
SCIENCE AND SEMANTICS: THE CASE OF VAGUENESS AND SUPERVALUATION*

BY

JOAN WEINER

Abstract: It is widely assumed that the methods and results of science have no place among the data to which our semantics of vague predicates must answer. This despite the fact that it is well known that such prototypical vague predicates as 'is bald' play a central role in scientific research (e.g. the research that established Rogaine as a treatment for baldness). I argue here that the assumption is false and costly: in particular, I argue one cannot accept *either* supervaluationist semantics, *or* the criticism of that semantics offered by Fodor and Lepore, without having to abandon accepted, and unexceptionable, scientific methodology.

Some years ago it became apparent that minoxidil, a drug used to treat high blood pressure, had an interesting side effect: hair growth. After carrying out the requisite minimum number of studies, a new marketing campaign was born, urging the use of the drug Rogaine to prevent or reverse baldness. That is, the notion of baldness, one of the archetypical vague concepts, has actually been the subject of scientific study. Moreover, the minoxidil studies are not the only sort of medical investigations of baldness. It is an established result of epidemiological research that baldness is associated with increased risk of heart disease.¹ Yet these medical investigations of baldness do not figure in the numerous discussions of baldness that appear in the vagueness literature. Nor do people who use the term 'thin' as an example of a vague term discuss the epidemiological literature on body weight, fat and size. In fact, there seems to be a widespread assumption that, in our discussion of the semantics for vague predicates, no empirical research need be taken into account. The views that a single grain of sand cannot make the difference between a heap and a non-heap,

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that a single hair cannot make the difference between a bald and non-bald person, etc. are simply advertised as intuitions. And, while these intuitions can be (and often are) challenged, the challenges are rarely based on empirical evidence. Most discussions consist of more comprehensive considerations of the intuitions involved. Typically, the only empirical research that is thought to be relevant is research that concerns how competent speakers will behave when confronted with a sorites sequence.² Appeals to non-linguistic, non-psychological facts about, for instance, heaps or baldness rarely appear in this literature.³

Does the lack of such appeals show that there is something wrong with the vagueness literature? This much is certainly true; we want to know whether minoxidil can prevent or reverse baldness; whether bald people are at increased risk of heart disease; whether thin people have a lower risk of heart disease; whether rich people are healthier than poor people. It may be an inalienable part of our understanding of the term 'bald' that anyone with no hairs growing out of her/his head is bald. But it is surely no part of our understanding that it is not possible to address the issue of whether or not minoxidil can prevent or reverse baldness. If it were, the text of Rogaine commercials would make no sense. There would be something wrong with an account of vagueness or the semantics of vague predicates that tells us that we cannot address questions about baldness via empirical research.

Do contemporary accounts of vagueness suffer from this sort of difficulty? In this paper I will argue that a particular variety of contemporary account does. The accounts on which I will focus are those in which it is a necessary part of the semantics of vague predicates that some sentences in which they appear to fail to have truth values.⁴ This view is one of the cornerstones of the supervaluationist semantics for vague language. But it is also the foundation of a critique of supervaluationism by Jerry Fodor and Ernest Lepore.⁵ I shall argue that this view – either as it is understood by Fodor and Lepore or as it is understood by the supervaluationist – conflicts with the results of empirical research.

This does not, on its own, provide any critique of the view in question. Philosophical arguments can certainly bring to light problems with a scientific practice. A conflict between a view about the semantics of vague predicates and empirical research *could* show that there are hitherto unrecognized difficulties with the methodology of some empirical research. In this case, however, the conflict provides no criticism of empirical research. To accept this view is to commit oneself to rejecting not just accepted, but unexceptionable, scientific methodologies. It is for this reason that, I argue, it should be rejected. For philosophical arguments that show that there is a problem with some of our scientific practices are also arguments for the reform of science. If we accept such an argument, we should also be willing to reject the problematic practices. Claims about meaning must not issue in practical advice that we are unwilling to follow.

I.

To see the difficulty, let us begin with Fodor and Lepore's comments about the predicate 'bald'. There will be, they say, individuals who cannot be correctly classified as bald or non-bald. They write,⁶

We claim that, if there is no matter of fact about whether someone one ninth of whose head is covered with (his) hair is bald, then it is *necessary* (indeed *conceptually* necessary) that there is no matter of fact about whether someone one ninth of whose head is covered with hair is bald.

The point, of course, is not specific to the figure of one ninth – they invite their readers to choose a different fraction if this seems wrong. Their claim is that there is *some* amount of hair coverage for which there is, necessarily, no matter of fact about whether someone with that amount of hair coverage is bald. Fodor and Lepore take this to be characteristic of all vague terms and to yield a constraint on correct semantics for a language with vague predicates. The view that vague predicates have vague meanings, they think, plays the role of a conceptual truth. And they take this to be the source of a devastating critique of supervaluationism. For the supervaluationist proposal, they argue, runs afoul of this necessary feature of vague predicates. But their point is not meant to be specific to supervaluationist proposals. Any attempt to tidy up the extension of 'bald' and use the result in an account of its semantics will likewise violate their constraint. Indeed, their target is not so much supervaluationism as it is the conviction that we can use the model-theoretic interpretations of classical logic in explanations of the contribution vague predicates make to the truth-values of sentences in which they appear. It is, they claim, extremely dubious that a language that violates conceptual truths of natural language can do this sort of work.

But do the statements in question really express conceptual truths? In defense of the putative conceptual nature of the claim that there is no matter of fact about whether or not a borderline-bald person is bald, Fodor and Lepore offer only the following challenge:

If you doubt that this is *necessary*, ask yourself what fact about the world (or about English, for that matter) would convince you that, by gum, people one ninth of whose heads are covered with hair are definitely bald after all. If you doubt the necessity is conceptual, remember that baldness does not have a "hidden essence." (Fodor and Lepore, 1996 p. 523)

This challenge does seem unanswerable as long as we restrict our attention to the sort of examples that figure prominently in philosophical discussions of vagueness.⁷ Nearly all these examples fall into one of two categories. One category consists of sentences that express particular classifications

of particular objects, e.g. 'Al is bald'. The other consists of sentences – e.g. 'there are people who are borderline-bald', 'the loss of a single hair cannot make a non-bald person bald' – whose truth we seem to know by virtue of our understanding the term 'bald'. The data typically used to address questions about the semantics of vague predicates concerns our views about the correct evaluation of such sentences. And the considerations that are adduced in discussions of these sentences are almost invariably limited to mining intuitions about the meaning of the predicate in question.

Now, it is certainly important to give some consideration to these sorts of examples. But if we want to determine what sort of contributions such predicates as 'bald' make to the truth-values of sentences in which they appear, there is, for reasons I suggested earlier, something odd about limiting the data to these examples. Given our everyday concerns, these examples are not the only (or the most interesting) sorts of statements involving vague terms. In everyday life, we don't generally ponder or attempt to determine the correctness or incorrectness of classifying a particular person as bald. As for the other examples, they have their life primarily in discussions of semantics. Even those who are concerned about their own impending baldness do not tend to concern themselves with the possibility that the loss of a single hair might make a difference between baldness and non-baldness. Nonetheless, someone who is concerned about hair loss may well be concerned with the issue of whether such drugs as minoxidil can prevent or reverse baldness. If our interest is in the contributions that such predicates as 'bald' make to the truth-values of sentences in which they appear, then the available data are not limited to the correct evaluation of the standard sorts of examples. The available data include also our views about the correct pursuit of empirical research in which these predicates play a role.

Once we take these data into account, however, the challenge issued by Fodor and Lepore becomes much less compelling. For we can see that there are circumstances in which it is reasonable to overrule our initial assessments: to conclude that people who – we antecedently believe – are not definitely bald *are* definitely bald. Indeed, it is not simply that it is reasonable to overrule our initial assessments – we actually routinely do overrule our initial assessments. For, as Hilary Putnam has famously pointed out, a division of linguistic labor that involves deference to experts is an inextricable feature of our use of language. I may, like Putnam, be unable to distinguish an elm from a beech. But this does not and should not prevent me from making statements about elms and beeches. When I say that elms are good shade trees, I mean to be talking about the trees that experts identify as elms. And although 'bald' may not be a natural kind term – although baldness may, as Fodor and Lepore claim, not have a hidden essence – it does not follow that we do, or should, refrain from deferring to experts about who counts as bald. For we know, as a result

of actual empirical research, that there is abundant evidence that underlying laws and regularities apply to baldness.

What, then, should we say about the challenge Fodor and Lepore issue in the passage quoted above? Is there really no fact about the world that would convince us that people one ninth of whose heads are covered with hair are definitely bald after all? Perhaps there is none that would convince Fodor and Lepore. But it is not difficult to imagine situations in which many would be convinced. Suppose, for example, that research is underway to test a drug, call it *Vancobald*, for its efficacy in reversing hair loss. And suppose a threshold is found: if a person has hair coverage of more than $1/9$, treatment with Vancobald dramatically reverses hair loss. However, if the coverage is $1/9$ or less, the hair loss has advanced too far – Vancobald will be entirely ineffective. In these circumstances, it does not seem unreasonable to consider changing our evaluation of someone with $1/9$ hair coverage from borderline-bald to bald. A sufficient number of such results may well convince us that our initial judgment that someone with $1/9$ hair coverage is not determinately bald was incorrect. These considerations suggest that it is not unreasonable to suppose that Fodor and Lepore's purported conceptual truth is not a truth at all.

Fodor and Lepore might respond that, if there is such a threshold, this just shows that 'bald' was not vague after all. But if this so, we sometimes cannot tell without scientific research which predicates are vague. That is, it can be an empirical result that a purported conceptual truth is not true at all. Once we concede, as we must if this is so, that we cannot tell whether or not such claims as Fodor and Lepore's are conceptual truths or not, their argument loses its force. If they are mistaken about this, then why think the other purported conceptual truths involving vague predicates are conceptual truths? Moreover, there is something very troubling about labeling as a conceptual truth something that might be overturned by empirical evidence.

Our examination of Fodor and Lepore's rhetorical challenge, far from giving us a compelling reason to agree with their remarks about conceptual truths, suggests that many of their purported conceptual truths may not be true at all. Moreover, as I shall argue shortly, there is a far more serious problem with their views about vagueness and conceptual truths. If Fodor and Lepore are correct, then the techniques and arguments used by epidemiologists in empirical investigations are flawed. Indeed, if they are correct, there are no good techniques for determining whether obesity increases risk of heart disease or whether minoxidil can prevent baldness. Such everyday notions as baldness and obesity simply should not be the subject of scientific research. In fact, one of the interesting features of Fodor and Lepore's criticism of supervaluationism is that it focuses on something important that the supervaluationist proposals get right. Our understanding of vague predicates (at least those that might be used in

empirical investigations) is tied to the enterprise of precisification. But the supervaluationist strategy, I will argue, mistakes the significance of precisification. Supervaluationism, too, issues in an unreasonable criticism of epidemiological practice.

II.

The supervaluationist directs our attention to precisifications – ways of sharpening the bounds of a predicate. Given a particular precisification of the term ‘bald’, each person is either bald or not bald.⁸ On the supervaluationist view a sentence containing a vague predicate is true just in case it is true given any admissible precisification. One condition on the admissibility of a precisification of an everyday term is that it agree with our everyday verdicts in all determinate cases. Consider, for example, the sentence ‘Al is bald’. If this sentence is determinately true given our everyday understanding of ‘bald’, then Al will be bald on each admissible precisification. Thus on every admissible precisification of baldness, someone who has not a single hair on her/his head is bald. Similarly, if this sentence is determinately false, given our everyday understanding of ‘bald’, then Al will be non-bald on each admissible precisification. If Al is a borderline case of baldness, a precisification must still classify Al as bald or non-bald, but there will be admissible precisifications on which he is bald and admissible precisifications on which he is not bald.

The classification of borderline cases, however, cannot be entirely arbitrary. One of the purposes of this approach to the semantics of vague predicates is to preserve what Kit Fine calls *penumbral truths*. Fodor and Lepore give the following example:

If, in a precisification, Al goes into the extension of ‘bald’ and Bill has the same number or fewer hairs than Al, then Bill must go into that extension of ‘bald’, too. (Fodor and Lepore, 1996, p. 519)

That is, supposing Al and Bill are both in penumbra of ‘bald’ (i.e. both count as borderline-bald), the admissibility of precisifications of baldness is constrained by Al’s and Bill’s relative lack of hair.

The result of precisifying all vague predicates is a classical valuation – a valuation on which, for any predicate, and any member of the domain, that member is either inside or outside its extension. The supervaluationist notion of truth for vague languages is *super-truth*, truth in every admissible classical valuation.⁹

For classifications of individuals, super-truth and truth are indistinguishable. If ‘Al is bald’ is determinately true (false), it is true in every admissible precisification, hence super-true (false). If ‘Al is bald’ is borderline (neither

true nor false), there will be admissible precisifications in which it is true and admissible precisifications in which it is false. Hence it is neither super-true nor super-false. That is, supervaluationism gives us a three-valued semantics. But supervaluationism is distinguished by its treatment of the quantifiers and sentential connectives. For example, in the penumbral case described above, ‘if Al is bald, then Bill is bald’ will be true (super-true) – despite the fact that both ‘Al is bald’ and ‘Bill is bald’ count as neither true nor false. Moreover, since on every classical valuation each person either is or is not bald etc., the law of the excluded middle is super-true – despite the fact that many of its instances are neither true nor false.

Fodor and Lepore’s objection is not to the supervaluationist verdicts about the truth of particular sentences. They object, rather, to the fact that the verdicts are reached by examining precisifications. Since each precisification of baldness eliminates vagueness, the precisifications are valuations that violate conceptual truths about baldness. On their view, once we advert to a classical valuation in our understanding of the term ‘bald’, we have left the English word behind in favor of a homophonic non-English word. There is a response to this objection in Fine’s article, which Fodor and Lepore characterize as follows,

So the language for whose semantics precisification preserves the truths of classical logic is not, strictly speaking, English. But, gee, it is a lot *like* English. Like enough so that we can learn interesting things about what ‘bald’ means in English by attending to the behavior of its not-quite-English counterpart. (Fodor and Lepore, 1996, p. 528)¹⁰

They reply that it is unclear how the meaning of English expressions “is illuminated by investigating the homophonic expressions in a language that is *not* English and *none* of whose terms is vague” (1996, p. 528).¹¹

There is, of course, a respect in which Fodor and Lepore are right. After all, it is not as if the homophonic languages represented by the precisifications of supervaluationism are languages that people actually speak. So the activity of precisifying may seem to count as inventing a new language. It is this view about precisifying that leads Fodor and Lepore to write, “you cannot precisify English” (1996, p. 523). Fodor and Lepore are committed to dismissing the resulting precisified sentences as non-English sentences whose investigation can tell us nothing about the homophonic English sentences with which we began. This is a view that can seem reasonable in the abstract. But, as I suggested earlier, it seems a good deal less reasonable when we look at other contexts. For many people (epidemiologists, for example) precisification is part of the everyday use of English. Indeed, most of us typically take the results of epidemiological research – research that involves precisification of an everyday term – as teaching us what we are talking about when we use the term. To see this, it will help to say a bit about how such research proceeds. I will focus on the significance

epidemiological research has for our understanding of a particular vague predicate, 'obese'.

It may seem, however, that this focus is inappropriate. For one might suspect that, although this term is currently used in everyday language, it is and always has been a technical scientific term. But a look at the use of this term in the popular press suggests that it is very much a term of everyday natural language. There is, in fact, considerable evidence that, for many people, 'obese' is merely a substitute for 'fat' (which clearly *is* an everyday term from natural language). For example, the results of a recent study about the percentage of obese people in the US broken down by state, was widely reported in the press. Among the headlines of the stories were, "We're fat and that's that"¹², "Land of the Fat"¹³. In a Cox News Service story, the following sentence appears: "There is a statistical correlation between being fat and living in the land of fried chicken, cornbread, grits with red-eye gravy, sweet iced tea, pecan pie, porch swings and Sunday afternoon naps."¹⁴ In addition to the numerous articles that use the term 'fat' in descriptions obesity research, there are also articles that use the term 'obese' in descriptions that have nothing to do with scientific research – for example, in a BBC review of *Casablanca*, Sydney Greenstreet is characterized as 'obese'.¹⁵

Moreover, the use of 'obese' in the popular press as described above is not in any way due to ignorance about the work and attitudes of obesity researchers. In particular, the term 'obese' was not introduced as a technical term for some well-understood and determinate property. As we shall see shortly, among obesity researchers the term 'obese' is regarded as an expression for someone who weighs a lot relative to his/her height – not appreciably different from the everyday word 'fat'.¹⁶ It is perfectly reasonable to regard this research as research about the effects of what seems to be something we talk about everyday in non-scientific contexts. Lest this seem a point about what researchers "must think", it may be useful to note that I write, not as a philosopher who has read a few epidemiology articles on obesity, but rather as someone with a degree in epidemiology and who worked for a brief period as a biostatistician in a medical college, where my field of study was obesity.¹⁷ My characterizations are of what many actual epidemiologists *do* think and say.

Finally, regardless of whether 'obese' is a technical term or a term of everyday language, it is indisputable that it is a vague term – indeed, a vague term whose proper definition has been the subject of debate for some years. Vague terms of everyday language are used all the time in empirical research (consider, for example, the result to which I alluded earlier that bald people are at increased risk of heart disease). The treatment, in medical research, of 'obese' is not appreciably different from the treatment of other vague terms that are clearly used in everyday language. Thus an account of the semantics of vague terms, if it is not to conflict with our

understanding of what can be (and has been) shown by empirical research will still have to be in line with the epidemiological practices described here.

III.

What methods are involved in the investigation of the claim that, for example, obesity increases risk of heart disease? When such a hypothesis is first formulated, the use of the term 'obese' is clearly the everyday use. But how do scientists go about determining whether this hypothesis is true? In the early stages of investigation, the most efficient strategy is to conduct a case-control study. The first step is to identify a number of individuals who suffer from heart disease (cases) and a number of individuals who do not (controls). The next step is to determine the proportion of each group that is obese. The results of the study will come from a comparison of the proportions in the two groups. In order to determine the proportion of obese subjects in each group, each person in the study must be classified as obese or not. But 'obese' is a vague term. There is always the possibility that there will be subjects in the study who are not determinately classifiable given the everyday understanding of obesity. What is the researcher to do?

She does not simply hope for the best and decide to deal with borderline cases when they arise – perhaps failing to classify them at all. Each study subject must be classified, otherwise there will be no way to determine proportions. Nor does she resolve that, should she be unlucky and find that there are such individuals among the subjects in her study, she will make a decision when the time comes. After all, it is desirable that there be consistency in the classification. And a researcher who makes ad hoc decisions when the need arises may make inconsistent decisions. Worse still, her decisions might systematically conflict with those of other researchers in a way that is undiscoverable. For example, supposing (as many researchers do) that obesity supervenes on relative weight, one researcher who makes ad hoc judgments about many of the subjects in her studies may routinely classify as obese people with lower relative weight than those routinely classified as non-obese by another who makes ad hoc decisions. The solution, in obesity research, is to use a measurable index of weight relative to height – typically, body mass index (BMI).¹⁸ And the researcher will decide, antecedent to beginning the study, on a sharp cut-off (say, BMI > 30) between the obese and non-obese. It is essential to such an investigation to draw a line. This is so even if, as sometimes happens in such research, the decision is made to exclude borderline cases from the study.¹⁹

This much being so, according to the Fodor and Lepore criteria the language spoken by obesity researchers is not English, but a homophonic

language. Epidemiologists, at least in the context of their research, are not speaking English. In such a case, Fodor and Lepore's position not only excludes epidemiologist from the ranks of English speakers, it also precludes the possibility that epidemiological research has something to teach us about obesity. The problem is that the researchers misunderstand their research. If they think that the results of this research tell us something about obesity, it is, presumably, either because they are deluded – they do not realize that, once the research is underway, a new distinct term that is not English but is homophonic with the English term 'obese' has been adopted – or because they are content to replace the term 'obese', not just for purposes of research, but for ordinary use as well. In either case, the topic has changed. And the new topic, 'newobesity', is not simply different from the old topic – conceptual truths about obesity are violated. 'Obese', as used by epidemiologists, is a technical term not to be confused with our everyday term.

How seriously are we to take the distinction between 'obese' and 'newobese' ('obese' as used by the epidemiologists)? Fodor and Lepore's rhetoric suggests that there is no interesting semantic relation between the vague term and a homophonic precisified term – that the distinction is much like the distinction between the everyday word 'charm' and the physicists' word. But this is unreasonable. It isn't as if most of the objects that possess charm in the everyday sense also possess charm in the physicists' sense. There is no overlap at all. The physicist does not mean to be saying anything at all about charm in the everyday sense. The epidemiologist, however, *does* mean to be saying something about obesity in the everyday sense. And, while the epidemiologist does not attempt to determine exactly who is obese in the everyday sense, there is considerable overlap between those who are obese in the everyday sense and those who are obese given the epidemiologist's definition. For all that, the epidemiologist may have gone astray. Although the epidemiologist may begin with a hypothesis (formulated in everyday language) about obesity, she might end up with a definition of obesity so distorted that the results seem to have changed the subject.

Such changes in subject can certainly occur. Many believe that this is exactly what happened with psychological research on intelligence. As with obesity, if we are to get empirical results, intelligence must be measured. But the IQ tests that are used for this purpose, many believe, tell us little if anything about intelligence. Indeed, some have argued that there is reason to believe that the original subject – intelligence – is really not the sort of subject that can be studied in this way.²⁰ If they are right, the subject has been changed. But there is little reason to think that obesity research changes the subject in this way. Obesity research has been much written about recently in the popular press. Yet there are no raging controversies about the definitions employed in obesity research. This is not to say that

there is universal accord about the correctness of the currently used definition of obesity (BMI > 30). There are reasons to think that obesity should not be defined in terms of BMI. But even those who have reservations about this definition think that the research tells us something about obesity. It is difficult to imagine anyone claiming that, because the obesity researchers have drawn a sharp distinction, the results can tell us nothing about obesity, as we antecedently understand it.

One might suspect, however, that the research should be described differently. One might say that contemplation of obesity and people who seem to be obese has led researchers to consider and investigate some issues concerning the relation of various levels of BMI and various sorts of morbidity and mortality. But this not only seems somewhat convoluted, it also makes it somewhat mysterious that researchers should continue to use the word 'obese' at all. If they are not investigating obesity, why mislead people by using the word? Why not simply write about BMI? And what accounts for the debates over whether BMI really is a good index for use in defining obesity?²¹ There is something very peculiar about employing such a redescription of this research in a discussion of the semantics of 'obese'. For if we think that a good account of the semantics of 'obese' must answer to the way speakers of English use and understand this term, we should be taking the researchers' use of 'obese' at face value – not re-describing it. The data to which an account of the semantics of 'obese' should answer surely include the widespread belief – shared by epidemiologists, the National Institute of Health, the World Health Organization, the journalists who write about science, and those of us who take the reports at face value – that epidemiological studies have shown that obesity increases risk of heart disease.²² The problem with Fodor and Lepore's understanding of vagueness is that it does not seem to fit the available data – the data comprised by how competent speakers use and understand vague predicates.

IV.

Does supervaluationism fit our understanding of what epidemiological studies have taught us about obesity? Fine writes,

[A] vague sentence is true if it is true for all admissible and complete specifications. An intensional version of the theory is that a sentence is true if it is true for all ways of making it completely precise . . . As such, it is a sort of principle of non-pedantry: truth is secured if it does not turn upon what one means. Absence of meaning makes for absence of truth-value only if presence of meaning could make for diversity of truth-value. (Fine, 1975, p. 278)

As written, this passage states sufficiency conditions for truth. And it is difficult to see any reason to take issue with these conditions. But these

conditions are also, of course, much too weak to give us an account of truth. When Fine actually turns to the task of defining truth for vague language, he takes these conditions to be not just sufficient, but also necessary for truth. The upshot is that, on Fine's view, the everyday sentence 'obesity increases risk of heart disease' is true (super-true) just in case it comes out true on every admissible classical valuation. It follows that, to test the hypothesis that obesity increases risk of heart disease, the epidemiologist must provide evidence for (or against) the claim that *all* precisifications of 'obese' give us a particular result.

In some respects, this supervaluationist advice seems to accord with our view of what epidemiologists do (and should do). As I argued earlier, the epidemiologists' definitions are very like the supervaluationist's precisifications. Moreover, typically a variety of different definitions of this sort are used in testing hypotheses about the effects of obesity on morbidity and mortality. The supervaluationist emphasis on precisification thus draws our attention to something important in our understanding of how to determine the truth value of sentences containing vague predicates. When we want to do this, we precisify and we do not rely on a single precisification.

But the supervaluationist account of truth is also too strong. The epidemiological research is not designed to assure us that our hypothesis is true in the supervaluationist's sense – there is no attempt to show that on *any* admissible precisification of the term 'obese', we will find that obesity increases risk of heart disease. In fact, the epidemiologist assumes that not all admissible precisifications will speak with one voice. To see the significance of this difference, suppose, for example, that $BMI > 30.5$ and $BMI > 30$ are both admissible precisifications of obesity and that a large number of independent studies are conducted using each of these precisifications. Suppose, further, that the upshot is that all the studies with the former index provide good evidence that people with $BMI > 30.5$ are at increased risk of heart disease while none using the index $BMI > 30$ provide evidence that these people are at increased risk. In such a case, what should we conclude about obesity and risk of heart disease?

According to the supervaluationist, our hypothesis is true just in case it holds given *any* admissible precisification of 'obese'.²³ The evidence in the situation just described is that, although there are admissible precisifications on which it is true that obesity increases risk of heart disease, there are also admissible precisifications on which it is not true. For both $BMI > 30.5$ and $BMI > 30$ are, by assumption, admissible precisifications of obesity. Thus what the evidence tells us, according to supervaluationist semantics, is that 'obesity increases risk of heart disease' is neither true nor false. After all, our interest in obesity is not an interest in $BMI > 30.5$ (although we might, of course, decide that we are also interested in the effects of having $BMI > 30.5$). The epidemiologist's answer, in contrast, is that we have evidence that obesity is associated with increased risk of

heart disease and, also, that $BMI > 30.5$ would be a better definition of obesity than $BMI > 30$.

It should be evident that, were epidemiologists to heed the supervaluationist counsel, the upshot would be to impede our attempts to learn about obesity. For one goal of obesity research is to say, with some precision, what obesity is – where it is assumed that obesity is some weight related characteristic associated with increased morbidity and mortality.²⁴ If there is a particular point after which increases in weight begin to have adverse effects on health, epidemiologists may conclude that this is a good place to draw the line between the obese and non-obese. It will be of small moment if the result does not exactly fit the usage of the untutored competent speaker. Untutored competent speakers once classified whales as fish.

Moreover, it should be evident that, as with Fodor and Lepore, the supervaluationist says nothing about vagueness that gives us a convincing critique of this feature of epidemiological practice. These practices have been immensely successful both for purposes of pure research and for public health policy. Thus, if we agree that claims about meaning must not issue in practical advice that we are unwilling to follow, the appropriate moral to draw is that the supervaluationist account is mistaken.

But the supervaluationist may well respond that this objection is based on a mistaken assumption about admissibility. Why think admissibility is determined solely by the usage of the untutored competent speaker? Admissibility may well be determined by a number of factors – including the results of, and the assumptions involved in, our scientific inquiries. Researchers assume that obesity is some characteristic related to high relative weight and associated with increased morbidity and mortality. Thus the supervaluationist may claim that, should it turn out that there is a precise point – say, $BMI > 30.5$ – at which there is a sharp increase in risk of morbidity and mortality, this suffices to show that $BMI > 30$ is not (and never was) an admissible precisification for 'obese'. One might suspect, that is, that in order to be admissible, a precisification must be one on which it turns out that those who are obese (or have high relative weight) are at increased risk of morbidity and mortality.

But this is surely too strong a condition. To see why, suppose it turns out that, while high relative weight is associated with increased risk of a number of diseases (heart disease, diabetes, and high blood pressure, among them) it is also the case that high relative weight is associated with decreased risk of a wide variety of cancers. Suppose, furthermore, that the increased risks associated with high relative weight are exactly balanced by the decreased risks associated with high relative weight. In the situation I have just described, we would not deny that there is such a thing as obesity. Rather, we would say that obesity is associated with increased risk of some diseases and decreased risk of others, but not with an overall increase in morbidity and mortality. Given that this conclusion is neither

nonsensical nor incomprehensible, it cannot be part of the meaning of 'obese' that obesity is associated with overall increase in morbidity and mortality. A good account of the semantics of 'obese' must allow for the possibility that the researchers' assumptions about the effects of obesity are mistaken.

Moreover, even if we agree that admissibility is determined in part by researchers' assumptions and facts about the world, there is still no reason to suppose that all admissible precisifications will speak with one voice. I suggested earlier that in the situation described above (one in which people with BMI > 30.5 are at increased risk of heart disease and people with BMI > 30 are not), we have evidence that BMI > 30.5 is a better definition for obesity than BMI > 30. On the response just imagined, the supervaluationist claims that this is evidence that BMI > 30 is not an admissible precisification. But now suppose we have some additional information about another disease connected with high relative weight: diabetes. And let us suppose that the increased risk of diabetes begins whenever BMI exceeds 29. If our earlier evidence suggested that BMI > 30.5 was a good definition for obesity, this new information is evidence that BMI > 29 might be a good definition of obesity. And although our original story suggested that BMI > 30 might not be admissible as a precisification of 'obese', the additional evidence is that BMI > 30 is admissible – although perhaps not what researchers will ultimately settle on. Returning now to the issue of whether obesity is associated with increased risk of heart disease, we are back in the situation with which we began. The admissible precisifications do not all speak with one voice.

As before, the supervaluationist is committed to saying that what we have is evidence that our hypothesis (that obesity is associated with increased risk of heart disease) is neither true nor false. Indeed, if our evidence is good enough, the supervaluationist would have us stop with this conclusion. For one of the central features of supervaluationism is that it is designed to accommodate the existence of real truth value gaps. The supervaluationist account is designed to preserve the verdict that, for example, it might be neither true nor false that Al is bald. If so, nothing short of further hair loss or hair growth will (or should) change this verdict. In particular, no further investigation or decisions on our part will change this verdict. And, if it can be a stopping point to say that Al is neither bald nor non-bald, it can also be a stopping point to say that it is neither true nor false that obesity is associated with increased risk of heart disease.

What will the epidemiologist say? There is an obvious sense in which the epidemiologist and supervaluationist are in agreement: absent a definition of obesity, we cannot say that obesity is associated with increased risk of heart disease; we also cannot say that it is not. And no definition is forced on us by any combination of our evidence, our understanding and our use of the term. But the epidemiologist, unlike the supervaluationist,

will not stop with the verdict that it is neither true nor false that obesity is associated with increased risk of heart disease. The very fact that the admissible precisifications give us different answers is a reason, on the epidemiologist's view, for choosing among them. Actual precisification is part of the enterprise.

It should also be evident that the sort of considerations often described in the vagueness literature will not enter into the considerations that go into the choice of definition. In particular, the classification of particular individuals will not enter into it. It will be of no interest, for example, for the epidemiologist to consider how she would be inclined to classify each of a series of individuals with different relative weights. What is important is the significance the definition will have for our theory of the effects of obesity. Suppose, as I suggested earlier, it turns out that the increased risk begins at different relative weights for different diseases. It may seem reasonable to define obesity using a point at which there is increased risk of *all* these diseases. But this also might not be the right choice. Suppose, for example, that physically active people are at very little increased risk of heart disease no matter what their relative weight. Suppose, also, that physical activity does not affect the risk of the other diseases at issue. In that case it may make sense to define obesity without regard to the point at which we find increased risk of heart disease. What the epidemiologist looks for is a good theory – that is, the epidemiologist looks for answers to questions such as “is obesity associated with increased risk of heart disease?” that make sense. This consideration influences the decisions about which definition to choose.

Thus the methodology that gives us the answers typically published in medical journals rides roughshod over one of the central aims of the supervaluationist semantics for vague language: the preservation of truth value gaps. We saw earlier, in the discussion of Fodor and Lepore, some of the drawbacks of the two obvious options: either to decry the methodology or to declare that the subject has been changed. But there is another option for the supervaluationist. As I mentioned earlier, the epidemiologist's use of ‘obese’ is very different from the physicist's use of ‘charm’. The epidemiologist starts out, after all, with the intention of studying something that we are talking about when we use ‘obese’. Only in very unusual circumstances is it at all likely that everyone will agree that what we mean by ‘obese’ is simply $BMI > 30$ – even if that turns out to be the definition on which we settle permanently. For the notion of obesity surely *is* vague and the ultimate definition, even if it is one on which everyone agrees, is likely to be at least in part stipulative. Thus the supervaluationist might take the epidemiologist's use of ‘obese’ as an approximation of the real use – a definition that is precise enough to be used for scientific purposes but, at the same time, close enough to the actual notion of obesity to give us actual answers to the questions with which we begin. The epidemiologist's

results, on such a view, give us approximate truths about obesity. Moreover, it need not be a criticism to say that the results of obesity research give us only approximate truth. Many think that approximate truth is all we can expect from any scientific research.

But this strategy does not solve the problem. Consider again a situation in which there are a number of admissible precisifications (say, BMI > 29, BMI > 30, BMI > 31) for 'obese' – only some of which are associated with increased risk of heart disease. The epidemiologist will, as we have seen, choose an actual precisification as a definition. What should the supervaluationist think about this? The supervaluationist semantics tells us that, in the case described, it is neither true nor false that obesity is associated with increased risk of heart disease. On this view, although the epidemiologist's definition of obesity may be an approximation of real obesity, the epidemiologist's answer is hardly an approximation to the real answer. Indeed, it is clearly the *wrong* answer. For the epidemiologist will have concluded either that it is true that obesity is associated with increased risk of heart disease or that this is false. Moreover, the epidemiologist's choice of the wrong answer is not dictated by practical limitations or ignorance. The epidemiologist *knows* how things stand. Obesity, precisified in some admissible ways, increases risk of heart disease; obesity, precisified in other admissible ways, does not. The epidemiologist knows, that is, that the hypothesis in question is neither true nor false. The epidemiologist's choice is to dismiss this real knowledge of the truth about the effects of obesity, in favor of an approximate truth about these effects that she *knows* to be incorrect. The researcher is supposed to be (and takes herself to be) aiming at truth. Yet, on the supervaluationist account, the researcher is not aiming at truth. The researcher chooses approximate truth *over* real truth.

V.

The focus in this paper has been on two views: supervaluationism and the Fodor-Lepore critique of supervaluationism. But the point of this paper is not to urge that these views be rejected. Rather, as I stated at the outset, my interest is in a particular sort of data that is missing from discussions of the semantics of vague predicates: the data constituted by empirical studies in which these predicates figure. What I have tried to show is that this omission is not innocuous. The target of my arguments has been a view that underlies many of the currently popular accounts of the semantics of vague predicates: that it is necessary that some sentences in which vague predicates appear to fail to have truth values. I have argued that, if we accept this view about the semantics of vague predicates, then we are forced to say that there is something wrong with the practice of epidemiologists – epidemiologists are not in pursuit of the truth. And this conflict is more

a reason to question the view about vague predicates than it is to suggest that epidemiological practices be changed. Thus I offer these arguments, not as an attempt to urge the rejection of supervaluationism (or any other account of the semantics of vague predicates), but as a challenge – to come up with an account that does not do violence to our views about what we have learned from empirical research. For the data comprised by the practice of epidemiologists are special in a way that many of the data typically exploited by the vagueness literature are not. The untutored speaker who denies (for instance) that the loss of one ounce can make the difference in the classification of someone as obese or not is likely to be just as willing (if not more) to say that obesity is associated with increased risk of heart disease. It is not particularly surprising that the untutored speaker does not realize that these views may be in conflict. After all, the untutored speaker is someone who has not devoted much thought to the nature of obesity. It is part of the task of obesity research to come up with a notion of obesity that makes sense and that fits with the empirical data. And it is, I want to argue, part of the task of someone who searches for a semantics for vague predicates to come up with an account that fits another kind of data: our views about what we have learned from epidemiological research.

Department of Philosophy
Indiana University

NOTES

* I would like to thank Gary Ebbs, Charles Travis and, especially, Mark Kaplan for discussions and comments on earlier versions. I have also benefited from discussion of two related papers at conferences. One of these was a distant ancestor of this paper, titled “Science and Semantics: The Case of Vagueness”, presented at the 1998 Bled conference on vagueness. Another paper also titled “Science and Semantics: the Case of Vagueness”, focusing on the problems epidemiological research creates for some approaches other than supervaluationism, was presented at the 2003 Barcelona Workshop on Issues in the Theory of Reference.

¹ See, e.g. Herrera, C. R., D’Agostino, R. B., Gerstman, B. B., Bosco, L. A. and Belanger, A. J. (1995). “Baldness and Coronary Heart Disease Rates in Men from the Framingham Study,” *American Journal of Epidemiology* 142 (8), pp. 828–33; Lotufo, P. A., Chae, C. U., Ajani, U. A., Hennekens, C. H. and Manson, J. E. (2000) “Male Pattern Baldness and Coronary Heart Disease: The Physicians’ Health Study”, *Archives of Internal Medicine* 160 (2), pp. 165–71.

² See, for example, Kenton F. Machina’s suggestion in “Truth, Belief and Vagueness” for determining the people to whom ‘bald’ applies by determining “at approximately what point people begin to feel unsure whether sample baldish men are really bald”. Machina, K. (1999) “Truth, Belief and Vagueness,” in R. Keefe and P. Smith (eds) *Vagueness: A Reader*. Cambridge, MA: MIT Press, pp. 187–188. See also, Raffman, Diana (1994) “Vagueness without Paradox”, *The Philosophical Review* 103 (1), pp. 41–74; Soames, Scott (1999) *Truth*, Oxford University Press, New York, chapter 7; and Black, Max (1999) “Vagueness: An Exercise in Logical Analysis,” in Keefe and Smith, pp. 69–82. Raffman cites some empirical evidence from research in psychology.

³ One notable exception is W. D. Hart, who offers an elaborate argument (which includes scientific evidence) that four is the minimum number of grains of sand in a heap. See Hart, W. D. (1992) "Hat-Tricks and Heaps," *Philosophical Studies* (Dublin), XXXIII, pp. 1–24.

⁴ This is not, of course, meant to be a claim about the context-dependence of vague predicates. The view is that, even when the context is fixed, there will be such cases. And, as is well known, this view is not universally accepted. For example, Roy Sorenson and Timothy Williamson are well known dissenters. For expositions of their views, see for example, Williamson, T. (1994) *Vagueness*, London: Routledge; and Sorenson, R. (2001) *Vagueness and Contradiction*, Oxford: Oxford University Press. I do not discuss these so-called epistemicist views in this paper. However, I have argued elsewhere, (Weiner, J. (ms) "Science and Semantics: What Purpose Would a Line Serve?"), that these views as well as a variety of other views about vagueness are also inconsistent with accepted, and unexceptionable, scientific methodology.

⁵ Fodor, J. and Lepore, E. (1996) "What Cannot Be Evaluated Cannot Be Evaluated, and It Cannot Be Supervalued Either," *The Journal of Philosophy* 93 (10), pp. 516–535.

⁶ Fodor and Lepore, 1996, p. 523. Fodor and Lepore do not devote a great deal of argument to this claim, which they take to be evident. And they are not alone. In a response to Fodor and Lepore, (1999) "Supervaluation Can Leave Truth-Value Gaps after All," *The Journal of Philosophy* 96 (3), pp. 148–156, Michael Morreau writes that his aim is to show that supervaluation can respect "the conceptual truths that they [Fodor and Lepore] have in mind" (p. 148).

⁷ It is, however, worth noting that there is something strange about Fodor and Lepore's example. They concede that they are uncertain about the fraction in question. But there is something very odd about their claim that it may be that a conceptual truth that there is no matter of fact about whether someone, one ninth of whose head is devoid of hair, is bald. What's odd is that they acknowledge that they (and, perhaps, the rest of us as well) don't really know whether this is true. This is rather different from the sorts of statements usually taken as conceptual truths – statements such as that what is pink is not red – which virtually everyone agrees are true.

⁸ Or at least this is so on Kit Fine's version of supervaluationism, the version on which Fodor and Lepore focus. See Fine, K. (1975) "Vagueness, Truth and Logic," *Synthese*, XXX, pp. 265–300. In his response to Fodor and Lepore, Morreau offers an account on which precisifications need not classify every individual.

⁹ Although Fodor and Lepore focus on this particular version proposed by Kit Fine, their criticism is meant to apply to virtually all attempts to use the notion of precisification in an account of truth conditions for sentences in vague languages.

¹⁰ This is Fodor and Lepore's characterization also of a response found in Fine, 1975, p. 275.

¹¹ Actually, they say that it is unclear how the "vagueness of English expressions" is to be illuminated by such an investigation. However, as the previous quote indicates, the view they are criticizing is not a view about illuminating the *vagueness* of 'bald' but rather about illuminating its meaning. Thus I am assuming that "meaning" is appropriate here.

¹² *The Herald-Times*, Bloomington, Indiana, August 24, 2005.

¹³ *Indianapolis Star*, August 24, 2005.

¹⁴ August 24, 2005. This conflation of 'fat' and 'obese' also appears regularly in BBC stories. See, for instance, a story titled "Fatbusters" from 24 January 2002, in which the following sentences appear, "In 1994, research into a fat mouse was the starting point for a revolution in the science of obesity. The obese mouse was missing a hormone called leptin, which turns off the feelings of hunger."

¹⁵ See the BBC website. This website also has a review of a movie titled *Shallow Hal* in which the word 'obese' is used to describe a character.

¹⁶ Why, then, one might ask, should researchers not simply use the word 'fat'? I don't think that it is particularly difficult to see why it might be useful to substitute 'obese' for fat. One reason is that 'fat' is a pejorative. Another is that 'fat' has other uses in what is more or less the same field of research (e.g. how is obesity related to the percentage of dietary fat?).

¹⁷ I interrupted my work in philosophy for several years to study and work in epidemiology. After finishing a master's degree in biostatistics and clinical epidemiology in 1993, I worked for half a year as a biostatistician for the Medical College of Wisconsin. My research during that period (as well as for the Master's thesis) concerned obesity.

¹⁸ BMI is defined as weight in kilograms divided by height in meters squared.

¹⁹ For obvious reasons – the exclusion of borderline cases requires two sharp distinctions: a distinction between those who are obese and those who are borderline-obese and a distinction between those who are borderline-obese and those who are not obese.

²⁰ Many people believe that there is a single, well-defined and measurable entity that can reasonably be viewed as general intelligence and that is measured by IQ tests. This view about intelligence and IQ tests played, notoriously, a central role in R. Herrnstein and C. Murray, (1994) *The Bell Curve*, New York, Free Press, which gave rise to an enormous critical literature. However, this view has been controversial for some time. One of the most well known critiques, first published in 1983 and now in its 10th edition, is Howard Gardner (1993) *Frames of Mind: The Theory of Multiple Intelligences*, New York: Basic Books. Two other books, which take different sides on the issue are, Daniel Goleman (1995) *Emotional Intelligence*, New York: Bantam Books, and John Bissell Carroll (1993) *Human Cognitive Abilities: A Survey of Factor-Analytic Studies*, Cambridge: Cambridge University Press.

²¹ For discussion of this issue and its history, see, for instance, R. Kuczmarski and K. Flegel (2000) "Criteria for Definition of Overweight in Transition: Background and Recommendations for the United States," *The American Journal of Clinical Nutrition* 72 (5), pp. 1074–1081, November 2000; and "Use of the Body Mass Index (BMI) for counseling" and Editorial in *Journal of Sports Sciences* 2002, 20, pp. 515–518.

²² For example, in epidemiological research and the guidelines the currently accepted by, among others the CDC and the WHO, obesity is defined as body mass index (BMI) > 30. This definition is also widely used, without comment or objection in newspapers and television reports as well as countless non-medical web sites. For some recent examples, in addition to those listed in footnote 12 above, see the CBC news web site, <http://www.cbc.ca/news/bigpicture/obesity/bmi.html>: USA Today, (Jan 22, 04), Newsweek (Dec. 8, 03). This is, at least, evidence that reporters defer to experts. What of the rest of us? Since I have been emphasizing the importance of empirical studies, I am on somewhat shaky ground asserting, without the benefit of such studies, that the rest of us agree with these reporters. It does, however, seem somewhat unlikely that reporters would continue to write such articles if the view of the general public is simply that the results of using such measures as BMI > 30 are statements that are not really about obesity.

²³ This is not to say that the supervaluationist need require that every approximation or acceptable precisification be investigated. Suppose, for example, we can show that people with BMI > 27 are at increased risk heart disease and that, as relative weight increases above this level, so does risk of heart disease. This will suffice to show that people with BMI > 28 are at increased risk of heart disease; that people with BMI > 28.01 are at increased risk, etc. Also, not all approximations need be tested on the supervaluationist view. For example, approximations of obesity that differ from the everyday classification on determinate cases need not be tested.

²⁴ The issue of what is common to the understanding of 'obese' among obesity researchers is not, of course, the subject of discussion of in the epidemiological literature. The characterization is my own. For example of the sort of discussion on which I have based this characterization, see W. H. Mueller, *et al.* (1991), "Which Measure of Body Fat Distribution is Best for Epidemiological Research?" *American Journal Of Epidemiology* 133, p. 9.