

Consistent Inconsistency Theories*

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In this paper I critically evaluate a number of current ‘consistent inconsistency theories’ and then briefly motivate a rival position. The rival position challenges a consistent inconsistency theory, by sharing many of its basic commitments without suffering the problems that such a theory appears to face.

§0 Consistent Inconsistency Theories

A number of philosophers are prepared to concede that, in a certain sense, there is no satisfying solution to the liar and related paradoxes (where a satisfying solution manages to preserve many of the logical, semantic, expressive, etc. features that we once attributed to languages and their users). These philosophers are not prepared to grant that impending (or inevitable) inconsistency should be accepted, in the manner of dialetheists. They point out that, while dialetheists may be in position to resolve some of the liar-like paradoxes, the very sort of ‘expressive’ worries that motivate dialetheism appear to plague it just as they plague consistentists. (I think, for example, of Curry’s Paradox, but there are other examples that may also make the same point.) This foils the philosopher’s attempt to ‘update’ the logic that governs inconsistency in such a way as to ensure that trivialism—the claim that every sentence is true (as well as false)—is avoided.

A dialetheist accepts that some sentences are true with true negations—that some contradictions are true (as well as false). A dialetheist thus accepts inconsistency. We

might call such a theorist an *inconsistency theorist* and we might advertise her view as an *inconsistency view*. Actually, these labels are somewhat imprecise, for one—a dialetheist, for example—can accept that there is inconsistency, while also accepting that which is inconsistent. Let's call such a person an *inconsistent inconsistency theorist* (herein, an 'IIT'). And let's call one who accepts that there are inconsistencies without granting (or accepting) the resultant inconsistency a '*consistent inconsistency theorist*' (herein, a 'CIT').¹ Unlike an IIT, a CIT will explain both the putatively problematic feature of our language (or our theory of the language, etc.) and why those problematic features don't have the impact that we previously took them to have. Insofar as a paradox involves *both* a diagnosis of the apparent pathology *and* a treatment, which is meant to render the pathology merely apparent, we might say that a CIT does not see all putative paradoxes as paradoxical, since, while they grant that there is a diagnosis, they do not think that a treatment is, or must be, in the offing.

In this short note, I look critically at a few of the recent CITs.² My goal is to determine whether the CITs offer a genuine alternative that we should embrace, granting the problems with standard consistent and dialetheic solutions to the semantic (for the most part, liar-like) paradoxes. To this end, I contrast two different sorts of CITs. According to the first (herein, 'CIT¹'), the language that, according to him, causes the appearance of paradox, is inconsistent, or the semantic theory that yields it is untrue. According to the second (herein, 'CIT²'), though the language is consistent (and otherwise true), there are problems regarding what competence with a language appears to demand. As I will show, in some ways, CIT¹s appear to be more resilient than a CIT². That said, once we see what a CIT¹ amounts to, we will find that there is some reason for being

attracted to an alternative position, which grants some of what the CIT¹s endorse without the consequences of denying the paradoxicality of certain (and apparent) paradoxes. In the concluding section of this note, I briefly sketch such an alternative position.

§1 CIT¹

Matti Eklund (2002, 2005) has developed and defended a CIT¹. He calls the untrue premises and invalid steps in the liar and the sorites the *culprits* of the argument,³ and contends that being semantically competent with the expressions that are employed in the paradoxes involves being disposed to accept the culprits as true or valid. When speakers are disposed by virtue of their semantic competence to accept the culprits in an argument, Eklund says that the unsound argument *exerts pull*. Pull exertion explains the puzzling feature of the paradoxes: What is puzzling about the paradoxes is that semantic competence with the expressions involved demands that speakers be disposed to accept principles—meaning-constitutive principles (herein, ‘MCPs’)—not all of which can be true.

Eklund contends that a natural language, L, is inconsistent “just in case the principles—by which I will mean sentences and inferences—partially constitutive of the meanings of expressions of L are jointly inconsistent, where a principle is constitutive of the meaning of an expression just in case competence with the expression involves being disposed to accept the principles as true or valid.” On Eklund’s view, the language is inconsistent because the MCPs are jointly inconsistent.

It doesn't follow from the fact that the collection of MCPs (e.g., for 'true') that competent users are disposed to accept are jointly inconsistent (nor does it follow from the fact that not all of them can be true) that the expression is, therefore, inconsistent or is, in some way, meaningless. Ensuing inconsistency or incoherence is only directed at what competent users are compelled, by their competence, to accept. To explain: Suppose that the MCPs for 'true' consist of all instances of the disquotational schema, DS. And suppose, further, that the meaning of 'true' is provided by instances of DS, not in the sense that the meaning of 'is true' makes all these instances true, but that this meaning is determined by them in some more complicated way. Eklund's claim is that semantic competence demands that a language user be disposed to accept the instances of DS, even though that principle is invalid, as not all of its instances can possibly be true.

Although Eklund takes semantic competence with the truth predicate to involve a disposition to accept inconsistent claims, he (2002, p. 322) claims that "[t]he semantic values of expressions are such as to make the [MCPs] come out as nearly correct as possible, where for a principle to come out correct is for it to be true or valid, respectively." With this in mind, we can see how 'true' can be meaningful, even though competence demands that the liar exert pull:

"Let the sense of an expression be the principles that competence with the expression disposes a speaker to accept. Then the semantic values of the expressions of a language are determined by the senses of the expression of the language in that the semantic values are whatever comes *closest* to satisfying the conditions laid down by the senses."

Eklund's point about pull exertion suggests that there is some sort of inconsistency or incoherence inherent in the sense of certain expressions, or in the concepts they express. Is that the only sense in which the liar makes evident inconsistency?

Here is a part of the picture: Although some sentence will fall into the extension of 'is true', no sentence is such that both it and its negation do. Moreover, even if competence with 'true' requires that DS be in some sense valid, it does not follow that the extension of 'true' is such that the schema really is valid. Given an assignment of semantic values to expressions of the language, there will be some expressions (sentences) that will always be in the extension of 'true'. But there will be others that will never be in the extension of 'true'—for example, a liar sentence,

(L) L is not true.

Indeed, as a competent user, Eklund may even claim to know that the liar will not be in the extension of 'true'.

Return to Eklund's contention that the liar exerts pull. We should distinguish two ways in which pull exertion relates to competence. On a *strong reading*, that the liar exerts pull is a necessary condition for competence with 'is true', on a *weak reading*, that the liar exerts pull is a sufficient, but not a necessary, condition for competence with 'is true'. The strong reading will be difficult to establish, as Eklund (this volume) is aware. To see why, consider a putatively competent user of English who is aware of the paradoxes and the problems that they appear to raise. Perhaps initially the user was disposed to accept every instance of DS, which, by assumption, are all MCPs for 'true'. But once she became familiar with the paradoxes, she (correctly) lost the disposition to accept every MCP, in which case the liar failed to exert pull and, on the strong reading,

she would have been deemed semantically incompetent. So, if Eklund's point about the exertion of pull is to be preserved, it will have to figure into a sufficient condition for competence. Although that contention might also be hard to establish, I will assume it, for what follows.

Eklund sees a problem with this reading, weak or strong, which he calls *the problem of discipline*: "If our semantic competence involves being disposed to accept jointly inconsistent claims, why do we not, as competent speakers, feel compelled to accept inconsistent verdicts?" Consider Eklund's solution to the problem. He (Ibid.) contends that his problem can be solved, provided we grant that speakers have "an implicit grasp of how the correctness of the [MCPs] is maximized."

I won't worry about whether regular folk actually have such a grasp. I am interested to understand this third position (one that is neither consistentist nor dialetheist) but I am having a hard time seeing how Eklund's position is actually a 'third position'. Eklund is competent with 'true' and is familiar—perhaps *too* familiar!—with the liar paradox. Accordingly, we may reasonably assume that he is not disposed to accept every instance of DS. Given his solution to the problem of discipline, it follows that he has an implicit grasp of how to maximise the correctness of these putative MCPs. Accordingly, it seems that Eklund is in the following boat:

- (i) He is a competent user with 'true';
- (ii) The liar does not, for him,, exert pull (and he firmly believes that not all of the principles are true); and
- (iii) He has a grasp, however feeble, of the principles that are true or valid.

Here's the problem: If this is an accurate reading of his position, given the weak reading and his solution to the problem of discipline, *then in what sense is Eklund's position an inconsistency view?* Of course he can characterise his position however he he'd like, but, knowing what he does and grasping what he can, in what sense does Eklund take the concept of truth to be inconsistent?

Actually, the rhetorical can be put more strongly. An implicit grasp of how to maximise the correctness of the putative MCPs for 'true' requires that there be a consistent maximalisation. Even assuming that there need not be a *single*, determinate consistent maximisation, if we grant, with Eklund, the (mere) *presence* of consistent maximalisations, we will be forced to conclude that liar-revealing inconsistency is merely an appearance. If this is right, aren't we forced to conclude that Eklund does not, after all, hold an inconsistency view?

§2 Eklund and Revenge

Let's suppose that Eklund has an answer (and ignore any appearance of *semantic epistemicism*). Recall that, according to him, while semantic competence compels us to accept inconsistent claims, the apparent inconsistency does not filter down to the meanings of our semantic expressions. Thus, even if a disposition to accept liar-involving instances of the disquotational schema is a necessary condition for semantic competence, that we accept such instances does not impact the meaning of 'true' (although it may bear on the concept that we employ, where concepts employed are individuated by the MCPs competence requires us to accept, and not merely by those that are true). Thus, even if we

are driven by semantic competence to accept inconsistent claims, it does not follow that the expressions of the language are meaningless; that is, that they are without semantic values.

Consider our liar sentence, L (= 'L is not true').⁴ On Eklund's view, no liar sentence will be in any acceptable extension of the truth predicate. Now, Eklund accepts his position and knows that if the position is correct, L will not be in the (acceptable) extension of the truth predicate. Thus he will grant that L cannot be in the extension of the truth predicate and accept that L is not in the extension of the truth predicate. If this is right then, given some platitudes about extensions and predicates, it seems that he should be prepared to grant that L is not true. But if he grants, as it seems that he must, that L is not true then, unless he has a very unorthodox account of assertion, he will assert that L is not true. But L = " L is not true" and, while he clearly accepts that L is not true, he does not accept L and, thus, does not believe *that L is true*. But, if he does not believe that L is true then, since he is aware of the relevant equivalence, he will not assert that L is not true. Hence, it seems both that he will assert that L is not true and he will not assert that L is not true.

I see three options, by way of response, none attractive:

- Option #1** He can assert that L is not true, in which case he asserts something that he does not believe to be true (which is problematic, at least *prima facie*);
- Option #2** He can assert that L is not in the extension of "true" but cannot assert that L is not true (which is *ad hoc*); or
- Option #3** He cannot assert that L is not in the extension of "true", even though it's part of his view.

None of these seem acceptable. Accordingly, if the above is correct then it seems either that Eklund is stuck with revenge, or he avoids it only by revising or rejecting certain principles (e.g., regarding assertion) that we had no reason to question.⁵ In this sense, it appears that Eklund is plagued by the very worries that drove consistentists to a CIT.

§3 CIT²

In the last section, I worried that Eklund faces revenge and that, even if he we leave that aside, it remains unclear in what sense his is an inconsistency theory. I turn now to the positions put forward by Azzouni and Patterson, both of whom are CIT²s.

Jody Azzouni (p. 18, this volume) reports that English is trivially inconsistent:

“Speakers have tacit knowledge of the rules of their language. Those rules are inconsistent; and because inconsistency implies everything, it follows that in unregimented English everything is both true and false.”

Doug Patterson agrees to a point but does not claim that natural languages are inconsistent. According to Patterson, it is, rather, that competent speakers process English in accordance with an inconsistency theory, while acting as though theories that are in fact inconsistent are true.

How, one might wonder, can English speakers tolerate this? Azzouni is not worried, for he claims that, trivial inconsistency notwithstanding, we language users presume that our language is consistent. That is (Ibid), “we take ourselves as able to use talk of the true and the false” even though “these tools can’t make the distinctions we want.” How does this help? Azzouni (Ibid.) continues:

[C]ontradictions in natural language are *local* and *isolated*: Although the implication relations that ordinary speakers adhere to make natural languages (globally speaking) inconsistent (they induce everything to be true and false) — almost all of the specific reasoning that speakers engage in is itself free from such. This is especially clear in the case of liar paradoxes, and their kin: Such are routinely ignored (“shunned” might be a better word) by ordinary speakers. This means that no one actually draws any implications from them and so the body of purported knowledge that speakers (collectively) are building up is not—strictly speaking—tainted by such.

On Azzouni’s view, although our language is inconsistent (so that everything is true and false), this inconsistency is not a *worry*, given a general presumption of consistency.

Even seeing inconsistency, Azzouni will not go on to accept that everything is true.

Regimentation offers as an answer to the apparent disease (Ibid. p?): “[R]egimentations of natural languages *must* reconstrue our ordinary inference practices so that they can be taken to be ones that are occurring in a consistent medium.” According to Azzouni, in regimentation, we treat features of natural language as if they occur in a consistent language and we treat the day-to-day practice of inference *as if* it takes place in a ‘consistent medium’, even though it doesn’t.

I will not be concerned with Azzouni’s *Vaihinger-esque* claim about our use of, or our need for, consistent settings. What I am particularly interested in is his argument for the claim that English is inconsistent. Azzouni maintains that inconsistency emerges from the apparent fact that certain expressions of the language (‘true’, ‘false’, ‘not’) exist and have the properties ordinary speakers take them to have. More specifically, he (Ibid., p?) maintains that because “there is nothing apparently available in natural language [to resolve the liar paradox], it follows that natural languages are inconsistent”.

Azzouni's argument does not establish that a natural language is, or must be, inconsistent. All that follows from his premises is that natural languages are *apparently* inconsistent.⁶ Perhaps that is enough. Perhaps what is really important is whether Azzouni's regimentation will succeed. This may be, but it is worth pointing out two features of the language—or our use of it—that Azzouni does not consider.

First, notice that the very same reasoning that Azzouni employs to 'establish' that the language is inconsistent, would also 'establish' that the language is indeterminate (I suspect that the same can be established for Patterson, who contends that the semantic theory for English is false since, if true, everything is). I think, for example, of the truth-teller,

(T) (T) is true,

which can consistently be assigned either truth or falsity. It is easy to establish that, given any apparent case of inconsistency, there is, in addition, a case of indeterminacy.⁷

Reasons for thinking that the language is inconsistent are thus reasons for thinking that it is indeterminate.⁸ This is *not* to say that the language cannot be inconsistent. It is rather to say that one cannot *just* endorse a consistent inconsistency view, for it should follow that she should also endorse a consistent indeterminacy view.

Before moving to the second consideration, it bears noting that both Azzouni and Patterson endorse a version of *semantic psychologism*, viz., the view that the expressions of a language cannot have meanings that are *other than* the ones that competent speakers take them to have.⁹ In this they differ from Eklund, who, as we have seen, takes expressions to have semantic values about which competent speakers are straightforwardly wrong. One brand of psychologism breaks down the barrier between a

folk semantic theory and a semantic theory. The thought is that if the folk operate under a folk semantic theory for English then, whatever that folk theory is, *that* is the semantic theory for English.

That said, unlike Azzouni, Patterson does not conclude from the fact that our folk semantic theory is inconsistent that the language is therefore trivially inconsistent. But he does acknowledge that it would be inconsistent, if the semantic theory for English were true. Why think that the semantic theory is untrue?

Patterson claims that the semantics for English is untrue because, if it were true, everything would be the case. But why think that? He reads the untruth off of what he takes speakers of English to accept. He (2007, p. ?) contends that speakers of English “are inclined to accept that a predicate applies to all of the things that have the property it expresses.” From this, he contends that speakers are inclined to accept that “does not apply to itself” applies to itself if, and only if, it does not apply to itself, which is logically false. What, then, about the liar? Patterson (forthcoming) proposes that we “let the arguments that demonstrate inconsistency stand, accept that they show that competent speakers are inclined to accept something false and try to understand what follows.” Put differently, Patterson counsels that we “simply accept that understanding English is best represented as a relation to a logically false semantic theory.... Speakers process in accord with inconsistent theory, but since communication does not require their impressions about what their sentences mean to be true, and since they do not believe all of the consequences of what they believe ..., they get along fine.” He suggests that we accept his solution, since it is the cheapest one on offer. We should “go with the

inconsistency theory, let the contradictions flow, and get on in life ignoring them most of the time.”

This might be well and good, but what are we supposed to do if we grant that the semantic theory for English is untrue? Patterson (Ibid.) has an answer: “I and readers who believe me ... understand English even though we do not believe the [semantic theory for English].” He (Ibid.) further contends that speakers have “misleading impressions about the expressive power a language can have because they process their own languages according to logically false theories.” Thus, just as Azzouni charges the folk with assuming that their language is consistent, even though it isn’t, Patterson charges the folk with assuming that they understand English even though, in a certain sense, there is nothing there to be understood.

§4 Azzouni, Patterson and Revenge

Let’s be clear on where we are. Azzouni and Patterson propose that, as far as the semantic theory for English is concerned, each of the steps in liar is correct. This is not to say that each of the steps are true (in Azzouni’s case, true only), for either the semantic theory that yields them is false or the language in which they are couched is trivially inconsistent. What about revenge? Are Azzouni and Patterson at risk in the way that I have suggested that Eklund’s CIT² appears to be?

They are not. That said, I do think that they face what could be a problem. One of the standard criticisms of dialetheism is that the dialetheist faces expressive limitations. Say that you are a dialetheist and that you are asked whether a particular sentence is true

or false (or, you are asked whether you accept it or reject it). Say that you reject it and so would assert that it is false. Can you express your denial of it, simply by asserting that it is false? If you reject all contradictions, it would seem that you can. But if we know that you are a dialetheist then, from the fact that you ascribe falsity to a sentence, we cannot conclude that you wish to deny that sentence, for you might also take that sentence to be true. The question for the dialetheist is this: Can the dialetheist (truly) express that a sentence (proposition, or what have you) is *false only*?

If this worry about dialetheism is serious (and others contend that it is, though I think it can be solved), it will plague Patterson (and Azzouni, if it is given the status of a regimentation), as well. Suppose that Patterson is asked whether he rejects and so would deny the liar. Can he express his denial of the liar assertorically, for any liar sentence that we can dream up? Perhaps it seems that he can. But if we are aware of his response to the liar, we will not accept that anything that he says is true. So, we get the parallel question: Can a CIT² (truly) express that liar (of whatever stripe) is not true? I am not saying that this is a *devastating* problem, if the CIT² cannot; I am merely noting that CIT²s face the very same criticisms that have been made against the dialetheists.

Whatever the merits of that worry, there is an important sense in which Azzouni and Patterson have a satisfying response to the question of revenge. Suppose that Azzouni and Patterson reject, and so would deny, the liar. Accordingly, one might think that, like the rest of us, they, too, must semantically characterise the liar (or at least express that it has whatever 'semantic status' they take it to have). This thought would be incorrect for, if we take the CIT²s respective 'diagnoses' seriously, we should conclude that they will not be able to express in English everything that we want to express. Hence,

we will allow that they will claim the liar to be meaningless (or that it fails to express a proposition, or what have you), but will go on to note that, given their ‘solution’, the fact that they cannot express this *in English* is hardly surprising.

Let’s assume that the CIT²s can answer the threat of revenge in roughly the way that I noted above. Let’s also suppose that the CIT²s can resolve the dialetheic-like worry, so that that worry may be put to rest. Must we, then, endorse and adopt their position on the liar? I think not or, to put it more accurately, I am not convinced that one must be a CIT², even if they are right that the standard dialetheist or consistentist solutions to the paradoxes are going to be (in some sense) expressively problematic. In order to show this, let’s return briefly to some of the features of their respective accounts.

§5 Error Theories and Confusion Theories

Azzouni (this volume) contends that Patterson endorses an *error theory of understanding*. Azzouni attributes the error to Patterson’s view about speaker understanding: Ordinary speakers believe that they understand sentences and expressions of a language when, in fact, they do not, as, in a certain sense, there is nothing there to understand. If that is the view (and I believe that it is), I am not sure if it is quite right to say that Patterson’s account involves an *error theory*. As I understand it, a classic ‘error theory’ holds that some fragment of discourse—e.g., some sentences of the language—is different from other such fragments in that all of the sentences of that fragment are false. If we insist on standard labels, it is not clear that Patterson is an error theorist about understanding, since

his view is *not* that all or some of the sentences of English are actually false. Of course, he *still* attributes some sort of error; in that sense, Azzouni is clearly right.

Let's reserve 'error theory' for the classical notion and let's introduce a new term to capture the sort of error that Patterson (and, as we shall see, Azzouni) attributes to language users. Call a theory (or, more accurately, an account) according to which people (in this case, speakers) are systematically in error about the features or the functioning of some fragment of their language a *confusion theory*. And let's say that one who endorses a confusion theory is a *confusion theorist* (which is *not* to say that the theorist is in anyway confused, of course). I will claim that Azzouni and Patterson are both error theorists and confusion theorists and that, although neither necessitate the other, both theorists employ confusion theories because of their error theories.

Patterson holds that English does not manage to express what it appears to express, owing to the falsity of the semantic theory for that language. In this sense, he is an error theorist about the semantic theory for English (although it does not follow that he is not an error theorist about English itself). Patterson's error theory raises a puzzle: If the semantic theory for English is actually false, why do rational language users continue to employ that language, attempting to express what English does not, and cannot, express? Patterson's solution, which I discussed previously, is to employ a confusion theory. So, while he is a confusion theorist about the language and its users, he is an error theorist about the *semantic theory* for English. In this way, we see the relationship between Patterson's error theory and his confusion theory: The confusion theory resolves an apparent puzzle, which is the result of the error theory, together with what it appears to imply about speakers of English.

Azzouni also appears to be both an error theorist and a confusion theorist. Unlike Patterson, though, Azzouni is an error theorist about the language; he is not (or is not obviously) an error theorist about the semantic theory for that language.¹⁰ That said, like Patterson, Azzouni also faces a puzzle, which is generated from his error theory (together, again, with what it appears to imply about speakers of English). His puzzle: If English is actually trivial, why on earth do its users employ it? Azzouni's response is a confusion theory: Although the language is trivial (in the afore noted sense), competent users make as if it is consistent. They are confused about the actual features of English, but their confusion is employed in a very useful way.

Of course, we can now ask questions about whether the confusion theory is properly motivated in the way that the error theory is? One might worry, for example, if the only motivation for one's confusion theory is that it promises to resolve the puzzle that is a by-product of her error theory. I shall leave those worries aside. We can all agree that if we can find an alternative account, which explains the data that the CIT²s explain, without a commitment to error and confusion, then common sense and (a version of) Occam's Razor demand that we prefer it.¹¹ In the last section of this paper, I sketch—very briefly—such an account.¹²

§6 Default Reasoning and Semantic Psychologism

One of the points I mentioned in §3 was that the arguments that Azzouni and Patterson muster, if effective, should also have us conclude that the semantics for English is indeterminate, or that English itself is. This is important. If we began by considering

cases of indeterminacy, we might not have concluded that our semantic theory was false, or that English was trivially inconsistent; instead, we might have concluded that English, or more specifically, our theory of it, is, in a certain sense, *under-specified*.

Say that a language user who has mastery of every feature of a language is *maximally competent* and that one who has mastery of at least a sufficient chunk of the language is *adequately competent*. No language user is maximally competent; at best, we strive for adequate competency. If we allow in further distinctions (and ignore Patterson's error theory of understanding), we would conclude that no one fully understands their own language, though most users at least partially understand it. This should not be a surprise; after all, for the most part, we are born *into* a language, but not *visa versa*.

If all of this is right then one might wonder: How on earth do we arrive at linguistic conclusions (principles, say) and draw inferences from those conclusions, if no one has complete knowledge of any natural language? And, as is perhaps more pressing, given that we arrive at such conclusions and go on to draw inferences from them, are we right to do so? I will not be concerned to answer the first question but I will provide an affirmative answer to the second one. My interest here does not really regard why *we* are right to go on to arrive at these conclusions and draw inferences from them. Rather, I want to propose that a partial answer to the second question can shed light on an alternative to the CITs.

We sometimes accept claims, even though we recognise that there could be exceptions to it. So, for example, we might accept

(1) Dogs bark,

even though we recognise that not *all* dogs bark. We might conclude (1) by observing a sample of dogs, in normal, unexceptional circumstances, and observing that all of them bark. Our reasoning will not be deductively valid, yet, by *default reasoning*, we are entitled to conclude it, provided we do not have any sort of overriding information to the contrary.

In ‘default reasoning’, we draw a conclusion from evidence, even though we know (or at least strongly believe) that the evidence does not guarantee its truth. Such reasoning is said to be ‘default’, and such statements are said to be *default statements*, because we are justified in making them provided we have no information that would lead us to doubt them.^{13,14}

In general, a *default statement* is supposed to be true of a class of objects the statement describes, despite the possible existence of “exceptional instances” of the class. (Cf. *Ibid.*, p. 3). One of the central features of default reasoning is the employment of *generics*, where a generic (of which (1) is an example) talks about *genera*, and does so in a fairly generic way. Here’s how it works. If you come across Snuffy, who appears by all counts to be a normal, non-exceptional dog then, given what you believe about Snuffy and your acceptance of (1), you will draw the conclusion,

(2) Snuffy barks.

If you come to learn that Snuffy is in some way ‘exceptional’—a non-normal dog—then you will likely withdraw your conclusion. You will not, however, go on to reject or revise (1).

It bears noting that when you withdraw (2), upon learning that Snuffy is in some way exceptional, you are *not* denying that Snuffy barks, for example, claiming that she

does not bark. You withdraw your conclusion because you incorrectly assumed that Snuffy was not exceptional. This puts no pressure on (1): if Snuffy is exceptional, there is no reason to think that the status of (1) changed. Of course, if Snuffy is not exceptional and if (2) ends up being false then you will have cause either to question (1) or to question class membership for the objects that (1) purports to describe.

Let's move away from dog talk and return to truth talk. My suggestion is that the semantic principles on which Azzouni and Patterson rely admit of generic readings, and the reasoning that we use, when we draw inferences from them, goes by way of default reasoning.¹⁵ The going suggestion—an alternative to the CITs—is to opt for generic readings of the semantic (and perhaps logical) principles and, in effect, to assign those principles a sort of *quasi-valid status*.¹⁶ Just so, a generic statement like

(3) Instances of DS are true

can be true (and, in a sense valid), though the relevant generalisation,

(4) Every instance of DS is true

(or something like it, such as), is itself not true.

If the situation with liar is as Azzouni and Patterson report, an approach that endorses default reasoning and sees semantic (and/or logical) principles as generics might be appealing. More importantly, if their inconsistency theories are to be preferred to the currently floated suggestion, they will have to insist that the semantic theory for English demands that semantic principles be given a (4)-style reading, rather than a (3)-style reading. I am not sure how that argument will go but already it raises a challenge for the CITs.

One reason for favouring a reading of (3) over (4) is that the former, but not the latter, tolerates exceptions. More importantly (as Pelletier and Elio (Ibid., p. 2) suggest, though they are only concerned to motivate default and non-monotonic reasoning), if we retract a particular conclusion, say

(5) “L is not true” is true iff L is not true,

when confronted with liar reasoning, we are not thereby enjoined to retract either the original premises or the generic statement that those premises support. If anything, we retreat to (3), disavow a commitment to (5), which we may well not have accepted, anyway, and maintain that (3) holds for the *relevant*—that is, the non-troubling—cases.

Before closing, I highlight a few virtues of the account and raise a challenge for the position that I have just sketched. Let’s start with the virtues. First, a general generic account seems to comport closely with actual linguistic practice. So, if semantic psychologism—some version of it, in any case—is true, we may have a reason for favouring a view according to which our semantic theory is under-specified, while also possibly being true. Second, unlike a CIT², this position is not error-theoretic and, at least *prima facie*, it does not obviously carry a commitment to confusion. Third, if the principles of our semantic theory are default statements, they can be generally accepted with the understanding that there may well be certain ‘exceptional’ cases (e.g., liar, truth-teller, etc.). This would make sense, given the observation that the best we strive towards is adequate competence. Finally, while we might, as a matter of empirical fact, contend that the folk come to accept their semantic principles through default reasoning, when we are specifying the semantic theory for English, we can treat these principles as, in effect, generics. In this sense, a generic reading can function as a sort of regimentation.

Virtues aside, here is an obvious worry: How on earth is this going to help us with the liar? The proposal that the paradox-yielding principles are in fact default statement assumes that there is a class of sentences that the statement aims to describe. In particular, the reasoning employed assumes that there are objects—in this case, sentences—that are properly taken to be ‘exceptional’. But, as we all know, there is no algorithm for winnowing out all and only the problematic sentences. Hence, won’t we have to end up saying something about what the exception cases are? And won’t that leave us with many of the problems that standard consistentists face?

This is a familiar worry and I have no doubt that in some cases it is entirely legitimate. But simply from the fact that it is legitimate, when applied to some cases, it does not follow that it is legitimate *tout court*. For example, one charge might be that if we decree that the exceptions are all and only the sentences which, when applied to the default statement, result in inconsistency or indeterminacy then the solution will be *ad hoc*. That may be so but it raises a question: Why must we set out criteria in terms of which we can identify the exceptional cases? Why not simply acknowledge that, when we apply a principle like (3), we, who aspire towards adequate competence, are (often, always) acting in the presence of incomplete knowledge? We can then take advantage of the fact that our default reasoning allows us to retract certain conclusions—viz., those that logic and (3) generate, when we assume that a liar or truth-teller is not exceptional—without undermining, revising or in anyway affecting the principle(s) that we take to be true or correct.

The position, as I have sketched it (and I really have barely sketched it) would treat our semantic (and, perhaps logical) principles as, in effect, *quasi-valid*. A quasi-

valid principle is not, strictly speaking, valid, but it is valid-for-relevant-purposes. One might think that the liar and other allied semantic spandrels are precisely the sorts of things that are classic “exceptional” cases for the semantic principles from which they generate. Whether, in the end, this fourth position can be put to work, it poses a challenge for an advocate of the CIT².

* Thanks to Jody Azzouni, Matti Eklund and James Woodbridge for helpful discussion. Special thanks to Doug Patterson for helpful comments and for very stimulating (and ongoing) discussion.

¹ For what follows, I will let ‘CIT’ stand for either a consistent inconsistency *theorist* or a consistent inconsistency *theory*, trusting that context will serve to disambiguate the relevant senses.

² I focus on Azzouni, Eklund and Patterson. I do intend to extend my points to the interesting work that Kevin Sharp has done—but in a different paper.

³ For what follows, I shall stick with the liar, leaving Eklund’s response to the sorites for another time.

⁴ We can generate the same result given

(L+) L+ is not in the extension of ‘true’,

which Eklund will know cannot be in the extension of ‘true’.

⁵ Thanks to Eklund and Patterson for helpful discussion. Eklund tells me that Alexius Burgess has raised a similar worry. I have not had a chance to look at his work but, if he is making a similar point to the one that I am making, that is all the better for my argument (and for his, I suppose)!

⁶ Azzouni (personal communication) disagrees. He contends that empirical evidence points to inconsistency. I cannot see that this has been established, but the points that I make will go through, even if this stronger reading—to the effect that Azzouni has proved that English is inconsistent—is correct.

⁷ See Armour-Garb and Woodbridge (2006). As Woodbridge and I have shown (2006), for every putative case of inconsistency there is both a dual case of indeterminacy and a case of (what we call) higher-order indeterminacy wherein it is indeterminate whether a given n-tuple of sentences is indeterminate or inconsistent.

It is also interesting to note that, while one may complain that (T) is not pernicious in the way that a liar sentence is, revenge appears to emerge, even if we were to maintain that because nothing makes (T) true or false it is, thus, neither true nor false. To see this, consider a revenge problem for (T), viz.,

(T*) T* is not false,

which can consistently be assigned either truth or falsity. Notice that T* appears to yield paradox, if we were to ascribe it a truth-value gap.

⁸ We can strengthen the argument, given a proof that we can construct a variant of Tarski’s theorem. To do this, we enumerate the predicates, prove the diagonal lemma and show that it follows from this that, for some sentence, *k*, such that true(*k*) if, and only if *K* is true, from it follows that *k*’s truth depends on whether *k* is true.

We get a similar result if we use diagonalisation. Thanks to Doug Patterson for helpful and enjoyable discussion. I pursue these points in a paper, currently in construction.

⁹ There is a difficulty in formulating ‘semantic psychologism’ if one insists either that competent speakers have introspective (or even very good introspective) insight into the semantic rules for (if any), and (or) the meanings of, our expressions, or that the expressions of a language cannot have meanings that are

other than the ones that *competent speakers actually, consciously, take them to have*. But one can endorse semantic psychologism without accepting either of these (stronger) formulations. Thus, it is possible for one who endorses semantic psychologism merely to insist that the expressions of a language cannot have meanings that are other than the patterns of usage that competent speakers actually exemplify.

¹⁰ One might complain that Azzouni isn’t an error theorist in the ‘classic’ sense, since he thinks that the language is trivially inconsistent, in which case no fragment of it is *false only*. I grant the point but I do not see that it matters. Let a *new error theorist* contend that a fragment of discourse is *untrue*. Azzouni is thus a new error theorist, where the relevant ‘fragment’ contains every sentence of English.

¹¹ Patterson suggests that we should adopt his position on grounds that it is simpler and requires fewer logical and semantic revisions than standard solutions promise. That may be so, but it bears noting that attributions of (fairly massive) error are rarely a simple matter.

¹² Thanks to James Woodbridge for helpful discussion of a number of the points in this section.

¹³ See Pelletier and Elio (2003).

¹⁴ Default reasoning is *non-monotonic* in the sense that it is not always the case that if $\Gamma \vdash A$ then $\Gamma \cup \{B\} \vdash A$.

¹⁵ To be clear: I am not contending that we *actually* engage in default reasoning. I am, rather, suggesting that if we take the relevant semantic principles as default statements, we will have an explanation as to how the semantic principles can be valid, in spite of the appearance of paradox.

¹⁶ Thomas Hofweber (2007) has recently proposed a solution to the liar paradox according to which logical principles are treated as generics and classical logic is, *in a sense*, non-deductive. There are affinities between the position that he has put forward and I am very sympathetic to much of what he says. That said, insofar as I am concerned primarily with *semantic* principles, there may be important differences between the two positions.

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