

More on ‘A Liar Paradox’

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Tim Maudlin observed some time ago that, even if we discard Tarski’s T-biconditionals in favor of rules of inference, as Kripke (1975) suggests we should, we can still derive a contradiction from the law of excluded middle, if proof by cases is available.¹ Suppose we have a sentence Λ for which

$$(1) \quad \Lambda \equiv \neg T(\ulcorner \Lambda \urcorner)$$

Then we can reason as follows:

(i)	$\Lambda \vee \neg\Lambda$	Excluded Middle
(ii)	Λ	Premise
(iii)	$T(\ulcorner \Lambda \urcorner)$	T-intro
(iv)	$\neg\Lambda$	(iii), (1)
(v)	$\neg\Lambda$	(i), (iv), Proof by Cases
(vi)	$T(\ulcorner \Lambda \urcorner)$	(v), (1)
(vii)	Λ	T-elim
(viii)	$\Lambda \wedge \neg\Lambda$	(v), (vii), $\wedge+$

But we can improve this argument if we instead have a term λ for which

$$(2) \quad \lambda = \ulcorner \neg T(\lambda) \urcorner$$

Then we can reason this way:

¹ Maudlin showed me these sorts of argument when he visited Harvard in 1996. They play an important role in his book *Truth and Paradox* (Maudlin, 2004).

(i)	$\mathbf{T}(\lambda) \vee \neg\mathbf{T}(\lambda)$	Excluded Middle
(ii)	$\mathbf{T}(\lambda) \vee \mathbf{T}(\ulcorner\neg\mathbf{T}(\lambda)\urcorner)$	Transparency
(iii)	$\mathbf{T}(\lambda) \vee \mathbf{T}(\lambda)$	Identity
(iv)	$\mathbf{T}(\lambda)$	$p \vee p \vdash p$
(v)	$\mathbf{T}(\ulcorner\neg\mathbf{T}(\lambda)\urcorner)$	Identity
(vi)	$\neg\mathbf{T}(\lambda)$	Transparency
(vii)	$\mathbf{T}(\lambda) \wedge \neg\mathbf{T}(\lambda)$	(iv), (vi), $\wedge+$

Whereas the first argument shows, for example, that the supervaluational version of Kripke's theory of truth cannot validate proof by cases—which supervaluational views rarely do, anyway—this argument shows that no supervaluational view can treat the truth-predicate as transparent, since the other inferences used in the derivation are clearly going to be valid on any such view. It's not, of course, that this reveals some new problem with supervaluational views, since extant forms of such views (e.g. McGee, 1990) do not validate transparency. But it still seems worth observing that transparency only *can* be (consistently) combined with excluded middle if one is willing to give up either Leibniz's Law or the inference from $p \vee p$ to p .² Or to do something even more radical and declare that implication is not transitive.

Of course, the first of these arguments, which depends upon (1), can be recast in various ways. One might use Transparency and the inference from $p \equiv q$ and $p \vee r$ to $q \vee r$ instead of the T-rules and proof by cases. But, obviously, since the argument rests crucially upon (1), it will have to appeal to some sort of principle involving the biconditional (or, at least, the conditional). In the argument using (2), on the other hand, the conditional does not even appear.

One lesson of these reflections is thus that we can reduce the expressive and logical resources such arguments require by appealing to the 'strong' form of the diagonal lemma that delivers, for each formula $A(x)$, a term t for which

$$\vdash t = \ulcorner A(t) \urcorner$$

² The inference just mentioned would itself be proven by cases in many presentations, but that does not mean that the plausibility of that inference depends in any way upon that of proof by cases. It does not seem to me to do so. (This is one place that formalization seems to obscure rather than illuminate epistemic relations.)

rather than to the ‘weak’ form that delivers only a formula G for which

$$\vdash G \equiv A(\ulcorner G \urcorner)$$

And part of my purpose in “A Liar Paradox” (Heck, 2012) was to call attention to this difference, which I’ve also emphasized elsewhere (Heck, 2007), but whose significance still seems to me not always to be appreciated.

That was not, however, my main purpose. I showed in the later parts of the paper that the two truth-theoretic principles:

$$(3) \quad \neg(S \wedge \mathbf{T}(\ulcorner \neg S \urcorner))$$

$$(4) \quad \neg(\neg S \wedge \neg \mathbf{T}(\ulcorner \neg S \urcorner))$$

lead to paradox, given only Leibniz’s Law, the inference from $\neg(p \wedge p)$ to $\neg p$, and such background assumptions as that implication is transitive. I then argued that (3) and (4) are intuitively compelling, even absent transparency (given which they are equivalent to non-contradiction), and, in particular, that they are *more* compelling than Tarski’s T-biconditionals, to which they are classically equivalent but, in a broader sense, weaker. In conclusion, then, I wrote:

... [T]his form of the Liar shows... that there can be no consistent resolution of the semantic paradoxes that does not involve abandoning truth-theoretic principles that should be every bit as dear to our hearts as the T-scheme once was. And that leads me, anyway, to be tempted to conclude that there can be no truly satisfying, consistent resolution of the Liar paradox. (Heck, 2012, p. 39)

It is, of course, a matter of judgement what counts as “truly satisfying”, and I did not attempt to explain in any detail why I find the various moves that can be made here not, in the end, to be such. I was simply assuming that many people—though not, obviously, *all* people—would agree with me, for example, that abandoning the transitivity of implication in the face of the Liar is the formal equivalent of sticking one’s head in the sand.³ Or to put the point differently: If the choice is between the transitivity of implication and inconsistency, then the latter is starting

³ Or, perhaps better, it’s the formal equivalent of a well-known joke from Smith and Dale’s famous vaudeville routine “Dr. Kronkheit and His Only Living Patient”. Smith: “Doctor, it hurts when I do *this*.” Dale: “Don’t *do* that.”

to look pretty good. And, indeed, I have elsewhere expressed sympathy with the view, deriving from Tarski, that the concept of truth is intrinsically inconsistent (Heck, 2004, pp. 345–6). So, when I said that there may be no “truly satisfying, *consistent* resolution of the Liar paradox”, the word “consistent” was there for a reason. That does not mean that I favor paraconsistent or dialetheist treatments of the Liar. My views are closer to those of Eklund (2002; 2007) and Patterson (2006; 2009), and the relation of those views to dialetheism remains somewhat unclear (Beall and Priest, 2007; Eklund, 2008).⁴

Having thus explained what I did, and did not, intend to accomplish in my earlier paper, I now turn to two replies to it, one by David Ripley (2013c) and one by Julien Murzi (2013).

1 Reply to David Ripley

There are two sorts of claims that Ripley makes in his response. They are made back to back at the beginning of his paper:

Unlike some of the theories Heck considers [elsewhere], existing transparent truth-theories are already fully compatible with terms like Heck’s λ . Because of this, transparent responses to the usual formulation of the liar paradox extend without modification to Heck’s variants. (Ripley, 2013c, p. 220)

In the first sentence, Ripley is contrasting “existing transparent truth-theories” with other collections of truth-theoretic principles I’d considered in an earlier paper (Heck, 2007, §3.2). Those ones are, as he implies, consistent with the existence of sentences like Λ but inconsistent with the existence of terms like λ . Ripley seems to think that I am playing the same game again, and he is claiming, in response, that using λ instead of Λ does not affect whether any of the “existing transparent truth-theories” are consistent. He then goes on to make the stronger claim that the existing theories are not just consistent, but that the resources they

⁴ One point perhaps worth making is that it is one thing to think we need a good account of how we should reason in the presence of contradictions, on the ground that we might sometimes be in a position where we have no real option but to do so, and quite another to think that some contradictions might actually be true, so that we need a logic in which contradictions do not entail everything. This is one place, that is to say, that it is important to respect the distinction between validity and inference, famously emphasized by Harman (1988).

deploy in responding to the “ordinary liar”—as we shall see, it is not entirely clear what that is supposed to mean—can simply be re-deployed, “without modification”, to respond to the allegedly new paradoxes I’d described. That claim is re-iterated at the end of his paper:

...[T]he existing approaches to transparent truth give responses to the ordinary liar that extend without modification to all three of Heck’s arguments. ... Any reason not to like a transparent truth theorist’s response to Heck’s arguments is already a reason not to like their response to the ordinary liar, since these responses match. (Ripley, 2013c, p. 222)

So, Ripley is claiming, there is nothing new in my discussion that should worry fans of transparent truth-theories.

Let’s begin with the weaker claim: that using λ instead of Λ does not affect whether any of the “existing transparent truth-theories” are consistent. Indeed, it does not. But that is so obvious that I can only conclude that either Ripley is badly misinformed about some of the relevant literature, or else he thinks that I am. After all, in the three-valued versions of Kripke’s theory of truth (these being the ones that are transparent), the strong version of the diagonal lemma is the only one available. We *do not* and *cannot* have

$$(1) \quad \Lambda \equiv \neg T(\ulcorner \Lambda \urcorner)$$

in Kripke’s theory, for any sentence Λ , since this will, in Kripke’s proprietary sense, always be paradoxical: It cannot be true in any fixed point. I made exactly this point in footnote 4 of my paper. Ripley, by contrast, indicates in footnote 2 of his paper that he regards Kripke’s theory as merely “allow[ing] for th[e] possibility” of using the strong version of the diagonal lemma (rather than the weak one) to generate self-referential sentences. But, as I have said, and as is clear from careful presentations of Kripke’s theory (e.g. Burgess, 1986, §2), this isn’t just an option, it’s required. So I certainly wasn’t arguing that “existing transparent truth-theories” are only consistent because they are formulated in languages lacking the expressive power needed to prove the strong version of the diagonal lemma.

Let us turn, then, to the stronger claim, that “existing transparent truth-theories” can reply to my versions of the Liar the same way they respond to the “ordinary liar”. In some cases, this is true. In fact, it is true for three of the four sorts of transparent theories that Ripley

distinguishes: paracomplete, paraconsistent, non-transitive, and non-contractive. But it is not true in the case in which I was most interested.

So-called non-transitive theories are a curious hybrid of paracomplete and paraconsistent theories,⁵ and in many cases their response to the Liar coincides with more familiar ones. That is true in the case of the paradoxical reasoning I presented, at least. The steps involved in that reasoning are valid both for ‘strict’ and for ‘tolerant’ assertion. In the former case, however, the starting point—excluded middle or non-contradiction—is not accepted, just as with paracomplete theories; in the latter, the paradoxical conclusion is accepted, just as with paraconsistent theories. So, in that regard, the reasoning I presented does not engage the distinctive features of these theories but rather highlights ways in which they are not particularly distinctive.

Non-contractive theories evade the Liar by rejecting the structural rule of contraction. I will discuss these theories in the next section, since Murzi defends such an account.

Paraconsistent theories do not evade the Liar, but happily accept the conclusion that the liar sentence is both true and false. Instead, they reject the inference known as *ex falso quodlibet* and thereby prohibit us from inferring that the moon is made of cheese. In this connection, Ripley (2013c, p. 221) writes: “All three of Heck’s arguments finish in a contradiction, and it is simply assumed that that’s a bad thing”. This is not only incredibly uncharitable—vast swaths of the literature, even on the Liar, assume that contradictions are a bad thing—but it completely ignores the conclusion for which I was actually arguing: “. . . [T]here can be no truly satisfying, *consistent* resolution of the Liar paradox” (Heck, 2012, p. 39, my emphasis). What I was assuming was not that contradictions are bad but that, if a theory proves contradictions, it is inconsistent. Surely that should be uncontroversial.

That leaves us with paracomplete theories. Such theories are, as Ripley notes, immune to the paradoxes I presented because they reject their starting points: $T(\lambda) \vee \neg T(\lambda)$ or $\neg(T(\lambda) \wedge \neg T(\lambda))$, as the case might be. But that does not, by itself, show that “transparent responses to the usual formulation of the liar paradox extend without modification to [my] variants” (Ripley, 2013c, p. 222). It only shows that existing

⁵ The term “non-transitive” seems to me to be very poorly chosen. There is a consequence relation in such theories that is non-transitive, namely, the relation between ‘strict’ assertions taken as premises and ‘tolerant’ assertions taken as conclusions. But there is no reason whatsoever to think such a relation should be transitive, and the fact that this relation happens to be classical seems to me little more than a curiosity.

paracomplete theories aren't vulnerable to these paradoxes, which, as I have said, should have been antecedently obvious.

Part of the difficulty in evaluating Ripley's claim is that there isn't any such thing as "the usual formulation of the liar paradox", so it is not terribly clear what claim he is making. Surely, however, the following is as usual a formulation as any:

(i)	$\mathbf{T}(\lambda)$	Premise
(ii)	$\mathbf{T}(\ulcorner \neg \mathbf{T}(\lambda) \urcorner)$	Identity
(iii)	$\neg \mathbf{T}(\lambda)$	See Below
(iv)	$\mathbf{T}(\lambda) \wedge \neg \mathbf{T}(\lambda)$	$\wedge+$
(v)	$\neg \mathbf{T}(\lambda)$	Reductio
(vi)	$\mathbf{T}(\ulcorner \neg \mathbf{T}(\lambda) \urcorner)$, etc.	

Kripke (the original paracompletist) would reject this reasoning because he takes the inference from (ii) to (iii) to be justified not by the T-biconditional $\mathbf{T}(\ulcorner \neg \mathbf{T}(\lambda) \urcorner) \equiv \neg \mathbf{T}(\lambda)$ but by the rule of inference $\mathbf{T}(\ulcorner A \urcorner) \vdash A$, and such rules cannot be applied in subordinate deductions, e.g., in the context of proofs by *reductio*. So the move to (v) is invalid.

The following seems pretty familiar, too:

(i)	$\lambda = \ulcorner \neg \mathbf{T}(\lambda) \urcorner$	(2)
(ii)	$\lambda = \ulcorner \neg \mathbf{T}(\lambda) \urcorner \rightarrow \mathbf{T}(\lambda) \equiv \mathbf{T}(\ulcorner \neg \mathbf{T}(\ulcorner \lambda \urcorner) \urcorner)$	Leibniz's Law
(iii)	$\mathbf{T}(\lambda) \equiv \mathbf{T}(\ulcorner \neg \mathbf{T}(\ulcorner \lambda \urcorner) \urcorner)$	Modus Ponens
(iv)	$\mathbf{T}(\ulcorner \lambda \urcorner) \equiv \neg \mathbf{T}(\ulcorner \lambda \urcorner)$	Transparency

Of course, we still have to get a contradiction out of the last line, but we can show, quite generally, that $p \equiv \neg p$ implies $\neg p \wedge \neg \neg p$ without using excluded middle but only *reductio*,⁶ and the use of *reductio* that is needed

⁶ Thanks to Guilherme Araujo for reminding me of this point. One simple argument is:

(i)	$p \vdash p$
(ii)	$p \equiv \neg p, p \vdash \neg p$
(iii)	$p \equiv \neg p, p \vdash p \wedge \neg p$
(iv)	$p \equiv \neg p \vdash \neg p$

The same sort of argument then shows that $p \equiv \neg p \vdash \neg \neg p$, so $p \equiv \neg p \vdash \neg p \wedge \neg \neg p$. (Note the use of contraction at (iii), by the way.)

here is one that is acceptable to Kripke, since no T-related inference is involved in that argument. But Kripke would still reject this form of the paradox because of its reliance at (ii) upon the conditional form of Leibniz’s Law, which is invalid in Kripke’s theory.⁷ Rather, we have Leibniz’s Law only as a rule: $t = u, A(t) \vdash A(u)$.

Although Kripke does reject excluded middle and non-contradiction, then, that does not seem to me rightly to be described as part of his “response to the ordinary liar”. Rather, Kripke’s response involves trading, at crucial points, conditionals for (proper) rules of inference.⁸ The best-known of these, of course, is trading the T-scheme for the T-rules but, as the second example illustrates, there are other places Kripke makes the same sort of move. Granted, too, Kripke rejects the law of bivalence: the assumption that every sentence is either true or false. But, as I emphasize in my paper (Heck, 2012, p. 37), although rejecting bivalence provides excellent motivation for rejecting excluded middle, it provides very little motivation for rejecting the law of non-contradiction. To respond to this observation as Ripley (2013c, p. 222) does—by pointing out that excluded middle and non-contradiction are equivalent “in the logics paracompleteness tend to favor”—just misses the point, which was to draw attention to the fact that paracompleteness *cannot* accept either principle without abandoning the paracomplete approach.

One can think of the matter this way. The sort of theory introduced by Field (2008) is often said to improve on Kripke’s by introducing a ‘reasonable conditional’, one that satisfies such otherwise appealing principles as $A \rightarrow A$, which Kripke’s theory rejects.⁹ It is a natural question whether we might also salvage $\neg(A \wedge \neg A)$ within a consistent,

⁷ Since (ii), in particular, is not true but paradoxical: It has a true antecedent and a paradoxical consequent.

⁸ It is often said that Kripke rejects *reductio*, conditional proof, and proof by cases, but that is not really the right way to put it. As I said above, what he rejects is the use of certain sorts of inferences *within subordinate deductions*, which is what makes them proper inference rules. Such rules can be formalized using the same sorts of resources I have discussed elsewhere in connection with vagueness (Heck, 1993, 1998).

⁹ Feferman (1984, p. 95) famously claimed that, without such a conditional, “nothing like sustained ordinary reasoning can be carried on”. I’ve never quite understood his reasons myself, so I don’t find the introduction of such a conditional all that impressive philosophically, however impressive Field’s technical achievement may be. That said, Field also uses his conditional to introduce various notions of determinacy. That is where the action really is, since these are what fuel Field’s claim that his theory is revenge-immune (Field, 2003). But we do not need the conditional to introduce determinacy operators—that can be done directly—and, as Welch (2008, §5) has argued, such operators are present also in revision-theoretic approaches to the paradoxes.

transparent theory of truth: Is it possible to have a ‘reasonable negation’ and a ‘reasonable conjunction’ that together validate the law of non-contradiction? It may yet be possible, but one lesson of my paper is that, if we are going to save $\neg(A \wedge \neg A)$, then we *must* abandon one of a small set of otherwise appealing principles. So my point was not that paracompletists “use[] slack in their theory of the biconditional to respond to the liar paradox” (Ripley, 2013c, p. 221). Rather, my points were: (i) that problems very much like the one that leads Field to be dissatisfied with Kripke’s theory afflict his own account; (ii) that Field’s ‘reasonable conditional’ cannot help with those problems, since the conditional is not involved in the paradoxes I discussed (that being true only because we are using λ rather than Λ); and (iii) that, since the principles used in deriving a contradiction from $\neg(T(\lambda) \wedge \neg T(\lambda))$ are exactly the ones that other sorts of transparent truth-theories already abandon anyway, paracompletists have no option but to reject the law of non-contradiction. That seems to me to mark “a serious, if often overlooked, limitation” of paracomplete theories, to quote Murzi (2013, mp. 6).¹⁰

It is, however, an obvious question why it is not sufficient simply to deny all instances of $A \wedge \neg A$, rather than to assert their negations (Ripley, 2013c, p. 222). Why, in particular, is it not enough simply to deny $T(\lambda) \wedge \neg T(\lambda)$, rather than to assert $\neg(T(\lambda) \wedge \neg T(\lambda))$? My worry about this move, which I expressed very briefly in a footnote (Heck, 2012, p. 39, fn. 6), is that we must also deny $\neg(T(\lambda) \wedge \neg T(\lambda))$, since it leads to a contradiction. That is not (yet) incoherent, but it fails to respect the felt asymmetry between $T(\lambda) \wedge \neg T(\lambda)$ and $\neg(T(\lambda) \wedge \neg T(\lambda))$. Treating $T(\lambda) \wedge \neg T(\lambda)$ and $\neg(T(\lambda) \wedge \neg T(\lambda))$ exactly the same way just feels wrong.¹¹

This point is related to ones made by Shapiro (2004), whom I cited in this connection. Shapiro is concerned with paraconsistent theories, and he argues that, while it is all well and good for dialetheists to accept both $T(\lambda)$ and $\neg T(\lambda)$, we still need *some* way to assert a proposition that excludes the possibility that its negation might also be asserted:

¹⁰ Murzi is actually talking about such theories’ failure to validate (3) and (4), above, but, given transparency, these are equivalent to non-contradiction. Moreover, Murzi approvingly cites Zardini’s remark that non-contradiction is an “extremely plausible law” that “disappointingly fails even in the best [paracomplete] theories” (Zardini, 2012, p. 513). So I take it he would agree with my use of his words.

¹¹ For what it’s worth, I think there are other asymmetries the usual paracomplete theories fail to respect, too, such as that between the liar and the truth-teller. This is because such theories fail to make any real use of the full range of fixed points.

a way, that is, to say that a given proposition is only true and not also false.¹² Asserting the negation of the proposition is obviously not enough: paraconsistent negation is too weak. Nor, Shapiro notes, is it enough simply to invoke some notion like denial, since one wants to be able to *suppose* that a given proposition is only true, or to embed that thought in the antecedent of a conditional. (That, of course, is a version of the Frege–Geach problem (Geach, 1965).) So what we seem to need is an ‘exclusionary’ negation. But that negation will sustain exactly the form of the law of non-contradiction that dialetheists reject, and new paradoxes will threaten.

These sorts of considerations apply not just to paraconsistent theories, but quite generally.¹³ If we make the distinction between assertion and denial, then, for the same sorts of reasons we need a truth predicate, we will also need an ‘untruth’ predicate that stands to denial much as the truth predicate stands to assertion—or so Shapiro’s arguments seem to me to show.¹⁴ It should be clear that, in the context of Kripke’s theory, at least, such a predicate would allow us to define the sort of groundedness predicate that is known to reinstate paradox.

Obviously, this sort of point needs much more development, and I cannot pursue it here. I mention it only to make it clear that, though I am, admittedly, somewhat skeptical about the distinction between denial and assertion of the negation, that general skepticism was not what was fueling the (all too) brief remarks about the issue in my paper. My real worry is that this distinction, whatever its status overall, simply cannot help us with the Liar.

2 Comment on Julien Murzi

Two days after I approved the final proofs of “A Liar Paradox”, I received the December 2011 issue of *The Review of Symbolic Logic* in the mail. That issue contained two (!) articles by my colleague Josh Schechter, as well as Elia Zardini’s paper “Truth Without Contra(di)ction”. Being a long-time fan of Zardini’s work, I read his paper that afternoon. (I’d

¹² I believe this sort of point was first made by Parsons (1990). Related points are made by Weir (2004, §6) and by Littman and Simmons (2004, §§4–6).

¹³ In particular, a similar point holds about the the distinction between so-called ‘strict’ and ‘tolerant’ acceptance upon which non-transitive approaches rely. Indeed, tolerant assertion can often be identified with rejection of the negation. One can even build a non-transitive theory out of Kripke’s this way.

¹⁴ Ironically enough, Ripley (2013a) offers arguments in this same ballpark.

already read Josh’s.) The connection with my paper was immediately obvious, and I thought about asking the editors if I could have it back to make some additions. But we had been through several rounds of proofs, and I was fairly certain that there would be another opportunity to say what needed saying. Which, now, there is.

Zardini (2012) shows that, if we reject the so-called structural rule of contraction—which, in effect, affirms that the premises of an inference constitute a set, rather than a multi-set or a sequence—then that is sufficient by itself to restore consistency to the naïve theory of truth. The inferences $p \vdash p \vee p$ and $\neg(p \wedge p) \vdash \neg p$, on which the paradoxes I’d presented crucially depend, are precisely the sorts of inferences for which contraction is needed. In this case, then, the sort of response Zardini would have us give to the “ordinary liar” really is exactly the same as the response he’d have us give to the semantic paradoxes presented in my paper: On his view, all such paradoxes turn on invalid appeals to contraction.

I thus find little to which to object in Murzi’s paper. It is true that I underestimated the appeal such a response to the Liar might have. But I want to emphasize that what makes Zardini’s response potentially attractive is not just the formal consistency of the sort of theory he develops, nor even the possibility that it might extend to related sorts of paradoxes, such as the paradox of naïve validity (Murzi, 2013, §5). What made me take notice was the intriguing metaphysical motivation that Zardini gives for restricting contraction.¹⁵ What we want to know, in the present context, is how it could be true that $\neg(p \wedge p)$ but not be true that $\neg p$. Zardini’s suggestion is that p here might be ‘unstable’, somehow oscillating between truth and falsity, and that the two occurrences of p in $\neg(p \wedge p)$ might be, so to speak, co-unstable, so that each is true when the other is false. Then $\neg(p \wedge p)$ would be true, no matter which stage of the oscillation we were at, but $\neg p$ would only be true from time to time. And the same sort of reasoning can be used to explain how $p \vee p$ could be true, though p was not.

¹⁵ It was not, then, that I did not realize one could, in principle, reject $\neg(p \wedge p) \vdash p$. What I did not realize was that there might be a principled way of rejecting it. Not because I wasn’t aware that there are logics in which contraction fails—I well remember the excitement generated by the publication of Troelstra’s *Lectures on Linear Logic* (Troelstra, 1992)—but because I couldn’t see any natural motivation for rejecting it in the present context. By contrast, the philosophical justifications offered for non-transitive theories (Ripley, 2013b) strike me as wholly uninteresting. (Similarly, there have been authors who would reject the appeals to Leibniz’s Law in the paradoxes I presented. But I just do not see the motivation for such a rejection.)

It's an interesting idea, though I for one have a hard time seeing how the two occurrences of p in $\neg(p \wedge p)$ might be oscillating against one another. As Zardini observes, there are clear analogies between his idea that paradoxical sentences are 'unstable' and some of the underlying motivations for the revision-theoretic approaches due to Gupta (1982) and Herzberger (1982). But, so far as I know, there is no prospect, on revision-theoretic treatments, of a single sentence oscillating in opposition to itself. Still, it would be nice to know if such analogies could be developed and exploited, especially since the determinacy operators that are available in such theories (Welch, 2008, §5) might make it possible to address revenge worries.¹⁶

At this early stage, it would be foolish for any of us to pass final judgement on this sort of approach. There are more questions than answers so far, as Zardini (2012, §5) makes clear. What certainly is true, however, is that the paradoxes presented in my paper shine a bright light on the role played by contraction in the semantic paradoxes. The right thing for supporters of contraction-free approaches to do, then, is to co-opt my work and use it for their own purposes, much as Murzi does and, indeed, as Zardini himself has done in work as yet unpublished.

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¹⁶ In linear logic, there are unary connectives (the exponentials) that allow us to express when contraction is permissible and when it is not. Can these connectives, or similar ones, be added to Zardini's system and used to express, within the system, the idea that a particular sentence is 'stable'? Or would that reinstate paradox? I'm guessing the latter, which would mean Zardini had a revenge problem.

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