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Discussion

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Replies

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First, let me thank the contributors to this issue for engaging with my thoughts, and the interesting ideas they have put forward. Unsurprisingly, I do not agree with some of their central claims – but if philosophers were ever to agree on all things, we would all be out of a job! I will take the contributions one by one – the order is purely alphabetical – and give my thoughts on them.

1. Odrowąż-Sypniewska

Joanna Odrowąż-Sypniewska (2013) discusses the matter of "faultless disagreement" in the context of statements about borderline cases of vague predicates. She argues that my account of vagueness can accommodate the existence of such disagreement, but offers a contextualist account, which will be preferable to the 'enemies of contradiction' (p. 27).

To illustrate the matter at hand, take a sorites progression between things that are red and things that are blue. In a borderline case, *a*, one speaker, *A*, may assert that *a* is red (p), and another, *B*, may assert that it is not ($\neg p$). They disagree, but the disagreement is, supposedly, faultless. Odrowąż-Sypniewska quotes Kölbel's definition of faultless disagreement as follows (p. 22):

a. A believes (judges) that p and *B* believes (judges) that not-p
b. Neither *A* nor *B* has made a mistake (is at fault).

According to my account of vagueness, in the borderline case, '*a* is red' and '*a* is not red' are both true. So, says Odrowąż-Sypniewska, the account can explain faultless disagreement: both *A* and *B* are right.

Though she does not seem to take its to be a serious problem, Odrowąż-Sypniewska worries that this account delivers the result that a dialetheist who says both p and $\neg p$ is in a state of faultless disagreement with themself,

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which is 'hard to swallow' (p. 27). I think that such a conclusion is not just hard to swallow. It is quite unacceptable to suppose that the dialetheist is in disagreement – faultless or otherwise – with themself.

The problem is that Kölbel's definition builds in the Principle of Non-Contradiction. Utterances of p and $\neg p$ need not signal a disagreement. What signals a disagreement is when one speaker accepts (asserts) p and the other rejects (denies) p – and rejecting something is not the same as accepting its negation.[1] If there is such a thing as faultless disagreement, then, clause (a) of the definition has to be replaced accordingly.

And now, if, in our dialetheic borderline case, A asserts p and B denies p, B has made a mistake. The account does not accommodate faultless disagreement, so construed. As is clear, the very possibility of such disagreement depends on there being no fact of the matter in the borderline case – or one party *has* made a mistake. Thus, if one is a consistent epistemicist, p is either true or false, though we may not know which. Someone who states the opposite gets it wrong. Similarly, if p is both true and false, someone who denies p (or $\neg p$) gets it wrong. So whether there is faultless disagreement is a theory-dependent question, and its treatment cannot be invoked as a theory-neutral criterion for evaluating accounts of vagueness.

Having said that, the notion of making a mistake can be understood in many ways. In particular, it may be understood not only in an alethic fashion, but in an epistemic fashion. Sometimes, the evidence can point mercilessly in one direction; sometimes it can point mercilessly in the other. But, sometimes, which way it points can be a matter of judgement, and legitimate disagreement is possible. Thus, there is evidence that there is intelligent life in other places in our galaxy: that it should have evolved only on earth is most improbable. There is evidence that there is not: if it did evolve elsewhere, given the age of the galaxy, some of this must have happened a long time ago; and in this case we would expect to find evidence in the form of communication, which we do not. Scientists may legitimately disagree about which of these pieces of evidence is more significant.

In exactly the same way, one might suppose, given a borderline case, the evidence – perceptual or whatever – can be of this kind. Thus, an epistemicist may hold that there is a fact of the matter about the alethic status of p, but that given our evidence, it is reasonable to go either way on the matter. Similarly, the dialetheist. Whether there is any phenomenon of

[1] Priest, (2006), 6.2.

faultless disagreement generated by vagueness is not at all clear to me; but if there is, I suspect that it is of this kind.

Let us now turn to Odrowąż-Sypniewska's own account. She calls this contextual; but, in truth, context seems to have little to do with the analysis of faultless disagreement as such. (It is deployed to account for the borderline between where faultless disagreement operates, and where it does not.) According to her, if, in a borderline case, someone asserts that *a* is red, this means '*a* is red-to-me'; similarly, if someone asserts in such a case that *a* is not red, what this means '*a* is not red-to-me'. They can both be right. Hence the faultlessness – but at the cost of there being no disagreement, and so no faultless disagreement, at all! Odrowąż-Sypniewska is aware of this. Her reply is that there can still be a *feeling* of disagreement since, where speakers have similar standards (of redness, etc.), such utterances would betoken a disagreement. Perhaps; but there are simpler ways of analysing the situation than Odrowąż-Sypniewska two-stage strategy. Come back to our two scientists. One judges that there is life elsewhere in the galaxy; one that there is not. Clearly, they do have different standards of evidence, in some sense, and this is part of the explanation of what is going on. But, first, we do not have to *reinterpret* the claims made to show why there is no fault. And, secondly, there is still an actual, not merely *prima facie,* disagreement. This strikes me as doing better justice to the phenomenon.

Let me finish with a couple of very general points. The debate about faultless disagreement in borderline cases of vague predicates is part of a much bigger issue about how to handle vagueness in general, including sorites paradoxes. Even if Odrowąż-Sypniewska's account of faultless disagreement were preferable to a dialetheic account, this hardly settles the matter of how to handle vagueness. One must look at all aspects of the phenomenon and see which is overall best. It seems to me that the dialetheic account is both simpler and more natural than any of its rivals – contextualist or otherwise. But that is obviously too big an issue to take on here.

Of course, the dialetheic account does require one to reject the Principle of Non-Contradiction. Odrowąż-Sypniewska's says that her account will be more acceptable to the 'enemies of contradiction.' Indeed so, but that is a *sociological* fact. In the same way, discriminating against women on the ground of their gender will be more acceptable to 'enemies of gender equality,' than not discriminating. If the point is to carry any *rational* weight, we are owed an *argument* as to why no contradiction is acceptable. It is all too easy to accept received attitudes – about gender inequality or dialetheism – without the critical reflection due. To make matters clear: I'm

not at all suggesting that Odrowąż-Sypniewska is guilty of this. The point is simply about how this kind of remark can pander to thoughtless attitudes.

2. Pietryga

Anna Pietryga's essay (2013), centres around two questions, which seem to be run together:[2]

1. Does Tarski take natural languages to be semantically closed?
2. Are natural languages semantically closed?

The first is a scholarly question. The second is a substantial philosophical question. In my work, the answers to both questions are 'yes.' Hers are 'no.' I will take up 1 first, and turn to 2 afterwards.

Since this is a matter of scholarship, let us look at texts. In discussing Tarski in *In Contradiction*, here is what I say:[3]

> Tarski ... located the root of the semantic paradoxes in semantic closure, and more specifically in a set of closure conditions. Tarski's point may be shown as follows. I will say that a formal theory satisfies the Tarski conditions if it is such that...

Three conditions are then cited: that every sentence has a name; that the Satisfaction Schema is a theorem; and that the underlying logic of the theory satisfies the inference $A \leftrightarrow \neg A \vdash A \wedge \neg A$. It then goes on to derive the Heterological Paradox. It continues:

> So much for formal theories. Let us now turn to natural languages. Tarski claimed, and I shall agree with him, that a natural language, such as English, satisfies these closure conditions.

It then goes on to note that in talking of a natural language, notions that apply to a theory make no sense, and have to be replaced. Theoremhood, especially, is replaced by truth.

Here's what the text referred to by way of justification says. Drawing together the threads of a section entitled 'The Concept of True Sentence in Everyday or Colloquial Language,' Tarski says (my italics):[4]

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[2] There appear to me to be some other confusions in the paper, but unless they relate to these two questions, I will pass over them here.

[3] Priest, (1987), 1.2. The reference given is to the German translation of Tarski (1933).

[4] I quote from the English translation, pp. 164-5. I am not competent to judge the accuracy of the Woodger translations. However, when it was published, Tarski

> A characteristic feature of colloquial language (in contrast to various scientific languages) is its universality. It would not be in harmony with the spirit of the language if in some other language a word occurred which could not be translated into it; it could be claimed that 'if we can speak meaningfully about anything at all, we can speak about it in the colloquial language.' If we are to maintain this universality of everyday language in connection with semantical investigations, we must, to be consistent, admit into the language, in addition to its sentences and other expressions, also the names of these sentences and expressions, and sentences containing these names, as well as such semantic expressions as 'true sentence', 'name', 'denote', etc. *But it is presumably just this universality of everyday language which is primarily the source of all semantic antinomies*, like the antinomy of the liar or of heterological words. These antinomies seem to provide a proof that every language which is universal in the above sense, and for which the normal laws of logic hold, must be inconsistent. This applies especially to the formulation of the antinomy of the liar which I have given ... [above]. If we analyse this antinomy in the above formulation we reach the conviction that no consistent language can exist for which the usual laws of logic hold and which at the same time satisfies the following conditions: (I) for any sentence which occurs in the language a definite name of this sentence also belongs to the language; (II) every ... [instance of the *T*-schema] is to be regarded as a true sentence of the language; (III) in the language in question an empirically established premise having the same meaning as ... [*c* is '*c* is not true'] can be formulated and accepted as a true sentence. [There is then a footnote pointing out that one can dispense with (III) if we use satisfaction instead of truth.]

The passage does not use the term 'semantic closure.' I agree with Pietryga that this is not used in the 1933 paper. It is coined only later in Tarski (1944), as follows:

> We have implicitly assumed that the language in which the antinomy is constructed contains, in addition to its expressions, also the names of these expressions, as well as semantic terms such as the term "true" referring to sentences of this language; we have also assumed that all sentences which determine the adequate usage of this term can be asserted in the language. A language with these properties will be called "*semantically closed*".

I think that these two passages together amply demonstrate that Tarski took the root of the Liar paradox and its kind to be semantic closure, as I claimed. Note that there is no suggestion that the notion applies only to formal languages. Indeed, it is clear that a natural language such as English

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had been living in the US for 17 years, so I presume that by that time he was a fair judge of the translations and did not object.

does appear to satisfy the requisite conditions. It has names for its sentences; it contains the predicate 'is true'; and the instances of the *T*-schema do appear to be true – or the Liar would not be a paradox. Indeed, Tarski calls the schema a 'condition of adequacy' on an account of truth.[5] Calling it so would appear to imply that if the schema is not satisfied, we are not dealing with *truth*.[6]

Pietryga suggests that Tarski required two other clauses for semantic closure (p. 40):

- [one must] know the complete lexicon of the language in question
- [there must be] purely formal syntactic rules delimiting the set of sentences of this language.

She references Tarski (1936). Here's what the passage says:[7]

> In the solution of this problem [cf. laying the foundations of a scientific semantics] we can distinguish several steps. We must begin with the description of the language whose semantics we wish to construct. In particular we must enumerate the primitive terms of the language and give the rules of definition by which new terms distinct from the primitive ones can be introduced into the language. Next we must distinguish those expressions of the language which are called sentences, separate the axioms from the totality of sentences, and finally formulate the rules of inference by means of which theorems can be derived from these axioms. The description of the language is exact and clear only if it is purely structural, that is to say, if we employ in it only concepts which relate to the form and arrangement of the signs and compound expressions of the language. Not every language can be described in this purely structural manner. The languages for which such descriptions can be given are called *formalized languages*. Now, since the degree of exactitude of all further investigations depends on the clarity and precision of this description, *it is only the semantics of formalised languages which can be constructed by exact methods*.

As is clear, Pietryga's extra conditions have nothing whatsoever to do with semantic closure: they are required for the possibility of a scientific semantics (by which, I take it, Tarski means formulating a definition of truth).

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[5] E.g, Tarski, (1944), Section 4.

[6] On p. 49, Pietryga says that 'Tarski would not apply the *T*-schema to any natural language at all.' She cites a passage from Tarski (1933), but this says no such thing. What it says (my italics) is that the: '*attempt to set up a structural definition of the term 'true sentence'*—applicable to colloquial language is confronted with insuperable difficulties.'

[7] Woodger (1956, pp. 402-3). Italics original.

So much for the question of whether Tarski took semantic closure to be the root of the Liar and similar paradoxes. Turning now to the question of whether Tarski took natural languages to be semantically closed, let us return to the quote from Tarski (1933). This continues:

> If these observations are correct, then the very possibility of the consistent use of the expression 'true sentence' which is in harmony with the laws of logic and the spirit of everyday language seems to be very questionable, and consequently the same doubt attaches to the possibility of a correct definition of this expression.

The next section, entitled 'Formalized Languages, Especially the Language of the Calculus of Classes' then begins:

> For the reasons given in the preceding section I now abandon the attempt to solve our problem [cf. framing a correct definition of truth] for the language of everyday life and restrict myself henceforth entirely to *formalized languages*.

True, Tarski's explicit conclusion about the semantic closure of natural language is more guarded than the view I attribute to him – what he says is that the possibility of a consistent account of truth for natural language is 'very questionable'; but Tarski gives no ground as to why one might doubt that the Liar reasoning can be carried out in natural language, and, as he says, the antinomy moved him to give up trying to give a truth definition for natural language. So I think that what I said about Tarski's view in this regard is warranted.

Tarski (1944) is a non-technical summary and discussion of his 1933 paper. In this, we do find something of a softening of his attitude to the claim that natural language is inconsistent. He says (p. 349):

> The problem arises as to the position of everyday language with regard to ... [semantic closure]. At first blush it would seem that this language satisfies both assumptions (I) and (II), and that therefore it must be inconsistent. But actually, the case is not so simple. Our everyday language is certainly not one with an exactly specified structure. We do not know precisely which expressions are sentences, and we know even to a smaller degree which sentences are to be taken as assertible. Thus, the problem of consistency has no exact meaning with respect to this language. We may at best only risk the guess that a language whose structure has been exactly specified and which resembles everyday language as closely as possible would be inconsistent.

Pietryga's statement (p. 47) that in this passage 'Tarski state[s] *expressis verbis* [that] natural languages are not semantically closed' seems a very inaccurate paraphrase. What he claims is that the question has 'no exact meaning' – but that one may guess that a precisification of the relevant parts

of natural language would deliver a positive answer. And Tarski's guess would seem well justified. Let us suppose that we may take *ZF* set theory as capturing at least the mathematical part of English. We know that adding an unrestricted *T*-schema to its axioms gives inconsistency.

I note that the point of my own discussion in *In Contradiction* was not to give a scholarly account of Tarski's views on natural language, which play no further role in the discussion. It was to isolate the conditions Tarski gives as generating the Liar and Heterological paradoxes, which structure the subsequent analysis. Still, scholarly matters might have been better served had I noted that Tarski's views are not entirely uniform over the period in question. Pietryga's piece is a scholarly one. So I think that she *should* have noted it.[8]

Let us turn, finally, to the question of whether natural languages are semantically closed. This can now be dealt with quite quickly. It certainly *appears* to be so, as I have already noted. To the extent that Pietryga's argument that it is not so is not just an appeal to the claim that Tarski did not think so, which would not be a very good argument – the topic is not an appropriate one for an appeal to authority – it is to the effect that the notion does not apply to natural languages *by definition*. This, as we have seen, is not true.

That natural language *is* semantically closed is argued at length in Chapter 1 of *In Contradiction*.[9] Since Pietryga does not comment on these arguments, there is nothing further to be said here.

3. Schetz and Szymańska

Adriana Schetz and Katarzyna Szymańska (2013) discusses my claim that one can perceive contradictions; that is, have perceptual experiences whose contents are contradictory. As far as I understand them, they take me, in this, to be providing an argument for dialetheism. One has contradictory perceptions and infers that the world perceived is contradictory. The move, they note, requires direct realism: what one sees is actually there. Representational realism will not do: the fact that the world is represented in a certain way, or appears in that way, does not imply that it *is* that way.

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[8] In her discussion, Pietryga also cites a later semi-popular paper, Tarski (1969). Given the short time I have had to comment on Pietryga's paper, I have not been able to put my hands on a copy of this. So to what extent it contains a further shift in Tarski's attitude, I am not able to judge here.

[9] And at greater length in Priest (1984), on which much of this chapter is based.

If my aim were to show in the discussion at issue that the world is contradictory, I think their argument would be right. But it was not. One can have perceptual experiences whose contents are contradictory: the visual illusions they describe. But it is no part of my view that these experiences are veridical; indeed, I hold that they are not. That is why they are illusions. The point of the discussion was merely to show that it is *possible* to perceive contradictions; so if an actual contradictory observable situation *were* to obtain, there would be no bar to our seeing it. This was a step in an argument for the *consistency* of the observable world.[10]

This leaves the question of whether I take perception to be direct or representational. The answer is a nuanced one. When I see an object, there is a direct relationship between myself and the object. If I see the Empire State Building, *e*, the pair ⟨Priest, *e*⟩ is in the extension of the predicate *sees*. But equally, if I hallucinate a dagger, *d*, the pair ⟨Priest, *d*⟩ is in the extension of the predicate *sees*. The object of the perception may or may not exist: the direct relation is there, none the less.[11] (It should be evident that I am not using the verb *see* (*something*) in a veridical sense. The veridical sense can simply be defined as seeing an existent object.)

None the less, when I perceive an object, something must be going on between my ears, or in my mind, and the object *itself* – existent or nonexistent – is obviously not there: the perceptual state (brain or mental) is. And in *some* sense or other, this obviously *does* represent the object – though whether this is a sense of representation of the kind suggested by traditional representational realists[12] is another matter entirely – as is the question of what causal processes are involved within the brain, or between the brain and any external stimulus, in this situation. The matter may safely be left to cognitive scientists.[13]

4. Sendłak

Maciej Sendłak (2013) takes issue with my account of the Characterisation Principle (CP) and offers one of his own. The bald CP is that for any condition *P*, the/a thing, *p*, which is *P* is, indeed, *P*: *p* is *P*. The bald form cannot be accepted by anyone on pain of proving the existence of anything

[10] See Priest, (2006), ch. 3, esp. 3.5

[11] See Priest (2005), esp. ch. 3.

[12] See Bonjour (2007).

[13] Further on the notion of intentionality and representation, see Priest (2014), ch. 10.

– indeed, proving anything. On my account, *p* is *P*, but not necessarily at the actual world; *p* is *P* at the world or worlds the agent using the description envisages in using it. (The CP is embedded in an account of agents and their intentional actions.) I note *en passant* that Sendłak describes my view (p. 70) as follows: every object exists, but only a few of them exist at the actual world. This is not correct. I take it that *some* objects exist, and *some* do not. If an object does not exist (like Sherlock Holmes), it may exist at other worlds (such as those that realise Conan Doyle's stories). But there is no reason why an object must exist, even at worlds where it satisfies its characterisation. Thus, if *P* is 'purely fictional object', '*p* is *P*' is true (at this world), even though *p* does not exist.[14]

Anyway, Sendłak objects to my account of the CP: take *P* to be 'actual *Q*'. Then *p* is an actual *Q*, which may well be false, since there are no actual *Q*s. This just does not follow. What follows is that, *in some worlds*, *p* is an actual *Q*. 'In some world *A*' does not entail *A*. (The fact that I am a woman at some worlds does not entail that I am a woman.)[15] Sendłak seems to suppose that this means that I take 'actual' to have a different meaning at impossible worlds, or when applied to impossible objects. However, the word changes its meaning in such circumstances no more that does 'woman' – or, for that matter, 'exists'.[16] Words mean the same whatever world or object they are describing. It is simply the case that for *no* predicate, *P*, can one go from: '*Pa* is true at world *w*', where *w* is an arbitrary world, to '*Pa* is true at @' (i.e., at the actual world). Neither is this *ad hoc*. Such a move is ruled out quite independently by the theory of intentionality.[17] One can imagine anything one likes. In particular, if *Pa* is anything meaningful, one can imagine that *Pa*. There are worlds, then (namely those that realise the way things are imagined to be), at which *Pa* holds. Clearly, one cannot move from the contents of a state imagined to a state actual.

Sendłak has another objection to my treatment of the CP later in the paper. Consider the property 'is Modal Meinongianism (MM) and is false' ('MM' is his name for my view). Given my account of the CP there must be

[14] A similar mistake is made by Warzoszczak (p. 86-87) in his remarks on my account of the CP.

[15] The objection was, in fact, made by Beall (2006) and I replied to it in Priest (2011).

[16] So I do not treat 'actual' and 'exists' differently, contra Sendłak's remark on p. 70

[17] As Priest, (2011) notes.

a world, *w*, and an object, *a*, such that it is true at *w* that *a* is MM and *a* is false. Now:[18]

> *a* has to be an element of either possible or impossible worlds. It is reasonable to believe that if a given metaphysical theory of modality is true then ... it is necessarily true. Because of that if MM is true, then *a* cannot be a element of a possible world—it has to be an element of an impossible world. None the less, if the description is satisfied, then it is also true that there are no impossible worlds (Sendłak, 2014, p. 75).

The theory, then, is self-refuting, since it entails that there is no such object. Now, there is an initial confusion here. *a* is a member of the domain of *every* world. What is at issue, in fact, is the status of world *w*. If MM is a necessary truth, *w* is indeed an impossible world. I really do not understand why the last sentence of the quote is supposed to follow. Even if the falsity of MM entailed that all words were possible (which it does not). It would still be the case only that this is true at some (impossible) world, not that it is actually true.

Turning to Sendłak's own account of the CP, he distinguishes between object language and metalanguage. The object language contains our ordinary "pre-theoretical" vocabulary; the metalanguage contains our metaphysical discourse. One may then formulate the CP as follows (p. 76):

> Every non-empty set of properties, which are expressed in terms of non-theoretical (object) language, corresponds to an object which posses those properties.

This strikes me as unsatisfactory for several reasons. First, it would appear to be a version of the strategy which distinguishes between nuclear and non-nuclear predicates, and restricts the CP to conditions containing nuclear predicates. Sendłak himself considers this strategy earlier in the paper, and rejects it. His version would seem to be no improvement. The problem is to find a principled way of drawing the distinction between the two vocabularies. The new understanding does little to help. Why are status-terms such as *possible* and *impossible* ruled out from the object language? We use these all the time in our non-philosophical vernacular. Worse: *existence* has to be kicked upstairs into the metalanguage too. And as the paper notes (p. 75) a statement using *exists* 'might easily be accepted as a formulation of our everyday language.' Indeed so. The parent who tells their five year old that Father Christmas does not exist is no high-powered

[18] I have adjusted the notion to bring it into line with that used here.

theoretician. Sendłak's comment on this is puzzling. He notes that the status of the existence predicate is a problem for the bald CP, but fails to address the question of why it is not a problem for him.

Next, the CP as formulated is certainly false. Let us characterize an object, *s*, as a detective of acute powers of observation and inference, living in 221b Baker St, using cocaine, etc. It is just not true, and never has been, that such a detective lives at 221b Baker St. This is true in Doyle's stories, or the worlds that realise them; but anyone who took it to be literally true would be seriously confused.[19] Indeed, the matter is worse than that. One can prove anything in the "object language" vocabulary with this version of the CP. Let *B* be any such sentence. Let *P* be 'is red and *B*.' This version of the CP gives us: *p* is red and *B* – and hence *B*.

Finally, the home of the theory of non-existent objects is not fictional truth (though that is subsumed by it); it is the theory of intentionality. And just as we can imagine anything meaningful, we can think of anything if it is characterised in any meaningful way at all. Thus, even if an object is characterised by "theoretical" vocabulary, we can think about it, and it must have its characterising properties in *some* sense, or what are we thinking about? Sendłak notes the problem. He says (p. 75):

> Naturally, we can [think about such objects], but it seems that it is better not to treat them as objects of metaphysical theories, but rather as objects of metametaphysics or metaphilosophy.

I must confess that the relevance of this comment to the objection eludes me.

5. Warzoszczak

Piotr Warzoszczak (2013) discusses the account of worlds deployed in the services of the semantics of intentionality in *Towards Non-Being*, and finds a problem concerning trans-world identity. In particular, there is a tension, he claims, between the account of identity and what I say about there being indiscernible objects.

Objects can certainly be in the domain of more than one world. In a world where, for example, I am a woman, it is *I* who am a woman. (Indeed, in *Towards Non-Being*, all the worlds have exactly the same domain.) So

[19] See, further, Berto and Priest (forthcoming).

what is the criterion of identity? The answer is that *a* and *b* are identical (i.e., $@\models^{+} a = b$) iff:[20]

> [C] for every closed world, *w*, and every atomic property, *P*, *a* has *P* at *w* iff *b* has *P* at *w*

Now, take distinct objects which are indiscernible with respect to their properties at this world. It does not follow that they are identical. For they could differ in their properties at other worlds. Warzoszczak notes that for any world, w_a, where *a* has some property, there will be a world, w_b, where *b* does. Quite so. This does not threaten [C] in any way.

He also notes that [C] makes use of the notion of worlds, and so presupposes an identity criterion for them. What is it? The same, of course! (I should use a *different* criterion?) Worlds can be in the domain of worlds, and w_1 and w_2 are identical iff:

> [C_w] for all closed *w* and all atomic *P*, w_1 has *P* at *w* iff w_2 has *P* at *w*

Warzoszczak holds that such a criterion is trivial and unsatisfactory: a nontrivial criterion is needed. Unfortunately, he never explains what he means by 'trivial'. The first occurrence of the term in the paper is a reference to a passage by me, where I say that if the *P* in [C] may be *being identical with* something, then [C] is trivial. It is trivial in the sense that it is then simply a logical truth. Whatever else it is, C_w should not be that; but given that identity is ruled out for use in the property *P*, it is not.

It is true that, in giving the identity criterion for worlds, C_w deploys the notion of worlds, and so presupposes their identity conditions. That is circular, it is true. But circularity inhabits some of our most fundamental concepts. In standard set theory, two sets are identical iff they have the same sets as members. The definition requires us to quantify over the domain of sets (even in the case of the empty set, which is a set with *no set as a member*). It therefore presupposes the criterion of individuation for sets.

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[20] Priest (2005), 4.4. A closed world is a world that is closed under entailment. Warzoszczak also claims to find a different criterion in the book, where the worlds in question are not the closed worlds, but the possible worlds. I take it that this is just a mistake. In the page reference he cites it is clearly closed worlds that are referred to.

This is certainly circular, but not viciously so. It just marks the fact that we have hit bedrock-interconnections. So it is with worlds and identity.

In an attempt to explain why a non-trivial criterion of world-identity is necessary (and so, presumably, cast some light on what 'trivial' means), Warzoszczak asks us to consider a problem posed by Chandler. Consider a sequence of objects a_1, a_2, ..., a_n, and worlds w_1, w_2, ..., w_n, such that for each $1 \leq i < n$, a_i in w_i differs by only one property from a_{i+1} in w_{i+1}; but a_1 in w_1 has no properties in common with a_n in w_n. Assume a certain criterion of identity, namely one that entails that an object can maintain its identity under the change of one property, but not of all; then it follows that for $1 \leq i < n$, $a_i = a_{i+1}$, but $a_1 \neq a_n$. So the transitivity of identicals fails. Chandler suggests that it follows that the modal accessibility relation is non-transitive; so the correct modal logic is not *S*4 (or stronger). Thus, says Warzoszczak, a substantive criterion of identity is necessary to determine the correct modal logic. This by no means follows, however. The failure of the transitivity of identity is quite compatible with a transitive accessibility relation for metaphysical necessity. And given that such necessity is the most general form of necessity, one should expect every possible world to access every possible world. In which case, the accessibility relation is transitive.

To see that the failure of the transitivity of identity is compatible with the transitivity of the accessibility relation, note that the piecemeal-change phenomenon is really just a modal version of the problem of the Ship of Theseus; simply replace worlds with times. Its solution, then, will follow from a general solution to the sorites paradox. And there are certainly suggested solutions to the paradox in which the transitivity of identity fails, and which have nothing whatsoever to do with the temporal ordering (which may simply be a linear ordering).

A paraconsistent solution is one such.[21] In a standard sorites argument the major premises are naturally thought of as material equivalences, $A \equiv B$, and given that some of the statements in the sorites progression are dialetheias, this is not transitive, breaking the argument. With an identity-sorites of the kind of the Ship of Theseus, we can think of identity, $x = y$, as defined *à la* Leibniz, as $\forall P(Px \equiv Py)$. And = inherits its non-transitivity from $\equiv$. Nothing to do with the temporal accessibility relation here.

Now, the Leibniz condition is obviously not quite the same as its generalisation given by [C]. However, the point remains: we may interpret the 'iff' of [C] as a material biconditional, and transitivity will then fail. *Towards Non-Being* is a book on existence and intentionality; vagueness

[21] See Priest (2010).

was not on the agenda there. Once it is, and assuming a dialetheic solution to the sorites paradoxes, identity is non-transitive.[22]

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[22] I note that in Priest (2014), the Leibniz definition itself is deployed. This gives a different solution to Warzoszczak's original problem concerning things indiscernible at a world – and, moreover, gives a solution to the failure of substitutivity of identicals across worlds which is simpler than that in *Towards Non-Being*. See esp. chs. 2 and 5.

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