Kuhn and the Historiography of Science

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Abstract

This article discusses Kuhn conception of the history of science by focussing on two respects in which Kuhn is an historicist historian and philosopher of science. I identify two distinct but related aspects of historicism in the work of Hegel and show how these are also found in Kuhn's work. First, Kuhn held tradition to be important for understanding scientific change and that the tradition from which a scientific idea originates must be understood in evaluating that idea. This makes Kuhn a historicist in a sense we may call *conservative* (drawing on Mannheim). Secondly, Kuhn held that there is a pattern to the development of science. In the light of the fact that he held scientific change to be law-like, we can call this second aspect of Kuhn's historicism *determinist* (in a parallel with Marx). I discuss the relationship of Kuhn's historicist historiography to the philosophical purposes he had for his history of science, namely to refute a conception of scientific progress as driven towards increasing truth by something like 'the scientific method'. I argue that while this determinism refutes certain positivist conceptions of scientific change, it also requires *internalism*—the view that the causes of scientific change come from within science, not from outside. Consequently, Kuhn historiography of science contrasts with that implicit in much of post-Kuhnian science studies.

1 Introduction

A useful way to understand Kuhn's thinking about the nature of the history of science is to see it as embodying two principal strands of historicism, both of which we can find in Hegel. While Kuhn certainly did not acquire his views about the history of science from thinking abut Hegel on history, brief reflection on Hegel's historicism will help illuminate analogous elements in Kuhn's thought. Doing so will bring us to a better understanding of the relationship between Kuhn's conception of the history of science and that promoted by many more recent students of science studies, in particular exponents of the Sociology of Scientific Knowledge (SSK). In the light of his historicism, we can see why Kuhn took a predominantly internalist approach to the explanation of scientific change whereas SSK adopted externalism.[1]

2 Hegelian strands in historicist historiography

Hegel's view of the relationship between philosophy and its history, as articulated in his *Lectures on the History of Philosophy* (1825), is often summarised as claiming that

[1] This articles expands on ideas first presented in Bird 2012b and discussed at a meeting of the Institute of Historical Research, to whom I am grateful for helpful comments.

philosophy is the history of philosophy. Hegel contrasts his approach to philosophy with the ahistorical approach typical of the enlightenment. Descartes, for example, sought a method for philosophy and for the foundations of science that would be a valid method in any context of enquiry and would yield results of permanent value. Similarly, Kant sought to develop an ethics from principles of pure reason; again, both the validity of the method and the truth of its deliverances were intended to be sempiternal. For both Descartes and Kant, and the majority of the philosophers of the enlightenment (and indeed before and since), if a philosophy is to be satisfactory, both its methods and its results should stand for all time, independently of the particular historical conditions in which they were produced. According to Hegel, a philosophical idea or argument can be neither understood nor (therefore) evaluated independently of the historical context in which it is produced. To engage with philosophy is necessarily therefore to engage with its history. (This historicist approach to philosophy and to ideas in general is also to be found in the work of Vico and of Hegel's immediate predecessor, Herder.)

As I have just articulated it, Hegel's historicism may seem to imply both relativism and contingentism, that there are no absolute truths since all truth is relative to an historical context and that there are no general explanations in history, even in history of philosophy, since ideas and actions are the product of local rather than general factors. What makes Hegel's historicism interesting is that it denied both of these. On the contrary, according to Hegel there are important absolute truths and values, and furthermore these play a role in explaining the historical development of ideas. For Hegel, the Absolute Idea or World Spirit determines the evolution of history: 'History is the Idea clothing itself with the form of events' (Hegel 1821: §346). That is, there is an underlying 'logic' to history, from which a pattern emerges in historical evolution.

Thus there may be appear to be a tension within Hegel's historicism, as both implying and rejecting relativism—perhaps encapsulated by the tag 'objective idealism' used to describe Hegel's philosophy. But on closer inspection this tension disappears. First, the relativism of the first really concerns the rationality of agents and societies. It is in the interpretation and evaluation of what people say and do that we need to refer to their historical context. *Methodologically* it might also make sense to treat truth as relative: in assessing the genius of Ptolemy's *Almagest* it is not appropriate to criticise him on the grounds of being factually wrong in his geocentrism. This relativism is entirely consistent with the claim that underlying the evolution of human history is a process that has a certain direction and structure or mechanism. There is an analogy here with Kant's distinction between the phenomena (which are relative to the subject's forms of intuition and categories) and the noumena (which are absolute).[2] Indeed, it is the very evolution of history that gives rise to the differ-

[2]There are of course this important differences. Hegel's Absolute is part of the world of ideas, whereas Kantian noumena are not. Kant denies that the noumena are knowable, whereas Hegel does claim to know something about the Absolute. The latter leads to a familiar point on which there is indeed a tension, how can the relativist rationally make any claims about general and absolute truths. Hegel is alive to this point, even if he does not resolve it entirely. On the one hand the philosophical historian seeks an insight into the abstract reason that lies behind historical processes. On the other hand, no historian can avoid some element of subjectivity, for that is essential to historical interpretation. A reflective, philosophical historian is aware of this and so may avoid merely imposing their preconceptions on the historical data, which is the danger facing an historian seeking general laws but who is unaware of the historical conditioned nature of their own thought. By being aware both of the contextual nature of thought and

ent eras of thought in relation to which particular ideas must be evaluated. So, in fact, the two elements of Hegel's historicism are connected. It is *because* thought or consciousness evolves that we need to considers ideas in their historical context.[3] Beiser (1993: 279–80) distinguishes a horizontal level in Hegel's philosophy of history from a vertical level. The former concerns a society's or nation's particular circumstances (e.g. geographic or demographic), and the uniqueness of these means that there is no comparison of societies against an absolute standard; we can assess them only relative to those peculiar circumstances. But there is also a vertical level, that of the development of world history, and nations can be judged according to their contribution to progress towards the end of history, the self-consciousness of freedom.

The first dimension of Hegel's historicism, that which is concerned with tradition and understanding ideas from their historical context, I call the *conservative* strand of historicism,. In so doing I employ Mannheim's contrast between 'conservative' thinkers who emphasise the importance of tradition and history and those who endorse an Enlightenment 'natural law ideology' (Mannheim 1953).[4] The second dimension, which identifies laws or patterns in the development of history, I call the *determinist* strand. This strand is exemplified also by Marx and by Comte, and is the dimension of historicism attacked by Popper (1957). According to determinist historicism, the historian is not limited to describing and explaining particular events but may hope also to see in the many particular events an underlying pattern. In this respect history has one affinity with the sciences. The historian of genius, like a great scientist, will not only identify such patterns but may also seek to explain those patterns by reference to an underlying mechanism.

3 Historicism in the work of Kuhn

3.1 Conservative historicism in Kuhn

Kuhn's approach to the history of science exemplifies both the strands of historicism identified in the work of Hegel.[5] From the perspective of the philosopher of science, the conservative strand is the most striking. For the logical empiricist and positivist philosophy of science that preceded Kuhn held that the evaluation of a scientific theory is *sub specie aeternitatis*; theory assessment is a matter of applying the laws of theory confirmation to the total available evidence.[6] Those laws should tell one how well the theory is supported by that evidence. And those laws, like the laws of

of the existence, albeit obscured of an underlying reason, the philosophical historian can partially transcend their own era. But only partially, which is why Hegel (1821) tells us 'The owl of Minerva spreads its wings only with the falling of the dusk', intimating that it is only with the end of history that we can be in a position to understand fully how the Absolute shaped the unfolding of history.

[3] To give an anachronistic analogy: it is because lifeforms have evolved that to understand the nature of a species, we need to understand the ecological environment in which that species originated. (This analogy stands despite the important difference between biological evolution and Hegel's historical evolution in that the latter has a teleological aspect that the latter lacks.)

[4] See Bloor (1997) for more on understanding Kuhn in the light of Mannheim.

[5] Reynolds (1999) also mentions Kuhn in connection with different species of historicism.

[6] While there was disagreement about what those laws were, or whether they should in fact be laws of falsification, logical empiricists agreed that such laws would be perfectly general and context-independent.

deductive logic, hold for all people at all times. Kuhn's radical departure from this application of 'natural law ideology' to science was to suggest that the evaluation of a theory is relative to a specific tradition of puzzle-solving.[7] Kuhn (1970: 174–5) regarded the term 'paradigm' as having two senses. The broader sense encompasses the shared commitments of a scientific community, for which he also used the term 'disciplinary matrix' (1970: 182). The second sense is more narrow, and refers to the most central of the community's commitments, its *exemplars* (1970: 187). Exemplars are the community's exemplary solutions to its scientific puzzles. These set the standards for subsequent science in that domain. A piece of good science, a satisfactory proposed solution to a puzzle, will resemble an exemplar puzzle-solution. Hence theory evaluation is not context-independent, but is relative to a puzzle-solving tradition. Furthermore, the puzzles themselves emerge from the puzzle-solving tradition. Worthwhile puzzles are ones that resemble existing puzzles or emerge from gaps in the existing tradition. So the Newtonian tradition sets the puzzles of reconciling the observations of planets, satellites, and comets to Newton's laws and of measuring the value of the gravitational constant G (among many others). The importance of tradition shows itself also in the phenomenon of incommensurability.[8] For Kuhn (1970: 149–50) claims that a scientific theory may not even make sense to someone working outside the tradition from which it originates. There may be incomplete understanding because not only is evaluation anchored in the exemplars of the tradition but so also are important elements of the meanings of scientific terms.

3.2 Determinist historicism in Kuhn

From the logical empiricist perspective, the history of science should show no interesting patterns. The evolution of modern science is the story of discoveries building on and adding to the stock of pre-existing knowledge. Armed with a general scientific method and logic of confirmation, science will inevitably accumulate discovered truths. The process may be accelerated thanks to scientists of genius in the right place at the right time, it being acknowledged that the process of discovery (as opposed to justification) cannot be entirely methodical. But deviation from the accumulation of knowledge is rare, being mostly due to (often externally driven) deviations from proper methods (e.g. Lysenkoism); in some cases a false theory might look initially attractive (e.g. the caloric theory of heat), but the growing weight of evidence would eventually point in the right direction.

Against this expectation of accumulation, Kuhn's claim that there is a much more interesting, fundamentally cyclical pattern with its alternating phases of normal and extraordinary (revolutionary) science, represents a significant challenge. In comparison to logical empiricism it underplays progress. If true it would suggest that there are *systematic* deviations from the method and logic of science. The transition from

[7] The idea of *tradition* is central to Kuhn's description of normal science and the function of paradigms (see for example Kuhn 1959: 227, and Kuhn 1962: 10). Hegel (1825: 2–3) also refers to the importance of tradition in science, 'likewise, in science, and specially in Philosophy, do we owe what we are to the tradition which, as Herder has put it, like a holy chain, runs through all that was transient, and has therefore passed away.'

[8] Beiser (1993: 279–80) says that as portrayed by Hegel, the values of each nation and the manners in which they achieve the self- awareness of freedom are incommensurable between nations.

normal to revolutionary science, says Kuhn, proceeds via a crisis. Revolutions are messy affairs in which the change of paradigm is contested. The existence of regular, seemingly inevitable crises and revolutions indicates that scientific progress cannot in fact be the accumulation of truths. The fractious nature of revolutions suggests that scientists cannot be applying general rules of confirmation. Thus the idea that there is a pattern to scientific change, a law of scientific development is an important component of Kuhn's thought and represents a radical break with the preceding orthodoxy. In passing we may note the parallels between Kuhn's account of change in scientific ideas with Hegel's account of the transformation of the Absolute Spirit (Bird 2000: 129–30). In the latter a thesis gives rise to a second idea, the antithesis, in conflict with the first, the conflict being resolved in the synthesis; likewise in the former research within a paradigm generates an anomaly, leading to resolution through revolutionary change, where the new paradigm retains elements of its predecessor while creatively accommodating the anomaly that caused the revolution.

3.3 The two strands of historicism united in Kuhn

I mentioned that the two strands of historicism are linked in Hegel's work. The same is true for Kuhn, though in an importantly different way. In Hegel's case, it is because of the historical development of the absolute that ideas are historically conditioned. So determinist historicism implies conservative historicism. And to an extent we may say the same about Kuhn, for if there are radical, incommensurable breaks in scientific thought, then the assessment of a scientific idea will require placing it in its correct scientific era.

There is however another, deeper link between the conservative and determinist strands in Kuhn's historicist historiography of science, operating in the other direction, from the conservative to the determinist. As mentioned, scientists may identify patterns in the phenomena; they may also wish to explain those patterns by reference to underlying mechanisms or more general laws. Kepler identified the elliptical nature of the orbits of the planets, and other patterns besides; Newton explained these with his theory of gravitation. Mendeleev discovered the periodic pattern among the elements.; this was explained by the atomic theory developed by Rutherford, Bohr, Thompson, and Chadwick. To see a pattern in the history of science is one thing, to explain it is another, although in reality these two processes are not so easily separated. The determinist strand in Kuhn's thinking gives us his belief that there is a cyclical pattern in the history of science. The conservative strand, the fact that science evolves by exploiting a paradigm-based tradition of puzzle-solving, explains the pattern.

Normal science exists because a scientific field is dominated by a set of exemplars. As mentioned above, these exemplars set the agenda for future research, such as showing how all objects in the solar system conform to Newton's laws (in the Newtonian paradigm). Not only were these problems made relevant by Newton's *Principia*, but the latter also furnished the means of solving those problems, primarily though examples of using the theory to solve problems of this sort. This explains the existence of normal science. Not all normal science puzzle solving is straightforward. For example, mathematical astronomers in the eighteenth century

found it difficult to reconcile the observed orbit of the moon with Newton's theory. Clairaut and d'Alembert calculated the value for the period of revolution of the Moon's perigee, which is the point on the Moon's orbit that is nearest the Earth. This they found to be eighteen years, though it was known from observation to be half that. Such apparent conflicts between the observed phenomena and theory, along with other cases where scientists fail to solve puzzles, are *anomalies*. Kuhn (1970: 80) explains that anomalies are not regarded as counter-evidence against the theory at the centre of the paradigm. During normal science, the failure to solve a puzzle is attributed to the scientist or the scientific community. But if anomalies accumulate that are particularly significant and recalcitrant, then the blame for the anomalies may begin to shift away from the scientists and towards the paradigm. This is what occurs during the periods of crisis. The anomalous motion of the moon was sufficiently serious for Leonhard Euler to suggest that Newton's law of gravitation might need adjusting, until Clairaut showed that the anomaly was due largely to the inaccurate approximations being used. This might be thought of as a mini-crisis that was successfully resolved within the paradigm. More serious was the crisis that arose in the late nineteenth century stemming from the anomalous precession of the perihelion of Mercury, reported by Le Verrier in 1859 and the null outcome of the Michelson–Morley experiment. Since, according to Kuhn's conservatism, normal science requires an established tradition with a credible paradigm, crises must be resolved. If they are not resolved within the existing paradigm, then that paradigm must be replaced. In particular it must be replaced by a paradigm that can support a puzzle-solving tradition. Thus we have scientific revolutions. In this way the conservative strand in Kuhn's historicism (the emphasis on a tradition of puzzle-solving) explains the determinist strand (the law-like cyclical pattern of scientific change).

4 Kuhn's internalist historiography of science

Kuhn's work was stimulus to a wide range of science studies from history of science through to sociology of science, and many of those working in these fields see themselves as, in a loose way, heirs to a Kuhnian legacy. Barry Barnes's book *T. S. Kuhn and Social Science* is just one prominent example of this. Kuhn (1992) himself nonetheless repudiated in the strongest terms the most important (and philosophically most sophisticated) school within science studies, the Strong Programme in the Sociology of Scientific Knowledge (SSK), of which Barnes was a leading light. Furthermore, the scope of Kuhn's criticisms embraced implicitly a broader spectrum of the science studies movement than the Edinburgh School alone.

In this section I wish to explain in what way Kuhn rejected the social constructivism found in much SSK, and why he was right to do so in the light of his commitment to historicism. The social constructivism that Kuhn found antithetical to his own ideas holds that the principal factors in determining the outcome of a scientific episode, such as a crisis, are social and political factors originating outside science—what came to be known as *externalist* history and sociology of science.[9] According to this approach, the triumph of Pasteur's theories rejecting spontaneous generation is not the result of the probative power of his experiments with swan-necked flasks.

[9]The internalism versus externalism debate is perhaps somewhat outdated now. Yet it was very much alive in Kuhn's lifetime and his work gave an impetus to it—often in a manner of which he disapproved.

Rather that success may be attributed to the fact that his ideas were better attuned to the views of the conservative, Catholic hierarchy that ruled in the France of Louis Napoleon (Farley and Geison 1974; Farley 1978). The success of Darwinism is not held to be a consequence of the arguments presented in the *Origin of Species*, but is instead to be explained by the natural sympathy of free-market Britain to the idea that improvement is the outcome of unfettered competition (Young 1969). These are examples of external explanations of scientific change, contrasting with internalist explanations that refer only to aims, values, practices, and beliefs originating within science.

Kuhn's own account of science leaves little room for such external influences, certainly not enough for them to be the principal determinants of the outcomes of scientific debates. Let us first consider normal science. As explained, progress during normal science is driven by the paradigm, the set exemplary puzzle-solutions that define a puzzle-solving tradition. These set the agenda—they define what kinds of puzzle are worth pursuing; and they set the standards by which proposed solutions to those puzzles are assessed. Kuhn's account leaves no space for external influences in either regard. Since, Kuhn emphasizes, the bulk of scientific activity is normal science, it follows that at least most scientific change is governed by factors internal to science.

Kuhn explicitly endorses a predominantly (but not exclusively) internalist approach. He (1971: 137–8) tells us that '... the ambient intellectual milieu reacts on the theoretical structure of a science only to the extent that it can be made relevant to the concrete technical problems with which the practitioners of the field engage' and goes on to criticize historians (from outside history of science) who ignore this fact. Kuhn (1971: 148–9) acknowledges that (typically older) history of science could be limited by exclusive internalism, but 'that limitation,' he says, 'need not always have been a defect, for the mature sciences are regularly more insulated from the external climate, at least of ideas, than are other creative fields.' To the general insulation of scientific ideas from external influences, Kuhn makes two exceptions. While the development of a tradition is internally driven, the *origins* of that tradition need not be. 'Early in the development of a new field ... social needs and values are a major determinant of the problems on which its practitioners concentrate' (1968: 118). Kuhn contrasts this with the later evolution of a scientific specialty, 'The problems on which such specialists work are no longer presented by external society but by an internal challenge to increase the scope and precision of the fit between existing theory and nature' (1968: 119).[10]

Kuhn's second exception concerns the *rate* at which science develops. Kuhn (1968: 119) tells us that the timing of a scientific advance can be conditioned by external factors. That must be correct, if only because prevailing economic conditions will determine the quantity of resource put into research. Kuhn also suggests that because the various scientific disciplines interact, there may be a cumulative effect

[10] I note that Kuhn's internalism does mark an element of difference from Hegel's conservative historicism. For the latter is justified in part by a holism about thought. According to Hegel, the various components of a society, from its politics and religion to its culture and philosophy form an inseparable whole. And so when one element changes so do all including its philosophy (Hegel 1861; *c.f.* Beiser 1993: 274). One would naturally take Hegel to include science in this. Kuhn, however, claims that a modern science is largely insulated from external changes, its origins and pace of progress excepted.

from external factors on the evolution of science. Advances in technology clearly make a difference to the ability of science to progress.

It is important to note that neither of Kuhn's two exceptions suggest that external influences regularly influence the *outcome* of a scientific investigation or debate. In SSK one can distinguish a *weak* programme, which looks at the broad social and political environment and its effect on, for example, the existence of scientific institutions, as exemplified by Merton's 'Science, technology and society in seventeenth century England' (1938), and a *strong* programme, as exemplified by Shapin and Schaffer's *Leviathan and the Air-Pump* (1985), according to which the content of the accepted results of science and the very terms of scientific discourse are influenced by social and political factors. At most Kuhn's work gives partial support to the weak programme. For example, concerning the crisis in Ptolemaic astronomy preceding the Copernican revolution, Kuhn (1970: 69) tells us that one ingredient is 'the social pressure for calendar reform, a pressure that made the puzzle of precession particularly urgent'. He goes on to tell us, concerning a scientific crisis, that 'In a mature science—and astronomy had become that in antiquity—external factors like those cited above are principally significant in determining the timing of breakdown, the ease with which it can be recognized, and the area in which, because it is given particular attention, the breakdown first occurs.' While acknowledging that such factors can be important, Kuhn emphasises that 'technical breakdown would still remain the core of the crisis.' So while external factors may influence the manner in which this episode occurs, it remains the case that internal factors explain why it could occur at all.[11]

Even if normal science and crisis can be explained on purely internal grounds, perhaps we might expect externalism to be more likely to be true of Kuhn's account of revolutionary science? Kuhn (1962: 152–3) himself writes:

> Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons—for example, the sun worship that helped make Kepler a Copernican—lie outside the apparent sphere of science altogether. Others may depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role.

One should not over-emphasize the externalism of even this passage. As Kuhn says, *some* of the reasons an individual has for adopting a paradigm may lie outside of science, and he gives only one example, Kepler's sun-worship. By implication, he thinks that the 'others' he mentions do not lie outside of science. Clearly reputation is internal to science. It is true that differences in personality might make scientists differ in the degree to which they are disposed to adopt radical ideas. Or a scientist's openness to an idea might be influenced by the fact that she was trained in a laboratory where such ideas were developed, or because working on that theory offers better job prospects. But again it is not clear that these are considerations external to the practice of science, at least not in a way that threatens the central internalist claim that is important to Kuhn, that it is the requirement of puzzle-solving that overwhelmingly determines which ideas are developed and adopted. As Kuhn says

[11] For more detail on Kuhn's internalism in relation to SSK, see Bird (2012a).

immediately after the quoted passage, 'Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis. When it can legitimately be made, this claim is often the most effective one possible.' The most effective way of advancing a new paradigm is to show that it solves the problems that led the old one into crisis. Kuhn then goes on to point out that this may not always be possible; indeed the new candidate paradigm may not help at all with the crisis-evoking problems. In that case novel predictions, predictions of phenomena that would be entirely unsuspected under the old paradigm, can be persuasive (such as the prediction of the phases of Venus by Copernicus's theory). Kuhn then mentions the role of aesthetic considerations. He also discusses at length the characteristic of revolutions we have subsequently called 'Kuhn-loss' and the importance of a new paradigm being a fruitful basis for new problem-solving research. In assessing whether Kuhn gave direct encouragement to externalist study of science, we must set the short quoted passage against the six pages that follow, in which he emphasizes in detail the importance of the puzzle-solving tradition in determining its own development.

During revolutionary science, however, it might be thought that there is no puzzle-solving tradition to provide this determining role. And this would suggest that extra-scientific forces may be able to fill the gap and swing the outcome, as Barnes (1981, 1990) argues? This, I believe, is a misinterpretation of Kuhn that is based on the idea that revolutions are all-encompassing and radical breaks with the past. While it is true that Kuhn may have overstated the difference between normal and revolutionary science, it is also true that Kuhn lays great emphasis on progress and continuity through revolutions. The final chapter of *The Structure of Scientific Revolutions* is entitled 'Progress through Revolutions'. The penultimate chapter, 'The Resolution of Revolutions', describes the constraints imposed on the new paradigm by the long-standing success of its predecessor in puzzle-solving. Such constraints mean that there is significant continuity in revolutionary science.[12] From the perspective of the internalism/externalism debate there is more in common between normal and revolutionary science than the are differences. In both normal and revolutionary science the principal driver of progress is scientific problem solving. During normal science the need to solve problems is satisfied by the paradigm. During extraordinary science the need remains, but now must be satisfied by finding a replacement paradigm. What determines the outcome will still be, above all, the power of a proposed paradigm to solve puzzles. That may not determine the outcome uniquely and unambiguously—Kuhn stresses that there is room for rational disagreement about the relative problem-solving power of a proposed new paradigm compared to the old one or a competitor. Nonetheless, the fact that the dispute is about scientific puzzle-solving power restricts the choices available. The puzzle-solving tradition thereby still exerts its force during revolutionary science, though not in as straightforward or as decisive a way as during normal science. The participants in the debate must be able rationally to believe that their favoured solution will deliver more and better puzzle solutions than its competitors.

[12] Kuhn returned to this theme in several of his later writings, for example in his essay 'Objectivity, Value Judgment, and Theory Choice' (1977b), in which he articulates the five scientific values (accuracy, consistency, breadth of scope, simplicity, fruitfulness). His concern throughout is to reject accusations of subjectivity in theory preference, while allowing space for reasonable disagreement across paradigms.

In particular, supporters of a new paradigm must, in most cases, be able to show that it solves a sizeable proportion of the most significant anomalies that beset the old paradigm, while also preserving the bulk of the puzzle-solving power of its predecessor. Since finding an innovative solution that achieves this is not easy, most episodes in revolutionary science will provide very few choices, typically there will be only a single revolutionary proposal to challenge the old paradigm. Given the infinite range of beliefs a scientist could have about a given subject matter, all but a small handful are straightforwardly excluded by factors internal to science, even during extraordinary science.

Of course, this does seem still to leave some room for external factors to influence the outcome of a scientific revolution. Nonetheless, I do not think that it was Kuhn's view that such factors play a determining role. The fact that there is room for rational disagreement does not mean that the view of any individual scientist let alone the views of the community as a whole must be swayed by something else. What it does mean is that the resolution of a revolution will be a much more protracted affair. Within normal science there may be disputes, but typically these can be resolved using the resources of the paradigm. The causes of AIDS were initially disputed but standard techniques identified a particular virus as the cause in a way that is beyond rational dispute. In such a case there is no Kuhn-loss—no pre-existing beliefs and commitments need to be given up; the success of the viral explanation is clear by established standards; and the research opportunities (and so scientific benefits) afforded by the new discovery are transparent.

On the other hand, in revolutionary science there is Kuhn-loss to be weighed against the claims of puzzle-solving; there are at least some conflicts with existing standards (or at least beliefs); and because of this the potential for the alleged new discovery to support a fruitful programme of research (future puzzle-solving) is unclear, especially when we have to given up an existing tradition. When Barry Marshall and Robin Warren proposed that the principal cause of gastric ulcers is a bacterium rather than, as had been believed, excess acid brought about by factors such as stress, a whole sub-field of research (as well as treatment) was under threat; consequently it was unclear at the time whether, in puzzle-solving terms, the new proposal would be more productive than the established view. We are not comparing like with like in such a case, because we are comparing an existing track-record with future promise. So there is plenty of room for difference of opinion as to whether the new view should be adopted or not. Biographical factors, as Kuhn says, may play a part in determining how individual researchers respond. Older scientists will have kudos, expertise, research programmes and laboratories invested in the established approach whereas younger scientists will see the newer view as offering them opportunities for speedier advancement than they might otherwise have had. But such room for differences of opinion and influence by professional considerations cannot persist for ever. As time goes by the puzzle-solving power of the new view will turn from potentiality to actuality and a more direct comparison of old and new will be possible. In the case of Marshall and Warren, after initial resistance, community opinion did concur reasonably speedily. While there may be no definitive point at which it becomes irrational to stick with one or other view, that does not mean that it is reasonable to hold on to either view indefinitely. Although one can find scientists who continued to believe in the electro-magnetic aether in the 1920s, most

theoretical physicists had accepted Einstein's (special) theory of relativity, originating in 1905, well before the outbreak of World War I. At the same time, the advantages that may attract an ambitious young scientist to a new field will soon tarnish if it fails to live up to its promise as a vehicle for a productive puzzle-solving tradition; cold-fusion is a case in point. So the difference between normal and extraordinary science is not one between phases when internal or external factors are decisive. It is a difference in the speed and manner in which internal factors, unaided, reach their conclusion.

A different reason for thinking that external factors must be significant is the thought that the questions upon which scientists work are very frequently determined by the material needs of broader society. Bacon's vision in the *Novum Organum* is for a scientific enterprise that leads to economic prosperity and so one would expect such science to be concerned with questions directly connected with problems arising in the social and economic sphere. One might think of the effort made by astronomers to solve the problem of longitude in this vein. Given Kuhn's insistence on the insulation of mature science from external sources of problems, it is not surprising that Kuhn (1971: 143) makes an explicit differentiation between science and technology. 'As a first approximation,' he says, 'the historian of socio-economic development would do well to treat science and technology as radically distinct enterprises, not unlike the sciences and the arts.' Technology does respond to external demands but science does not. Of course, that may be a mere definitional distinction, but Kuhn (1971: 142, 147) gives us reason to think that it is not. For, he argues, science and technology have historically been distinct spheres of activity. It was only in the middle third of the nineteenth century that Bacon's vision began to be achieved, and knowledge generated by scientists began to make a technological difference to society, first through dyestuffs and then later through electrical devices and techniques such as the pasteurization of beer, wine, and milk. Of course, the insulation of science from technology is not guaranteed, and one might wonder whether modern science is in a different position. And certainly the requirements of governments for research to respond to externally generated challenges may blur the distinction between science and technology to the point of eradication. Whatever the truth may be about the practice of science today, Kuhn's view of the distinction between science and technology aligns with the difference of the source of the problems—internal and external respectively—that motivate the intellectual activity in each

5 Historicism and internalism

Above I noted a *prima facie* tension between the relativism implied by Hegel's conservative historicism and the objectivism implied by his determinism. Likewise it would appear that Kuhn's emphasis on paradigms (disciplinary matrices, tradition) implies relativism whereas the rejection of externalism, implied by his determinism, is associated with objectivism about scientific knowledge. This tension is only apparent in both cases. I briefly discussed Hegel's attempted resolution above: it is the deterministic evolution of the Word Spirit that generates the different stages of development in which ideas are anchored; the philosophical historian can transcend this to some extent, if not completely, to see the working of reason in the Absolute.

Because Kuhn's direction of explanation is the other way around, from the existence of traditions (conservatism) to the pattern of normal science and revolution (determinism), Kuhn's emphasis on relativism is somewhat stronger than Hegel's.

In brief, the answer in Kuhn's case is this: while externalism leads to relativism (or scepticism), the reverse is not the case—relativism does not necessarily lead to externalism. It is true that objectivists, those who believe that science has reasonable success in uncovering facts about an independent world, will be internalists. But it does not at all follow from this that all internalists must be objectivists. Internalism makes room for both objectivists and relativists who believe that the determinants of scientific change are encapsulated within science itself. And that is the kind of internalist I take Kuhn to be.

Indeed Kuhn has to be this kind of internalist if he is to be true to the determinist strand of his historicism. If externalism were true, so that factors originating outside science were the main drivers of scientific change, then there would be no reason to suppose that there would be any patterns in the history of that change. Instead one would expect the history of science to demonstrate the same chaos and contingency that we find elsewhere in human affairs. Take the two examples from nineteenth century biology I mentioned earlier—Pasteur's rejection of spontaneous generation and Darwin's account of evolution through natural selection. If the externalists are right, then the inception and success of these two theories are the results of socio-political forces that happen have opposing natures, occurring at the same time in different countries: clerical conservatism in France, economic liberalism in Britain (note that one of the alleged political advantages of Pasteur's results is that they challenged atheistical Darwinism, which many held to require some form of spontaneous generation). Since these different social forces are the products of different sequences of historical events in the two countries, it is difficult to see how the totality of forces such as these could conspire to produce the orderly cycle that Kuhn sees in the history of science. To use a mechanical analogy, Kepler was able to discern the elliptical orderliness of the solar system because the solar system is a simple and isolated system. If it were frequently perturbed by large inter-stellar objects passing through or nearby, then there would have been no elliptical orbits for Kepler to discover. Likewise, a necessary condition of the truth of Kuhn's theory is that the drivers of (the content of) scientific development are local to science, which is largely isolated from the influence of other developments in history.

Externalism and determinist historicism in science could be reconciled if the patterns in the history of science reflected larger patterns in history that pervade the social and political as well as the scientific. The laws of scientific development would be manifestations of a broader historicist truth. Yet this seems implausible for two reasons. First, such global historical determinism has little credit. The great schemes of Hegel and Comte are believed by few, if any, today, and not even all Marxists accept a strict determinism along the lines, for example, of Lenin's version of dialectical materialism. Secondly, such external historical determinism must *explain* the Kuhnian cyclical pattern. No attempt has been made to show how such an explanation would proceed. Indeed, it seems implausible that there could be any such explanation. For historical determinists tend to see history as exhibiting large-scale stages (Comte, Marx), but those could not explain the Kuhn's cyclical pattern (especially as there is not one such pattern, but many, as the pattern for one field of

science need not coincide with the pattern for another field). Furthermore, historical deterministsm tend to believe that history has a direction, even a goal (Hegel). Not only does Kuhn (1962: 172) deny that the history of science has a direction, but to accept that it does would be to permit a kind of whig history of science that SSK rejects (note that whiggism is a feature of Marxist historians). So, even if (implausibly) an external determinism could account for Kuhnian patterns, that would be antithetical to the kinds of externalism promoted by many practitioners of science studies.

6 History of science and philosophy of science

'History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed' (Kuhn 1962: 1) The image Kuhn has in mind is partly historical, but is primarily philosophical. Kuhn's historicism, I argue, makes an important contribution to meeting his philosophical aims. Kuhn's philosophical target was logical empiricism. The logical empiricists, construed broadly enough to include Popper, were concerned to give normative accounts of theory change. History of science can be used to test these normative accounts—on the assumption that scientists normally reason as they ought to reason. This last assumption is important, for without it, the normative theory could be a theory about how scientists ought to change their reasoning habits in order to improve them. One can see Bacon's philosophy of science in that light. As it was, the logical empiricists did believe that scientists reason correctly, by and large; their theories therefore aimed to articulate how scientists do in fact reason. Popper not only thought that should scientists reject theories that are falsified, he also held that they do in fact reject such theories. So Popper's view would itself face falsification if the history of science could show that scientists regularly do hold onto theories in the face of apparently contradictory evidence. This is indeed what Kuhn aims to show with the conservative component of his historicism, according to which normal science is governed by a puzzle-solving tradition. As we have discussed, in Kuhn's view scientists do not abandon a tradition in the face of an anomaly. Rather an anomaly will often be just another puzzle to solve. If a scientist tackles such a puzzle but fails to solve it, that failure is attributed to the limitations of the scientist, not of the tradition. So the very existence of normal science is a major challenge to Popper's falsificationism.

Things are somewhat different with respect to the more central inductivist strand of logical empiricism. Here Kuhn's target is the conception of science as an accumulation of true beliefs acquired by the repeated application of the scientific method (e.g. some form of inductive logic). Such a view is consistent with the existence of normal science. It is revolutionary science that creates the problem for logical empiricism, for these are episodes when well-established beliefs are rejected. However, since such episodes are, in Kuhn's terminology, 'extraordinary', there is room for debate as regards their evidential value against the logical empiricist picture. For their relative rarity will allow the logical empiricist to regard them as occasional exceptions, in some cases pathological episodes (or corrections to pathological science) or features of immature science, and so forth. This is where the determinist strand of Kuhn's historicism becomes relevant. Since Kuhn can show

that scientific change has structure, the cyclical structure of normal science–crisis–revolution–normal science, then non-cumulative episodes, revolutions, cannot be dismissed in this way; they are inevitable parts of the scientific process.

Kuhn himself aims for a major revolution in the philosophy of science. He rejects the common assumptions of the logical empiricists that the aim of science is truth and that scientific rationality consists in applying some kind of logic to the relationship between a theory and straightforward assertions concerning the scientist's experience. Kuhn's replacement paradigm is intended to be one in which the aim of science is puzzle-solving and scientific rationality consists in matching proposed puzzle-solutions to exemplary puzzle-solutions.[13] Kuhn's view need not seem quite so utterly radical when we consider that much human cognition takes place via pattern recognition (think of face recognition) (*cf.* Margolis 1987). However, in his own historical context, the proposal was radical and was perceived as more extreme than it ought to have been. For it was taken as a form of irrationalism about science. Once perceived in that light it is no surprise that Kuhn's detractors and supporters alike took Kuhn to be articulating a vision of science in which scientists and their ideas, unconstrained by rationality, are subject to social forces.

I have argued that Kuhn's view of science was precisely *not* this. Kuhn wishes to argue for his reconceived understanding of scientific rationality precisely by pointing to the pattern he perceives in the history of science, for the latter is explained by that reconception of rationality better than by the old conception. Thus it is the conservative strand of his historicism that supports that reconception of rationality. At the same time, the rationality of science according to either conception requires determinism. A significantly externalist component in science would undermine the deterministic strand of Kuhn's historicism, and so is antithetical to Kuhn's philosophical aims for the history of science. Thus I hope that thinking of Kuhn as a historicist regarding the history of science will allow us to rethink his understanding of that subject and in turn will allow us to get a better perspective on his philosophical aims concerning the nature and rationality of science.

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[13] See Bird (2005) for details. This view is contentious in that Kuhn did not proclaim himself as seeking to revise our notion of rationality. That is because the very notion of rationality is close to the idea of following rules of reason. Kuhn showed that science dispensed with rules but instead employed reasoning by analogy with exemplars. The latter, he was at pains to emphasize, is not in any way irrational.

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