# 18 Travelling in New Directions

*Steven French and Juha Saatsi*

## 1. Introduction

This chapter discusses some emerging trends, new directions and outstanding issues in philosophy of science. The next section places contemporary philosophy of science in context by considering its relationship to analytic philosophy at large, to the history of science and to science itself. Section 3 then takes a look at a selection of interesting trends emerging from current research and some important issues calling for further work. The presentation is inevitably coloured by our personal perspectives, and we cannot hope to do justice to even the majority of new developments. But we hope to be able to convey some sense of the range of problems, issues and areas in which new and interesting research is being conducted.

## 2. Philosophy of Science in Context

What does the future hold for the philosophy of science? Certainly the field overall appears to be in rude health, as we hope the essays in this volume testify. At the 'global' level, its changing and maturing relationships with both the history of science and philosophy – with epistemology and metaphysics in particular – offer the possibility of new perspectives from which to not only view but perhaps contribute to science itself. In its analyses of particular features of science – the nature of evidence, the role of explanation, the possibility of reduction and so on – new resources are being brought into play and fresh approaches delineated.

### 2.1 The Place of Philosophy of Science in Analytic Philosophy

Philosophy of science is central to many areas of contemporary analytic philosophy, due to the broadly naturalistic inclination of the latter. In as far as naturalistic philosophers see philosophy as 'continuous with science' and as



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being informed – to some extent, at least – by our best science, it is the philosophy of science that naturally assumes a *mediating role* between science and other sub-disciplines of philosophy. Such a mediating role can be fulfilled in many ways – as illustrated through examples below – and it need not be a one-way process: philosophical analyses of a more general sort can feed into philosophy of science and, ultimately, even into science itself.

Many current philosophers of science are taking the mediating role seriously, and rightly so. Take, for example, the concept of 'natural kind', central to numerous philosophical debates ranging from the philosophy of language to metaphysics. Several people have recently argued that the simple essentialist concept of 'natural kind', originating from Kripke and Putnam and still forming a staple of many a traditional debate, is problematic in the face of actual science. Regarding paradigmatic chemical kinds, for example, some have argued that there's an element of *stipulation* in specifying what a natural kind name like 'water' refers to, and others have argued that water, say, cannot be identified on the basis of some *microstructural essence*, as being simply $H_2O$. (For recent discussion on these issues, see Beebee and Sabbarton-Leary 2010.) Brigandt, in his contribution to the present volume, discusses related issues in the context of biology, and Hendry covers them in the context of chemistry. Both authors argue that the relevant scientific detail should be brought to bear on general philosophical debates, in as far as the concept of 'natural kind' is to be grounded at all in the way that science actually classifies the world.

'Natural kind' is but one example of a central philosophical concept that can potentially be sharpened by scientifically informed philosophy of science. Another example is that of 'cause'. To give an illustration of how much in analytic philosophy depends on our understanding of the concept of causation, consider the following. Many philosophical positions (in the philosophy of mind, for example) accept 'Alexander's dictum', according to which (qualitative) properties are real only if they are 'causally powerful'. Premised on this assumption, and armed with various kinds of conceptual analyses of causation – grounded mainly on armchair intuitions – philosophers of mind have debated for years about the reality of mental states and mental causation. The challenge has been to avoid the so-called causal exclusion problem, according to which the higher-level mental properties are causally otiose, and therefore epiphenomenal. Recently, some very profitable moves have been made in the debate by approaching the concept of causation from a more science-driven perspective: the counterfactual 'interventionist' theory of causation, due to Woodward (and others), is inspired by the actual structure of causal explanation in physics and in randomized experiments, for example. According to Woodward (and others who follow similar approaches to causation) much of the intuitions-based literature on the topic fails to analyse the nature of causation and causal explanation correctly 'from the armchair', and arguably their science-driven account – motivated by very different problems at the core of philosophy of science – provides a natural solution. (For recent discussion, see Hohwy and Kallestrup 2008.)

The debate about the reality of the mental is largely driven by the adoption of some kind of physicalism, according to which fundamental physics describes causal facts of the world at 'the bottom level' on which all the other facts supervene. In connection with this idea, it is very important to note that according to one rather influential line of thought in philosophy of science – drawing on modern physics, in particular – there is no objective, wholly mind-independent causation to be found in fundamental physics *at all*! (See, for example, Corry and Price 2007.) This causal anti-fundamentalism is a subject of ongoing debate, of course, but given that it is not at all clear that fundamental physics and its properties are naturally understood in causal terms, one ought to wonder where this possibility leaves the mental causation debate thus presented, and 'Alexander's Dictum' to boot! It is quite possible that the reality criterion for (qualitative) properties at the fundamental level is not best construed in causal terms at all. (We do not want to suggest that only metaphysicians and philosophers of mind outside contemporary philosophy of science have ignored such potential 'complications' arising from actual physics. Within philosophy of science itself, there are many programmes of research that similarly turn a blind eye to these matters. The recent revival of neo-Aristotelianism in 'metaphysics of science' is a case in point: there has been very little discussion so far of how a metaphysics based on fundamental *causal powers* ties in with the actual physics and the role of symmetries and conservation laws, for example, in identifying the fundamental properties in the world. From the point of view of philosophy of physics, dispositional essentialism is very much 'armchair' philosophy of science in its spirit.)

Here's another example of a notion central to analytic philosophy: the concept of 'concept' itself has recently attracted a similar kind of science-influenced attention. Wilson (2006) provides a book-length argument, drawing heavily on engineering and applied mathematics in particular, against (what he calls) the 'classical picture of concepts'. Even if Wilson's unorthodox 'patchwork' picture doesn't get purchase among philosophers of mind and language, the latter are likely to benefit from this extremely rich and detailed discussion explicitly inspired by subtle workings of concepts in actual science. A very different perspective on concepts is offered by Machery's approach (2009), which derives its eliminativism about concepts from psychology research and its historical development. Again, even if Machery's own position fails to convert philosophers who approach concepts from a more *a priori* perspective, his work is significant in uncovering important complexity and detail in the rapidly evolving scientific disciplines that ultimately must inform any philosophical analysis of 'concept'.



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Machery's work draws on psychology, including neuropsychology. The rapid development of neuroscience has given rise to philosophy of neuroscience, a relatively new field of philosophy of science. Many classic issues in the philosophy of mind get discussed in a different light in philosophy of neuroscience. Take the debates around the notion of 'multiple realizability' of psychological states, for example. While the original arguments for multiple realizability were based on thought experiments and conceivability claims, the current debate relies much more heavily on the actual neuroscientific findings and the methodological nature of the discipline itself. (See, for example, Wilson and Craver 2006.) And it is not only philosophy of neuroscience that has contributed to the debate on multiple realizability and related issues, such as the possibility of 'higher-level' causation and explanation: it is becoming increasingly clear that closely connected conceptual questions (regarding reduction, say) crop up already in physics, and that a physics-based perspective may throw significant light on the debates in the context of philosophy of mind. (See, for example, Batterman 2002, Wilson 2010.) There is considerable scope here for fruitful interaction between philosophers operating within different sub-disciplines, and there are signs that inter-sub-disciplinary research along these lines is an emerging new direction.[1]

Philosophy of mind stands to the philosophy of neuroscience much like (general) metaphysics stands to the philosophy of physics: in as far as (general) metaphysics concerns the fundamental nature of reality, fundamental physics can (surely) direct and throw light on it. Callender, towards the end of his essay, lays down hints for a 'scientific metaphysics' that represents another new direction for the philosophy of science through developing fruitful interaction with a thriving area of philosophy. Already there are signposts indicating the way to go. However, there are practical issues: how should metaphysicians be encouraged to join in partnership with philosophers of science? There have been any number of collaborative workshops and conferences, of course, and even large-scale funded projects in the metaphysics of science, but should this relationship be approached in global terms, looking at the methodologies adopted in the philosophy of science and metaphysics respectively, or via the frameworks they adopt, or should it be approached in piecemeal fashion, on the basis of particular issues to which both sides can contribute without fear of losing disciplinary identity?

Certainly when it comes to methodology, some metaphysicians themselves appear to be modelling their support of particular metaphysical views on the stances they assume scientists to be adopting with regard to their theories (see, for example, Sider 2009). Competing metaphysical claims are treated as if they were alternative hypotheses about the world, and are assessed by what are taken to be the criteria for theory choice we find in science: simplicity, unificatory power and depth of integration across domains. Close attention to the philosophy of science will reveal just how complex and multivaried those stances and these criteria actually are (and, of course, unlike the case of scientific hypotheses, empirical success plays little, if any, role when it comes to metaphysics). But perhaps Callender's aim of a scientific metaphysics is best achieved through piecemeal work that is focused on particular problems and issues. Hawley, for example, distinguishes between optimistic and pessimistic views of the relationship and argues that the choice as to which to adopt can only be made on the basis of a case by case examination of potential contributions of science to metaphysics (Hawley 2006).

Not surprisingly, perhaps, a number of metaphysicians wish to remain aloof from such collaborative endeavours, insisting, as Callender indicates, that the scope of metaphysics extends far beyond the limits of science and, hence, the philosophy of science. Indeed, they may insist that tying metaphysics too closely to science will constrain the field too tightly, shackling its fertility. The philosopher of science can still treat this kind of 'science-free' metaphysics as a rich source of plunder, drawing on both the concepts developed and the moves made in that development. One might draw an analogy here with the relationship between mathematics and science: the former generates considerable structure that at a particular state of scientific development may look surplus to requirements. However, as science progresses, this surplus mathematical structure may turn out to be heuristically fruitful, leading to new theoretical developments. The classic example of this is Dirac's famous equation for spin ½ particles like the electron: the solutions of this equation include some that appear to refer to particles with negative energy and thus could be regarded as non-physical, surplus mathematical structure. However, they came to be reinterpreted in terms of a new particle, subsequently identified as the positron (a form of antimatter), illustrating just how useful a resource this surplus structure can be. Now, metaphysics is not mathematics, of course, but still, even the far reaches of the field may contain elements that the philosopher of science can put to use: think of the metaphysical notion of 'grounding', for example, which may help explicate the relationship between different levels of ontology, or different fields, such as physics and chemistry (as touched on in Hendry's essay); or take the idea of property 'fusion' (Paul, forthcoming) and think how it might be applied as a metaphysics of entangled states in quantum physics.

So, even if metaphysicians resist the call to attune their subject more acutely to scientific concerns, there are still opportunities for the philosopher of science to explore.

Turning from metaphysics to epistemology, it may be that epistemological issues in philosophy of science often appear to be just a subset of the various topics that fall under general epistemology. Philosophers of science are typically more concerned with beliefs that are 'science mediated' – gained through scientific theories, instruments or a combination thereof – and less concerned with scepticism regarding more mundane forms of belief. But as Bird indicates in his essay, there is much in general epistemology that can guide these more specific epistemological debates in the philosophy of science: debates regarding general epistemological frameworks can have significant repercussions at the level of more specific arguments advanced in philosophy of science. The debate between internalism versus externalism with respect to justification and knowledge, for example, can rule out (or otherwise) certain argumentative strategies in the scientific realism debate. Thus Psillos, in his classic work on this debate, explicitly adopts an externalist perspective and uses it to analyse the relationship between the 'no miracles argument' and 'inference to the best explanation' (Psillos 1999), for instance.

More generally and, perhaps, problematically, conceptions of knowledge as 'situated' in a particular social context bring social factors into our epistemological framework and from there, into the philosophy of science. How to handle such factors alongside such familiar 'epistemic values' has been a problem at least since Kuhn's famous book was taken to sound the death knell of logical positivism (Kuhn 1962). Giere (1988) represents one attempt to bring epistemic and social factors together within a single framework, but many philosophers of science continue to insist that the social has no bearing on matters of justification, truth, empirical success and the like. One group who maintain the significance of at least one such factor are feminist philosophers of science who argue that gender, in particular, exerts a significant influence on not only the choice of problems and issues to be tackled by scientists, but also on the selection of relevant evidence, for example. Gender bias has been taken to have a major impact on scientific objectivity in particular, but although examples from primatology and early hominid evolution appear to carry some force, it is difficult to see how similar biases might be prevalent in physics, say. Early work in this area tended to focus on the deficiencies of positivist views of science, but recent trends have moved beyond this to embrace stances such as 'standpoint theory' and pluralistic perspectives. (For a discussion of gender and epistemology with useful sections on feminist philosophy of science, see Anderson 2010.) It is with regard to the inclusion of social factors in general that work in the history of science has tended to diverge from that in the philosophy of science, although recently, concerted efforts have been made to bring the two back together in a more productive engagement, as we shall see below.

### 2.2 Philosophy of Science versus History of Science

As Howard indicates, the relationship between the history of science and philosophy of science is entering a new productive phase, in which the old prejudices and distrust have been overthrown, or at least set to one side.

Friedman's neo-Kantian approach, which Howard considers in detail, offers one framework in which this relationship can be reconfigured. Chang's 'complementary' approach offers another (Chang 2004). Here the history of science and philosophy of science are brought together to not only present a complementary understanding of science, but also to contribute to scientific knowledge itself. The history and philosophy of science, conceived of as a unitary enterprise, does this 'by serving as the cold-case squad of science', (Sumner 2005, p. 411), unearthing those features of science that have been lost or papered over by the respective field's need to focus on certain problems, and generally subjecting both the foundations and framework to critical evaluation. Chang offers a reconciliation of Kuhn and Popper: to progress, science must become 'normal' in Kuhn's sense, but then it becomes less open and critical. Complementary history and philosophy of science then takes on the critical role insisted upon by Popper, and by illuminating anomalies and reanimating overlooked ideas, it can contribute to science itself. By doing so, history and philosophy of science engages with science in a way that is neither merely descriptive, nor crudely prescriptive. In this respect, Chang's proposal chimes with Callender's in seeing philosophy, and philosophy of science in particular, as broadly continuous with science, and thus capable of contributing to it as well as drawing upon it. (We shall return to this issue in the next section.)

In the reoriented relationship that Chang envisages, the history of science becomes a source of forgotten questions and overlooked facts that, once dusted off and appropriately scrutinized, become fresh and scientifically significant. It is the philosophy of science that provides that critical scrutiny, reaching back into history to not only reassess the treasures that are unearthed, but also to initiate and guide historical investigations. As Chang emphasizes, this leads to a complex intertwining of the history and philosophy of science (2004, p. 240) that requires the philosopher of science to become much more historically engaged and the historian of science to become much more philosophically aware.

Both sides may balk at this bringing together, however. The historian of science may insist that there is more to her field than a kind of interdisciplinary conceptual archaeology. In particular, she may complain that such a conception leaves out the social aspects of science, with which the history of science has been preoccupied for 40 years or more (see Sumner op. cit.). Indeed, it is precisely this preoccupation that many see as having created the barrier to a more productive relationship with the philosophy of science. One option is to situate this approach firmly within social history, thereby introducing problematic demarcations within the history of science itself. Another is to seek ways in which these social aspects can be introduced into the relationship with the philosophy of science, as touched on above. Kitcher, for example, accepts that theories develop over time, with contributions from numerous researchers, thus allowing some space for social factors to be considered (1993). More recently, he has argued that science should not be characterized as seeking objective truth; rather, this is an ideal and, in practice, what scientists pursue are significant truths, where significance reflects social interests and perspectives (2003). Giere, likewise, has attempted to accommodate the social context of science, as we have noted, by introducing a perspectival view, but one that remains objective (see also Giere 2006). Such approaches have obvious consequences for scientific realism, particularly of the form advocated by Psillos: if the kinds and classifications put forward by science are perspectival and interest relative, then how can a realist stance be maintained towards them?

Such approaches seek to reconcile the philosophy of science and socially oriented history of science by reducing or reconfiguring the realist commitments of the former and picking out those features of the latter that could plausibly be said to contribute to scientific objectivity. Whether this is a viable way to address the issue, or whether we should accept that such reconciliation is simply not possible, represents a further set of issues for the philosophy of science to address.

In addition to unearthing forgotten facts and ignored anomalies, the complementary approach encourages the historian and philosopher of science to consider the roads not taken, to explore alternative theoretical possibilities that remained undeveloped or were not taken up by scientists at the time. Cushing represents an early example of such an explorer, in suggesting that the famous Copenhagen interpretation of quantum mechanics, associated with Bohr, would not have achieved the hegemony it did if Pauli's apparently devastating criticism of the alternative pilot wave interpretation advocated by de Broglie (and subsequently developed by Bohm) had been seen to be misconceived at the time (Cushing 1994). More recently, and in the biological domain, Radick has suggested that Weldon's unpublished work on non-Mendelian genetics of the early twentieth century offers a plausible alternative to current conceptions in evolutionary biology (Radick 2005). Underpinning such proposals is a view of science as significantly contingent and open, and both historians and philosophers of science may reduce the scope for such alternative histories by cataloguing the heuristic moves made in scientific discovery. Furthermore, in judging such counterfactual claims, epistemic considerations must intrude. Typically, mere conceivability is appealed to as a guide to possibility, but this is too indiscriminate to help judge whether a proposed alternative road in the history of science counts as a 'genuine' possibility (see the collection of essays in Radick 2008). Here, general considerations of the epistemology of modality may help (see Vaidya 2007), as there may be useful tools and devices that can be imported, and certainly the philosophical evaluation of counterfactual history represents another new set of issues for the philosopher of science to consider.

Some of these issues are closely related to the debate about the force of historical evidence against scientific realism. Stanford (2006) argues that the history of science indicates that scientists are recurrently unable to conceive of radically different theoretical alternatives that *would be* at least equally well confirmed by the available data. This is the latest advancement of the famous anti-realist line of thought – the so-called pessimistic induction – going back to Putnam (1975) and Laudan (1981). Stanford's 'new' pessimistic induction has faced criticism from the realist quarters for its construal of contemporary realist positions, for example,[2] but the underlying historical scholarship is to be applauded. Stanford is exemplary in extending the examination of history in this context beyond its usual confines of physical sciences to the life sciences. (For another recent example in this direction, see Turner 2007.) But despite this profitable new trend, it remains the case that this dimension of the scientific realism debate is still being conducted on the basis of a very limited number of historical case studies, and more historical data should be brought to bear on these epistemological issues. We shall return to the scientific realism debate below.

### 2.3 Philosophy of Science versus Science

The relationship between science and metaphysics is not all one-way, as Callender notes in his contribution, with the latter acting as a descriptive hand-maiden to the former. Arguably, metaphysically informed philosophy of science can make a genuine contribution to science itself. Of course, the interesting question is: how receptive is science to such engagement? Feynmann famously declared that 'Philosophy of science is about as useful to scientists as ornithology is to birds.' Another Nobel Prize winning physicist, Steven Weinberg, is well-known for his dismissal of the philosophy of science, detailing the way particular forms, such as positivism, have hindered scientific progress and suggesting that the only value of philosophers of science in general is to protect science from the views of other philosophers (Weinberg 1994). Not all scientists are so negative, however, and as a counterweight to Weinberg, one can set Mayr, as an eminent scientist who is both receptive to and reflective upon the philosophy of science (Mayr 1997). That Mayr is an evolutionary biologist is significant, and here, as Brigandt notes in his essay, there is fruitful interaction between philosophers and biologists. (See the volume co-authored by McShea (a biologist) and Rosenberg (a philosopher) for one such example in practice: McShea and Rosenberg 2008).

As another example of potentially fruitful interaction, from physics, Callender mentions the theory of quantum gravity, which seeks to unify quantum physics and our best current theory of space-time, general relativity. Already there is evidence of scientists and philosophers of science coming together to share ideas and explore new ways forward in this area (see Rickles et al. 2006; Callender and Huggett 2001). Another example is that of string theory, where philosophers can help bring clarity to the foundations of this emerging programme. Of course, one issue that has to be faced concerns the lack of empirical evidence for such theories, given the extremely high energy regimes that are involved. In such cases, other criteria may be called upon in evaluating such theories, including unificatory power, for example (see Cartwright and Frigg 2007). Such virtues may still render a research programme 'progressive' in the absence of direct empirical support, as suggested by Lakatos in his 'Methodology of Scientific Research Programmes' (Lakatos 1970). Even in these, perhaps extreme, cases, there is useful work for the philosopher of science to do.

Yet another example of this ilk – suggested by Huggett in his contribution – concerns the foundations of quantum field theory (and indeed, Huggett himself has made significant contributions here; see Huggett 2000 for a review). As long ago as 1983, Redhead called on philosophers to address the conceptual issues in these foundations (Redhead 1983). Early hopes of productive interaction between physicists and philosophers in this area have been only partly realized (see the proceedings of the *1996 Boston University Conference on the Conceptual Foundations of Quantum Field Theory*, which explicitly attempted to bring physicists and philosophers of science together; Cao 1999). However, recent work has given new impetus to these discussions. Fraser, for example, has advocated a formulation known as 'Algebraic Quantum Field Theory' as the proper focus of philosophical investigation of these foundations (Fraser 2009). This is a highly formal approach that emphasizes the algebraic relations holding between the observables of the theory (see Halvorson and Müger 2006). Wallace, on the other hand, argues that the 'naïve' form of quantum field theory that physicists actually use (and that forms the framework of the so-called Standard Model of elementary particle physics) is sufficiently coherent and conceptually robust as to withstand philosophical investigation (Wallace 2006). This obviously raises a fundamental issue in the philosophy of science in general: do we explore the science that is actually used in practice, or should we restrict our attention to logically 'clean' formulations? The latter avoids the conceptual infelicities and, in some cases, outright inconsistencies of the former, but at a cost of losing the interpretational richness that historically oriented philosophers of science, in particular, will be interested in (Vickers forthcoming).

Yet another exciting new direction in philosophy of physics, where there is considerable overlap with current physics, involves quantum information theory. Information theory in general abstracts away from the physical representations of information, yielding a potentially powerful tool from which to view a variety of issues. Quantum information theory takes as its fundamental unit the 'qubit', which, unlike its classical counterpart, can represent states that are in superpositions. This suggests potentially very useful applications to computation and cryptography: the information in such a superposition cannot be accessed without destroying the superposition, for example. The non-local nature of such superpositions also raises the possibility of a form of teleportation, in which information is transferred between systems without the need for a traditional kind of signal (see Jin 2010). As well as these exciting applications, quantum information theory offers the possibility of shedding new light on a range of issues in the foundations of quantum physics, even if one remains unsympathetic to the more radical claim that the world is made up, in some sense, of information. (For an exploration of these issues and a sceptical stance on this last claim, see Timpson 2008).

A final, but thought provoking, example of the potential for fruitful interdisciplinary interaction between philosophers, physicists and even historians of science concerns the Large Hadron Collider experiment at CERN, which has also excited attention among members of the general public. The aim of the experiment is to recreate conditions a fraction of a second after the Big Bang and perhaps discover the famous 'Higgs particle', responsible for conferring mass to some elementary particles, according to the Standard Model of elementary particle physics. A recently launched large-scale research collaboration ('Epistemology of the LHC'; www.lhc-epistemologie.uni-wuppertal.de/) aims at an epistemological analysis of this piece of 'Headline Science', focusing on matters such as the nature of the Higgs mechanism and the Higgs particle that the LHC has set out to detect; the interaction between a large experiment and a community of theoreticians; and the epistemological implications of an experiment of enormous complexity and the need for highly selective data gathering. Studying the epistemological dynamics of the experiment in real time allows for the possibility of creating a feedback loop to suggest modifications in the course of the LHC's operation. Such an active role is a novel and promising one for philosophy of science to play.

In order for philosophy of science to interact with science in this way, the philosopher obviously needs to master the relevant scientific discipline well enough to collaborate and engage with the scientists. Although this can be a tall order, it seems that, over the past decade or two, philosophy of science has become sufficiently 'local' and focused to allow the requisite depth to be reached. Many outstanding philosophers of science are today less concerned with general features of scientific practice, and more tightly focused on conceptual issues that are specific to particular disciplines. Examples of technically sophisticated, unadulterated philosophy of particular sciences abound: one can have a look at the *Handbook of Philosophy of Physics* (Butterfield and Earman 2007), and its entries on 'Algebraic Quantum Field Theory' and 'Symplectic Reduction in Classical Mechanics', for example. Cutting-edge philosophy of biology can also turn on sophisticated technical detail, as in the case of Okasha's book on evolution (2006), for example.

Increased focus and interdisciplinarity pays off in the philosophy of particular sciences, as it allows the philosopher to fully take into account the best scientific understanding in its full intricacy and complexity, as opposed to merely paying it lip service by 'popularizing' it to other philosophers. The philosophy of science inevitably becomes more specialized and fragmented in the process, and it is harder and harder for anyone to be a 'jack of all trades'. But if the historical development of science itself is anything to go by, this is surely a sign of progress. Due to the nature of philosophy, there is, nevertheless, an inevitable balancing act between deeply focused, local philosophy of science and general philosophy of science that offers broad descriptions and perhaps also normative lessons about science in general. Given the breadth and complexity of its subject matter, general philosophy of science often walks a tightrope between accounts that approach triviality in their abstractness and lack of substantial detail, on the one hand, and accounts that can be falsified by a case study drawn from somewhere in science, on the other. By 'going local', it is easier to provide substantial accounts in agreement with scientific practice, but without any overarching accounts the whole discipline risks becoming fragmented into philosophies of various particular sciences. How to resolve this tension remains as a further issue for the field to tackle.

## 3. General Philosophy of Science: Some Emerging Issues

### 3.1 Realism, Indispensability, Models

The issue of scientific realism in one form or another has been central to philosophy of science throughout its history. Nevertheless, there are many unanswered questions in the realism debate (in addition to the issue mentioned above regarding historical evidence).

One issue concerns the nature of positive arguments for realism. In the recent literature, one can find broadly two kinds of arguments. On the one hand, one can argue for scientific realism 'globally', as in the case of the famous 'miracles argument': the success of science, suitably construed, is taken to be an indicator of science latching onto reality with its theoretical assumptions, since this is arguably the best explanation of the success (see Psillos' essay in this volume). On the other hand, one can argue for realism more 'locally' with respect to some particular theoretical assumptions: given the nature of those assumptions, and what we know of the world through experiments, say, we are arguably justified in making those assumptions (on pain of thoroughgoing scepticism). In this way, one can argue locally for realism about atoms, say, or microbiological mechanisms, without simultaneously advancing an argument for across-the-board realism including fundamental physics, for example (see, for example, Achinstein 2002, Kitcher 2001). One emerging set of issues concerns the relationship between these two very different realist strategies. Are they complementary, or in competition? Which one provides the best way for the realist to defend their ground? What can one be a realist about on the basis of the local strategy?

Another outstanding issue concerns the precise sense in which the realist takes theories to be 'latching onto reality'. Everybody agrees that even our very best theories are not perfectly accurate representations of reality, and various kinds of approximations, idealizations and inconsistencies abound in successful theorizing. The literature contains numerous responses to this prominent issue, of course, largely under the heading of 'approximate truth' or 'verisimilitude'. Nothing like a consensus has formed, however, and different ways of construing the notion of 'scientific theory' call for different approaches to 'approximate' (or 'partial') truth (or some cognate notion). As Contessa explains in his contribution to this volume, one currently influential way of construing theories and the way they relate to reality is in terms of non-linguistic models that connect to the world via a non-linguistic representation relation. Different views regarding models and representation have evolved recently, and there's work to be done in spelling out what 'approximation' or 'partiality' amounts to with respect to these views. Towards the end of his essay, Contessa explores some questions in this neighbourhood.

One reason models and modelling practices have received a lot of attention in the recent realism debate is this: it has become evident that much of the success of science that so impresses the realist turns on making some specific modelling assumptions that are not directly derivable from some overarching general theory. It appears that impressive predictive or explanatory success is gained often from models or simulations that incorporate various kinds of misrepresentations and idealizations that are (on the face of it) *indispensable* for this success. Hence, the following question arises. What is 'responsible' for success in these cases: is success down to models latching onto reality in some relevant respects – as the realist would intuitively have it – or is it in some sense down to clever idealization schemes in a way that supports anti-realism? In order to fully answer this question, one needs have a clear view of what models are, how they represent the world, and how idealizations function in modelling. A number of different positions on these issues have emerged of late, and there's work to be done in evaluating and contrasting the alternatives. (The essays from both Contessa and Pincock discuss these and related issues.)

One issue here concerns the role of mathematics in modelling and idealizations, and this issue has recently forcefully emerged in connection with the 'indispensability argument' for mathematical Platonism (covered by Pincock towards the end of his essay). The debate around this argument – in its most recent guise – in a sense concerns the limits of scientific realism. The advocates of the indispensability argument argue that ('standard') scientific realism implies a form of Platonism regarding abstract models or mathematical entities, due to the indispensability of the latter in scientific theorizing and explanations. Those unsympathetic to the argument, on the other hand, wish to draw the line of realist commitment at concrete, clearly spatio-temporal things, and explain the indispensability of abstract notions in fictionalist terms, for example. (See Leng 2010 for a recent monograph.) One very closely related set of issues that has recently begun to receive significant attention has to do with the role of mathematics in scientific explanations. Here the advocates of the indispensability argument view mathematical abstracta, due to their explanatory virtues, on a par with hypothetical concrete entities (such as quarks, say) as 'explanatory posits'. Arguably, a 'global' scientific realist who adopts an inference-to-the-best-explanation strategy in defending realism shouldn't discriminate against mathematical posits in her realist commitments. This kind of confirmational holism is a source of an ongoing debate, but there's a clear lacuna here, as no theory of scientific explanation exists that would regiment these debates by providing an overarching framework for mathematical (and more generally, non-causal) explanations in science.

### 3.2 Causation, Laws, Symmetries, Structures

The importance of metaphysics also features prominently in Psillos' essay as he charts the swing to metaphysics-friendly forms of realism. Although he expresses concerns about the neo-Aristotelian nature of some of these developments, there is clearly considerable scope here for the philosopher of science to draw on recent work in metaphysics. Again, consider causation, for example, a concept that, as we have already noted, crops up again and again in these essays and is clearly highly significant for our understanding of science. Here there is no consensus as to its metaphysical status, with some commentators insisting there is nothing to causation over and above the instantiation of some regularity, while others adopt an analysis in terms of certain counterfactuals, and still others argue that it is to be understood in terms of the necessary connections that arise from the powers and capacities exhibited by the properties possessed by the fundamental objects of our ontology (see Beebee et al. 2009). Psillos himself favours a pluralist approach, according to which there is an array of causal concepts specific to particular situations and, although these may bear a family resemblance to one another, there is no single umbrella account capable of capturing them all (Psillos 2009; for a survey, see Godfrey-Smith 2009). Admitting such an array may help resolve some of the disputes that arise with regard to both the existence of causation within various domains or at various levels and how best we should understand it (see Hall, this volume).



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The analysis of causation is often taken to be related to that of scientific laws. Here, too, one finds a much discussed division of views, between those who insist that law statements are nothing but condensed descriptions of regularities, to those who argue that they encapsulate necessary connections in the world (see Carroll 2006). Recently, new life has been injected into the debate by a focus on the governing role laws are supposed to play. How one understands this role is problematic, and it has been argued that laws should be removed from our ontological picture entirely, with the behaviour of physical objects accounted for in terms of the dispositional 'powers' they possess (Mumford 2004). In support of this radical view, it has been pointed out that the philosophers' simplistic concept of 'law' covers a varied array of principles, equations, rules and so forth in science. Clearly, in responding to such a position and clarifying the nature and role of laws in general, the philosopher of science needs to both pay close attention to scientific practice in order to develop an appropriate classification (see Chakravartty 2007) and then bring aspects of metaphysics to bear on the form of governance involved (see also Roberts 2009).

Laws are just one feature of theories, of course, and in modern physics in particular, symmetry principles have also played a prominent role (Brading and Castellani 2003). Indeed, van Fraassen has argued for the epistemological significance of such principles on this basis (1989). Symmetries not only play important heuristic and classificatory roles, they have also been used to generate novel predictions. Perhaps the most famous example is from elementary particle physics, where the $\Omega$- particle was predicted on the basis of such considerations of symmetry, and was subsequently discovered (Bangu 2008). Yet most philosophical analyses of laws are either dismissive of symmetry principles or fail to mention them altogether. Such a glaring omission calls for the philosopher of science to step in and develop an account that embraces laws and symmetries in a way that is consonant with the practices of physicists themselves.

Symmetries lie at the heart of certain forms of structural realism, a position that was developed as a response to the claim that a realist stance is unsustainable, since the history of science reveals a series of changing theoretical ontologies. By focusing on the laws, equations and, more generally, structures that are retained through these changes, it is claimed, a viable form of realism can be constructed (Worrall 1989). 'Ontic' structural realism adds to this a close analysis of certain aspects of the foundations of physics that expands the notion of structure to embrace symmetries and conservation laws and further shifts the underlying metaphysics away from objects (Ladyman 1998; French and Ladyman 2011). Indeed, it is through such symmetries that physical particles and their kinds are classified, and the structural realist takes this to reflect how the world is, with the particles themselves conceptualized not as objects per se, but as 'nodes' in a structure in some sense. Here metaphysics may be turned to again to defuse the explosive mix that Psillos sees in this view and explicate a notion of structure that can accommodate the kind of causality Psillos regards as crucial for our understanding of science. Alternatively, one could revive Russell's position and argue that causal notions have no place in physics, a stance that is attracting increasing attention again (Ladyman and Ross 2007; Norton 2007; Corry and Price 2007). And, again, a broadly pluralist approach may be appropriate here.

Laws and symmetries play a significantly smaller role in chemistry and biology. Again, metaphysics can be pressed into service in helping us get a grip on the kinds and substances of the former. And as in the case of laws, we need to pay attention to the science itself if we are to understand what is meant by a chemical bond, for instance, a concept that – as Hendry explains – plays a key explanatory role in this field. Here, structural realism may have something to contribute in urging us to stop thinking of electrons as tiny objects that 'compose' a bond, and in bringing to the fore the underlying symmetry that allows us to understand their behaviour in quantum mechanical terms.

Whether structural realism can be exported into the foundations of biology is a contentious issue. Nevertheless, concern about the nature of biological objects has arisen here too, with Dupré, for example, arguing that there exists a 'General Problem of Biological Individuality' when it comes to the issue of how one divides 'massively integrated and interconnected' systems into discrete components (Dupré and O'Malley 2007). He and his co-workers have urged a shift in philosophical focus away from individual genomes and distinct organismal lineages to the collaborative interactions between communities of entities from many different reproductive lineages, the very nature of which breaks down the object-oriented boundaries between such entities. In his essay for this volume, Brigandt maps out the diverse accounts of biological 'kinds' and advocates a version of the 'homeostatic cluster theory' that emphasizes the role played by relations in individuating kinds. This is consonant with a structuralist approach and the kind of contextualism that Brigandt discusses relates nicely to forms of 'contextual identity' in physics advocated by Ladyman, for example (Ladyman 2007). Granted the distinct differences between these two fields, perhaps such approaches offer the possibility of useful bridge-building being undertaken.

In biology, famously, one finds a dearth of laws, but a plethora of models (see Odenbaugh 2008). Bringing order to this diverse array requires the adoption of some meta-level framework, and the apparent lack of the kinds of laws one finds in physics, for example, has led many philosophers of biology to embrace the so-called model-theoretic approach to theories, particularly those versions that downplay the role of laws in general (see, for example, Giere 1999). Thus, to give just one example, one may find mathematical structures being devised which are then claimed to be similar to the spatial dynamics of butterfly metapopulations in California (Odenbaugh op. cit.). Such examples naturally invite application of the model-theoretic account, but of course they do not constitute the entirety of model-building in biology. More so than in physics, say, biologists deploy physical models, as exemplified by Crick and Watson's famous wire-and-tin model of the DNA helix. But more radically, they also use so-called model organisms such as fruit flies, flour beetles and so on. Can these be brought within the scope of the model-theoretic account? Some have suggested that this diversity of models should be accommodated by a sort of 'tool-box' philosophy of science, in which models are regarded as tools to be brought out and applied when needed (Cartwright et al. 1995). Others have responded by modifying the model-theoretic approach to capture this diversity, emphasizing the representational capacity that all such models have in common (da Costa and French 2003).

### 3.3 Theories and Things: Drawing on Art

The material nature of biological models also raises interesting issues when it comes to the ontology of the core elements that philosophers of science deal with, namely theories and models themselves. These are ascribed certain properties such as 'being empirically successful'. Correspondingly, they are taken to be related to 'the data' as well as each other, and they are identified in various ways from current scientific practice as well as the history of science, and so on. But what *kinds* of entities are these? A straightforward answer is to say that insofar as theories are just sets of propositions, related via logical deduction, they are whatever propositions are. Some adherents of the model-theoretic approach have insisted that, on the contrary, theories are nothing more than set-theoretical models, and one well-known position maintains that theories and models are abstract entities (Giere 1988). Others have argued that they have a hybrid status, embracing both linguistic and model-theoretic elements (Hendry and Psillos 2007). A further suggestion is that these approaches should be seen as just representational tools for philosophers of science and that theories and models should not be identified with either propositions, sets or abstract entities (da Costa and French 2003). This leaves their ontological status open to debate (see Contessa 2010).

Given this diversity of views, how might we pin down an answer to our question above? It is here that interesting comparisons can be drawn with related themes in the philosophy of art. Consider musical works for example. These should not be identified with a particular example of the score, nor with an individual performance. But if they are regarded as abstract entities, how can we understand their creation? Likewise, it seems implausible to identify a given scientific theory with the paper in which it is first presented, or with its textbook representation and even more so to identify it with the scientist's presentation of it at a conference (although this might well be regarded as a kind of performance!). But if theories are taken to be abstract entities, how are we to understand their discovery? Are we to imagine scientists stumbling across them somehow? Such a picture hardly fits with what we know of scientific discovery, where specific heuristic moves can be identified. Some philosophers of art have suggested that abstract artworks can be created, since they depend on acts of human intention for both their creation and continued existence (Thomasson 2006). Could such a view be extended to theories? Again, the heuristics behind scientific discovery suggest that there is more than merely human intention involved, and it seems odd to say that quantum mechanics or the theory of evolution depend for their continued existence on human intention. Nevertheless, there is clearly scope for importing a range of moves and views from the philosophy of art into the philosophy of science.

This has already been exemplified in the case of representation, where examples from the world of art, and painting in particular, have been deployed to counter certain claims about theoretical representation, not always appositely (Suárez 2010). A more nuanced reflection on the similarities and differences can be found in van Fraassen's recent work (2008), where connections are drawn between perspective in art and frames of reference in science, for example, and more generally between the kinds of distortions one finds in science, such as occlusion, and abstraction and idealization in science. In his essay for this volume, Contessa has delineated some of the issues involved in this recent shift to representation, and again, there is considerable further work to be done. Standing somewhere between musical works and paintings, theories and models may act as a kind of bridge between the philosophy of science and the philosophy of art, allowing for the fruitful exchange of views and approaches between both domains.

## 4. Conclusion

As we said at the beginning, this is a highly personal set of 'projections' of what we see as some of the emerging issues in the field. It is quite possible that in ten years' time, the next generation of philosophers of science (perhaps even some of the readers of this book), will read this and shake their heads in bewilderment or pity! However, we hope we have conveyed some sense of the excitement we feel over these new developments, and we hope to have indicated some of the areas in which productive new research can be undertaken. As the field develops, new problems and issues will arise, and new areas will open up. In part, this will be driven by new developments in science itself, which philosophers of the special sciences can be expected to respond to, and partly through the new relations that are emerging with other fields within philosophy as well as with the history of science and science studies in general. There are plenty of opportunities here and a lot of good work still to be done – far from degenerating, the philosophy of science has progressed enormously over the past 30 years or so, and we expect further progress in the future. And hopefully some of you now reading this collection will contribute to that progress.

## Notes

1 In this volume, Walter and Eronen discuss reduction and multiple realizability. Craver and Kaplan look at the notion of scientific explanation (and related issues from general philosophy of science) in the context of neuroscience.
2 See Psillos' essay in this volume for further discussion.

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