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Opinion Piece: *Theories in History and in Practice*, Stephen French

Steven French is Professor of Philosophy of Science at the University of Leeds. He has taught in Brazil and the USA and is the author or co-author of several books and numerous papers, including most recently *The Aesthetics of Science* with Milena Ivanova (Routledge 2020).

Introduction

What is a scientific theory? We can certainly point (figuratively and literally) to any number: the Boveri-Sutton chromosome theory, Ragnar Nurske's balanced growth theory of economics, the molecular orbital theory of molecular structure and of course those old favourites, Newtonian mechanics, Maxwell's Theory of Electromagnetism, the Special Theory of Relativity, quantum theory …the list goes on. But can we characterise or otherwise pin down what a theory *is* in terms that go beyond simply listing examples? One way of answering this question might be to look to the history of science and the practices of scientists themselves. Let's consider the last example in my list, quantum theory.

A Little History of Quantum Physics

As is very well-known, during the mid- to end-1920s there were various alternative theoretical constructions in play, including not only Schrödinger's wave mechanics and Heisenberg's matrix mechanics, of course, but also Dirac's 'general science of non-commuting quantities' and Weyl's group-theoretic approach (see Bueno and French 2018). However, as it turned out, despite Dirac's aversion to the latter, his 'transformational' framework is mathematically the same as Weyl's. And as Schrödinger indicated and von Neumann subsequently demonstrated, the former's mechanics and Heisenberg's are also equivalent (Muller 1997). Now, for many commentators, including physicists as well as philosophers of physics, it is the von Neumann formulation, with its representation of states as vectors (or more generally, rays) in Hilbert space, and observables as operators, that provides *the* theoretical framework 'of' quantum mechanics – question begging alert! – although many, especially physicists themselves, would agree that Dirac's approach, with its 'bra' and 'ket' formalism, offers certain pragmatic advantages.

von Neumann himself was dismissive of Dirac's framework, incorporating as it did the infamous 'delta function' which von Neumann regarded as mathematically self-contradictory (see Bueno and French 2018 Ch. 7). But then he also became dissatisfied with his own Hilbert space formulation, and attempted to delineate an entirely new framework based on a mathematical structure known as continuous geometry. And just as von Neumann criticised Dirac for his lack of rigour, so Weyl admonished advocates of Heisenberg's matrix mechanics as introducing treatments of variables that were 'mathematically unsatisfactory and physically unfeasible' (Scholz 2007), offering his group theoretic approach as a way of yielding 'deeper insight into the true state of affairs' (ibid.).

So, although these different mathematical frameworks can be shown to be interrelated – wave and matrix mechanics are just different representations on Hilbert space; Dirac's transformational account was equivalent to the group-theoretic; the latter yields the Hilbert space formulation via its representations – they embodied different motivations and offered different advantages. In particular, we all know (don't we?!), that Schrödinger was a 'naïve' realist, defending (hopelessly, or so it is typically claimed) a wave-based conception, whereas Heisenberg was – to put it crudely –an equally naïve positivist, focussing on the representation of observable quantities.

Of course, their attitudes and those of Dirac's were more complex than that (Kragh 1990) but even if one felt that such attitudes have more to do with the stance one should take with regard to 'the' theory, rather than how one delineates the latter, the crucial point remains that *the quantum 'revolutionaries' differed with regard to what they took 'the' theory to be and what principles they felt sat at the heart of it*. Thus, for Heisenberg it was wave-particle duality, understood, at least early on, in the context of Bohrian complementarity. Yet, this did not feature at all in Dirac's book, *The Principles of Quantum Mechanics*; rather he emphasised the analogy with classical mechanics afforded by the relationship between Heisenberg's non-commuting products and the Poisson brackets of classical dynamics (Kragh 1990). And as we have noted, von Neumann, who in his *Mathematische Grundlagen der Quantenmechanik* of 1932 undertook to provide quantum mechanics with a secure mathematical foundation, rejected Dirac's framework as insufficiently rigorous.

All of these authors were obviously seeking to disseminate what each thought were the basic precepts of the new theory. As Kragh puts it, with regard to Dirac's *Principles*: 'He wanted to shape a theory which had not yet found its final shape.' (ibid) Which raises the obvious question(s): How and when does a theory get its final 'shape'?

As Kaiser has noted:

> [r]ecent scholarship has highlighted the striking heterogeneity—even cacophony—of competing assumptions, approaches, and interpretations during the early years of quantum theory, even among physicists who worked closely together and whose views had earlier been considered synonymous …Indeed, we might well wonder whether any coherent conceptual trajectory connected, say, Planck's publications in 1900 with Heisenberg's, Born's, Jordan's, Schrödinger's, or Dirac's papers in the mid-1920s. (Kaiser 2013)

You might be inclined to dismiss these contrasts as a more or less natural result of the contestation that always follows a major scientific advance, with different parties pushing their different agendas. However, the issue of how we should delineate 'the' theory has continued to resonate. Certainly, these quick remarks do, at least, indicate that what was taken to be the theoretical content of 'the' theory, or even the extent to which it could be taken to 'have' such content, was disputed from the very beginning of the quantum revolution. And of course this point is sharpened further by the well-known divergences between the different 'interpretations' (so-called) of quantum mechanics, from Bohmian mechanics to the Many Worlds View, from the GRW interpretation to wave function realism (see for example French and Saatsi 2020). If part, at least, of the theoretical content of 'the' theory is expected to be cashed out in stating how the world is, or could be, according to that theory, then these interpretations offer alternative contents and the continuing debate demonstrates that this issue is not confined to the quantum revolution itself, nor its immediate aftermath.

The point, then, is that we need to abandon the idea that the history of the field, or the relevant practices of the scientists in general, supports the claim that there is 'a', or 'the' theory of quantum mechanics, as a unitary and well-delineated entity, with definite identity conditions. This was clearly not the case at the time of the so-called quantum revolution, nor in the immediate aftermath, nor subsequently, if we understand a theory, *qua* entity, as incorporating some claim as to how the world is, or could be.

Quantum Physics is Special

Now you might say that quantum mechanics is somehow a special case and that this point cannot be generalised. Well, we can easily go forward from the quantum revolution and ask the question: what is quantum field theory (QFT)? Is it the axiomatised construction beloved by those who are members of the so-called 'Algebraic QFT' camp? Or is it that which physicists themselves actually use? These are significant questions because QFT is widely lauded as yielding some of the most precise predictions ever made in science and therefore as clearly being worthy, and perhaps more so than other theories, of acceptance by the realist and also because these questions bear on concerns that are important for philosophers of physics and realists alike.

Advocates of Algebraic QFT insist that the only way one can avoid the infamous infinities that plague 'the' theory is by reformulating QFT on an axiomatic basis. The problem is that, as typically formulated, these axioms do not cover or accommodate interactions and hence if Algebraic QFT is taken to be 'the' theory, it is strictly empirically inadequate. In particular, the axioms do not cover the well-known Standard Model of high-energy physics, recently given a further epistemic boost by the discovery of the Higgs boson. Those who urge philosophers to shift their focus to what physicist actually use in their practice(s) have argued that this – termed 'Lagrangian' or 'naïve' QFT – can be rendered perfectly well-defined and kosher, subject to certain caveats, and, furthermore, that it is, in effect, 'the' QFT that underpins the Standard Model (Wallace 2001).

These two options have been presented as opposing horns of a form of underdetermination, with grounds given for favouring one over the other. Again, what we have here is a dispute over what should be 'the' theory of QFT. To what extent those grounds are decisive is dependent on the weight given to such virtues as consistency, for example, or to the promise of a research programme but the point I want to emphasise here is that both options can be seen as constructions or, better perhaps, representations and that we should not blithely accept that there is a real issue as to which one should count as 'the' theory (Fraser forthcoming).

Still, you might continue to be unimpressed, maintaining that even more so than QM, QFT is still in an indeterminate conceptual state and further work is needed before we can delineate the outlines of the theory itself. So, let's shift 'backwards', in a sense, and ask, 'what is *classical* physics?'

Classical Physics: Lifts, Quilts and Facades

This is the question with which Gooday and Mitchell kick off their historical analysis of the distinction between 'classical' and 'modern' physics (Gooday and Mitchell 2013). They argue that this distinction emerged over a long period of time, extending into the 1930s, and depending on the geographical location considered. And they conclude that classical physics only ever existed in the limited sense that the label was developed and attributed by theoreticians in the early twentieth century '…who sought to preserve a restricted role for established theory and techniques whilst setting forth a future research programme based on new forms of theorizing' (ibid., p. 751). Any reference to 'it' prior to 1900 implicitly adopts '…an anachronistic perspective that was created to legitimize the new foundations for physics proposed within relativity and quantum theory.' (ibid.)

As an antidote to such anachronisms, studies of the relevant continuities can be deployed and presented as 'ironic' rejoinders to Kuhn's claim about the rendering invisible of revolutions by the adherents of the new paradigm: rather than committing a form of patricide, physicists constructed a 'classical' identity for their forebears in order to serve their own interests. Thus, '…the apparent unity of 'classical physics' [should be seen] as the *post hoc* creation of twentieth-century theoretical physicists seeking to consolidate new departures within their discipline.' (ibid., p. 722)

But moving to the particular, what about classical mechanics itself? Surely, you might say, there is no doubt about *its* identity – we simply have to recall and write down Newton's laws and we're done! Setting aside for the moment the whole issue of whether Hamilton's 'formulation' counts as such or should be considered a distinct theory itself, the form in which these laws were given in the Principia is, of course, very different from how we would write them today. Furthermore, they have been subject to different interpretations that in some cases undermine their status as laws, at least laws as standardly conceived: Poincaré, for example, argued that the first law is a convention; the second has often (perhaps erroneously) been taken to provide the definition of 'force' and the status of the third has been described as 'hazy'. Indeed, as Wilson warns us:

> Classical mechanics is frequently characterized as 'billiard ball mechanics' or 'the theory of mechanism' on the grounds that the science treats its materials in the manner of colliding particles or clockwork. The reader should approach such stereotypes with caution because the basic framework of classical mechanics has long been subject to divergent interpretations that unpack the content of Newton's "three laws" in remarkably different ways. These differing interpretations provide incompatible catalogs of the basic objects that are supposed to comprise the 'classical world' – should they be point masses, rigid bodies or truly flexible substances? (Wilson 1996).

Taking up that last question, as originally stated these laws could not be applied to rigid or deformable bodies, and it was Euler (again) who generalised them, although Euler's laws can also be taken as a distinct set of axioms for the behaviour of such bodies. But the basis of this generalisation is not conceptually straightforward because thinking of rigid bodies or continua as merely 'swarms' of point masses held together by short-scale cohesive bonding cannot serve to underpin the empirical success involved, nor will it help illuminate the various conceptual issues in play (Wilson 2014). In particular, what might be seen as the 'triumphant hegemony' (ibid. p. 103) of classical mechanics owes a great deal to the often hidden contribution of what Wilson terms 'lifts', which are basically devices and manoeuvres that take one between different levels of description, demarcated by different characteristic scale lengths and typically different ontologies.

So consider the example of a steel beam and the shifts involved as we move from the level of the 'bulk' steel, to that of the crystalline grain, and then to that of the molecular lattice and finally to that of point mass atoms bound together. And these lifts may be infected with various dubious presuppositions, such as, and typically, that certain rules and principles applicable at one scale can be exported unproblematically to another.

Thus, the very notion of 'force' alters its significance via such lifts: consider friction, for example, regarded at one level as a straightforward Newtonian force opposing forward motion, but from the perspective of another, this 'force' incorporates the stretching effects that the mass of the object has upon the material, causing it to travel further than is apparent. Another classic example is that of the viscosity of a fluid, typically analysed in terms of the shear 'forces' on units or blocks of fluid that from a foundational perspective are, at best, ontologically ephemeral but essential for the relevant description at the level of fluid mechanics.

Contentiously, perhaps, Wilson claims that axiomatic presentations simply do not accommodate these shifts in ontological perspective and we are left with 'doctrinal holes', the filling in of which raises deep conceptual issues. But more significantly perhaps, these lifts and strategies, devices and moves of various kinds, form a crucial part of the practice of modelling, generating a 'compendium of descriptive lore' in terms of which classical mechanics is best viewed as a series of descriptive patches, linked together by these very manoeuvres (Wilson 2014, p. 19).

As a result, Wilson urges us to abandon the attempt to impose 'internal conceptual closure' in such cases and instead replace 'theories', as our unit of philosophical interest and as standardly conceived, with 'theory facades', which are quilt-like assemblages that 'look kinda like theories if you don't look at them too closely' (2014; p. 20; 2006). From this perspective, one can better appreciate and understand the kinds of moves we find in textbooks of classical mechanics, for example, as we move up (or down) from one descriptive level to another and also the kinds of conceptual shifts associated with such moves.

Conclusion

What should we make of all this? If we just take classical mechanics, Wilson's description of the relevant practices in terms of Frankensteinian 'theory façades', consisting of patches and lifts, holes and ladders, moves and manoeuvres of various kinds, all cobbled and bolted together, undermines any account that takes these practices to be 'about' the theory regarded as a clearly delineated thing with well-defined identity conditions. The similarly brief reflection on the history of quantum physics likewise raises concerns for such accounts. Certainly those who take theories to be 'things', in some sense, perhaps 'living' in some abstract realm or 'World 3', as Popper famously thought, face apparently insuperable obstacles in squaring such a view with these histories and the practices of scientists themselves.

These vignettes thus cast doubt on the idea that scientific practices, as represented in the history of science, or, I also argue, textbooks and the like, or scientists' reminiscences …demonstrate or indicate in some way that there *really* are theories 'out there' and as a result, following this train of thought, that philosophers of science should, indeed, come up with an ontology of theories that reflects these practices.

As I've tried to suggest, these practices are complex, overlapping and, in some cases, entangled. It is simply not clear how we should delineate classical physics from quantum physics, for example, or what counts as the relevant theory in either case. We can shift our terminology to 'theory façades' or 'frameworks' but those terms obscure the diversity and the complexity of what scientists do and come up with. Best, I would suggest, to drop the idea that they come up with something, that then lives in some Popperian realm, say, and accept that what we are presented with in the histories and the textbooks and the reminiscences is no more than a kind of construction for which certain features of the relevant practices have been emphasised and highlighted for all sorts of different purposes. In other words, we should accept that *there are no such things as theories* (French 2020).

Now, we can still make claims such as 'quantum mechanics is empirically supported' but what makes that claim true is not some feature of an abstract thing, 'the' theory of quantum mechanics; rather it is the set of relevant practices, both theoretical and empirical. Perhaps even more interestingly, shifting to these practices as the 'truth makers' of such claims also affords a new understanding of statements such as 'quantum mechanics is beautiful', for example. Taking this to attribute a quality to the theory conceived as a thing then takes us into the whole morass of issues regarding whether such aesthetic qualities are epistemically significant or not. If instead we take it to be made true by certain practices, that morass can be neatly sidestepped.

Finally, and even more broadly, such a move motivates a novel perspective on how we should see our own practices as historians and philosophers of science. Consider the on-going debate within the philosophy of science between the adherents of the so-called 'Syntactic' and 'Semantic' Approaches, about whether theories should be taken to be axiomatised sets of sentences or families of models. Both sides assume that there is something 'out there', *the* theory, that is better represented in formal terms one way or the other. However, from my perspective that assumption should be discarded and instead the debate should be recast in terms of which framework better suits *our* practices as philosophers of science, where these practices and their attendant aims may differ depending on which features of *scientific* practice we are concerned with. Although scientists' reflections on their own practice may lead them to present a certain 'formulation', for want of a better word, as the theory of their given field, we should be wary of falling into the same trap – what we are engaged in is not *representation* so much as *presentation*, in this case of a certain characterisation, whether formal or not, that we claim then enables us to better understand those scientific practices.

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