

# Ruby Grant Report: Metaphysical Indeterminacy

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## Abstract

We motivate and consider the ramifications of the thesis that there is worldly (or metaphysical) indeterminacy, not mere semantic indeterminacy (or vagueness) about the boundaries and identities of objects. We give general considerations about what objects are that we think help to show that it is plausible that their lack of sharp boundaries is a feature of them, not just of the language used to describe them. Then we consider the difficulties raised by the fact that it is then natural to suppose further that there can be indeterminacy about the identities of objects. In particular, we focus on the argument of Gareth Evans that purports to show (as we interpret it) that indeterminacy of identity statements could not be due to worldly indeterminacy, on pain of logical incoherence. We criticize the response to this argument of Terence Parsons, a leading advocate of worldly indeterminacy of identity. We offer our own approach, on which the threat of Evans' argument can be effectively defused.

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# 1 Composite Objects versus Mereological Sums and other *abstracta*

## 1.1 Why believe in some objects rather than others?

External objects are in a certain sense on no firmer epistemic ground than the gods of Homer, but we still have good reason to say that paper and pens, tables and chairs, sticks and stones exist, though Zeus and Hephaestus don't. Ordinary objects are not directly grasped as the foundations for all knowledge—they are indirectly justified, and in this way, they are as non-immediately known as the likes of Zeus. It is not obvious exactly what justifies in believing them, but one likely story, Quine's is, simplified, this: The postulation of ordinary material objects helps us to predict and control the triggering of our sensory receptors. Material objects are a conceptual apparatus that link sensory stimulation to sensory stimulation.<sup>1</sup> They help explain and piece together the fragments of sensory input to produce a single coherent picture of the world that enables us to tell causal stories and make reliable predictions. This picture has become even more intricate with the admission of things like subatomic particles and fundamental forces into our ontology. Some of us even countenance abstracta. But whether these things deserve a seat at the table of being depends on whether they can earn their living doing much-needed work for science in the broadest sense.

This last point might be controverted, but there does not seem to be any other way to adjudicate ontological disputes. Our general aversion to arguments that purport to demonstrate that this or that exists *a priori* has something to do with the demand that real entities contribute to science—that they somehow structure the world to assure the sequences of stimulation that our best theories anticipate. *A priori* arguments for the existence of, say, universals or God thus feel as though the philosopher were a magician trying desperately to get something for nothing. This is especially clear when we consider the Ontological Argument and feel a certain instinctive repugnance.

Thus it seems healthy to take as a general attitude that whether we are committed to abstracta depends on whether abstracta are useful, or, at

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<sup>1</sup>See "Things and their place in theories" in [15].

least, in some way beautify or systematize our picture of the world. Perhaps all disagreements in ontology actually are, ultimately, disputes over what is (or is not) a part of this picture. But whether something exists is not a matter that can be settled by just any aesthetic sense. Since this picture represents our best scientific theory of the world, disagreements in ontology are similar to disputes among scientists. So, we adopt the simplest, the strongest theory “into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense”.<sup>2</sup> Thus a condition of adequacy suggests itself. An ontology is *adequate* just in case it maximizes explanation and minimizes postulation. Call this the Maximin principle.

One might reject Maximin on the grounds that it leaves us with an unreasonably austere ontology, for a maximal explanation of any worldly phenomenon need not appeal to anything but the basic particles and forces of physical science. Putatively emergent phenomena like roses, rule governed trends on the New York Stock Exchange, and perhaps even intentionality can be functionally reduced to the stuff of physics and chemistry, so these things have a place in our ontology only if we are willing to admit epiphenomena, which is wasteful.<sup>3</sup> So on pain of cluttering our world with a slum of disorderly elements, Maximin implies that folk ontology is radically false. Yet this objection depends on an improper understanding of what we mean by a maximal explanation, so something should be said about this notion. A maximal explanation is under tension from two opposing forces: the drive for evidence and the need for system. Quine makes a similar point:

Scientific theories should be subject to observable criteria, the more the better, and the more directly the better, other things being equal; and they should lend themselves to systematic laws, the simpler the better, other things being equal. If either of these drives were unchecked by the other, it would issue in something unworthy of the name of scientific theory: in the one case a mere record of observations, and in the other a myth without founda-

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<sup>2</sup>“On what there is”, in [14] pp. 16–17.

<sup>3</sup>See [8] for a robust specimen of this species of argument.

tion.<sup>4</sup>

What we settle for is a trade off. We gain simplicity of theory by generality, which compromises a bit of evidence for the sake of system. So a maximal explanation is an explanation that arrives at an appropriate balance between the drive for evidence and the need for system; which way the scale tips depends on our philosophical temper, what we're trying to explain and maybe other circumstances too. But system demands a kind of impressionism that postulates higher-level entities like roses and beliefs. We say "higher-level" because these things in some sense arise out of fundamental particles. The "atoms" of physics are their building blocks, if you like. They stand in a relation of composition to our more familiar objects. Nonetheless, higher-level entities like billiard balls and pool cues are real. Postulating their existence simplifies theory. To adopt some technical jargon of Dennett: you and I don't take a physical stance when calculating the angle, force, and object of our next shot in a game of eight ball.<sup>5</sup> We think in terms of higher-level composite objects like balls and cues and corner pockets in order to make predictions. Let's call this the familiar object stance.

The kind of reasoning we engage in every day employs the familiar object stance. From playing pool to driving, interacting with others in conversation to thinking about what will happen if I don't pay that bill on time, we use the familiar object stance to deliberate and make predictions that, for the most part, serve us well. And now the rub: there is nothing more to being a real entity than figuring in as a component of causal explanations or reliable predictions via the familiar object stance. This point needs a bit of clarification.

To "figure in as a component" of a causal explanation is to be a postulate of that explanation. A postulate is a theoretical entity—an object whose being is assumed by a theory. Now we need some sort of standard that enables us to determine the entities that our theories assume. Quine has taught us that to ask what assuming an object consists in is to ask what referring to an object consists in. We can answer this question by regimenting our notation, admitting singular and general terms, truth functions, and quantification. Then we are able to say that the entities our theory

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<sup>4</sup>[16] p. 90.

<sup>5</sup>See [1].

assumes are the objects over which the variables of that theory must range in order for the theory to be true. Thus we eliminate dispensable acts of reference and make explicit our genuine ontological commitments. Where there is room for choice between theories, we should choose with a mind toward the maximality of explanation and the minimality of postulation: this is our Maximin principle.

One issue with this sort of semi-realism is that it might appear to make the existence of objects unappealingly “subjective”, or at least, “relative”. Suppose a highly advanced race of supremely intelligent beings from some distant galaxy revealed themselves, and these creatures were no less than Laplacean calculators, capable of predicting market fluctuations on Wall Street at a microphysical level, or of running the table at a game of pool on every turn because they could accurately calculate the angle and force necessary to sink their shots by employing a physical stance. And let’s say these beings can perform these complex calculations in the time it takes us to make our assessments with a familiar object stance. Would it be right to say that from their point of view, ordinary objects like billiard balls and pool cues don’t exist? It might seem that what we are entitled to is not the claim that familiar objects are real, but the more puzzling claim that if one adopts a certain way of looking at the world, ordinary objects are real. But what reason do we have to believe that material objects are really real, one might ask? It appears as though we end up defending folk ontology by offending what Russell has called the common man’s robust sense of realism.

We want to say that the theoretical entities of the familiar object stance are real in the strongest metaphysical sense, not merely relative to some conceptual scheme. The objectivity of their reality can be captured by another technical notion of Dennett’s.<sup>6</sup> Billiard balls and pool cues, indeed composite objects in general, are just patterns of atoms. In other words, composite objects are nonrandom distributions of fundamental physical entities in spacetime. We can concoct a language (absurdly complex though it be) that describes the collision of billiard balls using sets of ordered quadruples, but this information can be compressed and more efficiently described. We would do better to simply say, “At  $t$ , the cue ball took this route and collided with the eight ball thus”.

But, the dialectic continues: the skeptic asks “Are these patterns real?”

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<sup>6</sup>See [2].

Like Dennett, we say: Of course! You can see the eight ball yourself, can't you? But there is more to be said. Patterns exist whether or not we observe them. The aliens that have no apparent need to posit them might simply ignore them, but that does not mean they are not there. We must distinguish the *salience* of a pattern from its reality. The Maximin principle, in effect, tells us simultaneously which objects we should believe are real, and which objects are salient for us. But in principle the two notions can be separated.

Patterns, noticed and unnoticed, are everywhere. The Fibonacci sequence abounds in nature, though most people fail to spot it. And I'm sure many of us have had some experience with "Magic Eye" patterns. That I fail to observe the pattern in a Magic Eye field that you successfully spot does not mean that the pattern there isn't real for me but is real for you. Likewise, that these Laplacean calculators don't observe the patterns that emerge from microphysical phenomena doesn't imply that macrophysical entities are not real, nor even that they are "not real for them", whatever that relativized notion means. We just happen to observe these higher order structures because we see the world through a different lens, so to speak: the familiar object stance.

## **1.2 Composite objects are not mere sums of parts**

So far the project has been positive. We have tried to say something about what composite objects are. Now we will say something about what they are not. Composite objects are not mereological sums, if such things there be.

There are many arguments to be made against regarding ordinary composite objects as sums. For example, we have strong *de re* intuitions about how the world might have been that conflict with the thesis. Dan Hagen might have had one fewer hair on his chin. If Dan were a mere sums of atoms, this intuition would be false, for sums are individuated by their parts. Dan could not have had one fewer hair on his chin. Industrial strength essentialism of this sort is too extreme to be believed.

Let's call the doctrine that material objects are mereological sums of atoms "Monism", because this doctrine does not posit a dualism between a thing and its matter. On this view, the thing simply is the sum of its matter. The monist will not be moved by the meager argument that has

been rehearsed, because she has at her disposal a stock response: modal contexts are opaque. The substitution of coreferring terms in the scope of the modal operator “it is possible that” does not preserve truth, because it is sensitive to the mode of reference. To get around this response we disambiguate sentences in which “it is possible that” occurs primarily. For example, consider this sentence.

Possibly, the bearded mandolin player from Eugene has one more hair.

We can export the singular term in order to rid ourselves of opacity thus.

The bearded mandolin player from Eugene possibly has one more hair.

Now the context is transparent, and the sentence true. But the mereological sum of atoms that Dan is supposed to be could not have one more hair. So, Dan and the mereological sum with which he coincides in space are distinct. This consideration will not persuade the monist. She may simply insist that the bearded mandolin player from Eugene could not have been any more hairy.

The modal argument might not persuade. But there are others. Anyone who knows Dan well enough will agree with me when I say that he is quite disciplined. But it seems odd to say that the matter out of which Dan is composed is disciplined; or that the mereological sum of atoms that he is supposed to be is disciplined. Does it even make sense to say that Dan’s matter is disciplined? Maybe it makes a grammatical English sentence. But surely hunks of matter are not the type of thing that are disciplined. Consider a similar example from Kit Fine: we might say that a statue of bronze is well-crafted, but we would not say that the bronze it is made of is well-crafted. Intuitions of this sort are the basis of a good argument against monism, because there is no independent linguistic evidence for the opacity of the context “\_\_ is disciplined”, or “\_\_ is well-crafted”.<sup>7</sup>

In addition to these considerations, there are others. It seems as though the existence of composite objects is explicable in ways the existence of sums is not, which is compelling reason to believe that sums are entities of a different kind, if they are real at all. Imagine an eight ball on the table.

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<sup>7</sup>See [5].



We might ask why that eight ball exists. Well, because the material out of which the ball is composed was manipulated in the right sort of way. But what about the sum that ball is supposed to be—why does it exist? The sum exists because its parts exist. Though the existence of the parts that make up the sum are necessary for the existence of the eight ball, this much seems to be uninformative as an explanation of the eight ball's existence. It goes without saying that the eight ball exists only if the parts exist. What we are after when we ask why some entity *e* exists is an antecedent event (or series of events) that, in conjunction with those conditions that "go without saying", is (are) sufficient for the existence of *e*. What we want is *e*'s efficient cause. Sums are the sort of thing that can have an efficient cause only in a derivative sense.

To extend the point let's ask whether sums can play causal roles. Do they contribute anything to our picture of the world? I find no reason to think that arbitrary sums-like the fusion of my right index finger and the empty set-play any explanatory role whatever. No new light is shed on the world by postulating the existence of such things. If there are sums that we must postulate in order to maximize explanation, they must be natural sums, like the sum of atoms that compose sticks and stones. But what novel explanatory role do natural sums play? We get along well enough with ordinary material objects. What could these sums explain that is left inexplicable with an ontology of fundamental physical entities and composite objects? It seems we get little for the price we pay by cluttering our ontology with sums. We would do better to repudiate them altogether. Doing so will not require that we give up on the notion of parts and wholes, because it isn't intrinsic to the concept of a part that for any arbitrary collection of individuals there is a sum of all and only those individuals.<sup>8</sup>

What we are tempted to suggest, then, is a restriction on the relation of composition: there is not an object composed of all and only the members of every set of atoms. Only physically fundamental objects and patterns thereof qualify. Although this preserves the intuition that objects are either simple or things with a certain degree of spatiotemporal or functional cohesion, it seems to be committed to the view that there is vagueness in the

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<sup>8</sup>As Simons puts it in [18], p. 108.

world itself, not merely our ways of representing it.<sup>9</sup> So we shall defend the possibility of vague identity. This is a thesis that many philosophers regard with a great deal of suspicion, in part because of Evans' famous one page essay on its incoherence, which we will discuss in great detail later.

### 1.3 *E pluribus unum*

Consider the cloud out yonder; that one, over there. It appears to be a single, ordinary cloud, but no natural (or rational) boundary can be drawn to pick it out, because the swarm of water molecules that constitute the cloud gradually diminishes, leaving a region of space neither definitely part of the cloud nor definitely disjoint. Within this region (infinitely?) many boundaries might be drawn, yielding distinct objects, but each no different in any way relevant to being a cloud. Hence, each distinct object has an equally good claim to be identified with that cloud. We cannot say that any one of these things is the cloud without admitting that all are, nor can we deny that any one of these things is the cloud without conceding that none are. Hence, either there are many clouds out yonder, or there are no clouds at all.

Most (maybe all) material objects are composite, with outlying particles of indefinite status. We might ask, "Is this molecule a part of the cloud", to which a typical response would be, "Well, I suppose, but I don't know". And there is nothing to be known, for there is no fact of the matter. Nothing about our concept of cat and part or the nature of cats and the part-hood relation can settle the question. Therefore, Unger's "problem of the many" is a pervasive problem, one that threatens most ordinary material objects.<sup>10</sup> We suggest, however, that there is a good, straightforward, sense in which there is just one cloud out yonder. This argument will not rely on some clever way of interpreting language, which is the strategy preferred by Lewis [10] and McGee [12]. Instead, we rely on a genuine metaphysical difference between the one cloud and the many candidates.

We take the major premise of Unger's argument to be this:

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<sup>9</sup>It has been argued, on the grounds that ontological vagueness—vagueness of existence especially—is unintelligible or otherwise unacceptable that we therefore should not restrict composition. See for example Sider [17].

<sup>10</sup>See [19]

If something is the boundary of an apparent cloud, then there is, in a region of space centered on the boundary of any apparent cloud, an indefinite number of other boundaries, each of which delimits an entity distinct from the other entities so bounded, and each of these entities differs from the apparent cloud in no respect relevant to being a cloud.<sup>11</sup>

We reject this premise because there is indeed a difference between the cloud and the bounded regions of space that delimit densely arranged swarms of water molecules.

An important respect in which the bounded regions of space are not clouds is that they are merely bounded regions of space, and clouds are not. Clouds might have slightly larger or smaller boundaries than they in fact have. Clouds tolerate the loss of a single molecule, but the loss of a single molecule yields an entirely distinct bounded region of space. Clouds are vague (if only in a derivative sense), but bounded regions of space are precise. Clouds have efficient causes; just ask the meteorologist. What is the efficient cause for that bounded region of space, other than a mental act of selective attention? That bounded region of space exists because we attend to it; our mind picks it out by abstraction. In that sense, we might say these bounded regions are abstract, though the cloud out yonder is concrete. And whatever “many” objects might be alleged to be almost collocated with the cloud (sums or sets of molecules or atoms, etc.), of all these we will suggest that they are abstract objects not on a par with the cloud.<sup>12</sup>

#### 1.4 Taking indeterminacy metaphysically?

So concrete objects are not to be identified with nor reduced to such “precise” objects as mereological sums. But then how should we think of the indeterminacy they seem to be involved in? We consider indeterminacy about their boundaries (or the exact profile of which things are parts and which not) and about their identities.

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<sup>11</sup>[19] p. 424.

<sup>12</sup>*Masses* are different. If there is such a thing as “the matter in” a given region of space (compare “the orange juice in this cup”) that thing might be concrete. But, as discussed above, objects are not identical with the matter of which they are composed.

### 1.4.1 Boundaries

Suppose there is a cloud, yet it is indeterminate just which molecules are parts of it. If concrete objects exist, it is a natural companion thesis that we can name and quantify over them. If they are not to be identified with or reduced to precisely delimited mereological sums, but taken as things in their own right, then it is natural to think that the names and variables we use in talking about them do not suffer from *semantic* indeterminacy—the name simply names the concrete object; the variable has among its range of values the concrete object. Thus the indeterminacy about boundaries—about what is part of what—might itself be regarded either as worldly—as being a fundamental feature of reality—or as being a semantic feature of the relational expression “is part of”. Why regard it as one rather than the other?

Let us see what we can say that both views would agree on. They both agree that some sentences of the form “ $a$  is part of  $b$ ” are indeterminate in truth-value (whatever that amounts to). They will disagree whether there is some relation  $R$  that is (precisely) expressed by the relational predicate “is part of”. But both can agree (setting aside higher-order vagueness) that there is some set of precise relations  $\mathfrak{R}$  such that “ $a$  is part of  $b$ ” is determinately true just in case, for every relation  $R$  in  $\mathfrak{R}$ , the object referred to by “ $a$ ” bears  $R$  to the object referred to by “ $b$ ”. The members of  $\mathfrak{R}$  are the “precisifications” for the expression “is part of”.

Now these relations may be thought of set-theoretically, as sets of ordered pairs, or functions from pairs to truth-values. Or it may be thought that these mathematical objects are mere *representations* of the relations, and that the relations themselves are a different kind of thing: something like “universals”.

If we think of them the first way, then the set  $\mathfrak{R}$  has an obvious sort of equivalence with a different mathematical object: a *partial* function from ordered pairs to truth-values. Looked at this way, it seems we have a choice of characterizing the semantics of “is part of” as either (1) vague expression of all the members of  $\mathfrak{R}$ , resolvable as the expression of any member of it; or (2) expression of the associated partial function (or a suitable mathematical surrogate). Do we have semantic indeterminacy in which precise relation is expressed, or the expression of a relation that is not precise? What would motivate choosing one way of thinking over the other? It is hard to see how anything other than considerations of theoretical tidi-

ness could settle the matter.

If we think of relations the second way, as things that are merely *represented* as mathematical objects for heuristic purposes, then there does seem to be a possible motivation for insisting on regarding the expression “is part of” as being merely semantically vague. For one could hold that relations, by their very nature, are “precise” or determinate. Though there might be a mathematical representation that could be alleged to correspond to an indeterminate relation, just as there are mathematical objects that could be alleged to correspond to precise relations, it could be denied that there is anything in the world that corresponds in the same way. There simply is no indeterminate relation for “is part of” to express.

An alternative view is that there are indeterminate relations, and that the precise relations that are alleged to be alternative precise interpretations for the relational expression are artificial, and may not exist at all, while the indeterminate relation would be taken to be natural. On this view, just as the mountain itself is a more natural unity than any of the precise parcels of matter, so the part-of relation is a more natural relation than any of the precise relations that resolve it.<sup>13</sup>

The views of David Lewis and Vann McGee hold that the English “is part of” is actually already precise. The only way that a sentence of the form “ $x$  is part of  $y$ ” can be indeterminate is if the *terms* connected by the relational expression are semantically vague. But we reject this position when we hold that there really is such a thing as the one cloud there that we refer to with an expression like “that cloud” and “that molecule”. Thus we will have sentences of that form that are indeterminate even though the terms  $x$  and  $y$  are not semantically vague. We are then left with the question whether indeterminacy in such a sentence requires that the *relational expression* be semantically vague. To say “no” is to embrace worldly indeterminacy.

Once we do say “no”, however, we might be tempted to ask, in a case of indeterminacy not attributed to semantic vagueness, whether the source

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<sup>13</sup>What evidence is there that there exist all those precise relations? One might show that they exist by connecting them with the mathematical objects that represent them. Each such representative corresponds to at least one relation: the relation that holds of  $(x, y)$  just in case the mathematical object contains  $(x, y)$  (or maps  $(x, y)$  to the truth-value “true”). Thus there are at least as many real relations as there are mathematical relations-in-extension.

of the worldly indeterminacy is the objects (that is, the semantic values of the terms) or the relation (that is, the semantic value of the relational expression). But one might think that this question is misguided, and that there couldn't be a good reason to "place the blame" one way or the other. Once semantic vagueness has been eliminated, any indeterminacy that remains is *worldly*, but, beyond that, one should not try to locate the indeterminacy.<sup>14</sup>

### 1.4.2 Identity

When it comes to identity, there seems to be less wiggle-room: if a simple sentence of the form  $\lceil a = b \rceil$  is vague or indeterminate, and we do not regard this indeterminacy as flowing from semantic indeterminacy of the terms  $\lceil a \rceil$  and  $\lceil b \rceil$ , then it is extremely difficult to see how the remaining indeterminacy could be merely semantic.

What do we mean by indeterminacy about identity? What we have in mind is best understood by imagining a wide range of cases, at one extreme of which there is a clearly true identity proposition, at the other extreme of which there is a clearly false identity proposition, and such that adjacent cases are extremely similar. Here is an example we will continue to work on, one that involves indeterminacy of survival. Schematically, the cases share this in common: a cat enters the mad scientist's laboratory and the mad scientist performs some kind of "operation" involving the cat. The result of the operation then exits the laboratory. At one extreme, we have clear survival: imagine that the "operation" is a gentle pat on the head; the cat might have lost a hair off its head, but we would say that it certainly survives. At the other extreme, we have clear non-survival: the cat is dismembered and the resulting pieces are collected in a box; the matter in the box certainly does not constitute a cat, so the cat has not survived the operation. In between are a range of pairwise-similar cases. Somewhere around the middle are operations like these: the cat's head is removed and functionally joined to the body of another cat, and the resulting "cat" leaves the lab. The exact details of the cases need not concern us: all that matters is that there can be events involving a thing such that it is indeterminate whether the thing survives the event.

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<sup>14</sup>This view seems more plausible to us. Cf. Parsons remarks in chapter 2, section 2 of [13].

But then it is hard to see how we do could fail to have indeterminacy of identity: for if  $\lceil a \rceil$  is a name of the thing that entered the process, and  $\lceil b \rceil$  is a name of the thing that exited, the proposition  $\lceil a = b \rceil$  will be indeterminate in truth value.

Since we are suggesting that we can refer without indeterminacy to concrete objects, the only way to regard the resulting indeterminacy of identity propositions as being *semantic* rather than worldly is to regard the symbol for identity as semantically vague. But this does not seem like a plausible option. One way that it could be made out is if it were suggested that Peter Geach's doctrine of "relative identity" is the correct view of identity. On his view, there is no such thing as plain identity; identity is always relative to a "sortal". So we can speak of  $a$  and  $b$  as being *the same cat*, but not (or only elliptically) as being *the same*. But even if relativized identity propositions make sense, absolute identity seems to be definable in terms of it:  $a$  and  $b$  are *absolutely* the same if *for every  $F$ ,  $a$  and  $b$  are the same  $F$* . Even if we could come up with some relations that the identity symbol might be regarded as being "unsettled between", we suspect that there will be a natural way to define absolute identity in terms of them. But if absolute identity is always definable, then it seems that there is a relation of identity on which we have a clear grasp, and there is every reason to think that the identity symbol already expresses it.<sup>15</sup>

## 2 The Evans argument

If concrete objects are not to be identified with or reduced to precisely delimited things, then, since there are events involving them in which survival is indeterminate, there are indeterminacies about their identities; since the identity symbol is not semantically vague, we have worldly indeterminacy of identity. But we are immediately faced with a serious logical problem.

Gareth Evans' much-discussed short paper "Can there be vague objects?" suggests roughly the following: worldly indeterminacy involving identity is logically incoherent. He gives a formal argument which derives, from the premise that it is indeterminate whether  $a$  is  $b$ , the conclusion that

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<sup>15</sup>This is certainly the view of almost all philosophers who have contributed to the current debates on vagueness.

$a$  is not  $b$ :

- (1)  $\nabla a = b$  Premise
- (2)  $\lambda x(\nabla x = b)a$  From (1) by abstraction
- (3)  $\neg\nabla b = b$  By logical truth of reflexivity of identity
- (4)  $\neg\lambda x(\nabla x = b)b$  From (3) by abstraction (and commutation with  $\neg$ ).
- (5)  $a \neq b$  From (2) and (4), *Reductio*, Leibniz' Law (restricted<sup>16</sup>)

$a$  and  $b$  may be taken as schematic for “singular terms” or “referring expressions” of any sort: names, definite descriptions, or any others. But how one evaluates the argument may turn on what sorts of terms they are: given that there is any indeterminacy at all, one can craft definite descriptions that would clearly suffer from an inherited indeterminacy that would invalidate the argument. If the sentence  $P$  is indeterminate, consider  $\lceil$ the object  $x$  such that either ( $P$  and  $x = 1$ ) or ( $\neg P$  and  $x = 0$ ) $\rceil$ . If this description is  $a$ , and  $b$  is ‘1’, then, apparently, premise (1) is true, but the argument should be invalid: apparently, premise (2) would not follow. The indeterminacy that manifests itself in some unrelated sentence should not guarantee that there is an object indeterminate in identity between being 1 and being 0.

If  $a$  and  $b$  are natural-language proper names, then the question of the validity of the argument becomes more interesting. As David Lewis suggests, it is fairly obvious that there are proper names  $a$  and  $b$  such that  $\lceil\nabla a = b\rceil$  is true. Lewis plausibly takes Evans to be suggesting that *if* the indeterminacy involved were worldly, *then* the rest of the argument would be valid: but then we would have shown that there is at least a sort of incoherence, if not a contradiction, since  $\lceil\neg a = b\rceil$  would follow.<sup>17</sup> But, Lewis suggests, if the indeterminacy involved were semantic, then at least one of steps (2) and (4) would fail to follow from the true (1) and (3). (Lewis helpfully suggests that one compare by considering the logical analogy between determinacy and necessity: the semantic indeterminacy of the terms  $a$  and  $b$  is analogous to the non-rigidity of definite descriptions, and one can see how the argument goes wrong if we think of  $\nabla$  as meaning “it

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<sup>16</sup>As we will discuss below, if we had unrestricted Leibniz' Law, we could get (5) from (1) and (3) alone, using *Reductio*. The version here is intended to be restricted so that the property abstraction steps are needed to invoke the Law.

<sup>17</sup>[9]



is contingent whether” and use, as  $a$  and  $b$ , suitable descriptions.)

## 2.1 Parsons’ account

Terence Parsons defends the possibility and coherence of worldly indeterminacy of identity.<sup>18</sup> He suggests that the Evans argument is not valid. He offers different explanations for the failure of the argument, depending on how one understands the abstraction notation.<sup>19</sup> If, by definition,  $\lambda$ -abstracts denote genuine properties, then the steps from (1) to (2) and from (3) to (4) are invalid, for there is no genuine property expressed by such an expression as “being indeterminately identical with  $a$ ”. Alternatively, if, by definition,  $\lambda$ -abstraction and conversion are valid, then the argument is invalid because the step from (2) and (4) to (5) is invalid. The apparent “difference” registered by (2) and (4) is a mere appearance, since, again, there is no genuine property expressed by such an expression as “being indeterminately identical with  $a$ ”.

It is worth noting now another aspect of Parsons’ account. Parsons accepts the general validity of Leibniz’ Law. From

$$a = b$$

and

$$\phi(a)$$

it is legitimate to infer

$$\phi(b).$$

This suggests that one could use a simpler variation on the Evans argument to demonstrate the incoherence of indeterminate identity, using steps that Parsons would accept. The argument goes like this:

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<sup>18</sup>Parsons develops his views extensively in [13]. His is by far the most clear and developed treatment of worldly indeterminacy of identity in the literature.

<sup>19</sup>[13] pp. 48–49

- (1)  $\nabla a = b$  Premise
- (2')  $a = b$  Suppose for *reductio*
- (3)  $\neg \nabla b = b$  By logical truth of reflexivity of identity
- (4')  $\nabla b = b$  From (1) and (2') by Leibniz' Law.
- (5)  $a \neq b$  By *reductio*

Parsons rejects step (5). He accepts only a modified form of *Reductio*, on which all that would follow is

- (5')  $\neg \mathcal{D}a = b$

where  $\mathcal{D}$  is the determinacy operator.<sup>20</sup> Thus contrapositive forms of Leibniz' Law are not valid. In particular,  $a \neq b$  does not follow from the pair  $\phi(a)$  and  $\neg\phi(b)$ . On the other hand,  $\neg\mathcal{D}a = b$  does follow.

There is a connection to Parsons' rejection of the existence of a property of being not determinately identical with  $b$ . He asserts that the reason why the inference from  $\phi(a)$  and  $\neg\phi(b)$  to  $a \neq b$  is invalid, in a given case, is that the context  $\phi(\ )$  does not express a genuine property.<sup>21</sup> If it did express a genuine property, the inference would go through.

We find this idea of Parsons unintuitive, given that he claims to be defending a *worldly* conception of indeterminacy of identity.

## 2.2 Against Parsons on the non-worldliness of semantics

There is a meta-linguistic variant of Evans argument that it will now be helpful to consider. It also helps to show where we disagree with Parsons.

- (1') ' $a = b$ ' is not true Premise
- (2'') ' $a$ ' and ' $b$ ' are Millian Premise
- (3'') ' $b = b$ ' is true Logical truth?
- (4'')  $val('a') \neq val('b')$  From (1') and (3'')
- (5)  $a \neq b$  From (2'') and (4'')

Here "*val*" expresses "the semantic value of". Parsons accepts that in a case of indeterminate identity expressible with the terms  $a$  and  $b$ , (1') will be true. (2'') is a premise, and Parsons accepts (3''). Would he accept the inference to (4'')? It seems that anyone who accepts a general principle of the compositionality of semantic values will be strongly drawn to infer (4'') from (1') and (3''). He might suggest that we are covertly using

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<sup>20</sup>See section 2.6 of [13]. Parsons also refers to the  $\mathcal{D}$  operator, which he symbolizes as " $!$ ", as the "truth-operator".

<sup>21</sup>[13] p. 37

a contrapositive of Leibniz' Law, which, as we have seen, he rejects. But, it seems to us that (1') and (3'') express genuine properties of the relevant sentences, and, indirectly, express genuine properties of the semantic values of the terms  $a$  and  $b$ , so that (4'') should follow, even according to Parsons.

Let us spell out the argument in a slightly different way. Again, let "val" express "the semantic value of".

- |      |                                                                                                 |                          |
|------|-------------------------------------------------------------------------------------------------|--------------------------|
| (1*) | $val('a = b') \neq val('b = b')$                                                                | Premise                  |
| (2*) | $\exists f[val('a = b') = f(val('a'), val('b'))$<br>and $val('b = b') = f(val('b'), val('b'))]$ | Compositionality         |
| (3*) | $val('a') \neq val('b')$                                                                        | (1*), (2*), Leibniz' Law |
| (4*) | $val('a') = a$ and $val('b') = b$                                                               | Premise                  |
| (5)  | $a \neq b$                                                                                      | From (3*) and (4*)       |

We see the compositionality of semantics as requiring this: *If 'a = b' and 'b = b' differ in semantic value, then so do 'a' and 'b'.* We take it that Parsons disagrees, since he seems to be willing to allow that 'b = b' and 'a = b' differ in that the first is true and the second is neither true nor false, while it is indeterminate whether 'a' and 'b' co-refer.

The point of our consideration of the meta-linguistic variant of the Evans argument is that it seems to force Parsons to admit that the semantics of the terms  $a$  and  $b$ , and the sentences that involve them, are not matters involving genuine properties and relations among things. Otherwise, one way or another, we will be able to use a contrapositive version of Leibniz' Law to infer  $a \neq b$  from  $\nabla a = b$  (when  $a$  and  $b$  are Millian). Parsons is well aware of the sorts of considerations we have given, and, discussing closely related ones, writes

... conceptual or semantic predicates do not necessarily stand for worldly properties; such predicates instead characterize parts of our conceptual apparatus. 'N refers to  $x$ ' is a paradigm case of a predicate that does not stand for a property of  $x$ .<sup>22</sup>

We find this way of thinking unattractive. Language is part of the world it talks about, and the relations that connect them are just as real and natural as other relations among parts of the world. The fact that 'Snowball' refers to a certain rabbit, for example, seems no different in kind from the fact that a certain dog is looking at Snowball. One might, on the grounds of an

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<sup>22</sup>[13] p. 152

extreme Democriteanism, deny that the relation involved in the latter fact is genuine, because there are really only atoms and void, and there are not such things as rabbits and dogs and looking. But that sort of position is not under consideration here. If we are willing to consider the indeterminacy of identity of such things as cats and ships as an example of worldly indeterminacy—rather than conceptual ambiguity or confusion—it seems, we ought to include natural language and its properties as parts of the world as well.

### 2.3 Challenging $D_b = b$

In the many discussions of the Evans argument, premise (3) generally goes unchallenged. We would like to consider rejecting it. Consider again the modified Evans argument just given. We tend to find the *Reductio* step compelling. Or look at the original Evans argument. We tend to find the inference from (2) and (4) to (5) to be compelling. These inferences seem more compelling than premise (3) itself. Something has to give, and this is the place to look: The argument is valid, but unsound.

But note well that we would not accept the validity of the argument if one or both of  $a$  and  $b$  are definite descriptions, just as we would not accept the inference from

It is necessary that the winner is the winner.

and

It is not necessary that John is the winner.

to

John is not the winner.

We uphold the validity of the Evans argument (and its variation) only when  $a$  and  $b$  are taken as object-variables—that is, when they are taken to be the sorts of variables that show up, for example, as temporary names in the course of reasoning from existential premises, as in “Suppose that there is a largest prime number. Call it  $a$ . . . .” We are skeptical that the argument is valid when natural-language proper names are used in place of  $a$  and  $b$ : in those cases, we might accept premise (1) and accept premise (3), but reject the inference to (5). (This will be discussed further below.) As

discussed above, we will conclude, from the fact that  $a$  and  $b$  are not inter-substitutable without change of semantic value of the whole sentence, that  $a$  and  $b$  differ semantically. But we will conclude that they are not Millian terms, and that the (determinate!) semantic difference between them explains why the inference to (5) is invalid.

Of course many will think that rejecting (3) is ridiculous, and all the more so when we are clear that we are taking the terms  $a$  and  $b$  as object-variables. For, it is thought, no matter what indeterminacy is, for any object  $b$ , that  $b = b$  is just a logical truth—a truth that flows from the nature of identity, one of the clearest, most basic facts that there is. If anything at all is determinate, the thought goes, it is that  $b = b$ . On the contrary, we are inclined to think that it is a consequence of it's being indeterminate whether  $a = b$  (using ' $a$ ' and ' $b$ ' as object-variables) that it not be determinate that  $a = a$ , and that it not be determinate that  $b = b$ .

Intuitions must be loosened up a little. Keep in mind that we are starting with the assumption that there can be an object  $a$  and an object  $b$  such that it is indeterminate whether  $a = b$ . This assumption immediately takes us into wild territory, not domesticated by familiar formal logic: so we should be open to the failure of traditional assumptions.

One line of consideration that may help is this: Suppose it is indeterminate whether  $a = b$ . Then there is an unusual feature of  $a$ , that we might express by saying that there is indeterminacy about  $a$ 's identity, or that it is indeterminate just what  $a$  is. So is it still obviously guaranteed that there is no indeterminacy about  $a$ 's being  $a$ ?

The assumption that for any object  $b$ ,  $b = b$  has not (yet) been rejected. What is rejected is that for any object  $b$ , it is determinate that  $b = b$ . Now, one might wonder how the following could possibly be the case

(6)  $b = b$  and it is not determinate that  $b = b$ .

One observation is in order: (6) could never be determinately the case, given certain plausible assumptions about determinacy. For the following would seem to be a plausible pattern of inference:

$\mathcal{D}(P \wedge \neg \mathcal{D}P)$

$\mathcal{D}P \wedge \mathcal{D}\neg \mathcal{D}P$     Distributing  $\mathcal{D}$  over  $\wedge$

$\mathcal{D}P \wedge \neg \mathcal{D}P$     (Determinacy requires truth)

It is clear that we would never want to accept an instance of  $\mathcal{D}(P \wedge \neg \mathcal{D}P)$ . If *Reductio* is valid, we can infer the negation of any such.

## 3 Theories of indeterminacy of identity

### 3.1 Parsons' approach

Parsons takes it as given and basic that there are objects, properties of objects, and relations among objects. Further he takes it that for each object and property, there are (at least) three possibilities: either the object has the property, the object lacks the property, or it is indeterminate whether the object has or lacks the property.<sup>23</sup> Indeterminacy of identity is simply a special case: it occurs when there are objects  $o_1$  and  $o_2$ , and it is indeterminate whether the relation of identity holds between  $o_1$  and  $o_2$ .

One might suggest that in such a situation, we can concoct a property that  $o_1$  lacks and  $o_2$  has: namely the property of being a thing  $x$  such that it is determinate that the relation of identity holds between  $x$  and  $o_2$ . This is exactly what the Evans argument tries to do. But, of course, Parsons rejects the claim that there is such a property. Similarly, it can happen that  $o_1$  does have some property, being  $F$ , say, while it is indeterminate whether  $o_2$  has  $F$ , and yet, simultaneously, it is indeterminate whether  $o_1 = o_2$ . This is because there is no *further* property of "determinately having  $F$ " that  $o_1$  has and  $o_2$  lacks.

When there is indeterminacy about whether an object has a property, sentences that say that the object has the property will be neither true nor false, according to Parsons. Hence, a language that talks about objects and properties that are susceptible to indeterminacy will not be bivalent, and we will need a non-classical semantic theory and logic for such languages.

### 3.2 Semantic indeterminacy of terms

On Parsons' view, indeterminacy of identity is a special case of worldly (metaphysical) indeterminacy. Much closer to the mainstream is the idea that indeterminacy of identity is really indeterminacy of identity *statements*, and that it is to be traced not to semantic indeterminacy of the identity-symbol, but to the semantic indeterminacy of (at least one of) the terms that flank that symbol.

We will consider here only the supervaluational approach to semantic indeterminacy. This is the most popular, and, in our judgment, most plau-

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<sup>23</sup>Section 2.2 of [13].

sible approach to semantic indeterminacy. It is favored by David Lewis, among others, and we will focus on his version of it.<sup>24</sup>

Lewis' account says that sometimes we have not decided on exactly which object is to be the referent of some term, though we have greatly narrowed the range of candidates. For example, we have not decided exactly which of many overlapping objects the name 'Tibbles' refers to. The many candidates are mereological sums that differ from one another only in the inclusion or exclusion of a few little parts. Call these the p-cats. There is a co-ordinated indecision about the extension of the common noun 'cat': we have not decided exactly which things are to count as being in the extension of 'cat', but we have decided that any candidates for being the referent of 'Tibbles' is also a candidate for being in the extension of 'cat'.

The semantics Lewis suggests is supervaluational: a vague sentence is true iff in every (reasonable) precisification of the whole language, the (resulting, precisified) sentence is true. Similarly for falsity. Thus 'Tibbles is a cat' will be true, because in every reasonable precisification of English, the referent of Tibbles will be in the extension of 'is a cat'. But if  $h$  is a hair that is a borderline case for being a part of Tibbles, then there will be one precisification in which 'Tibbles' refers to a thing that includes  $h$ , and there will be one in which 'Tibbles' refers to a thing that does not include  $h$ . Thus ' $h$  is part of Tibbles' will be neither true nor false—in one precisification it is true, in another it is false.

The determinacy operator  $\mathcal{D}$  is treated semantically like the necessity operator in possible-worlds semantics, but the different reasonable precisifications play the role of the possible worlds. Thus, a sentence of the form  $\mathcal{D}\phi$  is true at one precisification iff  $\phi$  is true at all precisifications. Similarly, an open sentence  $\mathcal{D}\phi(x)$  is satisfied on a variable assignment  $\sigma$  iff  $\phi(x)$  is satisfied on  $\sigma$  at every precisification.<sup>25</sup>  $\nabla$  may be defined in terms of  $\mathcal{D}$ . Schematically,  $\nabla P$  is  $\neg \mathcal{D}P \wedge \neg \mathcal{D}\neg P$ . Semantically,  $\nabla\phi$  is true at a precisification just in case  $\phi$  is true in some and false in some precisifications.

Now let ' $p$ ' name one of the p-cats that 'Tibbles' can acceptably be precisified as referring to. Consider the sentence ' $\nabla$ Tibbles= $p$ '. This will be true, because there is one precisification in which 'Tibbles' is taken to re-

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<sup>24</sup>Lewis' [10] is a well-motivated and clear presentation of the view as applied to terms; Fine's [4] is a *locus classicus* of the general idea.

<sup>25</sup>The precisifications share a single domain of quantification, so this will make straightforward sense.

fer to  $p$ , and also another in which ‘Tibbles’ is taken to refer to something else; these two precisifications give different truth values to the embedded sentence ‘Tibbles= $p$ ’. And consider the sentence ‘ $\exists x \nabla x = p$ ’. This sentence will be false. The quantifiers range only over the genuine objects. For any variable assignment, whatever value it gives to  $x$  will be the same at each precisification. And the name  $p$  does not suffer indeterminacy of reference: it names the same thing at each precisification. So on any variable assignment, the open sentence ‘ $x = p$ ’ will be either satisfied at all, or dissatisfied at all precisifications. Hence for each assignment, ‘ $\nabla x = p$ ’ is dissatisfied; hence the quantified sentence is false.

### 3.3 Super-resolution

In section 5.4.3 of [13], Parsons introduces what he calls “super-resolutions” as part of a way of thinking about worldly indeterminacy that in some ways is quite like the supervaluational approach to semantic indeterminacy.<sup>26</sup> Indeterminacy can be *resolved*: if it is indeterminate whether John is bald, this indeterminacy can be resolved in such a way that John is bald, or, equally happily, in such a way that John is not bald. Resolution need not be thought of as resolving a *semantic* indeterminacy (though it can be, and that is how traditional supervaluation has thought of it). Rather, we may think that there is indeterminacy in the world in the very state of affairs of John’s being bald: it is simply indeterminate whether that object has that property. The central idea is that indeterminacy in the world entails that there be multiple equally acceptable resolutions of that indeterminacy; determinacy manifests itself as there being only one.

We have just motivated the notion of resolution by talking about resolving a single case of indeterminacy—John’s being bald. But we should also consider the idea of simultaneously resolving all of the indeterminacy in the whole world. We might then conjecture that what is determinately the case would be what is the case in every world-resolution, what is indeterminate would be the case in some but not others, and what is determinately not the case would be the case in none. This would make indeterminacy formally like vagueness, as construed on the supervaluationalist approach to vagueness: the various world-resolutions play the role

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<sup>26</sup>Parsons’ super-resolutional approach is similar in spirit to the approach to vagueness suggested by one author of this paper in his doctoral dissertation; [6].



of the various admissible precisifications of the language.

Parsons introduces super-resolutions only in order to diagnose certain intuitions that run against his main account. The issues concern complex sentences, like “John is bald or it is not the case that John is bald” and “If John is bald, then Jack is bald” (where John is hairier than Jack, though it is also indeterminate whether Jack is bald). Some have the sense that despite the indeterminacy of John’s being bald and of Jack’s being bald, these complex sentences are true, and their denials false. Parsons notes that it is very natural to suggest that these sentences are true in every super-resolution of the world, and hence determinately true; and thus suggests that the intuitions flow from a super-resolutorial perspective. Parsons himself goes for a truth-value functional approach that generally yields different truth-values for complex sentences, but he takes an attitude of tolerance toward those who insist on seeing the complex sentences differently—one can interpret complex sentences super-resolutorially, and get different results. It is almost, it appears, merely an issue of “semantics”; the super-resolutorial and the truth-value functional approach are just different ways of interpreting the logical connectives, and perhaps both are reasonable interpretations.

Yet, we feel, the idea of super-resolution provides an importantly different heuristic from Parsons’. Parsons gives diagrams which picture the (unresolved) situation, and explains how one can calculate, from a given diagram of indeterminacy, diagrams that picture the resolutions of the indeterminacy, but he focuses on the diagram of the unresolved situation. We feel that an advocate of super-resolution (as a heuristic) should suggest that we go the other way around: focus on representations (perhaps including diagrams) of the resolutions, and calculate the representation of the unresolved situation from them. The motivation for this orientation is in part heuristic: it is easier to understand a diagram of a determinate situation than an indeterminate one; and, perhaps, our best grasp on the indeterminate is mediated by grasping its resolutions.<sup>27</sup>

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<sup>27</sup>Lewis, in [10] complains that he has no clear grasp on what ontological indeterminacy for an object’s spatial extent would be. He says that three pictures come to mind: multiple overlap of possible precisifications, ignorance of the true location of the boundary, and the fading away of a kind of presence that admits of degree. Yet none of these, he thinks, is a picture of ontological indeterminacy. We suggest that perhaps the first, albeit indirectly, is.

But there is another aspect to our motivation, that runs deeper. At the center of Parsons' metaphysics of indeterminacy is the notion of an (indeterminate) state of affairs.<sup>28</sup> He gives no theory of states of affairs as entities,<sup>29</sup> but takes it as given that the world contains objects and properties, and that, given an object  $o$  and a property  $F$ , there is the state of affairs of  $o$ 's being  $F$ , which may obtain, fail to obtain, or be indeterminate. States of affairs are thus thought of as some kind of combination of objects and properties. Parsons apparently takes the articulation of the world into objects and properties to be *prior* to its articulation into (possible and actual) states of affairs.

The connection to the diagrams of the unresolved reality and of its resolutions is this: There is a single collection of objects and properties (and the induced combinations of these into states of affairs), such that the unresolved situation basically *is* the sum total of the holdings, failures to hold, and indeterminacies of holding, of these states of affairs.<sup>30</sup> Each resolution of the world, on Parsons view, will involve exactly the same states of affairs; they will differ from the diagram of the unresolved situation exactly by picturing each state of affairs (within it) as either holding or failing to hold.

But perhaps the indeterminacy of the world involves indeterminacy about which states of affairs there are. If it is indeterminate whether  $a$  is

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<sup>28</sup>Section 2.2 of [13].

<sup>29</sup>He thus avoids dealing directly with such a question as this: If it is indeterminate whether  $a = b$ , but determinate that  $a = a$ , is the state of affairs of  $a$ 's being identical with  $b$  not distinct from the state of affairs of  $a$ 's being identical with  $a$ ?

<sup>30</sup>Thus it would appear that there can be a diagram showing the relationships between the objects and the properties that correctly pictures this unresolved reality. This raises problems about higher-order indeterminacy: we would suggest that the relation of the real world to a mapping of a set of states of affairs as true, false, and indeterminate would be a *resolution*, and artificial, just as much as a mapping of a set of states of affairs to true and false.

This suggests another sort of argument for taking resolutions as the right heuristic: the set of all admissible resolutions is itself a mere resolution (of higher-order indeterminacy), and so forth. If any way of representing the indeterminate world can be systematically correlated with sets of resolutions (or sets of sets of resolutions, etc.), then it too can offer, at best, a mere resolution. Whether this argument persuades us that *all* representation of the indeterminate world is really just a resolution of it depends on how the non-classical representations are supposed to work.

$b$ , then it would seem to be indeterminate whether the state of affairs of  $a$ 's being identical with  $b$  and the state of affairs of  $b$ 's being identical with  $b$  are the same or different. (This is why we will not accept, as Parsons apparently would, that the first state of affairs might determinately hold, while the second does not determinately hold.) It seems to us that Parsons invites us to think of these as two distinct states of affairs. Further, it seems that there could be an object  $x$  such that it is indeterminate whether  $x$  exists. When this is the case, it should be indeterminate whether any states of affairs involving  $x$  exist.

### 3.3.1 A defense of super-resolution

We offer a further consideration in favor of super-resolution.

There seem to be at least three virtues that a representation of the world can have: accuracy, completeness, and explicitness. We conjecture that one representation is better if they possess two of these virtues to the same degree, but one possesses the third virtue to a greater degree. Let us ignore, for the moment, any other virtues that representations can have. (We hope that what we ignore here will not affect our argument.)

The super-resolutionist says that the first word about indeterminacy is that where there is indeterminacy, there is no best representation. The indeterminate is susceptible to different representations, each of which is maximally, but equally, good. To deny this is to suggest that we could, despite indeterminacy, in principle, formulate a representation that is maximally good—that stands out from its competitors as either more accurate or more complete.

One can see why this might be the case if indeterminacy were merely semantic indecision in a system of representations. For the sake of argument, let us assume that the English predicate “is bald” is semantically vague in this way. There might be a representation in a completely precise language that is more accurate and complete than any other in that language. (It does not contain “is bald”, though it might tell us how many hairs are on people’s heads, and how they are arranged. For this argument, we may pretend there is no indeterminacy about such matters. )

Could it be criticised as incomplete because it does not represent anyone as being bald? There seems to be a good defense against this charge: First, if it represents John as having virtually no hair on his head, then it might be said to *implicitly* represent that John as bald. Second, and more

importantly, there is no property of being bald, so the apparent failure of explicitness here is merely apparent. There is no fact reported by “John is bald” that is not already *explicitly* reported by the precise representation.<sup>31</sup>

But if indeterminacy is worldly instead of semantic, then the situation is different. There is a fact reported by “John is bald” that is not included, at least, not explicitly included, in *any* description in *any* precise language. But let us consider what happens when we include explicit reports of facts like this.

Imagine a typical *sorites* paradox for “is bald”. Let man #1 have no hair at all on his head, and let man  $\#(n+1)$  have one more hair on his head than man  $\#n$ , so that the men are pairwise indistinguishable, and man #100,000 is clearly not bald.

Consider first just the atomic sentences of the form “Man  $\#n$  is bald”, and the negations of these sentences. Any good description in a language that includes these sentences will include “Man #1 is bald” and “Man #100,000 is not bald”. But what is the last  $n$  such that it will include “Man  $\#n$  is bald”?<sup>32</sup> It seems that no matter which is the last one that is included, there is an alternative description that includes one more or one fewer, that is equally good. To deny this is to deny, at least, the existence of what is usually referred to as “higher-order vagueness.”<sup>33</sup> We find this denial to be equally implausible as the denial of (this sort of vagueness) in the first place—that is, as the epistemicist conception of vagueness.

But if we are not epistemicists, nor do we deny higher-order vagueness, nor do we think indeterminacy is mere semantic vagueness, then we are left admitting that there is no maximally good description of the world.

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<sup>31</sup>So does “John is bald” report no fact at all? Surely it reports something. The view has to be that the way that that sentence relates to the facts is not straightforward, due to its semantic vagueness. But however it connects to the facts, there is nothing it “reports” that could not be directly expressed in some precise language. Perhaps one needs logical combinations of the precise descriptions above, but there is no need to assume that we can find some *translation* of “John is bald” into the precise language. In fact, since “is bald” is vague, there is good reason to think that “John is bald” *cannot* be translated into the precise language.

<sup>32</sup>To be exact about quotation marks: what is the last  $n$  such that  $\ulcorner n \urcorner$  is a numeral that designates  $n$  and it includes  $\ulcorner \text{Man } \#n \text{ is bald} \urcorner$ ?

<sup>33</sup>And if bivalence is accepted, then it is to deny that there is any indeterminacy at all. It is conceivable, and indeed logically neat, that we accept bivalence and nevertheless accept indeterminacy. See [11] and [6], and Fine’s discussion of *Truth<sub>T</sub>* in [4].

This does not tell us that much about how we should think about indeterminacy. But it seems to us to support the super-resolutionist heuristic of imagining alternative equally good descriptions, and regarding a supervaluation of them as a reasonable guide to what is determinately the case. That is, if some representation shows up in every maximally good description of the world, it is reasonably regarded as being determinately true; if it shows up in none, then it can be regarded as being determinately false.<sup>34</sup>

### 3.3.2 Our method

We hope that this argument helps to motivate the super-resolutionist view. But even if it is accepted that this view is worth exploring, there is an important issue that we have not settled. If there is indeterminacy about some proposition expressed by  $P$ , will each maximally good description nonetheless contain exactly one of  $P$  and  $\neg P$ ? This is closely related to the issue of whether classical logic is correct, despite indeterminacy. For simplicity's sake, we will assume so, as super-resolutionists, for the rest of this paper.<sup>35</sup> Thus the maximally good descriptions in a given language that we look at will be "classically complete" in the sense that for each sentence in the language, either that sentence or its negation is included in the description.

We must distinguish between a language that does not talk about determinacy and indeterminacy from one that does. We will make this concrete by distinguishing between the unadorned language  $\mathcal{L}$  and the enhanced language  $\mathcal{L}^D$  that contains, in addition, the determinacy operator (and other operators definable with it). Super-resolution suggests that considering the maximally good descriptions in  $\mathcal{L}$  will shed light on, if not even fix, the maximally good descriptions in  $\mathcal{L}^D$ . For example, if  $P$  and  $\neg P$  each show up in at least one maximally good description in  $\mathcal{L}$ , then we should expect  $\nabla P$  to show up in at least one, and very likely in all, maximally good descriptions in  $\mathcal{L}^D$ . A simple conjecture of the relation between the descriptions in  $\mathcal{L}$  and those in  $\mathcal{L}^D$  would be this:

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<sup>34</sup>Cf. [12], in which virtually the same hypothesis is articulated, albeit in the service of a semantic conception of indeterminacy. We note that McGee's hypothesis does not explain determinacy in semantic terms, and is actually neutral about the nature of determinacy.

<sup>35</sup>[6] defends this assumption. Cf. footnote 33.

**Conjecture 1** for every maximally good description  $d$  in  $\mathcal{L}$ , there is one,  $d^+$ , in  $\mathcal{L}^D$  that is exactly like  $d$  except that for every sentence  $\phi$  of  $\mathcal{L}$ :  $d^+$  (1) contains  $\mathcal{D}\phi$  iff every maximally good description in  $\mathcal{L}$  contains  $\phi$ ; and (2) contains  $\neg\mathcal{D}\phi$  otherwise.

A difficulty arises however, when we consider sentences in which the sentential complement of a determinacy operator contains occurrences of variables bound to quantifiers outside it, as in  $\exists x\mathcal{D}Fx$ . The conjecture just given tells us nothing useful, because  $Fx$  is not a sentence that occurs in any of the descriptions in either language.

Our approach to this difficulty is to suggest that we should think of the maximally good descriptions in a given language as *models* rather than mere sets of sentences.<sup>36</sup> The role models play is quite different from one traditional role they have been thought to play. Traditionally, one may think of the objects and predicate-interpretations in a model as being a reality that the language describes; model-theoretic semantics gives a neat picture of how that reality systematically relates to the language—how it induces truth-values for all of the sentences of the language. We instead are thinking of the objects and predicate-interpretations as themselves being elements of a very elaborate representation of reality.<sup>37</sup> As mere representations, it is only *structure* that matters—not the actual identities of the objects in the domain. Thus, two models that are isomorphic are, as we see it, saying exactly the same thing about reality.

So our maximally good descriptions in  $\mathcal{L}$  are actually models.<sup>38</sup> Thus

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<sup>36</sup>There are alternatives. Perhaps we could stipulate that every object has a name, and interpret open sentences in terms of substitution instances. The results could be much the same as our system.

<sup>37</sup>There is an important way in which a model for a (countable first-order) language  $\mathcal{L}$  may contain strictly more information than the set of sentences of  $\mathcal{L}$  that are true in the model. The Downward Löwenheim-Skolem Theorem tells us, for example, that for any model with uncountably many objects in its domain, there is another model that contains countably many objects in its domain, and is hence structurally quite different, but in which exactly the same sentences are true. Thus what a model “says” about reality may be more than what is “said” by the totality of the sentences of  $\mathcal{L}$  that are true in the model. For example, the model might “say” that there are uncountably many things (by having uncountably many objects in its domain) though there is no sentence in  $\mathcal{L}$  that “says” this.

<sup>38</sup>Instead of models, we could use sets of sentences true at conservative expansions of our models—models that merely augment  $\mathcal{L}$  with “special names”, names unique to

we can begin to consider how such sentences of  $\mathcal{L}^{\mathcal{D}}$  as  $\exists x\mathcal{D}Fx$  will relate to the descriptions in  $\mathcal{L}$ . In particular, we now have the materials to consider formulas with free variables in them. Thinking of our Conjecture 1, to say something analogous we would want something like this: if an open sentence  $\phi(x)$  with free variable  $x$  is satisfied, with respect to a variable assignment, in all descriptions, then  $\mathcal{D}Fx$ , with respect to that variable assignment is also satisfied in every one. But this assumes that one variable assignment works for every description—in effect, all the descriptions have the same domain. This assumption is clearly unwarranted, for it rules out, for example, that it be indeterminate how many things there are.

Our solution assumes that there is a counterpart relation connecting objects in the domains to one another. For simplicity, we may assume that the domains of the models are actually disjoint.<sup>39</sup> Thus we will be able to speak of the satisfaction and dissatisfaction of an open sentence like  $Fx$  by *counterparts* of an object, in the same or other models. This allows us to give the natural extension of our previous thought about how  $\mathcal{D}$  works: roughly,  $\mathcal{D}Fx$  is satisfied when  $x$  is assigned to object  $o$  if  $Fx$  is satisfied whenever  $x$  is assigned to to a counterpart of  $o$ .

But a difficulty arises when we consider open sentences with more than one free occurrence of a variable, for example  $x = x$ . Suppose  $x$  is assigned to object  $o$ . We want to consider whether  $x = x$  is satisfied when  $x$  is assigned to counterparts of  $o$ . There is a choice here. We can consider what happens when the *variable*  $x$  is assigned to a counterpart of  $o$ . We will then see that no matter what counterpart is chosen,  $x = x$  is satisfied. Instead, we can consider what happens when each *occurrence* of the variable  $x$  is as-

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the model, one for each object in the domain of the original model. We will proceed with models-as-world-descriptions; those who find our use of models heretical are invited to replace our models with the sets of sentences true in their “expansions” in the augmented language. The effect is to replace our models with something like Carnap’s state-descriptions, where we assume that in each state-description, every object has a special name, and no special name occurs in more than one state-description. Our counterpart relation (discussed below) will have to be between special names, across expanded models. The logical results are the same, and to us it makes no metaphysical difference.

<sup>39</sup>There is an equivalent, but more cumbersome, way to insure that the occurrence of the same object in two domains has no significance. We could count as “objects” not the objects in the domain, but ordered pairs of those objects and the models whose domains they are in, and have the counterpart relation relate these pairs.

signed to a counterpart of  $o$ , leaving it open that the two occurrences might be assigned to different counterparts. In this case, it is not guaranteed that each of the resulting assignments will satisfy  $x = x$ .

Given our perspective, the second choice is the one to make. Only it gives us a way of regarding the identity formula  $x = x$  on which it might not be *determinately* satisfied. A quick example will help make clear what is going on. If an object  $o$  has two counterparts  $c_1$  and  $c_2$  in the very same model, this indicates that it is, as it were, indeterminate which thing this object is (in this model). Both counterparts are equally good choices. So  $o$ 's identity is indeterminate, and this should manifest itself when we consider whether  $o = o$  in this model (so to speak). Indeed it does, we suggest: to consider whether  $o = o$  in this model, we need to consider whether  $c_1 = c_1$ , whether  $c_2 = c_2$ , whether  $c_1 = c_2$ , and whether  $c_2 = c_1$ . All four considerations are relevant, not just the first two! Thus we see that it is not, after all, determinate that  $o = o$ , because there is at least one reasonable description of the situation involving  $o$  on which it fails to be the case that  $o = o$ .

If an object-representation has, in each model, exactly one counterpart, then the identity of the represented object is determinate: each acceptable world-representation contains exactly one representation of it. But if two object-representations in one world are counterparts of one another, this indicates that (1) they can be regarded as representing different objects (since they are distinct object-representations in the same world-representation, or model) and (2) they can be regarded as representing the same object (since they are counterparts). This is the core of our way of thinking of indeterminacy of identity.

We will suppose that the counterpart relation is an equivalence relation—it is reflexive, symmetric, and transitive. This choice has important consequences, most of which seem desirable, but we will not give an explanation of the choice here, beyond saying that the choice seems one natural way of proceeding in the super-resolutional framework, since the counterpart relation is supposed to relate object-representations (things in the domain of a model) that are not determinately “of” distinct objects. It is easy to motivate reflexivity and symmetry: surely  $o$  can be regarded as representing the same object as  $o$ , and if it is permissible to regard  $o$  and  $n$  as representing the same object, then it should be permissible to regard  $n$  and  $o$  as representing the same object. Transitivity is contentious however: from the fact that  $o$  and  $n$  can reasonably be regarded as representing



the same object, and similarly for  $n$  and  $m$ , does it follow that  $o$  and  $m$  also can reasonably be regarded as representing the same object? A case can be made for thinking that it does not. Nevertheless, there is a good reason for imposing this constraint: since the objects represented by  $o$  and by  $m$  can each be regarded as being represented by  $n$ , they can be regarded as identical—hence  $o$  and  $m$  can be regarded as representing the same object.

### 3.4 A representative case: Indeterminacy of survival

Suppose that an unlucky cat wanders into the mad scientist's laboratory. The mad scientist performs an operation, and a cat comes out of the laboratory. Due to the disturbing nature of the operation, it will not be described in detail here; what is important about it is that it is indeterminate whether the cat that went in is identical with the cat that came out. Let us attempt to describe the situation with some names: we will attempt to use the name  $a$  for the cat that went in, and the name  $b$  for the cat that came out.

#### 3.4.1 Parsons' treatment of the case

On Parsons' view, we have two names that do not suffer indeterminacy of reference, but there is indeterminacy about the identities of the object(s) named by these names. We may say the following:

It is determinate that  $a = a$  and determinate that  $b = b$ .

It is indeterminate whether  $a = b$ .

(It is determinate that)  $a$  entered the lab.

It is indeterminate whether  $b$  entered the lab.

(It is determinate that)  $b$  exited the lab.

(It is determinate that)  $a$  is a cat and that  $b$  is a cat.

There is an object such that it is indeterminate whether it is identical with  $b$ .

There is no object that determinately survived the experiment.

There is an object such that it is indeterminate whether it survived the experiment.

There is no object such that is indeterminate whether it is a cat.

Note that it might appear that we can argue that  $a$  and  $b$  are distinct on the grounds that  $a$  determinately entered the lab and that  $b$  did not. But, on Parsons' view, this apparent difference between them is not a real difference, for, in effect, we have cited a merely apparent property that  $a$  has and  $b$  lacks—the property of having determinately entered the laboratory. The expression “is a thing that determinately entered the laboratory” does not express a genuine property of objects, and hence cannot be used to tell  $a$  and  $b$  apart.

### 3.4.2 Lewis' treatment of the case

On Lewis' view, we have two names that suffer indeterminacy of reference, while the predicate “is a cat” (and perhaps others) suffers a coordinated indeterminacy of meaning. The language of which they are a part can be made precise in two ways: on one, both names refer to the sum of cat-slices that temporally extends through the operation, while the predicate “is a cat” has this object (and not any of its proper temporal parts) in its extension; on the other, the two names refer to distinct, non-overlapping sums of cat-slices, one of which ends at the operation, and the other of which begins there, while the predicate “is a cat” has each of these two objects (and not the sum of them) in its extension. We may say the following:

It is determinate that  $a = a$  and determinate that  $b = b$ .

It is indeterminate whether  $a = b$ .

(It is determinate that)  $a$  entered the lab.

It is indeterminate whether  $b$  entered the lab.

(It is determinate that)  $b$  exited the lab.

(It is determinate that)  $a$  is a cat and that  $b$  is a cat.

There is no object such that it is indeterminate whether it is identical with  $b$ .

There is an object that determinately survived the experiment.

There is no object such that it is indeterminate whether it survived the experiment.

There is an object such that it is indeterminate whether it is a cat.

Lewis thus disagrees with Parsons on all and only the claims that begin with “There is”. This seems to fit with the fact that Parsons conceives

of indeterminacy as worldly, while Lewis conceives of it semantically. A key point is the validity of existential generalization in any context, which Parsons accepts<sup>40</sup> and Lewis, effectively, denies, because of the “opacity” of the indeterminacy operator.

### 3.4.3 Our treatment of the case

We imagine two resolutions of the situation,  $R_1$  and  $R_2$ . According to  $R_1$ , there are two cats,  $\alpha$  and  $\beta$ , where  $\alpha$  entered the lab but ceased to exist in it, and  $\beta$  began to exist in the lab and exited the lab. According to  $R_2$ , there is a single cat  $\gamma$ , that underwent a change, and thus survived the experiment. It entered and exited the lab. We must also consider what the counterpart relations among these objects are. It seems clear that the two cats that feature in  $R_1$  are counterparts of the one cat in  $R_2$ . We assume also that the two cats in  $R_1$  are counterparts of each other. (This follows from our supposition that the counterpart relation is an equivalence relation.) So  $\alpha$ ,  $\beta$ , and  $\gamma$  are all counterparts of one another.

Let us first consider what things will be true that do not involve the names  $a$  and  $b$ .

Many intuitive things are the same as on Parsons account. For example

There is no cat that determinately survived the experiment.

There is a cat such that it is indeterminate whether it survived the experiment.

There is no object such that is indeterminate whether it is a cat.

The first is true because in each resolution, each cat (strictly speaking: thing that satisfies ‘is a cat’) has a counterpart (namely  $\alpha$ ) that did not survive the experiment. Yet, in each resolution, there is some thing that is a cat and has two counterparts (namely  $\alpha$  and  $\gamma$ ) such that the one survived the experiment and the other did not: thus, the second is true. The third is true because every counterpart of a cat is a cat.

Here are some things that are true on our account, but not on Parsons’ (or on Lewis’):

For every cat  $x$ , it is indeterminate whether  $x = x$ .

There is no cat that determinately entered the lab.

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<sup>40</sup>[13] p. 22.

The first is true because in each resolution, each thing that is a cat has two counterparts, namely  $\alpha$  and  $\beta$ , that dissatisfy  $\lceil x = x \rceil$  when they are assigned, respectively, to the first and second occurrence of  $\lceil x \rceil$ . The second is true because in each resolution, each thing that is a cat has a counterpart, namely  $\beta$ , that does not satisfy  $\lceil x \text{ entered the laboratory} \rceil$ . This is so despite the fact that it is true that it is determinate that some cat entered the lab. This is because in each resolution, there is some item that is a cat and entered the lab.

This brings out a key feature of our approach. Existence statements can be true even though there can be no name which forms a true sentential “witness” of the existential.<sup>41</sup> This we see as an inevitable fact about indeterminacy.

What about the names  $a$  and  $b$ ? One natural suggestion is that in  $R_1$ ,  $a$  names  $\alpha$  and  $b$  names  $\beta$ , while in  $R_2$ , both names name  $\gamma$ . This suggestion makes the names behave like definite descriptions. In effect,  $a$  is like the definite description “the cat that entered the laboratory.” As such, it is not behaving as a “rigid designator”, the way an object-variable does. Since the determinacy operator is sensitive to the “meaning” side as well as the “reference” side of the descriptions, the logical behavior of the names will not be those of normal “extensional” names—e.g., generalized universal instantiation and existential generalization, and substitutivity will fail (as it does on Lewis’ account of the names  $a$  and  $b$ .) For example we will get that it is determinate that  $a = a$ , even though it is also true that there is no object  $x$  such that  $x = a$  and it is determinate that  $x = x$ . And the Evans argument, beginning with the true premise that it is indeterminate whether  $a = b$ , will be diagnosed as invalid for much the same reason that Lewis gives. (But note well that if the symbols  $a$  and  $b$  in the Evans argument are thought of as object-variables, rather than terms with descriptive meanings, then the argument is valid but unsound, for it is not a legitimate assumption that  $\mathcal{D}a = a$ .)

That suggestion would make true the same sentences as the ones discussed above as being true on Parsons’ account. However, on Parsons’ account, the following would be false, while it is true on our account:

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<sup>41</sup>Nevertheless, all classical reasoning is valid. In particular, all closed sentences that can be classically derived from an existential sentence (using existential instantiation along the way!) can indeed be validly inferred from the existential.

There is no cat  $x$  such that it is determinate that  $x = a$ .

This is true because in each resolution, each thing that there satisfies  $\lceil x = a \rceil$  as a value of  $x$  has a counterpart (namely  $\beta$ ) that does not satisfy  $\lceil x = a \rceil$  (as a value of  $x$ ). Note that nonetheless the sentences

There is a cat  $x$  such that  $x = a$ .

It is determinate that there is a cat  $x$  such that  $x = a$ .

are true.

## 4 A formal representation

A *super-model* is a set of classical models (“models” for short, or “precisifications” or “worlds”), together with a counterpart relation. The models’ domains must be entirely disjoint.<sup>42</sup>

The *counterpart relation* on the models is any equivalence relation whose relata are objects from the domains of the models. (The minimal counterpart relation for a set of classical models is the identity relation on the set of all “objects” from the set; everything is a counterpart of itself.)

We will give a definition of truth at a classical model for a first-order formal language that includes a determinacy operator  $\mathcal{D}$ . This is done in more or less the usual way for everything except the  $\mathcal{D}$  operator. In particular, we define a notion of *satisfaction* of a sentence relative to a variable assignment; truth, for a closed sentence, will be satisfaction by all assignments.

### Valuated sentences

We will make our definition in a somewhat unusual way, the point of which will become apparent when we come to the clause for  $\mathcal{D}$ . First,

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<sup>42</sup>In fact, we could allow domains to overlap if we thought of objects as pairs, each of which is a model and an object from its domain. But the presentation is simpler if we simply assume the domains to be disjoint.

we will need the notion of a *valuated sentence*.<sup>43</sup> In essence, a valuated sentence is the result of replacing the free variables in an ordinary formula with objects from the domain. Thus if  $F$  is a one-place predicate of the formal language, and  $o$  an object from the domain of discourse, then  $Fo$  (the result of “concatenating”  $F$  with  $o$ ) is a valuated sentence. To make this rigorous, we could consider valuated sentences to be sequences (strings) of symbols-or-objects, where we would ordinarily consider formal sentences to be sequences (strings) of symbols.

A variable assignment and an open sentence together determine a single valuated sentence. And valuated sentences are identical if they have the same objects and the same symbols at the same positions. Some examples should suffice to convey the idea: Consider an assignment  $\sigma_1$  that assigns the variable  $x$  to the object  $o_1$  and the variable  $y$  to the object  $o_2$ . The valuated sentence determined by this assignment together with the open sentence  $x = y$  can be thought of as the sequence  $(o_1, '=', o_2)$ .  $\sigma_1$  together with  $y = x$ , on the other hand, determines the valuated sentence  $(o_2, '=', o_1)$ .<sup>44</sup> Now consider  $\sigma_2$ , which assigns the variable  $x$  to the object  $o_2$  and the variable  $y$  to the object  $o_1$ .  $\sigma_2$  on  $x = y$  determines  $(o_2, '=', o_1)$ , the very same valuated sentence that  $\sigma_1$  on  $y = x$  determines. Consider also  $\sigma_3$ , which assigns  $x$  to  $o_1$  and  $y$  to  $o_1$ . Then  $\sigma_3$  on  $x = y$  determines  $(o_1, '=', o_1)$ , which is also determined by either  $\sigma_1$  or  $\sigma_3$  on  $x = x$  or by  $\sigma_2$  on  $y = y$ .

An explicit characterization of the grammar of valuated sentences can be achieved in more than one way. What valuated sentences there are is relative to a model, since only objects from the domain should be appropriate “values” for the variables. Here is a loose but thorough characterization: all closed (traditional) sentences are valuated sentences; and if  $\forall x\phi$  is a valuated sentence, then so is the result of substituting an object (from the domain) for every free occurrence of  $x$  in  $\phi$ . (And similarly for all variables, not just  $x$ , and for the existential quantifier.) The resulting valuated sentence is a “valuation of” it. Thus  $\forall x(\exists yRxy \rightarrow Rxx)$  is a valuated sentence, and if  $o_1$  and  $o_2$  are in the domain, then  $\exists yR_{o_1}y \rightarrow R_{o_1}o_1$  is a valuation of it, and  $R_{o_1}o_2 \rightarrow R_{o_1}o_1$  is a valuation of that.

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<sup>43</sup>The term and the notion are from David Kaplan’s “Opacity” [7].

<sup>44</sup>But see Appendix C of [7] for an important qualification on this way of representing valuated sentences.

We will define truth and falsity for an ordinary sentence at a classical model in a new way, equivalent to the usual way, by way of defining truth and falsity for valuated sentences at the model. Let  $M = (D, I)$  be a classical model with domain  $D$  and interpretation function  $I$ . For atomic valuated sentences, with form  $F(o_1, o_2, \dots, o_n)$  (where  $F$  is an  $n$ -place predicate and  $o_1 \dots o_n$  are members of  $D$ ), the sentence is true just in case  $(o_1, o_2, \dots, o_n) \in I(F)$  (otherwise false). The clauses for valuated sentences whose main connectives are the truth-functional sentential operators are as one would expect. The clause for valuated sentences of the form  $\forall x\phi$  is this: the valuated sentence is true just in case every valuation of it is true.  $\exists x\phi$  is true just in case some valuation of it is true. Since closed (ordinary) sentences are valuated sentences, we have now defined truth for all closed sentences.

So much for classical models and the first-order language without  $\mathcal{D}$ . We turn now to super-models and the language that includes  $\mathcal{D}$ . The valuated sentences for  $S$  will now involve objects from the various domains in the various models in  $S$ . But we only allow valuated sentences with objects from the same domain. That is, the valuations of a quantified sentence  $\forall x\phi$  that already has an object  $o$  occurring in it must have as their value for  $x$  elements of the domain of which  $o$  is a member. We define truth for valuated sentences at a model  $M$  in a super-model  $S$  exactly as for a classical model taken on its own, except that we now need a clause for  $\mathcal{D}$ .

To give this clause, we need the notion of a “counterpart” of a valuated sentence. A valuated sentence  $\psi$  will be said to be a “counterpart of” a valuated formula  $\phi$  (in the super-model  $S$ ) if  $\psi$  arises from  $\phi$  by replacing zero or more occurrences of objects in  $\phi$  with counterparts of those objects, in such a way that the objects in  $\psi$  are all in the domain of some single model  $M$  in  $S$ . Thus if  $o_2$  from the domain of model  $M_2$  is a counterpart of  $o_1$  from model  $M_1$ , then the counterparts of  $R_{o_1 o_1}$  will include  $R_{o_1 o_1}$  and  $R_{o_2 o_2}$ , but not  $R_{o_1 o_2}$ .<sup>45</sup> If  $o_1$  and  $o_2$  are counterparts from the very same

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<sup>45</sup>It is exactly here that the use of valuated sentences and counterparts of valuated sentences easily allows for mathematical wiggle-room that is not straightforwardly provided by the standard Tarskian sequences (variable assignments). A sequence is sure to treat  $R_{xx}$  as involving the same object twice. We could define a notion of a “counterpart to a sequence”:  $\sigma$  is a counterpart of  $\tau$  if there is some model  $M$  such that for every variable  $x$ ,  $\sigma(x)$  is in the domain of  $M$  and is a counterpart of  $\tau(x)$ . But then  $x = x$  is satisfied by

model, then counterparts of  $R_{o_1 o_1}$  will include  $R_{o_1 o_2}$  and  $R_{o_2 o_1}$ . Notice that with counterparts  $o_1$  and  $o_2$  from the same model, the false valuated sentence  $o_1 = o_2$  will be a counterpart of the true valuated sentence  $o_1 = o_1$ .

The clause for the  $\mathcal{D}$  operator can now be given. A valuated formula of the form  $\mathcal{D}\phi$  is true (at a model  $M$  in a super-model  $S$ ) just in case for every model  $N$  in  $S$ , every counterpart of  $\phi$  whose objects are in the domain of  $N$  is true at  $N$ . Since a closed ordinary sentence is a valuated sentence with no objects in it, this clause implies that for closed ordinary sentences  $\phi$ ,  $\mathcal{D}\phi$  is true at one model in  $S$  just in case  $\phi$  is true at all models in  $S$ . When  $\phi$  contains objects, however, the truth of  $\mathcal{D}\phi$  at one model turns on the truth of appropriate counterparts of  $\phi$  at other models.

### Some interesting validities of the resulting system

*All validities of classical logic that are closed sentences are valid.*

*If  $\phi$  is valid, then so is  $\mathcal{D}\phi$ .*

*If  $\phi$  is an open sentence whose universal closure is valid, the universal closure of  $\neg\mathcal{D}\neg\phi$  is valid. For example,*

*$\forall x\neg\mathcal{D}\neg(\mathbf{F}x \vee \neg\mathbf{F}x)$  is valid.*

*$\forall x\forall y(\nabla x = y \rightarrow \nabla x = x)$*

*$\forall x\forall y((\phi(x) \wedge \neg\phi(y)) \rightarrow x \neq y)$*

*$\neg\mathcal{D}\exists x\exists y(\nabla x = y \wedge (\phi(x) \wedge \neg\phi(y)))$*

### Some interesting non-validities

*If  $\phi$  is an open sentence whose universal closure is valid, it does not follow that the universal closure of  $\mathcal{D}\phi$  is valid. For example,*

*$\forall x\mathcal{D}(\mathbf{F}x \vee \neg\mathbf{F}x)$  is not valid.*

*$\exists x\exists y \nabla x = y \rightarrow \exists x\exists y x \neq y$  (The quantified and conditionalized version of the Evans argument is invalid!)*

*$\forall x\forall y(\nabla x = y \rightarrow \neg(\phi(x) \wedge \neg\phi(y)))$  (Though instances of this scheme are not validities, no negation of an instance can be super-true! See the last item in the list of validities.)*

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every sequence, and every counterpart of it; to define the determinacy operator with the effect we get requires something closer to our method.



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