John Worrall

# Scientific Revolutions and Scientific Rationality: The Case of the "Elderly Holdout"

## 1. Introduction: The Rationality of Scientific Change and the "Elderly Holdout"

Many parents feel that there is no longer enough emphasis at elementary school on the "three rs". No one could complain that recent philosophy of science has failed to emphasise *its* "three rs": revolutions, rationality, and realism. In this paper I risk being a pain in the rs by returning to the well-worn topic of the rationality of revolutionary scientific change (a topic that in turn raises the problem of scientific realism). I concentrate on what appears to be a particularly sharp challenge to the claim that the development of science has been a rational affair, and try to use that challenge to clarify some aspects of the claim.

The (well-known) challenge stems from Kuhn's *The Structure of Scientific Revolutions* and concerns fundamental theory change. Kuhn's view, remember, was that an individual scientist's switch to a new paradigm is a "conversion experience" that "cannot be forced": reason – in the form of the "objective factors" of traditional philosophy of science (empirical accuracy, simplicity, and the like) – certainly plays a role but it *never dictates* the switch to the new paradigm.[1]

Consequently, on Kuhn's view, it is never actually irrational to *resist* the switch. This is true even of those "elderly holdouts" who continue to resist after pretty well all their colleagues have switched allegiance. There is, claims Kuhn, *no* "point at which resistance becomes illogical or unscientific." An elderly holdout, like Priestley "holding out" against Lavoisier's oxygen theory, may infuriate

This paper was originally written for the NEH Institute on Philosophy of Science at the University of Minnesota, and an early version was delivered there in April 1986. I should like to thank Philip Kitcher for the invitation to contribute to the Institute, and him and Wade Savage for their hospitality. Early versions were also delivered in Pittsburgh and in Edinburgh: I thank Adolf Grünbaum, Nick Rescher, David Bloor, and Steve Shapin for helpful, critical comments. I have also benefited from criticisms of previous drafts of the paper by my colleagues John Watkins and Elie Zahar.

his colleagues by his stubbornness, but they have no right to brand him irrational or unscientific:

> Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition . . . is not a violation of scientific standards. (1962, 151)

But if Priestley did not violate scientific standards in resisting the oxygen theory of combustion, are we not forced to say that modern day creationists, for example, equally do not "violate scientific standards" in resisting Darwinism? Indeed, if reason never dictates a preference for a new theory (even once the revolutionary dust has largely settled) *are there any* scientific standards to violate? Worries like these have frequently been expressed by Kuhn's critics. And Kuhn himself has almost equally frequently, but generally unsuccessfully, attempted to lay such worries to rest—claiming that, when properly understood, his views on this particular matter are rather less challenging to philosophical orthodoxy than might meet the eye.

The present paper attempts to clarify this confused situation via a case study of one elderly holdout. The historical details will, I hope, supply *both* a test of Kuhn's views *and* the means of clarifying at any rate some aspects of the general claim that the development of science has been a predominantly rational affair.

## 2. Sir David Brewster and the Wave Theory of Light

Obviously a case study of a holdout requires a scientist to do the resisting and a scientific revolution to resist. The scientific revolution I have chosen is the early nineteenth-century revolution in optics, which saw the wave theory of light emerge triumphant over its previously entrenched Newtonian emissionist rival. Among the handful of significant holdouts against this revolution two stand out: Jean-Baptiste Biot and David Brewster. For various reasons (not least because he published the more explicit accounts of his reasons for holdingout) I have chosen Brewster.

First, his credentials. Brewster was certainly no peripheral or negligible figure. He was the discoverer of a great many of the properties of polarized light, especially elliptically polarized light; he discovered "Brewster's law," relating the polarizing angle and refractive index of transparent substances; he discovered a whole new class of doubly refracting crystals, the "biaxal crystals"; he discovered that ordinary unirefringent matter can be made birefringent by the application of mechanical pressure; and he discovered the hitherto unknown general phenomenon of selective absorption.

So Brewster was a significant scientist, and he was certainly some sort of holdout. In 1831 another knight of the realm, Sir George Biddel Airy, published a

*Mathematical Tract on the Undulatory Theory of Light*, which began with the words:

> The Undulatory Theory of Optics is presented to the reader as having the same claims to his attention as the Theory of Gravitation; namely that it is certainly true. . . . (Airy 1831, vii)

In that same year, Brewster presented a "Report on the Present State of Physical Optics" to the British Association for the Advancement of Science, in which he asserted that the undulatory theory is "still burthened with difficulties, and cannot claim our implicit assent," (Brewster 1833a, 318). Two years later he reported:

> I have not yet ventured to kneel at the new shrine [that is, the shrine of the wave theory] and I must acknowledge myself subject to the national weakness which urges me to venerate, and even to support, the falling temple in which Newton once worshipped. (1833b, 361)

Although Brewster's "official" published position was usually one of (alleged) theoretical *neutrality*, there can be no doubt that he retained to the end of his life a soft spot for the Newtonian theory; that is, for the *ancien régime* in this revolution.[2]

The main features, as I see them, of Brewster's attitude towards the wave theory and its Newtonian rival are the following.

## (2a) Brewster's Acceptance That the Wave Theory Was Empirically More Successful

Brewster time and again expressed great admiration for the theory and fully acknowledged that it had enjoyed unparalleled explanatory and predictive success:

> I have long been an admirer of the singular power of this theory to explain some of the most perplexing phenomena of optics; and the recent discoveries of Professor Airy, Mr Hamilton and Mr Lloyd afford the finest examples of its influence in predicting new phenomena. (1833b, 360)[3]

Elsewhere (1833a, 318) he talked of the "theory of undulations, with all its power and all its beauty."

## (2b) Brewster's Belief That the Wave Theory, Despite its Empirical Success, Could Not Be True

Despite its empirical success, Brewster quite explicitly held that the wave theory was, when considered as a "physical theory," false. He produced two main arguments in this connection.

The first was of a general methodological kind: namely that the fact that the wave theory explained and predicted a whole range of phenomena did not establish it as a physical truth. Instead:

> Twenty theories . . . may all enjoy the merit of accounting for a certain class of facts provided they have all contrived to interweave some common principle to which these facts are actually related. (1833b, 360)

Brewster admitted that the wave theory's predictive success did indeed mean that:

> it must contain amongst its assumptions (though as a physical theory it may still be false) some principle which is inherent in . . . the real producing cause of the phenomena of light. . . . (Brewster 1838, 306)

But he went on to make his own opinion clear that, as a physical theory—as, that is, a fully realistically interpreted claim about the universe—the wave theory is indeed false. In particular, a full, realistic interpretation committed the wave theory to "an ether, invisible, intangible, imponderable, inseparable from all bodies, and extending from our own eye to the remotest verge of the starry heavens" (ibid.). This was always too much—or perhaps too little—for Brewster to swallow.

Whether or not Brewster would have subscribed to the strong view—that a theory may be totally empirically adequate and yet still not "physically" true—is not clear. It is not clear precisely because his second argument was that the wave theory's explanatory and predictive success, though impressive, was definitely limited. Indeed on Brewster's view there were two important areas in which the wave theory failed, and failed badly.

These were dispersion and selective absorption. Since similar methodological conclusions are drawn from the two cases, I shall just concentrate on the second phenomenon, which Brewster in fact discovered. Brewster found that if sunlight is passed through certain gases and then dispersed in a prism, its spectrum is marked by a whole series of sharp, dark absorption lines. For example, he noted more than a thousand such lines in the spectrum of sunlight that had passed through "nitrous acid gas." This implies that such media absorb particular elements of the solar spectrum (or at any rate very narrow ranges of such elements), while transmitting other elements "infinitesimally close" to the absorbed ones. This was not a question of the refutation of a particular version of the wave theory—instead it marked a difficulty for the whole approach. This was because, whatever the details, the *general* story that the wave theory was forced to tell concerning this phenomenon was, as Brewster emphasized, so farfetched. Let's concentrate for sake of illustration on just one dark line in the spectrum of light transmitted through "oxalate of chromium and potash." The wave theory is forced to say that the ether within that gas:

> freely undulates to a red ray whose index of refraction, in flint glass, is 1.6272, and also to another red ray whose index is 1.6274; while . . . its ether will

> not undulate at all to a red ray of intermediate refrangibility whose index is 1.6273! (1833b, 362)

In other words, whatever detailed account may eventually be given, a tiny difference in the length of a wave must be supposed to produce a black-and-white change from free passage through the ether within the gas to no passage at all. Brewster pointed out (1833a, 321) that:

> There is no fact analogous to this in the phenomena of sound, and I can form no conception of a simple elastic medium so modified by the particles of the body which contains it, as to make such an extraordinary selection of the undulations which it stops or transmits. . . .

He quite reasonably concluded that the phenomenon "presents a formidable difficulty to the undulatory theory." As we shall see, he also felt that this was an area where the emission theory scored over the wave theory, since it was easy to produce at any rate outline suggestions for how selective absorption might be accommodated within the emission approach.

### (2c) Brewster's Disagreement with the Wave Theorists Over the Way Forward

So far as the friends of the wave theory were concerned, Brewster's claims, considered simply as claims about the *present* version of the wave theory, were perfectly reasonable and indeed completely uncontroversial. Both Airy and Baden Powell, each of whom responded directly to Brewster, freely acknowledged that the wave theory *as it stood* had no adequate explanation either for dispersion or for selective absorption. Airy and Powell both insisted that the main question, and their main disagreement with Brewster, was about the appropriate *response* to those admitted difficulties. In other words, the main disagreement was *heuristic*.

Brewster does *seem* to have believed that, despite all the difficulties that had mounted against it, there was life left in the Newtonian emissionist theory. He echoed Herschel's sentiment expressed some ten years earlier that, were sufficient talent and energy invested in the emission theory, it might yet turn the tables of scientific superiority on its undulatory rival. Correspondingly, Brewster felt that the near monopoly of theoretical talent that the wave theory had attracted by the 1830s was unhealthy. He believed that the experimental difficulties he had pointed to were sufficient to justify a less committed, more theory-neutral approach than the one that then dominated optics in both Britain and France.

Airy and Powell both argued that, on the contrary, the only reasonable response to the empirical difficulties that beset the wave theory was renewed commitment to it, aimed at solving those difficulties within the general approach. They each argued as follows. First, the wave theory was already overwhelmingly superior in terms of empirical support. Airy and Powell both accepted that there were phenomena (including those that Brewster had highlighted) that the wave theory could not (or could not yet) explain. However they insisted that there were lots of phenomena that the wave theory explained and/or predicted, but that the emission theory could not deal with adequately at all; *and* that there was no phenomenon that the Newtonian theory could adequately explain (let alone predict) that the wave theory could not.[4]

Secondly, while the corpuscular theory had run into difficulty after difficulty and failed successfully to overturn any of them, the wave approach had, on the contrary, already demonstrated the capacity to be modified in the light of experimental difficulties in a scientifically fruitful way. Airy in particular stressed the example of Fresnel's switch from longitudinal to transverse waves.

What had happened in that case was, in outline, roughly as follows. Wave theorists before Fresnel had all assumed that the ether is an extremely rare and subtle fluid—how else could the planets move so freely through it? It is a theorem of mechanics that fluids transmit only *longitudinal* (pressure) waves. (Longitudinal waves are ones in which the particles of the medium oscillate in the *same direction* as the overall transmission of the wave through the medium; an example being a sound wave in air.) Fresnel's own initial theory was indeed that light is a longitudinal wave. However, he and Arago then established experimentally that if, say, the two beams emerging from the two slits in the double-slit experiment are polarized at right angles to one another (by passage through suitably oriented crystal plates), then the interference fringes disappear. It seemed that light beams polarized in mutually orthogonal planes *fail* to interfere (or, rather, fail to produce interference fringes). Neither Fresnel nor any other wave theorist had, at this stage, any theory of the polarization of light. But, so long as the light waves were assumed longitudinal, the precise account of what happened when light is polarized could make no difference. Assuming that the wave theory is *at all* correct, the longitudinal assumption alone means that the disturbances in the two coherent and near-parallel beams (the slits are, remember, very close together) must themselves be near parallel and hence must alternately interfere constructively and destructively for different path differences. The Fresnel-Arago experiment, therefore, put the wave theory into deep trouble. Fresnel took a still deeper breath and switched to the *transverse* wave theory: to the theory that the ether particles oscillate at right angles to the direction of the propagation of light. This yields an easy theoretical account of the process of polarization: the disturbance in an unpolarized beam has components in *all* planes through the direction of propagation; polarization (linear or plane polarization, that is) consists in restricting the disturbance to one such plane. This explained the apparent "sidedness" of polarized beams, and also explained the Fresnel-Arago results. The oscillations in beams that are polarized orthogonally are assumed themselves to be orthogonal. Hence, although the two sets of oscillations certainly interfere or superpose—to produce (in general) elliptically polarised light—they operate at right angles rather than along the same line, and hence can never *destructively* interfere so as to produce fringes. Although it straightforwardly dealt with this difficulty over polarized light, the switch to the transverse theory certainly required a deep breath. This was because elastic media can transmit such waves only if they exhibit resistance to sheer, that is, only if they are *solids*. But how could the planets move completely freely through an *elastic solid* ether?[5]

But whatever the conceptual difficulties, Fresnel's new transverse theory scored stunning empirical successes. Not least when Hamilton showed in 1830 that the transverse theory entails the hitherto unsuspected phenomena of internal and external conical refraction—predictions that were confirmed by Lloyd in 1833. Airy's point about this theoretical shift was that the phenomena of polarized light discovered by Fresnel and Arago were major difficulties for the original version of the wave theory, no less major than the difficulties now cited by Brewster against the new version of the theory. But, in the earlier case, rather than give up the whole theory, Fresnel had modified it and had in this way produced a theory whose empirical virtues Brewster himself now rightly applauded. On the other hand, Airy told Brewster:

> Had Fresnel proceeded as you (apparently) would wish us to proceed, the undulatory theory would not now have existed. (1833, 423)

And so the major predictive success would have been missed.

The wave theory had already shown the ability to turn major difficulties into major successes. Its only well-articulated rival seemed hopeless: the emission theory had simply stumbled from one difficulty to the next, without producing anything remotely resembling the predictive success of the wave theory. Airy and Powell both acknowledged that important empirical problems *did* face the wave theory in the 1830s, but given the overall methodological situation, the only reasonable reaction to these problems seemed to be to work on the wave theory in the attempt to eliminate them. As Baden Powell put it:

> no sound philosopher would for a moment think of abandoning so hopeful a track, and none but the most ignorant or perverse would find in the obstacles which beset the wave theory anything but the most powerful stimulus to pursue it. (Powell 1841, iii)

## 3. Did Brewster Blunder?

So, these are the main elements of Brewster's view of the wave-particle rivalry in optics in the 1830s, compared to the views of the contemporary proponents of the wave theory. Was Brewster's position "irrational"? The question, along indeed with the whole issue of the rationality of theory change in science, stands in desperate need of clarification. In this section I shall try to bring the general issue into focus through an analysis of Kuhn's influential views on theory change. This analysis will, in turn, sharpen the questions that need to be asked about Brewster's particular views.

### (3a) Reason and Theory Choice

Kuhn's account of paradigm change in *The Structure of Scientific Revolutions*, and in particular his claim that resistance of the new paradigm is never irrational, led critics to accuse him of holding that the decision to adopt a new paradigm is never "based on good reasons of any kind, factual or otherwise,"[6] and hence of making fundamental theoretical changes in science "a matter for mob psychology."[7] Kuhn has directly confronted such criticisms in his article "Objectivity, Value Judgement and Theory Choice."[8] He explains that he has never denied that "reason," in the form of the "objective factors" from the philosopher of science's "traditional list" (including such factors as empirical accuracy and scope, consistency, simplicity, and fruitfulness) plays a crucially important role in theory change:

> I agree entirely with the traditional view that [these objective factors] play a vital role when scientists must choose between an established theory and an upstart competitor . . . [T]hey provide the *shared* basis for theory choice. (322)

However, these objective factors supply no "algorithm for theory choice." At any rate when the choice is still a live one in science, they never *dictate* the choice of one of the rival theories. This is for two main reasons. First, single factors often turn out to be ambiguous when applied to the theories *as they stood at the time when the choice was being made*. It is often assumed, for example, that the Copernican heliocentric theory was empirically more accurate (that is, had a better detailed fit with the empirical data) than the Ptolemaic theory. This *eventually* became true, but only as a result of the work of Galileo, Kepler and others—who had clearly then already "chosen" the Copernican view for different reasons (if for any *reasons* at all). Secondly, even if the single factors in the list of objective virtues each point in a definite direction, it is by no means always the *same* direction. Again taking the Copernican revolution as example, and again taking the rival theories as they then stood (say in 1543), while *simplicity* (in a certain special sense) favored the Copernican theory, *consistency* (with other accepted theories) unambiguously favored the Ptolemaic theory.

It follows, therefore, claims Kuhn, that the objective factors must always be supplemented by "subjective" (or, rather, individual or idiosyncratic) factors in order to deliver an unambiguous preference:

> My point is, then, that every individual choice between competing theories depends on a mixture of objective and subjective factors, or of shared and individual criteria. (325)

This account of theory choice, according to Kuhn, diverges comparatively little from that "currently received" in the philosophy of science (321). He accepts, on his part, that the "traditional" objective factors play a vital role. While the philosophers have, on their part, abandoned the idea of an entirely objective algorithm for theory choice, or, at any rate, have relegated it to a practically unattainable ideal, they have further accepted that, as a *matter of fact*, subjective (or idiosyncratic) factors have played a role in the choices actually made by scientists. The gibes about "mob pyschology," therefore, "manifest total misunderstanding" (321): properly understood, he and the philosophers of science are in broad agreement. In particular, he does not deny (and never has denied) that those who switch to a new paradigm in a scientific revolution have *good reasons* for doing so. The only thing is that those who stick with the old paradigm do so for "good reasons" too: "there are always at least some good reasons for each possible choice" (328).

Is this rather cozy view of no real conflict correct? One major problem is that current philosophy of science is altogether less monolithic than Kuhn seems to assume (philosophy of science is "pre-paradigmatic"). There is no single received view in philosophy of science on theory change. At least two broad traditions need to be differentiated. One—the subjectivist tradition—is represented by personalist-Bayesianism. According to this view a person is rational if he assigns degrees of belief to the theories available to him in such a way that these degrees of belief obey the probability calculus and the principle of conditionalization.[9] The latter requires that the rational agent's *posterior* degree of belief in a theory T, that is, his degree of belief in view of the actual evidence e that has accumulated at some later stage, should be measured by the conditional probability p(T,e). The value of this latter quantity is, of course, dependent on the *prior* probability that the agent assigns to T, that is, roughly speaking, his degree of belief in T ahead of any (new) empirical evidence. On the personalist view, this prior probability is a purely subjective matter. This means that there is indeed no real clash between this view and Kuhn's account as just presented. It is in fact easy to give a personalist-Bayesian reconstruction of the theory choices of pretty well any scientist by making suitable assumptions about his distribution of priors. Did Fresnel switch to the wave theory while Brewster resisted it and stuck with the corpuscular theory? Well then, Brewster clearly gave the wave theory a very low prior probability—a much lower one than did Fresnel. Did Einstein resist the quantum theory while acknowledging the strength of the evidence in its favor at a time when Bohr was already fully persuaded of the theory by the same evidence? Well then, Bohr clearly assigned a higher prior probability to the quantum theory than did Einstein. The personalist-Bayesian can show that in the limit, as evidence accumulates, the degrees of belief of all rational scientists in certain circumstances converge on the same values, irrespective of their personal prior probabilities. But of course in real situations we are never at the limit, and resistance by a scientist to any theory in any evidential situation can be explained as rational by ascribing to him a sufficiently low *prior* degree of belief in the theory.

It is, however, precisely this aspect of personalist-Bayesianism that is found objectionable by the defenders of a second major tradition in philosophy of science. The objectivists, as they might be called, see personalist-Bayesians as in effect abandoning the whole idea that scientific change is a rational affair.[10] On the objectivist view, some scientific judgments about the relative merits of competing theories *are* dictated by objective factors (although not necessarily those on Kuhn's list). On this view, if some particular scientist, because of subjective or idiosyncratic considerations, fails to concur in such a judgment, then the scientist does indeed "violate scientific standards." Those who criticized Kuhn for making scientific change an irrational affair clearly belong to this *second* tradition: and it is by no means clear that Kuhn's subsequent clarification of his views does anything to reduce the gulf between him and them and hence to lead to a withdrawal of the mob psychology charge. There are two main reasons for this lack of clarity. The first is a misunderstanding on Kuhn's part about the objectivist claim that scientific change is a rational matter; and the second is great confusion on the part of the objectivists over *which* exact scientific judgments about the relative merits of rival scientific theories are dictated by "reason."

First, Kuhn (and some Kuhnians) often writes as if it were a surprise that holdouts like Priestley or Brewster produced *arguments* for their position and as if this on its own refuted the view that reason has dictated certain theoretical changes in science. But no defender of the objectivist approach, I take it, claims (or has ever claimed) that those who hold out for the older paradigm in what turns out to be a revolution will resort to simple dog-in-the-manger "Yah, boo, hiss!" tactics. *Of course* these holdouts will *argue* for their position—that is, give, in the straightforward sense, *reasons* for it. No one even denies, I take it, that some of these could be considered prima facie good reasons—in the sense that they cannot simply be dismissed without investigation as appeals to emotion rather than reason. Creationists, after all, have produced long books arguing their case; Jehovah's Witnesses will happily engage you for hours in arguments that purport to show that Nature has a design that bespeaks God's hand. Arguing *against* creationists, say, is, indeed, less easy than some other people think (as is shown by the hash that is sometimes made of it).[11] The objectivist-rationalist, however, claims that it *may* nonetheless turn out that the creationist's case disintegrates (or is shown to be extraordinarily weak) *on careful analysis*. This would mean that such a creationist *could* be judged "unscientific" or "irrational." But if this latter epithet is applied, then it should be understood as meaning that the person is not persuaded by *scientifically cogent* reasons, *not* that she has no reasons at all for holding the views she does. Of course, she *will* have such reasons—at least her prior belief in God and the 'literal truth' of the *Bible*; but also perhaps 'scientific evidence' that she holds to be strong and that may require a good deal of analysis before being revealed as bogus, *or* simply as of little weight when compared with the evidence in favor of rival views. (As I shall discuss in more detail later, Kuhn also writes as if his "rationalist" opponents were committed to the view that *all* the evidence standardly points in favor of the new theory in a scientific revolution; but it is surely clear that his opponents need only claim that *on balance* the evidence objectively favored the new theory.)

I said there are two obstacles in the way of clarifying the exact disagreement between Kuhn and those in the objectivist philosophical tradition. The second is that the upholders of this tradition have either not been clear, or have been in clear disagreement, about *exactly* which judgments they see as sometimes dictated by objective rational considerations.

In Brewster's case, as we saw, his "choice" of theory was not a simple matter: he expressed several quite different views about different aspects of the wave-particle rivalry, different views that might merit different responses concerning their rationality. In general, there are at least three judgments that a scientist might make about a particular theory and that ought to be distinguished. *First*, the judgment that that theory is *presently best favored* by the known evidence; *second*, the judgment that the theory is *true*, or, perhaps, "approximately" or "essentially" true; and *third*, the judgment that it is the best theory to *work on*, in that the general ideas underlying it provide the best opportunities for further scientific advance.

There are, of course, important connections between the three judgments. Indeed, if the development of science were "essentially" cumulative, at *all* levels—theoretical as well as empirical—there would hardly be any urgency in separating the three judgments. Suppose that—as "older" philosophers of science are usually accused of holding—new theories in science always included older theories (where "inclusion" is allowed to mean "inclusion with minor modifications"). Even in that case, there would, of course, be no question of our theories being *demonstrated* by the empirical evidence. But at least there would be nothing in the history of science that told *against* the view that in accepting a theory, a scientist accepts it as *true* (or essentially true). Similarly since we should then presumably make the inductive assumption that essential accumulation would continue to hold in the case of *future* changes in science, it would seem to follow that the only rational choice of theory to try to develop further would be the *presently accepted* theory. This is because any theory that was accepted in the future would be (inductively) guaranteed to be an extension (or "essential extension") of that theory presently accepted.

No knockdown refutation can be expected of this essential accumulation view of scientific development: because of the vagueness introduced by the modifier "essential." Nonetheless comparison with anything like an accurate history of science makes the view extremely implausible. Consider for example the history of optics. Even if this history is run only from the late seventeenth-century onwards, it contains a succession of quite different theoretical ideas. The idea that light consists of material particles was widely held in the eighteenth-century, until it was superseded, following Fresnel's work, by the idea that light consists of periodic disturbances transmitted through an all-pervading elastic medium. Particles in a void and waves in a medium certainly look as close to "chalk and cheese" as do chalk and cheese themselves. The wave theory was in turn superseded by Maxwell's theory of light as a disturbance transmitted through a disembodied electromagnetic field. Maxwell himself, of course, was convinced that a mechanical ether underlay the field, but a whole series of attempts to produce a mechanical model failed and left the field as an irreducible, or at any rate unreduced, primitive—meaning that here too there was radical discontinuity at the theoretical level: it is again hard to think of two things more different than an elastic disturbance and an electric (displacement) current. Finally, as part of the quantum revolution, the theory of the constitution of light was again fundamentally altered—according to this theory, light consists of photons obeying a new, entirely nonclassical mechanics.

Of course (and despite what Kuhn and Feyerabend at one time seemed to be claiming) there *has* been "essential accumulation" at the *empirical* level. Successive theories, despite being separated by "revolutions," dealt with an ever wider range of phenomena. The material particle theory could *at best* deal adequately only with simple reflection and refraction; Fresnel's wave theory added interference, diffraction, and polarization; Maxwell added various phenomena concerned with the interaction between light and electricity and magnetism; the photon theory added the photoelectric effect and many others. In this process no *empirical* explanatory power was lost, except perhaps momentarily, even though the explanations were radically altered.[12]

In this optical case (as I believe in *most* cases), more of the older theory enters the new than simply its empirical success: the mathematical equations, and hence, if you like, the *structure*, of the older theory are preserved as well (perhaps as limiting cases). An especially clear instance is provided by the transition from Fresnel to Maxwell. Fresnel's equations, which yield the relative intensities of reflected and refracted light beams in various circumstances, are preserved *entirely* intact within Maxwell's more general theory. However, this continuity is purely *structural* or *syntactic*. The equations remain the same, but the *interpretation* of the fundamental theoretical term involved in them changes completely. Of course, a theory-neutral term, like "optical disturbance," can easily be introduced to do service for both theories; but this should not be allowed to obscure the fact that the optical disturbance in Fresnel's theory represents the distance a particle of the ether has been moved from its equilibrium point, while in Maxwell it is a disturbance in a disembodied, nonmechanical electromagnetic field.

The picture, then, seems clearly to be one of theoretical discontinuity coupled with "essential" empirical (and indeed structural) continuity.[13] The need to distinguish the three judgments about scientific theories mentioned above becomes apparent. Acceptance of a theory as presently most favored by the evidence need *not* involve accepting that theory as *true*, or even "approximately true." It may empirically be the case that many scientists do believe in the truth (or approximate truth) of the latest scientific theories. And, of course, they *may* be correct: it is logically *possible* that, after a series of "failures" (glorious failures), science has now hit on the truth. But those scientists who are more historically aware are surely more likely to be persuaded by the so-called pessimistic induction that even our best current basic theories will one day be replaced by quite different ones. Similarly in the case of heuristic advice, the claim that the only rational course is to try to develop that theory that is presently best favored by the evidence depends crucially on the assumption that science is ( and will continue to be) cumulative.

Kuhn never explicitly defines his term "theory choice." On his construal it does however *seem* to involve taking the theory fully to one's breast, believing it and working on it to the exclusion of all others. He takes it that at any rate the *original* aim of the philosophy of science was to construct an algorithm for theory choice. And this seems to imply that the original view in philosophy was that the rational scientist must always choose, that is, believe and seek to develop, that theory that is already most favored by the evidence.

It is true that many philosophers of science—both subjectivist and objectivist—have talked in terms of rational degrees of belief in a theory, and that it is difficult to see what this can mean except for belief that the theory is *true*. However, there certainly is also a long-established anti-realist (or better: structural realist) tradition in philosophy of science (represented by Duhem and Poincaré, as well as more recent writers) and a fallibilist, or *conjectural* realist tradition (represented by Popper, Lakatos, and others). Duhem, Poincaré, Popper, Lakatos, and many others have all explicitly insisted that one can "rationally accept" a theory *without* believing it to be true. Moreover, so far as the *heuristic* question goes, those who think they can identify an orthodoxy or "received view" in twentieth-century philosophy of science all agree that an integral part of that orthodoxy was the distinction between justification and discovery, and the insistence that philosophy or logic of science was concerned only with the former. This would imply, of course, that theory appraisals have no heuristic consequences whatever. There are certainly some more recent philosophers (such as

Lakatos) who have held that an assessment of the heuristic power of a theory (or paradigm or research program) is an important part of the appraisal of its *present merits*. But Lakatos was careful to insist that there still can be no direct inference from "theory T is currently most favored by the evidence" to "the only rational course of action is to try to develop T" (or, "it would be irrational to try to develop any rival theory T*"). No sophisticated analysis is needed to see the complete untenability of any position that *was* committed to any such straightforward inference. Such a position would entail that the great geniuses of science acted irrationally: the wave theory of light, for example was certainly not unambiguously the best available theory when Fresnel started to work on it in the early nineteenth-century; it was Fresnel's work that *turned it into* overwhelmingly the best available theory.

As I see it, then, the objectivist philosophical tradition was never committed (or, at any rate, ought never to have been committed) to the view that the only rational course of action for a scientist was to "choose" (in Kuhn's sense) that theory that is presently objectively most favored by the evidence. The tradition *is*, I shall take it, committed to the view that there is always an objective ordering of the available theories. There is no reason why this should *always* be a strict ordering, but the objectivist is, I think, also committed to the view that what generally happens in scientific revolutions is that the previously entrenched theory is deposed by one that is strictly superior to it.

Over and above these two core views there has been little agreement between different proponents of the objectivist tradition. They standardly agree on the preference ordering of a given set of rival theories at a given stage of their developments, but often disagree about the general principles that underlie such orderings. More importantly for present purposes, they often disagree about what exactly these orderings of *theories* require from the rational *theorist* (beyond, of course, acceptance of the ordering itself). Against this background, Kuhn's arguments purporting to show that there is no "objective algorithm for theory choice" need carefully to be separated into two different groups. Those in the *first* concern the ranking of theories in terms of their objective merits and, in particular, claim to show that the new upstart theory in some scientific revolution was *not* objectively superior to its previously entrenched rival. The arguments in the *second* group point to difficulties in connecting objective rankings of theories with rational action and rational belief on the part of theorists. The two sets of arguments have very different statuses. Those in the first set would, if successful, knock out the core objectivist thesis. But those in the second set can only, I think, serve to clarify the *open* question of what exactly it is rational or irrational to do, given that one accepts that the scientific evidence currently favors a particular theory over all known rivals. (Since Kuhn himself does not make this distinction, I shall need to take the liberty in what follows of recasting his arguments slightly.)

## 3(b) Kuhn, Theory Appraisal, and the Objective Superiority of the Wave Theory Circa 1830

The objectivist holds that there is always an objective ranking of rival theories, basically in terms of the evidence in their favor. He also holds that, at any rate generally, a "scientific revolution" consists of the replacement of one theory by one objectively strictly superior to it. Two main arguments can be found in Kuhn's work that, if successful, would tell directly against these theses.

One, remember, is this. Kuhn gives a whole list of criteria, which he is ready to concede are "objective" or, rather, shared by all scientists. The list includes empirical accuracy (that is, detailed fit with the data), empirical scope, consistency (both internal and with other accepted theories), simplicity, and fruitfulness. One reason why these criteria do not supply a choice algorithm is that in live cases of theory choice, and, in particular, during scientific revolutions, these different criteria seldom, if ever, tell in the same direction. Much *later*, once the revolutionary theory has been developed and improved, it may outscore its older rival on all counts—but this happens *as a result* of the revolution and therefore can't form its rationale. For example, as I already indicated, Kuhn points out that, if the Ptolemaic and Copernican theories are compared, not as they stood *after* the work of Kepler, Galileo, and Newton, but at, say, the time when Kepler and Galileo were actually choosing to work on the Copernican theory, then the two factors of consistency and simplicity (or harmony) told in opposite directions. The Copernican theory, in its basic form, undoubtedly gave simpler explanations of, for example, the planetary stations and retrogressions and the limited elongation of Mercury and of Venus. But, the Copernican theory clashed wildly with the prevailing, Aristotelian physics and cosmology, while the Ptolemaic theory was, of course, an integral part of the Aristotelian worldview.

Although Kuhn clearly does not establish it in *every* case of fundamental theory change, his historical claim seems to me likely to be correct. Turning back to my own example, if the wave and emission theories of light are compared as they stood in 1830, then a case can certainly be made out that, whatever the other merits of the wave theory, the emission theory still outscored it in terms of mathematical manipulability. (Classical particle mechanics had long been fully articulated mathematically, while continuum mechanics remained partially undeveloped—despite having recently made major advances, often in tandem with wave optics.)

Assume, for the sake of argument, that Kuhn's historical claim is indeed correct in general. His argument against the objectivist nonetheless goes through *only* if we accept the initial assumption that the objectivist can do no better than supply a "laundry list" of objective factors, and is therefore left entirely without recourse when two factors from the list pull in opposite directions.[14] But I know of no objectivist who would accept Kuhn's list as it stands and none who would be happy to leave *any* such list unstructured. For example, for Duhem, Poincaré, Lakatos, and many others, there is a *basic* criterion: that of predictive empirical success. When this criterion is properly understood, it informs most of those on Kuhn's list. The basic idea behind this proper understanding is that a theory achieves predictive success by yielding an empirical fact *without* any prior tinkering specifically aimed at making the theory yield that fact.[15] So, stations and retrogressions, for example, "fall out" of the basic Copernican heliocentric idea, but have to be deliberately built into the Ptolemaic geocentric theory by suitable choice of auxiliary assumptions. Thus prediction properly understood need not involve a hitherto unknown fact—Copernican theory *predicted* the already well-known phenomena of stations and retrogressions.[16]

"Simplicity" and "unity"—in the scientifically most important senses of these terms—are closely related to predictive success. There are surely no clear-cut intuitions about when one *basic* theory in science is simpler than a rival. Is, for example, the idea that light consists of material particles more or less simple than the idea that it consists of waves in a medium? I don't see how even to begin answering the question. Where we *do* have clear intuitions is in cases where a basic theory has been so hedged around with qualifications and split into so many unrelated subcases that it clearly becomes too complex, not sufficiently simple, to be scientifically acceptable. But in all such cases the complexity and disunity have been introduced under the pressure of initially independent or recalcitrant experimental results. The basic theory has enjoyed no predictive success: it has either turned out to be silent about some phenomenon clearly in its field, or to yield an incorrect prediction. Special cases and exceptions have therefore had to be introduced to accommodate the facts—at the cost of increased complexity and decreased unity. This is clearly what had happened in the case of Ptolemaic astronomy; it also happened, as we shall see, in the case of the corpuscular theory of light.

"Fruitfulness" too is intimately connected to predictive success. A general theoretical approach ( a paradigm or research program) shows its fruitfulness by supplying ideas for developing specific theories *independently of empirical results*. Such an approach will be judged barren (as Lakatos put it, the research program's "heuristic" will have "run out of steam") only when all these ideas have been tried *without predictive success*; and hence the approach has been reduced to tagging along *behind* the empirical data, always accommodating it *post hoc* rather than predicting it in advance.

By the early to mid-1830s, for example, the emission approach to optics had very definitely proved barren. The ideas supplied by the general claim that light is a Newtonian particle had all been tried in the attempt to produce specific theories that dealt with optical phenomena. Particles were, of course, subject to forces; forces could be attractive or repulsive: all the apparent deviations from rectilinear propagation—reflection, refraction, interference, and diffraction—

might be explained by having ordinary "gross" material objects exert forces on the light particles. The idea that these are strictly point particles always had to be an idealization – the finite dimensions of the real particles might come in useful: it might for example be assumed that the particles have sides or poles and revolve with respect to these poles as they move along. Various isolated results could be explained (at any rate in outline) on the basis of these assumptions – but, when it came to anything like details, the "natural" assumptions about the forces and the polar revolutions unambiguously failed and instead the required theoretical assumptions had always to be "read off" the *already given* facts. There was never any correct prediction of a different phenomenon. Instead each new phenomenon required further elaboration of the theoretical assumptions (perhaps another complication in the field of force set up by the diffracting or refracting body or yet another axis of revolution in the particles). As Humphrey Lloyd put it in a famous report on the "Progress and Present State of Physical Optics":

> An unfruitful theory may . . . be fertilized by the addition of new hypotheses. By such subsidiary principles it may be brought up to the level of experimental science, and appear to meet the accumulating weight of evidence furnished by new phenomena. But a theory thus overloaded does not merit the name. It is a union of unconnected principles. . . . Its very complexity furnishes a presumption against its truth. . . . The theory of emission, in its present state, exhibits all these symptoms of unsoundness, . . . (1833, p. 296)

Similarly, by the early years of this century, ether-based classical physics was no longer fruitful. Instead of the general idea of an ether that fills space suggesting new specific theories, the ether had become an embarrassment – ad hoc explanations having to be provided one after the other for why otherwise expected manifestations of the ether failed to show up empirically.

As for the other "objective factors" on Kuhn's list, the philosophers I have mentioned would all, I think, either deny them *any* role or relegate them to subsidiary roles.

This is particularly true of consistency (that is, consistency with other, already accepted theories). It is surely a *virtue* in a theory, rather than a vice, if it clashes with some well-entrenched claim – *provided* that there is strong evidence for the theory in the form of predictive empirical success. The inconsistency of Copernican theory with accepted Aristotelian physics supplied interesting and demanding problems for further research. Scientists will, no doubt correctly, downgrade (or more usually ignore) new theories that clash with well-established ones – but *only* when there is no independent evidence for the new theory. The fact that various current hypotheses concerning the "paranormal" clash with accepted theories is currently regarded as an important argument against them, but again only because those hypotheses have showed no empirical predictive success. It is predictive success that flips inconsistency with other well-accepted theories over from a vice to a virtue.

Kuhn's demonstration that there are important historical cases in which different objective factors pulled in opposite directions need not, then, trouble this sort of objectivist. He will happily pronounce Copernicus's theory scientifically preferable to Ptolemy's in 1543, while admitting that Copernicus's theory was inconsistent with other previously accepted theories (and even, as we shall see, while admitting that the Ptolemaic still had, to some degree, superior established empirical accuracy). And, in my own example, such an objectivist will happily pronounce the wave theory of light well ahead in 1830, while acknowledging the emission theory's superior mathematical power. This is because the criterion of predictive success is dominant for him. And on that score, the wave theory of light (as Brewster himself more or less clearly acknowledged, as we saw) was simply miles ahead of its rival by 1830. In over 150 years, the emission theory had failed to produce anything remotely capable of standing alongside Fresnel's success in predicting in minute detail the sizes and separations of diffraction fringes, let alone his success with the "white spot" at the center of the shadow of a small circular disk, the emergence of circularly polarized light from a Fresnel rhomb, internal and external conical refraction, and so the list goes on.

A second argument is to be found in Kuhn against the idea that the winning theory in a scientific revolution is generally objectively superior to the older theory. The first argument was based, as we just saw, on the assumption that each objective factor tells unambiguously in favor of one of the rival theories, but then went on to claim that different objective factors may tell unambiguously in *different* directions. But Kuhn also argues that this initial assumption itself is often false: scientists may reasonably disagree over the way that *single* objective factors point. For example, simplicity told in favor of Copernicus over Ptolemy only when understood in a very special sense. In other senses, Copernicus's theory was by no means clearly the simpler. Similarly, although it is often assumed that the Copernican theory was better than the Ptolemaic in terms of empirical accuracy, in fact, as the theories stood in 1543, this criterion delivers no clear preference.

One major problem here is again Kuhn's nonanalytical, acritical approach to admission onto his list of objective virtues. The result—from the point of view of the analytic philosopher—is often a confused amalgam of various quite different ideas about criteria of scientific merit. It is then no news that such confused "criteria" supply no clear-cut judgments. Everyone would, for example, surely concede to Kuhn that a whole variety of notions of theoretical simplicity are to be found in science and philosophy. The moral seems to be that careful analysis is needed to sort out the really important notion or notions. (As I already indicated, on my view the important sense of simplicity is intimately related to predictive success.)

The case of Kuhn's criterion of empirical accuracy is similar. He describes this as "the most nearly decisive" of all the objective factors. But his notion of empirical accuracy is an unfortunate amalgam of two criteria that should be kept separate: predictive success and overall, detailed fit with all the known, relevant empirical data.

Kuhn points out that, contrary to widespread belief, the Copernican theory did *not*, as it stood in 1543, exhibit unambiguously better empirical accuracy than the Ptolemaic: the former did *not* account for every detailed empirical datum accounted for by the latter (plus some more) – instead *each* theory enjoyed empirical successes not shared by the other. Copernican theory did *eventually* come to dominate the older theory empirically, but only as a *result* of Kepler's and Galileo's decisions to "choose" Copernicanism. Similarly in the case of the chemical revolution, and again contrary to widespread present-day belief, there were empirical phenomena that the phlogiston theory could account for, but for which Lavoisier's theory could give no account. This is, in other words, the historical phenomenon, or alleged historical phenomenon, of "Kuhn loss." It acts as a further important source of reasonable subjectivism for Kuhn: if a scientist happens to give special weight to a phenomenon whose theoretical codification is "lost" in the switch to the new theory, then that scientist may reasonably resist the switch.

Is Kuhn loss a genuine historical phenomenon? Kuhn's own examples of lost content tend to be unconvincing. The example he tends to cite in the case of the chemical revolution for instance concerns the (alleged) fact that metals are "more similar" to one another than are metallic ores. But this is a curious empirical phenomenon – certainly it cannot stand as an observation report on a par with such things as "the needle in apparatus A pointed to near '5' on the scale" or "the measured angle of elevation of telescope T at time t was $\theta'$." Moreover the phlogiston theory's alleged explanation of this curious fact is more curious still. The "explanation" is that metals are more similar to one another "because," unlike the ores, they all contain a common ingredient: phlogiston. This "explanation" relies of course on the implicit assumption that any two things that share a common constituent are "more similar" to one another than any two other things which do not. This *either* makes no real sense (apart from the notorious multiple ambiguity of "similarity," there is also the question of how deep we go in the search for common ingredients – after all, we now think that *everything* is "made out of" elementary particles) *or* it is arguably false (whatever exactly Kuhn had in mind, it seems difficult to argue that, say, a piece of coal is more similar to the Koh-i-noor diamond than, say, oxygen gas is to hydrogen gas).[17]

The loss allegedly involved in the Copernican revolution is also unconvincing – though for a different reason of more general significance. Kuhn's brilliant analysis of this revolution showed that, while the new Copernican theory gave genuine explanations of various important qualitative phenomena (such as planetary stations and retrogressions and the limited elongation of Mercury and of Venus), which had only been forced into the Ptolemaic framework post hoc, the Ptolemaic theory could give detailed quantitative accounts of phenomena that the Copernican theory, *as it stood in 1543*, could not match. This, however, only illustrates the importance of keeping quite distinct the criteria of empirical *predictive* success and overall empirical content, rather than conflating them into one notion of empirical accuracy.

There is a crucial difference between the success enjoyed by the Copernican theory and the, admitted, extradetailed empirical content of the Ptolemaic. Namely that the latter, but not the former, can be achieved simply by hard work. Ptolemy's theory had, of course, been developed and applied for centuries when Copernicus challenged it. In general it is not at all surprising if the entrenched theory has detailed acounts of phenomena that the new upstart theory cannot yet match. No matter how successfully predictive a new theory might have been, there would always be some areas where it needed detailed emendation and elaboration. But this is largely a question of hard work ("normal science"). The Ptolemaic system had been developed by letting the already known data guide the construction of the required auxiliaries within the general geocentric framework: no predictive success having been achieved in the process beyond that secured by simple inductive extrapolation.[18] It was surely clear already in 1543 that, just as the detailed phenomena had been worked into the Ptolemaic framework by suitable elaboration of auxiliary assumptions and mathematical devices, so they could, with sufficient effort, be accommodated within a heliocentric (or, rather, heliostatic) framework. Given a general theoretical framework, specific theories with ever greater empirical content can generally be developed simply through hard work. What *cannot* by definition be achieved in this way is the sort of qualitative predictive success that made Kepler and Galileo think that that sort of hard work on the Copernican framework was worthwhile. These predictive successes occur precisely when the empirical result "falls out" of the general theory *without* any tinkering. The Ptolemaic theory had had no such success. So nothing was "lost" in this case that could not clearly be regained.

Our own optical example might seem to supply a rather more convincing example of Kuhn loss. Brewster, as we saw, made a good deal of the fact that the wave theory could explain neither dispersion nor selective absorption. But this case is not clear-cut either.

As Airy and Powell insisted in their replies to Brewster, neither of these phenomena was properly explained on *either* theory. Certainly neither was (in my sense) *predicted* by the emission theory—neither fell directly out of that theory in the way that diffraction patterns fall out of Fresnel's theory, or that the bending of light rays, say, falls directly out of the general theory of relativity. Indeed not only was neither phenomenon predicted by the emission theory, no *full* emissionist account of either dispersion or selective absorption was *ever* given—even post hoc. The most that the emissionist could argue is what Brewster did in fact argue: that it was easier to see how, *in general conceptual terms*, an explanation of the phenomena *might* be produced within the corpuscular theory, than it was to see how such an explanation might be produced within the wave theory.

Concentrating for simplicity just on the case of selective absorption: the wave theory, as Brewster forcefully argued, was bound to have great difficulty in conjuring this discrete, "black-and-white" phenomenon out of its underlying assumptions that were unambiguously assumptions of continuity. An infinitesimal change in a continuous parameter – the length of the wave – would somehow have to make all the difference between free passage through the ether within a selective medium and no passage at all. The emission theory, on the other hand, made light consist of *different* particles: if the emission theory made some effect depend on the value of some parameter associated with these particles, there was no need for it to assume that all possible values of this parameter were instantiated. It could always indeed explain any apparent continuity, for example of the "degrees of refrangibility" associated with the solar spectrum, as an illusion, deriving from the inability of our coarse senses to detect slight but nonetheless existent differences. No precise emissionist account of selective absorption was in sight in the 1830s; but such an outline account could readily be seen to be a conceptual possibility. It could, for example, readily be conceived that two different light particles, while having almost identical degrees of refrangibility, might nonetheless differ in some other important respect, which accounted for one of them being absorbed by the medium while the other passed through. Brewster's own suggestion was that the phenomenon might be *chemical* in nature – that the different light particles have different chemical constitutions, which might then explain why one is absorbed and the other not. Brewster had no more than this to say – hence his suggestion was certainly, as it stood, vague and untestable. But even if the wave theory had *almost* nothing to match in this regard, it must be admitted that in 1830 it could *not* match it.

Does this mean that, at any rate for the wave optics revolution, I have conceded Kuhn's case and accepted that empirical accuracy did *not* tell unambiguously in favor of the wave theory? Such an inference, clearly encouraged by Kuhn, would be an obvious *non sequitur*. Kuhn's argument again seems to presuppose that those philosophers who hold that theory change in science is generally a rational affair are committed to the claim that *nothing* ever tells in favor of the superseded theory. In fact, of course, such philosophers have long recognized the need to *weigh* evidence. An "objectivist-rationalist" clearly need not hold that the theory superseded in a revolution had *no* virtues, nor even the view that it had no virtues not shared by the superseding theory. It is enough if *on balance* the superseding theory is clearly better. This applies in particular to *empirical* or evidential virtues.

This simple point allows us to bring together much of the foregoing discussion. In the Copernican case, even if the objectivist acknowledged that the accounts it provided of detailed empirical phenomena favored the Ptolemaic theory, he would certainly hold that the qualitative *predictive* successes scored by the Copernican theory favored that theory *much more highly*. No attempt to explain theory change as rational can hope to succeed if it fails to give extra theory-confirming weight to predictive success over post hoc accommodation. Indeed since such post hoc accommodation can *always* be achieved at least in principle (this follows from Duhem's point that the central framework-supplying theories in science have *in isolation* no empirical consequences), some philosophers give *zero* confirming weight to empirical data that have simply been worked into a theoretical framework. For such philosophers, a genuine case of Kuhn loss would need to involve the loss of some *genuinely predicted* content: that is, a case in which some phenomenon "fell out" of the older theory, but not out of the newer theory. To my knowledge no such case has been presented. But even *without* adopting this extreme view, and even if there *are* genuine cases of Kuhn loss, the objectivist need not be in trouble. Let's accept (as I believe we should) that dispersion and selective absorption (weakly) favored the emission theory in the 1830s. Still, everyone accepts (including Brewster, as we saw) that by then a long list of phenomena that (strongly) favored the wave theory could readily be produced. This list includes several phenomena that had been genuinely predicted by the wave theory – such as various diffraction patterns, various results about circularly and elliptically polarized light, and the phenomena of internal and external conical refraction. Airy and Powell were right that the *only* theory to enjoy any genuinely predictive success was the wave theory.

On any account, then, and being as generous to the emission theory as one likes, the evidence, *on balance*, strongly favored the wave theory. Thus the objectivist could (rather generously) concede that there was a Kuhn loss involved in this revolution concerning dispersion and selective absorption, without threat to her position. This is because the Kuhn loss involved in *not* making the switch to waves would have been enormous. Philosophers of science have not achieved any great measure of agreement over the general principles involved in weighing evidence, but everyone surely agrees on the need to weigh. And it is clear that no adequate account of weight of evidence could fail to have the balance coming down with a mighty bang in favor of the wave theory in the 1830s.

I claim to have shown so far that nothing in either Brewster or Kuhn tells against the view that, by the 1830s, the wave theory was objectively superior to its emissionist rival. There remains, then, what I have insisted must be treated as a separate question: that of what acceptance of this appraisal requires from the "rational scientist." As I suggested, I shall use the example of Brewster in an investigative way to attempt to illuminate the murky issue of just how strong an implication for rational belief and conduct our theory appraisals ought to have. So: did any of Brewster's theoretical views place him beyond the "rational" pale?

## (3c) Was Brewster Irrational to Hold Out Against the Scientifically Superior Wave Theory?

Let me first clear away a possible misunderstanding of a purely linguistic kind over the terms "rational" and "irrational." Brewster was clearly a clever man, who dealt in arguments, who accepted all well-tested experimental data, made all the usual inductive generalizations of such data, and who did nothing to transgress the rules of deductive logic. Moreover, he clearly accepted that, in terms of predictive success, the wave theory had greatly outscored the Newtonian theory as things stood in 1830. If, despite all this, we end up saying that some of his views were "irrational," then this should clearly be understood in a rather special sense: one that carries no suggestion that Brewster is to be put on a par with Russell's famous "lunatic" (who believed that he was a poached egg), nor with anyone who, aiming to get down safely to the ground floor, proposes to take the window rather than the elevator, claiming that all evidence that this is foolhardy is evidence purely about the past. Without being irrational in any such blatant sense, Brewster might still have contravened best scientific practice in his attitudes towards the rival theories available to him.

It is (presumably) trivial that the objectivist-rationalist *would* pronounce Brewster irrational ("mistaken" would be better) if he denied that the empirical evidence currently favored the wave theory over its emissionist rival. But his acknowledgment of the "unequalled" predictive and explanatory success of the wave theory is surely tantamount to accepting this appraisal. The worry is that if—as Lakatos, for example, explicitly stated—this acknowledgment is *all* that is required from the rational scientist, then our rules of rationality say perilously little. Indeed Feyerabend has claimed that if all that Lakatos's methodology requires is an admission of the present score between the rival theories, then it is really "anarchism in disguise." It is wrong, I think, to underestimate the importance of simply keeping the objective score. (Try, for example, to get a creationist to say that he accepts that the evidence currently strongly favors the Darwinian theory, but that he nonetheless is working on the creationist approach, hoping eventually to reverse the evidential tables.) Nonetheless it is difficult not to yearn for a somewhat stronger theory of rationality.

Let's then look at Brewster's views about what follows (or fails to follow) from the acceptance that, in terms of empirical success, the wave theory was well ahead of its rival by the 1830s. Can any of these views plausibly be categorized as irrational and, if so, on what grounds?

As I already indicated, three of Brewster's views raise interesting questions in this connection. They are:

(1) Despite all its success in explaining and predicting optical phenomena, the wave theory is not *true* as a fully realistically interpreted "physical theory";

(2) Despite all its problems, the Newtonian theory might yet "stage a comeback" and ultimately turn the tables of scientific superiority on its rival.
(3) Therefore, Airy and Powell's view—that the only reasonable response to the difficulties facing the wave theory was to try to solve them *within* the general wave approach—was not correct.

I consider each of these in turn.

## (3ci) Brewster's Disbelief in the Wave Theory as a Physical Truth

Brewster argued that the impressive explanatory and predictive success of the wave theory does not logically entail its truth as a fully fledged, realistically interpreted physical theory. But Brewster went beyond this obviously correct logical claim and clearly held that, as a physical theory, the wave theory was actually false. Indeed he predicted that "after it has hung around for another hundred years," that theory will give way to a completely different physical theory.

Given that the wave theory was undoubtedly the best-supported theory available to him, was it irrational of Brewster to believe it to be false? This would seem a harsh judgement to make in view of the fact that Brewster's prediction was correct—indeed he was overgenerous to the wave theory, which lasted at best another 70 years, rather than another hundred.

It is true—and importantly true—that many of the mathematical equations supplied by the wave theory still live on in science; and it is true—and importantly (if rather obviously) true—that repeatable (and repeated) experiments do not change their results, so that all the correct empirical consequences of the wave theory are still, of course, correct. Nonetheless, at the theoretical level there has been radical, ineliminable change. The ether—at any rate in anything like the form understood by Fresnel—has been entirely rejected by present-day science; photons traveling through empty space, despite their so-called wave*like* characteristics, could hardly be more different than they are from waves in a mechanical, space-filling medium.

If current scientific theories are correct, then so was Brewster correct in believing the wave theory to be false. We surely should *not* then require the rational scientist to believe in the truth of the currently best-available theory. What if the rationality requirement is watered down so that it requires from the rational scientist only belief in the *approximate* truth of the current best theory? The notion of approximate truth has proved extremely resistant to precise analysis, but it does seem reasonably clear intuitively that, however approximate truth is eventually analyzed, Brewster's complete rejection of a real ether is inconsistent with an ascription to him of belief even in the approximate truth of the wave theory. It would, again, however, surely be difficult to find him guilty of irrationality on this score. After all he was surely *right*; no scientific realist will ever, I fear, produce an acceptable account of approximate truth that would yield the judgment that if the photon theory of light is true, then the classical wave theory is approximately true. "To a large degree empirically adequate"—yes; "to some degree *structurally* accurate"—no doubt; but "approximately true"—no.

Many scientists do seem to believe the presently best-available theory to be not just highly empirically adequate but actually *true* (or "very nearly" true); and the number of believers not surprisingly increases with the continuing empirical success of that theory. Certainly there were many scientists in the early nineteenth century (Fresnel himself among them) who believed in a real, mechanical ether and the full truth of the basic wave theory. On the other hand there have always been other scientists—perhaps more aware of the *history* of science—who find belief in the *truth* of the latest theory impossible. This may, as perhaps in Brewster's case, be motivated by prior metaphysical beliefs, but it may also be motivated, as in, say, Poincaré's case, by general methodological and historical considerations about scientific theory. Both because the agnostics often turn out eventually to be right and because to do otherwise would be to prejudge a live *philosophical* debate, it would surely be wrong to brand them irrational. Whatever consequence the judgment that T is the best available theory has about rational belief, it is not the consequence that the only rational course is to believe T to be true (or even approximately true).

As I indicated earlier, many of the problems that Kuhn raises about "theory choice" and the lack of an "objective algorithm" for it arise from his implicit assumption that in choosing a theory a scientist must take it fully to his bosom and believe it to be true. *Of course* there are always holdouts in this sense. For example, anyone who holds a thoroughgoing instrumentalist view of scientific theory will *always* be such a holdout—no matter how she ranks the specific scientific theories available to her. Although instrumentalism certainly tends to be more popular at times when even the best scientific theory is in clear difficulties, it is not *invariably* adopted in this defensive way. Interestingly enough, the two main British advocates of the wave theory, Airy and Baden Powell, themselves might have to be classified as holdouts in *this* sense: both adopted explicitly uncommitted views of the ether. I quoted Airy *above* (p. 321) claiming that Fresnel's wave theory had the same status as Newton's gravitation theory, both being "certainly true." However, later in this passage he asserted:

> This character of certainty I conceive to belong only to what may be called the *geometrical* part of the theory: the hypothesis, namely, that light consists of undulations depending on transversal vibrations, and that these travel with certain velocities in different media. . . . The *mechanical* part of the theory, as the suppositions relative to the constitution of the ether . . . though generally probable, I conceive to be far from certain.

Similar sentiments were expressed by Baden Powell. Yet both were fully committed to the wave theory as superior to all known rivals.

## (3cii) Brewster's Continued Belief That the Emission Theory Might Eventually Prove Triumphant

Brewster seems to have held that the emission theory—if diligently developed by scientists of talent—was likely *eventually* to prove superior to the wave theory (or at any rate *might* eventually do so). In general, on Kuhn's view, the main source of the resistance to new paradigms by elderly holdouts is the assurance that they feel that "the older paradigm will ultimately solve all its problems, that nature can be shoved into the box the [older] paradigm provides" (1962 151–2). Kuhn insists that this assurance—while it may irritate the revolutionaries—cannot be faulted as "illogical" or "unscientific" or "irrational." Is Kuhn right?

Suppose first that the claim that nature *can* be "shoved" into the older "box" is merely one about logical possibility. Then the claim is of course correct: put in more orthodox terms, and following Duhem (1906), the core theory underlying the older theoretical system will not be testable in isolation, and so it follows from deductive logic that there must be *some* assumptions that are consistent with that core theory and that, together with that core theory, entail *any* given experimental results. More strikingly, in our historical case (as well as others) the outlines of how actually to shove *most* known optical phenomena into the older box had been constructed by the 1820s and 1830s. Among such shovable phenomena were ones like interference and diffraction, which more superficial, later treatments presented as crucial phenomena—predicted by the wave theory, but quite beyond the scope of the emission theory.[19]

On this weak interpretation, Kuhn's assurance that the older paradigm *can* accommodate all the phenomena certainly exists. But, of course, it does *not*, as Kuhn seems to think, automatically make resistance to the new theory rational. *That* would require (at least) a quite different assurance—the assurance that the phenomena can be accommodated within the older paradigm *in a scientifically acceptable way*.[20] But this much stronger assurance does not follow from Duhem's point about untestability in isolation. We know that some sort of account could be given of *diffraction*, for example, within the emission theory—not only because of logical considerations, but because the outlines of such accounts were constructed. But those accounts *were* uniformly awful—the assumptions that had to be made about different masses of the different light particles, their rotations about various axes, and about the dependence of the "diffracting force" on the phase of rotation and on the distance from the diffracting object were simply "read off" the facts. Those assumptions had to be made more and more complex as more facts were taken into account. Even the most assiduous of emissionists, like Biot, quietly gave up long before *all* the facts had been accommodated. Deductive logic alone certainly does not, of course, *guarantee* that any nonawful account even *could be* constructed within the emission theory.

The only assurance really available to the holdout fails to explain his resistance as rational. But it is of course a further question whether or not it can actually become *irrational* to hold that an older theory will eventually provide scientifically acceptable explanations of presently recalcitrant phenomena. Was it irrational, for example, to believe in 1830 that the emission theory could eventually *adequately* explain diffraction?

As I just indicated, such a belief certainly does not run counter to deductive logic. If one is satisfied with deductive rationality, then the answer must be that it was *not* irrational to believe that the emission theory would eventually adequately explain diffraction. Nonetheless there were strong reasons for regarding this belief as false. Every explanatory avenue open to the emissionist had been tried and had failed to produce a scientifically adequate account of diffraction. It had to be *logically possible* to make sufficiently complicated assumptions about the diffracting forces exercised by gross matter on the light particles, about differences between the particles themselves, and about "fits" undergone by the particles so as to accommodate all the known facts. But actually putting this into effect had proved *practically* impossible. Moreover, any such theoretical accommodation was clearly going to involve extremely implausible general assumptions – for example, that the diffracting forces are quite independent of the chemical constitution of the gross diffracting object (two straightedges made respectively of, say, cardboard and copper, produce the *same* diffraction pattern). It is simply, as I see it, a *fact* about the emissionist approach in 1830 that it would require the incorporation of some radically new idea before diffraction could ever be *adequately* accounted for. To close one's eyes to this fact would surely be no less irrational than to close one's eyes when confronted with Galileo's telescope.

Brewster's optimistic remarks about the corpuscular theory's prospects are not specific enough to make it clear whether he did close his eyes to this fact, or whether he simply held the much weaker view that working on the general Newtonian approach *might* produce just the right radically new idea to revitalize the approach. Assume that Brewster's view was only this weaker one. Was *it* irrational?

Well, whatever its disadvantages, it again certainly does not contravene deductive logic: *of course* working on the Newtonian approach *might* have produced the required idea, if we are talking merely about *logical* possibilities. However, and quite unlike most heuristic judgments made in science, this view expresses no more than a pious hope. A mid-eighteenth-century Newtonian could quite plausibly remain unconcerned by diffraction fringes (which had, after all, been around since Grimaldi in 1665 and had been extensively investigated by Newton himself). The phenomena of reflection and refraction already established for him that gross matter was capable of exercising forces on the light corpuscles, forces that diverted those particles from their naturally rectilinear paths. It was not at all surprising if similar deviations occurred when those corpuscles passed close by the edge of a gross object. It was just a matter of investigating the diffracting forces in detail and thus building up a full theory of the phenomenon. An eighteenth-century corpuscularian who expressed the view that working on his research program was likely to produce an account of diffraction was not, then, simply expressing a pious hope—he could indicate in an abstract but reasonably precise way the approach to be adopted. But absolutely no success was, as a matter of fact, achieved in this way, despite a good deal of effort through the eighteenth century. Moreover, a great many fundamental problems accumulated as this effort was made: Quite unlike the refracting force which differed from substance to substance (with refractive index), the diffracting force (assumed by most Newtonians to be another manifestation of the same force) seemed to be quite independent of the constitution of the diffracting object. The diffracting forces had been assumed to switch from attractive to repulsive and back again with bewildering rapidity as the distance from the diffracting object increased; the effect of the diffracting force had been made to depend in various complicated ways on the phase of the light particle's periodic "fits of easy transmission/reflection" (conjecturally associated with periodic revolutions of the particles). All this and still no satisfactory theory was remotely in sight. No reasonably well-articulated idea existed that had not been tried and found wanting. Thus, as I suggested, by the 1830s Brewster's view was nothing more than an expression of a hope that some new idea could be conjured out of the blue, a new idea that fitted in with the general Newtonian approach, and that solved the problems with diffraction.

Well, one can *always* hope; but, there are, so far as I can tell, no historical cases in physics in which a theory subsequently recovered scientific credibility having earlier been in straits as dire as those the emission theory was in by 1830. No doubt there are cases of very general metaphysical ideas that have had a checkered history; once incorporated into a program that steadily degenerated, they have then much later been revived by incorporation into a different program that progressed. Atomism is often cited in this connection. But if we look for cases, not at this very general level but at the level of specific Kakatosian research programs (or Kuhnian paradigms), then I, at any rate, don't see any in the history of mathematical physics.[21]

Assuming for the moment that this is indeed the lesson of history, the question arises of whether it should be written into our account of scientific rationality, so that that account pronounces it irrational to try to resuscitate a theory that has degenerated beyond a certain point. If it is, there will be problems specifying exactly what that certain point is. Moreover, such a rationality principle would have nothing even resembling an a priori justification, but would simply rest on an inductive extrapolation from future to past—this time an inductive extrapolation of a methodological kind.

Let me postpone further discussion of this point until after considering

Brewster's third controversial view, which is closely related to the one we have just been discussing and raises similar methodological issues.

### (3ciii) Brewster and the Wave Theory's "Monopoly"

As I noted earlier, Brewster held that work on the emission theory was still likely to prove fruitful, and accordingly he regarded as ill advised the "monopoly" that he believed the wave theory had come to exert. He saw the experimental difficulties with dispersion and selective absorption as enough to deny the wave theory "our implicit assent" and enough to justify a less committed approach. Airy and Powell held that, on the contrary, given the established success of the wave theory, "none but the most ignorant or perverse" would do other than commit himself still more wholeheartedly to the wave approach in an effort to solve its difficulties. Were they rational and Brewster irrational?

The first point in favor of Airy and Powell was that, so far as dispersion went, there already existed within the general wave theoretical approach some hopeful lines of attack on the problem. Fresnel's theory, in its initial version, certainly entails no dispersion. But this initial version was based on a very simple theory of the ether – one that involved the assumption that its parts *strictly* obey Hooke's law of the proportionality of restoring force to displacement. Several general ideas were already around about how a somewhat less simple theory involving a slightly more complicated expression for the restoring force could be constructed that might yield dispersion. Though none of these had yet borne unambiguous fruit, equally they had not all unambiguously run into sand. This is again, as I see it, just a *fact* about the wave approach: it *already* possessed potential explanatory resources with respect to dispersion that had *not* been exhausted.

Airy and Powell went on to back this up with an argument that is explicitly inductive in nature. Airy in particular pointed out that, not only was the wave theory already far ahead in terms of explanatory and predictive success, but the way it had dealt with earlier experimental difficulties (especially those posed by polarized light) had already shown that it had the capacity to be modified and extended in a scientifically fruitful way. Moreover, there was only one alternative theoretical approach – the emissionist one – and it faced many more difficulties and had shown no capacity successfully to overcome them. Airy is claiming in my preferred terminology that, when confronted by the choice between a research program that has long been degenerating and a program that has been highly progressive, then the only reasonable course of action is to work on the second.

Should this principle be incorporated into our theory of scientific rationality? First let's be a little clearer about what exactly the principle is. Certainly *within* research programs there will often be alternative strategies available, and it may even not be too farfetched to regard the program as specifying rough probabilities for each alternative strategy's paying off. In such a case "let a thousand flowers bloom" and "don't put all your research eggs in one basket" will be the watchwords: even if there is a "most probable" strategy for advance, it will equally clearly not be irrational to pursue a different one. Indeed, as has often been pointed out, we would *like* some members of the scientific community to pursue high-risk strategies. In such cases—again as has often been pointed out—rationality applies (at any rate most directly) to decisions taken by a community collectively rather than to those of individuals.[22] Even *between* research programs, there are cases that are far from clear-cut—at around 1700, say, I would not want to say that working on *either* the wave *or* the Newtonian approach was in any sense irrational. But in just the sort of case we have been discussing—the sort of case exemplified by optics around 1830 and in general the sort of case covered by Kuhn's notion of a scientific revolution—the situation is quite different, I believe. So far as I can tell, once a truly progressive program has been developed in science, the way forward has *always* been to follow that program, ignoring its degenerating rivals. Kuhn suggests (and has been echoed by others) that it may be good for the community for some diehards to remain, since they may produce problems for the revolutionaries to solve. This suggestion, as plausible as it might sound prima facie doesn't, so far as I can see, wash historically. Certainly in the optics case, the post-Fresnel diehards were no more than a distraction. (Brewster did important experimental work, but *none of it* was informed by his emissionist views—even though most of his results were forced into the language of a sort of instrumentalized version of the emissionist theory.)

The general principle, then, that I tentatively propose as a candidate for incorporation into the theory of scientific rationality is something like this: When the choice is between a highly progressive program and a highly degenerate one, the only rational course is to pursue the progressive one until such a time as degeneration sets in there too.

But, assuming that such a principle were to be incorporated into the theory of scientific rationality, what would the grounds for such incorporation be?

## (3d) How Strong Should the Theory of Scientific Rationality Be?

Brewster in particular and—presumably—Kuhn's holdouts in general cannot be faulted on the grounds of consistency with experimental results and deductive logic alone. Long before the appearance of *The Structure of Scientific Revolutions*, Duhem had fully recognized that those grounds alone leave the theoretical scientist with enormous freedom; that is, they produce only a very weak theory of scientific rationality. Duhem went on, however, to applaud the "good sense" that was enjoyed by the best theoretical scientists and that in effect greatly curtailed this freedom. He was reluctant to incorporate the general principles underlying this good sense into what he called the "logic of science." Although he was not entirely clear about his grounds for this reluctance, they seem to have been essentially that the logic of science should somehow be self-justifying, while the principles of scientific good sense clearly involve substantive, and therefore challengeable, assumptions. Duhem's good sense, indeed, consists basically in following the types of procedures that have paid off for science in the past. Hence, as I have just tried to explain, if good sense is incorporated into our logic of science or general theory of scientific rationality, while it certainly strengthens that general theory, it also brings with it certain inductive assumptions. If these are challenged—"Why mightn't it happen, *just next time*, that pursuing a highly degenerate program suddenly pays off handsomely?"—it is difficult to see how they could be further defended.

But surely it cannot be a good idea to incorporate pure assumptions into our theory of rationality? I think we have to face up to the fact (as perhaps Duhem did not) that they are already there. Such assumptions are involved even at the level of theory appraisal (Duhem's "*logic* of science"). No acceptable system of appraisal can, for example, do without some principle that downgrades ad hoc explanations compared to non-ad hoc ones. For a nineteenth-century Newtonian astronomer, for instance, to respond to the difficulties with Mercury's orbit by saying "All bodies in the universe are Newtonian except for Mercury and its motion is described by the following empirical law . . . " cannot be scientifically acceptable. But who says that God's blueprint of the universe did not specify that there be one exception to every general rule? So far as I can see, we simply *assume* that this is not true. Similarly, most of us would presumably regard the person who proposes to get down safely to the ground by taking the window as irrational (though thankfully he won't be irrational for long). But long discussions of Hume's problem seem to me, at any rate, only to have revealed that even this judgment relies on a pure assumption about the uniformity of nature (even if it is an assumption that is genetically "hard wired").

To sum up, then: As should always have been clear, deductive logic alone, even when coupled with acceptance of *low-level* ("crude") observation results, supplies only a very weak theory of rationality. Making that theory stronger requires committing oneself to substantial assumptions: both general metaphysical assumptions and some frankly inductive assumptions based on past scientific success. It does seem to be a fact about the history of physics that no one who has stuck to a *highly* degenerating program when a progressive alternative was available has ever managed to reverse the situation. Hence, those willing to make the necessary inductive assumption provide themselves with a theory of rationality that says that the only rational course of action in such a situation is to follow and develop the progressive program, at any rate for the time being.

This would mean that when Kuhn claimed that there is no point at which "resistance becomes illogical, or unscientific" he conflated two claims: one right, one wrong. Applying the claim to the particular case of Brewster, there was certainly nothing "illogical" about his theoretical views. But this, as I just pointed out, is no surprise in view of the (of course crucial) but extremely weak requirements imposed by deductive logic alone. Brewster *was*, however, unscientific or irrational, *not* in any sense that suggests he was a danger to himself or others, but simply in the sense that he did not follow a procedure that seems invariably to have paid off in science. His belief in the continued viability of the emission approach and in the ill-advisedness of the wave theory's monopoly *were* contrary, not to any eternal rules of deductive logic, but to what appears to be best scientific practice. On the other hand, his resistance to believing in the full *truth* of the wave theory was not irrational. The history of science provides no inductive grounds for believing in the truth of the fully fledged, realistically interpreted versions of accepted theories. At any rate in some scientific fields, the truth is rather that, on the contrary, history supports the recently much-discussed "pessimistic induction," which concludes that every fundamental scientific theory, no matter how firmly entrenched it might appear for a time, is eventually rejected and replaced by another theory inconsistent with it.

So how, finally, might a "rationally reconstructed" Brewster have viewed the wave-particle rivalry? (Using this stronger theory of rationality as the basis for reconstruction.) Well, roughly as follows.

> Like my unreconstructed counterpart, I just can't bring myself to believe that there is an ether that fills space from my eye "to the remotest verge of the starry heavens." Nonetheless, the wave theory of light seems somehow or other to have latched onto part of the structure of the universe. As my real counterpart admitted, that theory's striking predictive success surely means that "it must contain among its assumptions . . . some principle which is inherent in . . . the real producing cause of the phenomena of light. . . . " Moreover, there is a good deal of "heuristic steam" left in the wave approach. It cannot possibly do any harm to pursue that approach wholeheartedly. I have no doubt that the approach will *eventually* be superseded by a theory based on a more believable metaphysics. But the lesson of history seems clearly to be that, at any rate *structurally*, the wave theory, like all predictively successful theories, will "live on" within that future theory, perhaps as a limiting case. The metaphysics underlying that future theory may well turn out to be closer to that underlying the present emission approach. But this approach *as it stands* is played out. And again the lesson of history seems clear: that no amount of flogging will revive a horse as dead as this one.

This rationally reconstructed Brewster would surely have found favor with "committed" wave theorists like Airy and Powell—who would have recognized his position as, to all scientific intents and purposes, indistinguishable from their own.

## Notes

1. Kuhn (1962, 151). Kuhn elaborates on his conception of the role of the "objective factors" in his (1977, especially chap. 13).

2. The issue is somewhat complicated by the fact that the orthodox eighteenth-century Newtonian was much more likely to talk about the "parts" or "elements" of light than about material light corpuscles; and indeed, if pressed, would tend to deny any commitment to the material nature of light. Brewster, very much the orthodox Newtonian, did adopt this position and seems to have believed that it implied that his view of light was, if not entirely theory free, then certainly theory neutral. Hence he sometimes presented himself, *not* as defending one theory of the nature of light against a rival theory, but rather as defending one scientific method—an empiricist one, which allegedly stuck closely to the facts—against a rival scientific method, which not only sanctioned highly theoretical entities, in particular the ether, but gave them full scientific honors. This view of his own position (which certainly derives from Newton himself) is confused: the "parts" of light, whether or not one abstains from a theory of their "ultimate nature," are assumed to retain their own identity as they travel, if undisturbed, along their rectilinear paths; if they fail to travel rectilinearly, this is assumed to be due to some action that was, in all but name, a force acting on a particle. The whole idea of "parts" of light, far from being theory neutral, is directly inconsistent in a number of respects, with the classical wave theory. (Although it is true that this was not generally clearly realized even by some of the early nineteenth-century defenders of the wave theory.) I have therefore indulged in a (surely permissible) "rational reconstruction" and treated Brewster as defending one theory against another (as, logically speaking, he surely was). The methodological aspects of the controversy will nonetheless be apparent. (For a detailed account of the "parts" of light and the inconsistency of this instrumentalized version of the emission theory with the classical wave theory, see my (1989).)

3. Brewster is clearly referring (a) to Airy's modification of the Newton's rings experiment in which a metal plate was used as second reflecting surface—Airy showed that, as predicted by the wave theory, the central spot was, for certain angles of incidence, light instead of dark; and (b) to Hamilton's prediction of the totally new phenomena of conical refraction on the basis of Fresnel's equation for the wave surface within biaxial crystals, a prediction experimentally confirmed by Humphrey Lloyd in 1833.

4. Airy and Powell both insisted that any advantage the emission theory had in regard to dispersion and selective absorption was marginal, since nothing like a fully adequate emission account could be given of either phenomenon. See also *below*, pp. 338–39.

5. This was by no means the only major theoretical problem associated with the switch to the elastic solid ether. Another was that a periodic disturbance in an ordinary elastic solid produces a wave with *both* transverse *and longitudinal* components. The longitudinal component seemed to play no role whatsoever in any optical effect. What happened to it? (Much of the history of attempts to solve the problems of the elastic solid ether theory is charted in Schaffner (1972).)

6. Shapere (1966), p. 67.

7. Lakatos (1970), p. 178.

8. Kuhn (1977), chap. 13. Unadorned page numbers *below* in this section refer to this work of Kuhn's.

9. Not all of those who think of themselves as Bayesians accept the principle of conditionalization as a constraint on rational belief. However, unless such Bayesians are ready to specify other constraints governing changes in belief, then the contrast that I want to emphasize between their position and that of the "objectivists" becomes still more marked.

10. They include some "objective Bayesians" who argue that there have to be rules about which prior probability values (or at any rate which *ranges* of probability values) are "rationally permissible."

11. Someone who does *not* make a hash of arguing against the creationists is Philip Kitcher in his (1982).

12. The empirical continuity which, contrary to much presently received opinion, I see in the development of science occurs at the level of what Poincaré called "crude facts" and Duhem "practical facts." Science is undoubtedly *not* cumulative at the level of "scientific" or "theoretical facts" – but this is (a) not surprising and (b) not the major difficulty for a general empiricist view that it is often taken to be. (For more details see my [1978] and especially my [1982].) The history of the simple law of reflection affords an instructive example. If it is regarded as an empirical fact that light is reflected from plane mirrors at an angle equal to the angle of incidence – which, despite its universal character, would not, I admit, strain ordinary usage – then the development of Fresnel's wave theory undeniably did lead to the *rejection* of previously accepted "empirical facts." Not only are the laws of geometrical optics *always strictly wrong* according to Fresnel's theory (on that theory there is just no such thing, strictly speaking, as a ray in the sense of geometrical optics), but also Fresnel's theory (correctly) predicts an *observable* divergence from the simple reflection law in the case of *very narrow* mirrors. But the actual *results* here were not the (very useful) *idealizations* of geometrical optics, but rough and ready ray tracings and the like – *all of which are yielded equally well by Fresnel's theory*. No one before Fresnel had experimented with mirrors narrow enough to produce the observable deviation from the geometrical law predicted by him. (Indeed prior to this prediction there would have been no interest in doing so.)

13. This picture was already completely clear to Duhem and Poincaré. It formed the basis of their insistence that the "explanatory" or "metaphysical" part of science (which had proved subject to change) is valueless, and that it is only the "representative" part of science that really counts (this representative part invariably being carried over into successor theories). For a clarification and defense of Duhem's and (especially) Poincaré's "structural realist" (*not* anti-realist) view of scientific theories, see my (1989).

14. The structure of Kuhn's argument here does seem implicitly to commit him to this "laundry list" view of the objectivists' position. It should be acknowledged, however, that he *does* on occasion nod in the direction of weighting the different criteria; and he is also, as we shall see, sometimes inclined to make empirical accuracy the single most important factor.

15. For further elaboration and references see my (1985).

16. In his (1957), Kuhn took this as an instance of Copernican theory's greater "harmony"; Lakatos and Zahar (1976) show that it is more perspicuously viewed as a genuine prediction (even though of an effect that had long been known to occur when Copernicus formulated his theory).

17. The examples that Paul Feyerabend cites to exemplify "Kuhn loss" are even more curious: the "fact" that the Brownian particle is a perpetual motion machine of the second kind ( a fact "lost" in the statistical-kinetic revolution) and even sometimes the "fact" that phlogiston is given off or absorbed in certain circumstances! The level at which mature science is cumulative is that of Poincaré's "crude facts" ("the needle pointed to somewhere close to 5 on the scale"). Of course, scientists often talk of much higher-level statements as factual or empirical *depending on which theories they take for granted as parts of "background knowledge*." Thus, given certain auxiliary theories the above crude fact may be rendered "the current in the wire was 5 amps." This is an example of a "scientific fact" for Poincaré. Of course if certain very high-level theories are taken for granted, then we can get *very* high-level empirical facts – a description of the individual situation as seen in the light of presently accepted theories ("certain free electrons move through the wire so as to create a current of a certain strength," or whatever). It hardly needs to be said that, in view of historical changes in accepted high-level theories, and even in "background knowledge" auxiliary theories, *such* facts described in these highly theoretical terms may well be "lost" as a result of theory change in science. (See my (1978) for further details.)

18. The predictive success enjoyed by Fresnel's and Copernicus's theories and not, so far as I can tell, by the emission theory or by Ptolemy's, concerns the prediction of general *types* of phenomena:

planetary stations and retrogressions, the diffraction pattern for small circular opaque disks, or whatever. Almost every theory, of no matter how "cobbled up," "degenerate" or ad hoc a kind, will of course enjoy "predictive success" of a straightforward inductive, extrapolative kind. Having fixed its various epicyclic parameters on the basis of the observation of an orbit (or series of orbits) of the planets, the Ptolemaic theory will, of course, go on to predict the future orbits of those planets.

19. For an account of some of the details of the corpuscular-theoretic accounts of interference and diffraction see my (1976).

20. See my (1985) for details. Kuhn's talk of "shoving" phenomena into the older paradigm's "box" seems to imply that he is implicitly aware of this point. But, so far as I can tell, he never explicitly considers how distinguishing between shoving a phenomenon into a theoretical framework and having a phenomenon "fall out" of that framework (and giving more epistemic weight to a theory in the latter case) might alter his view of the empirical justification for theory change.

21. Certainly the photon theory does not constitute such a revival of the Newtonian corpuscular theory in any significant sense. Photons are of course "something like" material particles—but then any two things, no matter how dissimilar, are something like each other in some respects; here the *dis*similarities are overwhelming.

22. See, for example, Musgrave (1976) and some very interesting, forthcoming work by Philip Kitcher.

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