

MODIFIED OCCAM'S RAZOR

Ben Phillips

According to the principle Grice calls 'Modified Occam's Razor' (MOR), 'Senses are not to be multiplied beyond necessity'. More carefully, MOR says that if there are distinct ways in which an expression is regularly used, then, all other things being equal, we should favour the view that the expression is unambiguous and that certain uses of it can be explained in pragmatic terms. In this paper I argue that MOR cannot have the central role that is typically assigned to it by those who deploy it. More specifically, I argue that potential justifications of the epistemic import of parsimony in semantic theorizing are problematic, and that even if MOR could be justified, it has a redundant role to play in adjudicating the debate between the ambiguity-theorist and the proponent of the pragmatic approach.

1. Introduction

Often when there are distinct uses for an expression it is clear that the expression is ambiguous: for example, 'bank' is clearly ambiguous between 'financial institution' and 'riverside', and so there are two distinct uses for the expression 'I'm going to the bank'. But, of course, positing an ambiguity is not the only way to explain the distinct uses of an expression. Sometimes it's more plausible to retain a single meaning for the expression in question and explain the distinct uses in terms of some kind of Gricean implicature. For example, in uttering the sentence 'John's a great conversationalist' in a sarcastic tone, I implicate that John is a bad conversationalist, even though the sentence that I uttered literally means that John is a great conversationalist—there is no need to posit an ambiguity here.

In many cases it's obvious whether the distinct uses of a given expression are best explained by positing an ambiguity or by positing an implicature. On the other hand, sometimes it's not at all clear which explanation is better. A case in point is the debate between Russellians and ambiguity-theorists over the question of whether definite descriptions are ambiguous between an attributive meaning and a referential meaning. Other well-known cases include whether 'and' is ambiguous between a truth-functional, a temporal and a causal reading; whether 'or' is ambiguous between an inclusive and an exclusive reading; and whether 'some' is ambiguous between 'at least one' and a sense that implies 'not all'.

Given that intuitions are indecisive in these kinds of cases, those who favour the pragmatic explanation often appeal to what Grice calls 'Modified

Occam's Razor' (MOR). According to MOR, 'Senses are not to be multiplied beyond necessity' [Grice 1978: 118–19]. In other words, MOR instructs us to refrain from positing an ambiguity unless it is necessary.

Is MOR a good principle, though, and does it have the central role that it is often seen as having in adjudicating the debates between ambiguity-theorists and their pragmatic opponents? In what follows, I'll argue that MOR does not have a decisive role to play in adjudicating these debates. In particular, I'll argue that potential justifications of the epistemic import of parsimony in semantic theorizing fall short, and that even if MOR could be justified, at best, it has a redundant role to play in adjudicating the debates between ambiguity-theorists and their opponents.

2. Deploying MOR

One well-known dispute in which MOR is often invoked concerns Donnellan's [1966] distinction between *referential* and *attributive* uses of definite descriptions. When I use the expression 'the *F* is *G*' *attributively*, I convey the quantificational thought that there is a unique *F* that is *G*.¹ On the other hand, if I use this same expression *referentially*, then the thought that I'm trying to get across is a singular one about a particular *F* that I have in mind, where *G* is predicated of this *F*.

Now, what is *not* controversial is that there are these two uses of definite descriptions and that definite descriptions have a quantificational meaning. What *is* controversial is whether, in addition to their quantificational meanings, definite descriptions have referential meanings. According to the ambiguity-theorist, a definite description is ambiguous between the quantificational meaning and the referential meaning, whereas according to the Russellian, the fact that singular thoughts are conveyed by referential uses of definite descriptions can be explained in pragmatic terms.

Similarly, consider the following sentences containing 'and':

- (1) Roger is tall and has brown hair.
- (2) Margaret put on her glasses and began reading.
- (3) John ignited the fuse and the bomb exploded.

Now it is uncontroversial that a sentence such as (1) exemplifies the truth-functional meaning of 'and' captured by the logician's truth-table for '&'. But notice that someone who uttered (2) would typically be taken to have conveyed the thought that Margaret put on her glasses *before* she began reading. And someone who uttered (3) would typically be taken to have conveyed the thought that in igniting the fuse John *caused* the bomb to explode. This leads the ambiguity-theorist to construe 'and' as ambiguous between a truth-functional, a temporal and a causal meaning, while her

¹According to Russell [1905], 'The *F* is *G*' has the following logical form: ' $(\exists x)[Fx \ \& \ (\forall y)(Fy \rightarrow x=y) \ \& \ Gx]$ '.

opponent retains the truth-functional meaning and explains the temporal and causal uses in pragmatic terms.

Other instances of this kind of dispute concern the expressions 'or' and 'some'. According to the ambiguity-theorist, in addition to the *inclusive* sense of 'or' (captured by the logician's truth-table for ' \vee ') there is an *exclusive* meaning, e.g. 'I'll either take the bus or walk to school today' is seen as meaning 'I'll either take the bus or walk to school today *but not both*'. In the case of 'some', the ambiguity-theorist claims that although one of its meanings is 'at least one', it also has a sense implying 'not all', e.g. 'Some years contain 366 days' is seen as meaning 'At least one year contains 366 days but *not all* years do'.

In order to support their claim that expressions such as 'the *F*', 'and', 'or' and 'some' are *not* ambiguous, proponents of the pragmatic approach frequently invoke some version of MOR. For example, Kripke [1977: 268] remarks:

It is very much the lazy man's approach in philosophy to posit ambiguities when in trouble ... Do not posit an ambiguity unless you are really forced to, unless there are really compelling theoretical or intuitive grounds to suppose that an ambiguity really is present.

Here is Recanati's [2004: 157] construal of the principle:

... the analyst who observes that a sentence has two different interpretations when uttered in different contexts ... must, if possible, ascribe this difference to a property of the context of utterance rather than to an ambiguity in the sentence itself.²

Thus MOR instructs us to favour the pragmatic account of the distinct uses of a given expression (assuming one is available) over the ambiguity account. But how exactly does the pragmatic account of the distinct uses go? At this point it's important to keep Grice's [1975: 56] distinction between *particularized* and *generalized* implicatures at the fore.

What marks particularized implicatures off from generalized ones is the way in which the audience infers them. The inference to a particularized implicature is highly context-dependent, whereas the inference to a generalized implicature is relatively context-independent. For example, suppose I answer your question 'Are you a good soccer player?' with the reply 'Well, I'm Brazilian', thereby implicating that I'm a good soccer player. This counts as a particularized implicature because in order to infer it the hearer has to draw on features specific to the context of utterance, e.g. the fact that I was asked whether I'm a good soccer player. Alternatively, if I were to answer with 'I *think* I'm a good soccer player', the implicature that I have some reservations as to my abilities can be recovered without any of the stage setting that is required for the former implicature to be successfully

²Other proponents of MOR include: Bach [1987], Bach and Harnish [1979], Bontly [2005], Carston [2002], Green [1989], Horn [1989], Levinson [1983, 2000], Morgan [1978], Neale [1990, 1992], Ruhl [1989], Searle [1975], Stalnaker [1974], Walker [1975] and Ziff [1960].

communicated. This is because in the past ‘I *think* that *p*’ has regularly been used to implicate the proposition that the speaker has reservations as to whether *p*, and as a result, this proposition has become a default interpretation for the sentence ‘I *think* that *p*’, even though it is not the proposition that is semantically expressed.

Now consider the case of definite descriptions. Given that we *regularly* use definite descriptions referentially and without any stage setting, it’s more plausible for the Russellian to construe the singular propositions that, according to her, are thereby implicated, as *generalized* conversational implicatures as opposed to particularized ones. The same goes for ‘and’, ‘or’, ‘some’, and other such expressions. In each case, the proponent of the pragmatic approach should explain those uses that, according to the ambiguity-theorist, exemplify additional meanings, in terms of *generalized* implicatures. Moreover, this makes it harder for the ambiguity-theorist to argue from the fact that the expression in question is *regularly* used in the relevant way, and, without the audience having to go through some Gricean derivation in order to recover the implicature, to the conclusion that there is a convention of using the expression in this way.³

Thus the core question at the centre of the debate between the ambiguity-theorist and her opponent is this: are the distinct uses of the expression in question best explained by positing an ambiguity or a generalized implicature? And for our purposes, the pertinent question is this: what role, if any, does MOR have to play in determining the answer to this question?

Before we look at some of the problems facing the claim that MOR has a significant role to play in adjudicating the debate between the ambiguity-theorist and her opponent, it will pay to consider Devitt’s [2004, 2007, and unpublished] point that there is a version of MOR that should be discarded from the outset. This version of MOR says that *one ought to refrain from explaining some use of an expression by appealing to a convention if a Gricean derivation of the meaning conveyed is available*. That is, on this construal of the razor, if the thought conveyed by some use of an expression can be recovered via a Gricean derivation, then one ought to refrain from construing this thought as the one literally expressed, choosing instead to put it in the implicature basket. As Devitt points out, the problem with this construal of MOR is that it fails to accommodate ‘dead metaphors’; expressions that now conventionally mean what they once meant only metaphorically. A dead metaphor is thus an expression whose meaning can be recovered via Gricean inferences despite the fact that the expression’s meaning is now conventional. Reimer [1998: 97–8] gives the example of ‘incense’, an expression whose only literal meaning used to be ‘make fragrant with incense’. The metaphorical meaning, ‘make angry’, became regularly associated with the literal meaning and, as a result, a convention was born and ‘incense’ became ambiguous. But the fact that the derivation of this second meaning is still available does not mean that it is not a

³Devitt [2007], an ambiguity-theorist, recognizes this point, before going on to argue that a strong case can still be made for the claim that definite descriptions have referential meanings.

conventional meaning. This goes to show that the mere existence of a Gricean derivation of a conveyed meaning does not render that meaning non-conventional.

Thus the formulation of MOR given above will not do. As Devitt [unpublished] points out, a better formulation of the razor is this: *If a Gricean derivation of the thought conveyed by some use of an expression is available and positing a convention does not better explain the data pertaining to this use, then one ought to explain the use in pragmatic terms.* In other words, if the explanation of a particular use of an expression in terms of a convention is equally as good as the pragmatic explanation, then the pragmatic explanation is to be preferred on grounds of parsimony. In what follows, I'll argue that even this improved version of MOR does not have a significant role to play in adjudicating the debate between the ambiguity-theorist and her opponent.

3. The Redundancy of MOR

As was just pointed out, the best formulation of MOR instructs us to favour the pragmatic account of the meaning conveyed by some use of an expression (assuming one is available) only if positing a convention does not provide a *better* explanation of the relevant data. Among the data that need to be explained are our intuitions as to whether an ambiguity exists.

Now there are *direct* intuitions regarding the existence of ambiguities, e.g. the direct intuition that 'bank' is ambiguous, and there are those *indirect* intuitions that are pumped by the various 'diagnostic tests' for ambiguity that have been put forward in the literature.⁴ For example, according to the 'test of contradiction', if a sentence is ambiguous then for a given state of affairs we will be able to both truly affirm and deny this sentence. For example, suppose John is a teetotaler. Then, given that 'drinking' is ambiguous between 'consuming liquid' and 'consuming alcohol', one can truly affirm and deny the sentence 'John is outside drinking' (because he consumes certain liquids such as water but doesn't drink alcohol). The details of these different tests are not important here. Nor is the exact way in which they rely upon intuitions. For our purposes, what is important is that in prompting us to make judgments about the truth-values/meanings of certain sentences in particular contexts these tests provide data points that serve as evidence for/against the ambiguity thesis in question.⁵

With this in mind, suppose we inspect the data provided by direct intuitions and ambiguity tests and it turns out that they decisively favour the view that expression-type *E* is ambiguous. Then, of course, the proponent of the pragmatic account cannot deploy MOR, for it tells us to favour the pragmatic explanation only if the ambiguity account *does not provide a*

⁴Zwicky and Sadock [1975] provide an extensive survey of ambiguity tests.

⁵Of course, there may be other ways to test for ambiguities. According to 'Kripke's Test' [Kripke 1977], if an expression in our language is ambiguous then we would expect the ambiguity to be absent in other languages. However, see Amaral [2008] for an argument that Kripke's Test will not work for definite descriptions (Devitt [unpublished] applies Amaral's argument to expressions such as 'and', 'or' and 'some').

better explanation of the data. On the other hand, suppose that the data provided by intuitions and ambiguity tests decisively favour the view that *E* is unambiguous. Then, once again, MOR has no decisive role to play in adjudicating the debate between the ambiguity-theorist and her opponent, for the ambiguity thesis provides a bad explanation of the data and thus there is no need to ‘break the tie’ between the two competing explanations in the first place.

As a matter of fact, when it comes to the disputes described above concerning whether ‘the *F*’, ‘and’, ‘or’ and ‘some’, are ambiguous expressions, the data provided by direct intuitions and ambiguity tests seem to be largely indecisive. Certainly, intuitions as to whether these expressions are ambiguous are nowhere near as strong as direct intuitions concerning paradigms of ambiguous terms such as ‘bank’ and ‘ball’.⁶ It seems to me that this is precisely why MOR has been seen as having such a central role to play in adjudicating the debates between ambiguity-theorists and their opponents; the thought being that if the linguistic data are neutral between the two views, MOR can act as a tie-breaker, telling us to choose the more parsimonious option, i.e. the pragmatic account. This seems to be the role for MOR that Bontly has in mind [2005: 289]:

But in many cases, semantic and pragmatic explanations both appear plausible, and the usual data—our intuitions about how the expression can and cannot be used—appear to leave the choice of a definite hypothesis underdetermined . . . To adjudicate these borderline cases, Grice (1978) proposed a methodological principle which he dubbed *Modified Occam's Razor* . . .

There is a problem with this construal of the role of MOR though, for if the linguistic data are indeed indecisive between the ambiguity view and the pragmatic view then there would seem to be a third explanation of the data that is better than both of these. The explanation that I have in mind is that it is *indeterminate* whether there is a convention in place for the referential use of definite descriptions, for the temporal and causal uses of ‘and’, for the exclusive use of ‘or’, and so on. Let me explain.

This is not the place to determine exactly what has to be added to a mere regularity in order for it to qualify as a convention. Plausibly, there has to be a certain relation between the beliefs, dispositions, etc., of those who are responsible for the regularity in question. Perhaps some kind of ‘mutual understanding’, as spelled out in Lewis [1969] or Schiffer [1972], is the extra ingredient that takes us from a mere regularity of use to a convention. At any rate, whatever the extra ingredients are, conventions are clearly not an all-or-nothing affair. Just as paradigmatically vague terms such as ‘red’ admit of borderline cases, the notion of a ‘convention’ is surely vague. That is, we can imagine a sorites series going from uses of expressions that are determinately not conventional (e.g. obvious implicatures), through those regularities in the borderline region that are neither determinately

⁶Also, notice that even if Kripke’s Test *does* apply to expressions such as ‘the *F*’, ‘and’, ‘or’ and ‘some’, as Amaral [2008: 294] points out, no one has in fact carried out the detailed cross-linguistic study that would be required to gain decisive data.

conventional nor determinately not conventional because the extra ingredients that are required for a convention to be in place are neither determinately present nor determinately not present, to those regularities that are clearly conventional. Focusing on the case of metaphors, one can imagine a series of metaphors going from ones that are definitely alive, through those in the borderline region that are dying, to those metaphors such as 'incense' that are clearly dead.

The upshot of the observation that 'convention' is a vague notion is that if direct intuitions and ambiguity tests are indecisive as to whether a given expression is ambiguous or not, then a plausible explanation of this fact is that the use in question is a borderline case of a conventional use, i.e. it is indeterminate whether the expression is ambiguous or not because it is indeterminate whether or not it has a second conventional meaning.⁷ This option has clearly been neglected in the literature, but it is not obvious why. Certainly, the fact that direct intuitions and ambiguity tests are indecisive when it comes to expressions such as 'the *F*', 'and', 'or', 'some', etc., cries out for explanation, and the appeal to indeterminacy looks like a perfectly good one.⁸

Moreover, for our purposes, the important point is that the availability of the indeterminacy explanation undercuts the claim that, given the indecisive nature of the data, MOR can be used as a tie-breaker in the dispute between the ambiguity-theorist and the proponent of the pragmatic approach, for recall that on the most plausible construal of MOR we are to favour the pragmatic explanation only if positing an ambiguity does not better explain the data. But of course, in its most general formulation, the most plausible version of MOR says that unless there is *some* better explanation of the data, we should favour the most parsimonious one. Thus if claiming that it's indeterminate whether there is a convention in place for a particular use of an expression best accounts for the data, then MOR cannot be used to support the pragmatic account. At the very least, it is incumbent upon the proponent of the pragmatic account to explain why the appeal to indeterminacy does not provide a better explanation of the linguistic data, thus undercutting her appeal to MOR. And it is hard to see how she could establish this in a way that doesn't also provide direct support for the claim that the linguistic data favour the view that there is no ambiguity, thereby making the appeal to MOR redundant.

4. Attempts to Justify MOR

Set aside the worry outlined above. That is, suppose that the indeterminacy explanation is off the table and that the only competing explanations of the

⁷Of course, the exact sense in which it is 'indeterminate' whether a given expression is ambiguous depends on which theory of vagueness is correct. If we are epistemicists about vagueness then the indeterminacy in question is epistemological, whereas if, for example, supervaluationism is true, the indeterminacy is not epistemic in nature.

⁸Suppose that within certain linguistic communities, intuitions/tests decisively favour the pragmatic explanation, in others they favour the ambiguity account, and in others they are indecisive. Then a plausible explanation of the data might be that within certain communities there is/isn't a convention in place, while in certain others it is indeterminate whether there is a convention in place.

data pertaining to the distinct uses of a given expression are the pragmatic account and the ambiguity account. There are still problems with the claim that MOR can be appealed to in order to support the pragmatic account.

First, notice that the kind of parsimony at stake here is not *qualitative* parsimony, for both sides of the debate take the distinction between *what is said* and *what is implicated* for granted. Rather, the debate is over which category the message conveyed by means of a particular use of the relevant expression falls into and, according to the proponent of the pragmatic approach, putting the message conveyed into the *what is said* category offends against parsimony. In claiming that the expression is ambiguous, her opponent posits more *conventional meanings* (all other things being equal). Thus perhaps the proponent of the pragmatic approach can be said to have a more *quantitatively* parsimonious account. But this is problematic in two ways. First, even if the proponent of the pragmatic approach countenances fewer conventional meanings than the ambiguity-theorist, she countenances more *implicata*. But why should we prefer the theory that posits fewer conventional meanings even if it posits more *implicata*? Second, why is quantitative parsimony to be valued when it comes to semantic theorizing in the first place? These questions have been given very little attention in the literature.

In answer to the first question, presumably the proponent of the pragmatic approach is thinking that, in order to go from a generalized conversational implicature to a convention, extra ingredients have to be added, e.g. mutual understanding or some such relation between the beliefs and dispositions of those responsible for the regularity in question. Thus the cognitive states and processes that have to be in place for a convention to exist are more complicated than those that have to be in place for a mere regularity to exist.

Let's grant that positing a semantic convention involves countenancing more cognitive states and more complicated relations between different people's cognitive states than the positing of a generalized implicature requires. This still leaves the question of why parsimony should be seen as an epistemic virtue in semantic theorizing. Perhaps the proponent of the pragmatic approach will reply that it is unfair to demand a justification of parsimony, for it is a virtue that is appealed to in scientific inquiry, metaphysics, ethics, and so on. I can see two problems with this reply.

First, even in scientific inquiry, where appeals to parsimony are commonplace, it is far from obvious that parsimony is an *epistemic* virtue as opposed to a mere aesthetic one; van Fraassen [1980: 87–8] is of course the most noteworthy defender of the view that parsimony is not an epistemic virtue of a scientific theory. Moreover, even if there are good reasons for believing that parsimony is an epistemic virtue in scientific inquiry, this doesn't mean that those reasons carry across to the specific domain of semantic theorizing.

For instance, one potential justification of the epistemic import of parsimony within scientific inquiry is the inductive argument that in the past simpler theories have tended to be observationally confirmed, while their more complicated rivals have tended to be observationally disconfirmed. Of

course, the problem for the proponent of MOR is that we just do not have the kind of evidence that would be required to show that pragmatic accounts of the distinct uses of single expressions have tended to be observationally confirmed, whereas those accounts that posit an ambiguity have not. Moreover, ambiguities are rife, and thus it can't be that pragmatic explanations of regular uses have tended to be confirmed whereas those that posit additional senses have not. Rather, the proponent of MOR would have to show that in those cases where the linguistic data were indecisive, the simpler, pragmatic accounts have tended to be confirmed while those accounts that posit ambiguities have not. Needless to say, no one has ever provided such evidence. Granted, once we have agreed upon what goes into a convention, the question as to whether a convention exists for a given use of an expression is an empirical one, and thus, in principle, there is no obstacle to constructing the kind of inductive argument outlined above. However, as long as one has the means for empirically observing whether or not a convention is in place for the given use of an expression, the appeal to MOR and the inductive argument that is meant to underpin it become redundant, for we can just carry out the relevant observations and ascertain whether or not there is a convention in place for the given use.

4.1 Bontly's Domain-Specific Justification of Parsimony

As was mentioned above, justifications of MOR are almost non-existent in the literature. Thomas Bontly is an exception. He provides a 'domain-specific' justification of MOR. Following Sober [1988, 1990], Bontly suggests that 'parsimony arguments function as tacit references to domain-specific process assumptions' where the processes in question are responsible for the phenomenon under investigation [Bontly 2005: 300]. If these processes 'tend to be frugal, parsimony is a reasonable principle of theory-choice' [loc. cit.]. Thus, according to this approach, the reason why parsimony is an epistemic virtue in one domain of inquiry will often be completely unrelated to the reason why it is an epistemic virtue in another.

Which domain-specific process assumptions could justify the appeal to MOR? According to Bontly, the processes involved in *language acquisition* are 'biased toward semantic parsimony and against the acquisition of multiple meanings for single phonological forms' [ibid.: 301]. Bontly sets out to make the case for this claim by outlining the 'child as scientist' model according to which word learning is a process of producing and testing hypotheses about the meanings of unfamiliar words. This process is not repeated with every subsequent exposure to a word—once a hypothesis about the meaning of a new word is accepted, the process stops and the child does not have to hypothesize about the meaning of the word upon subsequent exposures. How is this learning process triggered when a child is exposed to a familiar (sounding/looking) but ambiguous word? Plausibly, it will be triggered when the familiar word is used in such a way that the

familiar meaning does not make sense in the given context. Bontly [302] draws the following moral from this:

Together, these considerations point to the hypothesis that language acquisition is semantically conservative: children will posit new meanings for familiar words only when necessary—only when they encounter utterances that make no sense to them, even though all the words are familiar.

Bontly [302–3] goes on to cite experiments that seem to support the conjecture that language acquisition is semantically conservative, before drawing the following conclusion [303]:

And that, of course, is precisely the sort of process assumption that would make MOR a reasonable principle for theory choice in semantics. For we have been operating under the assumption that the principal task of linguistic semantics is to describe the competent speaker's tacit linguistic knowledge. If that knowledge is shaped by a process biased toward semantic parsimony, our semantic theorizing ought surely to be biased in the same direction.

Let's separate out the two claims Bontly is making here:

- (1) There is empirical evidence for the claim that a language learner will tend to posit a new meaning for a familiar word only when the familiar meaning makes no sense in the given context.
- (2) (1) provides support for MOR.

Now I do not wish to comment on the accuracy of the empirical studies cited by Bontly, so let's just grant (1). The problem I want to raise is with (2), for I don't see how the bias against positing ambiguities described in (1) provides support for the distinct claim that the *semantic theorist* should be biased against positing ambiguities. Take the case of the Russellian and her ambiguity-theorist opponent. Suppose a child is first exposed to the *attributive* use of a definite description and thereby learns the attributive meaning. The child is then exposed to the referential use but recovers the intended message without positing a referential meaning because a pragmatic inference to the intended message suffices for her to make sense of the utterance. In other words, the child does not need to posit a second meaning for 'the *F*' in order to make sense of the utterance in question. But so what? The fact that the *language learner* does not need to posit a referential meaning for 'the *F*' does not mean that the *semantic theorist* does not. In being exposed to the referential use of 'the *F*' the child may not need to posit an additional meaning in order to understand the singular thought being conveyed, but this has absolutely no bearing on the question of whether the extra ingredients that have to be in place for a regularity to count as a convention are present when it comes to the referential use of 'the *F*'. To put it another way, given that the child has not been exposed to referential uses of 'the *F*' before and thus has not been in a position to observe the relevant beliefs, linguistic dispositions, etc., of people in the

linguistic community who regularly use 'the *F*' referentially, the child is just not in a position to know whether or not a convention is in place for the referential use.

5. Conclusion

I have argued that, contrary to what many philosophers think, MOR does not have a decisive role to play in adjudicating the debate between those who would explain the distinct uses of a given expression by positing an ambiguity and those who would appeal to pragmatic features of the relevant speech acts. My argument for this claim has been two-fold. On the one hand, I have argued that there are problems with some of the obvious ways in which to try to justify the epistemic import of parsimony in semantic theorizing—in particular, I raised problems for the inductive argument as well as Bontly's domain-specific justification in terms of language acquisition. Perhaps there are other ways to justify the epistemic import of parsimony, where these justifications are not domain-specific and thus apply to scientific and semantic theorizing alike. Even so, MOR still cannot have the significant role it is frequently thought to have in adjudicating between pragmatic and ambiguity-based explanations of the distinct uses of a given expression, for, as I have argued, in order for MOR to have this significant role it would have to be the case that (1) the linguistic data are indecisive between the pragmatic account and the ambiguity account and (2) the pragmatic account and the ambiguity account are the *best* explanations of the data. But if (1) is true then (2) is plausibly false, for if the data are indeed indecisive between the pragmatic account and the ambiguity account then the best explanation of the data is that it is *indeterminate* whether the expression in question is ambiguous. It is surprising that philosophers have neglected this third option for there certainly does not seem to be a principled reason to rule it out—the notion of a 'convention' seems as sorites-susceptible as the paradigms of vague terms, e.g. 'red' and 'bald'. At the very least, if a significant role is to be found for MOR, the onus is on those who appeal to it to provide a principled reason to rule out the indeterminacy account.⁹

The Graduate Center, CUNY

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