,' and (3) speakers judge what B said accurate, then we have evidence that the proposition saliently asserted by the complement clause of that report is part of the speech act content of A's utterance.

As far as we can tell, Gross' objections do not apply to this principle. What we in effect have done is to choose (a version) of interpretation (a) above, but that's unproblematic when the consequent talks about what's saliently asserted, rather than the semantic content.

*Philosophy Department
University of Oslo*

*Center for Cognitive Science
Rutgers University*

References

- Almog, J. 2006: Is a unified description of Language-and-Thought possible?. *Journal of Philosophy*, forthcoming.

- Cappelen, H. and E. Lepore 1997: On an alleged connection between indirect quotation and semantic theory. *Mind & Language*, 12, 278–296.
- Cappelen, H. and E. Lepore 2004: Context shifting arguments. *Philosophical Perspectives*, 17, 25–50.
- Cappelen, H. and E. Lepore 2005: *Insensitive Semantics*. Oxford: Blackwell.
- Cappelen, H. and E. Lepore 2006a: Reply to Hawthorne. *Philosophy and Phenomenological Research*, forthcoming.
- Cappelen, H. and E. Lepore 2006b: Shared content. *Nóus Philosophy Supplement*.
- Cappelen, H. and E. Lepore in progress: *Shared Content and Semantic Spin*.
- Carston, R. 2002: *Thoughts and Utterances: The Pragmatics of Explicit Communication*, Oxford: Blackwell.
- Egan, A., J. Hawthorne and B. Weatherson 2005: Epistemic modals in context. In Gerhard Preyer and Georg Peter (eds), *Contextualism in Philosophy*. Oxford: Oxford University Press, pp. 131–168.
- Hawthorne, J. 2004: *Knowledge and Lotteries*. Oxford: Oxford University Press.
- Hawthorne, J. 2006: Testing for context-dependence. *Philosophy and Phenomenological Research*, forthcoming.
- Lasersohn, P. 2005: Context dependence, disagreement, and predicates of personal taste. *Linguistics and Philosophy*, forthcoming.
- MacFarlane, J. 2005: Making sense of relative truth. *Proceedings of the Aristotelian Society*, 105, pp. 321–339.
- Recanati, F. 2004: *Literal Meaning*. Cambridge: Cambridge University Press.
- Richard, M. 2004: Contextualism and relativism. *Philosophical Studies*, 119, 215–242.
- Salmon, N. 2005: Two concepts of semantics. In Z. Szabó (ed.), *Semantics vs Pragmatics*. Oxford: Oxford University Press, pp. 317–328.
- Stanley, J. 2005: *Knowledge and Practical Interests*. Oxford: Oxford University Press.
- Stanley, J. and J. King 2005: Semantics, pragmatics, and the role of semantic content. In Z. Szabó (ed.), *Semantics vs Pragmatics*. Oxford: Oxford University Press, pp. 111–164.
- Stanley, J. and Z. Szabo 2000: On quantifier domain restrictio. *Mind & Language*, 15, 219–261.
- Travis, C. 1985: On what is strictly speaking true. *Canadian Journal of Philosophy*, 15, 2, 187–229.
- Travis, C. 1989: *The Uses of Sense*. Oxford: Oxford University Press.
- Travis, C. 1996: Meaning's role in truth. *Mind*, 100, 451–66.