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Normal Science and Dogmatism, Paradigms and Progress: Kuhn ‘versus’ Popper and Lakatos

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One sixties’ summer, shortly before the ‘Summer of Love’, probably the two most widely influential philosophers of science of the twentieth century – Karl Popper and Thomas Kuhn – met at a conference in ‘swinging London’ to compare and contrast their views on the nature of theory change in science.

The debate was recorded (and extended) in an influential book called *Criticism and the Growth of Knowledge*.¹ Although Kuhn was at pains to begin his paper (1970) by stressing similarities between his own views of scientific development and those of ‘Sir Karl’, and although Kuhn’s official line was that the differences between Popper and himself were ‘comparatively secondary’, it soon became clear that those differences were in fact sharp and apparently rather deep. Kuhn claimed, for example, that ‘Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts’ (p. 6). And he suggested that to accept his own account of science was, in effect, ‘to turn Sir Karl’s view on its head’ by accepting that ‘it is precisely the abandonment of critical discourse that marks the transition to a science’ (ibid.). Popper responded² by, amongst other things, admitting that Kuhn’s ‘normal science’ is a real phenomenon and that he had indeed hitherto failed fully to recognise it – but he did so reluctantly in the way that a starstruck lover might be brought to admit that he had hitherto been blind to an imperfection in his inamorata. Normal science is, said Popper, ‘a danger to [presumably ‘proper’, critical] science and, indeed to our civilization [!]’ (p. 53), adding for good measure that ‘[i]n my view, the ‘normal’ scientist... is a person one ought to be sorry for’ (p. 52).

Although, of course, conducted in the best academic traditions of (at any rate, professed) mutual respect, this was, then, and despite the tenor of the times, no swinging love-in. It more than merits the name ‘Popper–Kuhn controversy’. Underlying the specific points of disagreement is, as we shall see, what many philosophers at least took to be a fundamental difference over the ‘objectivity’ of scientific knowledge and the ‘rationality’

of scientific change. Several other eminent philosophers of science contributed to the discussion in London, among them Paul Feyerabend, Stephen Toulmin, John Watkins, and especially Imre Lakatos, whose ‘methodology of scientific research programmes’ was explicitly developed as an attempted ‘synthesis’ of Kuhn’s and Popper’s opposing views. Kuhn’s exasperation with at least some of his critics shines through his long and revealing ‘Replies to Critics’. The controversy grumbled on for a number of years.

In this essay, I review what was at stake in the Popper–Kuhn controversy, and I try to assess the success or otherwise of the Lakatosian synthesis. Although Kuhn raised a number of interrelated issues – many of which merit detailed discussion – I shall focus the treatment in this essay sharply on the question of theory change in science and the role of criticism and testing in theory change. To avoid the danger of excessive rational reconstruction, I begin in fairly close contact with Kuhn’s London paper and Popper’s response to it. Having set them at odds with one another, I shall then, to avoid the danger of overscholasticism, analyse in a general way what I think was, and was not, at issue in the central part of the Kuhn–Popper debate. The debate is, as I hope to show, of more than merely historical interest. Involved in it were important problems that remain unresolved by current philosophy of science.

DISAGREEMENTS – THE ROLE OF ‘TRADITION’ AND THE ROLE OF ‘FALSIFICATION’

Kuhn begins his paper by stressing the extent of his agreement with Popper. They are, for example, united in rejecting the view that science develops by ‘accretion’ and in emphasising instead that change at the level of fundamental theory in science has sometimes been radical or ‘revolutionary’. The disagreements are about the extent and mechanics of such changes. Kuhn identified

two comparatively secondary issues about which my disagreement with Sir Karl is most nearly explicit: [1] my emphasis on the importance [in science] of deep commitment to tradition and [2] my discontent with the implications of the term “falsification.” (p. 2)

The two disagreements are in fact deeper than Kuhn’s emollient rhetoric suggests and are very closely related, as we shall see. Let’s begin by focussing on the second.

What exactly were Popper's views of the role of tests, and particularly of 'falsifications', in science? One side effect of this controversy, at least in some circles, was a series of heated debates on what Popper's views on these issues really were. The position that most commentators would take as the definitively Popperian one, however, is surely that articulated by Popper himself in his *Conjectures and Refutations* (1963). There Popper relates how, 'in the winter of 1919–20', he had responded to the confirmation of Einstein's General Theory of Relativity by the (just-published) results of the Eddington Eclipse Expedition. This striking success for Einstein's theory forced a comparison in Popper's mind with other theories, such as those of Freud and of Adler, which many of his contemporaries also saw as impressively (and multiply) confirmed but which he thought essentially worthless. The problem was that the supposed 'confirmations' in the case of those latter theories came too easily. Indeed, said Popper:

I could not think of any human behaviour which could not be interpreted in terms of either [Freud's or Adler's] theory. It was precisely this fact – that they always fitted, that they were always confirmed – which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness. (p. 35)

The confirmation of Einstein's theory was 'strikingly different':

The impressive thing about this [Einstein] case is the *risk* involved. . . . If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain [possible] results of observation* – in fact with results which everyone before Einstein would have expected. This is quite different from the . . . situation [in the Adler and Freud cases], when it turned out that the theories in question were compatible with the most divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories.

On the basis of this comparison, Popper succinctly characterised his basic position in the form of seven propositions:

1. It is easy to obtain confirmations, or verifications, for nearly every theory – if we look for confirmations.
2. Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should

- have expected an event which was incompatible with the theory – which would have refuted the theory.
3. Every ‘good’ scientific theory is a prohibition: it forbids certain things to happen. The more it forbids, the better it is.
 4. A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think), but a vice.
 5. Every genuine test of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.
 6. Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. . . .
 7. Some genuinely testable theories, when found to be false, are still upheld by their admirers – for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering its scientific status. . . . (pp. 36–7)

Popper does not explicitly include in this list his view on the correct scientific attitude to take when a theory fails a test. However, he does explicitly say in the preamble that ‘If observation shows that the predicted effect is definitely absent, then the theory is simply refuted’. And this is a message very strongly endorsed elsewhere in his writings – bold conjectures and hard refutations followed by new bold conjectures. Notice – it will be important later – that, as he emphasised in point 7, Popper *did* take into account the possibility of a theory’s ‘admirers’ continuing to ‘uphold’ a theory, even when refuted, that is, ‘found to be false’³, but he claimed that such a move carries the ‘price of destroying, or at least lowering its scientific status’.

Kuhn argued, contrary to Popper’s view, that there is only one straightforward sense in which a scientist can be said to be testing a theory. This is within the context of normal science – within a context in which the scientist simply postulates, and so takes for granted, his basic theory and basic methods; what *can* then be tested are ‘statements of an individual’s best guesses as to how to connect his own research problem with [that] corpus of accepted scientific knowledge’ (p. 4). Kuhn insisted that

[i]n no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must *premise* current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and his current theory is required to define that puzzle. . . . Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. (pp. 4–5)

In fact, as he notoriously went on to suggest,

if [this ‘personal conjecture’] fails the test, only [the scientist’s] own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested. (p. 5)

As Kuhn, of course, recognised, the ‘tests’ that Popper had in mind were, on the contrary, ones that (allegedly) *do* challenge fundamental theory. Kuhn listed, on Popper’s behalf, ‘Lavoisier’s experiments on calcination, the eclipse expedition of 1919, and the recent experiments on parity conservation’. Rather perplexingly, he conceded that ‘classic tests’ such as these can be ‘destructive in their outcome’ and concentrated initially on the criticism that such tests, contrary to Popper’s claims, are extremely rare in the history of science. This led to the already quoted remark that ‘Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts’ (p. 6).

Given the position developed in *The Structure of Scientific Revolutions* (1962), however, Kuhn could hardly have meant that the outcomes of these ‘classic tests’ were ‘destructive’ in the sense of directly knocking out the theories underlying the older paradigms concerned. In that book, he made it clear that no one experimental ‘anomaly’ is ever the single crucial piece of evidence that ‘refutes’ a theory. He can only have meant here, then, that these ‘classic tests’ were, *given the context of a feeling of ‘crisis’ induced by other anomalies and problems*, the final straw, or, more explicitly, that, *with liberal helpings of hindsight*, we can now see that they were the final straw. At the time when the test was actually performed, as he emphasised in *Structure* and again stressed elsewhere in his London paper, the negative outcome can only be the ‘final straw’ for *some* – perhaps most, but certainly not all – members of the community. The whole rhetoric of ‘refutation’ and ‘falsification’ suggests disproofs or at least results that will ‘compel assent from any member of the relevant professional community’ (p. 13). But

there are, Kuhn was clear, no such things. His real position, then, was that what Popper seemed to be saying about tests *never* really applies – either in normal *or* in extraordinary science.

The fundamental flaws in Popper's position on testing and 'falsification' stem, according to Kuhn, from his complete misconception (or perhaps lack of any conception) of the role and importance of 'normal science'. That is, of Kuhn's two 'comparatively secondary' points of disagreement with Popper, the first – his 'emphasis on the importance of deep commitment to tradition' – was indeed the more important. Popper's misconception of the role and importance of normal science led him, in Kuhn's view, *both* to an incorrect demarcation criterion between science and pseudoscience *and* to a misappraisal of the merits of holding on to a basic theory when it (or rather, as we shall see shortly, the latest theoretical system based on it) runs into experimental difficulties.

Popper's view was that astrology, for example, is a pseudoscience because it is unfalsifiable. Kuhn argued that this is incorrect – at least if unfalsifiability involves never making predictions that were agreed, on the basis of evidence, to fail. (Kuhn here cited Thorndike for mainly sixteenth-century examples of failed astrological predictions.⁴) The real reason astrology fails to be scientific, according to Kuhn, is that it has not yet developed, and of course may never develop, a puzzle-solving tradition; it has not progressed to the stage of sustaining normal science. For the sixteenth-century *astronomer*, the failure of an individual prediction was a fertile source of research problems. He had a whole armoury of ideas for reacting to failure: there were clear-cut ways in which the 'data' might be challenged (and 'improved') and, if that was unsuccessful, clear-cut suggestions for modifying theory by manipulating epicycles, eccentrics, equants, and so on. No such puzzle-solving ideas were available to the sixteenth-century astrologer. There were 'too many possible sources of difficulty, most of them beyond the astrologer's knowledge [or] control . . .' (p. 9), and hence a predictive failure was entirely 'uninformative'.

On the *central* issue of reacting to falsifications (or rather, according to Kuhn, 'anomalies') by continuing to defend the central theory, Kuhn argued that Popper's account is again quite wrong. Popper always acknowledged that it is possible to defend a theory against a potential refutation by, for example, 'introducing' an auxiliary or by questioning the data. But, as we just saw, he suggested that although undoubtedly possible, any such manoeuvre is automatically under suspicion: '[Such a 'defensive' move] is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering its scientific status.'

Kuhn argued that, to the contrary, not only is it true that ‘all theories can be modified by a variety of *ad hoc* adjustments without ceasing to be, in their main lines, the same theories’, but it is moreover ‘important . . . that this should be so, for it is often by challenging observations or adjusting theories that scientific knowledge grows’ (p. 13).

Popper’s response to Kuhn in his paper in *Criticism and the Growth of Knowledge* (Lakatos and Musgrave 1970) was very strange. He reiterated and re-emphasised his standard line that the scientific attitude, and indeed the rational attitude in general, requires that all assumptions be always open to criticism (and indeed requires a constantly questioning attitude; we must not only be open to criticism should it come along, but must constantly strive to ensure that good criticisms *do* come along). And yet he had, he stated, ‘always’ agreed with one aspect of Kuhn’s view – that ‘dogmatism’ has an important role to play:

I believe that science is essentially critical. . . . But I have always stressed the need for some dogmatism. The dogmatic scientist [this should surely be an oxymoron for Popper] has an important role to play. If we give in to criticism too easily, we never find out where the real power of our theories lies. (p. 55)

It is difficult to think of any passages that would support Popper’s claim that he had ‘always’ stressed the need for a whiff of dogmatism, but, much more importantly, it seems difficult to make sense of the position he now adopted. Are we supposed to believe that the success of the true critical scientist depends on the existence of others who are not properly scientific because they are dogmatic? Why this unnecessarily bipartite view? Since Popper now accepted that being somewhat dogmatic may help reveal the ‘true power’ of our theories, it seems that the right move for him would have been instead to agree that the idea of *effective* criticism is somewhat more nuanced than he earlier allowed (which, although he didn’t put it exactly that way, was essentially Kuhn’s line). Moreover, the dogmatism at issue presumably involves at least sometimes reacting to a negative test result for – a potential ‘falsification’ of – a theory by holding on to it, despite that result. But then what happened to Popper’s claim that this was the hallmark of pseudoscience or, at least, that such a move was always to be viewed negatively because it reduces, and perhaps even ‘destroys’, the scientific character of the theory? How can a move that reduces the scientific character of a theory at the same time perhaps reveal its ‘true power’?

A (LARGE) PART OF THE RESOLUTION – DUHEM'S ANALYSIS OF THEORY TESTING

Despite the major impact of the work, many of the points Kuhn made in *Structure* about the role of tests and especially 'anomalies' are in fact – and, of course, unrecognised by Kuhn himself – easy consequences of Duhem's analysis of the logic of theory testing in science. Despite the fact that he explicitly cited him on occasion, Popper never seems to have fully absorbed the simple lessons of Duhem's analysis. A sizeable portion of the debate between the two on the issues raised so far can be resolved simply by thinking through Duhem's points.⁵

Duhem remarked that the sort of claim that is usually taken as a 'single theory' in science – Newton's theory, Maxwell's theory, 'the' wave theory of light, for example – never has any empirical consequence 'in isolation' (or even when conjoined with other empirical statements taken as 'initial conditions'). Instead auxiliary assumptions are always needed. So, for example, Newton's theory (of mechanics plus universal gravitation) taken on its own has, of course, no testable implications about planetary positions – not even ones of a conditional kind such as that if Mars is at (x,y,z) at time t , then it will be at (x',y',z') at time t' . In order to draw such consequences, we need to make an assumption about the total force acting on the planet. This will, in turn, be based on assumptions about the number, masses, and positions of the sun and other planets in the solar system, together with a 'closure assumption' – to the effect that forces other than the gravitational interactions between Mars and the sun and other planets are negligible. The minimum testable unit in science always consists, then, of what might be termed a 'central' theory together with a (sometimes quite large, though of course finite) set of auxiliary assumptions. (This set often includes some 'idealising' assumptions such as the closure assumption just mentioned.)

Moreover, in some cases – such as 'the' wave theory of light, analysed at length by Duhem – the central theory itself naturally breaks down into a 'core' component (light consists of *some sort* of periodic disturbance transmitted through *some sort* of elastic medium) together with a whole series of more specific assumptions (associating particular kinds of monochromatic light with waves of specific wavelengths, specifying the precise properties of the elastic light-carrying medium, how those properties differ in the 'free' ether as opposed to the ether as constrained within transparent substances such as glass, and so on).

A trivial, but vital, result in metalogic says that if some conclusion C is validly derivable only from some finite set of premises $\{P_1, \dots, P_n\}$,

and if C is false, then all that follows is that *at least one* of P_1, \dots, P_n must be false. Duhem's analysis tells us that the full deductive structure of any test of some 'single' 'central' theory is at least as complicated as the following:

Central Theory (maybe \Leftrightarrow core claim and specific assumptions)

Auxiliaries

Therefore, testable consequence.

Two results follow straightforwardly concerning the points at issue between Popper and Kuhn. *Firstly*, contrary to Kuhn, scientists can, at least sometimes (the qualifications are spelled out in the next section), be regarded as involved in testing, and testing a chunk of theoretical material, not an individual scientist's capability. It is just that the unit being tested is not a single isolated theory but a sometimes quite complex theoretical system. This means, in turn, that a negative outcome may be of little significance since it seems overwhelmingly probable that it will be dealt with by changing some relatively secondary (and perhaps so far not very well thought-through) specific or auxiliary assumption. *Secondly* – it's really just the other side of the coin, but this time contrary to Popper – it becomes clear why a scientist may perfectly properly, without any hint of dogmatism, regard some negative result as a Kuhnian anomaly rather than a Popperian falsification. The falsity of the central theory does *not* follow from the falsity of the empirical conclusion. Moreover, even if it were decided that the central theory rather than some auxiliary was more likely to be at fault (remember: this decision cannot be based on logic alone, from what has already been said), it would still not follow that it was the core of the central theory that was false rather than some specific assumption. If a scientist is doing anything that resembles testing, then she is always – whether she is fully aware of it or not – testing a *theoretical system* rather than a single isolated theory. It follows that if the empirical consequence entailed by some initially accepted theoretical system turns out to be false, then it would be just as dogmatic to argue – in the way that Popper's rhetoric seems to endorse – that it must be the central theory or the core theory within the central theory that is false, as it would be to argue that the fault cannot be with the central theory but instead with some auxiliary. Similarly (responding to Popper's concession about the possibility of holding on to a theory despite a refutation) there is no reason to think that questioning a specific or an auxiliary assumption in the light of a refutation of a whole theoretical

system is *automatically* under any more suspicion from the point of view of good scientific practice than would be questioning the central theory.

Looked at in this way, the dispute about testing between Kuhn and Popper seems remarkably easy to resolve. As we shall see in the next section, there is in fact rather more to the dispute than my treatment so far has revealed. Before coming to this extra content, however, the Duhemian analysis that we already have on the table helps to clarify what was at issue in one point of apparent *agreement* between Kuhn and Popper that has played a significant role in the further debate and that we need to clarify.

Kuhn himself pointed out in his London paper that he and Popper agreed not just on the non-‘accretional’ nature of (some) scientific change, but also on the thesis that all so-called observation statements are ‘theory-laden’. One consequence of that now widely adopted thesis would seem to be that a further possible reaction for the scientist seeking to ‘hold on to’ a favoured central theory is opened up. Not only could such a scientist seek to replace some auxiliary in the Duhemian theoretical system necessary for the derivation of the observational or experimental ‘result’, she could also question and seek to replace that empirical result itself. (As Kuhn hinted, the fact that Popper elsewhere enthusiastically endorsed this point makes it still more mysterious why he should also claim that ‘rescuing’ a theory by challenging a theory-laden empirical result should always reduce the theory’s scientific status.)

I believe that, although of course directed at a real methodological phenomenon in science, the theory-ladenness thesis is *at best* a misleading way of representing it. There can, of course, be no doubt that every statement (at any rate every statement about the external world), no matter how ‘observational’, must count, in principle, as fallible; even claims like ‘the needle in this meter points to around the mark “5” on the scale’ presuppose, for example, that we are not being systematically deceived by a malign Cartesian demon. But Kuhn’s argument (or rather claim) that there are, in any genuine case of intratheoretic rivalry, no theory-*neutral* (notice: not theory-*free*) observation statements to act as arbiters between the rivals seems to me entirely unconvincing. What *is* true is that in order to get down to the level of such effectively incorrigible observation statements – ones whose truth value is agreed to on all sides and that can, therefore, pace Kuhn and *perhaps* pace Popper, act as neutral arbiters between rival theoretical systems – we need to augment those theoretical systems still further; and that further augmentation naturally makes the ‘Duhem problem’ (which of the many statements in such systems to ‘blame’ for a refutation) still more complex.

One incident that is sometimes cited as illustrating the significant theory-ladenness of observation statements was the dispute between Newton and Flamsteed (the first Astronomer-Royal). As told by Imre Lakatos, the story went roughly as follows.⁶ Newton sent some predictions about planetary positions made on the basis of his theory to Flamsteed and asked him to check their correctness. Flamsteed replied that Newton's predictions were incorrect. But Newton responded that in fact the predictions were correct and that it was Flamsteed's data that were in error. Told in this way, it sounds like a real case of Newton indeed being dogmatic in defence of his theory, and it seems to illustrate both the necessity for some dogmatism (since Newton was, we now believe, right) and the inevitable dependence of data on theory.

But neither in Lakatos's version nor in the real version was there ever any dispute between Newton and Flamsteed at the level of what Duhem (1906) called 'practical' (as opposed to 'theoretical') facts. Newton did not charge Flamsteed and his assistants with misobserving or misrecording the angles of inclination of their telescopes or the reading on their clocks when certain characteristic spots of light could be observed sitting at the centre of the visual field of those telescopes. (Even such assertions are obviously fallible in the trivial sense that a slip could have been made, or one of the assistants could have been drunk, and so on. But such mere slips can always be controlled for by independent checks.) Newton's real suggestion was that Flamsteed had 'miscalculated' his data – on the basis of an incorrect assumption about the amount by which light is refracted in the Earth's atmosphere and the dependence of the amount of refraction affecting the light entering a particular telescope on the air temperature in the locality of the telescope.

The best way to see what is going on is again through a Duhem-style analysis. Although assertions about planetary positions deductively follow from the – relatively slim – theoretical system discussed earlier (containing Newton's four laws and some auxiliary assumptions), nothing follows even from that theoretical system, let alone from Newton's four laws alone, about characteristic spots of light at the centre of visual fields of telescopes. In order to have a theoretical system that *is* testable at this very 'low level' of observability, we need further assumptions – ones that link real planetary positions to these telescopic phenomena. This link clearly requires the articulation of optical theories about the properties of telescopes, and it equally clearly requires an assumption about the amount of refraction that light undergoes in passing through the Earth's atmosphere. So, if we require our observation statements to be undisputed (I would suggest indisputable,

at least *via serious* considerations), then the full deductive structure of this observational test is

Central theory

Auxiliary theories

Instrumental theories

Hence, observational consequence (about angles of inclination of telescopes rather than planetary positions).

From this – altogether more revealing – viewpoint, Newton again treated Flamsteed’s results as anomalies: that is, he suggested some other part of the overall theoretical system (specifically, the assumption about atmospheric refraction from within the set of ‘instrumental theories’) as the primary target for replacement rather than his own central theory.

Despite occasional references to it, Popper seems never really to have taken Duhem’s point on board, and so, assisted by the fact that Kuhn did not express the challenge as clearly as he might have had he explicitly exploited Duhem’s analysis, Popper entirely misconstrued Kuhn’s challenge on the issue of tests. For his part, Kuhn failed to see that at least some of the Popperian testing rhetoric could readily be accommodated within his own view. Scientific tests *can* be analysed – in line with Popper’s general views – as the deduction of observation statements from a set of theoretical claims; if the test proves negative – that is, if the inferred statement is shown by experiment or observation to be false (and, unlike Popper, I think this latter process is essentially incorrigible if the observation statements are of sufficiently low level) – then the set of theoretical claims taken as a whole is falsified and needs to be replaced. Genuine tests are important, just as Popper claimed. However, the units of science that are tested in this way consist not of single scientific theories (these – again: Newton’s theory, Maxwell’s theory, ‘the’ wave theory of light, and so on – are, despite Popper’s rhetoric, unfalsifiable), and neither are they best seen, as Kuhn claimed, as tests of individual scientists rather than of any claims about the world. Instead, tests in science are of whole sets of statements organised in ‘theoretical systems’. The replacement theoretical system may – in principle – differ from the original in *any* of its parts – core, specific but still central, auxiliary, or instrumental. The only scientist who could reasonably be charged with dogmatism is one who refused to modify any part of her initial overall theoretical system – but, of course, no scientist would ever do this. In advance of consideration of *further* tests (a crucial consideration, as

we shall soon see), no particular type of reaction is under more suspicion from the epistemic point of view than any other, and none need be more dogmatic than any other. There is no prior reason why seeking to replace an auxiliary and retain the central theory should be judged any more dogmatic than the alternative strategy of retaining the auxiliaries and looking for a new central theory.

Recall that Kuhn specified two respects in which his own view differed from Popper's. The second of these was his 'discontent with the implications of the term "falsification"'. A major step in resolving this 'discontent' is again made once we accept that falsifications are of theoretical systems rather than central theories. Kuhn's anomalies are, then, at least in the simplest case, falsifications of overall theoretical systems that scientists regard – at any rate for the time being – as likely to be resolved by replacing that theoretical system with another one that shares the same central theory and differs only over some auxiliary or instrumental assumption. Most Newtonians in the nineteenth century regarded the observations of Uranus's orbit as anomalies for, rather than falsifications of, Newton's theory because they expected that the best replacement theoretical system that predicted the correct orbit for Uranus would also be built around Newton's theory and would differ from the current one 'only' over some auxiliary. This attitude was, of course, dramatically vindicated by Adams and Leverrier, who, 'holding on to' Newton's theory, replaced the auxiliary assumption about the number of other gravitational masses in the solar system and hence produced an overall system that not only correctly accounted for Uranus's orbit, but also predicted the existence of a new planet – Neptune. This success, in turn, made it more plausible to regard the difficulties with Mercury's orbit (known about, of course, long before Einstein) as similarly anomalous (rather than falsifying). It seemed likely that, by working within the basic Newtonian approach (that is, revising some auxiliary within the theoretical framework based on Newton's theory), a successful account of Mercury's motion could eventually be found.

WHAT KUHN ADDED TO DUHEM

Kuhn added at least two important points to anything that can be found at all explicitly in Duhem. *Firstly*, although it is sometimes reasonably clear what the 'best available' auxiliary assumptions are, so that we can, without too much rational reconstruction, see a particular scientist, or particular group of scientists, as testing a given, fairly clear, theoretical system built

around whatever central theory is at issue, in other circumstances – perhaps the majority of circumstances – such clearly preferred auxiliary assumptions are not to hand. The same point holds – perhaps still more importantly – in cases where the central theory breaks down into a core theory and a set of ‘specific assumptions’. Sometimes in such cases scientists who are working on the core theory may not know which specific assumptions are the best candidates for acceptance. What exact assumption should an eighteenth-century upholder of the corpuscular theory of light, for example, make about what differentiates the corpuscles that produce violet light from those that produce red light? No obvious answer was to hand. Such scientists are more naturally analysed not as *testing* any particular theoretical system at all (let alone, of course, as directly testing the central theory within that system), but rather as *working towards* the best candidate theoretical system based on that central theory. *Secondly* (and relatedly), the core idea behind a central theory will generally not only be an assertion about the universe, but will also be associated with a set of ideas (a ‘heuristic’) that can be used in working towards that best candidate theoretical system. Both of these additional features are connected with Kuhn’s insistence on the importance in science of ‘commitment to tradition’.

The first point is straightforward. Duhem showed that scientists only ever test complex theoretical systems built around core theories. It is by no means obvious, however, that there will always be natural candidates for ‘best available auxiliary theories’ within such a system. Indeed, it would be amazing if this were always the case. Where it is not, Kuhn is surely right that it is a stretch to speak of testing at all. Suppose that no value for the index of atmospheric refraction of light and its temperature dependence was taken as known in the late seventeenth century. It would, in that case, be foolish simply to make a Popperian bold conjecture about that index and test the resulting theoretical system based on Newton’s theory against astronomical data. Conjecturing would be almost bound to fail. Instead, given that we have independent reason to accept the central Newtonian theory (through its accounts of the precession of the equinoxes and other phenomena), it is clearly more reasonable to *premise* that central theory and address the question of which account of atmospheric refraction would, when added to that central theory (plus, of course, other accepted assumptions), produce an overall system that yields the observed results. The central theory is premised in order to use the observed phenomena to indicate an accredited value of this theoretical parameter.⁷

An especially clear-cut version of this sort of process had been described long before Kuhn – in fact by Isaac Newton – under the name

‘deduction from the phenomena’. Suppose, to take an especially clear-cut example, the general wave theory that light consists of waves transmitted through some mechanical medium is already accepted. That general theory itself, of course, specifies no particular wavelengths for light from particular monochromatic light sources (the latter *are* characterised by the general theory as those that emit light that undergoes no dispersion in refractive media). Such wavelengths – for example, of light from a sodium arc – are clearly *theoretical* parameters. Because the general theory does not specify the values of those parameters, it entails no precise values for the fringe distances in interference and diffraction experiments. In order to have such observational consequences, the general theory needs to be augmented by further specific assumptions about the wavelengths. Again, it would be absurd to make a bold Popperian conjecture at this point. Instead, a scientist will take the general theory as a premise and look for a consequence of it that identifies wavelengths of monochromatic light in general as some one-to-one function of measurable quantities, like fringe distances and slit separations. Thus, for example, the general wave theory, together with some approximating assumptions, entails the following functional relationship between, on the one hand, the wavelength λ and, on the other, the slit separation d , the distance from the two-slit screen to the observational screen D , and the distance X between the central bright fringe and the first bright fringe on either side of the centre in Young’s famous two-slit experiment:

$$X = \frac{D\lambda}{d}$$

Hence since d , D (‘initial conditions’), and X (‘experimental outcome’) can all be determined experimentally, the scientist can deduce a value for the theoretical parameter λ from the phenomena. This is, of course (and as always), really deduction from the phenomena *plus background knowledge* (here principally the general wave theory of light).⁸

Other cases in which background knowledge informs further scientific developments are rather less sharply delineated but are none the less important (and can, I believe, always be analysed in more clear-cut terms than Kuhn manages). Consider again the ‘classical’ wave theory of light. Fresnel produced, in 1819, a wave theory that accounted satisfactorily for a range of diffraction and interference results. Since the luminiferous aether, whatever its precise constitution, had to allow the planets to pass through it with negligible frictional effects (Newton’s theory already very successfully accounted for planetary motions purely on the basis of gravity), Fresnel

took it that the aether is in fact a highly attenuated fluid and that the waves of light are, correspondingly, longitudinal pressure waves. (A longitudinal wave – the only kind that a fluid can transmit – is one in which the particles whose motions constitute the wave oscillate in the same direction as the overall transmission of the disturbance. An example is provided by sound waves in air.) Polarisation effects, known about at least since Huygens, remained an outstanding problem. Naturally, since the general wave theory had been impressively successful when augmented to supply a precise theory of diffraction, Fresnel and others ‘premised’ that same general wave theory in attempting to build an account of polarised light (and of crystal optics more generally). Fresnel quickly ran into a major problem, however. As he and his friend Arago discovered, if the famous two-slit experiment is performed in such a way that the light coming through the two slits is polarised in mutually orthogonal planes (by interposing suitably oriented quartz plates), then the interference fringes disappear. And yet, if the waves were longitudinal, then near the centre of the pattern where the beams of light coming from the two slits are nearly parallel, the general theory dictates that there *must be* interference; and in particular, when the beams from the two slits travel distances that differ by a half-wavelength, there should be destructive interference and hence a dark band. Yet no such bands are observed in Fresnel and Arago’s experiment. Again, background knowledge saved Fresnel from a theoretical limbo. It specified another type of wave, a transverse wave (in which the particles of the waving medium oscillate at right angles to the overall transmission of the disturbance). If the two beams from the two slits in his modified experiment were transverse waves and, being orthogonal to one another, hence oscillated in orthogonal planes, then no destructive interference would be expected. As always, taking the general wave theory as given, Fresnel inferred that since the experiment showed that the waves could not be longitudinal, they are transverse, and he began to work on the problems that this assumption produces. Hence Fresnel *deduced*, rather than conjectured, the elastic solid theory of the luminiferous aether.⁹

Notice, however, that the sort of heuristic guidance exemplified in these cases is available only once science has become sufficiently mature to possess background knowledge of this powerful kind. In particular, this sort of heuristic guidance is available only once science possesses a general framework theory sufficiently well supported and sufficiently powerful to guide work in this way. This was surely the chief phenomenon that Kuhn was attempting to highlight using his notion of normal science. Although what is involved can at least in some cases be described much more sharply than

Kuhn managed (as I hope the preceding brief analyses show), there is surely no doubt that the phenomenon he was pointing to is of exceptional importance and had hitherto been ‘analysed’ by philosophers of science at best in a hand-waving way.

Popper seems to have been as good as blind to this important phenomenon. Of course, no one who thought about science as much as Popper did could be *totally* blind to it and – as he reminded Kuhn in his London paper – he had written in his *Logic of Scientific Discovery*:

A scientist engaged in a piece of research, say in physics, can attack his problem straight away. He can go at once to the heart of the matter, that is, to the heart of an organized structure. For a structure of scientific doctrines is already in existence; and with it a generally accepted problem-situation. (Popper 1958, p. 13)

There are also a couple of other passing remarks in Popper’s work about the importance of background knowledge and of a scientist’s being ‘immersed in a problem-situation’. But he seems to have done nothing towards developing this outline idea into a systematic account of the precise ways in which background knowledge can inform the further development of science. (Indeed, his well-known insistence that, while there is a logic of the appraisal of already-articulated theories, there can be, despite the English title of his best-known book, no such thing as the logic of scientific *discovery*, a ‘logic’ of how good theories get to be articulated in the first place, seems to indicate that he sometimes thought that no such development is possible.) And, of course, it follows that Popper never gave systematic thought to how such an account would affect his claims about falsification and refutation.¹⁰

On the other hand, Kuhn’s account of the puzzle-solving tradition that comes as the benefit of buying into a paradigm, and his insistence on the importance of exemplars, were both attempts to put some flesh on this outline idea of mature science ‘building on itself’.

In sum, then, Kuhn, contrary to Popper’s interpretation (and that of others such as Feyerabend), should be seen *not* as advocating dogmatism, but rather as advertising the fact that ‘commitment’ to the sort of framework supplied by well-developed science brings enormous epistemic benefits; without such commitments, mature science would be incapable of making the progress it has in fact made. Popper’s claim that normal science is ‘a danger to [real] science and indeed to our civilisation’ betrayed complete misunderstanding.

On the other hand, surely some of Kuhn’s claims gave Popper legitimate cause for concern. Kuhn did often seem to advocate a view altogether

stronger than the one I have just articulated and endorsed: that the commitments involved in adopting a paradigm are absolute, brooking no question; that it is in fact impossible for a scientist, no matter how hard she might try, to stand outside of her framework so that she can articulate or recognise those commitments; and clearly, if the commitments cannot be recognised, it follows that they cannot be questioned. Nothing in the preceding analysis endorses this extension of the view. It may be a psychological necessity for some scientists, in order to get themselves to put in the enormous effort necessary to develop specific theories within an accepted framework, to believe unquestioningly – at least *pro tem* – in the truth of the general principles that constitute the framework. But if so, this is indeed a purely psychological phenomenon and need not, and should not, be endorsed by any normative account of how science ought to proceed. And there are, after all, clear-cut examples of distinguished scientists who made contributions to theories in whose basic tenets they did not believe: Maxwell and the statistical-kinetic theory and Einstein and the quantum theory are two examples that spring immediately to mind. Although it may not sit very well with certain types of mind set, there seems to be no logical reason at all why it should be impossible for a scientist to ascend to the metalevel and stand outside her theories and perhaps have the view that, whatever may be their ultimate fate, they are the most interesting theories around, so that developing them will constitute a genuine contribution to science (if only perhaps by showing in which respects they need to be replaced).

Kuhn's – apparent – claims about the 'paradigm dependence of everything', the inability of a scientist to be able to step outside a paradigm and take part in a critical debate about its epistemic virtues and failings, of course achieve their sharpest focus and highest importance when it comes to the issue of theory – or paradigm – *change*, that is, when it comes to scientific 'revolutions'.

THE RATIONALITY OF 'REVOLUTIONARY' THEORY CHANGE IN SCIENCE

Whatever Popper's particular claims and occasional oversights, what really drove his resistance to Kuhn's account was surely what he, and many other commentators, perceived as a threat to the objectivity of science and the rationality of scientific progress. The simple account of scientific revolutions that Popper sometimes seemed to endorse – involving outright refutations of the older theory – may not supply the necessary rationale, but one should

not, of course, jump from the failure of that particular simple account to the conclusion that there is *no sort* of rationale for (revolutionary) theory change. Yet that is exactly the conclusion that many people saw Kuhn as espousing: successive theories are not comparable but instead ‘incommensurable’; the switch to the newer paradigm is a ‘conversion experience’ rather than a process governed by general rules of theory superiority; ‘hold-outs’ for older paradigms who do not accept the superiority of the revolutionary new paradigm are ‘neither illogical nor unscientific’. Popper, like many others, saw Kuhn as committed to historical relativism, to the claim that critical discussion always presupposes a framework, and therefore to the view that those who operate within different frameworks (support different paradigms) are incapable of fruitful critical interaction (the famous ‘dialogue of the deaf’). Hence Popper’s charge that Kuhn had succumbed to ‘the myth of the framework’; and hence also the charge of many other philosophers of science – Scheffler and Shapere included – that Kuhn had given up on any idea of objective *progress* in science.¹¹ (At any rate, he had given up on the idea of any progress through revolutionary change of paradigm, as opposed to ‘progress as judged within, and by the standards of, a paradigm’.) The charge was put in its bluntest form by Imre Lakatos, who suggested that Kuhn had reduced radical theory change in science to a matter of ‘mob psychology’. Elaborating on the point by contrasting what he took to be Kuhn’s views with Popper’s, Lakatos wrote (p. 93):

For Popper scientific change is rational or at least rationally reconstructible and falls within the realm of the *logic of discovery*. For Kuhn scientific change – from one ‘paradigm’ to another – is a mystical conversion that cannot be governed by rules of reason and which falls within the realm of the (*social*) *psychology of discovery*. Scientific change is a kind of religious change.¹²

Did Kuhn really hold the views he is here charged with? Are whatever views he did in fact hold well supported by argument and historical evidence? And do those views indeed challenge the idea that the progress of science has been – at bottom – a ‘rational’ affair? Did Kuhn win *this* – crucial – part of the argument against Popper (and others)?

Radical sociologists of science influenced by Kuhn seemed to take it that the answer to all four of these questions is ‘yes’; and therefore that Kuhn had opened the way to a ‘symmetrical’, naturalistic explanation of theory choice in science purely in terms of social and psychological factors – an explanation that eschewed any talk of the ‘correct’ rational choice underwritten by some logic of evidence. On the other hand, ‘rationalists’ about scientific progress, like Shapere and Lakatos, seemed to take it that Kuhn really did hold the

views at issue, that those views really do challenge the idea of scientific change as a rational process, but that they are not in fact convincingly argued and hence that there is no need to reject the older view to which Popper (amongst others) was committed. Matters are not as straightforward as either side imagined.

Kuhn was always insistent that the ‘mob psychology’ gibe was grotesquely misplaced. And there are indeed passages in his London paper, and more especially in his ‘Replies to Critics’, that seem to put him quite clearly on the side of those philosophers who took themselves to be his opponents and against those sociologists who took themselves to be drawing and endorsing the ‘antirationalist’ conclusions of his own analysis. He expressed his belief, for example, that science ‘is our surest example of sound knowledge’ (p. 20). Again, while accepting that his own account of the development of science shares a good deal with that of Feyerabend, Kuhn added that describing that account (as Feyerabend, of course, did) ‘as a defence of irrationality in science seems to me not only absurd but vaguely obscene’ (p. 264). And, more extensively and more strikingly, he took the following ‘evolutionary’ account of scientific knowledge to be very much part of his overall view:

Imagine . . . an evolutionary tree representing the development of the scientific specialities from their common origin in, say, primitive natural philosophy. Imagine . . . a line drawn up that tree . . . to the tip of some limb without doubling back on itself. Any two theories found along this line are related to each other by descent. . . . [C]onsider two such theories each chosen from a point not too near its origin [i.e., after the science concerned has achieved ‘maturity’]. I believe it would be easy to design a set of criteria – including maximum accuracy of predictions, degree of specialization, number (but not scope) of concrete problem-solutions – which would enable any observer involved with neither theory to tell which was the older, which the descendant. For me, therefore, scientific development is, like biological evolution, unidirectional and irreversible. One scientific theory is not as good as another for doing what scientists normally do. (p. 264)¹³

Except that he described as ‘easy’ the central task that ‘traditional’ philosophers of science have been working on for years and have still far from unambiguously achieved – that of articulating the criteria for one theory to be scientifically superior to another in the light of the evidence – and except perhaps for the striking qualification, to which we shall need to return, that the outside observer judging the two theories must not be ‘involved’ with either theory, Kuhn in this passage seems to have conceded to his

philosophical ‘opponents’ such as Popper and Lakatos all that they could want. Kuhn here acknowledged that there has been genuine progress in science, not simply mere change; later theories (at least in the mature sciences) are objectively superior to their predecessors. So what could all the fuss have been about?

One issue is, of course, whether the ‘pro-objectivity’ sentiments that Kuhn expresses here are really consistent with the main thrust of the position developed in *Structure*. A number of questions arise. How can the unambiguous assertion that theory change has been from good to better theories – better according to the sorts of criteria that philosophers have standardly endorsed – be consistent with claims that successive theories (or theoretical systems or paradigms) are incommensurable? How can that assertion be consistent with the famous claims about theory change, so far as an individual scientist is concerned, being a ‘conversion experience’? It would seem possible, according to the view just quoted from Kuhn, simply to show such a scientist that the new theory was better than the one he currently held based on the criteria at issue. Again, how is the view just quoted consistent with the famous claim that ‘hold-outs’ – scientists who continue to endorse the older paradigm in what turns out to be a revolution – cannot be judged ‘either illogical or unscientific’? Given that Lavoisier’s oxygen theory lies closer to the top of the scientific-evolutionary tree than the phlogiston theory, doesn’t it follow that, on the contrary, Priestley, in ‘holding out’ for the phlogiston theory, *was* unscientific, at least in the sense of continuing to somehow prefer a theory that was objectively inferior to an available rival?

Perhaps not all of these mysteries can be solved, but some of them can be if we go slowly concerning the difficult issues they raise. Let’s first return to the question of Kuhn’s account of scientists’ reactions to anomalies. We saw earlier that Kuhn’s disagreement with Popper over the impact on theories of negative experimental results is significantly clarified by recognising – with acknowledgments to Duhem – that the minimum unit of theoretical claims that can come into direct logical conflict with observation statements is not a single ‘isolated’ theory (such as Newton’s theory or Maxwell’s theory) but rather a theoretical system, built around such a theory but also involving a range of auxiliary assumptions. It follows that no such isolated theory is ever directly, logically refuted. Kuhn’s claim that scientists standardly do treat apparently negative evidence as anomalies rather than refutations, and that there is nothing ‘illegitimate’ in their so doing, is then, underwritten – at least to the extent that it is indeed always possible, so far as purely logical constraints are concerned, to hold on to the central theory and regard any

negative evidence, any anomaly, as requiring some change in the auxiliary assumptions. However, although Popper's blanket assumption that any such move (any such 'immunising stratagem', as he called it elsewhere) is automatically under scientific suspicion was misjudged, it is easy *both* to see what motivated Popper here *and* to empathise with that motivation.

Kuhn stressed in *Structure* that what sustains hold-outs to revolutions is their conviction that the evidence of their revolutionary opponents could be 'shoved into the box' provided by their preferred (older) paradigm.¹⁴ As a statement of mere deductive logical possibility, the claim that Kuhn makes on behalf of his hold-out is definitively underwritten by Duhem's analysis. However, a distinction in terms of scientific value between two quite separate types of case of 'shoving' erstwhile negative evidence into the paradigm's 'box' surely cries out for articulation.

Suppose, contrary to historical fact but for the sake of a simple illustration, that Priestley, in the face of the experimental result that burning mercury in a certain way produces a substance heavier than the original mercury, had held on to the phlogiston theory (whose core assumption was that, whenever anything burned, a substance, namely phlogiston, was given off) by assuming that phlogiston has 'negative weight'. (Hence, removing phlogiston from a substance increases its weight.) Contrast that with the case in which Newtonians insisted that the apparently negative observational results concerning Uranus's orbit can be shoved into the Newtonian box; and Adams and Leverrier postulated a hitherto undiscovered planet whose gravitational interaction with Uranus explained the initial apparent anomaly. Although both instantiate the Kuhnian 'holding on to an existing paradigm' scheme, the first seems purely defensive, while the second was regarded (surely correctly) as one of Newtonian theory's most impressive successes.

The difference between the two is not far to seek. The first was indeed *purely* defensive, ad hoc in the pejorative sense: at best, the move reconciled the preexisting framework with the initially negative-seeming evidence. As such, it stands on a par with the 'reconciliation' with the fossil record of the fundamental creationist claim that God created the world in 4004 B.C. with essentially the same 'kinds' as presently inhabit it. That record *apparently* attests to the existence of very many now-extinct species, but reconciliation can easily be achieved by postulating that God happened to choose to paint pretty pictures in the rocks that *look like* the imprints of the skeletons of animals from extinct species and to mix in with the desert sands some *bonelike* structures. In the Adams and Leverrier case, too, mere reconciliation is fairly cheap. It is always possible to produce a total force function that will

account for any observed motion of Uranus, and it may be possible to work back to what assumptions about an extra massive body in the solar system will, in concert with the effects of already known planets, produce that total force. However, there is a crucial difference: in all cases the initially negative experimental result is accommodated, but in the Adams–Leverrier case, quite unlike the phlogiston and creationist ones, the new assumption leads to independent tests. If there is an extra planet in the universe and if its mass and motion are such as to account for the initially anomalous motion of Uranus, then we ought to be able to observe that planet. And indeed, so it was (roughly speaking) that Neptune was discovered.

What distinguishes the scientifically impressive cases from those that are ‘mere accommodations’ is independent testability and independent empirical success. The new version of the theory, or rather the new theoretical system based on the same central theory, not only accommodates the initial anomaly, it also successfully predicts some new fact. This is exactly the distinction between progressive and degenerating research programmes that lies at the heart of Lakatos’s attempted synthesis of Popper and Kuhn: his ‘methodology of scientific research programmes’. Although the point is already essentially in Duhem, it is not one that Kuhn acknowledged (at any rate in any clear way) in *Structure*.

Lakatos accepted that not every move in response to erstwhile anomalies would be met with success even within programmes (or paradigms) that are scientifically in good shape. A well-known example concerns stellar parallax and the Copernican theory. If, as Copernican theory centrally postulates, we are on a moving observatory, the Earth, then two ‘fixed’ stars ought to appear to us at least a little closer together at certain times (when we are relatively far from them) than they do at others (when we are relatively close). Hence Copernican theory predicts stellar parallax: the apparently relative motion of any one fixed star relative to any other close to it. On the other hand, of course, Ptolemaic theory, since it postulates a stationary Earth, predicts no such motion. At the time, and indeed well into the nineteenth century, no stellar parallax was observed. The response of Copernicans was essentially that there must indeed be such apparent parallactic motions, and that the explanation of the failure to observe them must be that they are so small (since the radius of the Earth’s orbit is so small compared to the distance between the sun and even the nearest star) that they were invisible even to the best available telescopic observations. The new theoretical system does make a prediction that is, at least in principle, independently testable: that increasingly accurate telescopes will eventually reveal stellar parallax. But clearly in this case there was no question of an immediate

independent *success*. Hence Lakatos characterised a research programme as progressive if its successive versions (some, though not all, produced in response to negative evidence for their predecessor) are (i) consistently independently testable (they make testable predictions over and beyond those of the previous version about phenomena other than those that refuted the predecessor system) and (ii) at least now and then (and preferably often) are independently confirmed – that is, are successful in those independent tests. Otherwise, and particularly when successive versions do no more than accommodate what had been anomalies for their predecessors, the programme is degenerating.

According to Lakatos, progressive programmes are objectively scientifically superior to degenerating rivals. His characterisation of progress and degeneration is what inserts ‘hard objective elements’ into Kuhn’s account. It revises in a radical way the view expressed by Popper in clause 7 of his account of tests. Reacting to a negative result by modifying the theory (really creating a new theoretical system with the same central, or at any rate core, theory) need not in general ‘destroy or at least reduce’ the scientific character of the (central) theory. Instead such a reaction actually *increases* its scientific value, and hence the value of the research programme that it underpins, if the modification is independently testable and independently confirmed, and decreases its scientific value only if the reaction is purely ad hoc, that is, merely accommodatory, with no independent testability.

Lakatos’s claim was, of course, that scientific revolutions invariably consist of the – at least eventual – replacement of a degenerating research programme by a progressive one based on a rival central theory. This is what explains the development of science as a ‘rational’ process. By the early nineteenth century, the programme based on the particulate theory of light (that light consists of tiny material particles affected by various forces) had a long history of consistent degeneration; Fresnel produced a rival programme (or rather significantly developed an existing programme) based on the idea that light consists of periodic disturbances transmitted through an all-pervading elastic medium and made that programme impressively progressive. For example, in response to the initial difficulty produced by the observation that the interference fringes disappear when the two-slit experiment is performed with orthogonally polarised beams of light coming through the two slits, Fresnel shifted to a new theoretical system (involving transverse rather than longitudinal waves) that made exciting new predictions about crystal optics, and these predictions were empirically confirmed. This is why the revolution was rational.

Any Kuhnian hold-out to this revolution would have been trivially correct – courtesy again of Duhem – in claiming that the successful empirical

results pointed to by the wave revolutionaries could, *somehow or other*, be shoved into the corpuscular box (some scientists were tempted to explain interference fringes, for example, as physiological phenomena caused by two streams of light particles hitting the eye in such a way as to create interference at the retina); but they would have been quite wrong – as Kuhn at least in *Structure* failed to recognise – if they believed that such shoving automatically balances the evidential scales. A programme gets more scientific brownie points, higher confirmation, from data that it predicts than it does from data that it merely accommodates: if the wave theory predicts the interference fringes, then a hold-out would be quite wrong to think that producing an ad hoc not-further-testable accommodation of the fringes by invoking physiology, for example, automatically balances the evidential scales.¹⁵

Although Kuhn did not, either in the original London address or in his ‘Replies to Critics’, explicitly accept this point, he *did* express agreement with at least the broad outlines of Lakatos’s ‘often admirable’ paper. And he seems quite explicitly to have held that the difference between their basic views is little more than terminological: ‘Though [Lakatos’s] terminology is different, his analytic apparatus is as close to mine as need be: hard core, work in the protective belt, and degenerative phase are close parallels for my paradigms, normal science and crisis’ (p. 256).

This brief passage hides significant concessions.¹⁶ In particular, if Kuhn accepted that his ‘analytic apparatus’ is essentially the same as Lakatos’s, then he seems now to stand committed to an altogether more objective view of ‘crisis’ than most commentators had believed. It is not just a sociological fact that a scientific community is suddenly gripped by a feeling of crisis involving a loss of confidence in the ability of the paradigm to deal with the anomalies it faces, nor is it an internal paradigm-dependent matter whether a particular anomaly has been properly, scientifically resolved. Whatever the paradigm, the rules – at least at the abstract, general level – for what counts as an adequate resolution of an anomaly are always the same: the theoretical framework within the paradigm that resolves the anomaly should count as a ‘progressive shift’; the resolution, in other words, should not just resolve the anomaly, it should also produce independently testable predictions, some of which are confirmed. A crisis for a paradigm again seems to have a cross-paradigm characterisation: a paradigm is in crisis if it hits a consistently degenerating phase in Lakatos’s sense.

So Kuhn made two concessions to what we might term the ‘objectivists’: the ‘progress concession’ (the evolutionary tree) and the ‘same as Lakatos concession’. Whether he ever seriously thought through the question of how far these concessions cohere with the main body of the views he expressed in *Structure* is unclear to me. Indeed, it is not even clear if he thought

through the question of whether the two different concessions – for all that they undoubtedly point in the same direction – are themselves fully coherent. Consider again Kuhn’s list of objective factors that in combination will invariably distinguish the newer from the older theory on the evolutionary tree of scientific knowledge. The only one that might be thought to be Kuhn’s version of the crucial Lakatosian criterion of *independent* predictive success is ‘maximum accuracy of predictions’; and there Kuhn in fact seems to have been using ‘prediction’ just in the sense of empirical consequence and hence referring simply to the empirical adequacy of the theory. (Lakatos’s problem, following Duhem, was, of course, the ever-present possibility of producing specific theories based on different cores that are equally adequate empirically in the straightforward sense of entailing all the same empirical consequences and yet that, intuitively speaking, do not at all stand on a par with respect to the evidence.) The fact probably is, I suggest, that Kuhn had little interest in what he thought of as a relatively trivial issue; it was clear to him that later theories in the mature sciences are in objective ways superior to earlier ones. He was willing to concede entirely to the philosophers that there are objective cross-paradigm standards for when one theory is scientifically superior to another, and was happy to leave it to them to take their best shot at the – ‘relatively easy’ – task of articulating the details of those standards. He himself was interested in the question of ‘theory choice’ in some other, and for him more challenging, sense.

What exactly was this sense, and what exactly were Kuhn’s claims about it? As preliminaries to tackling this question, two issues require investigation. The first is Kuhn’s reaction in his London ‘Replies’ to Popper’s charge that he was guilty of historical relativism. Kuhn insisted that there are two senses in which he might be accused of relativism: in the first sense he is no relativist, and although he is guilty of relativism in the second sense, this is not a charge that anyone should worry about. Relativism of the first kind denies that science has made progress according to cross-paradigm criteria, and his remarks about the evolutionary tree are his explicit denial of guilt on that charge. What is the second sense of historical relativism? Kuhn explained:

[T]here is another step . . . which many philosophers of science wish to take and which I refuse. They wish, that is, to compare theories as representations of nature, as statements about ‘what is really out there’. Granting that neither theory of a historical pair is true, they nonetheless seek a sense in which the later is a better approximation to the truth. I believe nothing of that sort can be found. On the other hand, I no longer feel that anything

is lost, least of all the ability to explain scientific progress, by taking this position. (pp. 264–5)

In other words, Kuhn explicitly rejected any form of scientific realism but insisted that this did not imply the rejection of the thesis that science has made progress according to objective criteria. His argument against scientific realism was simply that he found it impossible to see in actual cases of successive theory changes from the history of science anything like a consistent movement towards greater ‘approximate truth’.

Whether or not the argument is convincing, his view that the realism issue and the rationality/progress issue can be treated separately surely ought to have been uncontroversial. Suppose that philosophers of science had succeeded in producing the correct ‘inductive logic’ (in the broadest sense) – the rules, common across the whole scientific endeavour, for how evidence relates to theories and for how, in some instances at least, the evidence may establish a preference for one theory over its rivals. And suppose that philosophers had shown that the actual progress of science could be fully explained according to these rules; each change of theory in the history of the mature sciences constituted a move to a theory that was (at least eventually – see later) better ‘supported’ by the evidence than its predecessor. They would then have shown that there is one set of rules (at least at the abstract level) that characterises the whole ‘game of science’.

The further question could still be raised of what *justifies* those rules: why play *that* game? Why prefer theories that are better supported by the evidence rather than, say, theories that show greater consistency with holy writ? One obvious (attempted) justification – no doubt the first we would think of – is the ‘realist’ one that playing the scientific game will (or, more plausibly, is more likely to) lead towards the truth. But one can clearly reject that answer, and perhaps substitute another, without at the same time questioning that the ‘right’ rules have been identified. An instrumentalist or another kind of antirealist, who denies that scientific theories are true or approximately true, can still hold that the way scientific theories are judged on the basis of the evidence is an objective matter, satisfying very general rules that remain the same throughout science (usually, in the case of instrumentalists, rules to do centrally with empirical adequacy and simplicity). Such an antirealist would continue to hold that there has been progress in science towards better and better theories, and would simply deny that ‘better and better’ here means ‘truer and truer’.¹⁷

This is certainly not a point on which Kuhn was in conflict with either Popper or Lakatos. Popper encouraged the *conjecture* that successive

theories accepted in science, each of which is (allegedly) an improvement over its predecessor according to the criteria he favoured, also have monotonically increasing ‘verisimilitude’ – but this was clearly an ‘optional extra’, not something that is inherent in rationality on his view.¹⁸ And similarly, Lakatos talked explicitly and often about *linking* what, using Popperian terms, he took to be judgments of corroboration, on the one hand, and judgments of verisimilitude, on the other. Science makes progress, scientific theory change is rational, because successive theories have greater corroboration. The link to scientific realism, via verisimilitude – that is, to the issue of whether, by preferring better corroborated theories, we are being taken closer and closer to the truth – is an independent, and philosophically challenging, matter.¹⁹

The second issue over which Kuhn still thinks of himself as in some conflict with ‘the’ philosophers despite the progress concession is aimed more directly at the rationality issue. Kuhn’s remarks in his London ‘Replies to Critics’ about theory choice presage those in Chapter 13 of his *Essential Tension* (1977). Conceding that there are indeed ‘objective factors’ (simplicity, empirical scope, and the like) that undoubtedly play an important role in theory, choice and conceding that these factors may all *eventually* point in the same direction and thus declare that the same one of two rival theories is superior, Kuhn none the less insisted that *at the time that the debate between the two theories was a live one in the history of science*, it is generally the case (i) that the objective factors are not univocal – some will favour one theory, while others favour its rival – and (ii) that different scientists may – legitimately – differ in their judgments as to which of the two theories is favoured, even with respect to a single objective factor. As Kuhn himself put it in his London ‘Replies’ concerning point (i): ‘[I]n many concrete situations, different [epistemic] values, though all constitutive of good reasons, dictate different conclusions, different [theory] choices’ (p. 262).

And he suggested, as an illustration, that ‘one theory [may be] simpler, but the other . . . more accurate’ (ibid.) Concerning point (ii), he wrote:

More important, though scientists share these values and must continue to do so if science is to survive, they do not all apply them in the same way. Simplicity, scope, fruitfulness, and even accuracy can be judged quite differently (which is not to say that they can be judged arbitrarily) by different people. (Ibid.)

The objectivist should surely have no problem with point (i): it may well be – in fact, it would be amazing were it not the case – that a clear judgment about which of two theories is objectively superior emerges only

after a protracted period of rivalry and development. Once we have the picture, not of complete theories springing in final form out of the heads of their creators, but rather of developing paradigms or research programmes (together, of course, with a developing evidential basis), this comes as little surprise. Moreover, the sensible objectivist will have some way of combining the various criteria, and so having two criteria point in opposite directions need not prevent her from pronouncing one theory superior to the other in an overall sense. (Indeed, for the ultrasensible objectivist, the criterion of independent empirical success is dominant.)

Point (ii) raises more difficulties. Indeed, it is not at all easy to see how to reconcile Kuhn's claim here with the progress concession quoted earlier. The image of the evolutionary tree involves an observer who stands back from the scientific process and is always capable of making seemingly definitive judgments about the overall scientific merit of competing theories (given, of course, the evidence that has accumulated up to a certain point). This seems to require clear-cut criteria, and yet now we are told that these objective factors operate instead as 'values' that, without being arbitrary, may none the less be 'judged quite differently . . . by different people'. This presumably has something to do with Kuhn's qualifying remark that the observer who judges which is the later (and better) of two theories must be 'involved' with neither; what exactly this is, however, is not clear. Adding to the confusion is Kuhn's continued insistence on the existence of incommensurability. Admittedly, Kuhn suggested in his 'Replies' that he had only ever regarded this as an 'obstacle' to adequate communication across a paradigm divide rather than as something that showed that such communication is impossible.²⁰ But it is not clear how the progress concession can be consistent with *any* claim of incommensurability.

I am more than happy to leave it to others to decide what, if anything, Kuhn really meant by this total package of remarks about progress, theory choice, incommensurability, and the rest. Here is my best shot concerning what he *may* have been getting at – a view that, although inevitably revisionary to some extent, is (i) consistent with *some* of the things he wrote, (ii) reasonably interesting, and (iii) arguably true.

It is a seductive idea that philosophers of science should be centrally concerned with explaining the attitudes taken towards rival theories by particular scientists. Was Priestley's choice to continue to favour the phlogiston theory irrational, while, say, Einstein's choice to abandon classical physics was rational? Were Kepler and Galileo rational in choosing to develop Copernican theory, while those who continued to espouse some sort of Aristotelian–Ptolemaic view were irrational? After all, it might be thought,

a scientific revolution consists at root of scientists making the decision to choose some newer theory in preference to a previously established one, and how could such a revolution be explained as rational except by exhibiting the choices made by the individual scientists, or by a large majority of them, as rational?

However, the primary concern of philosophy of science is surely not with the decisions of scientists at all but rather with the relationships between theory and evidence, and in particular with judgments about the strength of support provided by various pieces of evidence for particular rival theories. These judgments concern the abstraction that might be called the 'intellectual state of the debate at a given time'; they are logical judgments in a broad sense and make no reference to individuals at all.²¹ There is then the *further* issue of how such (inductive) logical judgments – the result of the two-slit experiment strongly favours the wave theory of light compared to its rivals, the fossil record is strong evidence for Darwinian theory despite the fact that creationists can accommodate that record by writing it into God's creation, and so on – relate to the decisions and preferences of individual scientists. It ought always to have been clear that this issue is a complex and difficult one. It is blindingly obvious – at least once the issue is addressed head on – that nothing as simple as 'the rational person chooses the evidentially best-supported theory' will work.

If choosing a theory involves choosing to work on it (or advocating that others work on it), then, as has often been recognised, such a link would automatically declare the great revolutionary scientists irrational. After all, these are the innovators who choose to work on some theory *before* it is 'the best available in the light of the evidence', and indeed through whose work that theory assumes that mantle. Suppose we could, for example, explain Kepler and Galileo as having made rational choices to adopt the Copernican theory in preference to the Ptolemaic or Tychoonic theory because the evidence available to them favoured the former. Even so, we clearly could not produce such an explanation in the case of Copernicus himself. No doubt in this case, as in all others, there was a preexisting reason to object to the prevailing theory – here the Ptolemaic one – but the latter was, of course, none the less the best-supported theory available to Copernicus at the time that he started to work on his own theory. It was only through the latter's efforts that the evidential tables *began* to be turned.

On the other hand, if choosing a theory means regarding it as true or as established by the evidence (and there is no doubt that some scientists have chosen theories in this sense), then it is not at all clear that such choices ought to be sanctioned by any adequate normative account

of the relationship between theory and evidence in science. It has been clear at least since the time of Hume (and in fact since the time of the ancient Greeks) that no amount of evidence ever deductively entails a general scientific theory. But it is not immediately obvious that we need to take seriously the mere possibility that a theory *might* turn out to be false, no matter how well established it might appear in view of the evidence accumulated at a particular time. That possibility might have been just a philosopher's fancy. The history of radical theory change in science – highlighted above all by Kuhn himself and earlier by Popper – shows that the possibility cannot be dismissed in this way. No theory seemed better established than Newton's theory of motion plus universal gravitation – to the extent that eighteenth- and nineteenth-century scientists were wont to lament that there was only one truth about the universe, that Newton had discovered it, and that all that was left to them was to fill in a few details and footnotes. Yet the Einsteinian revolution, while showing that Newton's theory is indeed a highly adequate empirical approximation in the case of relatively slow-moving bodies, also showed that the whole framework on which it is based – involving absolute space, absolute simultaneity, and action at a distance – is totally false. It seems that a scientist had better choose no theory at all, if choosing it implies believing it to be true.

Kuhn's notion of 'theory choice', employed both here in his London 'Replies' and in Chapter 13 of his *Essential Tension*, clearly requires clarification.²² One clear-cut way in which a scientist might choose a theory is by choosing to try to develop it (or, speaking our Sunday, or Duhemian, best, choosing to try to replace the currently best available theoretical system built around the theory with a still better one based on the same core). As we saw, there can be no rule always to choose to work on the core theory that presently gets most support from the evidence, and in fact, a scientist clearly might choose to work on a theory for a variety of reasons that have no uniform relationship to her (degree of) belief in the theory or in its current epistemic virtues. (So, for example, Newton worked on Descartes's vortex theory, which already looked highly problematic and had little empirical support, in order to show once and for all that it was hopeless. Einstein contributed significantly to the quantum theory, through his account of the photoelectric effect, while famously rejecting the idea that quantum theory could be, at any rate, the *complete* truth about its domain. Much of Einstein's attitude is captured by the judgment that, although quantum theory clearly had more support from the evidence than any other alternative, it needed to be replaced by a theory with quite different metaphysical commitments that

would nonetheless recapture – and indeed extend – the empirical success of quantum mechanics.)

Nonetheless, it may well be true, sociologically speaking, that as a broad statistical generalisation, most scientists who make significant contributions to a theory have ‘taken it to their hearts’ in a stronger sense than the *apparently* rather anemic one of simply regarding it as currently best supported by the evidence.²³ The great innovators no doubt believe that they can turn the theory they have chosen into the best-supported theory in its field – a belief that cannot, by definition, be justified by the current evidence. And no doubt the ‘normal’ scientists who choose to develop some theory have attitudes towards it that, if generally vague and sometimes misguidedly strong (such as outright belief in its truth), clearly go beyond that of merely regarding it as currently better supported empirically than any rival. It may also be true, as Kuhn, following Planck, suggested, that scientists who have contributed to one theory find it especially difficult to commit themselves in the same way to a newer theory even when, assessed on the objective factors, that newer theory looks superior.

It was this extra, and rather ill-defined, ‘oomph’ – the commitment factor, if you like – that chiefly fascinated Kuhn. And concerning *it*, it is unclear whether an orthodox rationalist philosopher such as Popper, Lakatos, or the others need object to talk of conversion experiences, only partial mutual understanding, or even incommensurability. This commitment involves, in all cases, at least a judgment made on the basis of the current state of the theory and the evidence for it about how some modified version of that theory will look in the light of *future* evidence – and such judgments obviously and inevitably lie outside of the purview of the sort of ‘inductive logic’ judgments that philosophers have traditionally sought to articulate and defend.

Here then is one way to understand Kuhn’s final position. *Firstly* there is, just as ‘the’ philosophers of science have insisted, always an objectively correct judgment to be made about how various rival theories, at a given time, stand in relation to the evidence. There is (this is the undeniably revisionary part, since Kuhn explicitly said the opposite) no leeway, no room for (informed) subjective disagreement, concerning judgments about the objective factors that go into making that overall judgment about what might be called the ‘state of the intellectual debate’ between the various rival theories at any given time in the light of the evidence available at that time. (In so far as there are genuine differences between individual scientists about these matters, they either result from a mistake by one of them or – no doubt more often – are best interpreted as views about how some *future* version of one or more theories, the outlines of which the scientist may feel she

has in mind, will look with respect to the objective factor concerned.) That state of the intellectual debate sets the context within which the individual scientist operates. However, *secondly*, there is *clearly* a lot more to the process of science than simply the state of the intellectual debate, much more to the choices and decisions of scientists; for one thing, because those scientists are engaged in *changing* that state of the debate. It is here that flesh-and-blood decisions, conversion experiences, disagreement, and failure fully to communicate all may come in.

On this interpretation, then, laying aside the (important) issues about falsification, there was no real need for Popper and Kuhn to be at odds. Popper could concede that the points that Kuhn made about theory choice all belong in the context of discovery (rather than the context of justification or, better for Popper, the context of appraisal). And Popper always insisted that only the latter context is ruled by logic. Popper needed to have no quarrel with Kuhn's claim that psychological and sociological factors play ineliminable roles in theory choice if that is construed as essentially a context of discovery notion. The issue of whether this analysis of theory choice reconciles Kuhn's views with those of Lakatos is altogether more difficult. Lakatos was always troubled by Feyerabend's charge that philosophy of science was rather empty if it *simply* laid down rules of appraisal and hence allowed any theory choice in this Kuhnian sense I have attributed to Kuhn, as long as the chooser correctly acknowledged the current 'state of the intellectual debate'. Moreover, Lakatos saw (if not always clearly) one element of the appraisal of the *current* state of a research programme as a measure of its current heuristic power – essentially of how many related ideas for constructing specific theories within the programme remained unexhausted. Even more than thirty years on, I believe that the issues raised by this suggestion and the related question of how much of the process of theory change in science can be explained as a rational process remain both pressing and unanswered.²⁴

Notes

1. Lakatos and Musgrave (1970). Unadorned page references throughout this essay are to this book.
2. In fact, Popper took the chair at the symposium led by Kuhn, but made several contributions to the discussion and, of course, developed his response in his (1970) paper.
3. This is, of course, based on a confusion. The problem, as we shall see in detail later, is exactly that the sort of theory that Popper had in mind: 'single' scientific

- theories, such as those of Newton or Maxwell, are not refutable ‘in isolation’ (as Duhem put it) and hence are never directly ‘found to be false’. It would be irrational indeed for a scientist to continue to hold a theory that had been ‘found to be false’. The fact rather is that such scientists are claiming that the theory may still be true and that the apparently negative evidence is explained by the falsity of some other theoretical assumption.
4. Kuhn’s reference is to Thorndike (1923–58), 5. In fact, astrology’s so-called failed predictions are unimpressive. Of course, there is an implicit assumption in talking about the predictive success of science that the predictions are properly derived from a theory (or rather theoretical system) and not just thrown out more or less at random, with little or no connection with any theory. But, so far as I can tell, the ‘predictions’ that Thorndike cites are all of the latter sort. But that means that if the prediction fails, that failure supplies no refutation of any set of astrological theories. Hence Thorndike’s examples seem to underwrite Popper’s point rather than challenge it.
 5. Duhem (1906). Although it is often nowadays referred to as the ‘Duhem–Quine thesis’, Quine in fact added nothing of substance.
 6. In fact, the real historical story was very different and much less confrontational. However, as so often happens, the rationally reconstructed account helps make the methodological issues much sharper.
 7. In fact, contrary to the Lakatos version, this is basically what happened historically.
 8. See my (2000b) article for an account of, and references to, the recent revival of the old Newtonian idea of deduction from the phenomena.
 9. See my (1996) work and especially my (2002b) article.
 10. See my (1996) work for references and discussion.
 11. See Scheffler (1967) and Shapere (1964).
 12. Lakatos here uses the term ‘logic of discovery’ in the Pickwickian sense that makes Popper’s book a real contribution to that field. What he really meant, of course, was ‘logic of theory appraisal’.
 13. Although the message is clear, Kuhn did not explain himself as clearly as he might have. Obviously, if the tree has already been drawn, one can tell which theory is the later one. What Kuhn clearly really meant was that such an outside observer could use the ‘objective factors’ to *construct* the evolutionary tree.
 14. See *Structure*, pp. 151–2: ‘The source of resistance is the assurance that the older paradigm will solve all its problems, that nature can be shoved into the box the paradigm supplies.’
 15. I have tried to clarify and extend the earlier treatments of the ‘prediction versus accommodation’ debate by Lakatos, Zahar, and myself in my (2002a) work.
 16. Again, this means ‘concessions relative to the position that most philosophers initially took Kuhn to be adopting’. It seems to me an unclear, and relatively uninteresting, issue whether they are concessions relative to Kuhn’s ‘real’ initial position or merely clarifications.

17. Indeed, the main thesis of van Fraassen's later – and very influential – (1980) book is precisely that the phenomenon of the rational acceptance of a theory in science can be explained without any assumption about the theory's truth.
18. Kuhn made a repeated mistake concerning Popper's notion of verisimilitude. He supposed that Popper (and Lakatos following him) intended it as an 'effective' notion: that there should be some algorithm for arriving at a value of a theory's verisimilitude. See in particular p. 238, where Kuhn explicitly talked about Popper attempting to provide 'an algorithm for verisimilitude'. But Popper was quite explicit that he was attempting to do for approximate truth what Tarski had done for full-blown truth – namely, providing a 'metaphysical' account of *what it would mean* for one theory to be a closer approximation to the truth than another in a way that need not (and did not) carry any 'epistemological' component – instructing one how to arrive at actual judgments about verisimilitude in particular cases.
19. For one thing, it involves a 'whiff' of induction. See in particular Lakatos (1974).
20. Kuhn talks, on pp. 231–2, of incommensurability as amounting to 'partial or incomplete communication'; and he acknowledges that those accepting incommensurable frameworks are *not* left without recourse – 'there must be recourse. . . . Given what they share, they can find out much about how they differ. At least they can do so if they have sufficient will, patience and tolerance of threatening ambiguity. . . .' (pp. 276–7).
21. Admittedly, Bayesianism, currently perhaps the most popular systematic philosophy of science, blurs the distinction by talking in terms of the degrees of belief of Bayesian *agents*. But this, in turn, is a logical abstraction. There is no such thing as a real Bayesian agent, since she would have to be, amongst other things, a perfect deductive logician. For a systematic treatment of the relationship between Kuhn's analysis of science and personalist Bayesianism, see my (2000a) work.
22. For systematic attempts to clarify this notion see Earman (1993) and my (2000a) work.
23. It isn't in fact so anemic; see my (1978) and (2000a) works.
24. I have tried to provide some important preliminary clarifications in my (2000a) essay.

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