Scientific Theory Eliminativism

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Abstract

The philosopher of science faces overwhelming disagreement in the literature on the definition, nature, structure, ontology, and content of scientific theories. These disagreements are at least partly responsible for disagreements in many of the debates in the discipline which put weight on the concept *scientific theory*. I argue that available theories of theories and conceptual analyses of *theory* are ineffectual options for addressing this difficulty: they do not move debates forward in a significant way. 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 progress in the face of the noted problem: post-reformulation disagreements about *theory* cannot compromise the debate, and the questions that really matter can still be asked and answered. In addition the reformulation process demands that philosophers engage with science and the history of science in a more serious way than is usual in order to answer important questions about the justification for targeting a particular set of propositions (say) in a given context. All things considered, we should expect the benefits of a theory-eliminating debate-reformulation to heavily outweigh the costs for a highly significant number of debates of the relevant type.

1 Introduction

A great number of debates in philosophy of science focus on the properties of, and relations between, particular, named scientific theories. Just some of the examples in the literature are as follows: (i) debates about the consistency of classical mechanics, classical electrodynamics, Bohr's theory of the atom, and Newtonian cosmology; (ii) debates about whether the following theories are deterministic: classical mechanics, general relativity, quantum mechanics, and evolution theory; (iii) debates about the time-reversal-invariance of classical mechanics, classical electrodynamics, and quantum mechanics; (iv) debates about whether particular theories make particular predictions (too many to mention); (v) the debate about the mutual consistency of quantum theory and general relativity, the debate about the reduction of thermodynamics to statistical mechanics, and the debate about the (non-)identity of Schrödinger's wave mechanics and Heisenberg's matrix mechanics. Clearly all such debates crucially depend on the content that is ascribed to the theory or theories in question. Accordingly, any such debate may be disrupted by cross-talk if two proponents use a theory-name to refer in two subtly different ways. And any such debate will be seriously compromised if two proponents disagree on the (debate-relevant) content of the theory/theories in question.

In fact the opportunities for using theory-names in different ways and for deeply entrenched disagreements on theory-content are legion. For eighty years the major debates about theories have focused on their overall nature and structure, but even here agreement has not been forthcoming. It is still debated whether theories are made up of (or best represented as) axioms of first order logic, propositions, abstract models, model-theoretic models, Bayes nets, a state-space or phase-space, configurations of synaptic weights, 'façades', or a combination of different types of representational media.[1] If this covers the 'nature' of theories, there still remains the question of the 'structure' of theories. Do they have a finite set of specifiable constituents, or a 'core' or 'essence' and an 'auxiliary belt'?[2] Do they have an 'open' or 'closed', or 'mechanistic', or 'modular' character?[3] But this is just the tip of the iceberg. Even if philosophers of science agree that theories are finite, specifiable sets of propositions (say), they may still disagree on precisely which set of propositions counts as 'the theory' in a given case. Philosophers of science often disagree on where to separate the theory from the 'background assumptions', the physics (say) from the maths, the theory from its idealizations, approximations, and simplifications, and the theory from its interpretation, or the underlying metaphysics. This is often because, at a deeper level, they disagree on whether theories are instruments for calculation, predictive or explanatory

[1] For a flavour of the range of opinions, see Suppes (1967); Van Fraassen (1980); Giere (1988), Ch.3; Suppe (1989); Churchland (1989); Mahner and Bunge (1997), §9.3; Da Costa and French (2003); Wilson (2006); Henderson *et al.* (2010); Muller (2011).
[2] Gould (2002); Morrison (2007); Lakatos (1970).
[3] Bokulich (2006); Craver (2002); Darrigol (2008).

devices, representations of our beliefs, or our working hypotheses, or any of a number of other possibilities.

These considerations might lead us merely to appreciate one of the reasons the debates mentioned above are usually so difficult, and recognisable progress often so slow: disagreements about the properties of, and relations between, particular, named theories supervene on deeper disagreements about the content of the theory or theories in question. But one might also wonder whether this supervenience relation might be severed by some kind of debate-reformulation strategy. There is plenty of inspiration in the literature: in recent years debate reformulations have become popular, especially those which urge the elimination of some concept (e.g. Machery 2009). If the theory-concept is causing so much difficulty in so many debates, we might wonder what would happen if it were eliminated.

Since *scientific theory* is such a central concept in philosophy of science, one's intuition may be that theory eliminativism will cause chaos and prevent us from talking about most of the things we want to talk about. On the contrary I will argue that, at least for the class of debates mentioned above—concerning particular, named theories—it can be an extremely powerful tool for the philosopher of science: it is possible to reformulate debates so that reference to 'the theory' and use of theory-names is completely eliminated, whilst retaining all of the questions that really mattered to the original debate. In this way a major obstacle to progress in philosophy of science is removed. More exactly, I will argue that we should expect the benefits of a theory-eliminating debate-reformulation to heavily outweigh the costs for a highly significant number of debates of the relevant type. I start with two concrete cases by way of example, before turning in later sections to the general story, consideration of benefits versus costs, and responses to objections.

2 Is Classical Electrodynamics Inconsistent?

My first example 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? In Vickers (2008) I argue that the disagreement hangs on the fact that what Frisch means by 'classical electrodynamics' is not what Muller and Belot mean by 'classical electrodynamics'. Muller 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.

One response may be that all we have here is a terminological/referential disagreement. But reading Muller's paper there is a clear (if implicit) view that Frisch's conclusion is without value since what it is attached to is 'not the theory'. Belot 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 is not. 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, I have argued (Vickers 2008) that if one is initially tempted by Frisch's conception of the theory, Belot's objections carry little weight.

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 [...].

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 to 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 that 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. It will be enough if I can show that, once one has filled in the three sets of square brackets in the reconstruction given above, asking this further question about whether we’re still talking about CED will not add anything of interest or importance vis-à-vis inconsistency in science, or how science works more generally.

In fact, Frisch fills in the first two sets of square brackets perfectly well (2005, p.33ff.). The big question, though, is why (if) it is an interesting or important result. 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’. 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, it is clear that the assumptions Frisch presents were not all considered ‘candidates for the truth’, so Frisch’s inconsistency could never be interesting in this sense.

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 at least one of which is definitely 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. 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 (e.g. the conflict between general relativity and quantum theory).

Whether Frisch's claims are in fact defensible in this way 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. One may ask how we would answer the question 'But is that really CED you're talking about?' The answer will be that that is beside the point. One might reply: "No, but so what? – It's nevertheless an interesting result for the reasons just given concerning trustworthiness of inferences." Or one might reply: "Yes, but that's not what makes it an interesting result – what makes it an interesting result are the reasons just given." Or one might reply: "You're question is based on a false premise about the identity of CED—the answer is neither 'yes' nor 'no'—but anyway, what makes it an interesting result are the reasons just given."

This shows one sense in which nothing of significance is lost if we reformulate the debate according to the above schema. Personally I find it difficult to imagine how else something important *could* be left behind if we eliminate theory-talk. Some further considerations on this point will be discussed in §4, below.[4]

3 Is Classical Mechanics Deterministic?

Before I turn to the general story it will be instructive to see how theory eliminativism applies in another case, concerning a different theory and a different property. One obvious candidate is the longstanding debate as to whether 'classical mechanics' (CM) is deterministic. Several authors have urged that it is indeterministic, whereas others maintain that it is deterministic, such that one reaches the opposite conclusion only by misunderstanding 'what the theory is'.[5] 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.

But instead of claiming that 'classical mechanics' is/is not deterministic, one can instead eliminate talk of 'classical mechanics' from the debate by reformulating one's statements as follows:

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

In this way, any disagreements about whether classical mechanics is deterministic based on disagreements about what the content of the theory *is* will disappear from view. In this way, progress is much more readily achievable on the question of whether there are any interesting or important indeterminisms concerning hypotheses made about the mechanics of moving bodies.

[4] The (in)consistency of classical electrodynamics is considered in greater detail in Vickers (2013, Ch.4).

[5] In the first camp we find Earman (1986), Hutchison (1993), and Norton (2008). Amongst those who think the theory is deterministic we find Arnold (1977), Korolev (2007), and Zinkernagel (2010).

But as with the case of the consistency of CED, the major question which arises is whether in eliminating reference to ‘the theory’ we lose something important from the original debate.

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.

But these issues can be debated more efficiently without talk of ‘classical mechanics’ or ‘the theory’. First (i) one can identify the assumptions pertinent to the point one is trying to make—e.g. Newton’s three laws of motion. Second (ii) one can show that, given these assumptions, the mechanics of bodies is indeterministic (say) 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’ (Norton 2008). One might argue that relevant scientists made other assumptions, in addition to Newton’s laws, which do then assure determinism in the given contexts (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 (Zinkernagel 2010), or by arguing that Newton’s laws by themselves entail neither determinism nor indeterminism (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. The answer to the question "But is that really *the theory* you're talking about?" will follow the model of the previous case study (above).

4 Costs and Benefits of Theory Eliminativism

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 is a set of analysanda which have the property X [...]. This can be demonstrated as follows [...]. What is interesting and important about the fact that these particular analysanda have that property is as follows [...].[6]

Mutatis mutandis for debates about relations between particular theories. The important question now is why we should expect such a reformulation to be beneficial to a 'highly significant number' of the debates mentioned in §1, above.[7]

First of all it is clear that the reformulation process will eliminate talk of 'the theory', and use of theory-names, for every such debate. This has some immediate benefits: miscommunication due to subtle differences in intended reference of theory-names ceases to be possible. More importantly, any stalemates due to deeply entrenched disagreements about the content of the theory in question are dissolved. This is because one does not just eliminate a word, keeping the corresponding concept in the background guiding one's analysis. Instead the debate continues entirely without the concept *theory* and (crucially) without other concepts dependent upon the concept *theory*. One talks instead in terms of the specified analysanda (assumptions, models, equations, axioms, whatever), one's conceptions of which do not depend on one's theory-concept, since they are more basic.

[6] The term 'analysanda' covers assumptions, equations, models, axioms, propositions, or whatever set of things the philosopher wishes to consider together as a unit of analysis. There need be no restriction here, since the justification for targeting that set of things will be given in the third set of square brackets.

[7] In Vickers (2013) I apply theory eliminativism to a number of debates concerning the (in)consistency of particular 'theories'. The fullest argument for theory eliminativism combines the more general considerations in this paper with the concrete applications found in Vickers (2013).

These will be important benefits of theory eliminativism only if there existed in the original debate some cross-talk or disagreement concerning the theory-content in question. Probably this won't happen in every case: in some debates we might expect that any disagreements over theory content will concern content that isn't at issue for the property or relation in question. E.g. if we are debating whether a theory makes a given prediction, it won't matter if philosophers disagree about theory-content that doesn't play any role in generating that prediction. But on the other hand one might well expect there to be relevant disagreements over theory-content for many of the debates in question. As I noted in §1, the conceptual space available to us when we consider the content of a given theory is extremely large. Philosophers cannot agree on even the *type* of thing theories are made up of (even philosophers working in the same sub-discipline, e.g. philosophy of physics—the debates discussed in §§2 and 3 are cases in point). And alongside these large differences of opinion on theory content, debates about the properties of/relations between theories are usually extremely sensitive to the content that is ascribed to the theories in question. The slightest change in content can make the difference to whether a theory is inconsistent, deterministic, time-reversal-invariant, and so on, or to whether two theories are equivalent, mutually inconsistent, reducible one to the other, etc.

With these considerations in mind it is no surprise that concrete examples in the literature are not hard to find. In addition to general considerations which suggest that there will be many such examples, I can here add a few more concrete examples to those already given. First, consider the consistency of Bohr's theory of the atom. Lakatos (1970), Brown (1992), Priest (2002), and Da Costa and French (2003) claim that it is inconsistent, whereas Bartelborth (1989), and Hettema (1995) claim that it isn't. Who is right? It depends on which parts of CED we include as part of Bohr's theory. Bartelborth argues that only 'quasi-electrostatics' should be admitted, whereas Brown suggests that the whole of CED must be included because there were applications of old quantum theory where CED was used. The question arises whether we have an internally inconsistent theory, or two theories (Bohr's theory and CED) which are mutually inconsistent. But even if we take the 'one theory' view, 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 (≈). (p.261)

So whether we should refer to 'Bohr's theory' as inconsistent depends crucially on one's views about how one should decide upon and interpret the content of the theory. And one finds multiple disagreements on just this issue in the relevant literature.[8]

Consider also the question of whether Bohr's theory predicts the intensities of the hydrogen spectral lines. Smith (1988) argues that it does because it includes (a particular formulation of) 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. And philosophers really do disagree on the content of the theory in such a way that they disagree on whether the theory makes the prediction in question.

As a final example—this time concerning a relation between two different theories—consider the debate as to whether Heisenberg's matrix mechanics and Schrödinger's wave mechanics are equivalent. The common assumption that Schrödinger in 1926 proved the equivalence of the theories has been labelled 'a myth' by Muller (1997a, 1997b), but yet we *do* have an equivalence according to Perovic (2008). The reason for the disagreement does depend in part on how one interprets 'equivalence'.[9] But it also undoubtedly depends on the content attributed to 'matrix mechanics' and 'wave mechanics': Perovic argues that Muller's inequivalence claim is based on 'a narrow model of physical theory'. In other words it is based on Muller's particular view of what constitutes the theories, a view that Perovic does not share.

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[8] For more on the (in)consistency of Bohr's theory of the atom, see Vickers (2013, Ch.3).
[9] Similarly with many of the other properties and relations in many of the other debates I have mentioned.

It remains an open question how many debates of the relevant type are currently stumbling on the concept *theory* in the stated way, such that theory eliminativism is warranted for this reason alone (given insignificant costs—see below). The prior discussion suggests it will be a significant number, but this is just one consideration: the warrant for theory eliminativism does not depend solely on this. Another important benefit concerns the new questions that the reformulation process forces one to answer. Specifically, 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. Instead the reformulation process demands that philosophers answer important questions about the justification for targeting a particular set of analysanda. Why, exactly, are the selected analysanda important to consider together, as a set, in the given context? Is it because of their relevance to the history of science, or certain scientific characteristics of the set, such as predictive and explanatory power? In forcing us to ask such questions, theory eliminativism works as an indirect means for bringing philosophers of science to engage more directly, more seriously, with science and the history of science. Consequently it will either be stated much more clearly why the claim in question is an important result, or in the process of reformulation 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'.

These are the principal benefits of theory eliminativism. What are the costs? In the case studies considered in §§1 and 2 I considered the possibility that reformulation leaves behind something important from the original debate, and argued that this was not the case. Should we expect this to generalise?

I can't hope to argue that *nothing* of significance would be left behind from *any* debate. This would require an in-depth study of each debate. But I can at least consider the most obvious questions that would be left behind if we eliminated *theory*. The first such question is the one already considered in §§1 and 2: one can no longer ask "But is that really *the theory* you're talking about?" Suppose somebody does ask this. Then there is no difficulty in generalising the answer already presented in §1, above. One answers "Who cares? I've already told you all of the reasons why it's interesting and important that the specified set of analysanda has the property it does—so it adds nothing to the importance of the claim if that set *is* the theory, and it takes nothing away if it isn't." (*mutatis mutandis* for relations between sets of analysanda).

But in addition one might wonder whether we lose the ability to ask whether the set of analysanda in question would have been (or is) referred to as (part of) 'the theory' by scientists. One might be concerned that, since one can't use the term 'theory', one can't ask this question, and so one loses the ability to consider a sense in which the established result (e.g. inconsistency of a set) has historical relevance. In other words one might think that without using the term 'theory' one can't properly fill in that all-important third set of square brackets.

But this is to mix up asking questions about theories and asking questions about how certain terms such as 'theory' and 'classical mechanics' were used by practitioners in the history of science. Theory eliminativism rules out conceiving of the history of science as containing theories as things that exist (in some sense) and have properties, stand in relations, etc. But it does not rule out questions about the way scientists use/used terms such as 'theory' and 'classical mechanics', or how they thought these terms referred. In the course of filling in that third set of square brackets one might want to start by arguing that scientists working at the time referred to the specified assumptions as 'the theory'. One would then have to make the case that this leads to important and/or interesting conclusions. In general, in filling in this third set of square brackets, it's going to be more important to consider which analysanda scientists used, how they used them, whether they believed them, and so on; it's not going to be so important to consider how scientists used certain terms to refer. But theory eliminativism, properly understood, does not rule this out.

Indeed, theory eliminativism does not even rule out contemporary philosophers making 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 analysanda '(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. In fact, in the debate over CED, Frisch tells us explicitly how he is using the term 'theory' (2005, p.26) but this didn't stop Muller and Belot responding 'That's not the theory!' (give or take).

These considerations go some way towards indicating why nothing of significance will be left behind if we reformulate debates of the relevant type. Some readers may think they don't go far enough. But given the potential benefits of theory eliminativism, and the concrete examples provided above (and also in Vickers 2013), perhaps it will not be too much to shift the burden of proof at this stage. Despite the fact that we are asked to manage without a concept which is currently ubiquitous in the literature, why should we expect something to be lost? We may ask: what can be said in terms of 'theory', that cannot be said in terms of analysanda (equations, models, propositions, etc.) which are being put together and considered as a group in a particular context for particular purposes? I put it to the objector to provide such examples. One example would not do, since I am not arguing that theory eliminativism should be applied to *every* debate of the relevant type: to repeat, I am only claiming here that we should expect the benefits of reformulation to heavily outweigh the costs for a highly significant number of debates of the relevant type. And it wouldn't be enough if the cost is small, since I've already indicated the significant benefits one might expect.

This covers the principal benefits and costs. How do they bear on my conclusion that we should expect the benefits of a theory-eliminating reformulation to *heavily* outweigh the costs for a *highly significant number* of debates of the relevant type? I use the word 'heavily', since the costs are almost non-existent (I have found nothing of significance that is left behind), and the benefits can be enormous (dissolution of stalemates in the literature; emergence of new questions which force philosophers to engage more directly and seriously with science and the history of science, and thus to consider more carefully the deep reasons why we should care about their conclusions). I say a 'highly significant number' for two reasons: first, I expect—given what I said about the sheer number of different ways in which philosophers can and do disagree about the content of theories, and the apparent ubiquity of examples of such disagreements affecting debates in the literature—that *many* of the debates I mentioned in §1 will benefit from theory eliminativism. But second, it would be highly significant if just a few debates benefited, since the reformulation of these debates would fruitfully re-direct the research energy of a large number of philosophers in the field. For a concrete example one need only reflect on how things would have developed if Frisch (2005) had followed the eliminativist model.

5 Objections and Replies

Responses to some of the most obvious objections are embedded in the clarifications of the previous section. Some further possible objections are considered below.

Objection 1. "Theory eliminativism does resolve certain problems, as claimed, but these problems get resolved anyway in the natural course of philosophical debate." I should perhaps emphasise that I do not claim that theory eliminativism is the *only* way to circumvent miscommunication and dissolve stalemates based on the identity of theories. It is just a way to achieve these ends quickly and effectively. And since I do not claim that the benefits of theory eliminativism will always outweigh the costs, I will happily grant that sometimes the problems will be quickly resolved *without* eliminating the theory-concept. But theory eliminativism is worth taking seriously if there are some *other* times when the problems would *not* be resolved quickly without eliminating the theory-concept. And in addition one needs to factor in the benefit concerning the justification for targeting a particular set of analysanda. In this case there isn't an obvious disagreement crying out for resolution, as in the case of a miscommunication or stalemate. Instead there is simply an important question which is not being asked. Can we sit back, confident that it *will* be asked, and answered, in the natural course of philosophical debate? Since there is no obvious disagreement there is every chance it will remain ignored or overlooked for a significant period of time, especially if protagonists tend to think one can justify targeting certain analysanda by simply saying "They *are* the theory" (cf. the concrete examples discussed above). Theory eliminativism ensures protagonists have something far more substantial to say about why exactly their chosen analysanda deserve attention in the context of the specific debate in question.

Objection 2. "If there's a problem with the way people are thinking about theories, which is then affecting other debates, why not put our energy into clarifying what a theory is, drawing on theories of theories or conceptual analysis?" Nickles (2002, pp.8-11) makes this suggestion for debates about inconsistency in science. We should sort out our theory of theories first, he says, since if theories turn out to be families of models then they won't even be the *kind* of thing which can be inconsistent. But this is too optimistic: it asks us to shelve our debates about the properties and relations of particular theories until we know what theories are. But eighty years of work in philosophy of science has brought us nowhere near to a consensus on the nature and structure of theories. And even such a consensus would not tell us how to identify the *particular* constituents of a particular theory. This is not to rule out literature on 'theories of theories' as worthless: it can still play an important role in any reformulated debate. Giere (1988) can put together a family of abstract 'scientific' models as his analysanda, and Muller (2011) can put together a "set of structures in the domain of discourse of axiomatic set-theory, characterised by a set-theoretical predicate." 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 set of square brackets.

Turning to conceptual analysis, one wouldn't even know which theory of concepts to start with to attempt a conceptual analysis of *theory*: options include neo-classical theory, prototype theory, the theory theory, the neo-empiricist view, and conceptual atomism. Then there is the question of the relationship between the concept *theory*, the concept of a particular theory (e.g. *classical electrodynamics*), and the content that should be ascribed to a particular theory. This is not a helpful solution; it is a minefield.

Objection 3. "You need to argue that theories don't exist. If they do, then 'Is that the theory you're talking about?' remains an important question. And in the reformulation, we lose this important question." In fact it's not such a popular claim these days that theories exist as human-independent entities; Popper's arguments to this end are widely discredited as unsound. At any rate, the burden of proof here lies with the objector, who is making the strong positive claim that theories do exist. And even if they do, my claim is only that one doesn't lose anything *from the original debate* if you reformulate. E.g. consider again the debate over the consistency of CED: the inconsistency can be highly interesting, regardless of whether the unit of analysis *is* or *is not* the theory, and also regardless of whether we *should* or *should not* call that unit of analysis 'the theory'.

Objection 4. "If lots of people have different views on theories, and in particular on the content of particular theories, why not be a pluralist about theories?" Pluralism would seem to resolve some of the tensions noted in the above case studies. For example, in the debate over the consistency of CED 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) and Suppe (1989) 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, or the three major acid-concepts? Given the multitude of disagreement already noted, 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 too 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.

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, and relations between, particular theories. The main problem here is 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 contents of CED provided by Frisch, Belot, and Muller in the debate mentioned above do not 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, 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.[10]

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

[10] 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.

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 analysanda. In fact this just brings us to the theory-eliminativism advocated here: if one has specified 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: she is committed to a plurality of individuals, all of which deserve the title ‘formulation *of the theory*’ for some reason that requires articulation.

Objection 5. Let me respond to the reader who would prefer the label ‘theory-quietism’ to ‘theory-eliminativism’. 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 (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 claim (so it’s not like claims for ‘species’-eliminativism, ‘innate’-eliminativism, ‘belief’-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 proposal is close enough to other eliminativisms that ‘theory eliminativism’ is a perfectly sensible term. For example, in Machery (2009) the motivation for ‘concept eliminativism’ is partly pragmatic, and so application will be selective (dependent on pragmatic factors), just like theory eliminativism.

6 Conclusion

These are necessarily preliminary considerations, and as such it is important to be neither too optimistic nor too pessimistic. Too optimistic and one is in danger of making overblown claims about a revolution in philosophy of science, a discipline which traditionally has put heavy weight on the concept *scientific theory*. Too pessimistic, and one may miss an opportunity. If, as I claim, the benefits of theory eliminativism will heavily outweigh the costs for a highly significant number of debates of the relevant type, then that would be remarkable. The two concrete examples of §§2 and 3, together with the general considerations of §4, persuade me that we have good reason to expect highly significant consequences to result from wider employment of theory eliminativism. But even if I'm overly optimistic, concerted action is warranted if there is but a small chance of a highly significant impact.

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