Holger Andreas and Peter Verdée (eds.), Logical Studies of Paraconsistent Reasoning in Science and Mathematics

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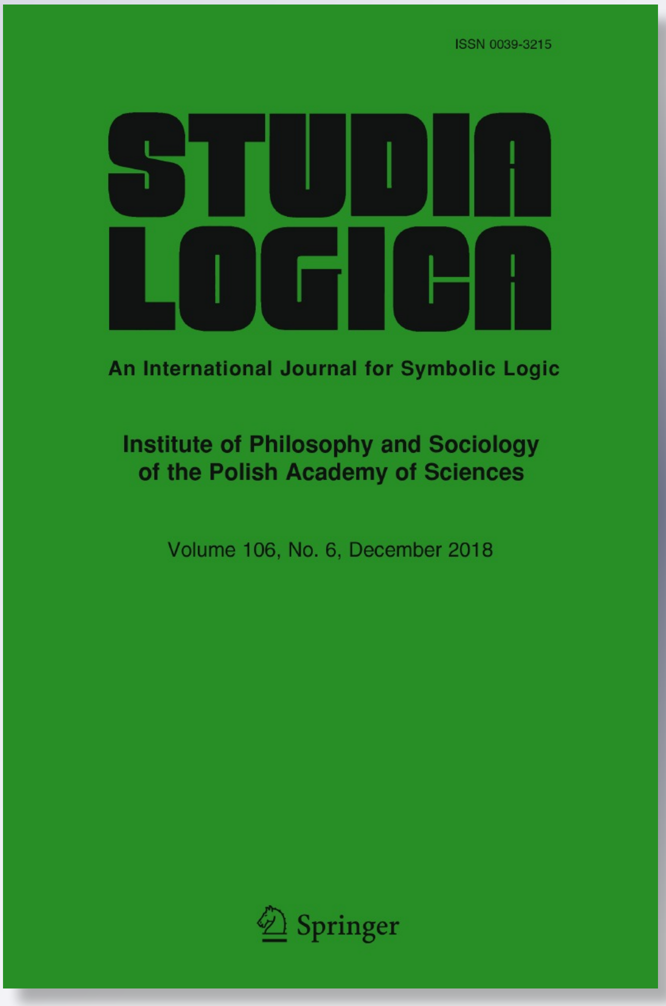
Studia Logica
An International Journal for Symbolic Logic

ISSN 0039-3215
Volume 106
Number 6

Stud Logica (2018) 106:1313-1318
DOI 10.1007/s11225-018-9826-4



Springer

Book Reviews

Holger Andreas and Peter Verdée (eds.), **Logical Studies of Paraconsistent Reasoning in Science and Mathematics**, Springer, Series: *Trends in Logic*, Vol. 45, 2016, pp. vi + 221. ISBN 978-3-319-40218-5, EURO 89,99.

In the 1970s praconsistent logic was something of an ugly duckling in the flock of non-classical logics. In the last 40 years I have watched as it has become, if not a swan, then at least a well respected member of the flock. And whether or not one thinks of the "One True Logic" as paraconsistent, the wealth and sophistication of both the technical results and the potential applications of this kind of logic can no longer be denied. Of course, this did not happen by magic: many papers and monographs on the area have been published, the collective weight of which established these matters.

Also important in this regard are the edited collections of papers that have appeared. I had the pleasure of editing with Richard Routley (and his Research Assistant, Jean Norman) what I believe was the first such collection, *Paraconsistent Logic: Essays on the Inconsistent* (Munich: Philosophical Verlag). The manuscript of the book was all but finished in 1981. For reasons beyond the editors' control, the volume did not appear till 1989—and when it did appear, it was outrageously expensive by the standards of the day. It has now been out of print for a long time. However, the substantial editorial introductions to its parts, by Richard and myself, appeared in 1983 as an Australian National University in-house preprint, *On Paraconsistency*, and can now be accessed at: http://grahampriest.net/publications/books/. Since I shall refer to *In Contradiction* (Martinus Nijhioff, 1987; 2nd edn, Oxford University Press, 2006) below, let me also mention that as soon as the ms for *Paraconsistent Logic* was finished, I took myself off to Pittsburgh, where that book was written in 1982. This did not, itself, appear until 1987—though for somewhat different reasons.

Anyway, since the appearance of *Paraconsistent Logic*, many edited collections have appeared, a number of them originating in conferences, such as the numerous World Congresses on Paraconsistency. The present book is the most recent installment of this genre. It originated in the conference *Paraconsistent Reasoning in Science and Mathematics*, organised by the editors in Munich, June 2014. Its aim, the editors tell us (p. 1), is to 'present a collection of papers on the topic of applying paraconsistent logic to solve inconsistency related problems in science, mathematics and computer science'. (An earlier collection on a similar theme is: J. Meheus (ed.), *Inconsistency in Science*, Dordrecht: Kluwer Academic Publishers, 2002.) After a very helpful editorial introduction, there are 12 chapters on these topics—though computer science doesn't get much of a mention, and some of the papers are about how *not* to apply paraconsistent logic. Some of the papers are by well known names in the field; some are by more up-and-coming young people.

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The papers introduce many new ideas, and are generally of excellent quality. Together, they provide a window onto the present state of paraconsistency—though the subject is now so vast that any window is bound to reveal only a small part. The editing and type-setting of the book are excellent (thank you, Latex). The editors are much to be congratulated for producing this very nice volume. (An index would have made it even better!)

In what follows, I will indicate briefly the contents of each chapter, and add a brief comment on each one. (There is much more to be said about all of them; but perforce I must be brief, and this is not the place to delve into technical niceties.)

There are five papers in the collection which deal with inconsistency in the empirical sciences (interpreting that notion in a reasonably generous way). The first, entitled 'Adaptive Proofs for Networks of Partial Structures' is by the editors themselves. Essentially the method spelled out in the paper is as follows. Suppose that we have an inconsistent scientific theory with universally quantified laws. A *preferred* model of the theory is a consistent model which makes true a collection of instances of the laws, such that there is no consistent model which makes more instances true. Validity is then defined as truth preservation in all preferred models. This generates a non-monotonic logic, and the paper shows very nicely that this is an adaptive logic, in the sense of Diderik Batens. The authors also show how this logic handles a very simple example concerning Bohr's inconsistent theory of the atom. Whether the method can handle more realistic examples remains to be seen.

The second paper in this group (about cognitive science) is entitled 'Inconsistency in *Ceteris Paribus* Imagination', and is by Franz Berto. When one imagines something, part of what one imagines is explicit, and part of what one imagines is implicit. Thus, when I imagine Sherlock Holmes and his doings, what Doyle tells me about this is explicit, but part of the content of what I imagine is not explicit, but is driven by things about 19th Century Victorian London which are taken for granted. Berto considers modal operators of the form $[A]B$ meaning that in imagining something with an explicit content A, B holds. These are given a world-semantics. The semantics do not require a paraconsistent logic as such, but since the contents of our imaginings are sometimes contradictory, the worlds must include impossible worlds. This is all nice. It leaves open, however, the crucial philosophical/technical question: in an imaginative act, what exactly is the filter which allows in the collateral information, and how does it work?

The third paper in this group, entitled 'On the Preservation of Reliability', is by Bryson Brown. This chapter contains a philosophical argument that in science one requires a logic defined, not in terms of truth-preservation, but in terms of reliability-preservation. Brown observes, correctly, that our most reliable theories may sometimes be inconsistent, so paraconsistent logic/s is/are required. No such logic is specified, though the method of Chunk and Permeate is mentioned *en passant*. I was not entirely clear what reliability-preservation amounts to. Brown glosses it as follows (p. 74): 'I propose that we should regard the kind of reasoning I've been discussing here as aimed at the preservation of *reliability*. That is, the inferences countenanced in successful models of these kinds are inferences that are found to produce reliable conclusions about the systems we apply them to'. Now, this doesn't seem to me to involve any kind of preservation—of reliability or otherwise. What is being endorsed is the use of whatever inferential machinery delivers reliable (presumably empirical) results. So what we have here is a version of instrumentalism. Use whatever machinery gives the right results [whilst trying to achieve as as much overall coherence as possible (Brown, email communication)].

The fourth paper in this group is entitled 'On Gluts in Mathematics and Science', and is by Andreas Kapsner. In this, he argues that for the purposes of science one might well want to deploy a many-valued logic, where one of the values is both *true and false*, though this should not be designated. A major argument for this is to the effect that designated values are the sorts of values that pertain to things to be asserted, and that we sometimes have evidence for contradictions which we do not want to assert. An example of this which Kapsner gives concerns views about the age of the Earth in the late 19th Century, when Thermodynamics and the Theory of Evolution—both well established theories—gave radically inconsistent verdicts of this. Now, it is true that this is a contradiction that one would presumably not want to endorse. But one would not want to endorse it just because it is implausible to suppose that the contradiction is actually true: at least one of these estimates is wrong. In other words, one would not take it to be both true and false, that is, to be assertable, that is, to have a designated value.

The last paper in this group is entitled 'Dialetheism in the Structure of Phenomenal Time', and is by Corry Shores. This is about time, and specifically our phenomenological awareness of it passage. Sometimes we know that a thing has moved because we remember where it was and can now see it somewhere else—e.g., the hour hand of a watch. But sometimes we can *see* something moving—e.g., the sweep hand of a watch. This is sometimes called the *specious present*, and what it suggests is that in some sense we can see the hand in more that one place at a single time. In the 2nd edition of *In Contradiction* (ch. 15), I suggested that to account for aspects of the flow of time, one should take it that more than one time occurs at one time—and specifically, that, at any time, there is a spread of times around it where it is also those times. Stokes suggests that this model can account for the phenomenology of the specious present. In the specious present we see all the things happening in such a spread of time. The idea is an intriguing one, but I'm not sure that it gets the phenomenology right. If we did see, e.g., the sweep hand at every place it occupies in a small amount of time, it should appear, not as a ray, but as a small sector of a circle, fanning out from the point of pivot. It is very had to describe exactly what it is that one does see, but it certainly doesn't seem like this.

There are three papers in the collection which deal with inconsistent mathematics. The first, entitled 'Prospects for Trivialism', is by Luis Estrada-Gonzalez. This takes on an argument by Chris Mortensen and others, to the effect that there is nothing of significance in a mathematical system which is trivial (i.e., in which everything holds), by pointing out that there is a perfectly fine trivial topos. Estrada-Gonzales argues that Dunn's proof of triviality from the identity of two distinct real numbers breaks down in the trivial topos. Perhaps; but the trivial topos is trivial anyway! And, I suspect that Mortensen *et al* would not be very moved by the trivial topos. After all, the topos may be trivial, but the topos theory in which it is defined, is not. If it were, it would indeed seem to have little mathematical interest—at least as far as anything in this chapter shows.

The second paper in this class, called 'Saving Proof from Paradox: Gödel's Paradox and the Inconsistency of Informal Mathematics', is by Fenner Stanley Tanswell. This points out (correctly) the (logically) informal nature of mathematical practice, and discusses the relevance of this for arguments aimed at showing that mathematics is inconsistent by applying facts about formal theories. In particular, an old argument of mine in *In Contradiction* (ch. 3) argues that informal mathematics—or even just the part of it which concerns natural numbers—could, in principle, be formalised, and, that when this is done, the resulting system would satisfy the conditions for applying Gödel's first incompleteness theorem. If the sentence 'this sentence is not provable (in the system)' were then provable, the system would be inconsistent; but it is provable, so the system is inconsistent. Tanswell takes on this argument. He raises several objections, but perhaps the most important one is that there may be many ways to formalise a piece of mathematics. The target to which one applies Gödel's first incompleteness theorem is therefore ill-defined. The point about many formalisations is well made. However, it seems to me that it does not undercut the force of the argument. For the argument would seem to apply to any adequate formalisation.

The third paper in this group is entitled 'Paraconsistent Computation and Dialetheic Machines', and is by Zach Weber. This is certainly the most provocative paper in the book. Taking up an idea of Richard Sylvan (Routley), Weber argues that we should consider seriously the possibility that the *reductio* involved in the proof of the Halting Theorem does not show that there is no halting function, but shows that it is an inconsistent one. Weber makes the idea much less implausible than it sounds. However, he leaves open the most intriguing question: what would it be like for a computing machine to both halt and not halt? Here is one possible answer. If we are to take inconsistent recursion theory seriously, we must take inconsistent number theory seriously (as Weber notes). But we now know well what such things are like. (2nd edn of *In Contradiction*, ch. 17.) In particular, it is quite possible for both the sentence 'this sentence is not provable' and its negation to hold. But what would it mean for something to be provable and not provable? In such systems, a sentence and its negation express independent states of affairs. Suppose that, say, 133 is the code of a proof of the sentence in question. Then it is provable. But no other number is identical to a code of a proof of the sentence, and if 133 is non-self-identical then there is no number which is identical to the code of a proof. That is, the truth of the claim that nothing is the code of a proof does not undercut the fact that there is a proof, but holds simply in virtue of the contradictory behaviour of the identity predicate. (For a full discussion, see 17.8.) In a similar way, if 133 is the code of a terminating computation, then the computation terminates. But no other number is identical to a code of the terminating computation, and if 133 is non-self-identical then there is no number which is identical to the code of the terminating computation. That is, the truth of the claim that nothing is the code of a terminating computation does not undercut the fact that there is such a computation, but holds simply in virtue of the contradictory behaviour of the identity predicate.

Despite how the editors describe the volume, the last group of papers in the volume don't have anything much to do with mathematics or science, but are simply about logic. The first in this group is entitled 'Contradictoriness, Paraconsistent Negation and Non-Intended Models of Classical Logic', and is by Carlos A. Oller. Oller takes on an argument due to Hartley Slater to the effect that paraconsistent negation is not *real* negation, since it allows for the possibility that a contradiction is true. Oller objects that if this is a good argument, classical negation is not real negation either, since, as observed by Carnap, there is a non-standard interpretation of classical logic in which everything is true, so classical logic cannot out a contradiction either. In response to the objection that this interpretation is non-standard, and so can be ignored, Oller replies, quite correctly, that the reply is question-begging. A better reply (following T. Smiley, 'Rejection', *Analysis* 56 (1996), 1–9) might be to formulate the classical rules of deduction in terms of acceptance and rejection, which can rule out the non-standard model.

The next paper in this group is entitled 'From Paraconsistent Logic to Dialetheic Logic', and is by Hitoshi Omori. Omori formulates a particular 3-valued logic. The logic is interesting for a number reasons. First, it contains a classically-behaving consistency operator. Secondly, it is a connexive logic, since the conditional verifies connexive principles such as $(A \rightarrow B) \rightarrow \neg(A \rightarrow \neg B)$. Thirdly, and in virtue of this, there are contradictory logical truths. Fourthly, the logic is functionally complete. This is a very impressive collection of properties. As far as solutions to the paradoxes of self-reference goes, a standard objection to logics with a consistency operator is that these produce triviality. Omori notes—following L. Goodship, 'On Dialethism', *Australasian Journal of Philosophy* 74 (1996), 153–61—that this is not so if principles such as the T-Schema are formulated as material biconditionals. No attempt is made to investigate the viability of this kind of solution, though.

The fourth paper in this group is entitled 'Paradoxes of Expression', and is by Martin Pleitz. Goodship's suggestion has been investigated in detail in G. Priest, 'What *If*: the Exploration of an Idea', *Australasian Journal of Logic* 14 (2017), https://ojs.victoria.ac.nz/ajl/article/view/4028/3574. There, it is suggested that though a material T-schema, since it does not admit detachment, is too weak to allow "blind endorsement", the same effect may be achieved by the use of propositional quantification. Pleitz observes that if one also has at one's disposal a form $E(x, p)$ expressing the claim that the sentence x expresses the proposition p, and satisfying the axiom:

- $(E(x, p) \wedge E(x, q)) \rightarrow (p \leftrightarrow q)$

triviality reappears. One possible reply is to the effect that if the T-Scheme is to be expressed as a material conditional, so should cognate principles. In particular, if D is the denotation predicate, the "D-Schema" should be expressed as:

- $D(\langle c \rangle, x) \equiv c = x$

(where angle brackets are a name-forming functor). Since E is the analogue of D, not for objects but propositions, we should expect it to be governed by the axiom:

- $E(\langle A \rangle, p) \equiv (A \leftrightarrow p)$

The material biconditional now breaks Pleitz' argument.

The final paper in this group is entitled 'On the Methodology of Paraconsistent Logic', and is by Heinrich Wansing and Sergei P. Odintsov. Many advocates of paraconsistent logic have held that a good paraconsistent logic should be a sub-logic of classical logic which is as strong as possible modulo invalidating *ex contradictione quodlibet*. The paper argues cogently against this methodology. The paper also endorses the thought that the correct approach to paraconsistent logic is to take such a logic to be one of information-preservation. It is not clear to me that this is actually very different form preservation of truth of a certain kind. After all, it is standard enough to cash out information as truth in a certain set of worlds (not necessarily possible worlds). Nothing in the authors' critique seems to turn on this point, though. The authors also consider (p. 194 ff.) a discussion of desiderata for a good paraconsistent logic presented by Richard Routley and myself in *On Paraconsistency*. This certainly does not endorse maximality of any kind (as the authors note). Indeed, Richard and I were not in the business of setting out *a priori* constraints on a good paraconsistent logic. Rather, we were arguing for the "One True Logic", drawing desiderata piecemeal from considerations concerning meaning, possible applications, and elsewhere. This was in the days before Logical Pluralism appeared on the scene. This appearance has of course introduced a whole new dimension of complexity into debates. But for what it is worth, I am still inclined to stand by the view that Richard and I endorsed then.

Be all that as it may, and as the papers in this collection show, paraconsistent logic has gone a long way since the 1970s. I am sure that it has much further to go still.

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