On Inconsistent Arithmetics: A Reply to Denyer

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1. Introduction

In "Is Arithmetic Consistent?" (1994a—hereafter, IAC) I drew attention to the fact that there are inconsistent but non-trivial theories that contain all the sentences true in the standard model of arithmetic, *N*. The theories are not, of course, classical theories, but paraconsistent ones. I also argued that it is not as obvious that *N* is the correct arithmetic as one might suppose, and that there are reasons for taking one of the inconsistent arithmetics, *M*, with least inconsistent number, *m*, to be the correct one. Two reasons were given. The first was a direct one; the second an indirect one, to the effect that *M* avoids most of the limitative theorems of classical metatheory. In "Priest's Paraconsistent Arithmetic" (1995—hereafter PPA), Nicholas Denyer gives a critique of the paper. The first four sections attack the indirect argument, the fifth the direct argument, and the sixth, and final, section is an *ad hominem* attack. The paper is a mixture of insightful criticism, over-swift argument and misreading. The purpose of this note is to point out which parts are which.

2. Misreadings

Let us start with some of the misreadings. PPA begins with the sentence "Graham Priest has recently contended that there are in fact only finitely many natural numbers". This is false—or at least highly misleading—for two reasons.

As specified in the paper, the model for *M* is indeed finite. And one might therefore use the existence of *M* as an argument for finitism. This is exactly how it is used, for example, by van Bendegem (1993). But van Bendegem's project is not mine. I nowhere endorsed the model of *M* as the correct model; it was the set of things true in it with which I was concerned. This can be specified in many different ways (as IAC, fn. 3 points out), e.g., through an axiom system or a decision procedure. I employed the model-theoretic construction to explain *M*, only because it is an easy

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way to grasp the content of M, and, historically, was the way that theories such as M were discovered.

As regards the content of M, there is a sense in which it is true that it claims that there are finitely many numbers, and a sense in which it denies this. The standard definition of finitude uses the language of set theory. It cannot, therefore, be expressed in the language of M at all. But

$$M \vDash \forall x(x = 0 \vee x = 1 \vee \ldots \vee x = m).$$

M therefore claims that there are $m + 1$ things, which is an expression of finitude. On the other hand,

$$M \vDash \forall x \, \exists y \, \forall z \leq x(y \neq z),$$

which is an expression of infinitude (or at least non-finitude). Hence, simply to say that I endorse the finitude of numbers, without giving the other side of the contradiction, is rather misleading.[1]

The claim that I have contended that there are only finitely many numbers is misleading for a second reason. The paper does not endorse M, as the correct arithmetic. The spirit of the paper is that of an interesting speculation—as the very title indicates—which is not as absurd as it might at first appear, and which has various things going for it. If I had wanted to endorse M as the correct arithmetic I would done so.[2]

I would not labour this trivial point, except that the final section of PPA is, as I noted, structured as an *ad hominem* attack, to the effect that I do not believe that M is the correct arithmetic. Some of Denyer's comments can be stripped of their *ad hominem* dressing and deserve further comment. I will return to these later. For the present, I simply want to point out that the *ad hominem* attack is not warranted. Denyer, it would appear, has allowed his desire to wield some nice rhetoric to compromise his reading.

Before we turn to matters of more substance, there is a third point of misunderstanding to be cleared up. Though the indirect argument that I give for M turns on the fact that it avoids most of the classical limitative theorems, the Löwenheim-Skolem theorem is not one of these. Non-categoricity is a feature of the inconsistent arithmetics, just as it is

[1] Denyer does note that M is inconsistent ("Priest contradicts himself", PPA, p. 567), but this still does not convey the right information, any more than saying that a dialetheic solution to the paradoxes of self-reference takes them to be untrue, but is inconsistent. This description holds, after all, of many attempts to solve the paradoxes consistently.

[2] The only part of the paper where the language might suggest an endorsement of M is §4, where the direct argument against N is discussed. For example, on p. 341 I say that the argument proves that N is not correct. But the point of phrases such as this (as I would have hoped the context would make clear) is to contrast this argument with the indirect one that follows, which provides evidence of a circumstantial kind.

of the consistent ones. (We can take any finite model with more than one thing in the domain, A, and collapse it to give a non-isomorphic model of the set of sentences true in A.) IAC is quite clear about this fact.[3] Yet PPA says: "Priest presumably means us to take M as evading the categoricity problem" (p. 569), and §4 of the paper provides an *ad hominem* argument against categoricity. Priest means no such thing.

3. Petersen's argument

Let us now move on to more interesting things. The direct argument for M is an argument, discovered by Uwe Petersen, whose conclusion is that $\exists x x = x + 1$. Denyer observes (for *reductio*, I take it), quite correctly, that there is a variant of the argument which establishes that the x in question is less than 10 (in fact, it could have been less than 2), which is unacceptable. What should one make of this?

Note, first, that two versions of the argument are given in IAC, an informal version in §4, and a formal version in Appendix 2. Let us take the formal version first. The x in question is a fixed point, π, of the form $\mu x\triangle(<\pi>, x) + 1$. ($\mu$ is a least number operator, $\triangle$ is a denotation predicate, and angle brackets are a name-forming device.) Employing various principles concerning denotation, including

$\triangle 1 \quad \triangle(<t>, t)$

(for all closed terms, t) we deduce that $\pi = \pi + 1$.

This argument, it turns out, is a special case of an argument to be found in Hilbert and Bernay's *Grundlagen der Mathematik* (as was pointed out to me by Uwe Petersen himself). Moreover, applying it in other cases, one can prove quite unacceptable things, such as that $0 = 1$.[4] Thus, fortunately or unfortunately, arguments of the kind cannot be allowed to stand. It is natural to object to arguments of this kind that $\triangle 1$ is not, in general, true, since it entails $\exists x\triangle(<t>, x)$, and some terms do not denote anything. Moreover, it is also natural (in fact, classically obvious) to claim that "π" does not denote anything. If this is right, and I think it is, we have an explanation of why the argument fails.

[3] "The Limitative Theorems of classical metamathematics are usually regarded as, at best, disappointing, at worse posing nasty philosophical problems. With the exception of the Löwenheim-Skolem Theorem ... paraconsistent arithmetic is free from all these theorems, and so problems" (IAC, p. 342). As IAC, p. 39, points out, the Collapsing Lemma is the ultimate downwards Löwenheim-Skolem Theorem.

[4] For references and discussion, see Priest forthcoming.

This suggests that we might try to repair arguments of this form by defining fixed point terms in such a way that they are guaranteed denotation. Let us write Ex for $\exists y\Delta(x, y)$. Then, in the case of the argument of IAC, we might define a fixed point, π, of the form:

$$\mu x((E <\pi> \wedge \Delta(<\pi>, x)) \vee (\neg E<\pi> \wedge x = 0)) + 1.$$

Reasoning in a natural way we can then establish that the μ-term, and so "π", denote. Now, however, the argument fails at another place, at least paraconsistently. For to show that the μ-term satisfies the first disjunct of its definition, we need to employ the disjunctive syllogism illicitly at a crucial point.[5]

We may now turn to the informal version of Petersen's argument given in IAC. In fact, it differs from the formal version, not only in being informal, but in taking account of the possibility of the non-denotation of π explicitly, in just the way I have indicated. It therefore fails, for the reasons that I have just given.

At last we come to Denyer's argument. It is a version of the informal version, and defines the fixed point thus: the least number ≤ 10 such that this very description refers to it (or 0 if it fails to refer to such a number) +1, i.e., π is:

$$\mu x((E <\pi> \wedge \Delta(<\pi>, x) \wedge x \leq 10) \vee (\neg E <\pi> \wedge x = 0)) + 1.$$

And the argument fails for the same reason. If we call the μ-term τ, then we can establish that:

$$(E <\pi> \wedge \Delta(<\pi>, \tau) \wedge \tau \leq 10) \vee (\neg E <\pi> \wedge \tau = 0)$$

but we cannot establish the first disjunct, as required, without using the fact that $E<\pi>$ and employing the disjunctive syllogism. And there is good reason to suppose that the sentence $E<\pi>$ ("π" denotes) is contradictory. The argument in question is one of the Berry family, where various devices may be used to define something that is indefinable, and so employ terms that both do and do not denote.

Where does this leave us? Denyer's version of Petersen's argument does not work. But equally, neither does my version of it in favour of M.[6] This certainly weakens the case for M, though it does not destroy it entirely, since the indirect argument, which I take to be weighty (IAC, p. 342), still remains.

[5] Details of all this can be found in Priest forthcoming.

[6] In IAC (p. 343), Petersen's argument is also used to reply to an objection to the effect that the choice of which inconsistent arithmetic is correct is arbitrary. This reply is therefore no longer available. A different reply would be provided if there were independent considerations determining the least inconsistent number. Such considerations are discussed in Priest 1994b.

4. Decidability

The indirect argument for the correctness of M is to the effect that it avoids the limitative theorems of classical metatheory; specifically, Church's Theorem, Tarski's Theorem, Löb's Theorem and Gödel's first and second Incompleteness Theorems. Denyer criticizes the claims concerning the first two of these. Even if he is right, therefore, the considerations concerning the others remain.

Concerning Church's Theorem, Denyer argues that if M is correct then the decision procedure offered in IAC does not terminate, and hence if M is correct, it is not decidable (if decidable as well). Two arguments are given for non-termination. The first is to the effect that if we start with a formula with m quantifiers, then since $m = m + 1$, it has $m + 1$ quantifiers. Thus, when we eliminate a quantifier, we still have m quantifiers, and so the reduction has not advanced. Secondly, when we eliminate a quantifier, we obtain $m + 1$ statements to check, and so essentially the same problem arises.

Now these are interesting arguments. Or rather, argument sketches; and therein lies the problem. That M is decidable is a simple number-theoretic statement. M can be represented in arithmetic (M and N) by a predicate $B(x)$ (IAC, p. 347). Now let T be the (3-place) Kleene T-predicate, and U the output functor; $Texy$ expresses the claim that y is the code of a terminating computation of the 1-place recursive function whose code is e, with input x; Uy is the output of the computation y. Then the decidability of M can be expressed by the sentence:

$$\exists e\ \forall x\ \exists y(Texy \wedge (Uy = 0 \equiv B(x)) \wedge (Uy = 1 \equiv \neg B(x))).$$

The reasoning that Denyer gives gestures at a proof for the negation of this sentence, and it can be agreed that the reasoning could be fleshed out into a full classical proof. But if the argument is to work, and is not to beg the question, it must hold not by classical standards, but by those of M. In particular, it must use only inferential moves that are valid in *LP*. And it is not at all clear that the reasoning can be fleshed out in a suitable way here.[7] For example, at many places, arithmetic conditionals must be applied and detached. For any such conditional true in N, M guarantees this in the form of a material conditional, but it may or may not hold in a stronger form. For example, the principle that subtracting 1 from $m + 1$ gives m, applied at crucial points in Denyer's argument, holds only in the form of a material conditional. (I will return to the issue of subtraction in §6.) Detachments of such conditionals are not valid in *LP*. This is not to say that

[7] A similar point applies to Denyer's argument (p. 570) that if M is correct, N is also decidable.

Denyer's proof-sketch cannot be suitably reconstructed (though I am skeptical), but without all the hard work required here, the idea is no more than an interesting possibility.

It might be suggested that if Denyer's argument is suspect because it uses classical reasoning, then so is my decidability argument and the case based on it. It is true that the argument I employed uses classical reasoning. It is also true that I gave this fact insufficient attention in the paper. But, in fact, there is no problem here. Since the decidability of M is expressed by a sentence of arithmetic, and since there is a classically correct argument for it, the sentence is true in the standard model, and hence in M. So the conclusion is paraconsistently correct.[8] Alternatively, and more simply, one can ensure that M is a decidable set by just defining it in terms of its decision procedure. M is then trivially decidable. I conclude that Denyer's argument fails, at least as it presently stands.

5. Truth

Let us turn to Tarski's Theorem. Denyer argues that M does not contain its own truth predicate, as IAC claims. His reason for this is that it contains the T-schema only in the form of a material conditional, which does not guarantee that φ and $T<\varphi>$ have the same truth value, as should be required for a genuine truth predicate.

What conditions a truth predicate should satisfy is a contentious matter. But I, for one, do not think that a genuine truth predicate does require the condition Denyer assumes—and neither do a lot of other people. For example, a number of people think that there are some sentences that are neither true nor false. Let φ be such a sentence. Then by this very claim, $T<\varphi>$ is *false*, not neither true nor false. Dually, if φ is both true and false, $T<\varphi>$ would seem to be simply true. It could be argued that $T<\varphi>$ is false as well in this case, but I have argued against this in general elsewhere (Priest 1987, 4.9). Thus φ and $T<\varphi>$ ought not to be expected to have the same truth value in general.

The relationship between φ and $T<\varphi>$ is, I think, one of biconditionality: if either holds, so does the other. What, however, is the nature of the conditional here? Is it material, an entailment, enthymematic? It seems to me that one might take several different positions on the question. Priest (1987, 4.9) argues that it is a detachable but non-contraposable conditional. I still think that this is correct, but recently Goodship (1996) has

[8] The same cannot be said for Denyer's argument, since it uses premises, such as $m = m + 1$, not true in the standard model.

argued plausibly that it is more sensible to take it to be a material conditional.

It might well be argued that whatever notion of conditionality is correct here, it cannot be a material conditional, for the conditional should at least support the inference from one side of the *T*-schema to the other (detachment), and the material conditional does not do so. Now, it is agreed that the *T*-schema should support some sort of inference, and this certainly puts a constraint on what the appropriate connective can be, but this fact does not rule out a material conditional. The question is what sort of inference the schema should support. The material conditional may not support an unconditional inference, but it supports a defeasible inference, with consistency as the default assumption.[9]

As pointed out in IAC (p. 347), although the Collapsing Lemma does not provide a theory with the T-schema in the form of a detachable conditional, a simple construction gives exactly this (in fact, in the model of the theory φ and $T<\varphi>$ do have the same truth value). One may therefore maintain the existence of a truth-predicate satisfying these stronger conditions. Denyer argues that such a truth-predicate is still inadequate. For let β be a liar sentence of the form $\neg T<\beta>$. Then β is both true and false. But (PPA, p. 571):

> even though Priest denies the Law of Contradiction, he also asserts it. He will therefore agree that no truth is false, and no falsehood is true. From these facts, together with his claim that β is both true and false, he will be prepared to infer that β is neither true nor false … .

And therefore that β does not have the same truth value as $\neg T<\beta>$.

This argument is just fallacious. I certainly endorse the Law of (Non-) Contradiction in the form, for any α:

$$\neg(\alpha \wedge \neg\alpha)$$

and so, in particular:

$$\neg(T<\alpha> \wedge \neg T<\alpha>).$$

But it does not follow from this that:

$$F<\alpha> \rightarrow \neg T<\alpha>$$

and:

$$T<\alpha> \rightarrow \neg F<\alpha>$$

as required for the inference to the conclusion $\neg T<\alpha> \wedge \neg F<\alpha>$. Even if one supposes that: $\neg T<\alpha> \rightarrow T<\neg\alpha>$ (= $F<\alpha>$),[10] the best one can get is $F<\alpha> \supset \neg T<\alpha>$ and its contrapositive. This will not even support a

[9] This is argued in Priest 1987, Ch. 8. I now think a better way of spelling out the idea formally is as in Priest 1991.

[10] Which I do not. See Priest 1987, 4.9.

default inference to the conclusion Denyer requires, since the formula in question is known to be inconsistent.

I conclude that Denyer's attack on this point, and, with it, his attack on the whole of the indirect argument for M, fails.

6. Subtraction

Let us turn, finally, to the last section of PPA. In this, Denyer moves from his critique of the arguments in IAC to an *ad hominem* attack. I have already explained why this fails; but, independently of this, the arguments are of some interest and deserve comment.

Denyer asks why I do not infer that $1 = 0$ from the fact that $m + 1 = m$ ($= m + 0$), by subtracting m from both sides; or claim that Fermat's Last Theorem is false since $m^m + m^m = m^m$. These two cases are, in fact, quite different. $m^m + m^m = m^m$ is true in M.[11] I am therefore quite willing to draw the conclusion that if M is the correct arithmetic Fermat's Last Theorem is false (though it may be true as well).

The situation with respect to the first case is quite different.[12] $1 = 0$ is not true in M. I would not therefore infer it, even if I took M to be correct. M contains the subtraction principle only in the non-detachable form $x + z = y + z \supset x = y$, and in those cases where the antecedent is both true and false, notably, those concerning m, not even a default detachment is possible.[13] But this suggests a different argument against M. In IAC (p. 340) I noted that one might argue that N is the true arithmetic because it correctly represents people's arithmetic practices. The preceding observation allows us to formulate a new version of this argument. If i, j and k are numerals, people *do* infer $i = j$ from $i + k = j + k$. Hence, if an arithmetic cannot license this inference, as M cannot, it is not the correct arithmetic.

[11] This is not quite right as it stands. The language of M does not contain exponentiation. However, it could be extended to do so with no problems. Alternatively, we could just express m^m as $m \times \ldots \times m$ (with m factors).

[12] And is similar to the case of division, which Denyer also raises.

[13] There is a technical point that is worth noting here. Subtraction is not functional on the natural numbers ($x - y$ is not defined if $x < y$), and so cannot be represented by a function symbol. Truncated subtraction, $\dot{-}$, (i.e., $x \dot{-} y = x - y$ if $x \geq y$, and 0 otherwise) is functional, however. If a function symbol for truncated subtraction is added to the language of arithmetic, then there can be no inconsistent arithmetics other than the trivial one. For if $m + n = m$, $(m + n) \dot{-} m = m \dot{-} m$, follows simply by functionality. Thus, $n = 0$. In M, as in the standard language of arithmetic, truncated subtraction is represented by the functional 3-place formula: $(x \geq y \wedge y + z = x) \vee (x < y \wedge z = 0)$. Classically there is no difference in the expressive power of a function term and a functional predicate. Paraconsistently, there clearly is. I do not know what significance this fact has, if any.

(I take this to be what Denyer's argument on p. 574f. amounts to when stripped of its rhetorical content.)[14] The answer is essentially still that given in IAC (p. 340). Agreed, people are normally disposed to make inferences of the form in question. But the normal context is that of very small numbers. If i is m and this is a number so large that it has no physical or psychological reality, then it is not at all clear that people have this disposition, or even that the question of what dispositions they have in this context makes sense.

A variation on this argument was put to me by Denyer in correspondence. Granted that people do not have the appropriate disposition concerning m, if someone knows that $x + z = y + z$ and that x, y and z stand for natural numbers, they do have the disposition to infer that $x = y$. Hence their dispositions confirm N, not M. This is another interesting argument; but again, it is too swift. Let us agree that people do have these dispositions; and let us assume—and one certainly might disagree with this—that people's arithmetic dispositions can decide which of M and N is correct. If *any* arithmetic disposition could settle the matter, the issue would be easy to decide. Doubtless, for example, most people are not disposed to assert the existence of a number greater than or equal to all numbers. But this simply reflects a belief that they have acquired during their education. To be constitutive of the truths of arithmetic, the dispositions must be of a more fundamental kind. What kind? We might argue about this; but a natural position is that it is those dispositions that deal only with numerals and numerical operations that should count here. It is, after all, the practices of counting, adding, etc., that constitute learning arithmetic. Dispositions concerning *generalised* claims (i.e., those employing variables) are part of a *theory* about numbers that people come to acquire later. In particular, then, the dispositions concerning subtraction that Denyer notes do not determine the correctness of N.

7. The size of the least inconsistent number

The reply of the previous section depends on m being incomprehensibly large. This was postulated in IAC, but little argument for it was given, other than noting that we find it impossible to provide a plausible candidate for m (p. 342); and there is an obvious reason. Exactly the sort of consideration we have just been considering knocks out the speculation

[14] "... we can ask Priest for some explanation of why he refuses to draw these corollaries [e.g., 1 = 0], and to treat his putative natural number m as normal people treat what they call natural numbers."

concerning M immediately if m is a small number, so it is hardly an interesting one. This answers Denyer's question: "we may wonder how Priest can be so sure that the magic number is so large" (p. 574).

He continues:

> For certainly the fact that we have conclusive reason for accepting that $10 \neq 20$ is not, by Priest's lights, conclusive reason against accepting that $10 = 20$ also. Indeed, even if the magic number is as large as Priest says it is, it could still by Priest's standards be true to say that the magic number is much, much, smaller ... .

The point carries no weight. The fact that it is logically possible for a contradiction to be true ("Priest's own standards") does not entail that one can rationally accept it. Quite generally, from the fact that something is logically possible, it does not follow that it is rationally possible to believe it, as familiar examples attest.[15] It is logically possible that I could fly to the moon by flapping my arms, but believing this is ground for certifiable insanity. In particular, then, it would be quite irrational to believe that the least inconsistent number was small, as well as big, unless we had some evidence to this effect. But there is none as far as I know.[16]

8. Conclusion

What have we learned? Two distinct arguments are given for M in IAC. The direct argument is incorrect, and, though he gives no explanation for this fact, Denyer is right about this. Denyer's attack on two parts of the indirect argument fails, and so this argument is still in place. Finally, Denyer's *ad hominem* arguments are inappropriate. But even stripped of their *ad hominem* elements, they fail. The case for M is therefore weakened, but not destroyed.[17]

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[15] For a discussion of this point, see Priest 1987, §7.5.

[16] Similar considerations apply to Denyer's final footnote: "Priest disagrees with much of what I write—which is not to say that he does not also agree with it". This is a good joke—or at least, it was the first few dozen times I heard its kind.

[17] I would like to thank Nick Denyer for very helpful comments on the note's first draft.

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