Theory Flexibility and Inconsistency in Science[1]

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ABSTRACT

For several decades now philosophers have discussed apparent examples of internally inconsistent scientific theories. However, there is still much controversy over how exactly we should conceive of scientific theories in the first place. Here I argue for a new approach, whereby all of the truly important questions about inconsistency in science can be asked and answered without disagreements about theories and theory-content getting in the way. Three examples commonly described as 'internally inconsistent theories' are analysed in the light of this approach. In the process, the question 'Is the theory inconsistent or not?' is identified as a bad, or at least unimportant, question.

> In physics it is usual to give alternative theoretical treatments of the same phenomenon. We construct different models for different purposes, with different equations to describe them. Which is the right model, which the 'true' set of equations? The question is a mistake. One model brings out some aspects of the phenomenon; a different model brings out others. Some equations give a rougher estimate for a quantity of interest, but are easier to solve. No single model serves all purposes best.
>
> Cartwright 1983, p.11

[1] Forthcoming in 'Is Science Inconsistent?', a special issue of *Synthese*.

1 Introduction

Most often, when a big deal is made of finding an inconsistency in science, it is an inconsistency internal to a scientific theory. Galileo (in the early 17th century) describes Aristotle's theory of motion as inconsistent; Berkeley (1734) describes the early calculus as inconsistent; Seeliger (1895) describes Newtonian cosmology as inconsistent; Lakatos (1970) describes Bohr's theory as inconsistent; Frisch (2005) describes classical electrodynamics as inconsistent. Each of these was meant to be a remarkable and important discovery, because it showed that the theory was wrong and/or because it showed that inconsistency does not prevent a theory from being successful and perfectly useful.

However, at one time or another all of these claims have been questioned (eg. Schrenk 2004, Malament 1995, Bartelborth 1989, Belot 2007). This might come as a surprise: why can't we simply agree on what the content of the theory in question is, and then see if a contradiction follows or not? Of course, the question of *what follows* from a theory may well be a difficult question in certain cases, something we can't readily agree upon.[2] However, here I want to argue that there is a second and more serious reason why we sometimes find it difficult to agree. The problem, in short, is that we can't agree on which set of 'things' constitute 'Newtonian cosmology', 'classical electrodynamics', and the rest. We'll see in the forthcoming case studies particular examples of disagreements about whether some theoretical constituent (equation/model/proposition) should or shouldn't be considered 'a part of the theory'. On this issue an option that hasn't been sufficiently explored is the possibility that, at least sometimes, *there is no fact of the matter*. Perhaps theory-names—such as 'classical electrodynamics'—can be sensibly and legitimately used in different, contrary ways. And if so it is a short step to admit that the theory itself (whatever ontological story we want to tell here) can be sensibly, legitimately identified in different, contrary ways. In this way theories can perhaps be described as 'flexible'. At least, our use of the word 'theory' is flexible, I want to claim.

[2] For example, does the Dirac equation entail the existence of positrons or not? Does Newtonian cosmology entail an indeterminate gravitational force on the earth or not (Vickers 2009)? And beyond such 'material' conundrums of entailment, there are even disagreements as to which *logical* inferences should be permitted.

However, my discussion will not depend on this (no doubt controversial) claim. Instead I will present an approach which is consistent with theory flexibility, but *also* consistent with the usual, 'static content' view of theories. This should satisfy everyone. However, it may seem impossible: if theory-content is static then there will always be a fact of the matter as to whether a theory is inconsistent or not, but if it is flexible then—at least sometimes—there won't be.[3] But this apparent impossibility can be overcome by making some changes in the way we talk about science. The approach will be presented and partly defended in §2, with further defence consisting in applying the approach in the case studies to follow. These case studies come in §§3, 4, and 5: (i) Bohr's theory of the atom, (ii) classical electrodynamics, and (iii) Kirchhoff's theory of the diffraction of light. In each case I will aim to show that one can ask and answer all of the truly important questions about inconsistency in science without worrying about the precise content of these theories, and so without worrying about whether each theory is *really* inconsistent or not. §6 is the conclusion.

2 Theory flexibility

The importance and ubiquity of scientific theories as units of analysis in science and the philosophy of science has long been recognised. Thus, over the last eighty years, many have asked 'What is a scientific theory?' As is well known, in the 1930s and 40s logical positivists and empiricists drew on recent developments in logic and mathematics, analysing theories syntactically, as sets of statements and their logical consequences. When serious difficulties for this programme presented themselves in the 1960s a new paradigm emerged, with theories analysed 'semantically', as families of models. The latter approach, in one form or another, is still very much favoured throughout the community, although there are continuing debates over how exactly we should conceive of the 'models'.

[3] This assumes that we have settled on a definition of 'inconsistent', of course. See Vickers (forthcoming b), Chapter 2, for discussion.

However, two very different levels of theory-identity have recently been emphasised by Hendry and Psillos (2007). They write,

> Both standard views [syntactic and semantic] have been comrades in their attempts to rationally reconstruct scientific theories. Where they differ is in the tools they use… [T]he standard views have alike aimed at *rational reconstruction*.
>
> We do not want to doubt the usefulness of (moderate) formalization and reconstruction. But we should not lose sight of the fact that they *are* reconstructions, or mistake their products for the theories themselves. (p.159, original emphasis)

In short, the distinction between 'theories themselves' and the products of the syntactic and semantic approaches is described by the authors as one between 'unreconstructed' and 'reconstructed' theories. The distinction is made especially clear if we ask the following question: if one wants to reconstruct quantum mechanics (say), either syntactically or semantically, how does one decide upon *what quantum mechanics is* in order to begin one's reconstruction? Whatever theoretical content one decides to start with, this is what one might call the 'theory itself', the 'unreconstructed theory'.

Another way to make the same point might be to simply ask the question, 'How should we decide upon the content of a given, particular, named theory?' This question has to date been given very little attention in the literature.[4] In practice, philosophers of science draw on their intuitions about the kind of thing scientific theories are, what they are supposed to *do*, and use this to decide upon the theory's content (or else they look to a book or article where somebody else has already done this). But, since different individuals draw on different academic backgrounds and perspectives, this leads to various different decisions on theoretical content, and deeply grounded disagreements.

One way forward would be to begin the necessary conceptual analysis. One could take pains to define what a theory is in terms of necessary and sufficient

[4] Of course, some might claim that the question is not independent of the syntactic and semantic approaches, since the content of a theory does not 'exist' until it is reconstructed by somebody, be that a scientist, historian, or philosopher. This issue won't affect the point I will be making in this paper.

conditions, starting, perhaps, with '*T* is a theory if and only if *T* is a proposed explanation of a certain specified phenomenon or domain of phenomena'. After considering various counterexamples we might end up with a more sophisticated version of this which became widely accepted. One could then consider a theory like quantum mechanics in the light of this definition, and attempt to put together the theoretical constituents which do exactly what a theory should do and no more. This could then be accepted as the content of quantum mechanics.

Anybody with a rudimentary knowledge of developments in conceptual analysis will know that such an approach would be hopeless (or else impossibly difficult). First of all the classical theory of concepts, where concepts such as *scientific theory* are defined in terms of necessary and sufficient conditions, has now been completely rejected in the face of a long string of serious objections. Instead, modern philosophers debate the pros and cons of various different theories of concepts, including prototype theory, theory-theory, neo-classical theory, conceptual atomism, and neo-empiricism. Thus, one would first have to engage with the literature on these theories of concepts and make a decision as to which to apply to *scientific theory*. This is difficult enough in itself, but even then, it isn't at all clear that the analysis would enable us to pick out the *content* of a given theory. Suppose it turned out that it is at least a *necessary* condition for theories that they provide explanations for certain given phenomena. If we don't agree on what exactly is meant by 'explanation' (and many people don't) we still wouldn't agree on what content to pick out for a given theory. The problem would just move one step along.

One thing we do know about concepts is that they can sometimes 'split', so that we end up with two or more different concepts coming under the same word.[5] This has often shown previously asked questions to be bad questions, because based on a false premise. For example, we know that 'Is water an acid?' is based on the false premise that there is just one, right way to define 'acid', so that there has to be a yes-or-no fact of the matter as to whether water is an acid. Another example is the concept *species*, which similarly has split into a number of different concepts (*biospecies*, *phylospecies*, etc.). Thus we might describe words like 'acid' and

[5] Cf. Piccinini and Scott (2006).

‘species’ as *flexible*: there are different, contrary ways in which the words can be legitimately and sensibly used. Using ‘acid’ in one way, water is an acid; using it in another, it isn’t.

A possibility which thus-far has not been seriously contemplated is that this also holds for the concept *scientific theory*. And if there are different, equally legitimate ways to think about ‘theories’, then there will be different, equally legitimate decisions on theory-content. If this sort of ‘splitting’ has happened for *acid* and *species*, then why wouldn’t we expect it for the concept of a theory? Especially when we consider just how varied the experiences of ‘theories’ can be for physicists, biologists, psychologists, economists, and so on. And perhaps there can be different, equally legitimate ways to think about ‘theories’ and particular theories even amongst physicists. In the context of criticising John Norton’s claim that ‘classical mechanics is indeterministic’, Wilson (2009) has put it as follows:

> Much contemporary commentary on philosophical theories of matter in the eighteenth and nineteenth centuries strikes me as greatly compromised by its inclination to assume that phrases such as ‘classical mechanics’ or ‘the Newtonian picture’ capture surgically precise meanings, when, in fact, such terminology can be readily applied to deeply incompatible doctrines … [W]e’re unlikely to find any wholly stable X upon which the phrase ‘classical mechanics’ can permanently and happily rest.’ (pp.174-5).

His favoured approach is to consider what he calls three different ‘species of classical mechanics’, and ask the question of whether each of *them* is indeterministic (and whether we should care). And indeed, he notes that these three species ‘split into further subdivisions as further questions are pressed.’ As he puts it, ‘Norton’s case nicely illustrates the care we must observe as we ponder the ‘content’ of ‘Newtonian mechanics’.’ (p.176). Taking inspiration from the epigraph we might say that we construct different formulations of a theory for different purposes. Which is the ‘right’ formulation of a given theory? The question is a mistake: no formulation serves all purposes best.

Suppose for now that this is the right way to think about theories. Then a theory can be inconsistent, and also not inconsistent. Or, better (and eliminating the apparent inconsistency at the meta-level), a theory might be sensibly identified such that it is inconsistent, and also sensibly identified such that it is not. What, then, will we want to say about the inconsistency? All that will really matter, in the end, is what we learn about science from the inconsistency *given the particular way in which the theory must be identified to make it inconsistent*. For example, we will want to say very different things if we end up with a group of inconsistent propositions all believed to be important candidates for the truth by the relevant scientific community, or if we end up with a bunch of inconsistent idealization assumptions. In the former case something will have to change, but in the latter it probably will not. The right way to proceed, as philosophers, is to ask in a given case why it matters, and what we learn, from the fact that *the particular theoretical constituents in question* are inconsistent. If we (perhaps charitably) allow them to be called 'the theory', or 'a formulation of the theory', then that will be neither here nor there.

Now we end up with an interesting situation. What we have is a strategy that should suit all sides. That is, if we approach inconsistencies in this way—*as if* theories are flexible—then all of the important questions about inconsistency in science can be asked and answered without thorny questions about theory-content playing any substantial role. Why is that? Well, suppose somebody claims that a particular, named theory is inconsistent. Then, whatever set of propositions their use of the term 'the theory' is meant to refer to, we can allow that usage, and just go on to ask the deeper, more important question: What do we learn about how science works from the fact that the particular propositions in question are inconsistent? We can ask and answer *this* question without bothering with the question of whether the use of the word 'theory' is appropriate. We can just accept that it is—accepting for the sake of argument that the word 'theory' is flexible enough to incorporate the attempted usage—and ask instead why (if) the particular inconsistency presented is interested or important.[6]

As mentioned, my claim will be that in this way one can ask and answer all of the truly important questions about inconsistency in science. Suppose one person says 'Theory X is inconsistent', and another disagrees. Then the approach will be to isolate the propositions which *are* inconsistent, and—allowing for the sake of argument that the term 'Theory X' can legitimately be used to refer to those propositions—ask the question of what we learn (if anything) about how science

[6] I'll talk about propositions in this paper, since these are the most obvious focus for inconsistency. One can substitute talk of equations, or models, or whatever one prefers.

works from the fact that those particular propositions are inconsistent. Disagreements about how we 'should' use the term 'Theory X' (even if there *is* a 'right way') are then rendered irrelevant. Instead we look at the propositions in question, the scientific work that they do (explanation? prediction?), the way the scientific community used and/or committed to them, and so on. In this way we find out if there is something interesting and important about the inconsistency, or not. And this is what matters, not the way in which one philosopher or another prefers to use his or her words.

This is only a whisper away from a certain kind of pragmatic eliminativism vis-à-vis 'scientific theory'.[7] One talks not in terms of theories, but instead in terms of the theoretical constituents scientists and philosophers use terms like 'theory' and 'classical mechanics' to refer to. Perhaps one would object that the theories are still there, and we're still talking about them, but just skirting the issue of what they are to make progress in debates about inconsistency in science. Well, perhaps, but I'm ultimately staying silent on this. In this paper I want to present a view that is consistent with as many other positions as possible. What I absolutely do want to avoid, though, is the sort of philosophy where one puts a lot of weight on the concept of a theory. For example, it has sometimes been claimed that a theory *is* or *is not* inconsistent (or indeterministic, or time-reversal-invariant, or whatever), as if that conclusion is an end in itself. The concept *theory* cannot cope with such an analytic burden, nor need we put such a burden on the concept of a theory. This is not helpful philosophy of science. What matters in any such circumstance is what we learn from the fact that *those particular theoretical constituents* (whether we call them 'a theory' or not) are or are not inconsistent/indeterministic/time-reversal-invariant or whatever. At least for debates about the properties theories have, there is just no need to put much—or even any—weight on the concept of a theory.

Perhaps the best way to argue for this approach is to show how it works in practice, and demonstrate what is gained in concrete cases. I will focus on three theories which have been labelled 'internally inconsistent scientific theories': Bohr's theory of the atom, classical electrodynamics, and Kirchhoff's theory of the diffraction of light.

[7] See Vickers (forthcoming a).

3 Example one: Bohr's theory of the atom

Bohr's theory of the atom has long been labelled an internally inconsistent theory. Ever since Jammer (1966) and Lakatos's seminal paper of 1970 which describes it as 'a research programme progressing on inconsistent foundations', Bohr's theory has been widely cited as the example *par excellence* of an internally inconsistent theory. For example, da Costa and French (1990) unambiguously speak of 'two contradictory propositions within … Bohr's theory of the atom.' (p.186). But when it comes to identifying the specific scientific content which constitutes the inconsistency important disagreements arise. And there are also some—Bartelborth (1989), Hendry (1993), and Hettema (1995)—who argue that the theory is not internally inconsistent at all. In addition at least some of those working at the time thought the theory to be consistent. Even as late as 1923 Rutherford was prepared to write,

> For the first time, we have been given a consistent theory to explain the arrangement and motion of the electrons in the outer atom. (In Kramers and Holst 1923, p.xi)

The strategy noted above dictates that we should proceed as follows. We accept for the time being that the term 'Bohr's theory' can—sensibly and legitimately—be used in these different, contrary ways, such that sometimes Bohr's theory comes out inconsistent and sometimes it doesn't. This is just to accept, for the sake of argument, that the term 'Bohr's theory' can tolerate a certain amount of semantic flexibility. This is harmless, because what really matters for questions about inconsistency is that we identify the propositions which really *are* inconsistent, and consider why (whether) that inconsistency is interesting or important from the point of view of philosophy of science. Within the literature one can identify three different aspects of the theory which are usually identified as the problem areas:

(a) The mysterious 'quantum transitions', as electrons 'jump' from one orbit to another;

(b) The non-emittance of radiation from a charged, orbiting electron;

(c) The non-classical character of the orbits coupled with the classical treatment of radiation interacting with an atom.

Aspect (b) is perhaps the most often discussed, so I will concentrate on that here.[8]

According to Bohr's postulates, in an atom electrons orbit the nucleus in certain 'allowed' orbits, and whilst in such orbits they do not radiate light despite being charged, accelerating particles. This was radical when it was introduced, since according to classical electrodynamics (CED) such a particle *must always* radiate light. In *Science and Partial Truth* (2003) da Costa and French start by focusing on the discreteness of 'allowed' electron orbits as a possible source of inconsistency, but then continue as follows:

> However it is not only in the discreteness of the states that we have conflict between quantum and classical physics but also in … the assertion that the ground state was stable, so that an electron in such a state would not radiate energy and spiral into the nucleus as determined by classical physics. *This* is the central inconsistency. (p.91)

The orbit closest to the nucleus, known as the 'ground state', was perhaps the most striking example of such a 'stable' orbit, but this peculiar departure from CED was part and parcel of every electron orbit in an atom, according to Bohr's theory. This is surely also what Lakatos was thinking of when he described the theory as inconsistent in 1970.

In order to reach inconsistency what we need here is the part of CED that dictates that accelerated charged particles must always emit radiation, along with the claim that electrons orbiting within atoms are accelerated charged particles which are *not* radiating. Now, it is hard to imagine the theory in any form which does not include the latter claim: one might argue that it is a *necessary* component, in every possible 'formulation of the theory', in the 'essence' or 'core' of the theory as Gould (2002) and Morrison (2007) might put it. But what about the former claim? Certainly

[8] In Vickers (forthcoming b), Chapter 3, I conduct a much more thorough investigation of this case study.

Bohr’s theory has often been described such that CED is included within it. Priest (2002) writes,

> Bohr’s theory … included both classical electrodynamic principles and quantum principles that were quite inconsistent with them. (p.122f.)

But why not reconstruct the theory so that CED is *external* to the other assumptions? Bartelborth (1989) writes,

> [T]he only necessary theory-element from classical electrodynamics for Bohr’s theory is quasi-electrostatics for point particles, because what Bohr really needed from classical electrodynamics was the concept of electric charge and Coulomb’s law. (p.221)

In other words, Bohr can at least manage to explain the spectral lines of hydrogen without appealing to the whole of CED. And Millikan took this approach at the time, writing,

> The radical element in it [Bohr’s theory] is that *it permits the negative electron to maintain this orbit* or to persist in this so-called ‘*stationary state*’ without radiating energy even though this appears to conflict with ordinary electromagnetic theory. (Millikan 1917, p.211f., former emphasis added)

However, in some contexts there might be good reason to put together the inconsistent assumptions in question. As Brown (1990) writes,

> [T]he radiation emitted by the atom is assumed to be describable in terms of classical electrodynamics (CED), while the emission and absorption processes, as well as the behaviour of electrons in stationary states, are accounted for in terms manifestly incompatible with CED. (p.285)

Of course, Brown is right here: scientists *had* to continue using CED in various contexts because they simply didn’t have anything else. And in some contexts they would use Bohr's theory to determine the frequencies of light emitted from an atom, and in the next breath make use of CED to think about the behaviour of that light, or perhaps to consider the light emitted from *free* electrons (not bound to a nucleus).

There might be a tendency here to say that on the one hand we have Bohr's theory, and on the other hand we have CED, and that this doesn't mean that Bohr's theory is inconsistent, but rather that we have two theories being used side by side which are mutually inconsistent. But to debate this is unnecessary: the approach introduced in the previous section reduces this debate to one about the use of words, instead of one about inconsistency in science. Let us accept—for the sake of argument—that the term 'Bohr's theory' can be sensibly and legitimately used to refer to the inconsistent assumptions in question. The next question is, 'So what?' What's interesting or important about this particular inconsistency? What do we learn about how science works from this inconsistency? For example, did scientists really believe these inconsistent claims? How did they reason with inconsistent assumptions without deriving anything and everything by *ECQ*? If we ask and answer these deeper questions, it becomes completely trivial whether or not we agree that the inconsistent assumptions can be sensibly referred to as 'the theory' or 'a theory'. So we might as well allow that they can be.

Let us proceed with the deeper questions. How could the inconsistency be interesting or important? Well, it would be remarkable if the community actually *believed* all of the relevant inconsistent assumptions. But, as we might expect, they didn't: scientists noticed the inconsistency immediately, and so those who made a serious doxastic commitment to the new quantum assumptions came to consider CED an approximation to something else. As Jeans (1924, p.36) wrote, 'The complete system of dynamics, of which it [the quantum theory] is a part, has not yet been found.' With this attitude there could be no doxastic conflict: the quantum theory was to be thought of as fundamental, and when one used CED one merely used it as an approximation to a new theory yet to be discovered. And it was rational to assume that the new theory would be consistent with the quantum theory.

So how else could the inconsistency be interesting or important? Well, one might note that scientists were *using* the inconsistent assumptions together, and—as is well known—if one uses deductive logic with inconsistent assumptions it is possible to derive anything and everything by *Ex Contradictione Quodlibet* (*ECQ*). It might turn out to be extremely revealing to see just how scientists handle this sort of situation. In these circumstances, how could scientists trust *anything* they derived?

One answer might be that, since they didn't think of CED as fundamental, the equations of CED should be interpreted as including approximate-equals signs instead of equals signs. In other words, we should not think of scientists as believing the assumptions to be *approximately* true, but instead as believing the assumptions—now with approximate equals signs—to be *true*. In other words, we should *internalise the approximation*: move the approximation from the attitude towards the assumptions to within the assumptions themselves. Compare Muller (2007) here:

> [P]hysicists are notoriously sloppy … a majority of the exact equality signs (=) in most physics papers, articles, and books mean approximate equality (≈). (p.261)

Interpreting scientific posits in this way the inconsistency disappears altogether: the scientists never were reasoning with the inconsistent assumptions in question.

However, this would do an injustice to how science really works: it just isn't how scientists reason. In practice the equality-signs are left in place, and inferences are made as if the assumptions are true. The scientists *did* reason with the inconsistent assumptions. But even if the approximation does remain in the attitude towards the assumptions, the fact that this approximation exists at all is enough to explain why inconsistency doesn't lead to logical explosion. As some have already stressed (eg. Azzouni 2011), scientists are very particular about what they infer from their hypotheses, and do not simply apply logical inferences to their assumptions to achieve deductive closure. If they thought CED was only *approximately* correct, but didn't know how to correct it, then they would be tentative about all of their inferences. There would be scope to doubt all of them, because they wouldn't know how far from the truth their use of a given element of CED was taking them. This would have to be judged depending on the result, on conceptual and/or empirical grounds. Making truth preserving inferences from *approximately* true hypotheses might well lead you to approximate truth, or even truth, but it might also lead you to radical falsity. In such circumstances there is no motivation for machine-like logical deduction in all directions. Consequently, *ECQ* doesn't threaten.

In other words, there isn't really a mystery concerning how scientists reason with inconsistent assumptions. They make derivations as normal, but then treat the resultant posits with a certain degree of suspicion, less certain than the original posits. Perhaps the most important result from the point of view of philosophy of science is that inconsistency doesn't have to get in the way of scientific reasoning. And just because one *can* derive anything and everything with deductive logic doesn't mean that there is a danger one *will*. After all, to get logical explosion from inconsistency one has to go *via* contradiction (that's what '*ex contradictione*' means), and there's no danger of scientists continuing to reason after they have derived a contradiction. At this point they know that their derivations have led them astray, and they will back up to safer ground.

There is more to say, of course. But it is not my intention here to investigate the 'inconsistency of Bohr's theory' in detail. Instead my main goal is to show how one can get past debates about whether 'Bohr's theory' *is* or *is not* inconsistent, and instead ask deeper questions about why/whether relevant inconsistencies teach us things about how science works, or could work. Another highly relevant example is the more recent debate concerning the internal inconsistency of CED itself, to be considered next.

4 Example two: classical electrodynamics

In a recent debate Frisch (2005) claims that CED itself is internally inconsistent, whereas others reject the claim (Belot 2007, Muller 2007). Frisch shows us that the Maxwell equations, energy conservation, and some modest ontological claims are inconsistent with one particular construal of the Lorentz force equation (LFE). Belot and Muller prefer to construe the LFE in another way, such that the theory is *consistent* (or at least not inconsistent in the way claimed). Who is right?

In my 2008 paper (Vickers 2008) I argue that neither is right. The short answer is that there are very good reasons to take seriously Frisch's version of the LFE, but also other reasons to take seriously Belot's and Muller's version of the LFE. What people call 'the theory' or simply 'classical electrodynamics' sometimes includes one version of the equation and sometimes includes the other (or even a third option). To put things bluntly, there are good reasons to *believe* the Belot/Muller LFE, but it isn't very useful at all, and there are good reasons to *use* the Frisch LFE, but good reasons to think that it shouldn't be believed (and wouldn't have been believed even in its heyday, in the early 20th century).

Now, there might well appear to be good reasons to think along the lines of Muller and Belot, that it is part of the definition of 'theory' that it should include things one *believes*—or at least believes to be good *candidates* for the explanatory truth—rather than things one uses for practical purposes but doesn't believe at all. As Belot puts it '[Frisch's CED] does not deserve to be called a theory precisely because it is inconsistent.' (2007, p.277). However, to go down this line is to raise all those questions about the 'correct' analysis of *theory* noted in §2, above. No doubt some people *do* conceive of theories, in general, as things which can't be known to be false at the outset (whether because inconsistent, or for any other reason). However, others would disagree, and even many of those who agree might change their mind about *scientific* theories in particular. Consider some of the points against this attitude. The LFE Frisch uses is valid in virtually any classical context, so the differences between it and the other LFE will never show up. As a result, the Frisch LFE is central to 99% of the relevant physics literature. As Belot himself accepts, '[I]t appears that at the level of official doctrine and at the level of problem-solving, the external version of the Lorentz force law is taken as standard by physicists.' (p.273).

Once we agree to disagree about what we want to call 'the theory' we can get on with the more interesting and important philosophical work of working out how scientists reconciled their beliefs and reasoned with the theory. On the former question of reconciling their beliefs, we find—as with Bohr's theory of the atom—that scientists never *believed* all of the inconsistent assumptions. They knew from the beginning that the most useful version of the LFE could not be true (given their other commitments). What about scientific reasoning in this case? Again we find a similar situation to Bohr's theory. Physicists don't just deductively close their hypotheses—they are much more careful about which inferences they make. Physicists didn't know how to work with the LFE they considered their best guess at ‘the truth’: this version of the equation multiplied complications far too quickly (in fact it is still an area of research today[9]).

But again, the main point I want to make is that it doesn’t make much sense to argue forcefully that CED *is* or *isn’t* inconsistent in this respect. Some people will prefer to put assumptions together in one way and say it *is*, and others will prefer to put assumptions together in another way and say it *isn’t*, and we don’t need to take pains to decide who is right. In fact Frisch comes close to the recommended attitude at times. He writes,

> Throughout my discussion I will refer to the scheme used to model classical particle-field phenomena as a ‘theory’. (Frisch 2005, p.26)

In other words, he means our attention to focus on the ‘scheme’—or rather the set of posits within the scheme, including his particular version of the LFE—and although he *calls* this a ‘theory’, it shouldn’t matter that he calls it that. However, in the end this does lead to unfortunate and serious miscommunication, because he also insists on calling the particular collection of posits he is interested in ‘classical electrodynamics’. But people just do think that this term should be reserved for something very special, and should not merely be used as a name for whatever set of things one happens to be interested in.

Better (safer) to just accept that the term ‘classical electrodynamics’ is flexible enough to be sensibly used in both of these ways (and others besides). If Frisch, Muller, and Belot had done this, and merely debated why (whether) the inconsistency presented by Frisch is an *interesting* inconsistency, then the argument between them could have been avoided, and the relevant research time could have been better spent.[10]

5 Example three: Kirchhoff’s theory of diffraction

[9] See Muller 2007 for details.
[10] For a more detailed analysis of this case study, see Vickers (forthcoming b), Chapter 4.

Kirchhoff's theory of the diffraction of light at an aperture has been discussed in the *scientific* literature, but has barely been touched by philosophers (although see Saatsi and Vickers 2010). In 1882 Kirchhoff wrote a paper in which he asked the question,

> (K) What intensity of light will we find at a given point beyond an aperture which is illuminated by a monochromatic source?

Putting together certain assumptions about the behaviour of light he was able to derive a formula which provided remarkably successful predictions. This obviously encouraged serious commitment to Kirchhoff's theory. But it turns out that in addition to Kirchhoff's successful formula the assumptions also entail a contradiction.

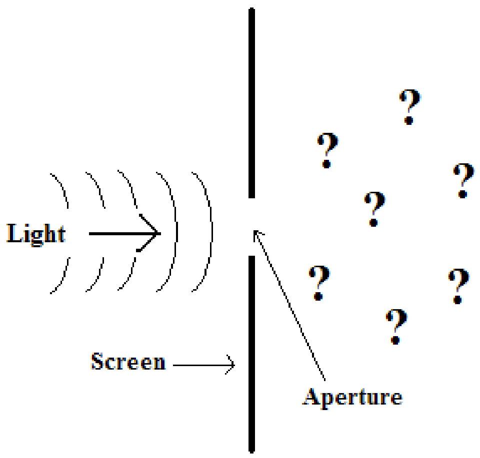

Figure 1. Kirchhoff's challenge: How does the light behave beyond the aperture?

Kirchhoff wanted to explain the distribution of light intensities which are detected beyond an aperture illuminated with monochromatic light. For the sake of simplicity Kirchhoff considered an idealised setup represented by Figure 1. Crucial features of the setup which aren't represented in Figure 1 are that the screen is infinitely thin and infinitely opaque. His assumptions were as follows:

(i) The light at the aperture behaves just as if the screen were not there.
(ii) The light source emits spherical, monochromatic waves of light.

(iii) The Helmholtz-Kirchhoff integral theorem (a result in mathematics).

(iv) The amplitude of light and the derivative of this amplitude are zero immediately behind the screen.

(v) The Sommerfeld radiation condition.

The main lesson is as follows. From (i)-(v) Kirchhoff was able to derive a startlingly successful formula for the behaviour of light in experiments which come close to recreating the idealised setup of Figure 1. But one can also derive a contradiction from these assumptions.

The most obvious manifestation of the inconsistency of assumptions (i)-(v) was noted by Poincaré in 1892, ten years after Kirchhoff's paper (Poincaré 1892, p.187). The problem is that Kirchhoff's successful formula disagrees with the boundary assumptions: if one considers what it says about light at the aperture it conflicts with assumption (i), and if one considers what it says about light immediately behind the screen it conflicts with assumption (iv). So it would seem that from assumptions (i)-(v) it is possible to derive a proposition stating 'A&~A', where options for 'A' include assumption (i) and assumption (iv). But then (one might think) by *ECQ* a logical explosion ensues: one might even conclude that this means that Kirchhoff's theory is really the set of all possible propositions!

One does not have to be too charitable to allow the use of the term 'Kirchhoff's theory' for something that is, or at least incorporates, assumptions (i)-(v). After all, they are all necessary ingredients in the recipe which leads to Kirchhoff's successful formula. But in what way is the inconsistency an interesting or important one? Did scientists believe the assumptions in question? Certainly not: the assumptions concern an idealised setup which couldn't possibly exist in the real world. No screen can possibly be infinitely thin (whatever that could mean) and infinitely opaque. So if the force of inconsistency is meant to be that it leads to real-world impossibility, the inconsistency of Kirchhoff's assumptions doesn't tell us anything we didn't already know.

But we might wonder whether Kirchhoff's assumptions are inconsistent when they are adjusted so that they *are* about the world. Suppose we have an experimental setup with a screen which is *very* thin and *very* opaque. Kirchhoff might then simply say that his assumptions are approximately true for this setup—in particular that assumptions (i) and (iv) are approximately true, since the non-zero width of the edges of the aperture would affect only these assumptions. But to make the assumptions about the world, and thus to make them relevant to present concerns, we can now 'internalise the approximation' (as we did with Bohr's theory in §3). Thus (i) and (iv) become,

(i*) The light at the aperture behaves *approximately* as if the screen were not there.

(iv*) The amplitude of light and the derivative of this amplitude are *approximately* zero immediately behind the screen.

Add these two assumptions to (ii), (iii) and (v) and we have a set of assumptions (i*)-(v) which Kirchhoff believed to be potentially *true of the world*. The question now is whether *these* assumptions are inconsistent. But in fact deciding this is a hopeless task, because of the vague nature of the word 'approximate', and complications which multiply extremely quickly (see Vickers forthcoming b, Chapter 7, for details).

The first conclusion has to be that there need be no *doxastic* conflict here. Any scientists using assumptions (i)-(v) would have known that they aren't even candidates for the truth, so any doxastic commitment would have applied only to (i*)-(v). And since there is no evidence that the latter set of assumptions are inconsistent, one can sensibly believe these assumptions to be serious candidates for the explanatory truth.

But it still remains the case that scientists, in practice, *used* the inconsistent assumptions (i)-(v). How did scientists avoid contradicting themselves? The answer is that inconsistency is avoided simply by accepting that the final formula only has a certain domain of application, that it doesn't apply when one approaches the boundary—these results are just ignored. If pressed to justify this move the scientist can simply say,

> "The diffraction formula is only approximate, so we already know it will give us false results, strictly speaking. The fact that it largely says sensible things can be put down to its being approximately true; the fact that it sometimes says things that are *not* sensible can be put down to its being strictly speaking false."

As already noted in §3, the rules of the game have changed when one gives up on truth-candidacy: one is not absolutely committed to *anything* one derives, because although one may be using truth-preserving inferences, one didn't *start* with truth. Truth-preserving inferences from approximately true assumptions can sometimes keep one close to the truth, and sometimes take one far from the truth. In such circumstances one has to base one's commitments on other factors, such as which results match empirical results, and which results seem inherently sensible.

In conclusion, we can consider how science and scientists are affected by the presence of inconsistency without worrying about how exactly we should use terms such as 'the theory' and 'Kirchhoff's theory'. Instead we debate scientists' epistemic and doxastic commitments, and scientists' reasoning techniques, and how these are affected by the presence of inconsistency. Since we can do all this, we might as well be charitable and let people use the term 'Kirchhoff's theory' to refer to the assumptions in question if they want to. If we do we should allow the statement 'Kirchhoff's theory of diffraction was inconsistent'. But that shouldn't alarm us.

6 Conclusion

In 1969 Dudley Shapere wrote,

> [There is a] lack of precision, in usual discussions, as to what is to count as "(part of) a theory." (Shapere 1969, p.139)

This was a *bad* thing in Shapere's view: philosophers should spend time, he thought, pinning down precisely how we should pick out the content of a theory in a given case. What I have argued is that the noted 'lack of precision' might well be here to stay, and that this needn't be a bad thing. And anyway, we might as well act *as if* it is here to stay: the questions that really matter don't depend on what 'is to count' as the content of a theory. Instead what matters are the deeper questions about what we learn from the fact that the particular propositions in question are inconsistent. Do scientists fail to notice inconsistencies in their beliefs sets? Do they manage to reason their way to sensible conclusions from inconsistent assumptions? Do they have good reason to trust such derivations? Can they successfully explain with inconsistent posits? I have not attempted to answer all of these questions in detail here, but have instead presented three cases of alleged 'inconsistency in science', and given a flavour of how one would go about answering the deeper questions. Some readers may feel that, even after the 'deeper' questions have been asked and answered, there still remains the question 'Is *the theory* inconsistent or not?' If theories are flexible, this may be a bad question. But even if theories are not flexible, it isn't at all obvious, to this author at least, that this is an interesting question. Suppose we have investigated all relevant inconsistencies, and considered their role vis-à-vis scientific reasoning, the epistemic/doxastic/instrumental commitments of scientists, and so on. What is then to be gained in asking whether *the theory* is inconsistent or not? At the very least, those who insist that there *is* something to be gained carry a heavy burden of proof.

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