Theory Eliminativism as a Methodological Tool

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Disagreements about the definition, nature, structure, ontology, and content of scientific theories are at least partly responsible for disagreements in other debates in the philosophy of science. I argue that available theories of theories and conceptual analyses of *theory* are ineffectual options for overcoming this difficulty. Directing my attention to debates about the properties of particular, named theories, I introduce 'theory eliminativism' as a certain type of debate-reformulation. As a methodological tool it has the potential to be a highly effective way to make real progress in the face of the noted problem. After the recommended reformulation questions of genuine importance to philosophy of science can still be asked and answered, but now without any possibility of disagreements about 'theories' compromising the debate.

1. Introduction. For several decades philosophers of science have spent much time and energy debating the concept *scientific theory*, asking questions such as 'What is a scientific theory?', 'What is the structure of a scientific theory?', 'What is the relationship between scientific theories and models?', and so on. The result is, on the face of it, a wide range of different conceptions, and very little agreement on the basics never mind the details. If this is right, then we have a bigger problem than is often appreciated: philosophy of science is defined by a family of debates, and most of these debates put considerable weight on the concept *scientific theory*.[1] The result is a very real possibility that disagreements about theories are at least partly responsible for disagreements in these other debates. Thus the time and energy devoted to the concept *scientific theory* appears to be very much justified.

It is this sort of thinking that brought Suppe (1977) to refer to our assumptions about the nature and structure of scientific theories as the 'keystone' of philosophy of science. He writes,

> If any problem in the philosophy of science justifiably can be claimed the most central or important, it is that of the nature and structure of scientific theories, including the diverse roles theories play in the scientific enterprise. For theories are the vehicle [sic] of scientific knowledge, and one way or another become involved in most aspects of the scientific enterprise. It is only a slight exaggeration to claim that a philosophy of science is little more than an analysis of theories and their roles in the scientific enterprise. A philosophy of science's

[1] Think of the pessimistic induction on past theories, the underdetermination of theories by evidence, theory confirmation, inter-theory relations, etc.

> analysis of the nature of theories, including their roles in the growth of scientific knowledge, thus is its keystone; and should that analysis prove inadequate, that inadequacy is likely to extend to its account of the remaining aspects of the scientific enterprise and the knowledge it provides. At the very least, it calls for a reassessment of its entire account of scientific knowledge. (p.3)[2]

I believe that there is some truth in this (I will argue as much), but as stated the claim is vague, and Suppe does little to fill in the details. Doubtless many debates in the philosophy of science discuss scientific theories, whether particular, named theories or theories in general. But we must ask: (1) Are these debates really very sensitive to how we conceive of theories? And: (2) Is there much reason to suppose that our 'analysis of the nature of theories' is really so inadequate?

Suppose we can answer 'yes' to both of these questions. Even then, the problem of 'the nature and structure of scientific theories' needn't be as crucial a problem as Suppe assumes. In what follows I wish to present an alternative approach to handling at least some debates within a certain class of debates in the philosophy of science which make heavy use of the concept *scientific theory*. In brief, the idea is that some debates can be usefully reformulated such that theories are no longer mentioned. In this way the questions that really matter in the debate can still be asked and answered (I will argue) but without any disagreements about 'theories' affecting the debate. If this is possible then, contra Suppe, one can move forward in at least some relevant debates without having to spend one's days deliberating over the nature and structure of scientific theories.

Of course, there are important details to add to this 'pragmatic eliminativist' proposal before it will be anything like viable. And there are several steps to take before it should even be considered. We need to answer questions (1) and (2), above, and we might ask whether one cannot simply draw on established literature on theories of theories (including Suppe's own work), or carry out a conceptual analysis of *scientific theory*. And whatever difficulties one might find with these options, isn't the 'pragmatic eliminativism' of *theory* going to be more trouble than it's worth? Won't the original disagreements show up again in the reformulated debate in some other form? If one eliminates theory-talk, doesn't that just change the questions being asked?

Each of these issues will be addressed in what follows. Given the complications, to focus this paper I will concentrate on a certain class of debates in the philosophy of science: those that discuss the properties of particular, named theories (so not debates about relations

[2] Other philosophers have made similar claims; cf. Niiniluoto (1984, p.111): 'The nature and function of scientific theories is perhaps the most central problem within the philosophy of science.'

between such theories, or debates about theories in general, for example). For the sake of clarity I present at this stage a semi-formal synopsis of my main argument:

P1: As things stand, there are many different views on the definition, nature, structure, ontology, and content of 'theories'.

P2: In at least <u>some debates</u>[3], within a certain class of debates in the philosophy of science which discuss the properties of particular, named theories, disagreements about theories are at least partly responsible for disagreements in the debates.

C1: [From P1 and P2] *If* we are going to continue <u>such debates</u> as currently framed, then we must accept that our conclusions are dependent on our view of 'theories', and thus are liable to be rejected by a large number of philosophers who hold different views about theories.

P3: Drawing on one or another available 'theory of theories' to support our conclusions will do little to help.

P4: Drawing on one or another conceptual analysis of *theory* will do little to help.

P5: Adopting a pluralist approach to theory-concepts (and particular theories) will do little to help.

C2: [From C1 and P3-P5] If at all possible we ought *not* to continue such debates as currently framed (we want to avoid the consequent of C1).[4]

P6: It is actually possible to reformulate <u>these debates</u> such that theory-talk does not feature at all, so that any disagreements about 'theories' cannot possibly affect the debate—this is the 'pragmatic eliminativism' of *theory*.

P7: The costs of the suggested eliminativism will usually be few: for example (i) the extra effort required to effect theory-eliminativism will be time well spent, and (ii) the questions that really matter in the debate will remain intact post-reformulation.

C3: [From C2 and P6-P7] The methodology of 'theory-eliminativism' is likely to be worthwhile for at least some debates.

The rest of this paper is devoted to working through this argument, defending the premises, and explaining just how the suggested 'theory-eliminativism' is supposed to work.

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[3] I use <u>underline</u> here and in what follows to emphasise where I am referring back to only certain debates within the class, where disagreements about theories affect the debate (as opposed to all debates in the class).

[4] There are (of course) suppressed premises here and elsewhere in the argument, but I am unaware of any suppressed premises that urgently require explicit defense. To reach C2, for example, I am assuming that we don't want to let disagreements about what theories are affect other debates if we can help it (we want to resolve disagreements in these other debates, if possible).

2. Premise P1: different views on 'theories'. This premise states that there are many different views on 'theories', including differences of opinion on the definition, nature, structure, ontology, and content of 'theories'. I take this to be relatively uncontroversial, but it is worth emphasising the extent of the disagreement. If the differences in opinion were only slight, or only a small minority held 'non-standard' views, then we might be justified in assuming either (i) that differences of opinion about theories don't affect our debates, or (ii) that we can consider the views of the small minority as probably misguided. But in fact there are major differences of opinion, and throughout philosophy of science.

Of course, the definition, nature, structure, ontology, and content of theories are not independent of each other, but they can nevertheless be taken in turn. To start with definitions, few serious attempts have been made at definitions of 'theory', beyond very general, vague, dictionary-style definitions. But such definitions are everywhere, and there are significant tensions between them even given their vagueness. One can find twenty different definitions of 'scientific theory' in five minutes using Google Books, searching for ' "definition of scientific theory" ' and ' "a scientific theory is a" ' (I leave this as an exercise for the reader). Many more are available in journal articles. Some authors note (and this seems right) that what one takes a scientific theory to be depends on one's academic formation. This might suggest that we should be pluralists about the theory-concept, an option that will be investigated in §5, below.

I'll take the nature, structure, and ontology of theories together. By this I will mean, respectively, the *kind* of thing theories are made up of, how these constituents fit together and relate to each other, and the sense in which they 'exist'. So, for example, one option—especially popular in the 1940s and 50s—is to treat theories as sets of statements and their deductive closure. Statements (and thus theories) might then be thought of (ontologically speaking) as linguistic, abstract, mental, existing in a 'World 3' (Popper), or otherwise. Despite strong objections to some of the details attached to this view in the 50s and 60s (eg. a logico-linguistic emphasis), certain variants of it are still popular (eg. Mahner et al. 1997, §9.3). More popular today is the so-called 'semantic' approach to theories, but it would be difficult to claim that there is a consensus. Numerous diverse views of theories have come under the label 'semantic' in the past thirty years or so (even if authors have sometimes rejected the label). It is sometimes said that on this view theories are 'families of models', but Ron Giere's abstract, scientific models grouped by 'family resemblance' is a world away from the Suppesian view, where a set of model-theoretic models satisfy a set of axioms written in the language of set-theory (cf. Suppes 1967, da Costa and French 2003, Muller 2009). Also within this semantic category one finds approaches such as that of the so-called 'German structuralists' (Sneed, Stegmüller, Balzer, Moulines) with their 'theory nets', and the 'state-space' or 'phase-space' approach (Suppe, van Fraassen). Then Nancy Cartwright has a distinct but related view of theories, which has sometimes been labelled ‘semantic’, but which makes a distinction between the roles played by models and theories in science. And against both the syntactic and semantic views, Hendry and Psillos (2007) have argued that theories don’t consist of just one type of thing, but are instead ‘complex consortia of different representational media’ (p.160).

Even within these different approaches one can take different views on what might be called the ‘structure’ of theories. For example, some claim that theories have a ‘core’ or ‘essence’ (eg. Gould 2002, Morrison 2007), that there are different ‘levels’ of theory (eg. Finkelstein 1966), that theories can be ‘open’ or ‘closed’ (cf. Bokulich 2006), that theories can be ‘established’ (Rohrlich and Hardin 1983), ‘categorical’ or ‘flexible’ (Finkelstein 1966), ‘mechanistic’ (Craver 2002) or ‘modular’ (Darrigol 2008). And even this is the tip of the iceberg: there are also the views on theories provided by Kuhn’s ‘paradigms’ (1962), Lakatos’s ‘research programmes’ (1970), Toulmin’s ‘conceptual systems’ (1972) and Laudan’s ‘research traditions’ (1977). One can add to this Shapere’s analysis of theories in terms of ‘domains’ (1977), Darden and Maull’s ‘interfield theories’ (1977), Churchland on theories as ‘configurations of synaptic weights’ (1989), Batterman’s ‘theories between theories’ (1995), Wilson’s ‘theory façades’ approach (2006), and the recent ‘Hierarchical Bayesian Perspective’ (Henderson et al. 2010).

Finally there are disagreements about the content of particular, named theories. In other words, even if we agree that theories are made up of propositions (or models, or modules, or whatever), there is the questions of *which* propositions (models, modules) belong to a given, named theory. The above views constrain this, but they don’t determine it: even detailed definitions of ‘theory’ allow for different views on the content that should be ascribed to some particular theory. Disagreements on theory-content will be the main focus of this paper, and are best demonstrated by way of the examples in the next section.[5]

3. Premise P2: disagreements about ‘theories’ affect other debates. Some readers may think that, despite the range of opinions sketched in the previous section, such differences of opinion don’t seriously affect many debates in the philosophy of science. First, it should be made clear that I am not arguing here that this happens in many or most debates (although I am certainly not ruling this out); instead I am arguing that it happens at least often enough for it to be a concern that is worth addressing. Second, in this paper I am going to focus just on debates about the properties of particular, named theories. In this section I will present some

[5] Much more could be said about (potential) disagreements about theories here. Where do we separate the theory’s ‘core’ from the ‘auxiliaries’? Where do we separate the theory from the background assumptions? Where do we separate the theory from its interpretation? And so on.

real cases where disagreements in the debate are at least partly (if not fully) caused by differing views on the content that should be ascribed to particular, named theories.

My first case is the recent debate as to whether 'classical electrodynamics' (CED) is inconsistent. Frisch (2005) argues that 'the theory' is inconsistent, but Muller (2007) and Belot (2007) disagree. Who is right? As Vickers (2008) points out, the disagreement hangs on the fact that what Frisch means by 'classical electrodynamics' is not what Muller and Belot mean by 'classical electrodynamics'. Muller (2007) is representative of a rather extreme—but not uncommon—view in philosophy of science that it is possible to 'define the theory'. He does this at the beginning of his paper as he sees fit, but unfortunately presents something different to Frisch's focus of attention. In what follows there is understandable frustration as Muller strives to understand where Frisch is coming from, claiming that Frisch 'has applied CED inconsistently', that 'the logic of the proof that has led him to this conclusion [is] flawed', and that Frisch makes 'two contradictory assumptions' such that 'we already have a contradiction by $\wedge$ – introduction!'

One response may be that all we have here is a terminological/referential misunderstanding. But reading Muller's paper, there is a clear (if implicit) claim that Frisch's conclusion is without value since what it is attached to is 'not the theory'. Belot (2007) is a little more charitable: he sees that Frisch is using the term 'classical electrodynamics' to refer in a particular way, such that we do end up with an inconsistent set of assumptions. He then asks the question whether this is a sensible way to conceive of 'the theory', and concludes that it isn't. In fact he goes as far as to say that Frisch's conception of CED 'does not deserve to be called a theory precisely because it is inconsistent.' (p.277). However, Vickers (2008) shows that if one is initially tempted by Frisch's conception of the theory, Belot's objections carry little weight.

It should be no surprise that there are other examples where disagreements about the content that should be ascribed to a theory cause disagreements as to whether it is inconsistent or not. Whether we are discussing Aristotle's theory of motion, the early calculus, Kirchhoff's theory of diffraction, or Bohr's theory of the atom, small changes in the content of these theories can make the difference between whether they are inconsistent or not. To take Bohr's theory, Lakatos (1970), Brown (1992, 2002), Priest (2002), and da Costa and French (2003) claim that it is inconsistent, whereas Bartelborth (1989), Hendry (1993), and Hettema (1995) claim that it isn't. Who is right? This time the main issue is which parts of CED we include as part of Bohr's theory. Bartelborth (1989) argues that only 'quasi-electrostatics' should be admitted, whereas Brown (2002) suggests that the whole of CED must be included because there were applications of old quantum theory where CED was used. Do we then have an internally inconsistent theory, or two theories (Bohr's theory and CED) which are mutually inconsistent? And even if we put them together we don’t have an inconsistency if we interpret some of the ‘=’ symbols as ‘≈’ symbols, a move that Muller (2007) urges:

> [P]hysicists are notoriously sloppy in this respect: a majority of the exact equality signs (=) in most physics papers, articles, and books mean approximate equality (≈). (Muller 2007, p.261)

As before, whether we should refer to ‘Bohr’s theory’ as inconsistent depends crucially on one’s views about how one should decide upon the content that belongs to the theory.

One may wonder at this stage whether this is a problem that can only arise in the context of debates about inconsistency in science, where small differences of opinion as to theory-content can make all the difference between consistency/inconsistency. But in fact, (i) there are often large differences of opinion as to theory-content (as indicated in §2, above), and (ii) there are debates about other properties of theories which are sensitive to the content that gets ascribed to the theory. One obvious example outside inconsistency in science is the longstanding debate as to whether classical mechanics is deterministic. Several authors have urged that it is indeterministic (eg. Earman 1986, Hutchison 1993, Norton 2008), whereas others maintain that it is deterministic, such that one reaches the opposite conclusion only by misunderstanding ‘what the theory is’ (eg. Arnold 1977, Korolev 2007, 2010, Zinkernagel 2010). And Wilson (2009) argues that the theory is neither deterministic nor indeterministic, because there are different ‘species’ of classical mechanics, some of which are deterministic and some of which are not. Again we have a clear case of disagreements about the content of the theory causing disagreements in the debate.

These cases are enough to show that this is a real phenomenon in the philosophy of science, but they are hardly the only cases. Since there are these clear disagreements about the content of particular theories, we can expect that there is the threat, at least, of similar problems arising in any debate about a particular, named theory. Consider predictions: does Bohr’s theory predict the intensities of the hydrogen spectral lines? Smith (1988) argues that it does because it includes the correspondence principle. But compare Shapere (1977):

> [T]he Bohr theory offered no way to account for the intensities and polarizations of the spectral lines… Use of the correspondence principle as a basis for calculating the polarizations of the lines is not considered here as a ‘part of the theory.’ (p.559)

Whether the theory makes the prediction or not (and hence whether it might be confirmed/disconfirmed) depends on your view of what the theory is.

4. Premises P3 and P4: theories of theories and conceptual analysis. How are we to determine who is right in the sorts of debate noted in the previous section? The obvious answer might be that we just need to get clear on who has the right view of what the content of the theory is. Or, better, that we need to decide who has the right view of whether the content at issue in the particular debate in question does/doesn't belong to the theory. But how is this to be decided? Nickles (2002), focusing on debates about inconsistency in science, suggests that we need to sort out our theory of theories first: after all, if theories turn out to be families of models then it looks like they won't even be the *kind* of thing which can be inconsistent (cf. pp.8-11). But this seems a rather hopeless strategy in the face of the mass of literature on theories indicated in §2, above. Instead of helping us to resolve debates about inconsistency in science, it asks us to shelve those debates until (optimistically!) we've resolved debates about theories of theories. In addition, even if we concluded that theories are (or should be represented as) sets of propositions, this does nothing to help us decide *which* propositions belong to a particular theory.

A more promising strategy might be to do conceptual analysis on *theory*. If one could establish, for example, that *T is a theory iff T is a set of propositions put forward as serious candidates for the explanatory truth of a given domain of phenomena, and the deductive closure thereof*, then this would give us at least some idea of which propositions do and don't belong to a particular theory. Belot takes some steps in this direction, and apparently agrees with at least some features of the definition just given: as noted above, he states that Frisch's conception of CED does not deserve to be called a 'theory' precisely because it is (known to be) inconsistent. This would be right if a theory was, by definition, something 'put forward as a candidate for the explanatory truth'. However, the definition provided is controversial in a number of ways, and Frisch can find good reasons reject it. For one thing, it rules out any theory being (known to be) inconsistent, and yet there are several examples of scientific theories widely acknowledged to be inconsistent theories. And the focus on deductive closure would mean that any two inconsistent theories would actually be the very same theory, since they would both simply be the set of all propositions (by *Ex contradictione quodlibet*). Again, this goes strongly against received wisdom.

Questions such as 'should "explanatory truth" play any part in our conception of a theory?', and 'should we include the deductive closure?' are just small elements of a huge project here. As indicated in §2, above, there are about as many different definitions of 'scientific theory' as there are philosophers. In other words, the problem with this strategy is very similar to the 'theories of theories' strategy: debates about the properties of theories are shelved, rather than resolved, until (optimistically!) we achieve an acceptable conceptual analysis of *scientific theory*. Complications multiply: First, which theory of concepts should we employ? The so-called ‘classical’ theory of concepts, whereby we search for necessary and sufficient conditions, is now totally out of favour. In its place we find several other possibilities, including the neo-classical theory, prototype theory, the theory theory, the neo-empiricist view, and conceptual atomism.[6] Then there is the question of the relationship between the concept *theory*, the concept of a particular theory (eg. *classical electrodynamics*), and the content that should be ascribed to a particular theory. So is conceptual analysis a good way to (help) resolve the sorts of debate discussed in §3, above? I submit that for at least some such debates there is a preferable strategy (to be presented in §6).

5. Premise P5: pluralist responses. Different scientists and academics have different intuitions about what theories are. It has been noted, for example, that one is likely to have very different views on what theories are if one has more experience of (say) economics, biology, or theoretical physics. This may suggest that we should be pluralists about the theory-concept. This would also seem to resolve some of the tensions noted above. For example, one might say that Frisch has one theory-concept (focusing on the equations that are *used* by scientists) and Belot and Muller have another theory-concept (focusing on the equations that are/were *believed* by scientists). One might draw here on Kenat (1987, p. 87) and Suppe (1989, ch.14) who (drawing on a paper by Sylvain Bromberger) distinguish two types of theory: ‘Theories1’ are ‘theories as techniques for developing answers to problems’, and ‘theories2’ are ‘propositions’. We might then index these theory-concepts, and say that CED_1 is inconsistent, and CED_2 is consistent.

This may seem like a good solution on the surface, but the details tell a different story. Do we expect that we will find a manageably finite number of theory-concepts, analogous to the three major species-concepts that have been put forward (*biospecies*, *phylospecies*, and *ecospecies*)? Given the multitude of disagreement noted in §2, above, this seems optimistic. The overarching question is, how should we identify and index (some of) the different theory-concepts? Obviously ‘theories as techniques for developing answers to problems’ and ‘theories as propositions’ are hopelessly vague to be useful (descriptions of) theory-concepts. However, one may also ask how useful more specific theory-concepts are, such as ‘a theory_i is a set of propositions put forward as serious candidates for the explanatory truth of a given domain of phenomena, and the deductive closure thereof’. This will still lead us to very little determinate content for a given theory, since we will have to ask just *how* ‘serious’ the candidacy is, just what is meant in real terms by ‘put forward’, how we delineate the ‘given’ domain of phenomena, and so on.

[6] These theories of concepts are introduced and discussed in any good handbook on concepts. A further complication, as Machery (2009) notes, is that ‘there are several prototype theories, several exemplar theories, and several theory theories’ (p.243).

Even if we could identify and index theory-concepts, the big question for this paper is whether pluralism could help us resolve debates about the properties of particular theories. The main problem here appears to be that pluralism is going in the wrong direction: philosophers of science don't first think of a theory-concept and then apply it to reveal a given theory's content (to the extent that this is possible, given the vagueness necessarily inherent in any theory-concept). Instead, theory constituents are put together based on years of experience analysing and solving problems with the theory. So, for example, the content of CED provided by Frisch, Belot and Muller in the debate mentioned above does not seem to fit comfortably in any obvious theory-concept that might be invented. There are issues that are theory-specific, that a general theory-concept would never accommodate. For example, Vickers (2008) shows why we might want to have different conceptions of CED depending on whether and in what way one wants the theory to accommodate the self-force effects of a charged particle and synchrotron phenomena. Similarly, the three 'species of classical mechanics' that Wilson (2009) distinguishes in the debate over the (in)determinacy of classical mechanics would not be identified by any general account of 'theory kinds', since they pertain to different ways of conceptualising the basic ontology of mechanics. The only theory concepts that could underwrite these three 'species' of the theory would be ones cooked up post-hoc, purely to accommodate this case. But under such circumstances the theory-concepts in question have failed to do what we wanted them to do: the idea was that delineating a plurality of theory-concepts could help us identify legitimate decisions on theory content, not the other way around.[7]

Finally, one might argue that pluralism about theory-concepts in general is little help, but pluralism about *particular* theories is more help. The idea here would be that one can delineate different 'contents' for any given theory, without any overarching general theory of theory-content explaining these different 'contents' (since that would just bring us back to a general theory-concept pluralism). Instead one would have to justify why, in the context of a given debate, it was appropriate to focus on a given number of theoretical constituents. In fact this just brings us to the theory-eliminativism I wish to advocate in the next section: if one has told us the precise content one is considering, and explained why it is appropriate to consider it in the given context, calling that content 'a formulation of the theory' is to use this locution as a mere label for the specified content. Otherwise the particular-theory pluralist has closely related problems to the theory-concept pluralist: he or she is committed to a plurality of

[7] There is also the question of why all the different theory-concepts get to be unified as *theory*-concepts, as opposed to being just a number of concepts. The pluralist might hope to get around this by simply dropping the term 'theory', and just referring to 'concepts' used to analyze science. This in itself would be a type of eliminativism, but not a particularly helpful one. It would still leave the problems of how we identify and index the different concepts, and how useful they are in practice for resolving debates about (properties of) theories.

individuals, all of which deserve the title ‘formulation *of the theory*’ for some reason that requires articulation (drawing on conceptual analysis, and all the difficulties that gives rise to).

6. Premise P6: theory eliminativism. So far I have argued that there are many different views on theories, and that these differences of opinion are at least partly responsible for disagreements in certain debates in the philosophy of science. I have further argued that available theories of theories, conceptual analysis, and theory-pluralism, are unsatisfactory ways of resolving this problem. On the face of it it looks like we have no other option, though, since the concept of a theory is so central to these debates. If we’re asking whether CED and Bohr’s theory of the atom are inconsistent, or whether classical mechanics is deterministic, or whether *any* given theory has some particular property, how can we manage *without* referring to the theory in question?

In this section I want to present as a methodological tool a certain type of debate-reformulation which might be described as ‘theory-eliminativism’. Before I give a general characterisation, let me first demonstrate by showing how it would work for the debates over the (in)consistency of CED and the determinism of classical mechanics. As noted above, the main reaction to Frisch’s (2005) claim that ‘CED is inconsistent’ is that he simply gets the theory wrong. Muller (2007) ‘defines the theory’ to demonstrate this, and Belot (2007) gives several reasons why Frisch’s conception of the theory is not tolerable. But now consider how things would have developed if Frisch had claimed not that ‘CED is inconsistent’, but instead the following:

> Here are some assumptions relevant to electromagnetic phenomena which are mutually inconsistent [...]. They are inconsistent in the straightforward sense that one can deduce a contradiction from them as follows [...]. What is interesting/important about the fact that these particular assumptions are inconsistent is as follows [...].

Clearly if Frisch had put things this way, no disagreements about ‘what CED is’ could have arisen, since no reference to ‘CED’ is made. Even those with the view that there is a single, canonical form to CED could not object that Frisch had got the theory wrong. In fact, *whatever* views readers had on theories, however wild or diverse, those differences of opinion could no longer affect the assessment of Frisch’s claim. Instead the focus of attention would all be directed where it should be, in whatever analysis is given in the third and final set of square brackets in the above statement.

The question arises whether this should really be described as a reformulation of the same debate, or whether a different debate altogether is being conducted post-reformulation. Obviously the debate has changed to some degree, and if the concept of a theory is really going to be eliminated some questions will disappear. The most obvious is ‘But is this really *classical electrodynamics* you’re talking about?’ I happen to think this is a bad question, based on a false premise, but there is no need for me to argue that here. The main point for this paper is that what is left out is a question about what the theory is, and *not* a question about inconsistency in science. What theory-eliminativism is designed to do is to enable one to move forward in one debate—for the purposes of this paper, a debate about a particular property (eg. inconsistency or determinism) of a given set of assumptions—without disagreements over ‘what the theory is’ getting in the way. Of course, the original disagreements about the concept *scientific theory* will remain, but the claim is that one can say everything it is important to say about the property in question without referring to ‘the theory’ (something I’ll argue in the next section).

The debate over the determinism of classical mechanics is another candidate for reformulation. Instead of claiming that ‘classical mechanics’ is/is not deterministic, one can instead eliminate ‘classical mechanics’ from the debate by reformulating one’s statements as follows:

> Here are some assumptions which imply that the mechanics of moving bodies is deterministic/indeterministic [...]. Let me demonstrate that they imply (in)determinism [...]. What is interesting and important about the fact that these particular assumptions imply (in)determinism is as follows [...].

Again, adding ‘and these assumptions actually constitute classical mechanics’ adds nothing to the importance of the claim being made: everything that is important about the claim is already stated explicitly in the final set of square brackets. Once again, in making this move one has managed to eliminate the concept of a theory from the debate, so that no disagreements about ‘what the theory is’ can possibly get in the way of the claim’s assessment.

Extension to the general case is straightforward. For any claim that might be made of the form ‘Theory *T* has property X’ one can say instead,

> Here are some theoretical constituents which have the property X [...]. This can be demonstrated as follows [...]. What is interesting and important about the fact that these particular constituents have that property is as follows [...].

In all cases this will preserve whatever it is that was important to say in the first place. Indeed, by forcing one to say explicitly what is interesting/important about the claim, it becomes impossible for one to hide behind the concept *theory*, as if it's obvious that it's an important claim simply because 'the theory' has that property. But the main point is that, in making such a reformulation, any disagreements about 'what the theory is' can no longer possibly affect the assessment of the claim being made. No appeal to a 'theory of theories' or a conceptual analysis of *theory* is necessary. And what is good about different 'theories of theories' is maintained in the process (I am certainly not dismissing the literature on 'theories of theories' as worthless). For example, if one wishes to discuss models, instead of propositions, one can: one simply presents the models, demonstrates that they have some feature, and then explains why that is interesting/important from the point of view of philosophy of science (eg. what it tells us about how science works). The most diverse 'theories of theories' can be fully accommodated: Giere can put together a family of scientific models as his 'theoretical constituents', and Muller can put together a 'set of structures in the domain of discourse of axiomatic set-theory, characterised by a set-theoretical predicate' (Muller 2009). What is left behind is any claim that one is focusing on these things because 'that's what a theory is'. Instead, the point of focusing on these things must be argued much more explicitly, by way of filling in that third and final set of square brackets.

Before I move on, three points of clarification may benefit certain readers. First, let me respond to the reader who would prefer the label 'theory-quietism' to 'theory-eliminativism'. In the end it is the methodology that matters, not the name we choose for that methodology, but equally this doesn't mean that any label will do. Unfortunately there are problems with both options, since what I am proposing is different in important ways from other 'quietist' and 'eliminativist' positions which have been put forward. For example, French (2008, 2010) proposes a 'pragmatic quietism' about the ontology of scientific models and theories, but this differs significantly from the position proposed here, for example because French makes no claim that we should stop making use of the concept of a 'model' or 'theory' in our debates, only that we should stop asking questions about their ontology. The main differences from other eliminativist positions are (i) that it is a pragmatic and not an ontological or scientific claim (so it's not like claims for 'species'-eliminativism, 'innate'-eliminativism, 'concept'-eliminativism, etc.), and (ii) it is selective in the sense that it is only to be applied when there is a warrant for it. However, my sense is that the suggested methodology is close enough to other eliminativist proposals that 'theory-eliminativism' is a perfectly sensible term.

Second, theory-eliminativism as I have described it does not necessarily rule out the use of terms such as 'the theory' and 'formulation of the theory'—one might follow the suggested model and then simply add 'and I call this set of constituents "(a formulation of) the theory"'. In that case the term in question is just being used as a label, and doesn't carry any conceptual weight. However, I don't recommend this approach: it presents a danger because many readers will insist that that's an improper use of the term, or may even fail to realize that the term is being used as a label only. In fact, in the debate over CED, Frisch (2005, p.26) tells us explicitly how he is using the term 'theory', but this didn't stop Muller and Belot responding "That's not the theory!" (give or take). The same problem arises for the particular-theory pluralism discussed in the previous section: it leaves room for the response "That's not a formulation of the theory!" This sort of problem just disappears if one simply calls what we're working with 'formulations' (say), and leaves theory-talk behind.

Third, I accept that in many cases the focus of debate may well be on the property in question, as opposed to on the identity of the theory. For example, there has been much dispute over precisely what we should mean by the 'time-reversal-invariance' of different theories. However, this doesn't mean the methodology isn't helpful: just because there is disagreement about the property doesn't mean that disagreements about what the theory is aren't *also* affecting the debate.

7. Premise P7: two concerns. Some concerns may arise at this point that the costs of the suggested reformulation will be too much for it to be worthwhile. For example, won't considerable effort be required to bring about the reformulation? Filling in the third set of square brackets in the examples given in the previous section will be hard work. However, this extra effort should not be regarded as a defect. Quite the contrary: the philosopher who wishes to make the statement 'Theory *T* is X' will be forced to think harder about just what is hidden within his or her conception of 'the theory' that makes the claim an interesting or important claim. In the process, it will either be stated much more clearly why it is an important result, or one may find that the claim isn't as interesting/important as originally supposed, and reconsider making it in the first place. For example, compare Frisch (2008) reflecting on his original claim that 'CED is inconsistent': 'I am inclined to agree with my critics that this inconsistency in itself is less telling than my previous discussions may have suggested.' (p.94). In short, the extra work required by reformulation is likely to be time well spent, not simply time spent saying in a convoluted way what one could have said much more succinctly in terms of 'the theory'.

More serious, perhaps, is the concern that something important is lost from the debate during reformulation. After all, one is being denied a concept—whether it be *theory* or some specific-theory-concept such as *classical electrodynamics*—which is ubiquitous in both the relevant science and the philosophy of science. Can one really describe science adequately if one takes away a term that the scientists themselves regularly make use of?

In this paper I cannot pretend to argue that in *any* possible debate nothing will be lost, but I can indicate how this might be the case by arguing that nothing important is lost in reformulating the concrete examples discussed above. First, is anything lost from the Frisch debate if we reformulate things so that no reference is made to ‘the theory’ or ‘CED’? To reformulate, Frisch first needs to state precisely which assumptions he is considering, which he does, more or less (Frisch 2005, p.33). He then needs to demonstrate that a contradiction follows, which he also does (p.34). But the question then is why this is an interesting or important result: do we learn anything about how science works, for example? There are two obvious ways in which an inconsistency in science might be interesting, both of which can be articulated and debated without making reference to ‘the theory’, as we will now see.

First, it might be that all of the inconsistent assumptions were genuinely believed by scientists (or at least believed to be *candidates* for the truth). This would only really happen if scientists were unaware that the assumptions are inconsistent, in which case it might then be interesting to analyse why, precisely, the inconsistency wasn’t noticed, and consider whether there are similar blind-spots in other corners of science. However, in fact, it is clear that the assumptions Frisch presents were not considered ‘candidates for the truth’, so Frisch’s inconsistency could never be interesting in this sense (as Frisch himself would admit, cf. Frisch 2005, p.35).

The second obvious way in which an inconsistency can be interesting/important is when all of the assumptions in question were *used* by scientists, even if they weren’t considered ‘candidates for the truth’. This is actually quite a common occurrence: it is well known that idealization and approximation techniques are ubiquitous in science, especially in physics. Especially interesting here is when the relevant de-idealizations are not possible (perhaps because the mathematics is intractable): in such circumstances one is sometimes forced to work with an inconsistent set of assumptions. In such a case the question arises how one can judge which derivations are trustworthy. Since one is working with assumptions which are necessarily false, even if one makes use of truth-preserving inferences one often has no way of knowing whether a given inference has taken one from close-to-truth to far-from-truth.

It may look like Frisch’s assumptions match this latter case. Certainly his assumption about the Lorentz force equation is naturally described as an idealization assumption, and it turns out severe problems accompany any attempt at de-idealization (cf. Frisch 2008, p.95). So one might defend Frisch by explaining his claim as one where the inconsistency of the assumptions in question is interesting because it is a case of scientists routinely reasoning with inconsistent assumptions. One might then examine this reasoning further, assessing how scientists went about judging which inferences were trustworthy, and whether there are lessons for how scientists should reason in the face of inconsistencies in current science (eg. the conflict between general relativity and quantum theory).

Vickers (2008) argues that we don’t quite have this situation, since there isn’t a clear sense in which Frisch’s version of the Lorentz force equation was used by scientists to make inferences. But this is not the point of current concern. The worry was that, in making the reformulation, something important is lost from the original debate. But the discussion just given shows how whatever it is that may be important about Frisch's original claim does not require mention of 'classical electrodynamics' or 'the theory' for articulation and discussion.

Taking now the debate as to whether 'classical mechanics' is deterministic, the question might be raised whether in eliminating reference to 'classical mechanics' and 'the theory' we lose something important from the original debate. Well, what is important about the original debate? Perhaps the most obvious answer is that, if it can be shown that classical mechanics is indeterministic, then one might be well placed to argue that individuals in the history of science were implicitly committed to something they would have wanted to reject. This would be interesting since it raises the questions of why they didn't notice, how it would have changed things if they had noticed, and what we can learn from this about things we might be blind to in current science.

If this is indeed the point of the debate, then it can continue at least as successfully without talk of 'classical mechanics' or 'the theory'. A rough overview of how it could continue is as follows. First (i) one can identify the assumptions pertinent to the point you are trying to make—eg. Newton's three laws of motion. Second (ii) one can show that, given these assumptions, the mechanics of bodies is indeterministic in certain contexts. Then (iii) one has to make the case that this is interesting/important. For example, one might carry out some historical work to argue that relevant characters from the history of science (1) accepted these assumptions, (2) weren't aware of the indeterminism, and (3) would have found the indeterminism intolerable, had they been aware of it.

Opposition to this story can then also proceed without reference to 'classical mechanics' or 'the theory'. One may argue that the indeterministic 'contexts' in question are either 'unphysical' or make use of 'inadmissible idealizations' (cf. Norton 2008, Korolev 2010). One might argue that relevant scientists made other assumptions, in addition to Newton's laws, which do then assure determinism in the given contexts (cf. Korolev 2007). Or one might argue against the claim that Newton's laws by themselves entail indeterminism, either claiming that Newton's laws in fact do assure determinism when properly understood (cf. Zinkernagel 2010), or by arguing that Newton's laws by themselves entail neither determinism nor indeterminism (cf. Wilson 2009). In this way everything we might want to say can be said. Adding claims such as 'classical mechanics actually consists of more than just Newton's laws' or 'determinism is an axiom of classical mechanics' add nothing of value, and only cause the sorts of damaging disagreement and miscommunication noted above.

Wilson (2009) at first seems to take a more sensible approach, distinguishing three different ways in which the term 'classical mechanics' might appropriately be employed. But in fact he also has a view on what we should mean by ‘classical mechanics’ that is unnecessary to the debate and can only cause trouble. In his view, classical mechanics does not merely consist of Newton’s laws, nor consist of Newton’s laws plus something else which can be explicitly given, but consists of Newton’s laws plus ‘descriptive gaps’, into which different assumptions can be plugged depending on the context (Wilson 2009, eg. pp.9-10). For example, he distinguishes three different ‘species’ of the theory, each of which fills these ‘gaps’ in a different way. But this conception of ‘the theory’ just isn’t necessary to make the main point he wants to make. What he really wants to establish is that people who use Newton’s laws to solve problems in mechanics also make use of certain other assumptions, depending on the context, and depending on which assumptions are added to Newton’s laws we may or may not reach determinism/indeterminism.

An objection that might arise for both of these examples is as follows. If we can’t refer to ‘the theory’, then we can’t ask questions such as ‘Were the assumptions Frisch identifies what scientists working at the time meant to refer to when they used the term ‘CED’?’ and ‘How did scientists at the time use the term ‘classical mechanics’ to refer—did they mean it to refer to something more than just Newton’s three laws?’ But this is really no objection at all: it is really a question about the use of a term, not a question about theories as things that ‘exist’ in the history of science. Theory-eliminativism as I have delineated it is compatible with discussing how any particular term is used: the discussion of the use of a term (eg. ‘theory’) should be distinguished from a discussion which invokes theories as things, with properties, etc. At any rate, usually it will not be important to consider theorists’ beliefs about how certain terms like ‘classical mechanics’ referred. What will be most important will be to consider the assumptions (models, etc) they believed, accepted, used, etc.—especially the ones that were unified together for particular purposes (eg. prediction, explanation, etc.).

8. Conclusion. This completes my defence of the premises of the semi-formal argument presented in §1. The conclusion of the argument—that the sort of theory-eliminativism described is likely to be worthwhile for at least some debates—is modest in many respects. No claim is made that it will be worthwhile for all debates. Indeed, in this paper I have focused solely on debates about the properties particular theories have, and not on other types of debate (eg. about relations between particular theories, or about theories in general). And the claim isn’t even that theory-eliminativism will benefit all debates in the particular class of debates I have focused on. However, that isn’t to say that nothing of significance has been achieved: I have outlined here a methodological tool for the philosopher of science which enables one to carry out debates without any disagreements, miscommunication, or plain confusion about ‘scientific theories’ affecting those debates, and such that little or nothing is sacrificed in the process. Further, I have shown that there are concrete cases in the literature which can benefit from this methodology. It remains possible that the methodology can benefit *many* debates in the philosophy of science, including debates about relations between particular theories (eg. the reduction of thermodynamics to statistical mechanics), and theories in general (eg. the realism debate). So, although a modest proposal for the purposes of this paper, something altogether less modest is an open possibility.

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