Immanent Philosophy of X

Robin Findlay Hendry
Department of Philosophy
Durham University
50 Old Elvet
Durham DH1 3HN UK
r.f.hendry@durham.ac.uk

Abstract

In this paper I examine the relationship between historians, philosophers and sociologists of science, and indeed scientists themselves. I argue that (i) they co-habit a shared intellectual territory: science and its past; and (ii) they should be able to do so peacefully, and with mutual respect, even if they disagree radically about how to describe the methods and results of science. I then go on to explore some of the challenges to mutually respectful cohabitation between history, philosophy and sociology of science. I conclude by identifying a familiar kind of project in the philosophy of science which seeks to explore the worldview of a particular scientific discipline, and argue that it too has a right to explore the shared territory even though historians and sociologists may find it methodologically suspect.

1. Science as shared territory

Philosophers used to attempt normative definitions of science in terms of its methodology, Popper (1959) and Lakatos (1971) being two of the best known examples. This was not just a philosopher's game: Kuhn (as described by Feyerabend (1970) and Merton (1973) tried the same thing. Historians and sociologists now think of themselves as having moved away from that kind of project. Among the philosophers, Feyerabend argued that no normative methodological definition of science can capture all of science: for every non-trivial methodological rule—one which is not so abstract that it fails to have genuine normative bite—there has been some scientist somewhere who has rightly violated it, in the sense that they achieved more progress in breaching the rule (Feyerabend 1988, Chapter 1).[1] Let us suppose, for the sake of argument, that Feyerabend was right about normative methodological definitions of science: accepting this needn't imply that we don't know what science is, or that its identification is a matter of arbitrary convention.

It is a familiar idea that science is a particular real-worldly phenomenon. Why not start there? The Departments of Biological Sciences, Chemistry and Physics at Durham University are parts of science, as are departments in other universities that have similar names or overlapping interests. They are the living products of distinct though connected intellectual, experimental and institutional traditions. Their people were trained by other people along with whom (and the ongoing traditions in which they work), they make up the living disciplines of biology, chemistry and physics. Together they are at least a part of science.[2] Whatever else it is, science can be regarded as a particular historical entity, though a composite one. It doesn't matter that, because we have no definition of science, we can't say when it came into being or what its boundaries are. Nor does it matter that, because we have

[1] Alexander Bird (2010) and Paul Hoyningen-Huene (2013) have recently advanced very general conceptions of scientific method, but each, I think, is abstract enough to preclude it from constituting a straightforward counterexample to Feyerabend's claim.
[2] A *part* of, because there are other scientific disciplines than biology, chemistry and physics.

no definition of science, there are controversial cases: epistemic activities that bear important similarities to the exemplars of science, but also important dissimilarities. Both of these things are true of life: there is no uncontroversial definition of what it is, and there are difficult cases like viruses and ant colonies. None of this means that biology fails to have a shared subject matter that its practitioners may approach in radically different ways.

An approach to science is *immanent* if it is an attempt to understand science as a phenomenon in the actual world, past or present: one's initial interest might be in its epistemic success (however one construes that), or in its social standing (if that is a different thing from epistemic success). Or one may simply be interested in a particular science because one is immersed in it, wanting to know what view(s) of the world it embodies, how it has got to where it is. Immanence in this sense does not preclude an interest in, or an ability to think about, the ways in which science (or particular parts of it, including theories, results, experimental methods or even whole disciplines) might have been different. Indeed an interest in the contingency of science *presumes* that we can make sense of science having been different.[3] Historians and sociologists warn us not to see various theoretical conclusions (e.g. that combustion is oxygen-gain rather than phlogiston-loss) as inevitable developments in science. Science might have developed with different interests, presumptions or contributors and so could surely have been quite different. Acknowledging this contingency presupposes that science is a transworld phenomenon, a something that might have been different in alternative histories. Historians, philosophers and sociologists may be interested in different sets of ways non-actual science might have been, and may disagree about what should be kept fixed. As long as the basic approach to science is an immanent one, grounded in actual science and its past, there will be some overlap in their subject matter, understood as the kind of this-world activity which is relevant to grounding general judgements about science (or particular sciences). This is the territory we share.

Epistemology of science need not be pursued immanently: one might identify some methodological rule as a precondition for any activity properly to be called 'science.' In *The Logic of Scientific Discovery* Popper rejects a naturalistic approach to scientific method (1959, 50-3), and identifies the avoidance of 'conventionalist stratagems' as a prerequisite for genuinely empirical science (1959, 80-4). This looks transcendental. Elsewhere he seems to take a more immanent approach: impressed by purported examples of science whether good (Einstein) or bad (Adler, Freud and Marxism), he (boldly) identifies bold conjecture as the hallmark of science (1963, Chapter 1). Yet in telling respects, Popperian philosophy of science is transcendental: both Popper and Watkins admitted that normal science as described by Kuhn is a genuine feature of science as we find it, but felt it to be an unnecessary and pernicious one (see Popper 1970, Watkins 1970). Thus the normative epistemology (no conventionalist stratagems) trumps the (this-worldly) phenomenon of science. There are *no* empirical conditions under which falsificationism itself should be given up. In contrast to transcendental falsificationism, an immanent approach to science, or to its individual disciplines, will take science and its actual history as a starting point and source of evidence, because only the *actual* world is available for inspection.

If science is a particular historical entity with a continuous existence over time, then we can talk about it without a definition of what counts as science, and also talk about it somewhat

---

[3] Daniel Nolan (2013) identifies eight good reasons why historians (and others) should be interested in counterfactuals, some of which are independent of whether their truth values can be known. One crucial such reason (2013, 329) is their use in causal reasoning where quantitative models, which are typically deployed for this purpose in the natural sciences, are unavailable.

independently of how we study it. So in some sense, philosophers, historians and sociologists of science can all be said to study the same thing—science and its past—even though they study it in different ways, and tend to disagree quite radically on how to describe its methods and products. We can say all that without presupposing that there are any interesting (non-trivial) metascientific generalisations that hold across biology, chemistry and physics.

2. We can get along

If science is shared territory, ranged over by historians, philosophers and sociologists, the question is whether we can all do so peacefully, and with mutual respect. Or are we condemned to conflict, or even mutual incomprehension, like scientists from rival Kuhnian paradigms? In this section I argue that all of us who are interested in science *should* be able to get along. It's a two-level argument: firstly, empirical uncertainty of any kind should allow for what I call basic collegiality, which I take to be a prerequisite for mutually respectful cohabitation; secondly, whoever thinks that this (very weak level of) empirical uncertainty is a feature of science itself, ought also to see it as a feature of the study of science. Hence we students of science should approach each other with basic collegiality. Conflict (like incommensurability) is a choice, not an inevitable consequence of disagreement or difference.

Let us start from empirical uncertainty. Quine contrasted the 'meagre input' of experience with the 'torrential output' of theory (in terms of content, at least), which, he thought, showed how 'one's theory of nature transcends any available evidence' (1969, 83). He also noticed the epistemic holism that gave rise to the problem with which he and Duhem are jointly credited: that in principle, more than one response is available to the empirical refutation of a given set of theoretical assumptions. Different responses are also available to *that* thought, however. (i) One might explore what resources for theory choice are available to scientists, as responsible thinkers, beyond those afforded by logic and experience. Call this 'augmentation' (of the criteria for theory choice). (ii) One might weaken one's theoretical commitments (call this 'weakening'). (iii) Or one might maintain one's theoretical commitments, but acknowledge that other commitments are rationally available. Call this 'basic collegiality'.[4] I will discuss them in turn, arguing that basic collegiality is compatible with the other two responses, and can be a component of any epistemic situation in which there is empirical uncertainty.

(i) Augmentation: Duhem (1954, 216-8) appealed to the 'good sense' of physicists, and hoped that in any particular situation the passage of time, and further information, would reveal whether radical amendment to fundamental theory or some lesser adjustment had been the right course. However, good sense was an ineliminable element of theory choice, and even of any retrospective judgement that science took the right path (see Duhem 1954, 218). For philosophers, the problem with Duhem's proposal is the ineffability of 'good sense', which comes down to a very vague and general kind of epistemic morality:

---

[4] These three kinds of response do not exclude meta-epistemological responses that reflect on the nature and scope of normative epistemological concepts, or their relationships to other normative concepts. One distinctive meta-epistemological response is epistemological relativism, which sees epistemic normativity as embedded within, and so bounded by, social structures, habits of thought or tradition. Blackburn (1998, Chapter 9) makes the analogous distinctions in the case of moral relativism.

> The sound experimental criticism of a hypothesis is subordinated to certain moral conditions; in order to estimate correctly the agreement of a physical theory with the facts, it is not enough to be a good mathematician and skillful experimenter; one must also be an impartial and faithful judge. (1954, 218)

Unfortunately he gives no binding principles to govern this judging, but others are more informative. Kuhn identified five theoretical virtues whose pursuit he regarded as constitutive of science: accuracy, consistency, broad scope, simplicity and fruitfulness (Kuhn 1977). Just as famously, he denied that such values could yield anything like an algorithm for deciding, on a given body of evidence, which of two alternative theories must be preferred over the other. Larry Laudan urged that the conclusions of Duhem, Quine and Kuhn are not catastrophic for the possibility of knowledge (Laudan 1990): even where there are two or more empirically equivalent theories, one might rationally prefer one over the other. If one understands rationality as permission, this is a weak conclusion: I may be entitled to prefer this theory, but that does not imply that it is obligatory for you to share my preference. Criteria for theory choice surely cannot render commitment to any given theory a rational requirement, or we would have squared many philosophical circles, by solving all manner of sceptical problems, including induction, other minds and the external world. Laudan's argument affords us rational entitlement to prefer one theory over another, but it cannot establish that those who do not share our preference are unreasonable. Basic collegiality is compatible with augmentation.

(ii) Weakening: Quine acknowledged the role of informal virtues of theories like simplicity and economy, although (somewhat sweepingly) he described them as 'where rational, pragmatic' (1953, 46). He also used logic itself to explore the possibility of weaker commitments that would avoid the need for choice between two empirically equivalent theories (1992, 95-101). One very basic move of this sort is to commit to a simple disjunction of empirically equivalent yet incompatible theories. Another, which was the core of van Fraassen's alternative to realism, is to commit only to some part of a theory. In neither case could it have been claimed that weakening one's commitment is a rational requirement, and for good reasons. Weaker commitments are not rationally required over stronger ones: opposing Clifford's evidentialist ethics of belief, William James (1948) pointed out that the pursuit of truth required more than just the avoidance of error. If there is no rational requirement to weaken one's commitments, one ought to accept that others may have good reasons not to. Basic collegiality is compatible with weakening.

(iii) Basic Collegiality: This is the bare acknowledgement that there is room for legitimate disagreement. There may be alternative theories to mine, adoption or pursuit of which are rational enough to be worthy of discussion. Those who pursue them need not be fools. Importantly, basic collegiality should not be thought to involve any weakening of commitment to one's own view. I have already noted that it is compatible with both augmentation and weakening; it is easy to see that it is compatible with any other response to empirical uncertainty that does not come labelled as a requirement of rationality.

Now consider that underdetermination arguments apply just as well to the study of science as they do to studies of nature within science itself. The natural sciences have more evidence to go on, for there are many more working scientists producing empirical evidence within science than there are working historians, philosophers and sociologists observing the workings *of* science. General claims about science are typically abstract and remote enough from concrete example to require the deployment of auxiliary hypotheses in testing them.

Unlike the natural sciences, there is little consensus on the basic causal mechanisms that underlie our field, and so little consensus on which general claims about science and which auxiliary hypotheses pass basic plausibility thresholds. Anyone who thinks that underdetermination arguments apply to scientific knowledge should therefore acknowledge that they apply with at least as much force to our knowledge of science itself. A few observations illustrate the point: not only is there plenty of disagreement among historians, philosophers and sociologists of science on matters metascientific, there is also plenty among scientists (see Shapin 2010, Chapter 3). On such matters it would be hubris for me to dismiss as foolish those who disagree with me. There should be room for legitimate disagreement about science. Mutually respectful cohabitation should be possible.

3. Live with me

The possibility of mutually respectful cohabitation does not, of course, mean that it always happens. Why does it break down? Potential sources of conflict among historians, philosophers and sociologists of science include their systematically differing tendencies to worry about present-centredness (the interpretation of the past in terms of the present) in their engagement with past scientists and other thinkers, and also their systematically differing tendencies to accept or reject various forms of epistemic relativism, and anti-realism about classification. These tendencies to interdisciplinary disagreement are aggravated on both sides by overblown rhetoric and what I will call 'sneaky transcendence', which is the tendency to regard the rules and results of one's own discipline as universal, and those who reject them foolish or ignorant. Mutually respectful cohabitation does not require that interdisciplinary disagreements must disappear. That would be improbable and undesirable, if one values diversity in academic life, and there is little reason to think that even deeply divergent methods and assumptions must rule out genuine communication and fruitful collaboration. All that is required is to keep in check the soaring rhetoric and the sneaky transcendence, and to approach our disagreements in the spirit of basic collegiality.

3.1 Present-centredness

Historians of science famously worry about the dangers of present-centredness and (even worse) 'Whig' history (Butterfield 1931; for context and analysis see Jardine 2003). Such concerns are motivated by history's conception of itself, but also, perhaps, by the history of science's particular disciplinary development. It started as an activity for superannuated scientists, whose interests grew out of, and were imbued with the values of, a life in science itself. Pursued in this way, history of science often set out to trace a cumulative path from pre-science to the modern day, identifying the earliest expressions of modern scientific ideas, and apportioning credit and blame for agreement or disagreement with modern science respectively. It assumed science to be an honourable activity driven by its own explicit values. From the mid twentieth century onwards, however, history of science became increasingly professionalised, developing a disciplinary infrastructure of learned societies, journals, book series, established chairs, named lectures and a secondary literature, knowledge and citing of which is required to join the disciplinary conversation.[5] Perhaps the history of science developed a particular horror of its own amateurish past, and its tendencies to present-centredness and Whig history.

[5] I owe some of these points to a (remembered) talk by David Knight, to Durham's Philosophy Department Seminar in the 1990s. Others I owe to Brush 1995.

Why should these methodological concerns be potential sources of conflict? If the ban on present-centredness is too restrictive, the danger is that certain philosophical positions, projects and debates simply cannot get started. Consider the following ideas: that inferential standards do not vary, that scientific theories are getting progressively closer to the truth, that science discovers natural kinds whose existence pre-exists it, and that intentional states such as beliefs essentially involve the material environment in which they are formed, so that their contents cannot fully be understood without reference to that environment. Whether or not one accepts these claims, philosophers are surely committed to regarding debate about them as open. A methodological requirement to suspend the worldview or inferential standards of modern philosophy or science would constitute a transcendental argument against one side, curtailing debate across epistemology and metaphysics, or at least any interaction between the debates in philosophy and relevant historical or sociological scholarship. In that case, philosophers might say, the methodological requirement to avoid presentism and Whig history has achieved too much.[6]

Most importantly, each of those ideas is a possible basis for engagement with the past, despite apparently being in conflict with the ban on presentism. Many philosophers will wish to approach scientists and philosophers of the past, no matter how distant, as people with whom we can disagree, who can make major mistakes that we may fruitfully point out, and who may fail to see alternative ways of thinking, ways that we may fruitfully point out. They may simply not know things that we know. But if we can disagree with these people, we must be able to think (enough of) the same thoughts as them, and make (enough of) the same inferences to the same (enough) standards. This assumes a fair amount of commensuration between their thought and ours. Aristotle, for instance, needs to be able to have thoughts about chemical combination if we are to compare his account of it to that of modern chemistry.[7] Lavoisier needs to be able to have thoughts about oxygen if he is to be able to claim (wrongly, we think) that oxygen confers acidity on its compounds with sulphur and nitrogen. The power to have thoughts about chemical combination does not depend on the modern concept of chemical combination; the power to have thoughts about oxygen need not wait for the twentieth century's discovery of nuclear charge.

Let me give a more detailed example: should chemistry be regarded as a science that (in some sense) discovered the elements as categories of stuff which (in some sense) pre-existed it? The major obstacle to thinking of chemistry in that way is the degree of change in the discipline's conception of the elements: the later conceptions depend on theoretical assumptions that were contested by earlier chemists (e.g. that elements are individuated by types of atom), or broadly empirical discoveries that were simply unavailable to them (e.g. that samples of the same element may differ in respect of their atomic weight). One way to address this is to start by asking which natural kinds were 'existentially given' (as Putnam used to put it) for scientists to name. For chemistry, this question becomes: what did Lavoisier have in his test tubes? This is in no way to interpret Lavoisier as an atomist. It is simply to give an account of what (referentially) he was talking *about* when he used the term 'oxygène'. I think this is useful and legitimate information in answering various philosophical questions, and it essentially involves the past.

[6] Historians differ on these matters: Brush (1995) defends the particular value a scientific training can bring to historiography, part of which is the ability to address presentist questions; Oreskes (2013) explains why she is a presentist. Philosophers differ among themselves too: see for instance Jonathan Bennett's (1992) review of Ayers' (1991) book on Locke. The disciplinary differences I note are only tendencies.

[7] See Needham 2006 and Bensaude-Vincent 2008.

It does, however, interpret the past in terms of the present: we imagine past scientists as working in a world as described by modern science. Now this is not to attribute to any past scientist a belief that the world is as described by modern science, nor to imagine their beliefs as being on a path set on convergence to ours. It is simply to see them as working in a physical environment.

3.2 Sneaky transcendence

The next barrier to mutually respectful cohabitation I would like to discuss is sneaky transcendence: taking claims whose acceptance is based upon norms or assumptions that are by no means self-evident, necessary or universal, and treating them as if they were. By calling it 'sneaky transcendence,' I do not wish to attribute any kind of dishonesty to those who are afflicted by it. Sneaky transcendence is a psychological phenomenon that historians, philosophers and sociologists have all noticed: a theoretical conclusion is accepted in a particular context, structured by a specific set of interests and assumptions, but becomes such a familiar and fixed point in thought and discussion that it comes to seem necessary or inevitable, and the local context of its acceptance comes to be forgotten. Why is sneaky transcendence a possible source of conflict between history, philosophy and sociology of science? Because it tempts me to assume that the methods, background assumptions and conclusions of my own discipline apply to others, and I criticise them for failing to observe rules that in fact do not apply to them.

In the history of philosophy, empiricists like John Stuart Mill offered sneaky transcendence as an error theory for judgements of necessity about merely contingent truths. Criticising what he calls 'absolutism', David Bloor notes that 'It is all too easy to mistake what is merely taken for granted for what is eternal and God given. Treating local conventions as absolutes is the oldest trick in the book.' (2007, 257) Durkheim, he notes, offers the 'classic analysis of this phenomenon.' But historians and sociologists of science are just as susceptible as philosophers, scientists and theologians. Here I briefly review three sneakily transcendent forms of argument that *p*.

'We've discovered that *p*': Two of the classic presentations of sociological programmes in the study of science cite large numbers of empirical case studies (see Barnes and Bloor 1982, 23-5; Shapin 1982, 162-4). What are these lists supposed to achieve? Shapin argues that they

> demonstrate that neither reality nor logic nor impersonal criteria of 'the experimental method' dictates the accounts that scientists produce or the judgements they make: they open the way to a sociology of knowledge, and for this reason they are invaluable. (Shapin 1982, 164)

Opponents of these sociological programmes sometimes say that invoking empirical evidence is self-defeating when one is committed to the underdetermination of theories by evidence. That is a crude and fallacious criticism, for the situation is more subtle: Barnes, Bloor and Shapin offer us a theoretical interpretation (a way the world could be) that, they think, optimises fit with the available empirical constraints, background assumptions and plausibility judgements. This is not self-defeating, but background assumptions and plausibility judgements differ across epistemic (and therefore disciplinary) boundaries. So Shapin's word 'demonstrate' must be interpreted carefully: neither reality nor logic nor impersonal criteria of 'the experimental method' dictate that adopting his theoretical interpretation of the historical and sociological evidence is a requirement of rationality. The demonstration is not a proof, but an exhibition of how things might be done.

'Acceptance that *p* is a methodological requirement': The historians' worries about presentism and Whig history, and the sociologists' adoption of epistemic relativism or symmetry principles share a common thought: one must suspend the commitments of one's own time or place if one hopes to understand the thought of people from other times and places. Barnes and Bloor are quite clear that they regard the symmetry principle as a methodological requirement:

> Far from being a threat to the scientific understanding of forms of knowledge, relativism is required by it. Our claim is that relativism is essential to all those disciplines such as anthropology, sociology the history of institutions and ideas, and even cognitive psychology, which account for the diversity of systems of knowledge, their distribution and the manner of their change. (1982, 21-22)

But why should there be just one scientific way to understand forms of knowledge? Might there not be other ways, deploying non-relativist ways of explaining belief? In another context Bloor asserts that

> The correct picture of concept application starts by grounding meaning in a finite number of exemplary instances. For this reason, the account is often called "finitism". (2007, 272)

It seems that, although the world was created without electrons or Coulomb's law (the existence of which had to wait for human scientific investigation), it is governed by immutable laws of concept application. Alternatively, one might see finitism as but one possible position in the metaphysics of thought, albeit a congenial one for sociological investigation.

'Widespread acceptance that *p* is the outcome of the historical development of our discipline': Suppose we all agree that scientific disciplines develop in contingent ways, and that historical change is mere change. The trouble is, developments in one's own discipline seem different. Steven Shapin's 2008 History of Science Society Distinguished Lecture (published as Shapin 2010, Chapter 1) gives an overview of changing approaches within the history of science. The overall impression is that (i) historians of science used to venerate science, assuming its epistemic and moral authority; but (ii) now they feel free to satirise and be sceptical about it; and (iii) on the whole that is a Good Thing, rather than a mere change (it is in fact a 'Noble Calling'). Nowhere, of course, does Shapin give anything approaching an argument of the form 'Historians of science used to think *q*; now they think *p*. Therefore, *p*.' But the risk is that, in the minds of the unwary, sneaky transcendence transforms the chronological presentation of his narrative into the march of progress.

It is sometimes said that academic disciplines are like conversations. Seeing them as conversations makes sense of some of their important features, which might otherwise seem to be in tension: their branching structure, the contingency of their development, and their responsiveness to reasons. Particular interpretations of empirical data, methodological rules, and changes of tone (e.g. from venerating science to problematising it) can all be thought of as developmental constraints. The point of this section is that you can't assume that you can take a development from one disciplinary conversation and drop it into another one: it might seem out of place. Even if symmetry and the rejection of presentism are developmental constraints on the history and sociology of science, philosophers are free to explore alternatives.

3.3 Rhetoric and polemic

As we have seen, historians, philosophers and sociologists exhibit noticeably differing tendencies to worry about present-centredness and Whig history, to accept or reject various kinds of symmetry principle, and to accept or reject the label of relativism. Such differing tendencies are probably non-accidental given the nature and history of the three disciplines, but in the end they are just disagreements, so need not necessarily lead to conflict. They often do, however, typically because one side or both approaches the disagreement with insufficient sensitivity, and inflames the situation with polemic: think of the 'science wars' of the 1980s and 90s, which generated so much more heat than light. Why is there so much polemic? Partly, no doubt, because it is such fun to write, but none of us has a right for our grumpy rants to be listened to, and maintaining good relationships means keeping such them in check.

Relativism-bashing is a well-known phenomenon among philosophers and scientists. In an article which is itself (as we shall see) a nice example of interdisciplinary polemic, Bloor (2007) observes that, when criticised, relativism is too often misrepresented and oversimplified. The main thrust of his complaint is borne out by many of the titles of the very well-known books he cites, among which are 'The Flight from Science and Reason', 'Higher Superstition' and 'A House Built on Sand.' Such polemics often violate philosophy's own values, according to which a critical examination of a position should accurately represent it. Imagine if discussions of *moral* relativism, which perhaps has more prominent defenders in philosophy, were conducted in this way. In the case of moral relativism, there at least seems to be some awareness of the different normative and meta-ethical dimensions to the position.

The defenders of relativism have their own rhetorical tricks and polemical misrepresentations, however. One of these is to bring all the different ways of *rejecting* relativism under the single inadequate heading 'absolutism': Bloor (2007, 251-2) is quite explicit about doing this, arguing that while the theological opponents of relativism (whether of semantic, epistemic or moral varieties) are clear-sighted in their adoption of absolutism, the philosophers suffer from false consciousness. This first rhetorical move brings so many others in its wake: those who reject relativism must dogmatically assert their own possession of the truth, which makes sense only of it is God-given. Words like 'truth', 'rationality' and 'method' are capitalised, so as to inflate the claims of the opponent of relativism (Shapin 2010, Chapter 1 provides multiple examples). The relativist adopts the stance of the cultural critic, the radical, the *enfant terrible* or the joker who Speaks Truth to Power. This stance is undermined, somewhat, by the fact that the authors of the classic programmatic texts of the sociology of science and socially-oriented history of science are now at the top of their fields. The project of problematising science is the orthodoxy, not dangerous radicalism (see Bloor 2007, Shapin 2010, Chapter 1).

4. Immanent Philosophy of X

I have been involved for the last fifteen years or so in a project which I take to be an interesting and important part of the philosophy of science: constructing an immanent metaphysics of chemistry. Immanent metaphysics of chemistry aims to provide an account of what the world is like, according to modern chemistry, in the respects in which chemistry studies it. This involves studying chemistry's currently accepted theories, and the implicit assumptions underlying its practices, and then thinking about how the world would be, in the respects in which chemistry studies it, if those theories and assumptions were broadly true. This project is metaphysical in two ways. Firstly, it concentrates on how reality *is* (represented to be), according to chemistry, rather than how the scientists working within chemistry know about it. (Clearly this is a broadly realist project, but that is just the requirement for the exploration of the worldview.) Secondly, it is helpful to draw on various theories and arguments from a particular sub-discipline of philosophy—metaphysics—in the hope that drawing on these theories and arguments may help us to fill out chemical theories more fully, helping us decide between different interpretations of them. It may not always be relevant to chemists to fill out an ontological picture in detail, or to decide between two or more metaphysical possibilities which are compatible with some theory as it is normally formulated. It cannot be taken for granted that chemistry's theories will speak for themselves, from a metaphysical point of view. Bringing in metaphysics might seem to undermine the immanence of this approach, but it need not: metaphysics provides various theories that can be regarded as telling us how things *might* be, developed in more detail than other areas of inquiry. In fact the development of such theories, which set out the metaphysical possibilities as we currently see them, is the core business of metaphysics.[8] These insights should not be regarded as being *imposed* on chemical theory, for two reasons. Firstly, what the interaction with metaphysics provides is a set of theoretical resources which are put at the disposal of the immanent metaphysics of chemistry, not imposed on it. Secondly, what metaphysics actually provides is not set in stone: rather it is a *defeasible identification* of a range of (competing) philosophical theories that exhaust (we hope) the different ways the world might be in some respect, such as change over time, or the relationship between a complex object and its parts. A fruitful interaction with chemistry might even lead to a revised view of what the metaphysical possibilities are with respect to diachronic identity or composition, much as an interaction with analytical geometry (and, later, physics), opened up new possibilities in the metaphysics of space.

Immanent metaphysics of chemistry is an original project only in the sense that hardly any philosophers have previously bothered to study chemistry in any detail. Projects very much like it have been pursued for many years with respect to various parts of physics and (more recently) within biology. Indeed as it has become much less fashionable recently to study science as a whole, the philosophy of science has been somewhat taken over by philosophical studies devoted to particular sciences. One might call the broader kind of project of which it is an instance 'immanent metaphysics of X,' where X is some particular empirical science. We stand to learn something from immanent metaphysics of chemistry because chemistry has, for the past two hundred years, made a major contribution to knowledge by successfully gathering evidence and constructing theories about the nature and structure of chemical substances. Moreover, the philosophical debate about scientific realism encourages us to worry about retention: how far the successful theoretical claims of earlier scientific epochs are preserved in later science. This is a real worry in physics, where Newtonian mechanics has given way to classical and then relativistic dynamics, but chemistry's current body of theory appears to have been gathered in a more cumulative fashion. In the late eighteenth century, Lavoisier showed that oxygen and hydrogen are components of water. Modern chemistry still deems them to be so, adding claims about the proportions of that composition

[8] E.J. Lowe (1998), and L.A. Paul (2012) each present the contribution of metaphysics to understanding science in this way: articulating metaphysically possible ways the world could be, empirical science's aim being to decide between the various possibilities. This need not imply that the metaphysical work is prior to the empirical science (example: the interpretation of quantum mechanics).

in the nineteenth century, then a formula to represent that proportionality, and finally a structural interpretation of that formula. So we might reasonably expect to learn something about the nature of substances by carefully and critically examining its theories. Whatever the ultimate fate of chemistry's current theoretical claims, they are not passing fashions.

Why do I call it *immanent* metaphysics of chemistry? Firstly because it is guided (so far as is possible) by empirically-supported claims from within chemistry itself, rather than by any self-appointed transcendental perspective that seeks to place limits, from outside chemistry, on the scope of its claims, or on their epistemic status. Immanent metaphysics is not, of course, the only way to look at chemistry. From time to time philosophers, historians and sociologists set out other perspectives with which to approach the theory and practice of science in general, or of one particular science. Each such perspective has its own motivations, its own rules of interpretation, and consequently its own merits and demerits. For instance, a perspective might be inspired by the philosophical tradition of empiricism, or of classificatory nominalism, or the anthropological tradition of suspending the moral or epistemic commitments of one's own culture. We may well learn much from these perspectives, though we should be careful not to confuse the fundamental posits of an approach with the results of its interpretation. If empiricism, classificatory nominalism or the symmetry principle are imposed from the start then they should not be regarded as something we find in the raw data. In the same way, by assuming a realist stance, one must be careful in using immanent metaphysics of chemistry as part of an argument for realism. Moreover we need to be aware of the particularity and contingency of these perspectives: it shouldn't be assumed that their fundamental posits are inevitable, transcendental requirements which can then be used as a basis on which to interpret chemistry's methods or results. They may only generate pseudoproblems for the study of chemistry, in terms which are unrecognisable from the point of view of that science. Or they may simply be irrelevant to the project of understanding how, from the point of view of modern chemistry, the world is, in the respects in which chemistry studies it.

While pursuing this project, it has been instructive to pay close attention to chemistry's historical development, for two kinds of reason. Firstly, one can only fully understand chemistry's current view of how the world is (in the respects in which chemistry studies it) by understanding what is excluded by that view. In the history of chemistry, and in the much longer history of thinking about composition and structure that preceded the existence of chemistry as a scientific discipline, one can find alternative accounts of chemical reality formulated by thoughtful and intelligent people who are wrestling with the very puzzles about composition, structure and change that modern chemistry engages with. Studying the history of chemistry (and the preceding history of compositional thought) enriches our understanding of modern chemistry by encouraging us to explore the space of possible positions in the metaphysics of chemistry. Secondly, understanding how chemistry has got to where it is helps us to understand how path-dependent various features of modern chemistry are: that is, how different its current theories might have been had it developed differently, in specified respects.[9] In short, the history of chemistry can help us to understand both the content and the contingency of chemistry's current view of the world.

---

[9] Ian Kidd (2015) rightly rejects the 'put up or shut up' defence of the view that there are no viable alternative theories than those currently accepted by science ('put up' the alternative theories or 'shut up', i.e. withdraw the claim that current science is contingent). I am presuming here that science might well have had a different trajectory, and would argue that this can be accepted even by the strongest kind of scientific realist. The two issues of the contingency of the development of science, and of scientific realism, should be fully uncoupled.

Historical contingency is explored within immanent metaphysics of X by keeping fixed the subject matter of a particular science—the natural properties it studies, and their causal role—while varying the historical development of its theoretical or experimental engagement with that subject matter. This allows one to test how inevitable certain theoretical conclusions are, given that fixed subject matter. For instance, one might wonder whether twentieth-century chemists might have come to agree that the chemical elements are classes of atoms which are alike in respect of their nuclear charge *and* their mass, rather than just their nuclear charge.[10] The point of this kind of counterfactual scenario is to wonder whether scientists might have come to different conclusions if (for instance) the evidence had come in a different order, and even to question whether the evidence *could* have come in a different order, thus helping to throw light on vexed historical questions concerning how far particular scientific conclusions (e.g. that elements are classes of atoms of like nuclear charge) should be regarded as having been a matter of well-supported theoretical conclusion, broader theoretical choice, or outright convention. Hasok Chang has recently explored a different kind of contingency in chemistry (Chang 2012). As I understand it, Chang seeks to keep fixed just the empirical evidence available to chemists, while allowing variation in the theoretical conclusions based on that evidence. Thus he argues that, given the evidence, historical chemists might have continued to think of water as an element, or that phlogiston is a chemical component of many substances. Compared to Chang's project, keeping fixed the subject matter (based on modern science's view of how it is) is already to keep too much fixed. He prefers to take a slimmer basis, allowing him to explore how an alternative (or counterpart) chemistry might have viewed the world had it come to a different theoretical conclusion, however remote that worldview might seem from the point of view of contemporary chemistry. Now the kinds of contingency, and the pluralism they support, may be quite different in the two cases in which we keep the subject matter fixed, or just the evidence. Keeping the subject matter fixed, yet finding that modern science might describe it differently from the way they actually do, has a direct bearing on questions of how much of current science is a matter of choice, convention or (mere) perspective: *we* might just as well accept the alternative, given the (empirical) facts. Keeping just the evidence fixed we can investigate the epistemic possibility of the alternative conclusions.

Many historians and sociologists will find immanent metaphysics of chemistry to be something close to incoherent, because it combines realist aims—articulating how the world *is*, according to some part of science—with a genuine interest in the history of science. Does this mean that no-one may engage with past science unless they avoid presentism? Even if historians all agreed on these constraints (as we have seen, they do not), couldn't there be different, perhaps mutually incompatible, ways to engage with past science, each of which is legitimate in its own way? If science is *shared* territory, none of us has the right to exclude others from it.

References

Ayers, Michael 1991 *Locke* (London: Routledge)

Barnes, Barry and David Bloor 1982 'Relativism, rationalism and the sociology of knowledge' in Martin Hollis and Steven Lukes (ed.) *Rationality and Relativism* (Oxford: Blackwell), 21-47

[10] See Kragh 2000 for the background to the actual decision. Philosophical arguments concerning exotic counterfactual histories have been discussed by Donellan 1983, LaPorte 2004, Hendry 2010 and Massimi 2012, among others.

Bennett, Jonathan 1992 'Understanding Locke's Essay' *Times Literary Supplement* 4642 (March 20, 1992)
Bensaude-Vincent, Bernadette 2008 'Chemistry beyond the 'positivism vs. realism' debate' in Klaus Ruthenberg and Jaap van Brakel (eds.) *Stuff: The Nature of Chemical Substances* (Würzburg: Königshausen und Neumann) 45-53
Bird, Alexander 2010 'The epistemology of science—a Bird's-eye view' *Synthese* 175, 5-16
Blackburn, Simon 1998 *Ruling Passions: A Theory of Practical Reasoning* (Oxford: Clarendon Press)
Bloor, David 2007 'Epistemic grace: antirelativism as theology in disguise' *Common Knowledge* 13, 250-280
Brush, Stephen 1995 'Scientists as historians' *Osiris* 10, 215-231
Butterfield, Herbert 1931 *The Whig Interpretation of History* (London: Bell)
Chang, Hasok 2012 *Is Water H_2O? Evidence, Pluralism and Realism* (Dordrecht: Springer)
Donnellan, Keith S. 1983, 'Kripke and Putnam on natural kind terms' in Carl Ginet and Sydney Shoemaker (eds.) *Knowledge and Mind: Philosophical Essays* (New York: Oxford University Press), 84-104
Duhem, P. 1954 *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press), translation by P. Wiener of *La Theorie Physique: Son Objet, Sa Structure*, Second Edition (Paris: Marcel Rivière et Cie)
Feyerabend, Paul K. 1970 'Consolations for the specialist' in I. Lakatos and A. Musgrave (eds.) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press), 197-230
Feyerabend, Paul K. 1988 *Against Method* Revised Edition (London: Verso)
Hendry, Robin Findlay 2010 'The elements and conceptual change' in Helen Beebee & Nigel Sabbarton-Leary (eds.) *The Semantics and Metaphysics of Natural Kinds* (London: Routledge), 137-158
Hoyningen-Huene, Paul 2013 *Systematicity: The Nature of Science* (Oxford: Oxford University Press)
James, William 1948 'The will to believe' in *Essays in Pragmatism* (New York: Hafner Press), 88-109
Jardine, Nick 2003 'Whigs and stories: Herbert Butterfield and the historiography of science' *History of Science* 41, 125-140
Kidd, Ian 2015 'Inevitability, contingency and epistemic humility' *Studies in the History and Philosophy of Science*, this volume.
Kragh, Helge 2000 'Conceptual changes in chemistry: the notion of a chemical element, ca. 1900-1925' *Studies in History and Philosophy of Modern Physics* 31B, 435-50
Kuhn, T.S. 1977 'Objectivity, value judgment, and theory choice' in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press), 320-339
Lakatos, Imre 1971 'History of science and its rational reconstructions' in R.C. Buck and R.S. Cohen, (eds.) *P.S.A. 1970*, *Boston Studies in the Philosophy of Science* Volume 8 (Dordrecht: Reidel), 91-135
LaPorte, Joseph 2004 *Natural Kinds and Conceptual Change* (Cambridge: Cambridge University Press)
Laudan, L. 1990 'Demystifying underdetermination' in C. Wade Savage (ed.) *Scientific Theories* (Minneapolis: University of Minnesota Press), 267–297
Lavoisier, Antoine 1790, *The Elements of Chemistry* (Edinburgh: William Creech) Translation by Robert Kerr of *Traité Élémentaire de Chimie* (Paris, 1789)
Lowe, E.J. 1998 *The Possibility of Metaphysics: Substance, Identity, and Time* (Oxford: Oxford University Press)

Massimi, Michaela 2012 ‘*Dwatery* ocean’ *Philosophy* 87, 531-555
Merton, Robert K. 1973 *The Sociology of Science* (Chicago: University of Chicago Press)
Needham, Paul 2006 ‘Aristotle’s theory of chemical reaction and chemical substances, in Davis Baird, Eric Scerri and Lee McIntyre (eds.) *Philosophy of Chemistry: Synthesis of a New Discipline* (Dordrecht: Springer), 43-67
Nolan, Daniel 2013 ‘Why historians (and everyone else) should care about counterfactuals’ *Philosophical Studies* 163, 317-335
Oreskes, Naomi 2013 ‘Why I am a presentist’ *Science in Context* 26, 595-609
Paul, L.A. 2012 ‘Metaphysics as modeling: the handmaiden’s tale’ *Philosophical Studies* 160, 1-29
Popper, K.R. 1959 *The Logic of Scientific Discovery* (London: Hutchinson)
Popper, K.R. 1963 *Conjectures and Refutations* (London: Routledge and Kegan Paul)
Popper, K.R. 1970 ‘Normal science and its dangers’ in I. Lakatos and A. Musgrave (eds.) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press), 51-58
Quine, W.V. 1953 ‘Two dogmas of empiricism’ in *From a Logical Point of View*, Second Edition, Revised (Cambridge: Harvard University Press, 1980), 20-46
Quine, W.V. 1969 ‘Epistemology naturalized’ in *Ontological Relativity and Other Essays* (New York: Columbia University Press), 69-90
Quine, W.V. 1992 *Pursuit of Truth* (Cambridge, MA: Harvard University Press)
Shapin, Steven 1982 ‘History of science and its sociological reconstructions’ *History of Science* 20, 157-211
Shapin, Steven 2010 *Never Pure: Historical Studies of Science as if it was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority* (Baltimore, MD: Johns Hopkins University Press)
Watkins, J.W.N. 1970 ‘Against “normal science”’ in I. Lakatos and A. Musgrave (eds.) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press), 25-37