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FACING INCONSISTENCY: THEORIES AND OUR RELATIONS TO THEM

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ABSTRACT

Classical logic is explosive in the face of contradiction, yet we find ourselves using inconsistent theories. Mark Colyvan, one of the prominent advocates of the indispensability argument for realism about mathematical objects, suggests that such use can be garnered to develop an argument for commitment to inconsistent objects and, because of that, a paraconsistent underlying logic. I argue to the contrary that it is open to a classical logician to make distinctions, also needed by the paraconsistent logician, which allow a more nuanced ranking of theories in which inconsistent theories can have different degrees of usefulness and productivity. Facing inconsistency does not force us to adopt an underlying paraconsistent logic. Moreover we will see that the argument to best explanation deployed by Colyvan in this context is unsuccessful. I suggest that Quinean approach which Colyvan champions will not lead to the revolutionary doctrines Colyvan endorses.

INTRODUCTION

We begin with a puzzling phenomenon. Both unknowingly and knowingly we have used inconsistent theories. Why is this puzzling? Well, according to the classical conception of logical consequence, each and every inconsistent collection of sentences has the same collection of consequences: the collection of all sentences. Considered as a theory, the set of all sentences can be regarded as a theory without discrimination: it fails to discriminate between any sentence and its negation by including them all. Not without reason have logicians called the collection of all sentences 'the absurd theory'. Classical logic is not, however, unique in reducing inconsistent theories to absurdity. Any logic which admits as valid the argument form *ex falso quod libet*, that from a contradiction any sentence follows, shares that property with classical logic; as does, for example, intuitionistic logic. Yet here is a phenomenon not to be ignored: not all inconsistent theories are alike in usefulness and productivity. Some inconsistent theories are useful. Moreover in actual cases where researchers discovered that a theory was inconsistent, they did not, as licensed by *ex falso quod libet*, immediately start inferring all manner of conclusions from the theory, even while they kept using that theory then recognised as inconsistent. To what, then, are we implicitly committed by accepting an inconsistent theory? Some have suggested the solution to this puzzle is that classical logic fails to capture the underlying logic of inquiry and that we get a better purchase on our use of theories and inconsistent theories

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in particular by using a paraconsistent logic.[1] I shall suggest that this train of thought is mistaken. Classical logic, I shall suggest, gives the best account of the logic of our sentences, and even for those within inconsistent theories. I shall further suggest that the important difference between conceiving of logic as a theory of inference and as an account of consequence makes many of the arguments for paraconsistent logics otiose.

PARACONSISTENT MODESTY AND CLASSICAL PERMISSIVENESS

Classical logic can be regarded as permissive. This leads to what Graham Priest has called its clunkiness. Mark Colyvan (2008a) suggests that the permissiveness of classical logic sits ill with the actual inferential practices of those who use inconsistent theories. None of those use their inconsistent theories to infer merely arbitrary and irrelevant sentences even though, according to classical logic, such inferences are valid. From this observation Colyvan infers the logic underlying scientific practice cannot be classical and must perforce be a paraconsistent logic, a logic which rejects *ex falso quod libet*.

Colyvan is surely right about a number of issues here. Not all inconsistent theories are equally useless. Further, no-one is tempted to infer arbitrary sentences from their theory once they discover it is inconsistent even when they still use the theory. But is the explanation for these observations the one Colyvan offers? I shall argue that it is not. There is a much more plausible explanation from which we stand to learn quite a bit about the nature of theories, our relationships to them and something quite surprising about the notion of verisimilitude.

I shall show that Colyvan's solution of invoking a paraconsistent underlying logic is mistaken. I shall first show that in his discussion of the indispensability of objects he has confused the indispensability of a theory with the indispensability of an object to a theory. Then I shall argue that the ontological commitments of a theory are the logical consequences of that theory which are of existential form. But it is a further question which theory we ought to be committed to; our ontological commitments are the ontological commitments of the theories we believe.

I then further address the question of whether we can use theories without believing them, for this would be a way of avoiding the ontological commitments of those theories. I shall address that question by reference to Quine's work on theories and our relationship to them. We will see how the key notion of the home theory plays a central explanatory role in Quine's thinking on this matter and how that gives us the resources to chart different sorts of relationship we can stand in to theories. In all this, the notion of a theory itself needs some discussion. It is interesting that, to make some of the distinctions I want to make, particular understandings of 'theory' are called for. Once this is in place we can also shed light on the notion of verisimilitude and the ways in which we develop our theories and adopt cautious attitudes to theories even while we use them.

1 See Priest (1987), Priest and Tanaka (2009). There are many other paraconsistent approaches but what they share with Priest is the view that classical logic cannot accommodate non-trivial inconsistent theories. See for example, Newton Da Costa *et al* (2003, 2005) and many of the papers collected in Meheus (2002). It is this view that I aim to show to be mistaken.

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INDISPENSABILITY AND THEORIES

Colyvan has prominently argued for the indispensability of mathematical objects (Colyvan, 2001). He has gone on to explicitly align his later argument for inconsistent theories and objects with the indispensability argument for mathematical objects. Colyvan (2008a) argues that inconsistent theories and inconsistent objects are indispensable so that we ought to take on commitments to such theories and objects:

> It is not too hard to see how a Quinean argument for inconsistent mathematical objects would go. It would proceed in much the same way as the regular one for realism about mathematical objects: given the indispensability of calculus in the 18th century, 18th century metaphysicians (if not scientists) ought to have believed in the existence of inconsistent objects, namely infinitesimals. The case for belief in other contradictory objects, such as inconsistent spiral galaxies and oceans … is much the same. (Colyvan 2008a: 119)

This second and more radical argument, I shall argue, fails. First, it involves a confusion about what is indispensable to what. Second, that mistake is compounded by a mistake about the epistemic obligations to which we are subject.

The key to appraising this argument for inconsistent objects is to consider these sorts of indispensability arguments. Colyvan (2008a: 119) presents the arguments as follows:

> The crucial premise of such indispensability arguments is that we ought to be ontologically committed to all and only the entities indispensable to our best scientific theories. … I take an entity to be indispensable to a theory T if there is not a competing theory that is as least as good as T and which does without the entity in question. (Colyvan, 2001, p. 76–78 [his reference]) … Moreover being the best theory is simply being better (in this sense) than all extant competitors.

I will argue that the crucial premise is actually false. The premise says that we ought to be ontologically committed to all and only the entities indispensable to our best scientific theories. There are also questions about what 'indispensable to our best scientific theories' might mean. To show that the crucial premise is false it would suffice to see that we need not be ontologically committed to an entity which is indispensable to the best extant scientific theory for the reason that we need not be committed to that theory.

Let us briefly set aside these questions about indispensability and ask whether the seemingly more innocuous and analogous claim 'We ought to be ontologically committed to all and only the entities which are the ontological commitments of our best scientific theories' should be regarded as true. Here the indispensability has been sidestepped but it is clear that if we need not be ontologically committed to the ontological commitments of our best scientific theories then neither need we be ontologically committed to those objects indispensable to our best extant scientific theories. The claim that we should be ontologically committed to the ontological commitments of our best scientific theories would follow most naturally if we ought to believe our best scientific theories.

While this sounds obviously true, the claim that we ought to believe our best scientific theory is false. Our best scientific theories after all are, as Colyvan insists, just the best of the current bunch of theories. There is no reason to think they must have the virtues which demand our belief. Even on a very permissive account of rationality which allows that we may rationally believe such theories, we would lack a compelling reason to require belief in such theories. Why am I being irrational if I fail to believe the best current scientific

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theory on some topic? It is mistaken to say that I ought to believe a poor theory which happens to be the best of the current bunch. I might choose to and justify my choice by saying it is the best theory on offer but the question is whether I am compelled to accept a theory because it is the best of the extant theories. It is clear that I am not.

For example, I might be convinced that we have a biological evolutionary history and yet doubt that any current theory of the evolutionary origin of our mathematical ability is credible. Even the best of these, I might think, is mere speculation. I am simply not rationally obliged to believe the best current theory. However, it is clear that Colyvan cannot weaken his claim to being one of mere permissibility to accept the best theory on offer since such a weakening will not allow him to conclude that an 18th century metaphysician ought to have believed in inconsistent mathematical objects. At best, this makes it permissible to believe this theory. Further, we will see that even the claim of permissibility can be undermined if the best theory is impossible.

Thus we see that the thesis that we ought to be ontologically committed to the ontological commitments of our best theory is false since we may not believe such theories, we turn our attention to the key notion of indispensability. There is a series of problems in Colyvan's definition of this key notion. Colyvan (2008a: 119) says:

> I take an entity to be indispensable to a theory T if there is not a competing theory that is at least as good as T and which does without the entity as question.[2]

What is it for a theory to do without an entity? This sounds as though doing without an entity is simply dispensability, that is, that the theory can do without the entity just when an entity is dispensable to a theory. But then the definition of indispensability would become:

> An entity is indispensable to a theory T if there is not a competing theory that is at least as good as T for which that entity is dispensable.

The problem with this formulation is that it is viciously circular. If we are to understand what it is for an entity to be indispensable it turns out that we need to already understand what it is for that entity to be dispensable to the theory.

Suppose we could get past the worry about circularity in his formulation. There is a further oddity. The definition makes whether an entity is indispensable to a theory T depend on what other theories are in the environment. This is quite unlike a theory's ontological commitments. A theory T has an ontological commitment to an entity *e* just when that theory entails that *e* exists.[3] A theory's entailments are not affected by what theories are also in the environment. So a theory's ontological commitments are not affected by the theories in the environment.

The theory of phlogiston does not stop having an ontological commitment to phlogiston when the competing theory that there is oxygen comes on the scene. Nor does phlogiston stop being indispensable to the theory that entails the existence of phlogiston when a competing and better theory is discovered. Rather it is the phlogiston theory itself that is no longer indispensable. Since we the users of theories no longer need to have any

2 I have corrected a typographical error in the printed version of Colyvan's paper in the quote.

3 See e.g. Quine (1948), Jackson (1998), Michael (2008).

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commitment to the theory, we no longer have any commitment to the existence of phlogiston. Again, theories do not change their ontological commitments; agents change their ontological commitments by changing their commitments to theories. Agents incur ontological commitments by dint of their being committed to theories with ontological commitments.

In Colyvan's presentation there seems to be an attempt to distinguish between an entity being an ontological commitment of a theory and an entity being indispensable to a theory. This is an invidious contrast. An entity's existence is indispensable to a theory if that theory cannot be true if that entity does not exist. In other words, an entity is indispensable to a theory just in case it is an ontological commitment of that theory. However whether an entity is an ontological commitment of a theory is independent of whether or not that theory is the best theory around. Whether an entity is an ontological commitment of a theory depends on whether the sentence expressing that entity's existence is a logical consequence of that theory. That does not depend on whether there are any better competing theories.

The explanation for the confusion in Colyvan's discussion of indispensability seems to be that he has simply confused the indispensability of an object for a theory (ontological commitment) with the indispensability of a theory to an inquirer. This would explain why he thinks other theories in the environment are pertinent. But these are clearly two different issues here. A theory does or does not have an ontological commitment and that is settled by what the theory logically entails, quite independently of what other theories are in the environment. A person gets to be ontologically committed to objects by believing a theory which has that ontological commitment. The question of which theories we ought to believe is entirely separate. Colyvan's crucial premise which I have argued seems to require the thesis that we ought to believe our best extant scientific theories, would provide an answer to that question; however it is an answer we have seen to be false.

On Colyvan's characterisation there is an easy way to avoid commitments: choose to be committed to not so good theories. For example, if I am committed to phlogiston theory, that theory entails the existence of phlogiston. However since there is an alternative to my theory which does without phlogiston, I can assert that although my theory entails the existence of phlogiston, it is not indispensable to my theory since there is another extant theory about which does at least as good and yet does without phlogiston. The thing to notice is that according to Colyvan's criterion, once oxygen theory is around, my theory, i.e. phlogiston theory, no longer has phlogiston as indispensable to it. So my theory gains a virtue. It explains burning while not depending on phlogiston, in the sense that phlogiston is not indispensable to phlogiston theory. This is clearly absurd.

There are two questions which Colyvan does not clearly distinguish. First, what objects are indispensable to a theory? To which we say, its ontological commitments. And, second, which theories ought we believe? We have not answered this question but we have rejected Colyvan's answer, required by his crucial premise that we ought to believe our best scientific theories. We have seen that however plausible it may seem that we ought to believe our best scientific theories, recognising a theory as the best available does not rationally commit us to adopting it and thus adopting its ontological commitments.

We now move to address what a broadly Quinean approach to metaphysics, and ontological commitment in particular, commits us to and what sorts of commitments come with the uses we make of theories.

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METAPHYSICS AS HOUSEKEEPING IN THE HOME THEORY

According to a broadly Quinean conception of things, a conception Colyvan and I share, metaphysics is not first philosophy; it is last science. We figure out our theory of the world first and then at the end of the day discover our metaphysical commitments by looking at what our theory says there is. There is the business of neatening up our theory and, if we like, we can dignify that activity by the honorific 'metaphysics'. However the real work is figuring out what we think the world is like. The Home Theory is our theory of the world in terms of which we make sense of everything else. Metaphysics becomes an immanent activity: Housekeeping in the Home Theory.

For Quine, the question of a theory's ontology is a question about what the theory says there is, and that will be a subset of what the theory says in general. An explicit statement of a theory has logical consequences which are its implicit commitments, among which are its implicit ontological commitments.

Quine's own version of this endeavour of discerning a theory's commitments involved his view that the theory be expressed in a regimented first order and extensional language, and that the logical consequences are those derivable by first-order logic. Neither of these two constraints is essential to an immanent understanding of ontological commitment and we can see Quine's particular views as one of a family of views which are broadly in keeping with his views about ontology as immanent to a theory. Another particular view in that family is espoused by David Lewis (1973) and Frank Jackson (1989, 1998). They differ from Quine in their rejection of a strictly formal notion of consequence, favouring instead the relation of necessitation. Moreover they did not put first order and extensional constraints on the language the theory was expressed in.

My own views differ from both Quine's strict formal consequence and also the necessitation relation used by Jackson and Lewis since I insist on the key role played by the notion of *a priori* consequence (Michael, 2006, 2008). After all, commitments must be rationally accessible or else failing to recognise them cannot be thought of as a rational failure. Notice though, these are three sharpenings of the same view: the commitments of a theory are its logical consequences. Only this more general view is required for this paper.

Indispensability and ontological commitment

As Colyvan tells the story, a Quinean ought to be embarrassed by the use of idealisations, a phenomenon no-one actually seems troubled by.

> Various idealisations in science, when seen in the right light, deliberately introduce contradictory assumptions. Maddy (1992) presents a nice example of the assumption of infinitely deep oceans and how this assumption is required by the theory of waves on the open ocean. Maddy points out that the assumption of infinite depth raises problems for Quinean realists because this assumption would seem to be indispensable to our best theory of ocean waves. By Quine's line of reasoning, it would seem that we ought to believe in infinitely deep oceans. So what we are really dealing with here is a contradiction between two pieces of theory. Taking the conjunction of the two pieces of theory, we have it that oceans are both infinitely deep and not infinitely deep. (Colyvan, 2008a: 117)

It should be noted that the idealisation, even for Colyvan, is not something which is itself inconsistent but rather the contradiction is supposed to be a feature of total theory. For this inconsistency to raise a problem at all, we need to be confident that all bits of the

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total theory take their place in that totality in the same way. But this is implausible. Is any theorist committed to the thesis that the ocean is infinitely deep? Even theorists who use the assumption will be quick to explain that they don't actually think that the ocean is infinitely deep but that it simplifies their equations: that way you don't get the reflections of waves bouncing off the bottom complicating the wave function. Using a simplifying assumption like this is innocent if the theorist doesn't believe it but also has good reason to believe that the real values and the calculated values using the assumption are relatively close. Had we thought that that false assumption would have markedly changed the result we reached, we would not have made that assumption.

The fact that we use a theory which treats planets as extensionless points of various masses, does not mean that we are wholeheartedly committed to or even seriously open to the prospect that we should discover that the planets are extensionless points. We use such a theory to calculate various physical features of planets we strongly believe are not extensionless points. Contradiction would only threaten if we took the same attitude to the theory which treats the planets as extensionless points as we do to the theory that they are not extensionless but extended. Since we do not, we are not lumbered with an inconsistent theory. Using idealisations like this need not lead to a commitment to inconsistent theories. For that argument to work, we would need to accept that any use of a theory as an idealisation is associated with believing the theory. But this is implausible.

What should a Quinean say about theorists who use idealisations? The intuitive thing to say, and what people using the idealisations tend to say about themselves, is that the idealisations are false, but that as a calculation device they get an answer which is close to the truth. There is no reason to think that the Quinean is barred from saying just that. It is Colyvan and Maddy who would give the theorists commitments they themselves would not recognise. However, the real commitment the theorist takes on is not that the world is as the idealisation represents it, but rather that the idealisation can be used to calculate values relevantly similar to the real world values. Not every theory the Quinean uses has to be thought of as something the Quinean is committed to. Maddy, and after her, Colyvan, seem to ignore this important point.[4]

There is no problem here, even for the hard-nosed Quineans amongst us. Quineans do not need to treat all theories used as part of the home theory. The gold standard of understanding is translation into the home theory. Yet we can use discourse which is not readily translated into the home theory. Quine himself talks about the way intentional discourse is ineliminable and useful though not part of his home theory which is an austere desert landscape of physical objects and their behaviour:

> Not that I would forswear the daily use of intentional idioms, or maintain that they are practically dispensable. (Quine, 1960: 221)

The centrality of the home theory seems not to be emphasised by many people.[5] However, it was important to Quine.

4 The point can be generalised. David Hilbert famously denied that universally quantified sentences were true. Yet he accepted that such sentences played an important role in facilitating the derivation of genuinely true sentences from genuinely true sentences. Hilbert's position shows us that not every sentence used in a moving from truths to truths needs to be regarded as true or even as having a truth value.

5 A notable exception is Harman (2011).

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> In practice, of course, we end the regress of background languages, in discussions of reference, by acquiescing in our mother tongue. (Quine 1968: 201)

For this reason it is

> meaningless to ask whether, in general, our terms 'rabbit', 'rabbit part', 'number', etc. really refer respectively to rabbits, rabbit parts, numbers, etc., rather than to some ingeniously permuted denotations. It is meaningless to ask this absolutely; we can meaningfully ask it only relative to some background language. (Quine 1968: 200)

It is this which allows us to also make sense of understanding and interpretation as translation into the home theory. The further question of making sense of the home theory just makes no sense.

There are, however, other activities we can engage in which remain sensible. For example, there are questions of just which of all our commitments we take to be in the home theory and so take to be the commitments in terms of which we make sense of everything else. This is a pragmatic matter just as everything is but it is simply redescribing things to say that here we are doing metaphysics. The role of the home theory is to be that in terms of which we make sense of everything else. Suppose ordinary objects and people are in the home theory. The idea is not that we have a reductive programme which shows us how to reduce talk of atoms, space-time, species and nations into talk about people and chairs. After all, I accept that people are made up of atoms and that therefore atoms are metaphysically more fundamental. However I am more likely to give up the theory that there are atoms than the theory that there are (or have been) people. Centrality in my commitments is not a matter of metaphysical fundamentality.[6]

From a Quinean point of view, what attitudes do we take to and how do we use theories? From Colyvan's discussion we get the suggestion of a very simple way we relate to theories. It is as though whether we *use* a theory is the criterion for whether we take on the ontological commitments of the theory. This explains why, according to Colyvan, the use of idealisations brings with it a commitment to inconsistent objects: believing a theory and taking on such commitments, both seem to be of a piece with using the theory. However, from a Quinean point of view, this is certainly not so. Not all theories we believe, let alone all theories we use, are part of the home theory. We can use theories of which we are not convinced but which we think might be true. And we can use theories which we are convinced are false. The second case is demonstrated by our use of idealisations.

Using a theory is not sufficient for that theory to be in the home theory. Moreover, accepting that the theory is the best of extant competitors does not oblige me to either use that theory or to make that theory a basis for future research, let alone believe that theory.

The lesson here is that Colyvan is too quick to embrace contradictions. He misses the significance of the various relations we stand in to our theories. This is odd for someone who thinks of themselves as following in Quine's footsteps. Quine made important and

6 On the story given, Schaffer (2008) is mistaken in treating the notion of fundamentality as a distinct story about ontological commitment augmenting a 'quantificational account' of ontological commitment. Rather metaphysical fundamentality is part of the articulation of our theory of the world, an articulation which does not march in step with notions of centrality, as we have seen above.

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significant progress in our understanding of an immanentised ontology. We take on ontological commitments by dint of our belief in, that is, our commitment to, theories. We have these ontological commitments even when our theories are not the best theories about. We discard the ontological commitments by discarding commitment to the theories that carry the commitments. A theory has an ontological commitment to an entity if that theory logically entails the existence of that entity.

Separate is the question of when we should take on a commitment to a theory. Colyvan suggests that we should do so when it is the best extant theory which does some work we want done. I have suggested that this might be true in some cases but there are too many cases where we don't have any reason to believe that the best extant theory is good enough. Choose your favourite area of speculative science as an example here. Moreover, not all theories we use are used in the same way. Some we use as calculation devices, knowing full well that they are false and include false assumptions but which we believe yield an answer close enough to the truth. Some we use and have some doubts about, doubts which further work might assuage. In the latter case these are not theories which are in the home theory, but may stand in some problematic relation to the home theory.

On the usefulness of some inconsistent theories

A key argument presented by Colyvan in favour of a paraconsistent underlying logic is that we use inconsistent theories and that some of them are useful. We have already seen that using a theory and being committed to it are not as closely related as Colyvan seems to assume. We might nevertheless find that he is right that in using a theory which is inconsistent, classical logic is too 'clunky' in that it is unable to distinguish among inconsistent theories, treating them all as trivially absurd. It is to this question I now turn.

Colyvan's suggestion that we need to appeal to a paraconsistent underlying logic to make sense of the way we use inconsistent theories can only be sustained if classical logic is unable to explain the uses to which we put inconsistent theories. Is it true that from a classical point of view all inconsistent theories are trivial and useless? I shall argue that is quite mistaken even though this suggestion is a common one among paraconsistent theorists.

> A most telling reason for paraconsistent logic is the fact that there are theories which are inconsistent but non-trivial. Once we admit the existence of such theories, their underlying logics must be paraconsistent. (Priest and Tanaka 2009: sect 2.1)

There are two senses of triviality here run together: There is the sense in which a theory is not useful and there is the sense in which a theory entails the absurd theory. These two senses are quite different and only if we thought that the only use that we make of theories is to derive consequences would we be tempted to follow this all too common argument deployed by paraconsistentists. Certainly if a theory just were a logically closed set of sentences then all inconsistent theories would indeed be trivial, consisting of the set of all sentences. However, there are different things we can mean by 'theory'.

We can think of theories as

1. A logically closed set of sentences.
2. A set of axioms.

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3. A set of models, situations, worlds.
4. A procedure for determining a set of models, situations, worlds.

(1) and (2) are syntactic construals of theories. (2) and (4) are semantic construals. And of course I am not covering all possibilities here but simply indicating just some of the many different ways we have of thinking of theories. (1) and (3) are such that any two logically equivalent theories will be given the same representation. (2) and (4) however, allow for logically equivalent theories to be represented by distinct objects. For example, that a set s of axioms has logical closure S does not preclude that a distinct set of axioms s* has the same logical closure. Similarly, that a procedure p determines a set of worlds W does not preclude that a distinct procedure p* determines W as well. By contrast when we are thinking of theories by (1) and (3) we are unable to distinguish logically equivalent theories. They just are the same by that standard. Thinking of theories as (1) does not mean that we are thinking of them as axiomatisable. We know that there are logically closed sets which are not effectively axiomatisable.[7]

However when we look at a theory's usefulness, its simplicity, etc., we are not looking at logically closed sets of sentences but rather something more like (2) a set of axioms.

It is interesting that this specification of what we mean by 'theory' makes available distinctions which make good sense in our dealings with theories but which are not available from certain other but influential specifications. For instance, from (2) we can, by being explicit about the logical principles we adhere to, further specify immediate consequences of a theory. Just as the sets of axioms s and s* can be distinct even though their logical closure is the same, so can their immediate consequences I(s) and I(s*) be distinct.

Notice that the distinctions made so far in this section have not at all traded on which logic is in question. Whichever the logic is we are treating as underlying, we will need to distinguish between axiom sets and the possibility that distinct axiom sets have the same logical closure. Further, once we make that distinction we can introduce the notion of immediate consequence on the basis of explicit rules of proof. The paraconsistentist also needs to make this distinction. That such a distinction is necessary is all the classical logician needs to explain how false and even inconsistent theories can be distinguished and so how some among them can be useful.

By treating theories as sets of axioms we can talk about one set of axioms being simpler than another even when they determine the same set as a logical closure. It can also be that one set of axioms is more similar to a true set of axioms, than another which determines the same logical closure. How? A set of axioms can show similarity to what we think are the true set of axioms by using the same concepts interacting in the same sorts of ways. We might find reason to use the inconsistent set of axioms because it is easier to use than the true set of axioms. One such example might be the way we have a notion of membership in the naïve theory of sets which is like the notion we find in the presumably consistent and true theory of Zermelo-Fraenkel (with Choice) set theory. Yet the axioms of ZFC set theory obey include caveats and complications not present in naïve set theory.

The shape of the axioms and the way the key notions occur in them seems to be important in making a false simple theory useful, but there are other ways. If the result of the application

7 By analogy not all sets of worlds can be determined by an effective procedure. I will focus on the syntactic characterisation for definiteness. Nothing substantial depends on this decision.

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of the simple theory is for a range of cases to be close enough to the result which the use of the true theory would deliver, then that theory can work as an idealisation. It is this way that Newton's theory is still useful and also the way that Ptolemy's charts can be usefully deployed.

Notice we now have the idea that two logically equivalent theories may differ in how close to the truth they are. Sometimes the similarity of the axioms to the true set of axioms is sufficient to make a set of axioms worth working on, even when there is another logically equivalent set which is quite different from the truth. This distinction is simply not available when we choose to look at theories only from the perspective of their logical closure. That is a legitimate perspective but it misses some of the key aspects of the way we relate to theories. What makes a theory worth working with will sometimes be how fruitful it is for development along lines suggested by its form and whether we have some confidence will end up with a viable true theory.

Two views of logic: Theory of Commitment or Theory of Inference?

> We may agree that in some sense it is the business of logic to tell us what we ought to think; but the interpretation of this statement raises considerable difficulties. (Ramsey 1926: 80)

Asking why mathematicians didn't claim a proof of Fermat's last theorem by inferring it from the contradiction in naïve set theory (Colyvan 2008b: 24) makes two significant mistakes. The first is to confuse validity with soundness. For a classical logician the discovery that a theory is inconsistent shows that that theory is false. So while the argument from the theory to the theorem is valid it is not sound. Second and very importantly it treats logic as a theory of inference. This is an important mistake. Logic is not the theory of inference.[8] Logic does not tell us how agents should *change* their commitments. On the contrary, logic gives us a theory of commitment; it tells us what by dint of their explicit commitments agents are implicitly committed to. The difference is vast. Logic is not concerned with the dynamics of commitments but rather their statics.

Suppose that an agent is explicitly committed to a theory T and finds that a certain sentence P follows from that theory. That is, suppose they discover that one of their implicit commitments is P, an implicit commitment they incur by dint of their explicit commitment to T. Should they update their explicit commitments to include P? Not necessarily. They may choose to change their commitment to the theory T instead particularly if P is for some reason unattractive. What the validity of the argument from the theory T to P shows is that their present commitments of being committed to T but not to P are unstable. Rather than seeing the implicit commitment to P as a reason to adopt P, the agent might treat this as a reason to revise their commitment to T.

Indeed seeing that we have a contradiction in our theory is not like seeing that the theory is less elegant than we had hoped. It is a sign that the theory is flat out false. Far from showing a lack of acceptance of the validity of the argument from T to P, the refusal to infer P from T is just what we would expect from a rational agent who uses the validity of that argument as a reason to reject T. Similarly, that agents do not infer arbitrary sentences from their explicit commitment to a theory which has turned out to be inconsistent is no reason to see them as using a non-classical logic. Their attitude to their theory is no

8 See Harman (1986), especially chapters 1 and 2.

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longer wholehearted. More generally if logic is a theory of commitment rather than a theory of inference we should not expect to see too immediate a connection between patterns of updating of explicit commitments and the underlying logic.

An agent whose explicit commitments are

T, If T then P.

As a theory of commitment, logic dictates that this agent has an implicit commitment to P. However, logic does not force them to infer P, either in the sense of actually making them accept P or in the sense of putting a rational obligation on them to accept P. They may, after all, reject either explicit commitment or both. Logic leaves open these changes to explicit commitments. Logic is not a theory of inference.

So far I have shown that a refusal to draw arbitrary consequences from a contradiction does not show that the agent is using a form of non-classical reasoning. Such a refusal is just what we would expect from a rational agent using an underlying classical logic and who is updating her commitments rationally. It is because no contradictions are true that finding out my theory is inconsistent means I will not draw just any consequence from it.

However if I am using a theory, for which I have no replacement and that theory is now, I see, inconsistent, I have a problem. This is the position Frege found himself when Russell wrote to him about his discovery of the paradox in his formal arithmetic couched in second-order logic in his *Grundgesetze der Arithmetik*. In Jean van Heijenoort's book *From Frege to Gödel* are collected the remarkable exchange of letters revealing Frege's response to Russell's discovery of the contradiction. This is intriguing enough but there is also a short letter from Russell to van Heijenoort sixty years later recalling Frege's response. Russell says in 1962 'As I think about acts of integrity and grace, I realise that there is nothing in my knowledge to compare with Frege's dedication to truth.' (van Heijenoort 1967: 127) Frege acknowledged Russell's discovery of a contradiction. At the time of responding to the letter bearing the news, Frege seemed to think, or at least to hope, that he would have found a response he could publish with the second volume of the *Grundgesetze* which was to be published shortly thereafter. Frege writes in 1902 'I shall no doubt have to add an appendix in which your discovery is taken into account. If only I already had the right point of view for that!' We know now that responding to Russell's paradox caused Frege years of concerted effort and mental anguish.

The discovery of a contradiction in our theory is most often treated as a problem with the way we have formulated our views, just as Frege's response suggests. The hope seems to be that some way of avoiding that contradiction is possible. We will look for a reformulation of the theory which is not subject to the contradiction yet delivers the theory's explanatory power. And this does not just arise in the far reaches of formal arithmetic. Ordinary philosophers and ordinary folk in general when confronted with a contradiction in their views think that there will be some way through this which preserves the central commitments and shows where their views have been misstated. Since their views are revealed to be misstated, they cannot hold them with the unreserved confidence they may have had. Suspicion may fall on some commitments more centrally than others but suspicion nevertheless there is.[9]

9 The charge Aristophanes levels in *The Clouds* at Socrates as a 'logic chopper' shows that the idea that contradictions are often due to superficial problems in formulation of a view rather than its substance

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Frege's cognitive situation is the same as those who are waiting for a bus after having been in a philosophy class discussing Zeno's paradoxes without finding a convincing solution to those paradoxes. Such a person has two commitments which are in tension, or flat contradiction: The bus can never reach the stop and the bus can reach the stop. Confronted with the argument the bus can never reach the stop, one can imagine a student regarding it a strong and as yet unanswered argument. Such a person might rationally hold that there is some answer to the argument which they are not yet in a position to identify. They have a reason to think that the argument must be fallacious; after all, the bus does reach the bus stop. That means that the person is committed to the existence of a solution to Zeno's paradox, even though they do not at the moment have such a solution.

Such a situation is not rare in philosophy. Don't most of us feel that way about many challenges our commitments confront, for example, the paradoxes about truth? Don't we think that the paradoxes are important and yet feel that there must be some way through them to legitimate a coherent and consistent use of the truth predicate, even though most of us do not even have a clear and settled view about whether truth should be taken as a predicate of interpreted sentences, of propositions, of beliefs, or of assertions?

In this sort of cognitive situation we see that we are not at an equilibrium point. Our commitment to the total theory we espouse is not wholehearted. We might be more committed to some parts than to others and we have good reasons to see that the whole is mistaken.

Verisimilitude and theories

Some of the different understandings of what we mean by 'theory' allowed us to explain how two inconsistent theories can have the same logical closure and yet differ in usefulness and productivity. If the absurd theory and the naive theory of sets differ in usefulness, that cannot be because they have different consequences. Indeed they have the very same logical closure. However, they do differ in usefulness. Arguably the naive theory of sets is not too dissimilar from many seemingly consistent set theories, such as Zermelo–Fraenkel (with the axiom of Choice) or von Neumann–Bernays–Gödel set theory. While it does differ from each, it nevertheless has a resemblance and a greater one than either does to the absurd theory. Another clear example of differing closeness to truth of impossible falsehoods is provided by the following two sentences:

$$217 + 568 = 783$$

$$217 + 568 = 126,124,541,132$$

It seems very clear that one of these is closer to the truth than the other. Yet they are both necessarily and *a priori* false. Indeed each together with the axioms of arithmetic will allow the derivation of all sentences.

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and that a fixed interest in such contradictions is a mark of a superficial thinker is not a new complaint but one non-philosophers have perennially raised.

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The notion of verisimilitude, of how close to the truth a theory is, is sensitive to features of theories more fine-grained than the conception of theories as logically closed sets of sentences can sustain. Rather than a black mark on the copy book of verisimilitude or even on the copy book of this conception of theories, this should be seen as reflecting what we are interested in when we talk about likeness to truth.

Sometimes, although we recognise that a theory is false, we nevertheless believe that it is something like the truth. Even if the theory is not true, it at least has verisimilitude. Because it is like the truth such a theory is useful to us. It may be closer to the truth than other theories in the area we have considered, for example. It may point the way to the truth. But what does this claim that this theory is like the truth mean? Verisimilitude figures prominently in Karl Popper's (1959) account of science and he offered a famously unsuccessful formal account. It is not, however, a new notion. It was used by Cicero in his attenuated version of scepticism. '[T]he sole object of our discussions is by arguing on both sides to draw out and give shape to some result that may be either true or the nearest possible approximation to the truth.' in H. Rackham's translation.[10]

Today there is a small industry of attempts to develop metrics which arguably capture some sharpening of the notion of being like the truth starting with Tichý (1974, 1976). See also Niiniluoto (1977, 1978a, 1978b, 1979). A sophisticated defence of these is Graham Oddie (1986). Even he however supposes that a relation of verisimilitude on theories should be invariant to axiomatisation and translation. In part this is because Oddie is responding to the arguments of David Miller (1972, 1974, 1975, 1976) that any theory of verisimilitude which uses the notion of likeness will not be translation invariant. If the arguments above are correct then, even if Miller is correct, this is not a problem for verisimilitude but rather what we would expect. Two theories may have the same logical closure and yet one is closer to the truth than the other. It is interesting that Tichý (1974) who initiated the likeness approach to verisimilitude and in particular treated that as a relation among sets of worlds, in fact explained that relation in terms of relations of the worlds' syntactic surrogates.

Rather than offering an account of verisimilitude, I suggest there is no one thing we mean by the phrase 'is like the truth' since, for one thing, there is no one notion of theory as that which stands as the relatum of that similarity relation. This is not trivial. It is a significant claim that whether a theory has verisimilitude depends on which understanding of 'theory' we are using and in different contexts we use different understandings, and appropriately so. For example, sometimes we mean by 'being like the truth' that the worlds characterised by the theory are like the actual world. This is a relation between worlds themselves. On the other hand there is the idea that the formulation of the theory is somewhat like a formulation which is true. This is a syntactic similarity. Perhaps one formulation has the right terms, or it has the terms articulating in more or less the right manner. Being similar to the true theory in that respect can be very useful for further development our theories. After all, it is formulations of theories we write down. So as we move through different formulations of our theories, to find a formulation which suggests other more accurate formulations is very useful. It can happen that a consistent theory is less useful in this regard than an inconsistent theory. In this way an inconsistent

10 Cicero, *Academica*, II, 7–8, see also II, 66; see Rackham translation (1933).

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theory can be more like the truth than a consistent theory: its formulation is more like the true theory in this area than is some other salient formulation of a consistent theory. Let us be clear that there is no reason to think that the characterisation in terms of worlds should march in step with the characterisation in terms of syntax and there is no reason to think that just one of these notions is what we have in mind when we talk about a theory being like the truth. And although I have focused on just two of the many notions of theory to make this point, it should be clear that the same point can arise with any of the distinct notions of theory. If the notion is distinct then there are the various things we can mean by saying that theories (so understood) are like the truth.

CONCLUSION

Colyvan argued from the usefulness of certain inconsistent theories to the conclusion that investigators who make use of such theories have the ontological commitments of those inconsistent theories and since such investigators do not infer just any sentence from those inconsistent theories they must be using a paraconsistent logic. I have argued that there is no compelling argument to accept inconsistent objects along these lines. I have first argued that we saw that using a theory does not mean you believe the theory. This is most obvious with idealisations. Contrary to Colyvan's suggestions, the use of idealisations is common and those who use them are perfectly able to say that they are using idealisations, that is, false theories the use of which delivers results relevantly similar to that which would be delivered by the true theory. 'Relevantly similar' is a pragmatic matter. This makes it clear that the belief of the person using the idealisation is not that the idealisation is true. This can be garnered to sketch a notion of verisimilitude which is sensitive to presentation of a theory.

Second, the fact that someone does not make a certain inference is no indication of their underlying logic. Logic governs commitment not inference. The commitments of an inconsistent theory include every sentence. As a consequence, inconsistent theories have an utterly indiscriminate ontology. That follows from the classical notion of logical consequence. It explains our reluctance to fully accept theories which imply contradictions. But we can and do use theories we know to be substantially false and we use theories which are inconsistent, as idealisations or as theories which are relevantly like the truth. In both cases we are using theories which we do not fully believe but which we believe used in this way yield results relevantly like those yielded by the truth. We are not logic engines. Logic is a theory not of inference but rather of commitment.

We have seen how even a classical logician can distinguish between inconsistent theories and say that some are closer to the truth than others. Indeed an inconsistent theory can be closer to the truth than a merely false theory if the merely false theory is off on the wrong track and the inconsistent theory uses the right concepts and even aligns them in more or less the right way but slips into inconsistency by not making key restrictions, for instance. Notice none of this depends on changing our conception of logical consequence.

Classical logic might be clunky, as Graham Priest once said, but it allows for more subtle and nuanced distinctions in our appraisals of theories than its critics have allowed. The resources deployed by the classical logician seem to be the sorts of machinery required by any coherent account of our use of theories. We need, for example, to distinguish

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between axiom sets and the logical closure of the axioms sets. Since the non-classical logician needs this distinction too, its use by the classical logician can hardly be objected to.

Discovering that a theory is inconsistent is progress. We learn that this set of axioms is false, but what went wrong? This is productive in science, in philosophy and in our inquiries in general. It would be a grave mistake to accept an inconsistent theory as true, and the danger of the paraconsistent approach is that it encourages us to do just this. Insofar as we are rational we do not acquiesce with arguments to contradictions. The discovery of a contradiction serves to show us that our corpus of commitments is not at a stable equilibrium point. Something must give. In that way even though logic deals with the statics of commitment it can, and perhaps should, indicate that change in those commitments is required.[11]

REFERENCES

Cicero, *Academica*, trans. by H. Rackham, 1933, The Loeb Classical Library. Harvard University Press.

Colyvan, M. 2001. *The Indispensability of Mathematics*. New York, NY: Oxford University Press.

——. 2008a. 'The Ontological Commitments of Inconsistent Theories.' *Philosophical Studies*, 141: 115–23.

——. 2008b. 'Who's Afraid of Inconsistent Mathematics?' *Protosociology*, 25: 24–35.

Da Costa, N. C. A. and French, S. 2003. *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*. New York, NY: Oxford University Press.

Da Costa, N. C. A., Béziau, J.-Y. and Bueno, O. 2005. 'On the Usefulness of Paraconsistent Logic.' In D. Vanderveken (ed.), *Logic, Thought and Action*, p. 465–78. Dordrecht: Springer.

Harman, G. 1986. *Change in View*. Cambridge, MA: MIT Press.

——. 2011. 'Quine's Semantic Relativity.' *American Philosophical Quarterly*, 48: 287–9.

Jackson, F. C. 1989. 'A Puzzle about Ontological Commitment.' In J. Heil (ed.), *Cause, Mind and Reality*, pp. 191–9. Dordrecht: Kluwer.

——. 1998. *From Metaphysics to Ethics*. Oxford: Oxford University Press.

Lewis, D. K. 1973. *Counterfactuals*. Cambridge, MA: Harvard University Press.

Maddy, P. 1992. 'Indispensability and Practice.' *Journal of Philosophy*, 89: 275–89.

Meheus, J. (ed.) 2002. *Inconsistency in Science*. Dordrecht: Springer.

Michael, M. 2006. 'Accounting for Commitments: *A priori* Knowledge, Ontology and Logical Entailments.' In S. Hetherington (ed.), *Aspects of Knowing: Epistemological Essays*, pp. 35–52. Amsterdam: Elsevier.

——. 2008. 'Implicit Ontological Commitment.' *Philosophical Studies*, 141: 43–61.

Miller, D. 1972. 'The Truth-likeness of Truthlikeness.' *Analysis*, 33/2: 50–5.

——. 1974. 'Popper's Qualitative Theory of Verisimilitude.' *British Journal for the Philosophy of Science*, 25: 166–77.

——. 1975. 'The Accuracy of Predictions.' *Synthese* 30/1–2: 159–91.

——. 1976. 'Verisimilitude Redeflated.' *British Journal for the Philosophy of Science*, 27/4: 363–81.

Niiniluoto, I. 1977. 'On the Truthlikeness of Generalizations.' In R. E. Butts and J. Hintikka (ed.), *Basic Problems in Methodology and Linguistics*, pp. 121–47. Dordrecht: Reidel.

——. 1978a. 'Truthlikeness: Comments on Recent Discussion.' *Synthese*, 38: 281–329.

——. 1978b. 'Verisimilitude, Theory-Change, and Scientific Progress.' In I. Niiniluoto and R. Tuomela (eds), *The Logic and Epistemology of Scientific Change. Acta Philosophica Fennica*, 30/2–4, pp. 243–64. Amsterdam: North-Holland.

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——. 1979. 'Truthlikeness in First-Order Languages.' In I. Niiniluoto et al. (eds), *Essays on Mathematical and Philosophical Logic*, pp. 437–58. Dordrecht: Reidel.
Oddie, G. 1986. *Likeness to Truth*. Dordrecht: Reidel.
Popper, K. 1959. *Logic of Scientific Discovery*. London: Routledge and Kegan Paul.
Priest, G. 1987. *In Contradiction*. Dordrecht: Martinus Nijhoff; 2nd edition, 2006, Oxford: Oxford University Press.
Priest, G. and Tanaka, K. 2009. 'Paraconsistent Logic.' In E. N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2009 Edition). http://plato.stanford.edu/archives/sum2009/entries/logic-paraconsistent/.
Quine, W. V. 1948. 'On What There Is.' *Review of Metaphysics*, 2(5): 21–36.
——. 1960. *Word and Object*. Cambridge, MA: MIT Press.
——. 1968. 'Ontological Relativity.' *Journal of Philosophy*, 65: 185–212.
Ramsey, F. 1926. 'Truth and Probability.' In D. H. Mellor (ed.), (1990) *Philosophical Papers*. Cambridge: Cambridge University Press.
Schaffer, J. 2008. 'Truthmaker Commitments.' *Philosophical Studies*, 141: 1–17.
Tichý, P. 1974. 'On Popper's Definitions of Verisimilitude.' *British Journal for the Philosophy of Science*, 25: 155–60.
——. 1976. 'Verisimilitude Redefined.' *British Journal for the Philosophy of Science*, 27: 25–42.
van Heijenoort, J. 1967. *From Frege to Gödel: A Source Book in Mathematical Logic, 1879–1931*. Cambridge, MA: Harvard University Press.

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