

which, in a way, is just fine and dandy, but unless he is committed to the primacy of idiolects and the authoritativeness of agents regarding their own idiolects, the agent's beliefs regarding precisifications of the vague predicates of her language have no part to play in providing a semantics of vague terms, not that Bennett suggests otherwise, despite calling his paper 'Standpoint semantics'. Speakers of a language can have all sorts of beliefs, *including mistaken beliefs*, about the meanings of expressions in their (native and other) languages. In one sense, these beliefs are distinct from how they take the world to be—represented by some subset of *W*. But for most, if not all, of what we have beliefs about, there are standards of correctness, albeit perhaps transcending all possibility of verification. When Bennett offers us a predicate grounding theory for the colour terms *red*, *orange*, *pink*, *peach* and *purple*, he presents it for all the world as though he were making *approximately correct* observations about English usage. He does *not* report it *merely* as part of his idiosyncratic standpoint (and, frankly, if he had, I doubt I'd have been interested in it).

In addition to the set of precisifications an agent considers to make reasonable assignments to all threshold parameters and the set of predicate grounding theories that characterises all possible definitions of ambiguous predicates that the agent regards as acceptable, there are the set of precisifications of vague predicates in English that make assignments to all threshold parameters and the set of predicate grounding theories that characterises definitions of ambiguous predicates in English *compatible with English usage*. It is these that determine whether the agent's idiosyncratic beliefs about the reasonableness of precisifications and the acceptability of predicate grounding theories are right or wrong. It is these that, conceivably, play a role in the semantics of the vague predicates of English. And, of course, there is undoubtedly an element of vagueness in the determination of which are compatible with English usage.

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## Reply to Peter Milne’s Comments on *Standpoint Semantics: A Framework for Formalising the Variable Meaning of Vague Terms*

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In his comments on my paper, Peter Milne raises a number of interesting points about my semantics, and also makes a number of less interesting and somewhat misleading comments regarding my notation. I shall address what I regard as Milne’s most significant observations in more or less the sequence that they occur in his commentary. Luckily this means I shall start by considering one of the more interesting points.

In his Section 1, entitled ‘Degrees and modality’, Milne draws attention to the fact that, in founding the theory of measurements, strong arguments can be made in favour of the view that all measurement systems must originate from comparative observations of the relative properties of individuals of a domain (e.g. ‘John is taller than Mary’), rather than from intrinsic absolute properties of objects (e.g. ‘John is 6’ tall’). Nevertheless, as Milne agrees, it is often natural to think in terms of the degree to which a particular individual exhibits some property—i.e. the magnitude of some observable associated with the individual. The idea that the state of the world (and indeed any possible world) can be fully described in terms of such magnitudes is fundamental to my semantics, since I explicitly identify possible worlds with valuations of measurement functions.

Milne finds it puzzling that articulating a semantics in terms of magnitudes should appear natural and attractive, whereas the theory of measurements apparently requires that comparative relations should be prior to magnitudes. I am no expert on theories of measurement, and should really learn more about them,<sup>1</sup> but it seems to me that, even though comparisons may be epistemologically prior to magnitudes, this need not necessarily bear direct correspondence to the structure of a semantic model. I see the situation as somewhat analogous to that of Euclidean geometry, where our perceptions of geometrical configurations of points are typically described by relational terminology (such as ‘point  $x$  lies between points  $y$  and  $z$ ’), whereas the standard Cartesian model associates each point with a tuple of real numbers.

I now consider Milne’s Section 2, where he recapitulates my representation. One point that he picks up on concerns my stipulation that the values of measurement functions should be rational numbers, whereas the values of threshold parameters must be irrational. I originally took this approach for purely technical reasons because I wanted to avoid the possibility that a measurement value should exactly coincide with a thresh-

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<sup>1</sup>From my cursory reading of Milne’s references, they seem to be an excellent starting point for studying this topic.

old which would lead to an awkward complication in the specification of the truth conditions of vague predications. It is likely that the semantics could be formulated differently, such that measurement functions and thresholds both have  $\mathbb{R}$  (or perhaps both have  $\mathbb{Q}$ ) as their value domain. However, I am still inclined to the view that separating the domains is both natural and technically advantageous. Moreover, I think that many physicists would reject the idea that physical objects have intrinsic properties whose values are real numbers, and are possessed independently of any actual measurement.

In the remainder of Section 2, Milne proceeds at some length to take issue with some minor aspects of my notation. If these criticisms were justified, I would describe them as pedantic. But, as it happens the criticisms are both unjustified and misleading. Milne's main bone of contention seems to be that in specifying a structure to support the semantic interpretation of a logical language, I often present the structure in terms of a tuple, in which I include not only interpretation functions but also the symbol set or sets that are to be interpreted. This enables me to specify the interpretation functions (which form the meat of the semantic structure) in a self-contained way, since their domains are explicit in the tuple rather than given in some external specification of the vocabulary.

Milne considers this to be mixing syntax and semantics, which of course would not be a good idea. But Milne's use of the term 'syntax' is sloppy and confusing. Symbol sets are *vocabulary* not syntax. Syntax concerns the ordering of symbols into meaningful expressions, and semantics concerns the meaning of these symbols and expressions. Many well known and highly rigorous logic texts<sup>2</sup> present formal semantics in a style similar to my own—i.e. without the use of 'signatures' and with symbols of the object language incorporated into the model structures. Typically, in expositions of this kind, the vocabulary sets are not specified as top-level elements of a model tuple but are instead referred to as domains of the interpretation functions in the tuple. But the symbols of the vocabulary are none the less included in the model, since these functions will correspond to sets of pairs, with a symbol being the first element of each pair. Moreover, in presenting algebras and algebraic semantics, it is very common to incorporate the signature explicitly into the model structure specification.

Just to check that my notation is not hopelessly antiquated and out of line with current thinking, I re-read the beginning of Wilfred Hodges excellent 'A Shorter Model Theory' (1997, Cambridge University Press). On page 2, in his definition of 'structure' Hodges makes it very clear that he considers the symbols to be interpreted as essential ingredients of a structure. Then on page 4 we find the following remarks about the particular way a structure should be formalised:

Exactly what is a structure? Our definition said nothing about the way in which the ingredients ... are packed into a single entity. But this was a deliberate oversight—the packing arrangements will never matter to us. ... The important thing is to know what the symbols and the ingredients are, and this can be indicated in any reasonable way.

<sup>2</sup>To take just one classic example, Hughes and Cresswell's 'An Introduction to Modal Logic', 1969, Methuen.

Hodges follows this by an example of a structure  $\langle \mathbb{R}, +, -, \cdot, 0, 1, \leq \rangle$ , saying that  $+$ ,  $-$ ,  $\cdot$ ,  $0$ ,  $1$ , and  $\leq$  are symbols naming functions, constant elements and relations over the domain  $\mathbb{R}$ . In Milne's terminology this would be described as a "ghastly mélange of syntax, interpretation and signature".

Milne (who seems to have a very broad idea of perversion) claims further that my notation is perverse because "it rules out the possibility that sentences of different languages have the same interpretation". Of course this is true, since a measurement structure gives an interpretation for a particular set of function symbols. But this is normal in model theory. As Hodges says [p4]: "We shall assume that the signature of a structure can be read off uniquely from the structure", and indeed, Hodges usually refers to any particular structure as an  $L$ -structure, with  $L$  being its signature. Of course we may still want to say that two languages have isomorphic interpretations, where the isomorphism is characterised by a bijection between symbols of the two languages. Indeed the notion that sentences of different languages could have the same interpretation only makes sense in relation to a particular translation establishing a correspondence between the symbols of each language. (Such bijections can be specified by means of signatures, but there are other ways to do this.)

It is true that one can separate language vocabulary from semantics by regarding a language *signature* as providing an enumeration of symbols of each syntactic category. An interpretation can then be cast in terms of these enumerations (i.e. as a mapping from natural numbers to denotations), rather than the symbols themselves. This formulation of semantics may be advantageous for certain purposes, but such technicalities bear little relevance to the points I was trying to make in the exposition of my semantic framework.

I now return to more substantial issues. In his Section 3.2 Milne examines the three dimensions of variability in the interpretation of vague predicates that are modelled my system. He notes that the grounding theory parameter  $\theta$ , which is supposed to model vagueness of the conceptual ambiguity variety, could potentially impact upon variability that one would normally regard as sorites vagueness. This is true. However, as Milne also notes, the  $\theta$  parameter is not intended to range over arbitrary predicate grounding theories but only some particular non-empty set  $\Theta$  of grounding theories. Perhaps I did not make it clear enough that I am assuming that the theories in  $\Theta$  are carefully constructed in order that the semantics only allows a range of 'reasonable' groundings for each vague predicate. It is  $\Theta$  that enforces the specific semantics (what Montague would call 'meaning postulates') of the system.

Finally, we come to Section 3 of Milne's commentary, concerning my notion of 'Standpoint'. Milne suggests that a standpoint may be regarded as characterising an agent's idiolect, i.e. their personal attitude to language and its semantics, and rightly points out that the view that semantics is determined primarily by idiolects is untenable, since the expressions of a natural language must have meaning independently of any particular agent's personal beliefs and idiosyncratic attitude to the meaning of terms.

It seems I did not sufficiently explain my notion of standpoint, since I do not intend standpoints to correspond to the idiolects of particular agents. What I call a 'standpoint' corresponds not to the general linguistic dispositions of an agent, but to a particular attitude held at a particular time in a particular situation. Thus an agent can and often will change their standpoint depending on the situation at hand and the kind of information

they wish to convey. Answering the question of when and why an agent would adopt a particular standpoint in a particular context is of course crucial if one wants to move beyond my standpoint-relative semantics, to a broader account of the semantics of vague terms. In fact I have already directed considerable attention to this question, and have made some progress in formulating a more general framework, within which the standpoint of a particular agent in a particular situation can be evaluated in relation to a corpus of linguistic acts representative of the patterns of vocabulary usage in a community of agents sharing a common language. This generalised standpoint semantics involves the introduction of a level statistical machinery on top of the semantics that I have presented in my contribution to this volume.