Eliminating inconsistency in science

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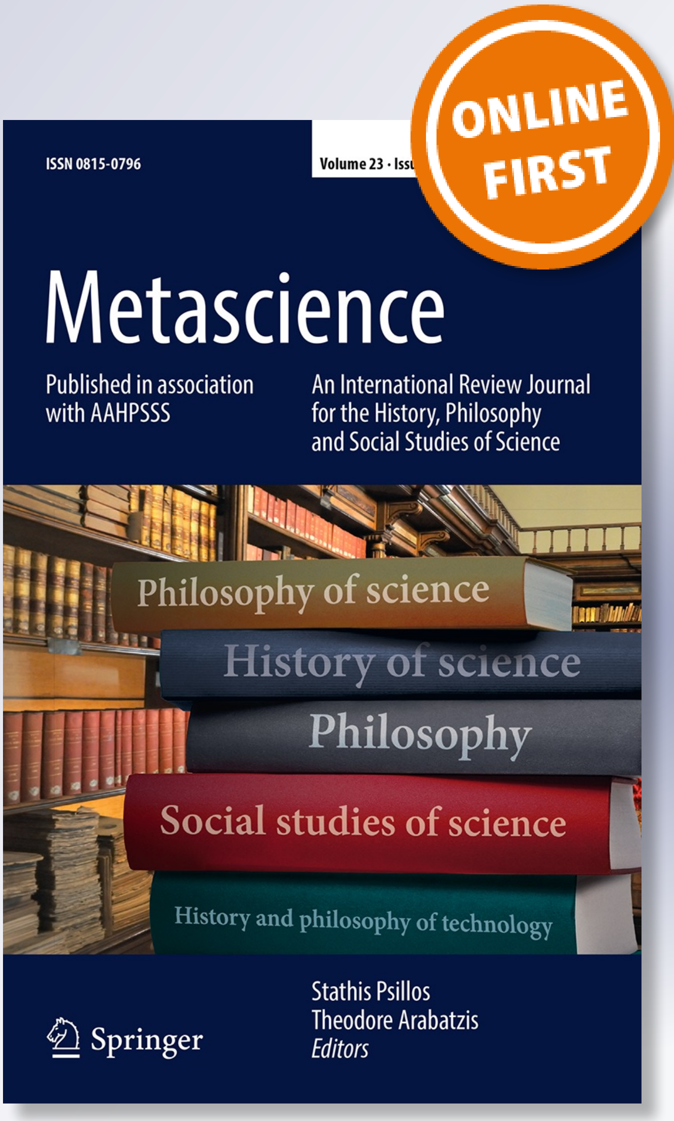

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BOOK REVIEW

Eliminating inconsistency in science

Peter Vickers: Understanding inconsistent science. Oxford: Oxford University Press, 2013, xii+273pp, £30.00 HB

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In this book, Peter Vickers argues that inconsistency in science has been massively exaggerated by philosophers. In his view, inconsistent science is neither as rampant nor as damaging as many have supposed. To argue his point, he develops a specific method he calls *theory eliminativism* and applies it to four case studies from the history of physics and mathematics (there are four additional cases he considers in the penultimate chapter, but they are rather brief and are apparently less highly cited in the literature, so I will skip them in what follows).

The method is original and convincing, and the case studies well researched and compelling. Vickers' monograph provides a challenge to any philosopher of science who takes inconsistency claims seriously, while also introducing a potentially very useful methodology for analyzing away problematic 'theory' discourse in other philosophical debates. Overall then, this is a very creative text and useful for both those directly interested in the inconsistency debates and also those looking for a novel approach to solving other problems in philosophy of science.

I will summarize the motivation and methodology adopted by Vickers, and then move on to describe how this works in his cases. I will close with some friendly critical comments and a suggestion for how theory eliminativism might usefully be extended to at least one other debate in philosophy of science.

First the motivation: Vickers claims philosophers of science quite commonly assume that the history of science is littered with internally inconsistent theories. Of course an internally inconsistent theory must be false, so this is an interesting claim, especially for theories still accepted as approximately true. But rampant inconsistency is also an interesting claim regarding those now rejected theories which were for a very long time believed to be at least approximately true. What were their advocates thinking?

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Vickers spends some time in the opening chapter introducing the most common examples of inconsistent science: Bohr's theory of the atom, classical electrodynamics, Newtonian cosmology, and the early calculus. He also provides some nice structure for what follows by raising important questions regarding inconsistency in science. For instance, what do/ought scientists do when faced with an inconsistent theory? How should we represent inconsistent theories? Which scientific theories are inconsistent? These questions set up his argument for theory eliminativism in Chapter two: In order to avoid currently persisting disagreements in our philosophical discussions, we should reject putting any weight on the concept 'theory'. It is just a misleading and confusing concept.

Theory eliminativism then is the claim that we are better off not using the term 'theory' when talking about scientific claims. As he puts it on page 243, 'If theories are not mentioned then any disagreements about the definition, nature, structure, ontology, and content of "theories" cannot affect the debate.' This is important, Vickers claims, because the history of philosophical debates on inconsistency makes precisely this mistake.

But if we do eliminate theory talk, then what are we talking about when we say a theory is internally inconsistent? Vickers' novel idea, and the most potentially fruitful thought coming out of the text, is to use six 'non-theory' questions to guide our philosophical investigations. Methodologically, the first step is to identify all the supposedly inconsistent propositions in what was previously called a 'theory' in science. The next step is to ask the following questions (and I list them in full because they each play an important role in his project):

1. Is the set of propositions really inconsistent?
2. In what sense are the propositions historically relevant? What evidence is there that these propositions played a genuine role in the relevant history of science?
3. What propositional attitude(s) did the relevant scientific community have to the propositions in question?
4. How did/do scientists reason with and draw inferences from the propositions? How was/is logical explosion avoided? [logical explosion is deriving any new 'truth' from a contradictory claim that 'p and not p' by the use of the addition rule in standard propositional logic]
5. How do the propositions tie together? What scientific work do they do *as a group*?
6. What is the relationship between the logical, mathematical, and physical content of the propositions?

These questions structure his approach throughout the rest of the book. I do not have space here to analyze their justification, but I will just say that Vickers' defense of each question is something along the lines of, 'well, this is what we really want to know, regardless of the term "theory" so let's just stick with this particular question'. I enthusiastically endorse this move.

His motivation for these questions is fairly obvious: By addressing each, the possibility of disagreements in the philosophical (as well as scientific) community about the concept 'theory' will not play an influential role in discussions over inconsistency. There is also the added benefit that philosophers will have to list very explicitly the propositions they find problematic. Consequently, dialectical clarity will be improved. To accomplish this will require a much needed and lamentably underrepresented, serious contact with the real history of science, something for which Vickers strongly advocates. In each of his chosen case studies then, Vickers pursues a clear answer to each of the above questions. In doing so, he often comes to the surprising conclusion that there never really was any inconsistency in the first place. This result makes his work novel in a substantial philosophical sense, and if we use his case studies as a test, then there is a reason to think his method successful. To give you a flavor of how this works for each case study, here is a brief overview of his answers to just the *first* question.

Bohr's theory of the atom: Here, Vickers finds there are four apparent but only one genuine inconsistency. This genuine inconsistency is Pauli's derived contradiction between Bohr's postulates and the adiabatic principle which tells us 'two systems are adiabatically related if the second can be achieved by taking the first and changing a certain parameter infinitely slowly and smoothly' (64). This inconsistency went unnoticed for almost a decade because it took a rather long and complicated derivation, which no one but Pauli attempted. The other inconsistencies are revealed as only apparent when one proceeds through questions 2–6.

Classical electrodynamics: There is genuine inconsistency here, as identified by Matthias Frisch, but it is not serious. Frisch has pointed out that on a particular interpretation of the Lorenz Force Equation, a charged particle experiences no force due to self-fields, but according to classical electrodynamics when accelerating, a particle *does* experience a force due to self-fields. The inconsistency [$\mathbf{F}_{\text{self}} = 0$ and $\mathbf{F}_{\text{self}} \neq 0$] seems dramatic, but Vickers points out it is much less so once we appreciate that scientists are perfectly aware of when to draw inferences from only approximately true assumptions. They do this all the time when using approximations and idealizations, and no scientists are so ignorant as to ignore conceptual and empirical conflict with their explicit assumptions—as if they were simply mechanical deductive machines churning out calculations without regard of fit between conclusions and assumptions. So, what we get from this case study is Vickers pointing out that yes, sometimes there is genuine inconsistency in science, but in this case, it is innocuous.

Newtonian cosmology: There are four genuine inconsistencies here, but Vickers argues that they depend upon much more subtle factors than recent philosophers have appreciated (especially John Norton and David Malament). The inconsistencies arise from different methods of answering the question 'what is the net gravitational force on a given test particle at an arbitrary place in the universe?' Vickers argues that some of the inconsistencies that arise from these methods are not historically relevant to Newtonian cosmology, while the last was overlooked because it required a deep appreciation of the nature of a divergent series in mathematics, and appreciating that took until well into the nineteenth century.

The early calculus: The inconsistency here is at best a result of a very specific 'instrumentalist' interpretation of how to use infinitesimals in performing a derivation. The contradiction is that of adding an infinitely small amount to a specified value, while also treating the result as of zero difference. Vickers finds that neither Newton nor Leibniz were inconsistent in their reasoning, though perhaps

Johann Bernoulli was. Still, finding just one individual committed to inconsistent assumptions does not an inconsistent theory make, as so many have claimed for the calculus.

With these conclusions, one starts to see why Vickers is skeptical that a general theory of inconsistency in science can be developed—there just is not much to worry about even in the most prevalent cases.

This all seems quite sensible, and if Vickers is right in his analyses, we can pretty much turn our backs on the problem. Still, there are a couple of reasons one might be concerned with his treatment of these cases. The first is that, in his analysis of two cases, he is heavily dependent on secondary rather than primary sources. On Bohr's theory of the atom, one of only two cases of genuinely worrisome inconsistency, his historical work is largely dependent upon the relatively recent writings of Abraham Pais, Max Jammer, Bryson Brown, and Thomas Bartelborth, without direct analysis of the works of physicists laboring on the theory at the time. In Chapter five, the story is the same. The topic is Newtonian cosmology, yet most of the historical work is done by appeal to the writings of Stanley Jaki, Ivor Grattan-Guinness, Jordi Cat, and Derek (Tom) Whiteside. None of their work falls outside the twentieth century.

This heavy reliance on secondary historical resources is not so extensive in the chapters on electrodynamics and calculus, so it is clear that Vickers has the talent to do his historical work. In the former case, Vickers' argument is primarily concerned with a discussion between contemporary authors, but when it comes to calculus there is a lot of first hand deep historical work he has done very well. It is a little disappointing then that given his talents Vickers would not spend more time on his own historical analysis of the literature in those chapters on Bohr and Newtonian cosmology.

This point may perhaps seem unfair, after all the resources Vickers uses are well-established historical works, and as such, they are likely to be more reliable than could be expected from a philosopher of science. Even Vickers himself says, 'this is not history of science; it is, rather, historically informed philosophy of science. Thus, I will mostly make use of secondary sources.' (36)

But this is terribly disappointing in light of his questions 2, 3, and 4 above, all of which seem explicitly to call for careful historical analysis. It is precisely in light of his own methodology that he should *not* be leaning so heavily on secondary sources. There is then a certain tension between his espoused methodology and its execution.

My second criticism really is much weaker and again requests of Vickers more than might reasonably be expected: The text is a collection of supposedly common claims to inconsistency in the literature, yet they all come from the exact sciences (physics and mathematics). He recognizes toward the end of the book that there is a conflict internal to evolutionary theory, but *his generalization* that inconsistency is not rampant or harmful can itself only be substantiated when apparent cases elsewhere are addressed. There is then an irony in Vickers claiming throughout the book that inconsistency is not sufficiently rampant to be generalizable across science and at the same time drawing his evidence only from (really) one of the sciences.

I would, however, like to close on a positive note, one regarding the extension of Vickers' methodology outside the inconsistency debates. I see very strong potential use for theory eliminativism in the scientific realism debate. This is a historically sensitive debate, especially when one considers the antirealist challenge posed by the pessimistic meta-induction: Past theories in science have mostly been false, so our best current theories are most likely also false. The challenge depends on case studies from the history of science, detailing theories which have been largely successful, yet turned out to be way-off the mark. The current debate only takes a few cases seriously (phlogiston, caloric, luminiferous ether), but those cases are difficult to answer. I think philosophers of science here too could benefit from adopting theory eliminativism if for no other reason than that it will force them to look very seriously at exactly which propositions led scientists into error. Indeed, since publishing the book, this route is being pursued by Vickers, and one can now see on his web site the description of a new project investigating potential further cases of successful but false science using his method. I strongly encourage and see great potential for this work and, given the high standards of his current publications, we can all look forward to seeing what Vickers and his colleagues come up with next.