Part I

Exposition

We now expound Kuhn's three major works, *The Copernican Revolution* (*CR*), *The Structure of Scientific Revolutions* (*SSR*), and *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (*BB*). Hardly anyone, even authors giving lengthy accounts of Kuhn's work, read his historical studies in conjunction with *SSR*, and this is, we think, one reason why the understanding of Kuhn is characteristically so poor. *SSR* is packed with historical examples, but these are brief and illustrate specific points of argument, giving insufficient guidance to the difference Kuhn's ideas were meant to make to our picture of science. The failure to understand how the ideas in *SSR* cash out in the historical studies almost invariably signals a failure to understand both. Exposition of Kuhn's main case studies is, then, in practice, an essential aid to explaining *what it is that Kuhn is talking about in SSR.* Bringing these case studies into our discussion more heavily than other authors have done will make it easier to see that Kuhn's arguments cannot be as *absurd* as they are accused of being. Much or all of what Kuhn says about the Copernican and quantum cases may (as a matter of historical claim) be partly or wholly false, but the kinds of claims he makes are intelligible enough, and propose nothing bizarre or fantastical. Kuhn's claims are, it is worth remembering throughout, claims about the working practices of natural scientists and *only* about that.

1

The Structure of Scientific Revolutions

Our presentation of *SSR* will itself be in two segments. We explicate the main elements of Kuhn's account of the dynamics of natural science in the West, paying particular attention to those that have been provocative or have spawned confusion. Then we introduce some of the philosophical 'matters arising' (that are dealt with more critically in part II) from that sketch of scientific change. Our account is *roughly* correlated with sections in *SSR*. (We deal with Kuhn's amendments to the main body of *SSR* later.) In the first segment of this chapter we deal with Kuhn's central – and, we think, fairly straightforward – concepts for depicting the main changes that take place in natural science: 'paradigm', 'normal science', and 'scientific revolution' provide key words. In the later segment we deal with ostensibly more problematical parts of Kuhn's case, where he introduces (initially at least) very strange sounding ideas: those of 'world changes', 'phenomenal worlds', 'incommensurability', not to mention his rejection of the idea of 'a fixed nature'. We will make a first attempt to show that these ideas are not as strange as they can seem, although that is a long way from maintaining that they are free of difficulties. Whether the difficulties basically invalidate Kuhn's approach is dealt with in part II of the book.

I

If Kuhn's image of science is as 'innocuous' as we say, how is it possible for his work to carry the extreme implications that are regularly attributed to it? We have already suggested that this is less because of what he says than because of what, in saying what he does, he undermines: various popular and ingrained and academically 'respectable' views about the sciences. He thoroughly undermines *not the sciences*, but entrenched *philosophical* assumptions *about* them. And that is not felt, by those he is subverting, to be innocuous.

But what does he actually *say?* And what does he mean by it?

His central platform is set out in *The Structure of Scientific Revolutions*. With some *comparatively* minor modifications, this is the one to which he adhered in the rest of the work published in his lifetime.

In the Preface to *SSR*, Kuhn writes the following autobiographical note, which we take to be absolutely central and essential to understanding his project:

> I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry and biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. (*SSR*, viii)

Thus, the difference so important to Kuhn's thought is one which he first notes in connection with *contemporary* work and the division between natural and social sciences, and that he realizes can be projected back into the history of the natural sciences themselves.

Further down the same page, Kuhn adds that *SSR* is 'an essay rather than the full-scale book my subject will ultimately demand'. Kuhn never wrote that book. A first step towards 'constructing' it is to gain a thorough base-level understanding of *SSR*. When that is combined with Kuhn's later (and earlier) work, a thorough picture of how that book might at least be virtually constructed is possible.

A work of history or of philosophy of science?

(On section I of *SSR*, 'A role for history')

Let us begin by mentioning again the central issue of the succession of major theories in science. Kuhn takes the received view in the philosophy of science to be making big claims about what criteria are *and should be* used to choose one scientific theory over another, and argues that these claims are 'falsified' by the historical record. Thus, a main purpose of *The Structure of Scientific Revolutions* is to make a case as to how scientists *do in fact* come to replace one theory with another. This makes it sound as though *SSR* is one of his historical studies, but it is not that. How, then, is the historical stuff a 'stalking horse' for the philosophical; how is this latter aspect dominant in *SSR*? *SSR* differs from Kuhn's properly historical studies for he is not, here, primarily concerned to detail what, as a matter of historical fact, occurred in various specific episodes in the history of science, but, instead, to say how the events in such episodes should be philosophically construed.

In the Introduction to *SSR*, Kuhn noted that he was already implicitly or explicitly querying such verities as, for example,

> the very influential contemporary distinction between 'the context of discovery' and 'the context of justification' . . . [H]aving been weaned intellectually on these distinctions . . . I could scarcely be more aware of their import and force . . . Yet my attempts to apply them . . . to the actual situations in which knowledge is gained, accepted and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, they now seem *integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed.* (8–9; emphasis added)

He ends the Introduction with the following, ringing question, which seeks rhetorically to insinuate that the needed transformation of philosophy of science has begun: 'How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?'

From immature to mature science

(On section II of *SSR*, 'The route to normal science')

It is important to emphasize that Kuhn is *mainly* concerned in *SSR* with the revolutionary transformation(s) of *mature sciences*, and not with the initial transition from immature to mature: the latter is only a preliminary, though important concern, highlighting the difference between a situation in which no cumulation of knowledge takes place . . . and one in which it persistently does, albeit with disruptions.

When Kuhn remarks (10) that 'normal' science is what most scientists spend most of their time doing, he is talking about operating within a setting where there is 'agreement on fundamentals' already in place, the possession of such agreement on fundamentals being the hallmark of a mature science. In the first instance, the notions of 'paradigm' and 'normal science' are not meant to express the difference of 'normal' science from 'extraordinary – i.e. revolutionary – science' but to capture, rather, *the difference between sciences that do and do not have* this kind of fundamental agreement. The *first* contrast that needs to be kept in mind, then, is between

(a) those areas of study in which a good many things are settled, and where there is some kind of broad consensus on the nature, main business and prevailing approaches of the enterprise; and
(b) those pursuits in which there is little if anything settled.

The contrast between physics and sociology over the past three hundred years is a good example of what Kuhn has in mind. There have been 'revolutionary' upheavals in physics but between these revolutions there have been stable and extensively shared frames of reference that encompass the vast majority of physicists. The same cannot be said of sociology, for example, which, though two hundred and more years old, is far from attaining anything approaching unification. The notion of 'paradigm' is meant, then, to serve in the first instance to illustrate (or constitute) the contrast between a science like post-seventeenth century physics and a would-be science like sociology (*SSR*, 15).

The great difference from the point of view of the practice of science is, for Kuhn, that it is only when there is extensive agreement among them, in their suppositions and practice, that scientists can really get on full-time with the job of empirical research, rather than being constantly diverted from this by the need to argue about the justification and rationale of what they do.

In this context, the notion of 'paradigm' functions actually in at least two distinct ways – and in his 1969 Postscript to *SSR* (in the second and third editions), Kuhn accepted that he had not demarcated the two as clearly as he might. (He would later adopt 'disciplinary matrix' as a more univocal term than 'paradigm' for referring to the encompassing and extensively (though never utterly) uniform body of assumptions shared within a mature discipline.[1] He attempted to restrict the word 'paradigm' to the paradigms – exemplary achievements – which founded sciences and around which subsequent revolutions were built (via their function as models of scientific 'good practice').)[2]

Kuhn argues that many of the natural sciences started off being more like sociology than like physics after Newton. Sciences often begin with a phase in which they are like the contemporary social sciences, where there is no fundamental agreement, where people keep trying to rebuild the science all over again, tearing up existing views of its nature and purpose, and trying to make a completely fresh start. The natural sciences that we now have, and that did start off that way, at some point decisively left this pre-agreement (pre-paradigmatic, as it is sometimes called) state behind them, never to return to it. Kuhn recounts the successive changes in the view taken by physics of the nature of light since Newton: light was conceived as corpuscular, then as waves and then as photons. Each was, in its turn, and for a time, the generally accepted view within physics. However, until Newton, 'no period between remote antiquity and the seventeenth century exhibited a single generally accepted view about the nature of light. Instead there were a number of competing schools and subschools' (12). So, the contrast is between the period prior to Newton, before the seventeenth century, when there was no general agreement in optics as to the nature of the phenomenon of light, but only competing views, and the period after Newton when there were drastic changes in the conception of the nature of light, but, *at any one time*, pretty general agreement held on a current view.

Kuhn holds that the predecessors of physics are rightly considered scientists,[3] and they 'made significant contributions of the body of concepts, phenomena and techniques from which Newton drew the first nearly uniformly accepted paradigm for physical optics' (13). But, while 'these men were scientists', anyone

> examining a survey of physical optics before Newton may well conclude that, though the field's practitioners were generally scientists, the net result of their activity was something less than science. Being able to take no common body of belief for granted, each writer on physical optics felt forced to build his field anew from its foundations. (13)

Not much serious cumulation in such a case! Clearly, the force of the contrast between the ventures with and without paradigms highlights the difference between the case

(a) in which there is, proportionately speaking, relatively little real scientific work (in the sense of empirical *investigation* etc.) and where that work is not in any real sense cumulative but is, rather, randomly assorted, being undirected and uncoordinated, with no integration between results of different studies, between the work of one scientist and another in the same field, and with no possibility of building further investigations upon established techniques and accepted findings; and
(b) in which there is unity and coherence in the investigations carried out, with generally accepted ideas and procedures and where the findings of one study build directly on those made by another.

Development-by-accumulation, again

Kuhn is a severe critic of the image of development-by-accumulation, but, as suggested earlier, he does accept *to a significant extent* the picture of science as involving the stockpiling of knowledge. *However* he does so on the basis:

(a) that the cumulation takes place against the background of a considerable measure of agreement on fundamentals, and on the basis of treating certain past achievements as generally exemplary, as a guide to how to do further work, providing a framework within which meaningful accumulation is possible, and a context in which each scientist no longer needs to be involved in beginning all over again for themselves;
(b) that the paradigm is seen to have been a crucially missing element from the simple stereotypical ('received') picture of development-by-accumulation, but is in practice *taken-for-granted in that picture*, as it is in normal scientific work itself; and
(c) that occasionally paradigms are overthrown and the 'development by accumulation' has to begin again, from a different – but 'upgraded' – starting point.

While a scientific field may develop out of a 'pre-paradigmatic' phase, once it has developed a paradigm, thereafter new disciplines and specialisms may spin off from that area of work without themselves having a prior pre-paradigmatic phase.

The emergence of a paradigm shifts the situation within the field, creating a more rigid and exclusionary situation; some people go along with the new paradigm, others literally die without having been able to reconcile themselves to the change.[4] The group associated with the now dominant paradigm is transformed from a loose group with shared scholarly interests into a profession with all its appurtenances, with journals, specialist societies and a control over qualification in its field. Textbooks, not the working scientist, take on the job of spelling out the science's fundamentals. Advanced research becomes interesting and accessible only to specialized colleagues and the scientific paper, rather than the book, becomes the means of communication within the profession (20).

The importance of *organization* now becomes very clear. It is not as if other philosophers of science had denied the existence of this *professionalizing* tendency of science, they just didn't seem very interested in it. Even common sense might have noticed it since, as the following quotation avers, 'it has become customary to deplore' this development, and to regret 'the widening gulf that separates the professional scientist from his colleagues in other fields, [though for Kuhn] too little attention is paid to the essential relationship between that gulf and the mechanisms intrinsic to scientific advance' (21).

The concept of 'paradigm'

Kuhn's most famous concept[5] is that of 'paradigm'. Not one that he initially coined, it has however caught on since he adopted it. One hears almost endlessly nowadays of 'new paradigms' arising or being needed in every area from Geology or Child Psychology or Management Science to the 'New Age'. Of course, just this ubiquity should be a cause of concern for us. What concept could it possibly be that could be understood and serve so widely?

Naturally enough, it turns out on closer examination barely to be Kuhn's concept at all. For the first point that must be borne in mind here is that, even in Kuhn's own work, the term 'paradigm' stands *for very different things.*

Let us begin by looking at where Kuhn begins, at the point where the term 'paradigm' – in Kuhn's particular sense(s) of it, in its paradigmatic sense for him – gets introduced:

> In this essay, 'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. Today such achievements are recounted by science textbooks. Before such books became popular many of the famous classics of science fulfilled a similar function. Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Lavoisier's *Chemistry* – these and many other works shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to solve.
>
> Achievements that share these two characteristics I shall henceforth refer to as 'paradigms,' a term that relates closely to 'normal science'. By choosing it, I mean to suggest that some accepted examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – provide models from which spring coherent traditions of scientific research. These are the traditions which the historian describes under such rubrics as 'Ptolemaic astronomy' (or 'Copernican'), 'Aristotelian dynamics' (or 'Newtonian'), 'corpuscular optics' (or 'wave optics'), and so on. (10)

It is interesting to note how strongly this, Kuhn's initial account of paradigms, draws on the 'literary' aspect of science: Kuhn emphasizes the importance of classics (then) and textbooks (now) – virtually no natural scientists read the classics any more (having no need to) in defining their field(s). Textbooks, written works, play a major role in laying down what a paradigmatic scientific achievement is, how it is to be understood, how it is to be *taken and used*.

Also emphasized is the sharedness and indeed *compulsoriness* of the paradigmatic. This is already a strong hint that any suggestion that one can choose to have or even try to have a scientific revolution, to move to another paradigm, is going to be wrong-headed. One is enormously constrained – by the world in one's lab . . . and by one's tradition and community.

Let us now focus in on what was for Kuhn the heart of his conception of 'paradigm', the sense in which the word must sometimes be meant (if one is to have an effective philosophy of science), whatever other senses it might also be used in.

Exemplars (artefact paradigms/construct paradigms) This is paradigms as exemplary, as acknowledged achievements providing models to follow, laws to explore and to find new versions of in new sets of circumstances, *etc*.

This usage of the term 'paradigm' derives from teaching grammar. A paradigm in grammar is literally an example that one is supposed to be able – once one has understood it – analogically to apply in new circumstances. For (a very simple) example: if one is given the endings to a verb in French (such as *bouger*), and then told that these are the endings to all such verbs – to all verbs ending in '-er' – then one has a paradigm for conjugating those verbs oneself. Similarly, Kuhn thought, in science – with an important proviso: in grammar

> the paradigm permits the replication of examples any one of which could in principle serve to replace it. . . . In a science, on the other hand, a paradigm is rarely an object for replication. Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions. (23)

Just as with the disciplinary matrix, so with the exemplars which form a vital part of the former, the extent to which they actually are fully-mutually-understood is, according to Kuhn, uncertain prior to being tested in times of crisis. It may turn out that they always were understood differently by different scientists, but the difference never previously gave rise to an issue as relevant cases never arose – the shared understanding and application of the paradigm, of the exemplar, never *needed* to be in question.

This is perhaps because rules of scientific procedure are rarely explicitly taught, but are rather absorbed with the paradigms – and the way in which the paradigms are presented and instilled may vary a little from one educational setting to another, resulting in variable understandings as to how exactly to go on from the paradigm. By hypothesis, being always potentially on the cutting edge of research, it must have unapplied instances easily within reach, and one must be ready for scientists to find that they do not agree quite as much as they thought they did, that when it comes to this new case, they diverge in their judgements of what the right thing to do is (though whether or not this will matter or ever be noticed will depend on circumstances).

Kuhn's use of the term paradigm can seem quite unsatisfactory in its ambiguity – though certainly not as unsatisfactory as Margaret

Masterman has led people to believe[6] – but it would help, perhaps, to notice the (harmless) *crudity* of the exercises that we are involved in here. Kuhn's account of the development of science is a pretty *gross* one. It is no more *gross* than any other philosopher of science's account, however, because discussion of the nature of science will unavoidably be carried on at such a gross level, consisting of wide-ranging and largely and unavoidably unsubstantiated generalizations.

We do not think that Kuhn's use of the notion of 'paradigm' is really meant to set one out on the meticulous classification of the different kinds and degrees of agreement that there might be within science, across and within disciplines. It is not a sociological term of art. The notion of paradigms is, in the first instance, meant to *highlight* a very stark contrast, between early stages in the development and later ones, and between the natural sciences (pretty much) and the social 'sciences' (pretty much): in other words, between the pursuits that have some kind of unity, as opposed to those that are in disarray. The 'exemplar' usage of the term highlights the degree to which there is *quite specific* agreement across the discipline or subdiscipline; the extent to which one piece of work can guide good practice, and enable the close relations that there can be between one bit of scientific inquiry and another in the same part of the discipline; the way in which studies in the natural sciences can often fit together in ways that studies in the social sciences seldom, if ever, do. (Without exemplars, no (real) science.) At the same time, this should not be overdone: the extent of agreement and disagreement is not to be treated as some absolute. As is plain, agreement is commonly and unproblematically (outside the world of philosophical fantasy) more or less: if one is involved in a relatively superficial transaction with others, then one might be in full agreement with them, but if one goes more fully into the terms of one's agreement one may find that the agreement is not so close as it seemed, or that there is much in the attempt at further and fuller specification of the agreement to disagree about.

Working on paradigms

(On section III of *SSR*, 'The nature of normal science')

The importance of paradigms (exemplars), in the initial instance, is that *they give scientists (real) work to do*. The fact that they contribute impressive solutions to existing problems is what makes them deserving of scientists' attention. That they solve some key problems is crucial to their attractiveness and acceptance, but another important source of their appeal is that they provide a rich source of *new* problems. Thus the paradigm is, we might say, a challenge – the challenge is to make it work as well as it can. Consider, for example, the attainments of the quantum physicists in the early part of the twentieth century, or even those of Darwin in the mid/late nineteenth century. These contributions have provided problems that have kept large numbers of scientists seriously and purposefully occupied full-time at least into the early part of the twenty-first century, though paradigms do not normally last forever, and there assuredly will be further (conceptual) change. And this is the main element of Kuhn's historical reconstruction, discussion of the reasons why and the ways that paradigms displace one another. In *SSR* Kuhn emphasizes the way one paradigm would displace another within the same scientific specialism, but later came to think that paradigm change often involves the spawning of a breakaway specialism, and that this was the more important focus.

Solving puzzles and displacing paradigms

(On section IV of *SSR*, 'Normal science as puzzle-solving')

We now reach the crucial point at which Kuhn's concept of 'normal science' is laid out. In order to understand why this is crucial, it will help to anticipate Kuhn's 'complementary' account of 'scientific revolutions'.

The introduction of a new paradigm into a science-with-a-paradigm is characteristically at the expense of the established paradigm (though separation and thereby greater specialization is another possibility), and successful installation of the new paradigm is the outcome of controversy. This displacement of one paradigm by another, and the controversy usually associated with it, is what Kuhn calls a 'scientific' revolution.

Scientific revolutions can, when completed, often be described as *total*. (This remark simply glosses Kuhn's important remark that scientific revolutions are *irreversible* (cf. *SSR*, 166).) However, it is equally important to Kuhn to stress that the grounds that produce such a clear-cut outcome are not necessarily themselves all that clear-cut. The 'received view' encourages the idea that scientists switch their loyalties from one scientific idea to another because they have established unequivocally that the new idea is better than the old, that new work decisively refutes prior work. Kuhn does not deny that the switch of a discipline's loyalties from an older to a newer idea, approach, etc., will *eventually* turn out to be absolute, and *looking back* it might, therefore, seem obvious to suppose that it was because of the plain, indisputable advantages of the new paradigm that it was universally preferred. But any such impression may well be entirely false to the historical record, and the choice between the disputed paradigms may have been anything but starkly obvious during the revolution.

Kuhn's *first* interest in 'scientific revolutions' then is in showing that the decisive results of these controversies may well stem from what were, *at the time*, less than conclusive reasons: the triumphs were not the 'knock-out' ones they might later seem. Any fair-minded comparison of the scientific rivals might give something rather closer to an 'on points' verdict, and recognize that the decision may have been 'a damn close-run thing' and akin, even, to a split decision. The fact that the verdict (for instance, in boxing) may involve a 'split decision' does not, however, make it any less final – its beneficiary is unquestionably the winner. Thus, it does not have to be – and in fact never is – that there is *nothing whatsoever* to be said for the scientific paradigm that loses out (see *SSR*, 99–100 and 107 for Kuhn's partial defence of the 'much maligned phlogiston theory' in chemistry). The victorious paradigm may well have won out over other contender(s) on *only a few points*.[7] The successful one may be neither 'completely successful with a single problem or notably successful with any large number' (23).

The differences – the *decisive* ones – between the paradigm installed as the new exemplar for up-to-date practice and the one it outmodes may be few and marginal ones from a point of view outside the science, but this is difference enough in the science. Kuhn's interest is in assessing what considerations played a part in driving the change *at the time*; his business is the depiction of the bases on which scientists satisfied themselves that they were doing the right thing.

Kuhn is not saying that one paradigm is demonstrably just as good as another, and denying there is any sense in which the election of one over another may ever be vindicated. It is worth remembering that Kuhn's concern is with reconstructing the historical situation *at the time*, without recourse to how things *later* turned out (his rejection of 'Whiggism').

Kuhn distinguishes between the fantasy of a completely successful paradigm and those that are in actuality encountered – relatively (more) successful ones (23). He echoes his more general conviction that the thorough exploration of nature can be pursued virtually indefinitely, and that continuous carrying through of the exploration will pose more – and more heterogeneous – problems than can be solved within any single framework of inquiry. Sciences attempt to capture the complexity of nature within a simple scheme, and the complexity of nature will always, in the end, overflow that scheme.[8] A new paradigm can be admirable or notable in that it can solve problems that are known to be more difficult than have been encountered before, or which have long proved intractable. It may identify a whole range of interesting new problems and have every prospect of satisfying them, but in all probability it will eventually encounter numerous problems that are not satisfactorily soluble in its terms.

Victorious paradigms, therefore, offer largely a *promise* of success, to which the achievement represents an initial guide. 'Few people who are not actually practitioners of a mature science realize how much mop-up work of this sort a paradigm leaves to be done or quite how fascinating such work can prove in execution' (24).

Normal science

The history of science after the formulation of a paradigm can be very roughly but profitably seen as an alternation of 'normal' and 'revolutionary' science. Normal science is that science which takes place on the basis of a paradigm, within a disciplinary matrix, and on the basis of accepted exemplars – the science done when the fundamentals stand beyond question. The idea of 'normal science' is one that can easily seem unappealing, making scientific work sound routine, dull and unimaginative, but this is a false impression. 'Normal science' is the condition under which most of the achievements of science are made, and the one under which the much vaunted accumulation of scientific results take place. Normal science is the expression of a humble truth at the heart of Kuhn's image of science – that investigation of nature is a complex task, most effectively pursued through a division of labour. It is only when the areas under investigation are 'typically minuscule' and where individual scientists operate with 'drastically restricted vision' – their attention entirely on their specific research studies, not distracted from this by arguing over the fundamentals – that a detailed and focused investigation of nature 'in ways that would otherwise be unimaginable' (24) becomes possible.

Kuhn insists that during 'normal science' scientists are not in search of fundamental innovations. They are working within pretty well defined limits with respect to what can be brought into question – what *they need* to question. Their scientific activity really amounts to the realization of the potential that the paradigm is expected to provide, and the promise that drew scientists to the paradigm to begin with. The development of the paradigm is *improvement on* its initial formulation, enhancing its precision and extending its scope. This is where the cumulation in knowledge takes place in something like the fashion envisaged by many philosophers of science (especially prominently perhaps in Logical Empiricism and its heirs), as a continuous addition.

To say that the aim of all this scientific work under conditions of 'normal science' is improving the precision and scope of the paradigm does not perhaps make perspicuous why scientists should display 'the enthusiasm and devotion' (36) that they clearly have. But Kuhn asserts that the individual scientist is *almost never* involved in doing the things that people perhaps stereotypically imagine is the greater part of scientific work, namely,

- opening up wholly new territory to investigation; or
- testing well-established belief.

(On this, see for instance, *SSR*, 37–8, 64–6, 77, 97 – and again 'compare' with sociology.) Scientists, according to Kuhn, are normally preoccupied with the technicalities of solving the problems left over by an earlier, and very striking, achievement in their area of work. What scientists find in their work is a challenge to their ingenuity. Weinberg's charge, cited in our Introduction above, that Kuhn has no explanation as to why scientists bother with these problems is falsified. We might paraphrase Kuhn as saying that people take up scientific problems because they find them deeply intriguing, and badly want to investigate what is going on; in many cases, they just can't leave these problems alone. Kuhn does not, however, identify what Weinberg perhaps seeks, any further purpose above and beyond the satisfaction of solving a difficult problem; and this satisfaction is having solved a difficult problem *and thereby* having made a contribution to human knowledge.

Before you react against the idea of normal science, pause – think about those libraries full of natural scientific periodicals, and of what the content of those must be. They must mostly fall somewhere between direct repetition of paradigm achievements and fundamental novelties. They can't be full of exactly the same stuff being done over and over. Sociologists of scientific knowledge have made a big, rather empty fuss about the fact that natural scientists don't replicate (much, if at all). Ask yourself who, with any minimal idea of how the natural sciences work, thought they did, imagined they were endlessly redoing each other's experiments? A stereotypical familiarity with the peer review process in a discipline like physics tells you that there are no rewards for coming second, that doing something over is of value only where something of importance hinges on it. A paper will get rejected just because the work has already been done: in Yazmina Reza's play *Life Times Three* (2001) terror strikes an astronomer when he is told that a paper on the very subject he is currently writing on has already been submitted to a journal.[9] His reaction: two years of his work has been rendered worthless.

Therefore, what is in the scientific periodicals must be stuff that produces novelty – it can't be straightforward repetition. At the same time, it can't all be ground-breaking, all-changing novelty: the kinds of 'fundamental novelties' we more or less non-scientific punters (who only keep up with the popularized stuff on science) do hear about are relatively few. Hence, most of what must be in those journals must be 'normal science': it does something that makes it worth publishing for the others in the same field, but it doesn't by any means turn everything upside down.

Thus, normal science is what Kuhn calls puzzle-solving (because it is – under normal science conditions – like ordinary puzzle-solving situations, where one is confident that there is, *that there has to be*, a solution and the only problem is to work out what it is) and the interest is only in those problems which can be assumed to have a solution. Problems will be set aside by scientists if it seems that they cannot be solved (37).

The analogy with puzzle-solving (36) is made to drive home the point that there are strong constraints on what it takes to solve a scientific problem in normal science. There are the conceptual, theoretical, instrumental and methodological commitments already in place within the scientific community (40). Also, scientific achivement demands a continual 'raising of the game': any result acknowledged to be an achievement must improve the scope and/or precision of the paradigm. The analogy with puzzle-solving is very important vis-à-vis saying what science is *for scientists*.

In short, Kuhn suggests that it is worth trying to see normal science *as* puzzle-solving – and that the results of doing so are illuminating, and have unfortunately been occluded from, and by, nearly all pre-Kuhnian (and much 'post-Kuhnian') philosophy of science.

On training and rules

(On section V of *SSR*, 'The priority of paradigms')

Kuhn now turns to the importance of *training* within the framework of normal science. The notion of the paradigm as 'exemplar' plays a part in challenging the idea that there is any 'scientific method' which could be specified as a set of rules prescribing in significant detail how a scientist should go about inquiry. No such set of rules is to be found spelled out in the scientific literature. Nor is instruction in such rules any part of training newcomers to a science, and they will then practise the science without having been taught any rules. Their training mainly involves confronting them with 'exemplars' (in the textbook, the lecture and the laboratory – or equivalent). It is through close study of these exemplars that trainees learn how to carry out scientific work (within their speciality).

There is one sense in which Kuhn is saying that science is dogmatic rather than critical, and here Popper voices a strong objection. However, we must be careful not to take the idea that it is authoritarian very far (cf. p. 113 below). Graduate training in the natural sciences can be dogmatic in the sense that students are presented with current science in a take it or leave it form: if they can't master and accept the current ways of doing things then they will not be admitted to a professional career in the field.[10]

In sum: paradigms are 'logically prior' to the research work that goes on within them. And, to continue with Kuhn's metaphor from Gestalt psychology, they are *the ground* against which innovations, anomalies, etc., can emerge as the *figure*. One sees anomalies 'against the background provided by the paradigm' (65).

Anomaly

(Section VI of *SSR*, 'Anomaly and the emergence of scientific discoveries')

According to Kuhn, the role of fundamental discovery, of fundamental factual or theoretical novelty, has been overstated.[11] Truly novel discoveries are not what are actually sought in the work of normal science, and the necessity for them is often recognized reluctantly. This does not say that normal science is hack work, that scientists are going for easy solutions – the work that is done in normal science is creative, productive and innovative, but innovation in the *fundamentals* of the discipline is relatively rare.

Fundamental novelty brings about changes in the way in which the science 'looks at the world': 'Assimilating a new sort of fact involves a more than additive adjustment of theory and until that adjustment is completed – until the scientist has learned to see nature in a different way – the new fact is not a scientific fact at all' (53). The change involved is, Kuhn's entire approach insists, neither a matter of accumulating, nor (*a fortiori*) one of accumulating *facts*. The change is of a kind that Kuhn sometimes terms a change in 'worldview'. In part this change alters the considerations that delimit what could possibly be accepted as a fact within the discipline. The change is not one that involves new findings as such, but one which – in accord with the idea that it involves a paradigm shift – involves a change in the idea of what properly scientific problems are and how they may be solved.

The appearance of anomaly

The key to fundamental novelty is, for Kuhn, the occurrence of 'anomalies', a term that precisely captures the implication that novelties are novelties *only relative* to some paradigm, are things which *do not fit* the existing scheme.

An example is what occurred in the 1770s vis-à-vis chemistry. Here is Kuhn: 'In 1774 [Priestley] identified the gas [produced by heated red oxide of mercury] . . . as common air with less than its usual quantity of phlogiston. . . . Early in 1775 Lavoisier reported that the gas obtained by heating the red oxide of mercury was "air itself entire without alteration [except that] . . . it comes out more pure, more respirable" ' (53–4).

One can see the burgeoning anomaly right there, in Lavoisier's peculiar, almost tortured language.[12] What Lavoisier eventually proposed – and what, as it happens, Priestley could never accept – was that the gas being produced here was not something which could be neatly fitted into the boxes provided by the paradigm of the time, 'phlogistic' chemistry. Kuhn concludes that 'Only when all the relevant conceptual categories are prepared in advance' can we intelligibly speak of discovery as a point event (55). The revolution from ‘phlogistic’ to ‘modern’ chemistry overturned conceptual categories, and so, Kuhn suggests, it is misleading to depict it as happening at one particular place and time, or being carried out by a single person. (One could describe the discovery of (say) xenon like that, once the periodic table had become well established – but not the discovery of oxygen, for which no place had been prepared in the chemistry which preceded it.)

No change without something to change to

(On sections VII and VIII of *SSR*, ‘Crisis and the emergence of scientific theories’ and ‘The response to crisis’)

Here Kuhn takes a first step towards the expression of what seems to many a very troubling idea – the rejection of the idea of a ‘fixed nature’ (see p. 58 below).

A crucial point in Kuhn’s argument, one that is broadly ‘Pragmatist’ in nature, is the idea that ‘radical critique’ alone is an idle wheel in science (77). Scientists only give up an accepted paradigm when there is some alternative they can attach themselves to: that there are some things the paradigm cannot do does not detract from the fact that there are many things it can do. That there are things which the paradigm cannot do is, Kuhn is suggesting, a normal situation, even a necessary situation in something that is (still) a science, and not, *of itself*, a fateful flaw. The fact that there are things that do not fit an existing paradigm does not result in withdrawal of the paradigm, which makes it plain why there are anomalies – if scientists followed the strict ‘logic of science’ (*à la* Popper for example, a logic of refutation, where a single negative instance can – ideally – invalidate a whole theory), then, when they found something which did not fit with the paradigm they would reject the paradigm and go back to square one, meaning, of course, that there would be no such things as anomalies in Kuhn’s sense. But this is not what scientists do.

Kuhn on the chemical revolution example again: Many things lose weight upon being burned. Well, at least, they *appear* to. If one investigates very carefully, collecting all the ash and the water vapour released and the smoke particles etc., one finds that they become slightly *heavier*. As Kuhn writes, ‘[Lavoisier] was much concerned to explain the gain in weight that most bodies experience when burned or roasted, and that again is a problem with a long prehistory. At least a few Islamic chemists had known that some metals gain weight when roasted' (71).

Ah, so there was a clear refutation of the phlogiston theory available, and it had been available for ages? Not so fast. While it is true that 'In the seventeenth century several investigators had concluded from this fact that a roasted metal takes up some ingredient from the atmosphere', still 'that conclusion seemed unnecessary to most chemists' (71). Why? Well, 'If chemical reactions could alter the volume, color, texture of the ingredients, why should they not alter weight as well? Weight was not always taken to be the measure of quantity of matter. Besides weight-gain on roasting remained an isolated phenomenon. Most natural bodies (e.g. wood) lose weight on roasting as the phlogiston theory [said] they should' (71). Longstanding anomalies are usually just – things to ignore.

When weighing became more accurate (leading to more and more cases of weight gain), and when 'the gradual assimilation of Newton's gravitational theory led chemists to insist that gain in weight must mean gain in quantity of matter', then phlogistic chemistry started to look bad. *Even then*, phlogiston was not done for, 'for that theory could be adjusted in many ways. Perhaps phlogiston had negative weight, or perhaps fire particles or something else entered the roasted body as phlogiston left it' (71). But phlogistic chemistry became less and less *attractive*, especially to newcomers to the discipline.

It requires more than the mere existence of anomalies to set scientists to re-examining the fundamentals, searching for solutions outside what the paradigm allows.

So, what do scientists do when their discipline seems to be in some kind of crisis?:

> [We should note first] what scientists *never do* when confronted by even severe and prolonged anomalies. . . . [T]hey do not renounce the paradigm that has led them into crisis. They do not, that is, treat anomalies as counter-instances, though in the vocabulary of [Carnapian, Popperian, etc.] philosophy of science, *that is what they are*. (77; emphasis is added)

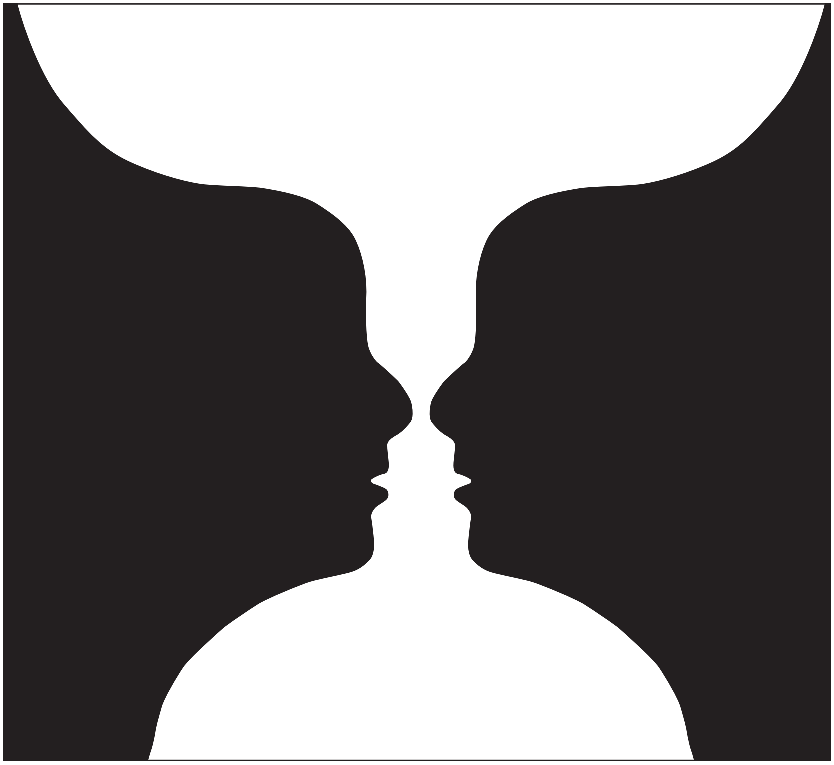
We hope it is now obvious that this does not mean that theories in science can proceed merrily along, without *regard* for how things are in the world, for how one's experiments are going, etc. There is no reason why anyone should misread Kuhn's claim that the history of science has never yet revealed anything which resembles 'that methodological stereotype of falsification by direct comparison with nature' (77) as suggesting that comparison with nature *has nothing whatever to do with it.* Comparison with nature takes place *in the context* of the comparison of paradigms, and on the terms provided by these. In a period of normal science, there is regular comparison of the paradigm's expectations with nature, for this is, of course, what puzzle-solving often consists of: seeing how well the paradigm works out in further cases. What else are anomalies except the cases in which the paradigm's expectations *are un*satisfied, in which nature *does not* behave according to these expectations, and the scientists can understand that this is not what they were expecting?

The search for fundamental novelty is provoked, if at all, by anomalies, but, since anomalies always exist without necessarily provoking such quests, the question remains: what makes an anomaly worth concentrated scrutiny? It depends on the specifics of the case. There is no algorithm for fundamental scientific change: this is a point which fundamentally disappoints rival philosophers like the Logical Empiricists or Imre Lakatos. In the 'extraordinary' period in science leading up to a scientific revolution, some scientists no longer depend on and work within the paradigm in the same unquestioning way, but, in their attempts to work out just what it is about the anomaly that is anomalous, attempt to sharpen the tension between the anomaly and the paradigm. They are thus apt to put the usual practices deliberately under strain. Since the capacity of the paradigm to serve as a reliable guide in exploring the area of the anomaly is what is in doubt, the scientists' behaviour will be less well directed than under 'normal science' conditions, and will be a bit more like the random casting about characteristic of the pre-paradigmatic case (with even occasionally explicit argument over fundamentals, or behaviour more like that of a philosopher than of a normal scientist (87–9)).

What are 'scientific revolutions'?

(On sections IX and X of *SSR*, 'The nature and necessity of scientific revolutions' and 'Revolutions as changes of worldview')

In an attempt to clarify what is involved in the substitution of one paradigm for another Kuhn makes an ultimately somewhat ill-fated analogy with the 'Gestalt switch' in which people are able to alternate between two discrete perceptions of the same thing. The 'Gestalt switch' is commonly identified in psychology by the 'duck/rabbit' in which a schematic drawing can alternately be seen as a duck and a rabbit, or by a picture in which the image of two faces alternates with that of a vase.



It was an analogy Kuhn cautiously made (albeit hardly cautiously enough). The analogy's value is in emphasizing that paradigms are rivals in the sense that scientists can accept either the prevailing paradigm or its proposed alternate as valid, but cannot simultaneously entertain or accept both (85). The central limitation on the analogy is that in the usual case people can switch back and forth between the two perceptions – now they see the duck, then a rabbit, and then revert to perception of it as a duck again. Scientists cannot engage in such reversion. When they move from one alternate to the other they have given up the first for the second. The switch is strictly one way, there is no going back. It is a permanent one-time-only Gestalt switch. Another disanology applies – the Gestalt idea involves talk of people 'seeing things as' this or that (for instance, now as a duck, now as a rabbit), which has an inappropriately provisional character in comparison with the categorical ways in which scientists express themselves: they do not, at least when committed to a paradigm, speak of themselves as seeing things 'as this' or 'as that', but just assert that they see those things (85, 114–15).

At last, scientific revolutions

Scientific revolutions are those times[13] at which one paradigm replaces another, or, as Kuhn later came to emphasize, a new area of research spins off from an established one on the basis of a new exemplar. Obviously, consideration of how these revolutions take place is critical to Kuhn's attack on the received image. It ought now to be clear that Kuhn will certainly decline to accept that a scientific revolution is a dispute between an obviously right party on one side and an obviously mistaken one on the other. Since the paradigm provides the means for settling disagreements in scientific results, if the paradigm itself is in dispute, then the usual means – the *only* means – for settling disagreements are out of order. During such revolutionary periods, the situation in the science may be more like the pre-paradigmatic condition than it ever is in periods of normal science: fundamentals are in question, there are meaningful possibilities of fundamental novelty, there is a lack of focus and a sense of casting about within the community. However, while this is more *like* the pre-paradigmatic situation, this is not a *return* to any such condition, and is certainly not going to involve any starting completely afresh and all over again.

The founding analogy – with political revolutions – is seriously and multifariously intended, but should not be taken too far (and Kuhn's rhetoric perhaps gets a little strong on *SSR*, 93, for example). The analogy's main value to Kuhn is to provide a reminder that the conflict between the defenders of a political status quo and their revolutionary opponents is one that cannot be resolved by neutral, authoritative adjudication. Where the society once had authorities that would settle disputes, in a time of revolution there is no longer any authority that is recognized by both sides in the struggle, and the only resolution possible is therefore the outright defeat of one or other party.

One of the key things that Kuhn wants to say about scientific revolutions is that their dynamics have to be understood at the level of the scientific grouping, rather than as a matter of individual choice (again like political revolutions, they involve choices between 'incompatible modes of community life' (94)). Considered at the level of the scientific community, scientific revolutions don't take place through – certainly not *only through* – switching of allegiances on the part of individual scientists. In Kuhn's considered view (this is obviously largely an empirical question), many individual scientists just don't *switch* allegiances at all. Those who have been trained and pursued their careers in the established paradigm may not give up their allegiance, and their resistance to attempts at change makes the attempt to bring in the new paradigm a revolutionary *struggle*. Equally, the protagonists of innovation have not switched their allegiances either – most *never were* attached to the older paradigm, but have entered the profession with the proposed innovation. Thus, the revolutionaries are often made up of younger scientists.

This is not to say that individuals *can't* switch. Kuhn's argument is that understanding what such individuals do is not the exclusive key to understanding scientific revolutions. Such revolutions are shifts in the collective balance within an area of scientific work, where a small minority may enter and eventually triumph over what was previously the as-near-complete-consensus-as-you-will-ever-get. Here, the idea is that the revolution takes place within the same field of scientific work, that the old paradigm is thrown out and the area of work reconstituted on the basis of the new paradigm.

We don't have to worry too much about whether *all*, or even large proportions of scientific revolutions are really like Kuhn's picture. (Nor indeed worry about which candidate revolutions were 'really revolutions'. Kuhn is providing one with a tool-kit, not an encyclopedia of truths – cf. pp. 91–2 on *BB*, below.) The relevance here is that the transition between one paradigm and another is not necessarily a matter of what has traditionally been imagined to comprise a rational choice. To the extent that it involves a changing balance in the composition of the profession, it is not a matter of *individual* choice at all. Neither, of course, is it a collective *decision* in the sense that all have been parties to a collective agreement: it is simply an emergent outcome of the exigencies of the struggle between the rival camps. Kuhn is mocking the idea of the community arriving at a decision by means of explicit *rational and conclusive debate*. This is not to say that there is not debate, for there surely is, still less to deny that there is rationality, of which 'science' *even at times of revolution* could arguably be seen as a set of paradigms; but the idea of scientific debate effectively and purely 'rationally' persuading people from the old to the new view is, at best, an oversimplification. There is plenty of debate, but where the debate affects people, and induces change, it does not do so in the way that the received view imagined. The change in those who do switch allegiance is (notoriously, as Weinberg complained) more like a religious conversion than a rational, that is impartial, reflection on and appraisal of two rival points of view. Kuhn's question is how far this debate features actual and direct disagreement rather than *arguing in circles*, *begging the question*, or *just talking past each other*. This is why scientists can't simply, by good logical and evidential proofs, establish to each other's general satisfaction which of two rival positions is correct.

The debate in scientific revolution, as in political revolution, is often circular. There are real obstacles to mutual persuasion since each party is appealing to principles that the other contests. For example, in order to accept a conclusion you have to subscribe to the premises it follows from, but this is just what the parties involved do not do (consider the face-off between the divine right of kings and the principle of democracy in France around 1789–92). In the scientific case, each group depends on its own presuppositions to justify and evaluate its results, but a logically convincing proof can be given only to someone who concedes the premises to begin with, and disputants cannot therefore *prove* to their rivals' satisfaction that they – the rivals – are wrong. This explains why there are many who are unaffected by the arguments of their rivals. Argument can provide a clear and vivid display of the vision of the new paradigm, of what scientific practice will be like for those who adopt it, and some people may respond to this. It cannot, however, be made compelling 'for those who refuse to step into the circle' (94). So, it is hard for controversialists in these debates *to understand each other*, to appreciate each other's point of view, and, Kuhn argues, the fact is that in one way or another, they *often don't.*

While the scientists are adamantly refusing to change sides, they may also be under misconceptions about what the other position actually is. It is not that the controversialists find each other's positions hard to believe, but they often find them hard to understand in that they cannot see that what the other says makes any real *sense*. This is a crucial innovation of Kuhn's: emphasizing that deep scientific disputes involve questions of sense/meaning just as much as (in fact, more than) questions of true and false, questions of fact.[14] If this is so, then the two sides – to some greater or lesser extent – are handicapped in explaining their respective positions to each other, and in appreciating each other's points of view.

Does it *have* to be this way? Must it really be that some new phenomenon or theory absolutely cannot be assimilated to the existing paradigm? If the anomaly could be smoothly integrated into the existing paradigm, then the stereotype of science that Kuhn has rejected, as developing in a 'fully cumulative manner', would be true. But the stereotype does not fit the facts: 'cumulative acquisition of unanticipated novelties proves to be an almost non-existent exception to the rule of scientific development' (96). There are good reasons for this. Normal science research *is* cumulative, but novelty of the sort at issue here can only exist to the extent that it does not square with the logical consequences of the paradigm in place. So, there *must* be a conflict (a 'logical gap'; a gap, that is, between two more or less logically coherent but distinct systems) between the existing paradigm in terms of which the anomaly is truly that, and the new paradigm in relation to which the phenomenon is no longer an anomaly but, rather, among its logical derivatives (97). The differences between them, further, are not just substantive. The new paradigm brings about reorganization (or 'cannibalization') of old science in the relevant discipline, perhaps reallocating some of its problems to another discipline, declaring others unscientific, and promoting things previously deemed not to be problems, or only trivial ones, to pride of place (103). In the Copernican context we will remark on Kuhn's talk of the 'hard core of knowledge' that remained constant across scientific changes, noting the extent to which the content of the previous paradigm is never entirely abandoned, with 'old science' carried over into the new context. However, in line with the Gestalt-switch analogy, there is in *SSR* much greater, or at least more explicit (than in *The Copernican Revolution* (*CR*)), emphasis on the extent to which the 'preserved old science' changes its character: what is carried over will be extensively altered by transplantation.

Once again we need to bear in mind Kuhn's emphasis on the issue of the complexity of paradigm-to-paradigm comparison. It is not that there are no quite general criteria which may be used for invidiously comparing paradigms,[15] but we can list four considerations:

(a) in real science people are characteristically already signed up to one or another paradigm when they make these comparisons;
(b) there are real obstacles to properly identifying the characteristics of the respective paradigms in the revolutionary situation;
(c) there is an *indefinite plurality* of general criteria, with any one paradigm scoring well on only *some* of these;
(d) in any case, there is no formula which dictates which of these criteria should be given priority or how they should be traded off. Using the same criteria people can end up drawing quite different conclusions as to which is – in an overall judgement – the better paradigm. There *are* rules for comparing paradigms, but no formula for applying the rules conjointly.

II
World changes
(*SSR*, 111–35)

In the latter sections of *SSR*, Kuhn starts to say some strange-sounding things, which seem to many at least troubling, if not outright bizarre. Having considered how paradigms 'constitute' science – that is, give order and structure to its inquiries – he announces that he now wants to explore the 'sense in which they [paradigms] are constitutive of nature as well' as science (110). Rather than hastening to agree with or dissent from this claim, readers might pause to reflect that it is already *qualified* in the light of being said to apply 'in a sense'. Rather than flatly and forcefully affirming that 'paradigms are constitutive of nature', Kuhn is *himself* pondering what, in saying this, he might mean – and does say that he is not yet sure what he means: 'I am convinced that we must learn to make sense of statements that at least resemble these' (121).[16] Next, another crucial, but also crucially qualified statement:

> Examining the record of past research *from the vantage of contemporary historiography*, *the historian of science may be tempted to exclaim* that when paradigms change, the world itself changes with them. Led by a new paradigm, scientists adopt new instruments and look in new places. Even more important, during revolutions scientists see new and different things when looking with familiar instruments in

> places they have looked before. It is rather as if the professional community had been suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well. Of course, nothing of quite that sort does occur: There is no geographical transplantation; outside the laboratory everyday affairs usually continue as before. Nevertheless, paradigm changes do cause scientists to see the world of their research-engagement differently. In so far as their only recourse to that world is through what they see and do, we *may* want to say that after a revolution scientists are responding to a different world. (111, emphasis added)

It is clear, noting that Kuhn is speaking of what the *historian* might conclude, and if one attends carefully to his phrasing, that Kuhn is largely but not entirely aware that saying that 'when paradigms change, the world itself changes with them' is a manner of speaking only. He is uncertain as to whether this only restates what he has argued thus far, or says something rather more than this. It has got some quasi-metaphysical anxieties attached to it – what we will shortly identify as the phenomenal worlds problem.

Kuhn's formulation is acceptable *only in so far as* he, Kuhn, is searching for a form of words that rids us of our temptation to misunderstand the history of science – he is not issuing a metaphysical thesis. What he gives here is a formulation that emphasizes (perhaps even exaggerates for thought-provocative purposes) the impact scientific revolutions have on the outlooks of scientists, to help in overcoming the received view.

For the scientist, the world might be said to change. As a *descriptive* characterization of the differences which the historian, comparing two successive periods, notices, it is unproblematically intelligible: it is *as though* there had been a change in nature itself, so very different are the pre-scientific and post-scientific environments. Before the revolution the scientists would talk prominently about a certain phenomenon that after the revolution they would never mention again. And, reciprocally, after the revolution, scientists start talking about phenomena that had never figured in their discourse before. However, though these are only strong metaphors to give a flavour of the changes paradigm shifts engender, Kuhn is often understood as implying more than this, as if he were saying in a literal way that 'paradigms do constitute nature', and that, therefore, when a paradigm changes *the world literally changes with it*. Thus, one can see how the 'world changes' issue gains its name.

We are not saying, at this point, that Kuhn *is wrongly understood* as intending something more literal than metaphoric with talk of such world changes, for we think he *is* drawn to the idea that more than a *façon de parler* is involved. Even so, we think that his work is clearly dissociated from any such drastic conception as that changing a scientific theory brings about changes in the natural world itself. However, there is an important change of gear taking place here, as the notion of 'different worlds' slips from being an idiomatic expression into a philosophical one, where 'world' often means much the same as 'reality' or, even, 'natural reality' – as in 'there is a world out there.'

Enter phenomenal worlds

Kuhn's attraction to saying that the world changes with paradigms leads him into a problematic as a result of the way he conceptualizes perception, and, therefore, observation. Kuhn is driven towards that conceptualization because of the implications of his arguments about paradigm shifts, considered in relation to the empiricist tradition in philosophy of science from which he is trying – but not quite managing – to make a decisive break.

Given Kuhn's views about the historical relation between an earlier and a later paradigm, he feels compelled to say that, descriptively, the Aristotelian perception ('of a pendulum's motion') is just as accurate (119) as Galileo's – and, indeed, that the triumph of the latter over the former was something of a swindle. Galileo's preconceptions about pendulum motion 'led him to see far more regularity than we can now discover there' (119). From Kuhn's point of view, it seems we have to say that *both* an Aristotelian and Galileo himself, observing the same case of motion (a stone swinging on a string), will make observations that are, in their respective terms, *largely, if not entirely accurate.* Given Kuhn's anti-Whiggism we cannot resort to the otherwise ready solution: to take Galileo's description of the case as identifying what is there to be observed, and deeming, therefore, that the Aristotelian has failed to observe correctly to just the extent that his perception deviates from Galileo. So, we can't say that one of the two parties, Galileo, correctly observed the facts, and the Aristotelian failed to do so. Without some independent and definitive source which says what the facts are, how are we to say whether one person has observed the facts more correctly, more accurately, than the other? We can actually say, or Kuhn does, that the two describe the facts equally well. Thus, we seem compelled to say that each party did observe what they reported themselves as observing – but each observed something different in the same place.

Having made an attempt to deal with the issue by allowing that each did observe what they said they observed, Kuhn has intensified, not eased, the pressure on his situation. What are we now to say about two scientists, confronted with the same phenomenon – a swinging stone on a string – observing something different: cases of constrained fall and pendulum motion respectively? Once we have got this deep into a hole, the Wittgensteinian philosopher would advise that we stop digging, but Kuhn is not that much of a Wittgensteinian. He keeps digging. Kuhn seeks his way out of his difficulties by declining to treat the question 'do they observe the same thing or something different?' as calling for a yes *or* no answer, and proposes instead to answer it with: yes *and* no, they do and do not see the same thing.

This alone is OK, but Kuhn thinks he now needs an account of perception/observation. He adopts what we will call a 'two moments doctrine'; in a philosophical terminology Kuhn does not himself use, this involves the staple of empiricist thought, the 'given' and its interpretation. Kuhn wants to escape from the untenable consequences of belief in a given – this is what his terminological contortions are about – but often, as here, he falls back into thinking that *some kind* of world, or some set of sensations, *is* given. There *is* only one physical world out there, and Kuhn (with virtually everyone else) wants to say that *this* does not change when scientific theories do, and he never suggested otherwise. *Therefore* there is one and the same natural world, having its natural effects on the scientists observing it, and, since they are biologically similar creatures, affecting them in much the same way. *There is* one and the same shining light up there in the sky that scientists from different astronomical paradigms *do see*. So each sees something, and in these terms, they both see *the same thing*. However, we cannot say in a scientifically neutral way what each sees, just because the identity of the (source of) light is what they are disputing. What each can possibly say they see in scientific terms is not at all the same thing, and whether one says a planet is observed and the other that it is a star will depend entirely on their respective scientific traditions. *Without the appropriate tradition*, one cannot say that one sees a planet or a star, and what, therefore, the scientist 'actually observes' is *a composite*, made up of the input from nature, and the input from the paradigm.

Kuhn therefore feels driven to say this: 'Until that scholastic paradigm was invented there were no pendulums but only swinging stones for the scientist to see. Pendulums were brought into existence by something very like a paradigm-induced gestalt switch' (120); but is puzzled enough by his own expressions to have to ask, 'Do we, however, really need to describe what separates Galileo from Aristotle . . . as a transformation of vision? Did these men really *see* different things when *looking* at the same sorts of objects. Is there any legitimate sense in which we can say that they pursued their research in different worlds?' (120).

Kuhn feels forced to continue with these strange locutions, though 'acutely aware of the difficulties I am creating' in so doing. He sees no choice but to reaffirm that 'though the world does not change with a change of paradigm the scientist afterward works in a different world. . . . I am convinced that we must learn to make sense of statements that at least resemble these' (121).

Still he does not convey a sense of a clear and confident understanding of what he is using these strange locutions to say. Kuhn is perhaps clearer on what he is trying to get away from than of what he is trying to put in its place, but maybe he has not yet got far enough away from some of the things that philosophers of science used to say, and as a result is creating a puzzle for himself, imposing strange expressions on himself. Perhaps this is substantially a good thing. Kuhn's locutions are, he thinks, forced on him by his desire to avoid the language of the largely discredited empiricist tradition, and (more generally) of the 'received view' in the philosophy of science. Kuhn is trying to *avoid* the doctrine of 'the given'. This is something that he has *in common* with most major recent philosophers, notably Sellars and Davidson (though Davidson misses this agreement, and so misses Kuhn). Our point is that: Kuhn has not *found* a way of avoiding the myth of 'the given' that does not yield new philosophical perplexities.

The empiricist tradition had not depended on the 'hypothetical fixed nature' but rather 'fixed experience' as the 'court of final appeal' in its analysis of scientific dispute. The notion of an 'observation language' distinct from the 'theoretical language' of a science is meant in philosophy of science to be what describes scientists' experience, describing what they observe in a way which is 'neutral' between competing theories.[17] Rival theories can be compared with experience described in the observation language, a language that is independent of both their terminologies. But, following Kuhn's arguments, this cannot be true – we will see, in Copernicus' case, how deeply (in Kuhn's view) the 'interpretation' goes relative to the observation. Indeed, given the 'doctrine' of the 'two moments of perception', the interpretation penetrates the observation. (If the two moments are only notionally separable, this is another way in which Kuhn genuinely *endeavours* to free us of the myth of the given.) Kuhn is arguing that if one is going to talk about anything that might be called scientific observation one cannot effectively disentangle the given from its interpretation. What counts as an observation in science is, for Kuhn, a composite of a given input from nature and of the interpretive capacity supplied by training in a scientific paradigm.

Furthermore, the 'given' in the laboratory should actually be termed 'the collected with difficulty' (126). It takes a full and deep absorption of the paradigm, plus the acuity and intelligence required to practise normal science effectively, before a scientist is in any position to make observations that are of interest to other scientists on the frontiers of research. Near those frontiers especially, there is no way that scientists' experiences can be decomposed into any more elemental form – direct reports of bare sense experience, divested of all the paradigm-based learning that is the precondition of the scientist being let loose in the lab at all (126–8) – that allows specification of what they observed in a way which does not draw on the 'theoretical' language of their paradigm. Kuhn insists that scientists are right to treat such things as pendulums and oxygen as 'fundamental ingredients' of their immediate experience, and not as theoretical construals of any more basic experience. This means though that there is no stable or common experience that can be appealed to in adjudication between paradigms either, because the experience itself is partially composed of the interpretation. Here Kuhn is most definitely not saying that one *first* perceives an input from nature, and *then* gives it an interpretation. What he is saying is: the scientist's perception is composed of both the input from nature and the interpretation. For the purposes of scientific observation, then, the experience cannot possibly be extricated from the interpretation, *and therefore* the scientists cannot have recourse to the pure, extricated element of 'the given' in their experience. Thus, the observational materials – the data – available to scientists cannot be said to remain stable across changes of paradigms, because the observational materials are *irreducibly* characterized in terms which draw on a paradigm. Thus: 'The data themselves had changed. [W]e may [therefore] want to say that after a revolution scientists work in a different world' (135).

Kuhn denies that the staple of the empiricist approach, a distinction between observation language and theory language, can be of use in understanding scientific change, but he has not thereby entirely abandoned the distinction, between the given and the interpreted, that the empiricist's distinction was meant to express. He has not succeeded in abandoning the myth of the given. He has retained in some measure the distinction between the given and the interpreted, but shifted the point at which the dividing line between them can be drawn. The line is no longer drawn between what is given in raw perception, providing 'sense data' and the interpretation that is then applied to that 'given', for, on Kuhn's account, the given and the interpreted are combined in perception, in what can meaningfully be called – in respect of *scientific observation – sense data.* Our view is that this is progress; though better still to find a way to *give up* the whole idea of 'the given'. However, it is retained, and its retention provides encouragement to maintain that two disputing scientists both do make genuine observations, for while they are observing something different they are nonetheless observing something that is the same: the same bits of the natural world enter into their perceptions in each case, and it cannot then be said that one scientist does observe something, and that the other does not observe anything at all. Both observe something that is real, but the way they experience what they observe differs (because of their different backgrounds). Therefore, *neither* observes the external world of nature *in itself*, but it cannot, on those grounds, be said that they make invalid observations. At the same time, the different experiences that their observations generate cannot be invidiously contrasted, for there is no way of saying that one observes *nature in itself* more closely or accurately than the other. Therefore, there is the temptation, to which Kuhn succumbs, to say that the respective scientists each observe something real, that the world observed by one is no more or less real than the other, and that, therefore, each observes a reality, each lives in a real, but different, world. The worlds found in the respective scientists' experiences are not, however, to be confused with the real world *in itself*: one way in which to express this (in accord with the Kantian connection) is to say that the scientists inhabit *phenomenal realities*, that they occupy different *phenomenal* worlds.

What the beginning scientist learns to see is determined *jointly* by the environment and the particular normal-scientific tradition that the student has been trained to pursue (112, 113). If *part* of that perception is dependent on changes, namely, the contribution of previous 'visual conceptual experience' because important – especially conceptual – parts have been withdrawn and replaced, then the scientist's perception must be re-educated, which among other things means acquiring a new Gestalt. This is another reason why discussion between different paradigms is always *at least slightly* at cross-purposes.

These considerations, whatever the merits of particular forms of words via which Kuhn (and we) try to adduce them, are clearly meant to fill out the analogy with revolution, and with the absence of any authority to adjudicate between the two sides. Kuhn has eliminated the empiricist's only possible 'final authority' – raw sense data – and, given the general pre-eminence of empiricism in Anglo-American philosophy of science, has virtually deprived philosophy of science of the notion of any authority external to the scientific schemes themselves.

The idea of a 'fixed nature' can play no part in what Kuhn envisages as a historical understanding of scientific change. The idea of 'world changes' is not, itself, a denial that there is any such thing as a 'fixed nature', compatible with a superficial interpretation of the claim that when the world according to science changes, nature itself changes with it. 'The world out there' retains a certain constancy throughout: stars do not transmute into planets, mixtures do not mutate into compounds. However, while one may be confident that nature does not alter with each paradigm shift, there is no way of saying *what it is that remains constant* throughout. The question of what it is that remains constant throughout is the very question over which paradigms are contesting each other: is it, and was it always, a planet or is it really a star; is it, and has it always been, a compound, not a mixture? Thus, while it might be accepted that there is a 'fixed nature' in this sense, it is not one that can be appealed to as an independent point of reference against which competing paradigms can be compared, that can play the kind of role as a final authority that philosophy of science requires of it. To try to say, in any substantive way, what is actually 'out there' as the object of scientific contest would be to make an (at least tacit) identification with one side of the argument or the other. A 'fixed nature' is a useless ornament on, not a working addition to, Kuhn's model of scientific change. He is saying that the idea of a 'fixed nature' is only an idealization and plays a distorting role in understanding these problems – that is, the problems of the philosopher and the historian, not necessarily the problems of the scientist themselves. His appreciation that he is resorting to a 'strange locution' indicates Kuhn's unease.[18]

The effects of incommensurability

(On sections XI and XII of *SSR*, 'The invisibility of revolutions' and 'The resolution of revolutions')

Incommensurability – which literally means 'cannot be compared by a common measure' – has been involved in the argument about the discontinuities between successive paradigms in the discussion both of paradigm shifts and of the 'world changes' issue. There is no independent, neutral standard against which two competing paradigms can respectively be compared. They can only be compared with each other, though the idea of doing that is complicated, and does not provide the kind of comparison that philosophers of science need – one that adjudicates which of the two is most closely in accord with empirical reality. The idea that philosophers can make that kind of comparison, standing transcendentally outside the hurly-burly of scientific dispute, is being exploded.

Perhaps the most important of *all* Kuhn's thoughts with respect to philosophy of science is on whether economics is the most successful social science because economists know more about science and truth than, say, sociologists do. Or is it that they know more about economic matters than sociologists do about society? Generalized, this question asks: could philosophers, who know about 'truth' and about 'science', possibly know more about what scientists should do than the scientists, who know, after all, nothing about 'truth' and 'science' in the sense that philosophers do, for the former only know about black holes, or quarks for example. From Kuhn's point of view this is a rhetorical question.[19]

We have not yet paid much attention to the *second* respect in which incommensurability matters to Kuhn, and that is as a source of misunderstanding between competing scientists. Far from rival scientists taking the true measure of each other's positions, Kuhn maintains, they often misunderstand each other's views, and, as a result, in scientific revolutions sometimes do not really critically engage with each other, just talk past each other – and after the revolution the losers are simply forgotten: 'communication across the revolutionary divide is inevitably partial,' he remarks (in)famously, at one point (149). To see just what Kuhn means here, however, let us see what else Kuhn actually says. He remarks:

> at times of revolution, when the normal-scientific tradition changes, the scientist's perceptions of his environment must be re-educated – in *some* familiar situation he must learn to see a new gestalt. After he has done so, the world of his research will seem, *here and there*, incommensurable with the one he had inhabited before. That is another reason why schools guided by different paradigms are always *slightly* at cross-purposes. (112; emphasis added)

And now a lengthy quotation, because the issue of incommensurability is so important. First:

> all historically significant theories have agreed with the facts, but only more or less. There is no more precise answer to whether or how well an individual theory fits the facts. But questions much like that can be asked when theories are taken collectively or even in pairs. It makes a great deal of sense to ask which of two competing theories fits the facts *better*. Though neither Priestley's nor Lavoisier's theory, for example, agreed precisely with existing observations, few contemporaries hesitated more than a decade in concluding that Lavoisier's theory provided a better fit of the two. (147; emphasis in original)

And, shortly after:

> If there were but one set of scientific problems, one world within which to work on them, and one set of standards for their solution, paradigm competition might be settled more or less routinely by some number of problems solved by each. But, in fact, these conditions are never met completely. The proponents of competing paradigms are always *at least slightly* at cross-purposes. Neither side will grant all the non-empirical assumptions that the other needs in order to make its case. Like Proust and Berthollet arguing about the composition of chemical compounds, they are bound partly to talk through each other . . . we have already seen several reasons why the proponents of competing paradigms fail to make *complete* contact with each other's viewpoints. Collectively these reasons have been described as the incommensurability of the pre- and post-revolutionary normal-scientific traditions. . . . In the first place, the proponents of competing paradigms will often disagree about the list of problems that any candidate for the paradigm must solve. Their

> standards of their definitions of science are not the same. Must a theory of motion explain the cause of the attractive forces between particles of matter or may it simply note the existence of such forces? Newton's dynamics was widely rejected because, unlike both Aristotle's and Descartes's theories, it implied the latter answer to the question. When Newton's theory had been accepted, a question was therefore banished from science . . . more is involved, however, than the incommensurability of standards. Since new paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had employed. But they seldom employ these borrowed elements *in quite the traditional way*. Within the new paradigm, old terms, concepts, and experiments, fall into new relationships one with the other. The inevitable result is what we must call, *though the term is not quite right*, a misunderstanding between the two competing schools. . . . to make the transition [from Newton's] to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole. (147–8; emphasis added)

And, finally,

> the third and most fundamental aspect of the incommensurability of competing paradigms [is that there is] a sense that I am unable to explicate further, [in which] the proponents of competing paradigms practice their trades in different worlds. One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. In one, solutions are compounds, in the other mixtures. One is embedded in a flat, the other in a curve, matrix of space. Practicing in two different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at *has not changed*. But *in some areas* they see different things and they see them in different relations one to the other. That is why a law that cannot even be demonstrated to one group of scientists may *occasionally* seem intuitively obvious to another. Equally, it is why, *before they can hope to communicate fully*, one group or the other must experience the conversion that we have been calling a paradigm shift. (158; emphasis added)

The reason for these extensive quotes is that they show that Kuhn's *SSR* formulations of the idea of incommensurability do not themselves settle a key issue, namely, just how great are the misunderstandings between scientists? Is it the case that scientists from different paradigms are hardly able to make sense of each other *at all*, that they misunderstand each other on almost every point, no matter how small or great? One could read these passages as licensing such a reading, yielding a picture of scientific revolutions as almost farcical episodes of mutual incomprehension. *On the other hand*, one can read the same passages as claiming something much more modest, and less far-reaching, namely, that incommensurability means that failure of mutual understanding is an exigency of revolutions, that there almost certainly will be times when minds do not meet, but that these will be occasional, on particular points, either major or minor. Kuhn eventually clarified that the latter reading was his intended and preferred one.

From these remarks, one thing is however certain: Kuhn is not saying that incommensurable theories *cannot be compared* – what they can't be is compared in terms of a system of common measure. He very plainly says that they can be compared, and he reiterates this repeatedly in later work, in an effort (mostly in vain) to avert the sometimes catastrophic misinterpretations he suffered from mainstream philosophers and postmodern relativists alike. Integral to – though tacit in – his point in saying this is that paradigms are characteristically *complex constructions* and that comparison of them is a multidimensional affair; he is considering theories as ('conceptual') schemes rather than, for instance, unconfirmed conjectures. Thus, it is not as if a theory can be individually accepted or rejected simply on the basis of fitting the facts, because, as we have explained before, in Kuhn's view any scientific scheme that has had any support both will and will not 'fit the facts' (79–83). There are always abundant – but largely inconsequential – anomalies to any theory, but there are also *lots* of instances that – in its terms – fit the theory too. Hence, the question cannot be asked, *does this theory fit the facts*, the only meaningful question is: does this theory fit the facts *better than this other one*. So, the two can be compared, and it can be *eventually* fairly conclusively decided by the science – as in the case of Priestley versus Lavoisier – that the latter's approach fitted the facts better than the former's. This offers no comfort to Whig historians or traditional philosophers of science. To say that Lavoisier fitted the facts better is *of course* what Lavoisier's heirs (that is, all of us, in so far as we have chemical knowledge) will say.

The importance of incommensurability for Kuhn is not that it presents a problem for science. It does not. The misunderstandings between scientists are virtually never mutually experienced as such;

it is not that scientists will be brought to a grinding halt by the realization that they really cannot understand each other, and that they need to establish reciprocal understanding before they go further. It is important in other connections for Kuhn that the scientists do not usually realize that they are talking at cross-purposes. They think that they are in straightforward disagreement, that they understand the other's position well enough, and that they can see just what is wrong with it. Thus, it may be on the basis of (some) misconceptions about the usurped paradigm that its replacement wins out, but the verdict remains irreversible.

Incommensurability presents a problem only for the philosophy of science. Kuhn is denying – as a cumulative result of the arguments about revolutions, world changes and phenomenal worlds – that it is possible to compare scientific theories in the way that philosophers of science imagined they could, that they can be matched – by those with a only a spectator's interest in science, such as philosophers – against each other in some decisive way. Kuhn is denying that two theories can be jointly compared against any independent, neutral standard or set of facts, and also – and this is the distinctiveness of the notion of incommensurability – that the two theories can possibly be lined up against each other as though disagreeing on the answers that they give to a list of common questions, allowing us to assess which gives the right and which gives the wrong answer to each question.

Even to speak of Kuhn 'denying' claims in the philosophy of science may be misleading. For Kuhn aimed usually to establish the *nonsensicality* of the claims he was rejecting. In roughly the following sense: he aimed to get his interlocutors to see that there was nothing that they really could want to mean by a point-by-point comparison of theories across major changes in science; that they would fail to understand the predecessor science as science, and/or simply fail to understand it, if they insisted upon such comparison, or 'translation' (discussed in chapter 5; see also 'Commensurability, comparability, communicability', in *RSS*, for Kuhn's attempts to persuade his opponents (in this case, Kitcher, and, by extension, Davidson and Quine) that they should give up their anti-incommensurabilism when they realize that it makes a complete nonsense of any attempt to render phlogistic chemistry in terms of our own, and still be understanding phlogistic chemistry).

If a point-by-point comparison is imagined as a matter of placing the assertions made by the empirical part of rival theories in 'conjunction' *with each other* and with cogent evidence bearing on the truth of each, then point-by-point comparison would involve (for instance) comparing Newton's answer to the question of what was the cause of attraction between particles with Descartes's or Einstein's answers to these questions (see *SSR*, 148). Aristotle says *this* is the cause of the attraction, Newton says *that* and Einstein says *the other* – now which, if any, is the nearest to being right?

However, Newton does not give *any* such cause but not because he overlooks doing so. Newton does not accept that his predecessors have identified the cause, but not because they identified the wrong cause and he is now about to identify the right one.[20] He disagrees with them over whether you *need* to answer the question, whether your account is significantly incomplete if you don't feature a cause of attraction. And that of course brings up in turn other differences between Newton and the rest. There is comparison here, but it is not 'point for point'. For the difference between Newton's theory and its predecessors – and its successor(s) – is not that each has a different answer to the same question, but that Newton's scheme *leaves no space* for the question that the others ask.

We mentioned the need to be wary of the use of the term 'coincide' to represent the point at which there are empirical results that can be compared between paradigms, even when paradigms look as though they are in respects directly comparable in a point-to-point way. This may be a false impression. It is one of the key suppositions of Kuhn's whole enterprise that resemblances between paradigms are often superficial only – the paradigms only *seem* to be in agreement, and the scientists adhering to them are making assertions that only look as if they are mutually empirically contradictory, and thus straightforwardly comparable (if one assertion contradicts another, then they are at least in the same ball park).

Take the case of Kuhn's remark (102) that mass *à la* Newton and *à la* Einstein can be measured in the same way at low velocity but 'must not be conceived to be the same'. This is surely illustrative of what Kuhn means by incommensurability. It might be objected against us (and Kuhn): If the measurements given, i.e. the quantities specified, are specified in the same units before and after the Einsteinian revolution and (in those units) give the same values, then how could they not be the same? Certainly Kuhn will not deny that these units and measures appear identical, but he is arguing that this is just the kind of confusion that motivates his whole enterprise – they may look as if they are just the same, but one had better not treat them as if they actually are. The difference is not in the values yielded, but in what it is that the values yielded *are measuring*. It is perfectly correct, from an Einsteinian point of view, to say that the correspondence in quantifications with Newtonian measurements is good enough *at low velocities*. However, this is itself an Einsteinian, not a Newtonian way of talking. To say that Newtonian calculations are correct when measuring mass at low velocities is, in Newtonian terms, to misrepresent what Newtonians were *doing*. Mass is a fixed quantity in the Newtonian scheme, and so when it has been correctly measured or calculated, that is what mass is. The velocities at which mass was measured were not conceived in the Newtonian scheme as 'low' velocities, for there was no physically relevant conception of high velocities – such as approaching the speed of light – with which to contrast them.

So, though it is perfectly correct in Einsteinian terms to say that the Newtonian scheme works perfectly well for masses measured at low velocities, this is not a correct characterization of what, in Newtonian terms, was going on. When Newtonians use 'mass', its 'grammar' is of a fixed quantity which is conserved. Just think of the difference between 'football' in 'association football' and 'rugby football', and between 'goal' in the former, and 'goal' in the latter (and in 'American football', too). Going 'over the bar' in one qualifies it as a goal, in the other, it disqualifies it as one. Wrestling with 'reference' does not really help, we think; the difference is in what you can say and mean in the language and in the way in which expressions fit in with other expressions: *in the context* of rugby football, 'It's over the bar' means 'He's scored', in that of association football, it means 'He's missed'.

The idea of Newton and Einstein being rivals is a *restricted* one if it is taken to mean that one must make contradictory claims, when, in fact, the rivalry is of a different kind – the Einsteinian calls for reformation of the Newtonian vocabulary so as to raise questions that couldn't be asked within Newton's scheme, and thereby displace Newtonian usage, so that for instance Newtonian 'mass' becomes 'mass at low speed' (which is certainly not what it originally meant). Thus, it is OK to say that 'the two theories remain conflicting accounts of the same thing'[21] so long as we remember that this means they are conflicting accounts of mass, and that mass in Einstein is by no means simply the same thing it is in Newton! There is no contradiction in saying this – if one understands Kuhn's 'position' appropriately.

In sum: Newton and Einstein are definitely in *competition* with one another – that's why you can *only* have one of them, not one as a special case of the other. But, to help avoid confusion we must insist that Newton and Einstein do not *contradict* each other – *not* because they agree (at all), but because the nature of their 'disagreement' involves them necessarily talking across each other (see 149 and 98).

So, what is plain about Kuhn's presentation of 'incommensurability' is that it raises issues that have to do with the mutual comprehensibility of rival paradigms to the scientists who are involved, in a partisan way, with them.[22] Clearly, incommensurability has considerable bearing with respect to the actual, not the *perceived* understanding of some scientists by others. If an Einsteinian scientist employs Newtonian calculations to determine mass-at-low-velocities, this is not actually using the Newtonian methods, since those do not calculate mass-at-low-velocities, but just calculate mass. *If* the scientist is indeed operating in this way, then she or he is working on a misconception, and is using not the pure Newtonian ideas, but only those ideas reconstrued in Einsteinian terms. Of course, it makes no material difference to the scientist's science (only to their image of their activity, their sense of their position in history, etc.) that this way of calculating is used as a convenience, or that in order to do this it is stripped of its specifically Newtonian aspects. It is not, either, at all possible for the Einsteinian to do otherwise. Einsteinians know that it makes a difference whether mass is being accelerated through low velocities or at very high ones, and they cannot competently operate 'the Newtonian techniques' save on the basis of this knowledge. There cannot be the sense, for Einsteinians, of calculations of mass, period, but only of calculations of mass-at-low-or-high-velocities. As used by Einsteinians, these calculations are *in the historical sense* not-really-Newtonian, only 'partially' Newtonian. Thus, the claim that Einsteinian physics has incorporated Newtonian physics as a special case is erroneous, for it has rejigged Newtonian physics or, in another metaphor, cannibalized it; or, as we ourselves would put it,[23] has *reconfigured the grammar*.[24] Einsteinian physics is a different language-game: though of course 'language game' is a term involving the *activities* with which these words are bound up.[25] Science isn't just words.[26]

Scientific development

(On section XIII of *SSR*, 'Progress through revolutions')

We now come to the last of the strange or outrageous things that Kuhn says in *SSR*, and one of the main topics which has been provocative of most subsequent debate, namely, the sense in which science can be regarded overall as ‘making progress’. In a way, Kuhn’s previous discussion has straightforwardly and effectively answered the question as to the sense in which science can be said to make progress – his description of the way in which paradigms replace one another (in concert with his earlier description of the accretion of knowledge in science under normal conditions) itself *describes the way in which science can be said to make progress.* What Kuhn has already done is, in a way, to describe how the scientific specialists determine that they are making progress by showing how they opt for one paradigm over another. Once this has been understood, further questions about progress – and progress ‘towards Truth’ – are rendered empty.[27] By excluding ‘truth’ – as it has traditionally functioned in the philosophy of science – and making, rather, such observations as those listed, Kuhn is effectively rejecting the idea that science is the fulfilment of the aims of metaphysics. Realists are apt to treat science as though it has delivered or will deliver what metaphysics sought for, namely a depiction of what there ultimately or finally really is, and their question then is how close is science to rendering this final, ultimate description? Kuhn doubts that this metaphysical notion is needed in characterizing the results of science, meaning that he cannot pose, let alone answer, the question that the Realists would want him to.

Now, Kuhn holds that the assertion that ‘science makes progress’ is to a considerable extent a circular one, for something does not qualify as a true science unless it makes progress. But how can such a claim be justified? If we are to accede even to this claim, do we not want some independent measure of progress to ensure that ‘making progress’ is different from ‘just substituting one paradigm for another’? But this is just what we cannot have. If we understand the relationship between scientific work and the scientific community then ‘the phrases “scientific progress” and even “scientific objectivity” may come to seem in part redundant’ (162). One can’t – at least Kuhn says he can’t, and doesn’t believe he needs to – find a way, independently of that scientific community, of saying whether a science is making progress. It is that ‘independently of that scientific community’ which is the crucial component. It is not as if scientists decide to replace one paradigm with another and then ask themselves whether, having done so, they have made progress. A paradigm is being displaced because the relevant scientists have decided that some alternative to it *is progressive,* and have decided this on the basis of the considerations integral to their science itself. Thus, viewed from within any scientific community, with its set of problems, strict standards, checks and balances, the result of its work *just is* progress.[28] This is not quite a matter of tautology, but of understanding the extent to which, for any science, *the content* of any notion of progress is provided by the practice of the science itself, and by the way that criteria for progress just are those which count for and against candidate paradigms. A mature science possesses 'unparalleled insulation' from external standards because the application of meaningful standards of assessment depends upon a familiarity with the technical complexities of the science. There is an intense collective assessment of work but this is done by the *cognoscenti*. The science decides both what the worthwhile problems are, and what can count as a solution to them. How can someone who cannot understand just what, scientifically, is going on (such as a 'methodologist of science' who is not actually a practising scientist in the field) contribute to this?

Further, saying that the scientific community judges progress overlooks the fact that the scientific community itself is defined through the replacement of paradigms, through the (eventual) marginalization and elimination of those remaining unregenerately attached to a sidelined paradigm. The scientific community is made up of the (current) victors, and they control the history of the science, automatically regarding their triumph as the measure of progress. This is not, however, to offer any cynical view of the matter as though might simply makes right, for to say that would be to ignore the fact that these are *scientific* revolutions, and that dispute over the *scientific* merits of the rival paradigms is the stuff of their opposition, that the triumph consists in the reconfiguring of the science itself. So, we do have scientific revolutions, and Kuhn *can* be called 'the philosopher of scientific revolutions', although he could equally well, perhaps even more so, be called 'the philosopher of ordinary science'. But this gives no support to those in sociology who think that now Sociology (or Philosophy) can lord it over the sciences: and it gives no support to those in the Science Wars who would like to *overthrow* the epistemological authority of science. Kuhn was right to be deeply wary of his 'followers'.

We might gesture at how progress in science can be indicated just in the expansion of the sciences, their division of labour, the dense content of their textbooks, and the sophisticated level of their puzzle-solving. Such gesturing is gross and scientifically entirely superficial, and therefore quite different from being able to say, in any *particular* instance of paradigm substitution (in any situation where the question as to where to stand in the science is still 'live'), what the specific features of the transition are which merit the description of the substitution as one of progress, unless one is in possession of considerable competence in the technicalities of the science in which the displacement is taking place.[29] To engage substantively in the comparison of the two paradigms would be *to engage in the scientific argumentation itself*, not in some separate philosophical adjudication of scientific argumentation.

So Kuhn is more 'moderate' than he has been seen by friends and foes in philosophy and the social sciences; but he is more 'radical' in a wholly different direction. Radical in the way that Wittgenstein, Peter Winch, much ethnomethodology (and much Foucault) are radical.