End of inclosure

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Abstract

This paper briefly defends theses in (Beall, 2013b) against objections advanced in (Weber et al., 2013). The second part of the paper both defends and fortifies an objection to the 'inclosure' argument for glut theory, spelling an end to the inclosure strategy (or at least its application to the sorites).

I am a glut theorist. I think that some truths are false. I join other glut theorists (e.g., Asenjo, Colyvan, Hyde, Mortensen, Priest, Routley, Tamburino, Weber, and more) in thinking that the liar paradox and other paradoxes of self-reference are the best cases for gluts.[1]

Recently, some glut theorists (Colyvan, 2009; Priest, 2010; Weber, 2010) have argued that we should extend glut theory beyond the familiar paradoxes to the sorites: the sorites paradox should be treated along glutty lines – a glutty penumbral region.[2] Why? The principal and explicitly given argument for extending glut theory to the sorties turns on Priest's 'inclosure strategy' – the inclosure argument, as I'll say – about which I say (much) more below.

In my (Beall, 2013b), I give a quick objection to the inclosure argument, thereby taking away what has been advanced by target glut theorists as the main argument for treating the sorites along glutty lines. In turn, I focus

[1] Actually, Graham Priest (conversation, Glasgow 'In Contradiction' conference, 2012), one of glut theory's chief advocates, has long thought that the best case for gluts comes from legal cases of inconsistent laws (see Priest, 2006). I have never seen the force of such cases, as they seem to be more a matter of 'according to' operators (e.g., *according to such-n-so legal authority*) which do not generally 'release' – that is, sentence A doesn't generally follow from *according to such-n-so legal authority, A is true.* But I set this topic aside for future debate (see Beall, 2009), mentioning it here only for historical background.

[2] Ripley (2011) was the first to officially advance an LP-based glutty approach to vagueness, though he himself, in subsequent work (Cobreros et al., 2010), endorses a different (and non-glutty) approach.

on spelling out another argument, and evaluating its merits as an argument against a classical approach in favor of the target glutty approach. Finding the latter argument wanting, I concluded that the glutty theorists were without a strong argument for treating the sorites along glutty lines.

My objections have met replies by a quintet of glut theorists and glut-theory sympathizers: Zach Weber, David Ripley, Graham Priest, Dominic Hyde, and Mark Colyvan – WRPHC, for short. Their paper (Weber et al., 2013), with which I assume the reader to be familiar, divides into two parts: one part arguing that, contrary to (Beall, 2013b), a classical-logic-based theorist cannot enjoy all of the benefits of their LP-based (glutty) approach; the other part, which is more foundational and of broader interest (beyond the sorites), defends the inclosure strategy against my briefly stated argument against its viability (Beall, 2013b). My discussion, in this paper, follows the WRPHC divide, though, for space reasons, I leave out a number of topics. Despite a keen temptation to discuss every turn and detail of both parts, I focus a bit more discussion (though not necessarily more pages) on the broader issue of the inclosure strategy (hence, the title of this paper). The inclosure strategy is by far the most prominent strategy for extending glut theory beyond what, by my lights, is its natural and limited place: standard paradoxes of truth – spandrels of truth, as it were (Beall, 2009).

The paper is structured as follows. I first provide a quick recap of (Beall, 2013b) in §1, followed in §2 by discussion of (the first part of) WRPHC's reply. (Again, for space reasons, I am selective in the given discussion, sticking only to the most salient points.) I argue that, on the whole, WRPHC's arguments do not undermine the main thesis advanced in Beall (2013b), namely, that the preserving-tolerance argument is not a strong one for the LP-based (glutty) approach to the sorites, since it is available to the classical-logic-based theorist. This leaves the principal argument for a glutty treatment of the sorites: namely, the inclosure argument. In §3, I take up the general topic of the inclosure strategy, rehearsing the basic strategy, my basic critique Beall (2013b), and finally turning to WRPHC's recent – and very interesting – defense of the strategy against my critique. I argue that their defense is not viable, and that even if the general inclosure strategy avoids dilemma, their defense undermines its application in the case of the sorites.

1 Review: an alternative argument for glut theory

Let me say, straightaway, that some of the criticisms that WRPHC launch against (Beall, 2013b) show that my discussion, in places, was at best unclear.[3] Still, the main gist of (Beall, 2013b) remains intact, despite their chief objections. My aim here is simply to (very briefly) rehearse the main gist of (Beall, 2013b); I reply to the WRPHC objections in §2.

The main gist of (Beall, 2013b) may be put as follows. The target glutty philosophers (viz., most of WRPHC, though I shall treat them all as indistinguishable for purposes of this discussion) agree, as I do, that standard 'spandrels of truth' – very familiar truth-theoretic paradoxes and other so-called semantic paradoxes – deliver gluts. But these are odd and rare beasts, tangled up in knots of truth and falsity on their surface – e.g., 'I am not true'. It takes substantial argument to extend the lesson of gluts beyond such a limited realm. In particular, given the vast ubiquity of vague language – most natural language predicates are vague – we need strong argument for applying the glutty lesson to vagueness, and in particular to the sorites. Acknowledging that 'this sentence is not true' is true and false is one thing; accepting that there are gluts nearly everywhere in the world (viz., in the vast samples of penumbral regions) is quite another. And WRPHC agree. Substantial argument is required to extend the glutty lesson to the sorites. They think that they have such argument.

WRPHC's chief argument for a glutty, LP-based approach to the sorites – the argument that is explicitly given – is Priest's inclosure strategy (to which I turn, at some length, in §3). That strategy, I argue in (Beall, 2013b, §4.2), is not strong, as it falters over Curry's paradox. (See §3 below for more discussion). The question, in turn, is whether there is a better argument. In (Beall, 2013b) I suggest what seems to be another argument available to the glutty approach: namely, what I call the *preserving tolerance* argument. This argument takes the project of solving the sorites – or, at least, one important project – to be a discovery problem. In particular, *tolerance conditionals* in the sorites appear to be true, but neither the conclusions of standard sorites arguments (which are by all lights absurd) nor all of the apparent logical consequences (e.g., the transitive closure of the conditionals) appear to be true.[4] The key project is accordingly one of discovery: finding 'true tolerance conditionals'. In large part, this project involves locating what might be called the *tolerance connective*. And here there are two

[3]This is particularly (though not only) so with my discussion of the extensional/intensional divide. I borrowed that terminology from the work of Weber and Priest, but regret doing so, as it proved to be more misleading than useful in my discussion.

[4]By 'transitive closure of the conditionals' I mean, in effect, further conditionals which, given transitivity, are implied by the standard tolerance conditionals. Example (if one likes, substitute standard instances involving one grain of sand, etc.): applying transitivity of a conditional $\rightarrow$ to $\{A_1 \rightarrow A_2, A_2 \rightarrow A_3\}$ delivers $A_1 \rightarrow A_3$, and so on.

salient constraints that emerge from the sorites:

C1. The tolerance connective – underwriting the true tolerance conditionals involved in the sorites – cannot be transitive, since not every conditional in the transitive closure of the tolerance conditionals is true.

C2. The tolerance connective (say, $\equiv_?$) cannot detach: $A \equiv_? B$ and A cannot jointly deliver B, for all A and B.[5]

These are necessary conditions; failing either one is sufficient for failing to find the target 'true tolerance' conditionals.

WRPHC agree that C1 and C2 are constraints on the project of finding true tolerance conditionals. Indeed, such agreement was behind my suggestion of the *preserving tolerance* argument in (Beall, 2013b), and my suggestion that such an argument – with the project of finding true tolerance conditionals so framed – appears, prima facie, to motivate the glutty (LP-based) approach. After all, the logic LP is a subclassical logic wherein the material biconditional $\equiv_{lp}$, in the logic LP, is neither transitive nor detachable. Hence, the glutty approach enjoys a very simple solution to the discovery problem: we need look no further than (the LP account of) the material biconditional.

My paper (Beall, 2013b) suggested such an argument in lieu of (what I argued to be) the much weaker, though explicitly given, inclosure argument (on which much more below in §3). The preserving-tolerance argument is an interesting one that, prima facie, gives support to the glutty approach. But this argument, I suggested (see Beall, 2013b), is also inadequate. And here is where the debate with WRPHC takes hold.

Why think the preserving-tolerance argument to be an inadequate argument for extending glut theory to vagueness? The answer in (Beall, 2013b), in short, is that a classical theorist can enjoy the LP theorist's tolerance connective too – though, clearly, under a different name. (This is *clear* given constraints C1 and C2, and the fact that the material biconditional, in classical logic, is both transitive and detachable.) What different name? The suggestion in (Beall, 2013b) is an *unassertability* operator, which in (Beall,

[5] I use the term 'deliver' here as a term to cover whatever the closure relation on one's theory of vague language winds up being (e.g., after you've thrown in a bunch of truths, perhaps contingent premises of standard sorites or etc., you want to invoke a closure operator on the theory to put in whatever else, according to the closure operator, 'needs' to be in there too). Logical implication is part of any such closure relation, and so constraint C2 demands that the tolerance connective not be detachable according to logic (i.e., that modus ponens not be valid for the cognitive); but the constraint needs to be maintained for your – usually stronger than logical implication – closure operator.

2013b) is defined 'intensionally' via a standard worlds-involving setup; but in fact the operator winds up behaving just like (and, in effect, just being) what is called 'negation' according to LP logical theory.

The LP theorist rejects the coherence of classical negation (see Beall, 2009, Ch. 3); but the classical theorist can enjoy 'LP negation' as being an interesting operator – in particular, an *unassertability* operator. Let μ be the given unassertability operator. If μ were negation (as LP theorists think it is), then $A \sqsupset B$, defined as $\mu(A) \vee B$, would be the material conditional; but instead, $A \sqsupset B$, so defined, is a conditional defined via *unassertability*, which, according to all theorists (in the current debate), is not negation. Either way, $\sqsupset$, so defined, is neither transitive nor detachable; and so constraints C1 and C2 are satisfied.

The suggestion in (Beall, 2013b) is that the classical theorist 'finds true tolerance' – solves the target discovery problem at the root of the sorites – in the same place that the LP theorist does: namely, the connective $\sqsupset$. In particular, the 'tolerance connective', conceived in biconditional terms, is $A \equiv_\mu B$, defined as $(A \sqsupset B) \wedge (B \sqsupset A)$. The LP theorist thinks of μ as negation, and so thinks of $\equiv_\mu$ as the material conditional (in LP) defined in the usual way (via negation and disjunction in LP). The classical theorist thinks that μ is not negation, and so does not think that $\equiv_\mu$ is the material biconditional. Instead, she thinks of $\equiv_\mu$ as a relation involving an elementary unassertability operator. According to glut theorists, the lesson of the sorites is the existence (at the penumbral region) of true falsehoods; such a lesson, of course, is rejected by classical theorists. But an exactly parallel lesson may be enjoyed by the classical theorist: true but unassertable sentences – again, at the penumbral region.

What was needed is a strong argument for applying glut theory to the sorites, for seeing gluts all over the place – the vast regions of penumbral phenomena. While the preserving-tolerance argument appears, by my lights, to be stronger than the inclosure argument, it is nonetheless not strong. The classical theorists can enjoy what is, in effect, the same solution; it's just that classical theorists see what is called 'LP negation' as some weaker-than-negation operator – unassertability. But if classical theorists can enjoy the solution – if they land on what, in effect, are the same 'true tolerance conditionals' – but do so without gluts, then the argument for gluts seems not to be strong.

So goes the main gist of (Beall, 2013b), and I still think it largely correct. But WRPHC lodge a variety objections, to which I turn.

2 Preserving tolerance: still not strong

I focus on five objections that WRPHC raise. For space considerations, I simply quote or briefly paraphrase their objection, and then briefly reply. More could be said on a number of fronts, but I say enough to give the main direction of reply.[6]

2.1 Ignoratio

WRPHC allege an ignoratio:

> We have proposed a solution to the version of the paradox with [the material biconditional defined via negation and disjunction]. He discusses a different version of the paradox. As such, what he says is an *ignoratio* (Weber et al., 2013, §1).

Reply. This is simply wrong. The project is to identify tolerance conditionals – at least if the preserving-tolerance argument is at all relevant to the LP approach (as WRPHC seem to agree that it is). The LP theorist says that LP material biconditionals $\equiv_{lp}$ are the solution; and the classical theorist (who thinks that the LP theorist is mistaken about what the real material conditional is) thinks that the LP theorist is right and wrong – right that something like $\equiv_{lp}$ is the right connective, but wrong to think that this is the material conditional. The material conditional, according to the classical theorist, detaches (i.e., modus ponens is valid for it) and is transitive, and these are definitely not marks of a suitable tolerance conditional. But the classical theorist still recognizes the existence and value of $\equiv_{lp}$; it's just that she thinks that it is built from an unassertability operator and not negation.

2.2 Changing the topic

Related to the ignoratio charge, WRPHC charge me with 'changing the topic', a charge notoriously made by Quine (1970) about subclassical and other non-classical logics. WRPHC say:

> Beall's approach...changes the subject: focusing on a claim about assertability, $A \equiv_{Beall} B$, rather than [on a material conditional

[6] WRPHC's paper is rich with ideas that, were it not for space limitations, I would like to engage with. My hope is that future debate will afford opportunities for discussing other elements of their critique.

> claim, defined via negation and disjunction, namely,] $A \equiv B$. (Weber et al., 2013, §1.1).

Reply. This charge is as unfortunate here as was Quine's notorious charge against applications of paraconsistent logics. In particular, the project is to find true tolerance conditionals; it isn't to give a theory of the material conditional on which tolerance conditionals can be safely true. (If that were the project, then, yes, LP is one very good candidate, but there are lots of others, including the non-transitive approach championed by Ripley and others Cobreros et al. (2010). And there are others; and so the LP-based approach would require much more argument even if we turn our attention to a save-material-conditionals-tolerance project.)

2.3 Material ('extensional') conditional and symmetry

Directly tied to the ignoratio and change-the-subject charges is the more foundational point that the proposed classical approach (in some sense, my proposed 'classical recapture of what's good about the LP approach') is not in a symmetric position with respect to the LP approach, contrary to the thrust of (Beall, 2013b).

> Beall's claim that the unassertability-based theory has all the virtues of the LP-based theory is false. An LP-based approach can accept extensional tolerance principles, and Beall's unassertability-based approach cannot; this is a key virtue of LP-based approaches, and it is not shared by the proffered replacement.

By 'extensional tolerance principles', in the given context, WRPHC are talking about tolerance principles expressed via *material biconditionals* or *material conditionals*, where these are defined via *negation* and disjunction.[7]

Reply. It is true, of course, that the classical theorist cannot locate true tolerance conditionals in the material conditional; for that conditional, in classical logic, is both transitive and detachable. But glut theorists are in precisely the same sort of situation.[8] In particular, glut theorists (or at least WRPHC) recognize conditionals beyond what's available in LP itself; they recognize a need for conditionals that are both transitive and

[7]'Extensional' is a broader term, but in the context their concern is the material (bi-) conditional.

[8]Again, I regret following the terminology of 'intensional' and 'extensional', as it wound up being misleading. The key points, highlighted by WRPHC's discussion, are independent of the extensional/intensional divide.

detachable (Priest, 2006; Weber, 2010).[9] Now, for any conditional, we have corresponding tolerance conditionals (viz., $P_1 \rightarrow P_2$, etc., where P_i is vague and $\rightarrow$ the given conditional). From this array of candidates, the project of locating 'true tolerance' emerges. Some of the conditionals are implausible for some reasons, some for other reasons.[10] But one key feature that makes many such conditionals implausible is either *transitivity* or *detachment*. And it is precisely these features that are invoked to reject many candidates from the pool of 'true tolerance conditionals'. On this point, WRPHC agree.

WRPHC correctly note that in (Beall, 2013b) I say next to nothing about the (classical) material-conditional candidate for tolerance conditionals. But something can be said. In particular, in the search for true tolerance conditionals, the material conditional – by classical lights – is ruled out on elementary grounds: namely, it fails not only one but both necessary conditions C1 and C2. Of course, one owes some account of why material-conditional versions of the sorites appear to be prima facie plausible (at least for a moment, perhaps without noticing even the transitive closure). And on this count the proposed unassertability account is offered. In particular, the proposal is that there's a conflation: one conflates the material conditional with true tolerance conditionals, which are in fact defined not from negation but from an elementary unassertability operator. That was the suggestion in (Beall, 2013b).

2.4 Intuitive reading

In the search for true tolerance conditionals, the LP theorist and (proposed) classical theorist wind up with (in effect) the same candidate; it's just that the former thinks that the candidate connective is the material conditional (as it appears in LP) and the latter thinks that the connective involves not negation but unassertability. On this score, WRPHC see a major victory for the LP approach versus the proposed unassertability approach.

[9]Some glut theorists (see Beall, 2009, Ch. 2) used to see the need for detachable conditionals but no longer do (see Beall, 2013a); but I leave further discussion for elsewhere.

[10]In (Beall, 2013b, §4.6) I objected to the standard explanation – viz., 'too strong' – given by glut theorists for why relevant-logic-related tolerance conditionals are not plausible, arguing that once the tolerance principles are 'fully dressed' the proffered explanation is not an explanation. WRPHC disagree, but I must confess to not seeing where my (fairly modest) point is wrong. But I think that this issue, as WRPHC seem to agree, can be set aside – and so I do. I note here only that my point about 'full dress' was not intended as an argument against the LP account or for the unassertability account; it was merely aimed at the standard (viz., 'too strong') explanation for why the given sorites arguments, involving relevant-logic-based tolerance claims, are implausible.

> The material equivalences are at the centre of the LP approach. Rightly so: that consecutive members of the sorites sequence are equally true/false is the key intuition driving the sorites paradox. This is exactly what the material biconditional states. It is the *obvious* statement of tolerance. (Weber et al., 2013, §1.1)

The thought, then, is that since both approaches wind up with what is, in effect, the same candidate for true tolerance conditionals, the better approach is to be decided by level of familiarity and intuitiveness of the given accounts. And WRPHC claim that the key intuition to be preserved is 'the' one according to which consecutive sorites pairs (of sentences P_1, P_2, etc.) share the same semantic status, not one involving assertability or unassertability.

Reply. I'm not convinced that WRPHC are right that it is *semantic status* – or *alethic status*, as they say – that is 'the' key intuition driving the sorites paradox. (Indeed, it's far from clear that there is a unique, even precise-enough such 'intuition', which is the reason behind my shuddering of 'the' in 'the key intuition'.) It seems to me that some notion(s) of assertability/unassertability are at play in the sorites paradox, but I will not try to develop or defend this claim here.

What is worth noting is that WRPHC cannot claim that the LP account of 'same semantic/alethic status' is the 'familiar and powerful intuition' behind the sorites. In particular, note that on this 'familiar intuition' we can – and according to the given glut theorists, do – have it that B is just false (indeed, it can be $1 = 0$ on the classical understanding of that) even though A is true *and* it's true that *A and B are equally true/false*![11] In other words, on the LP account, we truly say of some pair of sentences $\langle A, B\rangle$ that they're 'equally true/false' even when A is true and B is outright absurd (say, $1 = 0$ or 'every sentence is true'). This is just the way the material biconditional works in LP. By my lights, there is nothing incoherent about this; but it is simply wrong – at the very least, misleading – to claim that such a reading is a *familiar and powerful one.* As such, I disagree with WRPHC that the LP reading fares better than the unassertability account with respect to preserving familiar and powerful intuitions. In fact, I suspect that they are equally familiar/unfamiliar.

[11] I am not hereby arguing that the LP account of 'both true or both false/untrue' is somehow flawed or inherently problematic or the like. Not at all. I am noting only that it is very far from familiar, at least to the many who have thought about the sorites.

2.5 Gluts in the end: assertability paradox?

WRPHC raise one other point about the proposed unassertability approach for classical theorists. That approach relies on some notion of assertability. But if the notion of assertability itself is subject to paradox, generating contradiction, then one winds up needing a non-classical (presumably, subclassical) logic in the end, perhaps something in the vicinity of LP. If that's right, then the whole proposal on behalf of classical theorists would seem to be moot – or so WRPHC suggest. The question WRPHC raise is whether *assertability* does in fact generate contradiction. And they say that it does.

> Consider a sentence a of the form 'a is not assertable' [so that we have the true identity $a =$ 'a is not assertable']. Suppose that a is assertable. Then a is true.... So a is not assertable. Hence, by *reductio*, a is not assertable. But we have just established this. Hence, we can assert it. So, a is assertable – as well. It is precisely not the case, then, that one may purchase freedom from contradiction by invoking the notion of assertability. (Weber et al., 2013, §3)

But this argument assumes 'capture' behavior for the given assertability operator: namely, where α is the assertability operator, that arbitrary sentence A delivers (or perhaps implies) $\alpha(A)$. But whatever account of *assertability* may be like this (and I'm not convinced that there is one), it is precisely not the one at issue in (Beall, 2013b), wherein I explicitly reject that the notion of assertability so 'captures'.

> Assertability is an operator that 'releases' (i.e., has truth as a necessary condition) but does not 'capture' (i.e., truth is not sufficient for assertability). (Beall, 2013b, Postscript)

Of course, WRPHC may be – probably are – relying on background principles that tie *proof* to the relevant notion of *assertability*; but such principles themselves are tricky to articulate, and in all cases require defense. (The corresponding principles in standard knower-paradox cases are thought by many to be implausible.) Alternatively, WRPHC may be invoking a different notion of assertability, which does directly deliver paradox; but in that case, we need argument that connects the paradoxical notion to the not obviously paradoxical one at issue in (Beall, 2013b). Either way, the given argument against purchasing freedom from contradiction via a notion of assertability is itself not clearly a strong argument as it stands.

3 End of inclosure

I conclude, from §§1–2, that the preserving-tolerance argument does not adequately motivate glut theory with respect to the sorites. But it is worth recalling that the target glut theorists explicitly advanced a very different argument, one relying on the inclosure strategy. I argued in (Beall, 2013b) that the inclosure strategy was not viable, and set it aside to explore the preserving-tolerance argument. WRPHC have replied to my against-inclosure argument in an interesting way. Because of the weight that some glut theorists place on the inclosure strategy, this development is of broad and independent (of the sorites) interest. In what follows, I briefly rehearse the inclosure strategy, and my criticisms of it; and then examine the reply of WRPHC. In the end, I argue that, despite WRPHC's defense, the strategy is still not viable, in general; and that WRPHC's defense of it undermines its application to the sorites, in particular.

3.1 Inclosure schema: what is it?

I simply review the basic idea here. There are many points at which questions can be raised (including, e.g., background set theory, etc.), but I slide over many such questions here. The inclosure scheme, and overall strategy (for extending glut theory via the inclosure scheme), is discussed at length by Priest (2002).

The inclosure scheme involves a set Ω, two unary predicates φ and ψ, and a function δ, where the following conditions are satisfied (see Priest, 2002, §9.4ff):

1. $\Omega = \{y : \varphi(y)\}$ and $\psi(\Omega)$.
2. For any $X \subseteq \Omega$ such that ψ is true of X,
   (a) Closure: $\delta(X) \in \Omega$
   (b) Transcendence: $\delta(X) \notin X$.

In the limiting case, where $X = \Omega$, we have a contradiction: $\delta(\Omega) \in \Omega$ and $\delta(\Omega) \notin \Omega$.

Example: let Ω be the 'collection' of all truths (so that φ is truth). Let ψ be the property of being well-defined. In turn, δ can be thought of as taking subsets of Ω to sentences, namely, the sentence 'I am not in X'. By standard liar-like reasoning, we get a plausible argument for Closure and Transcendence: namely, that $\delta(\Omega)$, which is the sentence 'I am not true' or

'I am not in the collection of truths', is both in Ω and not in Ω. And precisely the same sort of situation exists in the case of the Russell–Zermelo–Cantor paradox (concerning all non-self-membered sets/collections, etc).

3.2 Inclosure strategy: towards gluttiness

In the background of the inclosure strategy is the principle of uniform solutions (PUS): namely, same problem, same solution. The difficulty with PUS is generally locating the 'same kind of problem'. And that is where the inclosure schema comes into play.

How is the inclosure schema used dialectically? The inclosure scheme is supposed to capture the essential structure of familiar paradoxes of self-reference, and thereby – together with PUS – serve as an argument for treating all such (inclosure-paradoxical) phenomena as gluts. The inclosure schema is supposed to capture an essential structure of paradoxical phenomena that, in turn, explains their paradoxicality.

3.3 Problem: Curry's paradox

The inclosure strategy, which has always turned on the principle of uniform solution (viz., same kind, same treatment), is undermined if there are inclosure paradoxes that do not get the same treatment as the other inclosure paradoxes. (Recall that the inclosure schema is supposed to capture the essential structure of an important kind, and that being an inclosure paradox just is being of the target kind.) In (Beall, 2013b), I leveled what has always seemed to me to be the simple problem with the strategy.[12] The objection, in a nutshell, is straightforward: Curry's paradox – I say – is an inclosure paradox; the argument for its being so is as strong or weak as those in the case of the liar, Russell–Zermelo's paradox, and the other familiar cases. In particular, *quoting* WRPHC themselves:

> Curry's paradox is said [by Beall] to fit the inclosure schema where these are as follows (and T is the truth predicate):
>
> - $\varphi(y) :=$ 'Ty'
> - $\psi(x) :=$'x is definable'
> - $\delta(x) := s$

[12]Others have shared similar concerns, including Grattan-Guiness (1998), Grim (1998), Williamson (1996), perhaps among others. Irvine (1992) also points to Curry concerns, though perhaps not directly to inclosure.

> where s is a sentence of the form $\langle s \in \dot{x} \rightarrow \perp \rangle$, and $\dot{x}$ is a name for x. Let $\Omega = \{y : y \text{ is true}\}$.
>
> The function $\delta(x)$ is clearly defined when $\psi(x)$ holds. The set Ω exists since it is a set of sentences of some (countable) language, and it is obvious that $\psi(\Omega)$. Take an x such that $x \subseteq \Omega$ and $\psi(x)$. Suppose that $\delta(x) = s$ and $s \in \dot{x}$. Then, since $x \subseteq \Omega$, it follows that s is true, and so $s \in \dot{x} \rightarrow \perp$, which gives $\perp$. Discharging the supposition, $s \in \dot{x} \rightarrow \perp$; that is, Ts; that is $s \in \Omega$ (closure). But since $s \in \dot{x} \rightarrow \perp$ is true, if $s \in \dot{x}$ then $\perp \in \Omega$, which it is not. Hence, by contraposition, $s \notin \dot{x}$ (transcendence). The limit contradiction arises when x is Ω. Then $s \in \Omega$ and $s \notin \Omega$, where $\delta(\Omega) = s$. (Weber et al., 2013, §2.1)

The given inclosure argument for Curry's paradox is as (im-) plausible as those for other inclosure paradoxes. Hence, as said in (Beall, 2013b, §4.2), it looks very much as if Curry's paradox is an inclosure paradox.[13] But, then, Curry's paradox seems to spell the end of inclosure.

In short, Curry's paradox – I say – is an inclosure paradox, which engenders a dilemma for the inclosure strategy: if Curry's paradox is treated as a glut, absurdity quickly follows (given detachment); if Curry's paradox isn't treated as a glut, then the inclosure strategy – treating all inclosure paradoxes the same – is undermined as being at best ad hoc.

3.4 In defense of inclosure

WRPHC give an interesting reply to the problem. On one hand, WRPHC concede that the sentence $c = \langle Tc \rightarrow \perp \rangle$ fits the inclosure scheme, in the sense of having an appropriately plausible argument for so fitting. How, then, do they avoid thereby undermining the inclosure strategy? Here is their reply (quoted at length, for clarity):

> So Curry's paradox fits the schema? No; this [e.g., $c = \langle Tc \rightarrow \perp \rangle$] is not Curry's paradox! ...The problem posed by Curry-style reasoning is that it allows us to establish an *arbitrary* sentence, not just $\perp$. If A is any sentence, and s is the sentence $Ts \rightarrow A$, the Curry reasoning allows us to establish A. This is problematic whether A is true or false. There is obviously something crazy about supposing that we can establish (the truth) that Canberra

[13] I assume, throughout, that we are talking about *detachable* Curry sentences – that we have a detachable conditional. None of WRPHC will dispute this.

> is the capital of Australia in this way, for example. And once one sees this, it is clear that the paradox does not fit the inclosure schema. Specifically, if we replace $\perp$ with a true A in the above reasoning, the argument to transcendence fails, since we can no longer appeal to contrapostition. Of course, individual instances of the Curry reasoning, say when A is $\perp$, may satisfy the inclosure conditions. ...But that does not show that the Curry paradox itself is an inclosure paradox—any more than the fact that there are instances of cubic equations, $ax^3+bx^2+cx+d=0$, which are quadratic (when $a=0$) shows that cubic equations are quadratic. And it is bad form to take special cases to deliver general conclusions. Hard cases make bad law. (Weber et al., 2013, §2.1)

The main line of reply, then, is that I have misidentified Curry's paradox; and once properly identified, Curry's paradox has no suitably plausible inclosure argument (i.e., plausible argument that it fits the inclosure schema); and so no difficulties with respect to PUS (see §3.2). WRPHC put the matter thus:

> Beall is attempting to appeal to the *principle of uniform solution*: same kind of paradox, same kind of solution (Priest 2002, §§11.5, 17.6). What is at issue, then, is the question of whether the Curry paradox is an inclosure paradox. All the instances of the Curry paradox—whatever the A is—are clearly of the same kind. And it is not an inclosure paradox, since instances of it do not fit the schema. By the uniform solution principle, the Curry paradoxes all require the same treatment, but not (necessarily) the same as that of the liar, the sorites, etc. (Weber et al., 2013, §2.1)

So, it looks like Curry's paradox is not a problem for the inclosure schema. But looks, I argue, are misleading.

3.5 A dilemma: the end of inclosure

The recent WRPHC objection is problematic. The problem can be seen as a dilemma: either the inclosure strategy captures too much (on pain of absurdity) or too little (it misses target phenomena/paradoxes). To see this, focus on a foundational question triggered by the WRPHC reply itself:

Q1. What, then, *is* an inclosure paradox?

Either inclosure paradoxes are individual sentences or they're sentential schemes (such as the sentential scheme to which WRPHC point). If the former (viz., individual sentences), then we have curry inclosure paradoxes; for, as in §3.3, we have that $c = \langle Tc \rightarrow \bot \rangle$ has a plausible inclosure argument. Turn to the latter case: inclosure paradoxes are sentential schemes. Another question arises:

Q2. What is it for a sentential scheme to be (inclosure-) paradoxical?[14]

Here, we again branch into two cases.

- Super-paradoxical: a sentential scheme is an inclosure paradox iff *all* instances are (inclosure) paradoxical.
- Sub-paradoxical: a sentential scheme is an inclosure paradox iff *some* instances are (inclosure) paradoxical.

If Q2 is answered along *sub-paradoxical* lines, then the WRPHC 'curry scheme'

$$s = \langle Ts \rightarrow A \rangle$$

counts as (inclosure) paradoxical – and we get absurdity, assuming (as WRPHC do) that the given arrow detaches (i.e., satisfies modus ponens). If Q2 is answered along *super-paradoxical* lines, we avoid absurdity at the cost of missing target paradoxes. Consider, for example,

$$g = \langle \neg Tg \vee \bot \rangle$$

which is no longer an inclosure paradox![15] To be an inclosure paradox, g would have to be a sentential scheme; and it isn't. The most obvious scheme for g is

$$h = \langle \neg Th \vee A \rangle$$

But this isn't super-paradoxical, since not all instances of it are inclosure-paradoxical: consider any true instance of A.

There are other natural (though suspect) schemes in the area.[16] Consider, for example, the following finer-grained and 'semantically constrained'

[14] I will leave implicit that we are talking about *inclosure paradoxes* – and so 'paradoxical' means inclosure-paradoxical.

[15] Similarly, consider Albert of Saxony's (1351) paradox: $r = \langle \neg Tr \wedge \top \rangle$. This too is a target inclosure paradox. (Thanks to Aaron Cotnoir, who, during discussion period, reinvented this paradox after my presentation of an earlier draft of this paper at St Andrews, and to Steve Read for the historical reference to Albert of Saxony.)

[16] One serious issue with the retreat to schemes is that we now have so many that it's difficult to know which of them count. But I flag this only to set it aside – not as unimportant but as something too big to discuss here.

sentential scheme, where A_{u} is schematic over (contingently or otherwise) untrue sentences:

$$j = \langle \neg Tj \vee A_{\mathsf{u}} \rangle$$

This *is* super-paradoxical, and so counts as an inclosure paradox on the super-paradoxical account. But, of course, if this scheme is what saves the inclosure strategy from losing target paradoxes like $g = \langle \neg Tg \vee \bot \rangle$, it also dooms the strategy by allowing the corresponding curry-ish scheme, namely,

$$s' = \langle Ts' \rightarrow A_{\mathsf{u}} \rangle$$

which would count as inclosure-paradoxical, since, even by WRPHC lights, all instances are inclosure-paradoxical.

3.6 Focusing on neo-curry paradox

If, as it appears, we are to turn to schemes as the genuine inclosure paradoxes, we seem to confront what we might call neo-curry paradox:[17]

$$j = \langle Tj \rightarrow (A \wedge \neg A) \rangle$$

There are three issues that arise from neo-curry paradox.

3.6.1 Neo-curry essentially involves negation

One point that WRPHC make about Curry's paradox is that it doesn't essentially involve negation:

> As (Priest, 2002, p. 169) notes, Curry's paradox has nothing essentially to do with negation: it would arise even if the language were entirely positive. It therefore has nothing to do with contradictions that occur at various boundaries (limits), which is what inclosures are all about. (Weber et al., 2013, §2.1)

But if inclosure paradoxes are sentential schemata, then at least neo-curry paradox does essentially involve negation (among other tools). Hence, were 'essentially involves negation' to be added to the necessary conditions for inclosure paradoxes – and I think that WRPHC are not suggesting that it be added – even this condition would not rule out neo-curry paradox.

[17]What follows emerged from a discussion with philosophers at University of Alberta's Logic Group, including particularly Allen Hazen, Jeff Pelltier, Grace Paderson, and Andrew Tedder, and others in attendance. Tedder provided useful follow-up discussion.

3.6.2 Neo-curry affords transcendence

WRPHC will likely reply to neo-curry by saying that it is clearly only an instance of 'the' Curry paradox, which, as they put it, *is paradoxical because its reasoning would allow us to prove whatever we want (be it good or bad)*. According to them, *real Curry paradox* doesn't afford contraposition (on the consequent), which is required to get the transcendence condition in the inclosure argument.[18] But neo-curry does.

Grant that 'real curry' is the WRPHC scheme $s = \langle Ts \rightarrow A\rangle$, and grant that we can't get inclosure for this, because we can't get $\neg Ts$ which is required for the target transcendence (see §3.3). Fine. Concentrate on *neo-curry paradox*: $j = \langle Tj \rightarrow (A \wedge \neg A)\rangle$. Reason as follows:

- Towards Closure: usual Curry reasoning. (See §3.3 above.)
- Towards Transcendence: in the target logic(s) – of the sort that WRPHC endorse – we have the truth of all instances of the sentential scheme:

  $$\neg(A \wedge \neg A).$$

  But, then, from contraposition of neo-curry j, we have

  $$\neg(A \wedge \neg A) \rightarrow \neg Tj$$

  which, together with the noted antecedent, implies $\neg Tj$, that is, as WRPHC put it (see §3.4 above), that j is not true, that is, that j is not in Ω, which in the given case contains all truths.

Let me repeat: we can grant that 'real Curry' is the general scheme, and that neo-curry is not general curry; but it's clear that neo-curry is a special sort of instance. Indeed, this is itself its own paradox (certainly for the inclosure scheme). And this is the second issue accentuated by neo-curry.

3.6.3 Pace wrphc, not all instances are of a kind

According to WRPHC, '[a]ll the instances of the Curry paradox—whatever the A is—are clearly of the same kind' (Weber et al., 2013, §2.1). But that's wrong, as neo-curry shows and as WRPHC themselves seem to admit: some

[18]Recall that WRPHC's point: transcendence comes from contraposition on $Ts \rightarrow A$, and in turn from detaching on the negation of consequent A. In short, their point – which I'm not contesting – is that we can't get $\neg Ts$ because in cases of true A, we don't also (in general) have $\neg A$ to detach on the contraposition of $Ts \rightarrow A$, namely, $\neg A \rightarrow \neg Ts$.

instances, like neo-curry, afford plausible inclosure arguments (via contraposition). The inclosure scheme is supposed to capture an essential kind; and at least one easily and naturally identifiable family of Curry instances *does* fit the paradox, namely, neo-curry.

Perhaps the truth about Curry's paradox is that it's a family of distinct kinds of paradox. (Maybe. I concede that WRPHC have raised this interesting possibility.) But none of this helps the problem that Curry's paradox, at least some types of them, raise for the inclosure strategy.

In short, we have a paradox – namely, neo-curry (or, for that matter, the 'old curry' using $\perp$) – which is a familiar paradox of self-reference; it has a plausible 'inclosure argument' (using contraposition). As WRPHC note, there is another paradox which is related to neo-curry but does not have an 'inclosure argument', namely, the general paradox where we ignore 'bad consequents', where the reasoning lets us prove anything – good or bad. It appears, at least prima facie, that *both* paradoxes are important kinds. Why not think that this appearance is in fact veridical? WRPHC offer no account here; and it is not clear that a viable account can be given, at least not one that keeps all the 'good' cases and misses the 'bad'.

Upshot. It is difficult to accept that, as it aspires to do, 'the inclosure analysis...explain[s] all the usual paradoxes of self-reference' (Weber et al., 2013, §2.1) when it can't accommodate standard target cases such as 'either I'm not true or *Mind* is a leading journal of poetry', that is, cases like $g = \langle \neg Tg \vee \perp \rangle$ or $r = \langle \neg Tr \wedge \top \rangle$ or the like.[19] Hence, I conclude as before (Beall, 2013b): namely, that the 'inclosure analysis' does not provide reason to accept that any of the familiar paradoxes of self-reference are glutty. In the end, the inclosure strategy faces a curry-related dilemma: it rules out 'bad' curry cases on pain of ruling out too many 'good' cases. But if that's right about the paradoxes of self-reference, then all the worse for extending the strategy to the family of sorites paradoxes, to which I briefly return.

3.7 Inclosure and the sorites: a distinct problem

I close with a final observation that brings us back to the WRPHC's target application: the sorites. Let us set aside whether the foregoing considerations undermine the inclosure strategy in general. There is a distinct problem that confronts the inclosure argument for the sorites, at least assuming the

[19] I should note that Priest (2002) is explicit that he thinks that such cases as g are inclosure paradoxes; and I expect that the others among WRPHC do too. But they aren't inclosure paradoxes if, as the WRPHC defense of the inclosure strategy seems to demand, inclosure paradoxes need to be super-paradoxical schemes.

WRPHC defense of the inclosure argument against my curry-related critique.

WRPHC claim that Curry's paradox isn't really an inclosure paradox, since what's at the heart of Curry's paradox is that its reasoning allows us to prove A no matter what A is – be A safely true, false, absurd, whathaveyou. Let us grant as much (though, in truth, I do so only for purposes of discussion). By parity of perspective (so to speak), the same is true of the sorites. After all, what is bad about the sorites is not that it lands in absurdity; rather, it allows us to prove whatever we want from an appropriate soritical series – including the truth that Dave Ripley is tall, that 10K grains of sand is a heap, that Marcus Rossberg is bald (on top), and so on. To 'prove' as much, one nearly needs to start with the right soritical series (viz., staying at the 'safe side' of a typical soritical series).

Of course, the sorites paradox also allows us to prove absurdities if we go to a sufficiently dangerous collection (or sequence) of objects (including, e.g., not just the good cases of tallness, but the untrue ones, etc.). Such dangerous collections of objects get all the attention in philosophy; and this is precisely the same as the dangerous Curry instances – the ones that have dangerous, untrue, absurd (etc.) consequents. But, then, applying the WRPHC reply in the case of the sorites, we conclude that what's troubling about the sorites is not essentially the absurd conclusions that one can derive from the given reasoning (and appropriate starting class of objects); it's rather that one can prove anything – true or not – depending on which 'instances' (collections of objects) one starts with. But, then, this undercuts the inclosure argument for the sorites at precisely the same point at which WRPHC arrest the corresponding argument for Curry's paradox: namely, contraposition. In the sorites case, one stipulates from the start that $\delta(X)$ is not in Ω.[20] But this is simply the stipulation that we're going to focus only on the dangerous collections of objects. If this is legitimate in the sorites case (to get the inclosure argument to work there), it is legitimate in the Curry case: namely, focusing on the dangerous collections of instances (namely, the untrue consequents).

I conclude that if the WRPHC defense of the inclosure strategy works in the case of Curry's paradox, it likewise 'works' in the sorites paradox. (In short, we generalize from the bad cases to a scheme covering all cases – good, bad, whathaveyou.) But if the WRPHC defense works in the sorites case, then the inclosure argument for the sorites is undermined. Transitivity

[20]In the WRPHC inclosure argument for the sorites paradox, $\delta(X)$ is the first object (in one's chosen sequence) which is not in the family Ω of objects satisfying some given vague predicate. See (Colyvan, 2009; Priest, 2010; Weber, 2010).

of our current ‘if’ makes for the rub. In short, if WRPHC’s defense avoids Curry’s paradox to save the inclosure strategy, WRPHC lose the inclosure argument for the sorites itself. The price of looking to extend glut theory via the inclosure strategy is stuck with horns.[21]

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