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VAGUENESS, MEASUREMENT, AND BLURRINESS

My thesis is that the sorites paradox can be resolved by viewing vagueness as a type of irremediable ignorance.[1] I begin by showing that the paradox cannot be solved through restrictions, revisions, or rejection of either classical logic or common sense. I take the key issue raised by the sorites to be "limited sensitivity": are there changes too small to ever affect the applicability of a vague predicate? I argue that the only consistent answer is negative, and blame our tendency to think otherwise on a fallacious proportionality principle and a background of anti-realist theories of meaning. These theories of meaning encourage the view that perceptual, pedagogical, and memory limits would preclude unlimited sensitivity. Refutation of this view comes in the form of a reduction of vague predicates to "blurry" predicates. Since blurry predicates have unlimited sensitivity and are indistinguishable from their vague counterparts, I conclude that either vague predicates are dispensable or they are identical to blurry predicates.

The sorites appears to have originated with Eubulides. One well known version is the paradox of the heap which takes the form of a mathematical induction. The base step of the induction claims that a collection of sand containing, say, one million grains of sand, is a heap. The induction step claims that any heap remains a heap if only one grain of sand is removed from it. Classical logic allows us to validly infer from these two propositions that a collection of sand containing one grain of sand is a heap. One has resolved the paradox of the heap iff one has shown how Eubulides' argument (and its variations) is unsound. Thus one can classify resolutions of the paradox in accordance with the manner in which they constitute objections to the soundness of Eubulides' argument. There are two basic kinds of objections to the soundness of an argument: a challenge to the truth of its premises and a challenge to its validity.

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1. REJECTING THE ARGUMENT'S VALIDITY

One can reject the validity of an argument in two ways. First, one can claim that the argument is not valid in the same way that, say, affirming the consequent is not valid. Here, one concedes that the concept of validity applies. So were Eubulides' argument to be rejected along these lines, one would be claiming that the sorites embodies a formal fallacy. The second approach is to claim that the argument is neither valid nor invalid. The most straightforward way of motivating "validity gaps" is to maintain that validity verdicts rest on a *presupposition*. When this presupposition fails to be true, the question of validity fails to arise.

A. *The invalidity approach.*

Joseph Wayne Smith argues that paradoxes such as the sorites illustrate the complete invalidity of mathematical induction.[2] In response to the objection that the sorites can be formulated without mathematical induction, as a giant chain argument, Smith replies that this only shows there are two types of paradoxes. Sorites arguments not requiring mathematical induction are not genuine sorites arguments. They demand separate treatment. This total rejection of mathematical induction is unacceptable because of its costs to mathematics and its incompleteness as a resolution of the problem. Giving up an important portion of mathematics for the sake of a solution to only some of the initial puzzles is a bad deal.

Paul Ziff's position is more moderate. Rather than rejecting mathematical induction, we should simply put restrictions on it.

> A man with only one penny is poor. That's true. And giving a poor man a penny leaves him poor: if he was poor before I gave him the penny he's poor after I gave him the penny. That's true too. Both of those statements are obviously true. Nonetheless if you keep doing this if you repeat this argument over and over again you'll get into trouble. You must stop before it's too late or you'll end up with a false conclusion. The moral of the fallacy is plain: it's a perfectly good inference to make if you don't make it too often. (How often can in fact be worked out in precise detail for it obviously depends on the size and character of the increment in question. Thus if one were concerned with increments of the form: 1 penny then 1/2 penny then 1/4 penny and so on one could go on *ad infinitum*.)[3]

Some may be attracted to this proposal because the "not too far" restriction guarantees that we will never draw a false conclusion from true premises. But notice that the same can be said of a "not too near" restriction. By substituting for 'poor' the predicate 'is either poor or rich', we are guaranteed to draw a true conclusion as long as the argument goes far enough. Despite this guarantee, the "not too near" requirement is suspicious because it seems to countenance an inference through a false lemma. For the corresponding chain argument would require us to draw intermediate conclusions to the effect that middle-income people are either poor or rich.

In addition to its resemblance to the problematic "not too near" requirement, the "not too far" restriction appears trivial. In order to know whether I have gone too far, I must already know the truth value of the conclusion. Ziff's claim that the question 'How far is too far?' can be worked out in detail is based on an unrepresentative sample. This question cannot be given an exact answer for standard sorites arguments. For an exact answer would require us to locate the last F in circumstances in which we are unable to draw the line between F's and non-F's.

Stephen Weiss has also proposed that mathematical induction be restricted.[4] Weiss is aware of the triviality problem and tries to avoid it with an analysis that is far more detailed and technical than Ziff's. Essentially, Weiss requires that the induction predicate be no less precise than the relation by which the objects in the induction class are ordered. Like Ziff, he regards the following argument as invalid.

(A)
1. A 500 pound man is fat.
2. If an n pound man is fat, then an $n-1$ pound man is fat.

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3. A 50 pound man is fat.

Both 'heavier than' and 'is fat' are partitions of the weight parameter. That is, both can divide, according to height, *some* groups of people into mutually exclusive and exhaustive subgroups. The present group of individuals vary in weight from 50 pounds to 500 pounds by one pound increments. The 'heavier than' relation can partition this group but 'is fat' cannot. Thus the ordering relation is more precise than the inductive predicate which invalidates the argument.

The most serious problem with Weiss' proposal centers on how he hopes to use his criterion to solve the paradox. He formulates the criterion as follows:

An instance of mathematical induction applied to any subject is an acceptable argument (sound) if it satisfies the conditions for induction within mathematics but does not satisfy the condition that with respect to the induction set, the ordering relation partitions more precisely one of the parameters than the inductive predicate partitions.[5]

Notice that this criterion is formulated as a sufficient condition for soundness, not a necessary condition. As such it has no power to show that an argument is invalid. Yet Weiss repeatedly treats the criterion as if its satisfaction were necessary for soundness. For instance, immediately after stating the above criterion he says that the criterion "has banned all mathematical inductions involving imprecise terms".[6] Since Weiss grants that the premises of standard sorites arguments are true, his criterion can only ban them as unsound if its violation is a sufficient condition for invalidity.

In light of this difficulty, one might suggest that Weiss meant by 'if' the stronger 'if and only if'. This interpretation has the advantage of bringing Weiss' criterion into harmony with the rest of his discussion. For the stronger interpretation does portray violation of the criterion as a sufficient condition for invalidity. The disadvantage is that it exposes Weiss to a fatal objection. First note that it would invalidate the following argument:

(A′)
1. A 500 pound man is fat.
2. If an n pound man is fat, then an $n-1$ pound man is fat.

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3. A 50 pound man is fat or squares are squares.

But since the conclusion is a tautology, (A′) is valid regardless of whether it is also valid by mathematical induction. Since Weiss also accepts the truth of the premises he must also admit the argument is sound even though the argument would count as unsound according to the strong interpretation of his restriction. Other counterexamples can be formed by taking any Weiss-invalid argument and adding a premise that implies the conclusion, or taking as a new conclusion the disjunction of the old conclusion and any implication of the premises.

The fatal flaw of this approach is that it overlooks the asymmetry of proving validity and proving invalidity. Since an argument is proved valid by showing that it instantiates a valid argument form, it is natural to assume that an argument is proved invalid by showing that it instantiates an invalid argument form. Logic textbooks promulgate this error through their lists of "formal fallacies". For example, it is said that valid categorical syllogisms must distribute their middle terms. But as critics of logic textbooks tirelessly point out, the fact that an argument instantiates an invalid argument form does not establish its invalidity.[7]

All bachelors are rich.
All unmarried adult males are rich.

All unmarried adult males are bachelors.

Although this argument violates the criterion, it is nonetheless valid. The only arguments that can be proved invalid by form are those with logically true premises and a logically false conclusion. For example, those of the form '*p* or not *p*, therefore, *p* and not *p*'. The remaining invalid arguments cannot be proved invalid by form because it is impossible to show that they instantiate *no* valid argument form. The absence of an exhaustive list of valid argument forms and scruples about argument translation block the possibility of a *principled* proof of invalidity. We can *informally* prove that a particular argument is invalid by what Gerald Massey dubs "the trivial logic-indifferent method" of showing that the premises can be true while the conclusion is false. Since this method can only be applied on a case by case basis, it cannot show the general invalidity of sorites arguments. Nor can their invalidity be shown by maintaining that their premises are always logical truths and their conclusions are always logical falsehoods. For some sorites arguments have contingent base steps, like the following:

1. Twiggy is a thin woman.
2. If a thin woman gains a gram, she will remain thin.

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3. If Twiggy gains a million grams, she will remain thin.

No amount of tinkering with the fallacy of undistributed middle will make instantiation of its form a sufficient condition for invalidity. Likewise, no amount of tinkering with Weiss' criterion will make its satisfaction a (nontrivial) sufficient condition for invalidity. The same holds for Ziff's suggestion that the sorites is invalidated by the fact that it goes too far. The whole project of devising formal criteria for invalid mathematical inductions is fundamentally misconceived.

B. Validity gaps

Given that we cannot formally demonstrate that the sorites is invalid, we could adopt the position that it is neither valid nor invalid. For instance, Bertrand Russell held that logic does not apply to the ordinary world, it only applies to the Platonic heavens.[8] The immediate objection to this view is that it makes the scope of logic intolerably narrow. Since most of our reasoning employs vague predicates, logic would be useless in the evaluation of most of our reasoning.

This objection divides the validity gappers into two subschools. Philosophers such as Carnap and Susan Haack claim that the restriction of logic to nonvague predicates is not as drastic as it appears at first blush.[9] They maintain that our vague predicates could be replaced by precise counterparts without serious loss. For example, the predicate 'swizzle stick' is vulnerable to the sorites because it seems that the removal of one atom from a swizzle stick never turns it into something that is not a swizzle stick. However, 'swizzle stick' could be replaced by 'one billion atom swizzle stick'. Since this new predicate is defined as a swizzle stick containing at least one billion atoms, it is sensitive to one atom differences. Thus it is immune to the sorites.

The other subschool of validity gappers denies the feasibility of the replacement project. For instance, Russell objects on the grounds that since our whole language is vague, there are no precise predicates to use as replacements.[10] A more recent objection to the replacement project has been raised by Patrick Grim.[11] As defined, something is a one billion atom swizzle stick only if it is also a swizzle stick (in the ordinary sense of the term). The precisified predicates will have logical relations with the vague predicates by which they are defined. So applying logic to precisified predicates will require the extension of logic to vague predicates. Thus the sorites will regain a foothold.

One might suggest that this consequence could be avoided through more sophisticated definitions employing no vague predicates. Perhaps this could be done with the help of a mathematical definition of a perfect swizzloid. A 'gizzle gick' might then be defined as an object varying within a precisely specified range of being a perfect swizzloid. This ensures that the precisifying definition will not explicitly license inferences involving vague predicates. One might then be able to avoid accepting 'If there are gizzle gicks, then there are swizzle sticks'. Notice that it is also important not to reject the conditional because its negation carries commitment to applying logic to vague predicates. Perhaps the persuasiveness of these bridge conditionals can be explained away. But in the absence of an explanation, the validity gappers' inability to preserve them will count against their proposal.

2. REJECTING A PREMISE

Since Eubulides' mathematical induction has only two premises, there are only two ways to solve the paradox by rejecting a premise. Both are problematic.

A. Rejecting the base step.

The most radical response is to reject the base step of the induction. According to Peter Unger and Samuel Wheeler, the sorites shows that there really are no heaps.[12] 'Heap', along with all other vague concepts, is incoherent. Since most, if not all, of our observational vocabulary is vague, it follows that there are no ordinary things such as chairs, trees, and people. The immediate objection to this conclusion is that metaphysical scepticism about ordinary things is wildly counterintuitive. A second objection is that the vagueness of 'vague' ensures that the premises of the Unger/Wheeler argument are jointly inconsistent:

(1) There are ordinary things only if the predicates used to describe them have extensions.
(2) These ordinary predicates are vague.
(3) All vague predicates lack extensions (for they are incoherent as shown by the sorites).

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(4) Therefore, there are no ordinary things.

If 'vague' is in the extension of 'vague', then 'vague predicate' lacks an extension by (3), which is inconsistent with (2). Hence, the Unger/Wheeler argument can be shown to be necessarily unsound if the vagueness of 'vague' can be established.

Since most predicates are vague, there is an inductive argument to support the widespread view that 'vague' is vague.[13] Reason to suspect that there is a deductive argument issues from the consensus that sorites arguments require a vague inductive predicate. For this suggests that the vagueness of a predicate can be established by embedding it in a sorites argument. For example, the vagueness of 'small integer' is established by:

(B) 1. 0 is a small integer.
2. If n is a small integer, then $n+1$ is a small integer.

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3. One billion is a small integer.

If a corresponding sorites argument can be constructed for 'vague', we will have sufficient grounds for concluding that 'vague' is vague.

The desired argument can be constructed with the help of a sequence of numerical predicates: '1-small', '2-small', '3-small' etc. The nth predicate on the list is defined as applying to only those integers that are either small or less than n. These predicates can be used to construct a sorites paradox for the predicate 'vague'.

(C) 1. '1-small' is vague.
2. If 'n-small' is vague, then '$n+1$ small' is vague.

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3. 'One billion-small' is vague.

The vagueness of '1-small' equals the vagueness of 'small' because both predicates clearly apply to 0 and apply exactly in the same way to all the other integers. The same holds for '2-small' and '3-small'. Slowly but surely we reach predicates in which the 'less than n' disjunct eliminates some borderline cases. Once we reach a predicate in which all borderline cases are eliminated, we have reached a nonvague predicate. But it is unclear where the predicates with borderline cases end and the ones without borderline cases begin. In short, 'vague' is vague.

The sorites embeddability test implies that almost every predicate in currency is vague, including some that we do not ordinarily take to be vague. In particular, it would commit us to the vagueness of sentential predicates such as 'true':

(D) 1. '1 is small' is true.
2. If 'n is small' is true, then '$n+1$ is small' is true.

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3. For all m, 'm is small' is true.

Some might be reluctant to concede 'true' has been shown to be vague in light of (D) because they feel that the real source of vagueness in (D) is 'small'. It is felt that 'true' only reflects the vagueness of 'small' just as the moon only reflects the light of the sun. This intuition can be accommodated through a distinction between internally and derivatively vague predicates. A predicate such as 'long sentence' is internally vague because it can generate borderline cases by being predicated of sentences devoid of vague predicates. For example, it will eventually produce borderline cases when predicated of members of the sequence 'One and two are numbers', 'One, two and three are numbers', 'One, two, three and four are numbers', etc. In describing 'true' as vague, we need not be ascribing internal vagueness to it. We are only saying there are borderline cases of it. Evidence that the embeddability test has not led us astray here comes in the form of the many theories of truth (correspondence, coherence, and pragmatic) under which 'true' is vague. What holds of our example 'true' also holds of other predicates that may not appear to be vague and yet are ruled so by the embeddability test. The appearance is due either to a distinction between types of vagueness (not between vagueness and nonvagueness) or our unfamiliarity with borderline cases of the predicate in question.

The vagueness of 'vague' also creates difficulties for validity gappers. The restriction 'Logic does not apply to vague predicates' will only be useful to validity gappers if they can draw inferences from it that exclude applications to vague predicates. But since the restriction contains the vague predicate 'vague', it forbids us from applying logic to itself, rendering it inferentially impotent. This self-referential problem cannot be avoided by substituting a synonym for 'vague' or reformulating it negatively as 'Logic only applies to nonvague predicates'. For synonyms and complements of vague predicates are themselves vague. Matters are not helped by switching to 'Logic only applies to clearly nonvague predicates'. For in addition to there being predicates that are clear cases of 'clearly nonvague predicate', there are borderline cases of 'clearly nonvague predicate'. Higher order vagueness ensures that 'clearly nonvague' is vague. An alternative strategy is to reject my argument that 'vague' is vague on the grounds that it involved use of vague predicates. Since this would not show that 'vague' is precise, the validity gappers would still have to convince us that there is no self-referential problem. They could not meet this obligation by any argument whose conclusion is the negation of my thesis that 'vague' is vague. For such arguments would involve vague predicates. To say that a predicate is not vague is to say it lacks borderline cases. But as a minor adjustment of the original argument will reveal, 'borderline case' has borderline cases, and so is vague. Thus any argument about whether a predicate is vague involves vague predicates. So in addition to being unable to support their assumption that 'vague' is precise, validity gappers would be unable to use any argument to identify the vague predicates ruled out by their restriction. Although logic will apply to the restriction, it will not apply to arguments supporting premises of the form '*F* is a vague predicate'. Thus the restriction will never be activated as the first premise of a (warranted) universal instantiation. Therefore, my objection that the restriction displays (direct) inferential impotence can only be escaped by conceding its indirect inferential impotence.

In addition to the self-referential problem, it should be noted that the restriction runs into difficulties with counterexamples to the compositionality of vagueness. As can be seen from his argument that all language is vague, Russell assumed that any term that is defined with vague terms must itself be vague. However, there are at least two ways to define precise predicates in terms of vague ones. First, one can superimpose vague predicates in such a way that everywhere one of them is indefinite the other is definite. For example, the indefinite range of 'is an integer somewhat greater than 104' is within the definite range of 'is an integer somewhat less than 106', and vice versa. Thus the conjunctive predicate 'is an integer somewhat greater than 104 and somewhat less than 106' is perfectly precise. Second, one can follow a method of exhaustion. The cases which are indefinite for 'small integer' and 'medium integer' are definite positive cases for 'is either a small or medium integer' because the disjunctive predicate exhausts the possibilities for these cases. Since the same holds for 'medium integer' and 'large integer', the disjunctive predicate 'is either a small, medium, or large integer' is precise. These counterexamples to the compositionality of vagueness show that vague predicates are not always infectious and can even "cure" prior vagueness. An expression such as 'is an integer somewhat greater than 104 and somewhat less than 106' creates an embarassment for the view that logic applies to all and only precise predicates. For its precision places the whole expression within the scope of logic while the vagueness of its defining terms places the parts of the expression outside the scope of logic. Yet the parts are related to the whole by the logical operation of conjunction!

Counterexamples to the compositionality of vagueness also create trouble for incoherence theorists. Those who believe that vague predicates are inconsistent can tolerate some failures of compositionality.[14] They can happily concede that the vagueness of 'heap' fails to make 'is either self-identical or is a heap' vague. For disjoining a consistent predicate with an inconsistent one yields a consistent predicate, as is illustrated by 'is a triangle or is a round square'. They can also concede that 'large' fails to make 'is large and is a round square' vague. For conjoining an inconsistent predicate with any other predicate yields an inconsistent predicate. However, they cannot tolerate the possibility that two vague predicates can be conjoined to form a precise predicate with a nonempty extension. If vague predicates are inconsistent, 'is an integer somewhat greater than 104 and somewhat less than 106' would be inconsistent. Yet it has the nonempty extension {105}. Notice that this example also poses a problem for anyone who might read 'incoherent' as 'meaningless'. For conjunctions (as opposed to concatenations) of meaningless expressions yield meaningless expressions.

B. Rejecting the induction step.

The more popular approach has been to reject the induction step of the argument. The initial appeal of this move is dampened by the recognition that rejecting the induction step is tantamount to asserting its negation. For the negation of the induction step is equivalent to the proposition that there is a precise minimum number of grains of sand necessary for being a heap. In other words, there must be a sharp division point between heaps and nonheaps. Philosophers usually try to avoid this counterintuitive commitment by altering the interpretation of the negation of the induction step through departures from classical logic.

(1) *Intuitionism.* Perhaps the most direct attempt to avoid commitment to precise thresholds is Putnam's.[15] Putnam points out that the offensive negation does not follow in intuitionist logic. So by abandoning classical logic in favor of intuitionism, we can comfortably reject the induction step. However, as Cargile has pointed out, the

paradox can be formulated in another way by appealing to the intuitionist's least number theorem.[16] Indeed, Stephen Reed and Crispin Wright have shown that the sorites can be couched in even low level intuitionist logic.[17] The paradoxical nature of the sorites persists even when deprived of the devices disallowed by the intuitionist.

(2) *Denying that the induction step has a truth value.* Max Black has suggested that classical logic be viewed in the same way as geometry – applicable only in a tentative, rough and ready way to our terrestrial surroundings.[18] Just as geometry *presupposes* rigid boundaries, logic presupposes sharp boundaries for predicates. We can confidently apply geometrical principles to physical objects to the extent that we are confident those objects approximate perfect rigidity. Likewise, logical principles such as the law of excluded middle apply just to the extent that our concepts are sharp.

Black concedes that the sorites is valid and also grants that the base step is true and the conclusion is false. However, he claims that the induction step lacks a truth value. Thus Black implicitly invokes a nonstandard concept of validity that permits an argument to be valid even if it has a false conclusion but no false premises. This deviation requires Black to also use deviant logical connectives. For instance, Black must reject double negation in order to escape sorites arguments such as the one resulting from prefacing the induction step of (A) with 'It is not true that it is not true that'. Black explains the induction step's lack of truth value by appealing to the fact that it quantifies over borderline cases. The premise "bundles together proper and improper instances" of the vague concept in question.

J. L. King has objected that the requirement that borderline cases be excluded from the domain of discourse would render many of Black's own statements neither true nor false. For instance, consider the statement "If a man is tall, then he is not a borderline case (for 'tall'), but some men are borderline cases (for 'tall')":

$$(x)(\mathrm{M}x \supset (\mathrm{T}x \supset -\mathbf{B}_0x))\ \&\ (\mathrm{E}x)(\mathrm{M}x\ \&\ \mathbf{B}_0x)$$

This statement is plainly true. Indeed, it is just the sort of statement Black could make in his analysis of the sorites. Yet it cannot come out true under Black's requirement. If the quantifiers range over borderline cases for 'tall', the first conjunct is illegitimate since it will have substitution instances in which the predicate 'tall' applies to borderline cases. On the other hand, if the quantifiers do not range over borderline cases, the second conjunct is false. King points out analogous difficulties for other seemingly true statements about the relationship between clear cases and borderline cases. He diagnoses the general problem as follows.

> The obvious source of the difficulty is the fact that, in general, the clear cases for the concept of a borderline case of a given concept are (necessarily) the borderline cases for the given concept. If we wish to apply both a concept C and the concept of borderline case for C, we will have to forego any general restriction on quantification over borderline cases. On the other hand, if we treat the general premise of the sorites argument as an inductive statement and do not impose any restriction on such quantification, the sorites paradox will be reinstated.[19]

King concludes that Black's rule against quantifying over borderline cases is too strong.

Problems with the logical status of Black's own analysis are made more vivid with the realization that his key concept of 'failed presupposition' is itself vague. To see this, consider the following sorites:

(E)
1. Any induction step using '1-small' has a failed presupposition.
2. If induction steps using 'n-small' have failed presuppositions, then so do those using '$n+1$ small'.

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3. Induction steps using 'one billion small' have failed presuppositions.

Since 'one billion small' is not vague, the conclusion of this argument is false. To solve this sorites, Black must say that the induction step has a failed presupposition due to the vagueness of its inductive predicate. The consequent vagueness of 'has a failed presupposition' places it outside the scope of logic. Since the restriction 'Logic presupposes sharp boundaries' uses a concept without sharp boundaries, inferential impotence haunts Black's restriction in the way it haunts the restriction of the validity gappers.

(3) *Many-valued logic.* Perhaps the most popular way to reject the induction step is through an attempt to reflect the intuition that vague predicates have degrees of applicability by introducing intermediate truth values. It can then be maintained that a one grain difference cannot be the crucial difference between whether a collection of sand is a heap. For one can claim that the grain only makes a difference to the degree of truth or accuracy there is in the claim that the body of

sand is a heap. The plausibility of the induction step is then explained in terms of the high degree of truth possessed by each conditional of the form 'If an n grain collection of sand is not a heap, then an $n+1$ grain collection of sand is not a heap'. Although each conditional of this form has a high degree of truth, the degree of some of them is slightly less than full truth. These small differences can accumulate into a big difference. Thus the induction step of Eubulides' argument should be rejected because it implicitly states that "If it is true to degree x that 'An n grain collection of sand is not a heap', then it is also true to degree x that 'An $n+1$ grain collection of sand is not a heap'".

One cost of this solution is the revision to logic. Intuitively, 'This collection of sand is either a heap or not a heap' is a tautology and so should have a degree of truth equal to 1. But given that the collection of sand is a borderline heap, 'This is a heap' and 'This is not a heap' will have degrees of truth equal to less than 1. The standard many-valued rule for determining the truth value of a disjunction is to assign the disjunction the higher of the truth values assigned to its disjuncts. Accordingly, the truth value of 'This is a heap or not a heap' will be less than 1. So unless the standard rule is replaced, many-valued theorists must either follow Sanford and deny the truth functionality of the logical connectives (to preserve classical theorems) or follow Machina and deny the classical theorems (to preserve truth functionality).[20]

Is there a reasonable replacement for the standard rule for assigning truth values to disjunctions? To answer this question, we need to get a firmer idea of what goes wrong with the standard rule. It should first be observed that grounds for rejecting the standard rule for disjunction are grounds for rejecting the standard rules for the remaining binary connectives. For example, the conjunction rule assigns conjunctions the minimum value of its conjuncts. So 'This is a heap and this is not a heap' receives a truth value of greater than 0 even though it is a contradiction. Since the rule guarantees that conjunctions will have truth values no lower than their lowest conjunct, the rule also has difficulty in explaining our growing clarity as predicates are added after the most doubtful one. For example, suppose a speaker begins by describing Ted as short and then adds that he is also fat, bald, smart, athletic, and rich. We assign a degree of truth of 0.5 to 'Ted is short' and 0.6 to each of the remaining attributions. But contrary to the conjunction rule, we do not believe that 'Ted is short, fat, bald, smart, athletic, and rich' equals the degree of truth of 'Ted is short'. Our uncertainties compound making us assign a much lower degree of truth to the claim that Ted exemplifies the conjunctive predicate. Also notice that 'Ted is fat or bald or smart' is less of a borderline attribution that 'Ted is fat'. Contrary to the many-valued disjunction rule, disjuncts of equally borderline predications can add up to produce a disjunctive statement "closer to the truth" than any of its disjuncts. Acceptance of both the disjunction rule and the conjunction rule requires one to assign the same degree of truth to disjunctions of propositions with equal degrees of truth as to the conjunctions of the same propositions. The error is akin to that of the card player who assigns equal probabilities to 'My first hand will contain an ace and my second hand will contain an ace' and 'My first hand will contain an ace or my second hand will contain an ace'. Indeed, the obvious solution to the problems with accumulation and compounding effects now appears to be to replace the standard rules with those given in the probability calculus. However, this would trivialize many-valued logic. If a proposition's degree of truth always equals its degree of probability, the difference between many-valued logic and standard logic is merely verbal.

(4) *Supervaluationism.* The dilemma of choosing between classical theorems and truth functionality is softened by supervaluationists such as Kit Fine.[21] Instead of appealing to intermediate truth values, supervaluationists only appeal to truth value gaps. According to Fine, simple propositions about borderline cases lack truth values. However, a complex proposition whose component propositions lack truth values may have a truth value. For it may be the case that the complex proposition comes out true regardless of the way we fill in the truth value gaps of its component propositions. Logical truths come out true under every (admissible) valuation of their components. When dealing with vague propositions, one need only consider the admissible precisifications to see that the vagueness of propositions does not prevent us from showing that compounds of those propositions can be logical truths. Thus 'Either this is a heap or not a heap' is a logical truth on the supervaluational approach because any precisification of 'heap' will ensure that one of the disjuncts is true while the other is false. In

fact, all of the classical theorems will be preserved by this approach. So Fine claims that the departure from standard logic occurs only in the meta-theory. Since the truth values of complex propositions do not depend on the (actual) truth values of their components, supervaluational logic is not truth functional. However, the important relation between the proposition and its component is preserved in the form of determination of the whole by hypothetical assignments of truth values to its parts.

Critics of the supervaluationists complain that hypothetical precisifications are irrelevant. Sanford points out that the supervaluational approach distorts the existential quantifier and disjunction.[22] For the supervaluationists are claiming that 'Either this is a heap or this is not a heap' can be true even if 'This is a heap' is not true and 'This is not a heap' is not true. To deny the induction step, they must say that there is a minimum number of grains necessary for a heap. Yet they say that no number is actually the minimum number. Machina further objects that the supervaluational approach requires us to abandon classical rules of inference even though the classical tautologies which mirror these rules remain.[23] In general, critics of the supervaluationists have maintained that the conservativeness of the theory is illusory.

(5) *The epistemological approach*. According to the epistemological approach, the induction step is false and there is no need to alter standard logic. There are precise division points. However, it does not follow that we are in a position to know where those division points are. Thus the existence of sharp division points is reconciled with our inability to specify them. According to Cargile, this ignorance is due to our imperfect understanding of vague words. Campbell describes our ignorance as "semantic uncertainty". Both urge the acceptance of the epistemological thesis on the grounds that it is far more plausible than any departure from classical logic.

Viewed from the epistemological perspective, there is a resemblance between the sorites and the lottery paradox. This resemblance can be highlighted by expressing a sorites as a chain of alternations. We begin with the observation that a 2000 millimeter man is not a short man but a 1000 millimeter man is a short man. We then consider the following alternations:

1. A 2000 and 1999 millimeter man are either both short or both not short.
2. A 1999 and a 1998 millimeter man are either both short or both not short.
.
.
.
1000. A 1001 and a 1000 millimeter man are either both short or both not short.

According the epistemic proposal, exactly one of the above propositions is false but we do not know which. At least one of the propositions must be false because the conjunction of (1)–(1000) implies the false proposition that either the two meter man and the one meter man are both short or are both not short. At most one of the propositions is false because otherwise there would be a short man who was taller than a nonshort man. Thus classical logic plus common sense knowledge of short men ensures that each of these propositions has a high probability, high enough to merit the belief of a man who had the acceptance rule: believe a proposition iff its probability exceeds 0.99. However, the probability of the conjunction of these propositions is lower than 0.01. Therefore, the follower of the acceptance rule would believe each member of the conjunction of (1)–(1000) and yet believe the negation of the conjunction. So he would have inconsistent beliefs. Given a lottery of 1000 tickets with only one prize, the same sort of inconsistent beliefs would be formed concerning propositions of the form 'Ticket number *i* is a losing ticket'. The lottery situation is paradoxical since it appears to show that one can have rational inconsistent beliefs. The proponent of the epistemological approach can maintain that the same holds for the above 1000 propositions. This resemblance is useful to the proponent of the epistemic view because he can use it to explain our willingness to assent to each step of the chain argument and our reluctance to accept the argument's conclusion. He can admit that vagueness engenders inconsistent beliefs without concluding that we are irrational or that vague language is incoherent.

Those not already in the epistemic camp will be quick to point out that the analogy between the sorites and the lottery paradox only holds given that we grant the key assumption that classical logic can be trusted. For it was needed to establish the falsity of one of the thousand propositions. It would be question begging to argue that since there is an analogy between the two paradoxes, the induction step is false (in just the way classical logic says it must be false). The advocate of the epistemic approach must claim that the analogy has the more modest purpose of forwarding the explanatory adequacy of his hypothesis. The point of the parallel is that the hypothesis can accommodate our ostensibly nonclassical pattern of assent to each step of the sorites (in its chain argument form). The lottery situation serves as a precedent for the rationality of believing an existential generalization ('There is a winning ticket') while disbelieving of each instance that it confirms the generalization.[24] Thus the "incoherency" is restricted to the doxastic level.

The epistemological approach provides a ready explanation of the many-valued theorist's troubles with his standard rules for evaluating disjunctions and conjunctions of vague predications: vagueness uncertainty, like all other types of uncertainty, falls under the scope of probability theory. Thus the noncompositionality of vagueness is no surprise. In addition to predicting that compounds of vague statements will tend to behave as applications of probability theory would lead us to expect, the epistemological approach predicts that these compounds will appear to behave counter-probabilistically just where we would expect probabilistic fallacies. For example, research in the psychology of reasoning documents our tendency to overestimate the probability of conjunctions and underestimate the probability of disjunctions. Hence people trained in statistics will tend to assign vague conjunctive predicates smaller positive extensions than those ignorant of statistics. The statistically sophisticated will also assign larger positive extensions to vague disjunctive predicates. Joining this pair of predictions enables us to picture the prediction in terms of a shift in the borderline area of the predicate. Suppose that C is a conjunctive predicate equivalent to F_1 & F_2. Its positive extension is conjunctive and hence overestimated by the probabilistically naive. Its negative extension is equivalent to the disjunction $-F_1 \vee -F_2$ and so is underestimated. Probabilistic sophistication corrects these errors yielding a rightward shift in the perceived borderline area as illustrated in figure 1.

In addition to predicting that probabilistic sophistication will produce a right shift for conjunctive predicates, the approach predicts a left shift for disjunctive predicates. Furthermore the magnitude of the

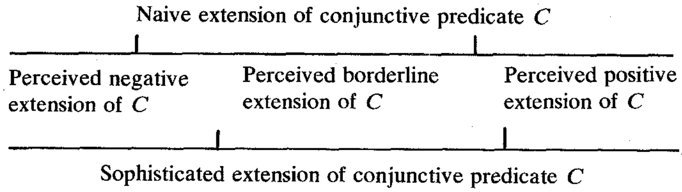

Fig. 1.

shift should be expected to increase with increases in the predicate's "degree of conjunctiveness" (number of conjuncts) or disjunctiveness. Since degrees of probabilistic sophistication vary, we can also test for a direct correlation between sophistication and shift magnitude. Expertise in psycholinguistics, experimental design, and the psychology of reasoning should put one in a position to check these predictions and generate new ones. What I have in mind is the construction of a questionnaire to be answered by groups of students ranging from the statistically naive to the statistically sophisticated. Since the gradations correspond to course levels in statistics, the cooperation of a statistics department would make the experiment affordable.

The key objection to the epistemological approach is that it makes an unrealistic assumption about the sensitivity of vague concepts. As J. L. King emphasizes, proponents of the epistemic approach must say that a millimeter difference can make the difference between a runner starting from New York being far from San Francisco and his not being far from San Francisco.[25] According to King, there can only be division points if there are determinants. The determinant for 'far' cannot be conventional, for ordinary usage is indecisive. The determinant cannot be natural, because there are no natural boundaries between far and nonfar points from San Francisco. Since the determinant must be either conventional or natural, there is no determinant and hence no sharp division point.

3. THE UNTENABILITY OF THE LIMITED SENSITIVITY THESIS

Although the sensitivity objection is rightly recognized as a persuasive point against the epistemic approach, it is not widely recognized that it is equally applicable to supervaluationism, the many-valued approach, and some incoherence theories.

Its impact on the supervaluationists and many-valued theorists was first emphasized by Unger.[26] Unger points out that the many valued theorists are committed to saying that a one atom difference can affect the degree to which a predicate like 'stone' applies to an object. Yet it seems absurd to suppose that the degree of truth or accuracy of 'This is a stone' can be decreased from say 0.7399995 to 0.7399994 by removing an atom. Likewise, the supervaluationists are committed to saying that the removal can make the difference between a proposition having a truth value and having no truth value. For they treat the set of admissible sharpenings as a sharply delineated set. The ultimate source of the commitment to varieties of unlimited sensitivity is the use of a classical meta-language. One can find supervaluationists and many-valued theorists who express awareness of this commitment. The supervaluationist, Hans Kamp, observes:

> This predicament is of course inescapable: any semantic account of a vague predicate P according to which at least some objects are definitely P and some others are either definitely not P or belonging to the truth value gap of P, will produce... a sharp distinction if the language in which this account is given contains only sharply defined predicates and the apparatus of classical logic and set theory. For whatever the condition may be which separates the objects that are definitely P from the others, it is bound to mark a sharp division – such is the classical theory of sets.[27]

Kamp's observation also extends to many-valued logic since it too has a classical meta-language. After all, fuzzy logic is a product of standard set theory. The many-valued theorist, J. Goguen, remarks:

> Our models are typical purely exact constructions, and we use ordinary exact logic and set theory freely in their development. This amounts to assuming we can have at least certain kinds of exact knowledge of inexact concepts. (When we say something, others may know exactly what we say, but not know exactly what we mean.) It is hard to see how we can study our subject at all rigorously without such assumptions.[28]

What makes the assumptions appear indispensable is the nonexistence of nonstandard meta-languages. This leaves the alternative logics without an alternative. This is bad news because all approaches having a classical meta-language fall prey to the sensitivity objection. So advocates of alternative logics that use the sensitivity objection against the epistemic approach are guilty of special pleading. Given that the supervaluationists and many-valued theorists cannot use the sensitivity issue to claim an advantage over classical logic, what is left to recommend their positions? The central motive for appealing to these alternative logics was to avoid the commitment to unlimited sensitivity. Once it is conceded that this appeal cannot succeed, there is no longer a point to departing from classical logic.

Does the limited sensitivity thesis deserve the credence we are inclined to lend it? The answer emerges as we draw out the details of the doctrine. A predicate is sensitive to unit *u* iff there is a possible positive instance and a possible negative instance which only differ by one *u*. Thus, 'short man' is sensitive to meters but apparently not millimeters. For a one meter man is short and a two meter man is not short, while two men differing only by one millimeter in height apparently must be either both short or both nonshort. Now consider the predicate 'is sensitive to unit *u*'. Is this a vague predicate? Here's an argument that should persuade the proponent of limited sensitivity. Consider the following sequence of propositions:

> 1. Two men differing by 1 millimeter can differ in that only one is short.
> 2. Two men differing by 2 millimeters can differ in that only one is short.
>
> .
>
> .
>
> .
>
> 1000. Two men differing by 1000 millimeters can differ in that only one is short.

Our proponent of limited sensitivity maintains that 'short man' is not sensitive to millimeters but is sensitive to meters. Thus he denies (1) and affirms (1000). We now ask him which of the members of the sequence is the first true proposition. Perhaps he will be confident that (999) is true and also affirm (998), (997), and (996). But eventually his confidence diminishes. He does not know which member is the first true member of the sequence and there appears to be no way he could find out. So he concludes that there is no first true member of the sequence even though he believes that there are some true members. This is hard saying because it violates the least number theorem which asserts that if there is a number that has a property, then there is a least number possessing that property. But the proponent of limited sensitivity cannot very well maintain that there is an unknowable first true proposition since he would then be left without an objection to the view that there is an unknowable first short man in a sequence of 1001 men descending in height from two meters to one meter by one millimeter decrements. The proponent of the limited sensitivity thesis will argue for a restriction, revision, or rejection of the least number theorem. He will maintain that vague predicates show that such modification is necessary. The proponent of the limited sensitivity thesis draws the moral that 'sensitivity' is vague; that 'limited sensitivity' has limited sensitivity.

Another way of looking at the autologicality of 'limited sensitivity' is from the point of view of someone trying to construct a sorites argument. Given that his inductive predicate is 'short man', he cannot use increments of one meter in his induction step to "prove" that all men are short. Nor can he use 0.9999 meter increments. What is the largest increment he can use and still have a true induction step? The question is made more vivid by considering a "meta-sorites":

1. A sorites argument concerning 'short man' has a false induction step if the step's increment equals or exceeds ten thousand millimeters.
2. If a sorites argument concerning 'short man' has a false induction step if the step's increment is n millimeters, it also has a false induction step if the step's increment is $n-1$.

---

3. All sorites arguments concerning 'short man' having induction steps with increments convertible to millimeters have false induction steps.

The proponent of the epistemological approach should view this argument as sound. He should accept the induction step because the unlimited sensitivity of 'short' guarantees that all sorites concerning 'short' have false induction steps.

The proponent of limited sensitivity is forced to agree with the classical logician but on different grounds. His commitment to (2) springs from commitment to the limited sensitivity of 'short'. To reject (2) as false is to affirm the unlimited sensitivity of 'limited sensitivity'. To refuse to accept (2) on the grounds that it is neither true nor false is doubly lamentable. First of all, to believe the limited sensitivity thesis is to believe that it is true. Since (2) is just a special instance of the thesis, it should be believed true. What are we to make of the claim that someone believes what he considers to be neither true nor false? Second, the position that (2) is neither true nor false is susceptible to the objections to which Black fell victim. The same goes for the position that (1) is neither true nor false. Since the proponent of the limited sensitivity thesis cannot reject (1), he can only escape the conclusion by denying the validity of the argument. And he must try to escape the conclusion because his talk of limited sensitivity was designed to deny the falsity of sorites' induction steps. Thus, these considerations constitute a third objection to Unger's position. For Unger claims that the sorites is valid and his commitment to limited sensitivity saddles him with acceptance of (1) and (2). So Unger's attempt to shore up the truth of the sorites' induction steps through appeal to limited sensitivity ultimately backfires.

The meta-sorites shows that the limited sensitivity thesis can only be consistent if the meta-sorites is not valid. For the limited sensitivity thesis commits its proponent to both of the premises of the argument and to the negation of the conclusion. However, the position that such arguments are not valid has already been shown to be untenable. Therefore, the limited sensitivity thesis is inconsistent.

Given its inconsistency, why is the thesis so compelling? Part of the explanation is suggested by John Stuart Mill's remark that "The most deeply-rooted fallacy . . . is that the conditions of a phenomenon must, or at least probably will, resemble the phenomenon itself."[29] Mill illustrated this fallacy with examples drawn from early stages of Western medicine. Because foxes have remarkable respiratory power, their lungs must hold a remedy for asthma. The brilliant yellow color of turmeric indicates that it has the power of curing jaundice. Richard Nisbett and Lee Ross have supported Mill's view with examples from psychoanalysis, the beliefs of the Azande, popular diagnoses of social problems, and experimental research indicating that

> People have strong a priori notions of the types of causes that ought to be linked to particular types of effects, and the simple "resemblance criterion" often figures heavily in such notions. Thus, people believe that great events ought to have great causes, complex events ought to have complex causes, and emotionally relevant events ought to have emotionally relevant causes.[30]

Applying the resemblance criterion to the question of whether the subtraction of one grain of sand from a heap can turn it into a nonheap yields a negative answer on the grounds that a tiny change cannot have a substantial effect.[31] Science supplies a large stock of counterexamples to this proportionality principle. For instance, an arbitrarily small change in the velocity of an object can make the crucial difference between whether it achieves escape velocity and travels far out into space, or fails, and crashes to earth. Of course, the smaller the change, the less *likely* it is to have the big effect. Perhaps a confusion between distributive and collective reliability enhances the persuasiveness of the proportionality principle.

Although misguided loyalty to the proportionality principle accounts for much of the raw intuitive support for the limited sensitivity thesis, it also draws support from current theories of meaning (especially those that are explicitly anti-realist). Against the background of these theories, one wonders how our predicates could distinguish between cases that their users could not. After all, 'red', 'heap', and 'child' were created by human beings who cannot make fine-grained judgments. These words are taught and used by people in a rough and ready way. Isn't the sensitivity of a predicate limited by the perceptual and memory limits of the users of the predicate? Wouldn't unlimited sensitivity make the predicates unlearnable?

4. A REDUCTION OF VAGUE PREDICATES TO BLURRY PREDICATES

Reason to believe that predicates with unlimited sensitivity could have a place in our language is provided by our mathematical vocabulary. When students are introduced to geometry, they are often tempted to answer questions through an empirical examination of drawn figures. Their instructors commonly warn against this temptation by emphasizing that drawn triangles and squares are not really triangles and squares. Triangles and squares are composed of *straight* lines. Microscopic examination of the drawn figures would almost certainly reveal a violation of the straightness requirement. When the topic is solid geometry, students are frequently told that sugar cubes and ball bearings are not really cubes and balls. Indeed, it is virtually certain that none of them has encountered a genuine cube or ball. For the tiny irregularities that pervade our universe are almost sure to be found in any physical cube or ball. Although it is logically possible for them to encounter a genuine cube, they could certainly never know it was a genuine cube. For reliable measurement is always finite. Even if there is no upper bound on how finely an object can be measured, we are never in a position to exclude the possibility that the next step in the measurement process will uncover a tiny bump or curve that will reveal that the surface of the "cube" is not really flat.

Given that we can never know that something is a cube, a student may well wonder how we can know anything about cubes. Some teachers give a platonistic response, claiming that although we cannot know that something is a physical cube, we can have knowledge of abstract cubes. Others claim that geometrical knowledge is always hypothetical; our knowledge of cubes is always knowledge of conditionals in the form 'If x is a cube, then x has property F'. One may then wonder how we can ever apply our knowledge of these hypothetical statements given that it is impossible to know the antecedents of the conditionals are true, given that it is overwhelmingly probable that the antecedents are false. We are thus led into the issue of how mathematics is applied. Other issues in the epistemology of mathematics loom.

The unlimited sensitivity of 'square', 'cube', and other geometrical terms raises a language learning objection paralleling the one raised against the thesis that vague predicates have unlimited sensitivity. Children are normally taught much of their geometrical vocabulary before they study geometry. They are taught largely by ostension by parents and peers who often are ignorant of the precise definitions given by experts in geometry. Moreover, many geometrical terms were in currency prior to the development of geometry. So if the language learning objection is fatal to the thesis that vague predicates have unlimited sensitivity, why isn't it equally devastating to the thesis that geometrical predicates have unlimited sensitivity?

One answer is that although our mathematical vocabulary is acquired in a rough and ready way, it could have been introduced by means of definitions which attribute unlimited sensitivity to the terms in question. It is only an historical accident that many mathematical predicates were taught by ostension, coaching, and rules of thumb. Their rough beginnings do not scratch the crystalline essence of these concepts.

If vague predicates could not receive the sort of rational reconstruction pictured above, we would have a relevant difference between mathematical predicates and vague predicates. However, a rational reconstruction of vague predicates is possible. Vague predicates can be defined in a way that attributes unlimited sensitivity to them without any detectable changes in their intensions. Indeed, the unlimited sensitivity of vague predicates can be piggy-backed on the unlimited sensitivity of mathematical predicates.

This rational reconstruction could proceed by defining vague predicates in terms of definientia that guarantee irremediable ignorance through the finitude of reliable measurement. If we know the precise lengths of the edges of an object, then we can know whether those edges are commensurable. But if we are presented with an object whose edges have unstipulated lengths, we cannot know whether they are commensurable. Measurement of the edges cannot give us the slightest reason to prefer 'The edges are commensurable' over 'The edges are incommensurable'. For reliable measurement is always finite and the question of whether the edges are commensurable requires a reliable infinite measurement. Notice that our necessary ignorance does not depend on uncertainty about what is and what is not an edge of the object. Even if the edges were perfectly distinct, their commensurability would be absolutely undetectable. Should the vagueness of 'edge of the object' intrude, we could always supply a precisifying definition that permits the investigation to continue. The arbitrariness of these precisifications is irrelevant since they are only made with a view to isolating one source of our irremediable ignorance about the commensurability of the object's edges. Clearly, *one* of the reasons that I cannot know 'The bottom left-hand edges of this sheet of paper are commensurable' is that the measurement problem is a hyper-task. Completing the task would require infinitely many steps. Thus the statement is an example of a symmetrically unknowable proposition, since both it and its negation are unknowable.

The hyper-task problem was behind Brouwer's comment that although finite inspection might reveal that there are three consecutive 7's in the decimal expansion of pi, the method cannot demonstrate the nonexistence of three consecutive 7's. For the three might be found further down the expansion.

Can we conclude that Brouwer's question, 'Are there three consecutive 7's in the decimal expansion of pi?', cannot be known to have a negative answer? No, because for all we know, a clever mathematician might come along and discover a property of pi that permits him to prove that the maximal string of 7's is less than 3. Of course, the fact that nothing we now know excludes the possibility of an answer by a clever mathematician does not imply that the possibility is genuine. But it does vitiate the claim that a negative answer to Brouwer's question is unknowable.

The hitch is that the person answering the question knows the generating function of the sequence. Knowledge of the function sometimes provides knowledge of the sequence that could not be derived from finite inspections. For example, consider the sequence formed by having each instance of the digit 1 preceded by n zeros and succeeded by $n+1$ zeros: 0.1010010001 .... Like pi, this sequence is irrational. Nevertheless, it can be demonstrated that it does not contain 3 consecutive 7's.

We can salvage the thesis that some questions cannot be knowingly answered in the negative by supposing that the questionee is faced with an opaque equivalent of Brouwer's question. This equivalent question will have the same answer as Brouwer's. However, the opacity of the question ensures that there is no alternative to the "brute force" method of finite inspections. For example, suppose we are asked to determine the size of a particular enclosure relative to a smaller enclosure. Unbeknownst to us, the area of the larger enclosure is pi times as large as the area of the smaller enclosure. We are asked whether the decimal expansion of the area has 3 consecutive 7's in it. The measurement process will only feed us new parts of the sequence in a piecemeal fashion, restricting us to the method of finite inspection.

The fact that opaque equivalents can be substituted for questions like Brouwer's suggests a way in which the rational reconstruction can be cast in quasi-mathematical terms. Recall that the goal of the reconstruction is to show that vague predicates could have been introduced by means of definitions which assign them unlimited sensitivity. This goal can be achieved by showing that vague predicates have intensions that are indistinguishable from the intensions of predicates that we admit to have exact intensions. These predicates, which will be called "blurry predicates", have exact intensions that we cannot know precisely. The imprecision of this knowledge is graduated, the gradations being reflected in probability judgments of varying strengths. For the blurry predicates are defined in terms of "mystery numbers" which possess unknowable identities but are still knowable enough to allow probability judgments about their identities. These numbers will be defined by posing certain questions similar to Brouwer's. They will differ in that they are to be taken opaquely to avoid the clever mathematician objection. Each question has an equivalent concerning a measurement problem that only permits answers by finite inspection. Since it would be cumbersome to ask about measurements, I will ask about their non-opaque counterparts. The fact that there is an opaque version of each of the questions legitimizes the restriction that the respondent not exploit his knowledge of the generating function in answering the question. For the mathematical questions are to be taken as ersatz measurement questions. Only brute force is allowed.

Instead of pi, we will make use of infinite sequences of irrational numbers. The first sequence is composed of the square roots of the prime numbers 2, 3, 5, ... Call the first member of this sequence index-1, the second member index-2, and so on. Since the *n*th root of any prime is irrational, it should be noted that in addition to our "horizontal" reference numbers obtained from successive primes along the number line, we can if need be, derive "vertical" reference numbers from the infinitely many roots of each prime. We let p_1, p_2, $p_3, \ldots, p_n$ stand for the primes and abbreviate the *n*th root of a number m as $m^{1/n}$. Thus the first row in figure 2 lists the horizontal reference numbers, and the columns below that row represent the vertical reference numbers.

Consider the decimal expansions of our horizontal reference numbers.

> 1.1 The decimal expansion of index-1 contains 1 consecutive 7.
> 1.2 The decimal expansion of index-1 contains 2 consecutive 7's.
> .
> .
> .
> 1.*n* The decimal expansion of index-1 contains *n* consecutive 7's.

At first blush one might feel certain that there is a false proposition in the index-1 sequence. If there is a false proposition, it is impossible to know that the proposition is false. However, the strings of 7's may be finite but of arbitrarily large size, making all of the propositions true. For example, ever expanding strings of 7's might be sandwiched between strings of 8's as in 878 ... 8778 ... 87778 .... Those im-

$$
\begin{array}{l}
p_1^{1/2}, p_2^{1/2}, p_3^{1/2}, \ldots, p_n^{1/2} \\
p_1^{1/3}, p_2^{1/3}, p_3^{1/3}, \ldots, p_n^{1/3} \\
p_1^{1/4}, p_2^{1/4}, p_3^{1/4}, \ldots, p_n^{1/4} \\
p_1^{1/5}, p_2^{1/5}, p_3^{1/5}, \ldots, p_n^{1/5} \\
\cdot \\
\cdot \\
\cdot \\
p_1^{1/n}, p_2^{1/n}, p_3^{1/n}, \ldots, p_n^{1/n}
\end{array}
$$

Fig. 2.

pressed by the notion that all possibilities are realized in the infinite might believe that such ever expanding strings of 7's surely exist in the decimal expansion of the square root of two ($2^{1/2}$). This worry rests on the principle of plenitude. I know of no coherent formulation of the principle that has application to the present case, so it is tempting to dismiss the reservation as incoherent. But even if there is a coherent formulation, it should be observed that we can appeal to "higher levels of plenitude". That is, in addition to expecting ever expanding strings of 7's in the square root of two, proponents of plenitude should also expect expanding strings of 7's in the cube root of two ($2^{1/3}$). However, they cannot reasonably have this expectation for all decimal expansions of numbers of the form $2^{1/n}$ collectively. Since there are infinitely many expansions of this form, the belief that all possibilities are realized in the infinite should lead one to the conclusion that some of these "vertical" expansions have a largest string of consecutive 7's. Of course, this argument does not guarantee that there are expansions having maximal strings. The argument is only intended to lower the probability to insignificance.

Nevertheless, to cover the possibility that there are no maximal strings, we shall define "mystery-1" as equal to 1 iff there are no largest strings of 7's in expansions of the form $2^{1/n}$. Otherwise "mystery-1" equals $x+1$ where x equals the position of the first false proposition in the index-1 sequence (or the nearest sequence with reference number $2^{1/n}$ that has a false member). In a like manner, we can define "mystery-2" in terms of the index-2 sequence which consists of propositions of the form "The decimal expansion of index-2 contains m consecutive 7's". So if all of the expansions of $3^{1/n}$ lack maximal strings of 7's, "mystery-2" equals 1. Otherwise "mystery-2" equals $x+1$ where x equals the position of the first false proposition in the index-2 sequence (or of the nearest sequence with reference number $3^{1/n}$). An infinite stock of other unknown numbers can be obtained with the index-3 sequence, the index-4 sequence, and so on.

Now consider the set containing the following propositions.

1. Mystery-1 is an even number.
2. Mystery-2 is an even number.

.
.
.

100. Mystery-100 is an even number.

The probability of each of the members of the set is 0.5 since we have no more reason to believe that a particular mystery number is even than we do to believe that it is odd.[32] Further, the probabilities are independent. Call the number of true propositions in this set "murk". Call a number "miny" iff it is less than or equal to murk. 0 is a miny number because the minimum number of true propositions in the set is 0. 101 is not miny because the maximum number of true propositions in the set is 100. Is 1 miny? Well, the probability of 1 not being miny is 0.5^{100} since all of the propositions in the set would have to be false. Thus the probability of 1 being miny is very close to unity. (By selecting a larger set, the probability can be made arbitrarily close to unity.) The probability of 2 being miny is also extremely high. Likewise for 3, 4, and 5. But gradually the probability diminishes so that once we reach 100 we are virtually certain it is not miny.

A predicate F is blurry iff there is a sequence of F's and non-F's such that all of the F's precede all of the non-F's but the position of the last F is unknowable. In other words, blurriness is a matter of not knowing where to draw the line between F's and non-F's. Here "not knowing" must be given the strong reading "not having any way of finding out". Sometimes, part of our ignorance about where to draw the line is remedial. For one can make better or worse probability judgments as to where the dividing line is. However, even the best judgment will leave a residue of ignorance. Blurriness imposes a graduated limit of knowledge.

Is 'miny' a vague predicate? It has the characteristic binomial probability distribution of many vague predicates and has an unknowable division point. Should it be introduced into ordinary English, it would behave like a vague predicate. If my neighbor permits me to pick miny apples from his tree, we will agree that picking six is within the bounds of his permission. But should my neighbor discover that I picked 60 apples we might quarrel over whether I took more than a miny number of apples. Neither of us can be certain, but it is possible to have better grounds. Controversy can be the result of honest mistakes due to the various difficulties and fallacies associated with calculating probability distributions and varying rules of acceptance. Of course, the debate can be sham where one party abuses the inherent uncertainty of the situation. To prevent abuse, quarrels, and to promote clarity, precisifying definitions could be introduced to reduce the blurriness. My neighbor could stipulate that 'miny' is to be understood as 'under 20'. Perhaps the stipulation would catch on and the original definition would be abandoned. But it is more likely that 'miny' would survive many ad hoc redefinitions. Notice that such precisifying definitions would not be purely stipulative. There are acceptable and unacceptable precisifying definitions. There would be arbitrariness in drawing the line but it would be false to say that anything goes.

Blurriness and vagueness carry the same linguistic advantages and disadvantages. Since advantages and disadvantages are relative to interests, the fact that vagueness can create uncertainty and irresolvable disagreement can be counted as a virtue of vagueness if one's purposes are served by it. A diplomat who warns of a "strong response" to the contemplated invasion of one of its allies may thereby deter the aggressor without committing his country to a predictable reaction. So the first advantage of blurriness and vagueness is that it can saddle others with disadvantages. A second, less paradoxical advantage, is that assertion conditions are easier to satisfy. Casual observation is usually sufficient for the application of 'small number' or 'miny'. From a glance I know that there are miny books on my desk but more than miny in my bookcases. A rudimentary grasp of 'miny' can be quickly acquired by children through a few examples and tips. Trial and error will refine their usage. Even if they never learn the formal definition of 'miny', they can teach it to others. 'Miny' can survive even if I take its definition to the grave. However, as the probability distribution associated with 'miny' is slowly but surely distorted, the authority of my definition is eroded, and the meaning changes.

A third advantage is that we can communicate whole probability distributions. Casual observation of a speckled hen does not enable one to know how many speckles the hen has. One could estimate by means of a range specification; you might say that there are between 10 and 100 speckles on the hen. But this description fails to convey characteristics of your probability distribution. There is a loss of information. If I tell you that I will arrive between 8 : 20 AM and 11 : 40 AM, you have no more reason to expect that I will arrive between 9 : 45 and 10 : 15 than to expect that I will arrive between 9 : 10 and 9 : 40. But if I tell you that I will arrive within miny minutes of 10 AM, you would consider it more likely that I will arrive within the 9 : 45 to 10 : 15 interval than within the 9 : 10 to 9 : 40 interval. Blurriness increases predictive power by allowing us to efficiently express and evoke confidence distributions.

Given that blurry predicates have the same linguistic advantages and disadvantages, there is no functional objection to the thesis that every vague predicate can be defined in blurry terms. One might define 'noonish' as 'any time that is within a miny number of minutes from noon'. Here one would be attempting to mirror the probability distribution people have for propositions of the form 'Time *x* is noonish'. Psychological research might disclose that our probability distribution for these propositions could be better reflected with a predicate other than 'miny'. If so, a superior redefinition could be offered by constructing another predicate from the mystery numbers. No matter what our ordinary probability distribution is, an identical one can be constructed with a predicate based on the mystery numbers.

Most of our ordinary language words could not be defined directly in terms of mystery integers; mystery rationals would have to be employed. For example, 'succotash' is a mixture containing an appropriate ratio of corn kernels and lima beans. If I mix 100 corn kernels with 100 lima beans, I have created succotash. But if I mix 100 corn kernels with 1,000,000,000 lima beans, I do not have succotash. A 1 : 1 ratio is permitted but a 1 : 10,000,000 ratio is not. But what is the minimum ratio? We do not know and we cannot know. However, this does not prevent the construction of blurry counterparts to succotash. For example, one proposal is that the actual ratio be between (murk +1) : (murk +10) and (murk +10) : (murk +1). This proposal yields a confidence distribution similar to that given by

"'Succotash' is roughly a 1 : 1 mixture of corn kernels and lima beans". However, to completely eliminate the vagueness one would have to define 'mixture'. In the ordinary sense of the word, mixtures must have their parts close to each other. They need not touch for I can spill my succotash on the floor and still have succotash to clean up. However, there are limits to how far I can scatter my succotash. If I enclose each kernel and bean in separate envelopes, and send the envelopes to friends and admirers all over the world, that is the end of my succotash. Further, the kernels and beans have to be integrated. If I pour half a barrel of lima beans into half a barrel of corn, I don't yet have a barrel of succotash. I still need to mix them up. As I begin to mix them, I wonder whether I have succotash yet. After 10 minutes of mixing, I am certain that I have succotash. But I do not know exactly when I first had succotash. This ignorance is a symptom of the vagueness of the integration requirement. The integration of a collection of objects is a function of homogenous distance and heterogenous distance. The homogenous distance between the kernels is the average distance from one kernel to another kernel; likewise the homogenous distance between the beans is the average bean to bean distance. The heterogenous distance between kernels and beans is the average distance from a kernel to a bean. A collection is integrated to the extent that the result of dividing the homogenous distance by the heterogenous distance is small. The blurry version of the integration requirement might read "The degree of integration must be a miny number". The question of how scattered the parts of the succotash might be could then be answered by determining the average distance between the kernels and beans. It could be proposed that this average distance be miny millimeters.

Regardless of the success of our definitions for 'appropriate ratio' and 'mixture', there still remains the problem of defining the vague words 'corn kernel' and 'lima bean'. These definitions would in turn appeal to parts of the kernels and beans. New definitions will be required for these parts and their subparts. Perhaps, eventually, we reach the atomic level. If there is no vagueness at this level, the reduction is complete. But must there be an ultimate nonvague level? Could the vagueness continue infinitely? There is a sense in which an infinity of levels would threaten the completeness of the reduction and a sense in which it would not. The reduction would be incomplete in the sense that there would always be an unreduced element. However, it would be complete in the sense that no particular element is unreducible. The important type of completeness is the latter. I need only claim that any vague predicate is reducible to a blurry one; I do not need to claim that, if need be, infinitely many replacements could be supplied in practice. Compare my reduction claim to the claim that any Spanish sentence has a French translation. If there is an infinite stretch of Spanish discourse or if the Spanish sentences are produced faster than their French translations, there will always be some untranslated Spanish sentences. But these sentences will not show that Spanish enables one to express facts that cannot be expressed in French.

As previously mentioned, there are many different ways to obtain the desired probability distributions. Each is equally successful in explaining our linguistic behavior. However, most will be undetectably inadequate from the point of view of truth. For example, by considering the second hundred mystery numbers, we can come up with a number resembling murk; call it "nurk". Nurk is the number of true propositions in the set of propositions of the form 'Mystery-$(100+n)$ is an even number' (for n between 1 and 100). Just as we defined 'miny' as less than or equal to murk, 'niny' can be defined as less than or equal to nurk. The predicates 'miny' and 'niny' give rise to the same probability distribution over propositions of the form 'n is miny' and 'n is niny'. Yet it is unlikely that the predicates have the same extensions because it is unlikely that murk = nurk. If there is a number n such that 'noonish' means 'within n minutes of noon' spawning the same probability distribution as murk and nurk, it is likely that the number is neither murk nor nurk. However, the number will be identical to one of the cousins of murk and nurk. The good news is that blurry counterparts to vague expressions can have the same probability distributions and extensions. Indeed, since identical numbers are necessarily identical, the blurry counterparts will have the same intensions as well. The definitions will hold across all possible worlds. The bad news is that we cannot know which of the definitions is correct. We can narrow down the field of candidates. But we cannot determine the winners.

The underdetermination of our blurry definitions of vague predicates is reminiscent of the underdetermination found with the reduction of numbers to sets. Whether one follows von Neumann in stipulating that $2 = \{0\} \cup \{\{0\}\}$ or one follows Zermelo in stipulating that $2 = \{\{0\}\}$ is irrelevant to the reduction of number theory. As far as the mathematician is concerned, all of the infinitely many possible definitions of numbers are equally successful. The set theoretic reduction does have the peculiar consequence of raising questions such as 'Is 2 a member of 5?'. Since the alternative definitions give conflicting answers to these peculiar questions, they are *not* extensionally equivalent. So, at most one set theoretic definition of 2 is correct. Paul Benacerraf has used the fact that we cannot know which definition of 2 is correct as grounds for concluding that all of the definitions are false.[33] Others take the underdetermination to be another example of the general phenomenon of indeterminacy of translation and conclude that there is no fact of the matter. Yet others view the underdetermination as showing that we can dispense with numbers in favor of sets.

Since the reduction of vague predicates to blurry ones also features an unbreakable tie between rival definitions, one's position on this reduction may well parallel the one taken in response to reduction of numbers to sets. As a proponent of the epistemic solution to the sorites, I deny that underdetermination (lack of possible knowledge) implies indeterminacy (lack of truth value) or falsehood. Thus I regard the modal version of the argument from ignorance ('*p* cannot be known' therefore '*p* is not true') as being on all fours with other arguments from ignorance. They are only acceptable as enthymemes having as their missing premise a conditional of the form 'If *p* is true, then *p* is known/knowable'. Benacerraf and the indeterminists draw whatever support they have for the missing conditionals from theories of meaning that my reduction is designed to reveal as misconceived. So it would be question begging to object à la Benacerraf that unknowability falsifies all the blurry definitions, or to object that my reduction reintroduces a type of vagueness in the form of translational indeterminacy.

Those who view the reduction of numbers to sets as showing that we can dispense with numbers in favor of sets might well suggest that the blurry reduction shows that we can dispense with vague predicates in favor of blurry ones. Talk of dispensability can be clarified with a distinction between positive, neutral, and negative ontology. One's positive ontology consists of what one affirms to exist; one's negative ontology consists of what one denies to exist; while what one neither affirms nor denies to exist constitutes one's neutral ontology. Thus God is in the positive ontology of the theist, the negative ontology of the atheist, and the neutral ontology of the agnostic. Our ability to paraphrase number talk in terms of set talk allows us to eject numbers from our positive ontology. Since few have denied the existence of numbers, weak ejection into one's neutral ontology is more popular than strong ejection into one's negative ontology. Since a strong ejection of vague predicates leaves one with a position much like Unger/Wheeler scepticism, most will have a parallel preference for placing vague predicates in their neutral ontology. This would enable one to avoid denying the existence of heaps, flowers, and chairs, while affirming the existence of blurry-heaps, blurry-flowers, and blurry-chairs. Since the 'blurry-' prefix would be pervasive, speakers would abbreviate it away yielding a language homophonic with the original. But when speaking without abbreviation, they will not affirm bridge conditionals of the form 'If there are blurry-*F*s, then there are *F*s'. For assent to these conditionals will force the return of vague predicates into one's positive ontology by *modus ponens*. Since these conditionals are more persuasive than the corresponding conditionals bridging sets and numbers, some may argue that this constitutes an important disanalogy between the set-theoretic and the blurry reduction. It may be insisted that conditionals bridging blurry predicates and vague ones are important enough to force the proponent of the blurry reduction to make an identity claim between the two. Under this account, 'heap' is identical to some blurry predicate that cannot be pinpointed.

Perhaps the importance of blurriness to vagueness bridge conditionals could be adequately assessed by comparing them to the bridge conditionals that arise for other reductions. But since the dispensability and identity positions are both compatible with the epistemological approach, I will not attempt to decide between the two of them here. A second unresolved issue is the question of what the rejected theories of meaning should be replaced with.[34] Last, my response to the problem of how rough and ready usage fails to preclude unlimited sensitivity in the case of vague predicates is not so much a solution to the mystery as it is a reduction to another mystery. For my response is to point out that the preclusion does not occur in the case of geometrical predicates and to show that vague predicates can be defined in terms of quasi-mathematical predicates that also illustrate the compatibility between rough usage and unlimited sensitivity. The mechanics of the compatibility is a residual mystery.

NOTES

[1] I thank Peter Unger, Kenton Machina, Christopher Boorse, and two anonymous referees for their many helpful comments and suggestions.

[2] Smith's scepticism about mathematical induction is advanced in 1984, 'The Surprise Examination on the Paradox of the Heap', *Philosophical Papers* **13**, 43–56.
[3] Paul Ziff: 1974, 'The Number of English Sentences', *Foundations of Language* **2**, 530. Ziff also presents this position in his 1984, *Epistemic Analysis*, D. Reidel, Dordrecht, pp. 141–42.
[4] Weiss' restrictions on mathematical induction are specified in 1976, 'The Sorites Fallacy: What Difference does a Peanut Make?', *Synthese* **33**, 253–72. Jay Rosenberg also appears to adopt the invalidity approach judging from his discussion of the ship of Theseus problem on pp. 42–43 of his introductory text *The Practice of Philosophy*.
[5] Ibid. p. 266. The above is actually Weiss' penultimate criterion. His final criterion is designed to handle a technical problem. Since the solution of the problem considerably complicates the criterion's formulation and nevertheless shares the same flaw with its predecessor, I have used the simpler one.
[6] Ibid.
[7] J. Willard Oliver makes the point in his 1967, 'Formal Fallacies and other Invalid Arguments', *Mind* **76**. The asymmetry of validity and invalidity has also been stressed by Gerald Massey in his 1975, 'Are there any Good Arguments that Bad Arguments are Bad?', *Philosophy in Context* **4**, 61–67, and his 1975, 'In Defense of the Asymmetry', *Supplement to Philosophy in Context* **4**, 44–56. The theme is taken up again in his 'The Fallacy Behind Fallacies', *Midwest Studies in Philosophy*, **6**, 489–500.
[8] Russell adopts this position in his 1923, 'Vagueness', *Australasian Journal of Philosophy* **1**, 297–314.
[9] In the first chapter of *Logical Foundations of Probability*, Carnap proposes that prior to formalization, qualitative expressions should be replaced by comparative ones, or better yet quantitative ones. Susan Haack supports Carnap in the sixth chapter of her 1974, *Deviant Logic*, Cambridge University Press, London.
[10] Russell argues this way in 'Vagueness' as does Max Black in his 1937, 'Vagueness', *Philosophy of Science* **4**. M. Kohl criticizes Russell's argument for the universality of vagueness in 1969, 'Bertrand Russell on Vagueness', *Australasian Journal of Philosophy* **47**, 31–41.
[11] Grim discusses this difficulty in 1982, 'What Won't Escape Sorites Arguments', *Analysis* **42**, 38–43.
[12] Unger and Wheeler defended this position in a number of articles. Representative are Unger's 1979, 'There are no Ordinary Things', *Synthese* **41**, 117–54, and Wheeler's 1979, 'On That Which is Not', *Synthese* **41**, 253–72. Sympathy to the incoherence thesis can be found in the writings of W. V. Quine, Michael Dummett, and Bertil Rolf.
[13] I have only seen the autologicality of 'vague' denied once, and that was fifty years ago in a discussion note by Virgil Aldrich. He denies it on p. 94 of his 1937, 'Some Meanings of "Vague"', *Analysis* **4**, 89–95. I first argued for it in 'An Argument for the Vagueness of "Vague"', *Analysis* **27**, 134–7.
[14] Indeed, an incoherence theorist, Bertil Rolf, was the first to point out that examples involving empty extensions refute the compositionality principle. See his 1980, 'A Theory of Vagueness', *Journal of Philosophical Logic* **9**, 315–325.
[15] Putnam makes this point in 1983, 'Vagueness and Alternative Logic', *Erkenntnis* **19**, 297–314.
[16] Cargile criticizes the appeal to intuitionism in 1969, 'The Sorites Paradox', *British Journal for the Philosophy of Science* **20**, 193–202.

[17] They present a low level version in 1985, 'Hairier than Putnam Thought', *Analysis* **25**, 56–58.
[18] Black advances this position in 1963, 'Reasoning with Loose Concepts', *Dialogue* **2**, 1–12.
[19] John L. King: 1979, 'Bivalence and the Sorites Paradox', *American Philosophical Quarterly* **16**, 20.
[20] David Sanford's approach is presented in his 1975, 'Borderline Logic', *American Philosophical Quarterly* **12**, 29–39. Kenton Machina's approach appears in his 1976, 'Truth, Belief, and Vagueness', *Journal of Philosophical Logic* **5**, 1976, 47–78.
[21] Fine's supervaluational treatment of vagueness appears in his 1976, 'Vagueness, Truth and Logic', *Synthese* **30**, 195–210.
[22] Sanford makes this point in 1976, 'Competing Semantics of Vagueness: Many Values versus Super-truth', *Synthese* **33**, 195–210.
[23] Machina makes this criticism in 'Truth, Belief, and Vagueness'.
[24] Those who doubt this point because of misgivings about probabilistic rules of acceptance are urged to consider the preface paradox. Here belief that one of the beliefs expressed in the book is false need not be interpreted as the product of an acceptance rule and yet nevertheless provides an example of rational inconsistent belief.
[25] This objection appears in King's 1979, 'Bivalence and the Sorites Paradox', *American Philosophical Quarterly* **16**, 1–36.
[26] Unger makes this criticism in 'There are no Ordinary Things'.
[27] Hans Kamp: 1981, 'The Paradox of the Heap', in Uwe Monnich (ed.), *Aspects of Philosophical Logic*, D. Reidel, Dordrecht, pp. 254–55.
[28] J. Goguen: 1968, 'The Logic of Inexact Concepts', *Synthese* **19**, 327.
[29] John Stuart Mill: 1974, *A System of Logic*, University of Toronto Press, Toronto, p. 765.
[30] Richard Nisbett and Lee Ross: 1980, *Human Inference*, Prentice-Hall, Englewood Cliffs, pp. 115–16.
[31] Derek Parfit appeals to this principle on p. 239 of his 1984, *Reasons and Persons*, Oxford University Press, New York, as does Graeme Forbes on p. 168 of his 1985, *The Metaphysics of Modality*, Clarendon Press, Oxford, 1985.
[32] Those who suspect that this probability assignment constitutes a fallacious appeal to the principle of indifference should bear in mind that the assignment does not have any of the features exploited by the standard objections to the principle. Specifically, it is not a geometrical probability (Bertrand's paradox), does not involve attributes which vary continuously (von Kries' problem), and the probability does not equal an irrational number (Kyburg's problem).
[33] Benacerraf advances this argument in his 1983, 'What numbers could not be', in Paul Benacerraf and Hilary Putnam (eds.), *Philosophy of Mathematics*, Cambridge University Press, Cambridge.
[34] In an unpublished manuscript, 'Realism and Vagueness', Stephen P. Schwartz suggests a replacement developed from some of Putnam's and Kripke's work on natural kinds.

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