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Fantl and McGrath: Loose Use

One of Jeremy Fantl and Matthew McGrath's (F&M's) main motivations (perhaps *the* main motivation they have) for moving from what they describe as my "cases-as-data strategy" to their proposed "cases-as-conclusions" methodology is that my principal argument for contextualism is open to the objection that the positive claim to know in my LOW case is an example of loose speaking. They suggest I could complete my argument if I would "develop tests for loose speech" and then argue that they're not met in my LOW cases—though they think I would be better off changing strategies altogether. The work of developing this "loose speech" approach to the data contextualists try

to use has been done for F&M, principally by Wayne Davis (2007), whom they can and do cite. By contrast, the critical points I believe need to be made about the "loose speech" approach are not out there (that I know of) in a form I can just cite. Since the viability of this loose speech approach is important to the methodological differences between F&M and me, I will take this section to briefly make some observations about that approach.

As Davis notes (Davis 2010: 1155), Peter Unger's theory of the early-to-mid 1970s ("early Unger") that I do argue against is an example of a loose usage invariantist theory—an invariantist account on which the intuitions about the truth-values of knowledge claims that go against the theory are explained away as examples of loose speech. If one takes this type of approach, one will naturally opt for a version of invariantism that sets the standards for knowledge very high, and early Unger certainly did that: His semantics for various terms (including "knows," "flat," and many others) was *maximally* demanding: for a surface to be such that it could truthfully be called "flat," for example, it must be such that it is impossible for any surface to be flatter than it is; for someone to be such that one can truthfully describe them as "knowing" that p, they must be so well-positioned with respect to p that it is impossible for any subject to be better positioned with respect to any proposition; etc. This makes all or almost all of our positive uses of the terms in question come out false, which, interestingly, Davis does recognize as a problem. But he proposes a solution, which seems to be to ease up just a bit on the severity of the semantics. At the end of my third chapter (DeRose 2009: 117–27), I argue against what I took to be the two best attempts to defend an invariantist approach: both that of early Unger, and the much later and very different invariantist theory of Patrick Rysiew, whose semantics were so forgiving that it's actually the denial of knowledge in my HIGH cases that he argues is false. Davis thinks I make it too easy on myself by picking two easy invariantist targets, one of which employs truth-conditions that are too demanding and the other of which is too lax. Davis feels his own approach to "knows" and other terms, which employs "strict but satisfiable truth conditions" (Davis 2010: 1156), provides the "Goldilocks" (not his term) form of invariantism that's just right (Davis 2010: 1155–6).

A problem with such an approach is that it's hard to see just how very demanding to make one's semantics. If you're not going to go all the way with early Unger, but after siding with the skeptics (even *extremely* annoying everyday, as opposed to philosophical, skeptics) and writing off what seem to be even *extremely* serious positive uses of "know(s)" (including, I presume, even some uses in high-stakes court

testimony) as examples of false, loose speech, one then seeks to suddenly draw a line in the sand and say, "Wait a minute, skeptics, now *that's* getting *too* demanding," it's easy to worry that one is trying to close the barn door after far too many of the horses have already escaped, and with little in our linguistic practices to look to for guidance in deciding just how few of the last horses should be kept.

But to seriously evaluate such an approach, we need to know just how demanding its semantics is, so we can evaluate just how much systematic falsehood it implicates us in on the one hand (how much truth-telling it's able to save in our positive uses), and on the other hand whether it can account for all the apparently high-standards denials of "knowledge" we should want to account for. I've never seen a way to plausibly thread that needle in general, but I must admit that I really don't feel I have a good grasp of just how demanding Davis's semantics is—just what it takes to know on his view. (Much depends here on what it takes on his view to be "completely justified" in believing something, but that's precisely what I don't feel I understand, in Davis's hands.) Readers are encouraged to read at and around (Davis 2007: 426–7), where we learn, for instance, that "When she arrives at the bank Saturday morning after nine o'clock and finds it bustling with activity, I think it is literally and strictly speaking true that Hannah now knows the bank is open." So Davis seems no early Unger. But he certainly does seem to get *extremely* demanding.

F&M agree with Davis that his approach has the advantage over early Unger that it is not "wedded to an 'impossibly demanding semantics'." But though F&M are quoting the phrase "impossibly demanding semantics" from my depiction of Unger's theory, it's not as if Ungers semantics for them implied that *all* positive uses of the relevant terms (the "absolute terms") are false. Unger's favorite example was 'flat', and though he held to a maximally demanding semantics for that term, according to which a surface must be one flatter than which no surface can be conceived (to use some appropriate Anselmian terminology to describe the view) before it can be truthfully described as 'flat', he always threw in some important qualifications when describing the results of such a demanding account, constantly writing such things as that, for instance, "We do not speak truly, at least as a rule, when we say *of a real object*, 'That has a top which is flat'" (Unger 1975: 49, emphasis added)—the implication being that we can truthfully say that an ideal plane we are discussing while, say, in working on a geometry problem, is "flat." (So his semantics only proves "impossibly demanding" for real physical objects of the type we're familiar with.) Perhaps Unger's theory (whether or not Unger thinks so) will allow true ascriptions of "knowledge" to God? And on the other side, there seems to be

no effective limit on how demanding Davis will get on claims like "It's three o'clock."[8]

At any rate, a main thrust of my third chapter is to argue that pragmatic explanations of semantic intuitions can be carried too far, and to start on the task of discerning when such maneuvers are legitimate and when not. I conclude that things aren't looking up for invariantist attempts to explain away the data supportive of contextualism, since they seem to get on the wrong side of the reasonable tests that seem to emerge from a look at some fairly clearly successful and also clearly unsuccessful uses of such maneuvers. While F&M seem to write as if arguing against pragmatic theories like Davis's would be something new added to my case, Davis himself sees that a pragmatic theory like his would be among my intended targets in the argument I have given,[9] and he seeks to explain why my argument does not work against *his* loose speech proposal. Key to Davis's attempted escape is the claim that there are clear examples of loose speech that he can use as the model for what he claims is going on when speakers claim to "know" things in my LOW cases.[10]

But isn't Davis right? *Isn't* it clear that, as F&M confidently assert in their second paragraph, "Loose speech is pervasive"? Well, certainly in *some* sense, yes, but I suggest caution here, because it might not be

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[8] Davis reports: "[Stewart] Cohen has objected that if all sentences ascribing knowledge of precise measurements are strictly speaking false, then no one ever knows what time it is, or when anything occurs, or how long anything is." Davis's response, that follows immediately, very much surprised me: "That conclusion would be absurd indeed." Perhaps not coincidentally, Davis thinks he can meet this challenge: "That conclusion would be absurd indeed, but it does not follow. I can know that the stick is *about* 3 ft. long, and that suffices to know how long it is" (Davis 2007: 417). But if claims that someone knows what time it is always have to have a hedge like "about" or "approximately" in them to be true, isn't that pretty bad already? If it would not just be a cost, not just damaging, or even very damaging, but positively "absurd" to have to hold that no one ever knows what time it is, isn't it at least a fairly serious cost to have to hold that no one ever knows such unhedged things as that it's three o'clock?

[9] (Davis 2007: 412). Davis is here responding to the argument as it appeared in (DeRose 1999) and (DeRose 2002), which later formed the basis of Chapter 3 of the book currently under discussion (DeRose 2009).

[10] Davis writes, "DeRose (1999, p. 200) knew of no case in which what a speaker says appears true just because of a true implicature," and he replies, "Loose use provides countless examples" (Davis 2007: 413). Now, what I had written was that none of the "*clearly successful*" pragmatic maneuvers of the relevant type explain away apparent truth by appeal to the fact that a true implicature is generated. Davis seems to get that qualification, because he has just made a claim that there are "clear cases" of loose use, writing, "On the contrary, I am accounting for the observed behavior of "know" in terms of the general phenomenon of loose use. I use the close similarity between cases of contextual variation in the use of 'know' and *clear cases* in which diverse terms are used with variable strictness as evidence that the variation is due in part to its being used loosely or strictly" (Davis 2007: 412, emphasis added).

the sense that the "loose usage" theorist needs as examples of appropriately asserted *falsehoods*. Let's begin by looking at one of Davis's presumably clear examples, his "Time Measurement Case":

> A. Wondering how hard the final exam was, I ask Mike how long he took to finish. He answers "Two hours." B. When Nora says that she took two hours and four minutes to finish the exam, Mike responds "You took even longer than me. It took me two hours and two minutes."

Immediately after presenting the case, Davis asserts, "What Mike said in A is false if what he said in B is true" (Davis 2007: 407). But why think that? I don't see any problem with ruling both claims as true (I'm assuming A and B represent distinct conversations), and indeed, those are my verdicts about the cases. If there's supposed to be some connection between these judgments that rules out both claims being true, that reasoning (and readers are encouraged to examine the brief paragraph in which Davis explains this verdict) escapes me, for reasons that may become apparent as we proceed.

But Davis may well mean not to be so much making some connection here, but just to be registering his sense or intuition that Mike's answer in A is false if he took two hours and two minutes to finish. And if Davis were to just take it to be obvious beyond the need for argument that what Mike says in A is false if it took him 122 minutes to finish, he certainly wouldn't be alone. In fact, he would be following the lead of perhaps the most important recent work on loose speech (not cited by Davis, but I assume he's aware of it), (Lasersohn 1999), which begins with a very similar case about time. Lasersohn's abstract begins with the claim,

> It is a truism that people speak 'loosely'—that is, that they often say things that we can recognize not to be true, but which come close enough to the truth for practical purposes.

And the body of his paper begins as follows:

> People speak with varying degrees of precision, and often speak quite loosely. Suppose, for example, I tell John that Mary arrived at three o'clock. In certain relatively unusual circumstances, the exact second of her arrival might be important, but most of the time this level of precision is not required. So if John finds out later that Mary didn't arrive at three but at fifteen seconds after three, it would be unreasonable of him to complain 'You said she came at three!'

> But whether or not John is acting unreasonably in this situation, I think we have to concede that he is, strictly speaking RIGHT: when I

told him that Mary arrived, I said something that was literally false, not true. (Lasersohn 1999: 522)

What I wish to suggest here is that while it is certainly true that "people speak with varying degrees of precision," and that it is clearly true that in *some* good sense we often speak loosely (and I'll explain in what sense I think that happens a few paragraphs down), it is very far indeed from clear, much less a "truism," that these (or other) examples are cases of "loose speech" *as Lasersohn analyzes that term*, and as Davis needs it to be understood, because it is very far indeed from clear that our speakers are asserting falsehoods.

Indeed, left to my own devices, I would have thought that it was at least somewhat clear in the relevant cases that the speakers were speaking *truthfully*. And *I* wouldn't be alone in that, either. As it happens, when I first discussed Lasersohn's example with a linguist I know who was clearly going along with Lasersohn's verdict, I happened to be carrying in my backpack John Hawthorne's then fairly recently published (2004), so I was able to pull that book out and show where Hawthorne claims that in many relatively undemanding contexts (no doubt including those like the context Lasersohn imagines), one speaks truthfully when one asserts "It is three o'clock," even when it's a full minute (and not just 15 seconds, as Lasersohn has it) after three o'clock! And Hawthorne gives no argument for this verdict. He issues it and then uses it in the argument he's giving (Hawthorne 2004: 99). So it must have seemed to him clear enough to be so used.

I suppose that, given this sharp disagreement in how things seem, I should no longer take it to be so clear that these "loose uses" are true. I'm just a little hesitant to do that, however, due to a suspicion that those on the other side are being misled by faulty reasoning. Space is drawing short, so I'll quickly state my suspicion in one of its main forms. I suspect that those who favor an invaryingly very demanding semantics in these cases are being (mis)led by such considerations as that

     a) "It's three o'clock, but it's actually a few seconds after three o'clock"

would seem to be just wrong, in just about any context they're said. To the extent that such considerations are responsible for their judgments, I believe those judgments are based on a bad underestimation of the ability of contextualist accounts to explain such data. Suppose that "It's three o'clock" gets used with varying degrees of precision in this (contextualist/indexicalist) way: how close to three o'clock it must be for such a claim to be *true* varies with context, and in less demanding contexts, it can cer-

tainly be true when it's, say, 15 seconds, or even a full minute, after exactly three o'clock. A "loose use" of such a claim, then, would be one governed by a relatively low standard of precision in that sense. The prospects for such a view to account for why a) sounds so bad seem to me bright, since it can be based on some claim to the rough effect that when you bring up the matter of a few seconds and treat it as relevant, you shouldn't/can't combine that with a low-standards-of-precision claim that "It's three o'clock," where a few seconds give-or-take need to be deemed irrelevant. The rough idea could be that even a context that starts out as governed by low standards of precision (that would render the first half of (a) true) would tend to *become* a high-standards context when the second half of (a) is asserted. And that could I think explain why (a) sounds so bad, in just about any context you imagine it being asserted.

I have similar reactions to Davis's other examples. Those wishing to pursue these issues might do well to start with the section of Davis's paper entitled "Semantic treatments of loose use" (Davis 2007: 415–17) where Davis reports suggestions made by Stewart Cohen (not in any work published yet, but in correspondence with Davis) for how to treat Davis's examples in the contextualist/indexical way alluded to in the above paragraph. (It's of course no surprise that Cohen and I have similar views here.) Davis argues against Cohen's ideas, but readers may well get good ideas of how to respond to Davis's arguments. I'll just report that I'm *still* finding it very far indeed from clear that the "low-standards" claims that serve as the examples of "loose speech" are false. A main problem with the account of early Unger was that, although he provided a lot of company for the misery he visited on our use of "know(s)," there was an alternative contextualist approach to all of the relevant terms that avoided the misery quite generally, and so Unger's general approach had no clearly correct applications and it was difficult to see where the pressure was coming from to make us put up with all the false-speaking the theory implicated us in. If the examples of Davis and Lasersohn were clear cases of false, loose speaking, that would have solved, for the loose usage approach, one of the main problems of Unger's approach.

Reply to Fantl and McGrath

So I'm a lot less worried about the threat of LOW turning out to be an example of false "loose speaking" than F&M seem to be. On the other side, I seem to worry a lot *more* than F&M do about the truth of general principles like their

> (Actionability) If you know that p, then p is actionable for you, i.e., you can count on p's being the case,