

## Semantic Accounts of Vagueness\*

*Richard G. Heck, Jr.*

Crispin Wright's reflections on vagueness have, for nearly thirty years now, been a touchstone for all serious work on it. His most recent efforts, beginning with the paper "On Being in a Quandary" (Wright, 2001) and continued in this volume (Wright, 2003), seem to me extremely important. What I want to do here, however, is not to try to evaluate his new position, nor even to engage it directly, but instead to defend one of the more traditional views he criticizes, namely, the view that vagueness is a 'semantic' phenomenon. I am not at all sure this sort of view is right, but I think we need a better sense than we presently have what might be wrong with it if we are to discover a better view. I will begin, however, by discussing another question that has obviously exercised Wright a great deal, namely, what is really wrong with Epistemicism and other views that are committed to the retention of bivalence for vague predicates.

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### 1 Do Vague Predicates Have Determinate Classical Extensions?

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Wright worries that Epistemicism has an unpaid, and unpayable, meta-semantic debt: It owes an account of what fixes the 'sharp boundaries' of (particular uses of) vague predicates. Many have voiced this concern, but, to the best of my knowledge, no principled reason has yet emerged to believe that no such account will be forthcoming. In the present paper, as elsewhere in his writings—and Wright is not unusual in this regard—we find little more than a profession that Wright is unable to see even the shape of a decent account. I can't see it either, but I'd still like to try to do better, to offer (what I take to be) a principled reason to doubt that vague predicates have determinate classical extensions.

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Most reflections on this problem begin (and end) with the thought that it must be our use of ‘heap’, ‘red’, and ‘chair’ that, somehow or other, fixes their extensions. Williamson (1994, §7.5) does a fine job arguing, however, that, if what is intended here is some sort of supervenience thesis, then it poses no threat to Epistemicism. So more has to be said. A second thought might be this one. By hypothesis, our linguistic dispositions leave certain sorts cases undecided, namely, the cases that we call borderline cases—these, obviously, being the ones that cause all the trouble. And it is hard to see what, if not our linguistic dispositions, is supposed to decide them. But Williamson could simply dismiss this sort of concern, too. It is based upon a conception of how the extension of a predicate is fixed by its use that is far too naïve to take seriously. The use of a predicate *never* fixes its extension by deciding every case directly. Still, the question remains what does fix it. I’ve heard it suggested that we should just ‘split the difference’: Take the boundary between the red and the not-red to lie exactly between the things our use of ‘red’ definitely determines should be so-called and those it definitely determines should not. But, for one thing, that’s uncomfortably *ad hoc*, and, for another, it is more easily applied to ‘red’ than it is to ‘chair’. I’ll raise a deeper worry about it later.

In one of his discussions of this matter, Williamson (1994, p. 213) writes, regarding the predicate ‘heap’, that its extension is not so hard to determine as one might think.<sup>1</sup> The *Oxford English Dictionary* says, amplifying a bit, that a heap is “[a] collection of things lying upon one another so as to form an elevated mass” | and in which some of the objects are completely and stably supported by others. Not much reflection is needed to see that the minimum number of grains of sand needed to construct a heap is therefore *four*: three on the bottom, and one sitting on top of them. It’s easy to get the sense Williamson’s tongue must have been firmly in cheek when he made this suggestion, and he is clear enough that he does not expect to be able to make similar remarks about every vague predicate. I am going to suggest, however, that his inability to do so is, ultimately, what underlies the impossibility of his answering the meta-semantic question what fixes the extension of a vague predicate. 108

Wright remarks toward the end of his paper that “the one reasonably clear model. . . we have of how the property presented by a predicate may not be transparent to those who fully understand that predicate [is] the

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<sup>1</sup> The argument originates with W. D. Hart (1991).

model of lay natural kind terms like 'water'..." (Wright, 2003, p. 104). The thought here is simple and, I think, compelling. Whether something is water is not decided entirely by our linguistic dispositions, that is, by our responses to putative samples of water. It is not even decided by how we would respond in epistemically ideal conditions. Our use of the term 'water' is 'focused' upon a particular property, and the world itself decides whether a particular bit of stuff has or fails to have that property. How we react to putative samples of water is, of course, part of what focuses our use of the term upon the property of being water. And so, in that sense, there is no threat here to the intuitive thought that use determines meaning. But, at the same time, we can easily imagine ourselves being, even under ideal epistemic circumstances, wrong about whether something is water, and for that reason it could well be, and remain, a mystery just where the boundary between what is and is not water lies.

Williamson's remarks about the word 'heap' apply this model to it. What our linguistic dispositions leave unresolved may be resolved by the world itself, for there is a particular property on which our use of the term 'heap' focuses: Our day-to-day use of the term—although it does leave borderline cases unresolved, in one sense—is focused upon a particular property, and the world itself decides whether a given object has it. One might speculate, then, that something similar should be true of color words, like 'red': Perhaps there is a particular property (be it physical, dispositional, or what have you) on which our use of *this* term is focused. Then, once again, the world itself could be left to decide, of any given object, whether it had that property, and our ignorance about where the boundary between what is and is not red lay would be no more puzzling than our ancestors' ignorance about where the boundary between what is and is not water lay.

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There may be some cases to which this model can appropriately be applied. But the strategy will not generalize to all cases of vague predicates. (Williamson does not suggest that it will.) So far as I can see, there is no mind-independent property that our use of the term 'chair' might plausibly be thought to pick out: Chairs form not a natural kind but an artifactual one, and they have no real essence, but only a nominal one. Even the case of 'red' is problematic. Color words like 'scarlet' and 'vermillion' are all the more so. Worse yet, most of these—and certainly such terms as 'flat'—exhibit context-dependence, as Diana Raffman, Delia Graff Fara, and others, have rightly emphasized. The fact that such terms can be used in different contexts to pick out different prop-

erties makes it obvious that there is not going to be a property that does for our use of 'flat' what the essence of heaphood might do for our use of 'heap'.

I began this discussion by quoting Wright's suggestion that natural kind terms provide us with "the one reasonably clear model...we have of how the property presented by a predicate may not be transparent to those who fully understand that predicate". We have, however, come close now to a stronger conclusion. As I said earlier, our use of the term 'water'—whatever precisely might be meant by 'use' here—does not decide *directly* how the term should be applied in every case: It is not as if every putative sample of water has been examined and determined either to fall within or without the extension of the predicate. We might then ask why, even in the case of a term like 'water', we should believe that every such sample is either one to which the predicate should be applied or one from which it should be withheld. The answer, it seems to me, is again that, although our use of the predicate certainly does not decide directly, of every such sample, whether the predicate should be applied to it, our use of the predicate does focus upon a particular property, and the *world* then decides whether the sample has the property or not. Our use therefore decides *indirectly* whether the predicate applies to a given sample: Our use focuses the predicate on the property, and the world decides whether the sample has the property.

Imagine now a world, otherwise like our own, *circa* 1500, but in which there is no water—one in which there is, moreover, no XYZ nor any other natural kind of stuff that fills the lakes and rivers. In this world, what fills the lakes and rivers is a motley collection of fluids having little more in common, chemically speaking, than the things we call 'fabric'. Now, supposing the use of the term 'water' in this world to be pretty much the same as it was in our world *circa* 1500, does this term have a determinate classical extension in this | world? There is a temptation to say it would: It would have applied to all the odorless colorless liquids, or something along those lines. But I see no particular reason to believe that. In this case, our linguistic dispositions certainly would not decide every case directly. There would be plenty of putative samples of water that were, in some ways, very similar to the other things we call 'water', and in other ways not quite like them, and there would be no chemical or other natural basis on which to draw a boundary. It seems to me, then, that the lesson of these sorts of examples is not just that the extension of such terms as 'water' is fixed by the world—so that, had the world not been co-operative, it would have been fixed entirely

by our linguistic dispositions. It is that *only* the world can fix a determinate classical extension for a predicate.

It would take more space than I have here to establish this claim, not to mention more and better arguments than I have available. But the basic idea is fairly simple and, I think, plausible. If we consider the matter just in terms of the possible extensions for the term ‘water’, then it is very difficult to see how one rather than another of the possible extensions could be uniquely determined as the extension of this term. But our use of the term ‘water’ is, as I put it before, focused on the *property* of being water, and the world itself is then left to decide what has this property. If properties were as common as extensions, of course, then reference to them wouldn’t help. But they aren’t. The property of being water is the chemical property of being H<sub>2</sub>O, and such properties are, so to speak, *sparse*. It is, for that reason, much easier for our use of the term ‘water’ to focus upon a specific property than it is for our use of the term to pick out a particular extension. It is only because the property of being H<sub>2</sub>O stands out from the crowd, as it were, that our use of the term focuses upon it: Our use of the term is, as it were, *drawn* to this property, because it is the only one in the neighborhood. If, as in the example mentioned above, the word ‘water’ did not pick out a natural kind, there simply wouldn’t *be* a natural property on which our use of it might focus, and its extension would be left indeterminate.<sup>2</sup>

What I am suggesting can be put this way. To ask why vague expressions fail to have determinate classical extensions is to ask the wrong question. The great mystery is how any expression ever comes to have a determinate classical extension. And so far as I know, the only plausible resolution of this mystery appeals essentially to the idea that the candidate extensions are | sufficiently scarce that our use of a given predicate may, again, be ‘drawn’ to one or another of them—the candidate extensions being the extensions of *properties*, these themselves being scarce. If the set of candidate extensions for the predicate were, on the contrary, plentiful, then there would be nothing in our use of the predicate that could distinguish one of them from all the others. 111

Let me say again that the preceding is not really intended as an argument. It has, rather, the status of a conjecture, one I think has some plausibility. But a similar line of thought is, it seems to me, particularly compelling in the case of vague predicates that exhibit context-

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<sup>2</sup> The intuitions I am exploiting are used to great effect by David Lewis in his paper “Putnam’s Paradox” (Lewis, 1984).

dependence. Consider, for example, a particular utterance of ‘He is tall’. For concreteness, assume a degree-theoretic account of adjectives of this sort (Kennedy, 1999). Such adjectives are associated with scales—in this case, a scale of height—and the context-dependence of such adjectives is explained in terms of the fact that ‘tall’ means roughly: of a degree of height greater than  $\delta$ , where  $\delta$  is a contextually-determined degree of height. Context is thus obliged, in any given case, to fix a point along the scale that will divide the tall from the not-tall.<sup>3</sup> So the question is: What reason do we have to suppose that, in typical cases, context is always sufficient to fix a particular degree of height dividing the tall from the not-tall? None of the degrees is, in any way, more ‘natural’ than the others. So the job context must do is very difficult indeed. I simply see no reason to suppose that ordinary contexts fix unique such degrees, nor even that they fix the degrees precisely enough to decide, of every object in some contextually relevant domain, whether it counts as tall or not. What I suspect, rather, is that context *restricts* the set of degrees as far as is needed for conversational purposes and that further such restrictions are negotiated as they become necessary.<sup>4</sup>

It is, indeed, hard to see why context ought to be expected to do any more than that. There is no *a priori* reason to suppose that every context is sufficient to fix a precise such degree—any more than there is *a priori* reason to suppose that every context is sufficient to fix the referent of the demonstrative ‘he’. If so, however, we should ask whether such an insufficiency, if it were to arise in a particular case, would have to frustrate the purposes of the conversational participants. In the case of ‘he’, the answer is that it typically would: If there is | no fact of the matter about to whom the speaker’s use of the word ‘he’ refers, then, presumably, one would expect no agreement among the conversational participants about to whom it refers, and that will lead to communication breakdown. But, in the case of ‘tall’, so far as I can see, no such breakdown need be expected. If context is insufficient to decide whether, say, Bob counts as tall in it, then, if it *matters* whether Bob counts as tall, there will be a problem. But it need not matter, and there need not be a problem. 112

At this point, there is a natural objection. Consider some vague predicate, say, ‘chair’, and suppose what I’ve said so far is correct: There are

<sup>3</sup> On Fara’s view, vague predicates are not really context-dependent, but the difference does not really matter for present purposes.

<sup>4</sup> The ideas I am expressing here thus have some similarities to those expressed by Jamie Tappenden (1993).

many classical extensions this predicate might have consistent with its use. Then the thought is that we should simply take the extension of ‘chair’ to be the intersection of these various possible extensions, that is, the set of objects of which it is determinately true, so that it is false of any object of which it is not determinately true. The idea that we should ‘close off’, as I’ll put it, borrowing the term from Kripke (1975), is a common one.<sup>5</sup> The suggestion is a non-starter, however, at least if it’s offered against the background I’ve just outlined. Suppose we have some independently motivated theory of what fixes the reference of a predicate and that, according to that theory, there are different extensions the predicate might have, consistently with the facts concerning its use. To suggest, in this context, that we just close off would be to abandon the theory I’m assuming we have. The closed-off extension may well be one of the possible extensions for the predicate, but, by hypothesis, the facts concerning the use of the predicate do not determine that the closed-off extension *is* the extension of the predicate. Closing-off is just an *ad hoc* construction of no semantic relevance. The same is true of the suggestion mentioned earlier, that we should just ‘split the difference’.

As it happens, the actual closing-off construction in Kripke illustrates this point. Let us recall how it works. On Kripke’s theory, the ‘use’ of the truth-predicate is completely captured by four rules of inference, the so-called T-rules:

$$\begin{aligned} A &\vdash TA \\ \neg A &\vdash \neg TA \\ TA &\vdash A \\ \neg TA &\vdash \neg A \end{aligned}$$

The proof of the fixed point theorem then shows that there is an extension the truth-predicate could have, consistent with these rules and, indeed, that there are infinitely many such extensions. One might think, of course, that there is more to our ‘use’ of the truth-predicate than just these four rules—something that has the effect of requiring truths to be grounded, for example—so that a unique extension is determined from among the fixed points, after all. But the suggestion of interest to us is another one Kripke makes: That we ‘close off’ by taking the classical

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<sup>5</sup> The suggestion is made explicitly by Williamson (1994, p. 208). Even Wright seems sympathetic with it, using something similar as an objection to what he calls Third Possibility (Wright, 2003, p. 98, fn 10).

extension of the truth-predicate to be the intersection of its extensions at the various fixed points—which, since there is a minimal fixed point, is just to take its (classical) extension to be its extension at the minimal fixed point.

I remember George Boolos defending this view in a class on truth. "So", said George, "the liar sentence is not true. Neither is its negation." I remember myself objecting, "George, you have just uttered the liar sentence! And you have simultaneously said that it is not true." George responded by asking me if I was calling him a liar. "Is that a moral criticism?" he asked, grinning broadly. I didn't know what to say then, but I do know now, namely, that the closing-off construction *begins* with the thought that the extension of the truth-predicate is fixed, if by anything, then by the T-rules. To close off is simply to abandon that idea. Indeed, it is to deny that the T-rules are even *valid* (and that is what was fueling my objection). Closing-off thus leaves the original construction of the minimal fixed point—the construction that determines the extension of the truth-predicate—unmotivated. Why should we care about the extension of the truth-predicate at the minimal fixed point if the T-rules are not even valid?<sup>6</sup>

I do not say that closing-off, in the case of vague predicates, would suffer from exactly this problem, though it might, in some cases: The intersection of all the admissible classical extensions could, in some cases, prove to be something an independently motivated theory of what fixes reference actually determined *not* to be the extension of the predicate. But, in most cases, the problem will not be that bad. Nonetheless, closing-off will always be *ad hoc*. To 'close off' is, once again, to take the intersection of a certain class of possible extensions for a predicate. So the closing-off construction *begins* with some set of extensions that our use of the predicate does not *rule out* as possible extensions. If so, however, then, by hypothesis, there are extensions the  $\perp$  predicate could have, consistent with our use of it, other than the one delivered by the closing-off construction. But if the closing-off construction is what determines the extension of the predicate, then those other extensions *aren't* really extensions the predicate could have, consistent with our use of it, and it is unclear what their status is. 114

<sup>6</sup> There is an independent way to get what is, in effect, the same theory as one would get by closing off: The theory in question is KF, the Kripke-Feferman theory. It may have some independent motivation, but my point here is simply that it *needs* some independent motivation.



## 2 Indeterminacy and the Appeal of the Sorites

I have argued, then, that typical vague predicates—certainly ‘chair’ and ‘tall’, though maybe not ‘heap’—fail to have determinate classical extensions. You will note that I have been careful not to say that their extension is *indeterminate*. If we understand indeterminacy as Supervaluationism, for example, would understand it, then it does not follow from what I have argued that the extensions of vague predicates are indeterminate. Wright’s view is, obviously, consistent with what I have been arguing, but he in particular would not want to make this claim. What I have argued could thus be put, neutrally, as follows: Although there are some classical extensions that a given vague predicate could not have, there will be many that it could have, consistent with the facts involving its use, even if use is conceived, as I think it should be, broadly, as involving relations with the world.

There are, obviously, a variety of ways to respond to this observation. One would be to regard vague predicates as having indeterminate extensions: A view of this sort is what Wright calls a semantic view of vagueness. Another view, Wright’s own favored view, is that the classical model breaks down here. The classical notion of an extension (more generally, classical semantics) is simply inapplicable to the case of vague predicates; something like the intuitionistic notion of a species (more generally, intuitionistic semantics) is needed here. As it happens, this view is one I’ve also suggested, though in connection with Evans’s argument that there are no vague objects, not the Sorites (Heck, 1998) But I’ve recently begun to wonder, myself, whether Supervaluationism is quite as lifeless as it is routinely claimed to be. In the remainder of this paper, then, I’d like to try to motivate a version of Supervaluationism and to defend it against Wright’s criticisms.

Before I begin, I want to emphasize one point. One sometimes gets the sense from the literature that what we need is *the* solution to the Sorites | paradox or *the* correct theory of vagueness. But what we regard as vague predicates, or even Sorites-susceptible predicates, may not form a semantic kind. If not, then different accounts will be needed of what is responsible for the apparent vagueness from which such predicates suffer—the presence of borderline cases, if that is what is distinctive of vagueness. One might hold, on broadly methodological grounds, that a unified theory would be preferable if one were available. But if one considers the full range of vague expressions, from adjectives like ‘tall’ to common nouns like ‘chair’ to verbs like ‘to shout’, then I see no a

*priori* reason myself to expect a uniform account. I wish, then, to defend the coherence of Supervaluationism not because I think it *the* solution to the Sorites paradox, but because I see no reason to doubt that semantic vagueness is a real phenomenon. Indeed, I think it likely, as I said above, that many uses of context-dependent vague predicates are semantically vague.

Wright suggests that semantic treatments of vagueness begin with the thought that vagueness “originates in shortfalls... in the meanings we have assigned to expressions” (Wright, 2003, p. 85). On my view, that is almost right. For emphasis, the view should be that *some* vagueness originates in semantic insufficiency. A more important point is that the shortfall need not concern the *meaning* of the expression, if by that is meant something at the level of Fregean sense. The view is that there is an indeterminacy regarding the *extension* of the predicate. But there is no obvious reason to suppose that this indeterminacy need be due to some indeterminacy regarding the predicate’s meaning, in the intuitive sense. Failure to appreciate this point is, I think, behind at least some objections to Supervaluationism.<sup>7</sup>

Semantic indeterminacy, however, is not vagueness, for the mere fact that it is indeterminate what the extension of a predicate is is plainly no reason to expect that it will give rise to the sorts of paradoxes to which vague predicates typically do. One should not, however, overreact to this observation. It does not give us reason to reject indeterminacy as a fundamental part of the theory of vagueness (at least for some expressions—a qualification I shall henceforth drop). Minimally, to be sure, a story needs to be told about what the indeterminacy of vague predicates has to do with the Sorites paradox. That question is equivalent, of course, to the question what vague predicates’ indeterminacy has to do with the appeal of the crucial assumption that, say, if one thing is red, anything pairwise indistinguishable from it is also red.

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What *is* the appeal of that premise? In his earliest writings on this topic, Wright (1976) suggests that it originates in a conception of the kinds of grounds one could have for judging that something is red. One would ordinarily suppose that whether something is red is the sort of thing one can tell by looking. If so, then surely you can’t have two things you can’t tell apart by looking one of which is red and the other of which is not. But it seems to me that the basic intuition here doesn’t really support that claim. Rather, the basic intuition is that a certain com-

<sup>7</sup> Such as those of Fodor and Lepore (2002).

combination of views is irrational: One cannot simultaneously hold that  $a$  is red, that  $b$  is not, and that  $a$  is pairwise indistinguishable from  $b$ .<sup>8</sup> That is already enough to get something like Sorites reasoning started. Faced with a chip I agree is red and another I agree is pairwise indistinguishable from it, it is easy to feel some compulsion to say that the second chip must also be red. After all, it would be irrational to say it wasn't. But, in fact, that move can be resisted. One might simply have no view about whether the next patch is red, and there's nothing irrational about that.

One might object that it *is* irrational to hold that  $a$  is red and that  $b$  is pairwise indistinguishable from  $a$ , but to refuse to commit oneself to the claim that  $b$  too is red. After all, one might say, one has the very same evidence that  $b$  is red as that  $a$  is red. I do not deny that the objection has some force, but it seems to me that one does *not* (or at least need not) have the same evidence that  $b$  is red. Why might one think otherwise? The evidence one has is simply how  $a$  and  $b$  look. But, one might suppose, if  $a$  and  $b$  are pairwise indistinguishable, then surely they look the same. But, of course, this move must be resisted, lest we find ourselves committed to the conclusion that all patches look the same. More positively, though, the evidence one has that  $a$  is red will, in a typical case, be how it appears to one: that is, one's evidence is one's perceptual experience of the patch—more precisely, what one's experience of it represents about its color. We can identify one's evidence that the patch is red, then, with the representational content of one's perceptual experience of the patch (so far as its color is concerned). I see no reason to suppose that, if patches  $a$  and  $b$  are pairwise indistinguishable, then the representational content of one's perceptual experience of  $a$  and of  $b$  (so far as their color is concerned) cannot differ. One might not *realize* that the content differed in the two cases, but that is another matter.<sup>9</sup>

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Sorites-type paradoxes can be resolved by distinguishing between their major premises—for example: if a patch is red, then any patch pairwise indistinguishable from it is also red—and what we might call

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<sup>8</sup> A similar observation has been made by Raffman (1994). As will be clear, I end up making something rather different of the observation, which, in its present form, seems to have been in the air for some time.

<sup>9</sup> I suppose one might feel some threat to first-person authority here, but I don't think we have any first-person authority about the contents of perceptual states, in the relevant sense.

an expression of the quasi-tolerance<sup>10</sup> of the relevant predicate: if one regards a patch as red, and regards another as pairwise indistinguishable from it, one cannot rationally regard the latter as not being red. The quasi-tolerance of 'red' derives from its observationality: Other observational predicates can therefore be expected to satisfy similar principles. Non-observational predicates that are susceptible to Sorites-type reasoning will satisfy similar principles, but these will derive from other aspects of their use.<sup>11</sup>

This interpretation of the appeal of the Sorites paradox is available to philosophers with many different views of vagueness. It is, for example, similar in feel to the margin of error principles deployed by Williamson (1994, §8.3).<sup>12</sup> But there is an important difference, one that can be revealed by considering a version of his example of the crowd. Say I'm seated at Fenway Park and someone asks me how many people are there for the ballgame. To hold any view, on the basis of a glance at the crowd, about exactly how many people were present would be unjustified. But it isn't as if *any* view about exactly how many people were present would be unjustified not matter what its basis. On the contrary, one could lock the doors and count everyone and then one could determine exactly how many people were there. In the case of vagueness, though, the intuition is different. It isn't just that one can't know *on the basis of casual observation* that one patch is red and one pairwise indistinguishable from it is not. This combination of views seems irrational whatever its basis. And surely Williamson would agree: He thinks it is impossible for us to know where the boundary is between what is red and what is not. But the example of the crowd and the margin of error principles it generates are insufficient to ground that conclusion. It's easy enough to see why the margin for error required for knowledge of color is greater than pairwise indiscriminability *if the ground on which one makes one's judgement is observational*. If I judge that patch A is red and do so on the basis of how it looks to me, then, plausibly, I cannot *know* that patch A is red if I would have been wrong had it been indiscriminably different.

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But if there is a sharp boundary between what is red and what is not, why can't there be some other way to know where it is? If one

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<sup>10</sup> The notion is similar to but different from the notion of *tolerance* that plays an important role in Wright's early work (Wright, 1976).

<sup>11</sup> The classic discussion of how such principles derive from aspects of our use of certain predicates is due to Wright (1976).

<sup>12</sup> As mentioned above, similar notions are found in Fara (2000) and Raffman (1994).

were to judge on some other ground, the margin of error principle just stated would not apply. One might reply that, since the location of the boundary is fixed by facts about casual observation, if its location can't be determined by casual observation, then it can't be determined at all. But that simply doesn't follow. How one might find out whether something is red is no more restricted by what fixes the extension of 'red' than how one might find out whether something is a virus is restricted by what fixes the extension of 'virus'—whatever that is.

Contextualists offer a different sort of answer. On Fara's view, for example,<sup>13</sup> if one sets out to find the boundary between what is and is not red, then, as one shifts one's gaze from one part of a Sorites series to another, the extension of the predicate 'red' itself shifts, in such a way that the boundary moves away from where one is looking. So even if, say, the boundary at time  $t_0$  were between patches  $p_{237}$  and  $p_{238}$ , if one were to look at these patches at time  $t_1$ , it would move to, say, between  $p_{125}$  and  $p_{126}$ . And if one looks there, it will move again. There is, as Fara puts it, always a boundary, but it will never be where one is looking for it. But this view too simply fails to answer the question why we cannot locate the boundary between what is and is not in the extension of 'red' *as that term is used at time  $t_0$* . Suppose I say, at  $t_0$ : Some of these patches are red; call them the reddies. I might then ask which is the last of the reddies. By hypothesis, it is  $p_{237}$ . Fara tells us that, if we look there, the boundary between what is and is not in the extension of 'red', as we would then be using it, would shift, so that it will not, at time  $t_1$ , be true to say that  $p_{237}$  is red and  $p_{238}$  is not.<sup>14</sup> But the question is not whether we can then say |that one is red and the other isn't. The question is why we cannot locate the last of the reddies. Maybe the extension of the word 'red' as we would then be using would indeed shift, but the point

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<sup>13</sup> I include Fara here, since, though her view is not, ultimately, contextualist, it is similar in relevant respects. Raffinan (1994) and Shapiro (2003) hold similar views.

<sup>14</sup> There are some subtle questions here about how exactly this point should be expressed on Fara's view. It seems uncomfortable to say that a patch whose color has not changed is no longer red at  $t_1$  but was at  $t_0$ , on the ground that our *interests* have changed. Something like that is, however, what Fara's view entails. Temporal rigidification on our interests may allow her to avoid a direct conflict with semantic intuitions, since she could then deny that we can truly say, in natural language, something like: "Although this patch is now red, in five minutes it will not be, even though its color will not change". A similar problem arises with modal contexts: "If we had had different interests, this patch would not have been red, even though its color would not have changed". Modal rigidification will allow Fara to avoid that consequence, but it causes other problems (Stanley, 2003).

does not seem relevant. There is no such shift in the extension of ‘the reddies’.

The point here can be put somewhat differently. Contextualists explain the appeal of Sorites reasoning as follows. When we consider any particular Sorites conditional, say ( $C_{125}$ ):

If  $p_{125}$  is red, then  $p_{126}$  (from which it is pairwise indistinguishable) is also red,

principles that govern how context fixes the extension of ‘red’ guarantee that the conditional will be true, for those principles imply that no two salient color patches whose pairwise indistinguishability is salient can be differently characterized in respect of color. But there is a boundary between that to which ‘red’ applies and that to which it does not—say, between  $p_{237}$  and  $p_{238}$ . But when we come to consider ( $C_{237}$ ):

If  $p_{237}$  is red, then  $p_{238}$  (from which it is pairwise indistinguishable) is also red,

the same principles will guarantee that it is true. The extension of the term ‘red’, however, will have shifted. Thus, the Sorites premise:

For every  $i$ , if  $p_i$  is red, then  $p_{i+1}$  is also red

has a certain kind of appeal: Whenever we consider one of its instances, the rules that govern how the extension of ‘red’ is fixed by context will guarantee that instance’s truth. But the Sorites premise itself is false. To think otherwise is simply to equivocate. The difficulty is that this strategy does not generalize. Confronted with the sequence of patches, I might say:<sup>15</sup>

Some of these patches are red. I wonder which of them is the last? It seems hard to imagine that there could be a last one. After all,  $p_1$  is one of the red ones. But since  $p_2$  is pairwise indistinguishable from it, surely it too is one of them. But  $p_3$  is pairwise indistinguishable from it, so surely it too is one of them. And  $p_4$  is pairwise indistinguishable from it, so surely it too is one of them. Etc., etc.

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<sup>15</sup> This observation was inspired by an argument due to Jason Stanley (2003). His argument makes use of VP elipsis and so does not apply to Fara’s view, but only to more straightforwardly contextualist views like Raffman’s.

Contextualists have no explanation of the appeal of this reasoning: The use of the anaphor ‘them’ fixes the collection of objects that is at issue, much as the introduction of the term ‘reddies’ did above, so that no contextual shift can occur as we proceed through the series. In principle, to be sure, one could respond that this particular reasoning is not as compelling as the usual sort of Sorites reasoning, or perhaps insist that it is compelling only because we confuse it with ordinary Sorites reasoning. But I see no plausibility in such responses.<sup>16</sup>

These might seem fiddly technical objections meriting fiddly technical responses. In fact, however, they go to the heart of the contextualist strategy. Contextualism is the view that what we experience as vagueness is a form of context-dependence.<sup>17</sup> And the first-blush response that almost everyone seems to have to it is: OK, *fix* the context; the extension of ‘red’ *in that context* is still vague. The objections just sketched simply refine this intuition. I suggest that they show that no view that attempts to disarm the Sorites by appealing to context-driven changes in the extension of the relevant predicate can succeed. The Sorites reasoning is just as appealing when one nails the extension down as it is when one allows it to vary.<sup>18</sup>

Now, as I said above, the fact that vague predicates lack determinate classical extensions cannot, by itself, explain the appeal of the Sorites premise. But I think we can now see that to think it did would be to get things precisely backwards. It is not the indeterminacy of ‘red’<sup>19</sup> that explains the appeal of the Sorites premise but the appeal of the Sorites premise—the quasi-tolerance of vague predicates—that explains, at least in part, why ‘red’ lacks a determinate classical extension. That one cannot rationally regard a patch as red and another pairwise indistinguishable from it as not red implies that there will be border-  
line cases—cases in which neither the view that an object is red nor  
the view that it is not red is rationally compulsory (or, perhaps, even  
rationally defensible). If so, then our ordinary use of ‘red’ will fail to re-

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<sup>16</sup> Another possibility would be to insist that ‘them’ is a pronoun of laziness, or that the reasoning is only appealing if it is. I see no plausibility there, either, however, and, in any event, that response is not available to the version of the argument using the term ‘reddies’.

<sup>17</sup> Again, Fara’s view should not really be so described, but the differences do not matter for present purposes.

<sup>18</sup> The point is not, of course, that ‘red’ and the like are not context-sensitive. The point is that their context-sensitivity cannot be used to disarm the Sorites.

<sup>19</sup> Indeterminacy, that is, in the extension it has when uttered on a particular occasion. I’ll drop this sort of qualification henceforth, when it’s not needed.

solve at least some such cases directly. I emphasized above that it does not immediately follow that our use of 'red' leaves such cases completely unresolved: Remember 'water'. But absent a natural kind or property to resolve for us what our own usage does not, 'red' will lack a determinate classical extension.

Let me say that again, because the point seems to me to be important, and as yet unappreciated: It is not because vague predicates have borderline cases that Sorites reasoning involving them is appealing. On the contrary, it is because Sorites reasoning involving vague predicates is so appealing, and because there is no natural kind or property to fix their extensions, that they have borderline cases. To the question what vagueness is we might therefore answer: Quasi-tolerance, in the absence of a natural kind or property that might fix a classical extension.

One other point. One sometimes gets the sense that, on semantic views of vagueness, vagueness is due to laziness, so that the extension of 'red' is indeterminate because we have not bothered to fix a determinate extension for it, though we could, if we wished to do so. I hope the foregoing corrects this misimpression. The indeterminacy, if such there is, in the extension of the predicate 'red' is due, on my view, to its observationality. As Wright (1976) long ago suggested, then, there is a sense in which we could resolve this indeterminacy only if we sacrificed something important about our use of this word, namely, its observationality. Other cases may be different.

### 3 In Defense of Supervaluationism

If vague predicates lack determinate classical extensions, then there will be no classical semantics for a language containing vague predicates. There are, to be sure, plenty of ways to proceed. But it is natural at this point, or so it seems to me, to suggest that we try to provide such a semantics in terms of what is already available to us, namely, a set of extensions that 'red' could have, consistently with our use of it. So consider an utterance of 'That is red'. When shall we regard it as true? Well, surely, if it would have been true (false) no matter which of the various possible extensions for 'red' it might have had, it is hard to call it false (true). Conversely, if there are extensions 'red' could have, consistently with our use of it, according to which this utterance would not have been true (false), it is hard to call it true (false). But that is just to say that the utterance is true (false) iff it is true (false) no matter which



of the possible extensions we suppose ‘red’ to have, and that is the basic idea behind Supervaluationism. It therefore seems to me—contrary to Wright’s remark that this idea “comes. . . completely out of the blue” (Wright, 2003, p. 88)—at least a well-motivated position.

More would need to be said, of course, to motivate Supervaluationism’s distinctive logical theses, in particular, to motivate its treatment of the logical connectives. But we may set that issue aside. The resolution of the Sorites paradox offered above does not depend upon the Supervaluational treatment of the logical connectives: Not only is it compatible with various treatments of those connectives, it is, as I mentioned, compatible with epistemicist and contextualist treatments, as well. The question how the logical connectives should be handled is an interesting and important one, as is the question whether we should regard the Sorites premise as false (as having a true negation) or merely as untrue, but both of these are less central than the question how we can coherently refuse to accept the Sorites premise and what its attractions were, in the first place.

I do not say that Supervaluationism, whether in its familiar classical form or in some other form, is the right view. But I am sure that it is not as flawed as most people seem to think it is. Let me close by defending it against some of the objections Wright brings against it. First, Wright endorses Williamson’s criticism that Supervaluationism “implicit[ly] surrender[s] the T-scheme” (Wright, 2003, p. 88). Speaking for myself, however, I see no force in this objection. For one thing, I take the T-scheme to be unmotivated, at least in so far as its content exceeds that of the T-rules, mentioned earlier, which Supervaluationism can happily endorse. But more seriously, in the context of *any* non-classical semantics, it must at the very least be an open question whether the truth-predicate and the biconditional behave in such a way that the T-scheme is validated. Even if one assumes that  $\ulcorner \text{‘S’ is true} \urcorner$  and S must always have the same truth-value, it does not follow that  $\ulcorner \text{‘S’ is true iff S} \urcorner$  is always true, unless one makes additional assumptions about the semantics of the biconditional. I see no reason to assume *either* that  $\ulcorner \text{‘S’ is true} \urcorner$  and S must always have the same truth-value *or* that a suitable biconditional will always be present in the language or definable if it is not. If, as deflationists hold, there were no way to explain what one means by ‘true’ except by appealing to the T-scheme, that would be one thing. But it wants argument that that is the situation in which we find ourselves.

Second, and far more seriously, Wright raises the specter of higher-

order vagueness. As he puts the point, “it is a generally accepted datum of the problem that... the distinction between the borderline cases and those which we have a mandate to describe as, e.g., ‘heaps’ is not a sharp one” (Wright, 2003, p. 89). This is, of course, a familiar and serious problem, and Wright has a clever new argument that purports to show it leads to an incoherence in Supervaluationism. The argument is as follows. On the semantic view of indeterminacy, for an object to be on the borderline between the heaps and the non-heaps is for there to be “no semantic mandate” either to describe it as a heap or to describe it as a non-heap. So consider something that is on the borderline between the heaps and the things that are on the borderline between the heaps and the non-heaps—something that would exhibit the second-order vagueness of ‘heap’. Then—applying the idea of vagueness as indeterminacy, that is, lack of semantic mandate—we should have no mandate to describe it as a heap, no mandate to describe it as within the first-order borderline—that is, as being on the borderline between the heaps and the non-heaps—and, of course, no mandate to describe it as a non-heap. But something we have no mandate to describe either as a heap or as a non-heap is, on this view, something on the borderline between the heaps and the non-heaps. So it “fits a certain description which there is no mandate to describe it as fitting”, and that commits someone who takes vagueness to be semantic indeterminacy to “a version of Moore’s paradox” (Wright, 2003, p. 89).

I think that overstates the case, however. The object we are ‘considering’—the one that allegedly exhibits the second-order vagueness of ‘heap’—is not an object we have actually identified as such, but just one we are supposing exists. If we had identified a particular object as such, then we would indeed have an object we were committed to saying (i) was on the first-order borderline but (ii) was something we had no mandate to describe as being on the first-order borderline, and that would be a semantic version of Moore’s paradox. But we have identified no such object. Perhaps the Supervaluationist could deny that we *can* identify any such object without denying that such objects exist. But the idea behind Wright’s objection can be restated in a way that makes it quite powerful. An object exhibiting the second-order vagueness of ‘heap’ is one that is on the borderline between the heaps and the things on the borderline between the heaps and the non-heaps. But then, by the hypothesis, it is *not* (definitely) a heap, so it would therefore seem as if it must be on the borderline between the heaps and the non-heaps. If so, there can be no second-order vagueness.

I think this argument too can be met, but let me not pursue that issue now.<sup>20</sup> The more important point is that this argument—like most of the discussion of higher-order vagueness in the extant literature, including my own previous discussions—assumes that the boundary between the heaps and the things on the borderline between the heaps and the non-heaps is vague *in the same sense* that the boundary between the heaps and the non-heaps is vague. Wright makes this assumption when he supposes that the semantic theorist is committed to explaining the vagueness of the second-order boundary just as she explained the vagueness of the first-order boundary: as a matter of semantic indeterminacy. That can be denied, and I hereby deny it. Imagine yourself in possession of the Philosophers' Grail: a solution to the problem of intentionality, a theory of what determines reference that is known to be correct. Suppose further, as I have also suggested, that this theory implies that vague predicates lack determinate classical extensions. Instead, there are some extensions that, e.g., 'red' might have which are consistent with our use of it and some its having which is not consistent with our use of it. *This distinction is perfectly precise*, or at least it can be supposed to be with no threat to any of the remarks I have made to this point and certainly without any threat to Supervaluationism. Of course, not being in possession of the Grail, we have little idea where the boundary lies. But that isn't vagueness. It's just ignorance.

The temptation, obviously, will be to reply that if one is going to make this move here, one ought to have made it earlier. Why not just say that there is a sharp boundary between the heaps and the non-heaps, but that we just don't know where it is? But again, from the perspective of the view I've been developing, the cases are not analogous: I'm suggesting that the correct theory of what determines reference *tells us* that there is no sharp boundary between the heaps and the non-heaps (because 'heap' has no determinate classical extension) and that it also *tells us* which extensions are consistent with our use of 'heap' and which are not. There is nothing *ad hoc* about the refusal to go epistemic at one point but not at the other, if that is in fact what the correct theory says.<sup>21</sup> I don't think there's even anything very counter-intuitive about this combination of views. 125

I argued above that the basic intuition about vague predicates, the

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<sup>20</sup> What is needed to meet the argument is greater sensitivity to the logical principles governing higher-order vagueness (Heck, 1993).

<sup>21</sup> Maybe it doesn't say that. But it is not *ad hoc* to suppose it does. As I argued above, there is in fact some reason to suppose that is how things are.

one that drives the Sorites, is that they are quasi-tolerant. I argued further that the quasi-tolerance of vague predicates is ultimately responsible for their lacking determinate classical extensions. But what is the analogue of quasi-tolerance for higher-order vagueness? The quasi-tolerance of 'red' consists in its being irrational simultaneously to hold that  $a$  is red, that  $a$  is pairwise indistinguishable from  $b$ , and that  $b$  is not red. For 'red' to be second-order quasi-tolerant would thus be for it to be irrational simultaneously to hold that  $a$  is red, that  $a$  is pairwise indistinguishable from  $b$ , and that  $b$  is borderline. Well, would that be irrational? As I said above, it certainly is not irrational just to have no opinion about whether  $b$  is red. But that is presumably weaker than holding that  $b$  is borderline. So perhaps it is true that it would be irrational to hold that  $a$  is red, that  $a$  is pairwise indistinguishable from  $b$ , and that  $b$  is borderline *on the sorts of grounds one typically has*, namely, observational ones. But I see no reason to suppose that it would be irrational on any possible grounds: If we possessed the Grail, we might have very good grounds indeed. Even if such a combination of views were irrational, on any possible grounds—even if there is real vagueness, rather than ignorance, about where the border between the definitely red and the borderline lies—it is far from obvious that the same reasoning can be used to establish the existence of third-, fourth-, and higher-order vagueness, through all the finite orders. The matter needs further consideration, at least.

Third, last, and most interestingly, Wright argues that Supervaluationism is extensionally inadequate, assigning the wrong truth-value to certain sentences involving vague predicates:

[T]here are additional concerns about the ability of supervaluational proposals to track our intuitions concerning the extension of 'true' among statements involving vague vocabulary: 'No-one can knowledgeably identify a precise boundary between those who are tall and those who are not' is plausibly a true claim which is not true under *any* admissible way of making 'tall' precise. (Wright, 2003, p. 88)

Let alone, obviously, under all.

There are various things one might say about this, but the best seems to me to be to acknowledge the problem and attempt to relocate it. The problem might seem to have to do with knowledge about where the boundary lies. But its real source is the presence of psychological vocabulary. Consider this statement:

(1) Bill believes that John is tall.

Is that true if, and only if, Bill believes that John is  $F$ , for every acceptable way,  $F$ , of making ‘tall’ precise? One can go some way towards defending the idea that it is. Bill will believe all those things, one might say, if, and only if, he believes John is in the intersection of all the possible extensions for ‘tall’. So that seems OK. But now consider:

(2) Bill does not believe that John is tall.

Is that true if, and only if, Bill does *not* believe that John is  $F$ , for every acceptable way,  $F$ , of making ‘tall’ precise? That would make its truth depend upon Bill’s not believing, for *any* acceptable way of making ‘tall’ precise, that John is  $F$ . It follows that neither (1) nor (2) need be true: If Bill believes that John is  $F$  for some but not all acceptable  $F$ , then neither will be. So such statements as (1) are, it would seem, vague, not because of any vagueness in ‘believes’ (though such there may be) but simply because of the vagueness in ‘tall’. That may or may not be a comfortable position.

But it is not one to which Supervaluationism is committed. We ought to take (1) to be true if, and only if, Bill stands in the believing-relation to the proposition that John is tall. And what these examples really show is that Supervaluationism needs an account of which proposition that is. That is to say, in its present state of development, Supervaluationism offers us at best an account of the truth-conditions of simple statements involving vague vocabulary. It has not yet offered us any account of the *meanings* of those statements. That, it seems to me, is the real challenge facing Supervaluationism. But I know of no reason to suppose it cannot be met, though I certainly do not myself know how (or that) it can be met.

Of course, one can simply stipulate, say, that a vague predicate contributes to the proposition expressed by a sentence containing it the set of its possible extensions, where propositions are conceived as structured. But that is not very illuminating. What one wants to know is what is involved in *believing* the resulting proposition, and, so far as I know, nothing has ever been said about this question. That is, to some extent, a result of the fact that vagueness is too often treated as a *linguistic* phenomenon. But it is not fundamentally a linguistic phenomenon. My belief that my pen is red suffers from vagueness as much as, and apparently in the much same way as, my utterance of “My pen is red” does. Perhaps surprisingly, it is a virtue of semantic accounts of

vagueness, such as Supervaluationism, that they extend smoothly from language to thought: My concept of red may fail to have a determinate classical extension for much the same reason that my word ‘red’ has no determinate classical extension. Wright’s view shares this virtue. So does Epistemicism. But not all theories of vagueness do. Contextualism, for example, does not. It is not at all clear that we have a single concept *red* exhibiting the same sort of context-dependence exhibited by our word ‘red’.<sup>22</sup> On the contrary, the belief I express when I say “My pen is red” is one I can retain through changes in the context that may force me to express this belief differently.<sup>23</sup>

## References

- Beall, J. C., ed. (2003). *Liars and Heaps: New Essays on Paradox*. Oxford University Press.
- Fara, D. G. (2000). ‘Shifting sands: An interest-relative theory of vagueness’, *Philosophical Topics* 28: 45–81. Published under the name ‘Delia Graff’.
- Fodor, J. and Lepore, E. (2002). ‘What can’t be valued can’t be valued, and it can’t be supervalued either’, in *The Compositionality Papers*. Oxford, Clarendon Press, 63–86.
- Hart, W. D. (1991). ‘Hat-tricks and heaps’, *Philosophical Studies (Dublin)* 33: 1–24.
- Heck, R. G. (1993). ‘A note on the logic of (higher-order) vagueness’, *Analysis* 53: 201–8.

<sup>22</sup> Fara’s version of the view fares better here, since, on her view, our concept *red* is not context-dependent but relational.

<sup>23</sup> Remarks bearing some non-trivial relation to the foregoing were made in response to Crispin Wright’s paper “Vagueness: A Fifth Column Approach” (Wright, 2003), published in the same volume as this one, and delivered at the conference whose proceedings this volume is. Much thanks for J. C. Beall and Michael Glanzberg for organizing a terrific conference, from which I learned a great deal.

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- (1998). 'That there might be vague objects (so far as concerns logic)', *The Monist* 81: 277–99.
- Kennedy, C. (1999). 'Gradable adjectives denote measure functions, not partial functions', *Studies in the Linguistic Sciences* 29: 65–80.
- Kripke, S. (1975). 'Outline of a theory of truth', *Journal of Philosophy* 72: 690–716.
- Lewis, D. (1984). 'Putnam's paradox', *Australasian Journal of Philosophy* 62: 221–36.
- Raffman, D. (1994). 'Vagueness without paradox', *Philosophical Review* 103: 41–74.
- Shapiro, S. (2003). 'Vagueness and conversation', in Beall 2003, 39–72.
- Stanley, J. (2003). 'Context, interest relativity, and the sorities', *Analysis* 63: 269–80.
- Tappenden, J. (1993). 'The liar and sorities paradoxes: Toward a unified treatment', *Journal of Philosophy* 90: 551–77.
- Williamson, T. (1994). *Vagueness*. London, Routledge.
- Wright, C. (1976). 'Language-mastery and the sorities paradox', in G. Evans and J. McDowell (eds.), *Truth and Meaning: Essays in Semantics*. Oxford, Oxford University Press, 223–247.
- (2001). 'On being in a quandary: Relativism, vagueness, logical revisionism', *Mind* 110: 45–98.
- (2003). 'Vagueness: A fifth column approach', in Beall 2003, 84–105.