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Essay Review

The slaying of the iMongers

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Understanding Inconsistent Science, P. Vickers. Oxford: Oxford University Press, 2013, 273 + viii pages, index, ISBN 9978-0-19-969202-6, cloth, price: 40,00 Euro.

Bohr's Theory of the Atom, the Theory of Classical Electro-Dynamics, Newtonian Cosmology, the Theory of Infinitesimals (Early Calculus), Aristotle's Theory of Motion, Olbers' Paradox, the Classical Theory of the Electron, Kirchoff's Theory of Diffraction: by far the most prominent (if not the only) cases of allegedly *inconsistent scientific theories* mustered so far from the staggering total amount of all scientific texts of past and present—recently babtised the *substratum of science*, by S. Psillos and R. Hendry. One case from mathematics invented for application in physics (to describe motion), and seven cases from physics. P. Vickers' expanded PhD Thesis *Understanding Inconsistent Science* is not the first book devoted to purported cases of 'inconsistent science', but it is the first monograph on the subject-matter by a single author. Perhaps it is also the last one, because Vickers concludes (p. 217, *spoiler alert!*, my italics):

(i) Many of the examples of 'inconsistent theories' or 'inconsistent science' in the philosophical literature *are not sensibly reconstructed as such at all*, and,
(ii) Of the remaining examples, most *are not interesting or important inconsistencies* from the point of view of 'how science works' and the philosophy of science generally.

Thus Vickers' title is misleading: there is nothing to understand! A sobering if not shattering conclusion for those philosophers and logicians who have craved for, relied on, hinted at or watched over inconsistent science (henceforth: iMongers). This reviewer has the strong impression that the iMongers are non-existent in science and form a tiny minority in philosophy. Among the iMongers we find philosophers of science who know what logic is (I. Lakatos, J. Meheus, M. Colyvan, O. Bueno, J. D. Norton, Vickers' thesis supervisor S. French) as well as logicians (N. Da Costa, B. Brown, G. Priest). How have they succeeded to convince themselves of the existence of inconsistent science when the deductions leading to scientific contradictions are, in fact, flawed, and if not flawed, turn out to be queer quirks at the periphery of science? This reviewer will refrain from speculating for answers to this daunting question about the credulous iMongers; rather he will attempt to summarise how Vickers has reached his conclusions in the eight cases listed (and will be critical of Vickers' treatment of one case); then he will address Vickers' more general views, which occupy a significant part of his monograph. But first things first.

The analyses of Vickers are historically well-informed, sometimes repetitive, exhaustive as well as exhausting, not seldomly leading to turtle prose—presumably motivated by a misunderstood principle of charity that every single thought or interpretation that comes to mind, no matter how trivial and how hopeless as soon as it is stated, needs to be explored with care and caution, or by the idea that even freshmen and freshwomen must be able to read the monograph without effort. The advantage of Vickers' unfaltering care and caution is that he is neither easily convinced of inconsistency claims nor of their scientific importance whenever they come through. This attitude has paid off supercalifragilisticexpialidociously, as his

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conclusions (i) and (ii) cited above testify. The first four cases in the opening sentence of this Review occupy four respective Chapters, the last four cases are treated in a single Chapter; these five Chapters are sandwiched between an Introductory Chapter plus one on concepts and methods (in total not less than 36 pages), and a long final Chapter (of nearly the same length), which ought to have been called 'Elaboration and Comparison', rather than 'Conclusion'. The inquiry into the eight cases occupies a little more than only two-thirds of the book; we turn to them now.

1. *Bohr, Pauli*: One of the postulates of Bohr's theory of the atom (1913) was that electrons in uniform circular motion around their nuclei do not radiate. An implication of Classical Electro-Dynamics (CED) is that accelerating charges radiate. Contradiction. Between Bohr's theory and *another* theory, that is, not an internal contradiction in Bohr's theory. Bohr saw sharply that precisely the mentioned implication of CED prevented an explanation of the stability of atoms, so he wisely abandoned it on the atomic scale. (Generally speaking, a new theory that is supposed to succeed where an accepted theory fails will always contradict the accepted theory somewhere. If the new theory were consistent with the accepted theory, how could it ever succeed where the accepted theory fails?) A second threat of inconsistency were Bohr's *discrete transitions* of an atom, when one of its electrons in an atom moves to another orbit, and the atom emits or absorbs a photon. Newton's law of motion yields differentiable and hence continuous trajectories for material bodies and particles. However nowhere did Bohr even suggest that the trajectories of electrons had to be described by discontinuous functions (Bohr did not use Newton's law of motion). The jumps from orbit to orbit could be jumps like those of athletes: fast and smooth, rather than Heaviside step-functions. So even if there had been a contradiction here, it would, again, have been one between Bohr's theory and another theory (Classical Mechanics, CM). The third and last smell of inconsistency comes from Bohr's *discrete energy levels*. This would not even yield a contradiction with another theory (CM), because CM allows for discrete energy levels, as Vickers points out while playing the violin—the kinetic energy of the strings vibrating in different modes (leading to a quantised wavelength and frequency) is also discrete. Bohr's Theory of the Atom was definitely not inconsistent.

Yet there was an inconsistency elsewhere in 'atomic physics'. In 1926, Pauli showed that there was a conflict between (a) Ehrenfest's Adiabatic Principle (two physical systems are adiabatically related iff one can be obtained from the other by varying some parameters smoothly and slowly), and (b) the spectroscopic rule that 0 is a physically impossible value for Sommerfeld's magnetic quantum number m, because the electron would swing back and forth *through* the atomic nucleus ($m > 0$). For unknown reasons, Vickers refuses to explain Pauli's argument (p. 66). The reader better reads German, then. The conflict evaporates as soon as one prohibits the parameters to which the Adiabatic Principle is applied to run over physically impossible values, e.g. $m = 0$; such restrictions on atomic numbers were at the time already accepted, think of atomic number k, which cannot exceed Bohr's principal atomic number n ($k \leqslant n$). But even this plausible obliteration of the inconsistency was not needed, Vickers notes, because in 1926 quantum mechanics emerged and quickly superseded the Old Quantum Theory with its prohibitions, pictures and principles (p. 70). As soon as the inconsistency appeared on stage, it could easily have been dealt with, but it simply left the stage and never returned again.

2. *Frisch*: The most sensational inconsistency claim is about CED, in 2004 defended by M. Frisch, in a paper entitled 'Classical Electrodynamics is inconsistent' (p. 76). Anyone with basic knowledge of physics would take that as the claim that Maxwell's equations are inconsistent, perhaps extended with Lorentz's Force Equation for the total force that the electro-magnetic field exerts on a particle having charge q and moving with velocity $\mathbf{v}$[1]:

$$\mathbf{F}_\mathrm{L} = q\mathbf{E} + \frac{q}{c}\mathbf{v} \times \mathbf{B}.$$

Frisch throws in further energy conservation in charge–field interactions, the existence of accelerating charges and Newton's Law of Motion, and allegedly deduces a contradiction. Vickers follows suit. The reason for the inconsistency, as Vickers makes clear (p. 83), is that Frisch knowingly ignored the contribution to $\mathbf{F}_\mathrm{L}$ of the self-field of the moving charge (thereby setting effectively $\mathbf{F}_\mathrm{self} = \mathbf{0}$), whereas Maxwell's equations entail that $\mathbf{F}_\mathrm{self} \neq \mathbf{0}$ due to the generated self-field. A debate ensued, not always about what moved Frisch to do a thing like that—in retrospect it should have been only about precisely that.

Full disclosure: I was involved in that debate (Muller, 2007). Besides revealing tacit assumptions that Frisch made for *his* deduction (not reported by Vickers), my criticism was essentially that setting $\mathbf{F}_\mathrm{self} = \mathbf{0}$, we are no longer talking about CED, because we are then accepting the negation of an implication of Maxwell's equations. G. Belot also responded to Frisch's claim and charitably asserted that there are two 'interpretations' of CED, of Lorentz's Force equation in particular: [i] $\mathbf{F}_\mathrm{L} = \mathbf{F}_\mathrm{ext}$ and [ii] $\mathbf{F}_\mathrm{L} = \mathbf{F}_\mathrm{ext} + \mathbf{F}_\mathrm{self}$. Vickers agrees. Since I took CED[ii] for granted, I was changing the subject and therefore Vickers dismisses my criticism of Frisch, who meant CED[i] (p. 85). In a note added in proof, I responded to Belot by flatly denying there are two 'interpretations' of CED and I still flatly deny it. Why? Not because CED[i] is inconsistent—although that seems to be a good enough reason to spurn an interpretation—but because it makes no sense.

Imagine an accelerating charge at space–time point p (in any state of motion for that matter) and an electro-magnetic field also at space–time point p. That field is the superposition of all fields produced by all sources present, including the charge itself, and that total field exerts a Lorentz-force $\mathbf{F}_\mathrm{L}$ on the charge. The charge does not decompose that force in $\mathbf{F}_\mathrm{ext}$ and $\mathbf{F}_\mathrm{self}$ embraces only $\mathbf{F}_\mathrm{ext}$ to let it cause a change in its motion, and decides to ignore $\mathbf{F}_\mathrm{self}$. Picky and choosy charges about how to decompose the electric and magnetic field vectors, and for which components of them to be susceptible and for which to be impervious make absolutely no sense. CED[i] is not a possible 'interpretation' because it makes no sense. Even when authors on CED, like Landau and Lifschitz, J. D. Jackson and Feynman write initially that $\mathbf{F}_\mathrm{L} = \mathbf{F}_\mathrm{ext}$, they all further on acknowledge they have ignored $\mathbf{F}_\mathrm{self}$, as Below has pointed out, cited by Vickers (p. 99). Eventually they all 'interpret' CED as CED [ii]. No one 'interprets' CED as CED[i]—except lone wolf Frisch, who therefore was, in fact, the one changing the subject when talking about CED. Why Vickers follows Frisch in this nonsensical 'interpretation' is a mystery.

But when CED *is* CED[ii], whilst in 99% of the applications authors set $\mathbf{F}_\mathrm{self} = \mathbf{0}$, as Vickers estimates (p. 89), do authors then not *use* CED[i] and do they, then, not *use* an inconsistent theory after all, as Frisch would have it? No. When authors write '$\mathbf{F}_\mathrm{self} = \mathbf{0}$', this *means* '$\mathbf{F}_\mathrm{self} \approx \mathbf{0}$', this *signals* they are going to ignore the contribution to $\mathbf{F}_\mathrm{L}$ of $\mathbf{F}_\mathrm{self}$ in calculations (because it is hilariously tiny). *There is no contradiction between* '$\mathbf{F}_\mathrm{self} \approx \mathbf{0}$' and the implication '$F_\mathrm{self} > \mathbf{0}$' of Maxwell's equations. Ironically, Vickers (p. 53) cites this reviewer writing that "Physicists are notoriously sloppy in this respect: a majority of the exact equality signs (=) in most physics papers, articles and books mean approximate equality (≈)." But the full implications of this remark seem to have been lost on Vickers (as well as on Frisch), for he defends this alleged inconsistency of CED and elaborates on its significance.

[1] The outer-product sign '×' is in the book almost everywhere amateurishly replaced with a boldfaced letter '**x**'. Oxford University Press doesn't know the difference? Another slap for the Press: the Index is embarrassingly incomplete.

Vickers has not taken the substratum of science seriously enough, in contradiction to his own ostentatious pledge that he will do so (pp. 22–25). Occasionally it all seems to dawn upon Vickers (p. 106), but before the insight breaks through full force, Frisch's clout pulls Vickers back to the dark side.

Fairness demands to report that in 2008, Frisch confessed that the inconsistency was less important than he had originally thought, and weakened his claim that CED is inconsistent to the inconsistency of CED-based models of accelerating charges where energy is conserved (p. 107). Recently however Zuchovski (2013) advanced a proof of M.K.H. Kiessling of 1999 that within the confines of CED a fully relativistic description of accelerating extended charges of arbitrary shape is consistent with energy conservation, thereby refuting also Frisch's weaker inconsistency claim. We hope that some day Frisch will return to us from the dark side. May the self-force be with him.

3. *Seeliger*: The German astronomer Hugo Seeliger was the first to point out, in 1895, more than 200 years after the appearance of Newton's *Principia*, that 'Newtonian cosmology' is inconsistent. When we assume that matter is on average homogeneously distributed over Euclidean space, then the calculation of the net gravitational force on a test-body anywhere in space spells trouble. Depending of the method of calculation, the answer varies, if an answer is obtained at all. Two out of six Contradictions considered by Vickers can be deduced convincingly from different assumptions: (C2) the average force on two widely separated test bodies differs significantly and does not differ significantly; and (C5) there is a unique gravitational force on a test body and there isn't. One reason why no one noticed these contradictions (C2) and (C5) earlier lies in the absence of scientific cosmology for centuries—it arose in the 20th Century, after Hubble's and Einstein's discoveries. Therefore no one asked the question about the mentioned net gravitational force on a test body. No one was committed to the various assumptions that go into the relevant deductions, and therefore no one would have felt committed to the ensuing contradictions. Another reason is that there was initially little appreciation of conditionally converging (i.e. diverging) series, so that no one was committed to an inference of there being no solution from indeterminacy (p. 143), which is however needed to derive contradiction (C5), and therefore, again, no one would have felt committed to this contradiction. In recent years, philosophers of physics J. D. Norton and D. B. Malament brought Seeliger's 'inconsistent Newtonian cosmology' to the stage of philosophy of science. Its historical relevance is however nil, Vickers soberly concludes. Not a case of inconsistent science.

The reviewer found this Chapter the most exhausting one to read. Instead of endless groups of propositions leading, or not leading, to some contradiction, occasionally tiresomely slowly explained (pp. 126–127), and instead of another displayed version of a conditionally converging series, the reviewer would rather like to have seen more mathematical physics.

4. *Newton, Leibniz*: The Early Calculus employed infinitesimals and infinituples (Leibniz), and fluents and fluxions (Newton). The very idea of 'infinitely small numbers', smaller than any number and yet not vanishing, evoked questions. Berkeley's famous critique *The Analyst* (1734) made these questions urgent. Vickers distinguishes between (I) the method and (II) the foundations of the Calculus. (I) Since the method always gave the right results, not a single soul doubted its efficacy, let alone wondered whether the method was somehow 'inconsistent'. Vickers tries to make sense of what 'inconsistent method' would mean (pp. 154–155). A method that gives no results, ambiguous results, several incompatible results, the wrong result? The Calculus unambiguously gave always the one correct result.

(II) Both Newton and Leibniz elaborated on the foundations of the Calculus in various writings. Both rejected the very idea of there being infinitesimals; both considered these as convenient fictions in the efficacious method of the Calculus. Newton spoke of 'convergence' and 'limits'; Leibniz spoke of differences getting smaller without end. Both did not reach the rigorous definitions of convergence, limit and differentiability that Cauchy and Weierstrass would reach more than a century later. But they did come awfully close.

Vickers, proudly following Kuhn's historical turn in the philosophy of science, relies heavily on historical scholarship in this Chapter. Sometimes, when it comes to (II) the foundations, he could have probed more deeply, by using modern formulae and less prose—fear of being Whiggish may block *our* understanding of the past. He prefers lists of quotations from the primary and the secondary literature and tops it off with the excuse that he wants "to give the reader a sense of the primary literature" (p. 163). In spite of this drawback, the reviewer found this Chapter the most enjoyable and informative of the entire book. Vickers approaches the subject with his care and caution and provides a nice overview of historical scholarship on the Early Calculus. Before we forget: the charge that what Newton and Leibniz wrote about (II) the foundations of the Calculus is inconsistent peters out as Vickers proceeds. The foundational writings of Newton and Leibniz admittedly are unsatisfactory by current standards of rigour and clarity, but they were pointing in all the right directions. Inconsistent they were not. In the closing Section of this Chapter, Vickers castigates M. Colyvan and G. Priest for having make stark assertions about the alleged inconsistency of the Early Calculus: "all of this is extremely contentious if not plain false." (p. 190).

5. *Aristotle, Galilei*: Who does not know Galilei's wonderful thought-experiment about the falling connected bodies? According to Aristotle's kinematics, the heavier body should fall faster and slower than the composite body. Aristotle was certainly committed to (A1) speed being proportional to weight, and to (A2) the additivity of weight, but not to the assumption that (G) speed is mediative. Therefore Galilei's charge against Aristotle's Theory of Motion is off target. Aristotle could have responded that (A1) only applies to composite bodies, not to *parts* of falling bodies, thereby blocking Galilei's inconsistency argument. By considering a lighter and a heavier *part* of any body one could reach the same contradictory conclusion. Galilei provided a *reductio* argument of (G) for Aristotle. Thanx. Vickers mentions more ways out for Aristotle that would have been perfectly consistent with (A1) and (A2). Aristotle's Theory of Motion has not been proved inconsistent.

6. *Olbers' Paradox* (1823) is about the dark night sky that should have been bright if stars are homogeneously distributed in infinite Euclidean space, because in which ever direction one looks, the eye meets an eternally shining star. To get a rigorous deduction of a contradiction, several premises are needed, Vickers submits (p. 198), and even then we have an argument bereft of any scientific significance, because no scientist known to historians of science underwrote all the premises needed to get a contradiction. The contradiction is obtained by adding 'The night sky is dark' as a *premise*! (p. 198). This is not a case of a logically inadequate set of premises but a case of an empirically inadequate set of premises. What moved Vickers to include Olbers' Paradox in the monograph?

7. *Lorentz, Abraham*: The classical theory of the electron, the first elementary particle, is one within the confines of CED. (Why was this problem not treated in the Chapter on CED? Vickers does not tell us.) Lorentz modelled the electron first as an extended tiny rigid body, because a point-electron would have an infinite amount of potential (electro-static) energy. This is not yet in contradiction with the postulates of CED, but it is in contradiction with $E = mc^2$ of the Special Theory of Relativity (STR): the electron would have infinite inertial mass, whereas this tiny speck of

matter has a tiny mass. Everything that contains electrons would then presumably also be infinitely *heavy*, and it would require infinite forces to change its state of motion. All this was too much to swallow for physicists. Conclusion: point-electrons are out. Treatments of moving *rigid bodies* in agreement with other exigencies of STR suffer however also from notorious difficulties. On top of this, how to understand that an electron does not explode due to the repelling forces of its parts? At some point, Poincaré postulated compensating stress forces. Lorentz frowned. Minkowski is quoted having written that "approaching Maxwell's equations with the concept of a rigid electron seems to me like going to a concert with your ears stopped up with cotton wool." (p. 206). A group of propositions can be collected from the substratum of science that inevitably gives rise to a contradiction. Therefore no one accepted all of them. The physicists in those days were fully aware of the difficulties and tried to resolve them. No resolution lasted. With the advent of quantum theory, it was game over for CED-models of electrons, atoms and molecules. Yet the subject never faded away entirely in the past substratum of science. The idea however that physicists of past or present are using or accepting an inconsistent CED-model of the electron is, again, plain false.

8. *Kirchoff* derived in 1882 an empirically successful formula for the amplitude of light at points behind an aperture in an infinitely thin screen through which the light passes. Vickers presents Kirchhoff's assumptions and his derivation, although regretfully he does not present the derivation of a crucial integral theorem that Kirchoff employs. Then Vickers reports that in his *Théorie mathématique de la lumière* (1892), Poincaré pointed out that Kirchoff's formula gives amplitudes in the aperture and just behind the screen in conflict with Kirchoff's assumptions. For unknown reasons, again, Vickers refuses to delve into Poincaré's contradictions. The reader better reads French, then. The simple resolution is to restrict the domain of application of Kirchoff's formula. Approximation and idealisation are held responsible. As soon as they enter the picture, Vickers contends (p. 213), all bets are off when it comes to inconsistency claims. (The reviewer wishes Vickers would have concluded the same in the Chapter on CED.)

So much for the case studies. In the illuminating Section 8.2 of the final Chapter 8, Vickers draws lessons. One is that in nearly all cases a specific scientific question is the catalyst of the alleged inconsistency. Another one is that there are interesting resemblances and differences between the various cases, neatly spelled out by Vickers.

The logic-content debate is about whether scientists, when hitting a contradiction, should replace classical logic with some paraconsistent logic or should replace some of the assumptions that lead to the contradiction. (Paraconsistent logic is pampering a few contradictions while hampering the deductive apparatus in order to prevent explosive self-destruction by *ex contradictione quodlibet sequitur*.) Vickers champions the content side and buries the iMongers for the simple reason that "it is hard to see what understanding might be gained by such a [paraconsistent, FAM] reconstruction" (p. 238). If contradictions between hypotheses, models and theories were on one hand, and phenomena on the other hand, or *within* hypotheses, models and theories, were no longer good reasons to adapt, change or replace these hypotheses, models and theories, but rather contradictions were pampered, then science would seem to halt. Enemies of science they are, these iMongers.

Finally we turn to an important side issue that Vickers expounds, which is the 'method' of *theory eliminativism* (p. 28): stop using the concept of a theory and stop mentioning the word 'theory'. Vickers' closing sentence of the book foretells "a quite startling transformation of philosophy of science" (p. 253) when we adhere to theory eliminativism. The reviewer agrees. Shivers run down his spine already. But is Vickers not inquiring mainly into inconsistent *theories* as we find them in the substratum of science? Does his ostentatious pledge to the substratum of science not command him to speak of *theories* whenever scientists do?

Vickers avers that going into the philosophical issue about the nature of scientific theories would not have been helpful, rather it would have hindered his inquiry into inconsistent science. Instead Vickers prefers to speak of *sets of pointedly grouped propositions* (SOPs), propositions grouped together by scientists for a point, such as describing or explaining a phenomenon, or solving some recognised scientific problem. They are found in the substratum of science. When the assumptions leading to an inconsistency do not form a SOP, the inconsistency may be dismissed as scientifically otiose. Bohr, Pauli, Frisch, Galilei and Kirchoff clearly have a SOP; the other cases have not. If the assumptions do not form a SOP, then Vickers asks whether most or some scientists were doxastically committed to them, call them *sets of doxastically grouped propositions* (SODs). If not, then again the derived inconsistency may be dismissed as scientifically otiose. Aristotle, Olbers, Lorentz and Abraham neither had a SOP nor a SOD. They may have had something weaker still: sets of propositions jointly entertained by scientists (SOEs, p. 29). Perhaps only Seeliger had a SOE, and he was the only one having that SOE. Newton, Leibniz and Aristotle did not even had a SOE.

Vickers' inquiry into inconsistent science by means of his SOPs and SODs makes the question whether these belong to a single theory, or even define a theory for many or some scientists, of little importance. What the reviewer fails to grasp is how wisely bracketing the issue about the nature of scientific theory when inquiring into inconsistent science, and playing down the importance of the issue whether the SOPs or SODs belong to, or define, a theory or not, entails that theories should be eliminated from the philosophy of science. One advantage according to Vickers is that it prevents confusion and miscommunication about *theory* (pp. 29, 253). Should we adopt *truth eliminativism* in epistemology in order to prevent confusion and miscommunication about truth? Should we adopt *value eliminativism* in ethics to prevent confusion and miscommunication about values? Should we adopt *reality eliminativism* in metaphysics to prevent confusion and miscommunication about reality? Surely this is a road to nowhere. Or a road to the end of philosophy altogether. A "startling transformation" indeed. When we exercise care and caution, confusion and miscommunication will also be prevented.

Another virtue of theory elimination Vickers perceives is that focussing on *theory* may distract from the content of the issues at hand. Really? Are philosophers that decrepit and dupable that they let themselves be distracted so easily from the issues at hand? Again, when care and caution are exercised, philosophy is going to be just fine.

How about the virtues of having and keeping, thus not eliminating theories? Will that have something to do with why we have theories in the first place? Vickers passes over in silence.

All in all this poorly motivated patronising 'method' of theory eliminativism, which has no virtues that cannot be had by exercising care and caution, casts an unnecessary shadow against a necessary inquiry into allegedly inconsistent science, which Vickers has executed with care and caution, and which has earned him the honourable title: Slayer of the iMongers.

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