*JOHN WORRALL**

THE PRESSURE OF LIGHT: THE STRANGE CASE OF THE VACILLATING 'CRUCIAL EXPERIMENT'

Introduction

DOES A BEAM of light carry a momentum and hence exert a pressure on any absorbing or reflecting body on which it falls? This straightforward question seems to admit of a straightforward decision procedure: train a beam of light on a very mobile object and see whether it moves. And indeed, a series of investigators from the turn of the eighteenth to the beginning of the twentieth century performed experiments of precisely this kind. These experiments had much more than the usual share of importance; not only is the question of whether or not light exerts a pressure an intrinsically interesting one, it also seemed to be of the highest relevance for the comparative appraisal of two of the general theories of light current in the eighteenth and nineteenth centuries: the corpuscular theory and the wave theory. Indeed these pressure experiments seem, superficially at least, to be potentially *crucial* experiments for deciding between those two theories.

In the first section of this paper I shall sketch the story of these experimental investigations. It is, as we shall see, full of fascinating twists and turns: both the experimental verdict and the supposed theoretical relevance of the verdict underwent more than one about-face. In the second section, I shall use this history to try to illustrate what I take to be important methodological lessons (about how science develops, about 'crucial experiments' and the role of experiments generally, and about the alleged 'theory-ladenness' of all observation statements).

I. Light, Waves, Particles and Pressure: Newton to Einstein

(i) *An overall view of the theoretical situation*

The usual view is that there were basically just two theories of the

*The London School of Economics and Political Science, Department of Philosophy, Houghton Street, London WC2A 2AE, U.K. I should like to thank Peter Milne, Heinz Post, Martin Sadler, Peter Urbach, John Watkins and Elie Zahar who all either heard or read earlier versions of this paper and made helpful criticisms. I am also very grateful for the encouraging and stimulating comments on an earlier version made by Dr. Geoffrey Cantor, Professor Gerald Holton and Professor A. I. Sabra. My biggest debt, however, is to Michael Redhead whose criticisms led to radical revisions of the paper and who supplied me with several important references.

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constitution of light in the period from the turn of the seventeenth century to the turn of the twentieth: the corpuscular theory (associated particularly with Newton, and then Laplace and Biot) and the wave theory (associated with Huygens and then Young, Fresnel and Maxwell). Not surprisingly, this view is a simplification of the real state of affairs.

First of all, there were great differences between the various views usually grouped together as 'wave theories'. For example, Huygens is usually regarded as the forerunner of Young and Fresnel. But, although Huygens shared the view of his successors that light consists of impulses transmitted through an all-pervading medium, his ideas differed from those of Young and Fresnel in several crucial respects. Not only did Huygens's etherial disturbances occur in the same direction as the overall transmission of light, whereas Fresnel eventually came to assume that the disturbances occur transversely to the direction of propagation, but also Huygens's disturbances were *irregular* 'pulses', quite different from the periodic wave-like disturbances of the later theory.[1] Euler held a view in some ways closer to the 'classical' wave theory whilst in other ways it was still more 'distant'.[2] Moreover, as I shall be emphasizing *below,* it is a mistake to see even the 'mainstream', 'classical' wave theory as undergoing a smooth continuous development.

The picture of the corpuscular theory becomes similarly less clear-cut when viewed more closely. Not only did Newton, the founding father of the theory, refuse to commit himself to it publicly, he also hypothesized an ether — disturbances in which play an important role in optical phenomena. Many of the eighteenth-century light-theorists who developed the 'Newtonian' corpuscular theory, while happy to accept the existence of forces acting on the light corpuscles, would have nothing to do with the 'occult' ether.[3] Moreover, whereas most Newtonians viewed their light corpuscles as ordinary material particles, Boscovich proposed indeed to replace all particles (including particles of light) by force fields, centred on the point-like particles but

[1]For more details of Huygens's theory and its differences from more modern views see particularly A. I. Sabra, *Theories of Light from Descartes to Newton* (Oldbourne, 1968).

[2]Some of these differences are touched on below, pp. 157 – 9. The most accessible account of the general features of Euler's theory of light is in his *Letters...to a German Princess,* trans. Henry Hunter, D. D., 2nd edn (London, 1802).

[3]They were, in the terminology of R. E. Schofield's fascinating book, 'mechanists' rather than 'materialists'. See. R. E. Schofield, *Mechanism and Materialism,* and, for a detailed account of how aversion to the ether affected the attitude toward the wave theory of light of such scientists as Henry Brougham, see G. N. Cantor, 'Henry Brougham and the Scottish Methodological Tradition', *Studies in the History and Philosophy of Science* **5** (1971), 69 – 89. There certainly were other scientists who held Newton's own dual view. Joseph Priestley, for example was entirely convinced of the materiality of light and at the same time that 'some phenomena give us reason to suppose' that the ether exists, *History and Present State of Discoveries Relating to Vision, Light and Colours* (1772), p. 384.

extending, strictly speaking, through all space.[4]

Finally, there were various theories which cannot readily be regarded even as variants of either the wave or corpuscular approaches. A celebrated example is Descartes' theory which, although it identified light with an impulse transmitted through a medium, was quite different from the later theory of Huygens, particularly as Descartes assumed that these impulses were transmitted *instantaneously*. Also, Descartes employed a *corpuscular* model when it came to explaining reflection and refraction.[5] And there were theories of light viewed as a fluid, like the heat fluid (caloric) or the electrical fluid (or fluids).[6] Whether or not these fluids should be regarded as 'material' is a matter of dispute; certainly they were not necessarily regarded as 'mechanical' — *i.e.* as obeying Newton's laws. (They were not usually regarded as possessing inertia, for example.)

But despite the existence of alternatives and of various 'mixed' theories, the two basic ideas about light — that it consists of particles emitted from luminous bodies *or* that it consists of impulses or disturbances created by luminous bodies and transmitted through an all-pervading medium — had many influential adherents in the seventeenth, eighteenth and nineteenth centuries. And it was the development of specific variants of these two ideas which was to play the most important role in the success of physical optics. This paper will be concerned with how these two views developed and particularly with the part that the question of the pressure of light played in their rivalry.

(ii) *The Newtonian, corpuscular theory and light pressure*

The corpuscular theory of light has a long history, but the man who did most towards turning it into a serious scientific hypothesis was undoubtedly Isaac Newton. The theory which his eighteenth-century successors took to be the orthodox Newtonian view is appealingly clear-cut.[7] Luminous bodies emit high-velocity particles. These are affected by forces which emanate from 'gross' matter and which act on the particles at a (short) distance to deviate them from their naturally rectilinear paths. Reflection is caused by a repulsive

[4]See particularly his *A Theory of Natural Philosophy,* trans. J. M. Child (M.I.T. Press, 1966). I was reminded of Boscovich's theory of light by Schofield's book (see preceding footnote) and by an excellent article by Morton L. Schagrin, 'Early Observations and Calculations on Light Pressure', *American Journal of Physics* **42** (1974), 927, which deals with the eighteenth-century experiments on light pressure in rather more detail than my paper does, and to which I am indebted for some references. (I have some disagreements with Schagrin's methodological remarks, as will emerge below.)

[5]For a detailed and particularly enlightening discussion of Descartes' theory, see Sabra, *op. cit.*

[6]See Schofield, *op. cit.*, and Schagrin, *op. cit.*, for more details.

[7]For a retrospective, but representative, account of this theory, see especially J. F. W. Herschel, 'Treatise on Light', *Encyclopaedia Metropolitana* (1827).

force, refraction into a denser medium by an attractive force and the various effects we now attribute to diffraction are caused by 'inflecting' forces which seem to be alternately attractive and repulsive at different distances. Once the forces are given, the path of any light particle is in principle specified by the laws of Newton's own mechanics.

Newton's great disciple Samuel Clarke, for instance, wrote

> The Reflexion of the Rays of Light is caused. . . by a certain power equally diffused all over the Surface of the Body whereby it acts upon the Ray to attract or repel it, *without immediate Contact;* by which same Power in other circumstances the Ray is refracted. . . as the fore-cited admirable Person has demonstrated by many Arguments.[8]

There is no doubt that this view is to be found in Newton's work on optics. His treatment of refraction in the *Principia* proved particularly influential. He there demonstrated that if a moving particle is subject to no net external force, except during its passage through a narrow region bounded by two parallel planes, and if the force in that region satisfies certain conditions,[9] then 'the sine of incidence [of the particle] upon either plane [bounding the 'active region'] will be to the sine of emergence from the other plane in a given ratio'.[10] This was probably the most often-cited result in optics in the eighteenth century. Prévost, for example, wrote:

> nothing is better proved in optical theory than the proposition which establishes that refraction is produced by an attraction directed perpendicularly to the refringent surface (*Principia* 1, Proposition XCIV).[11]

Biot and Arago claimed that 'Newton *proved* that the change in direction [in refraction] was owing to an attraction which bodies exercise upon the elements of light. . . .'[12] And Newton's work set a model for further developments: such as Laplace's attempt to deal with double refraction —

[8] *Rohault's System of Natural Philosophy; Illustrated with Dr. Samuel Clarke's Notes Taken mostly out of Sir Isaac Newton's Philosophy,* p. 209.

[9] That the force is perpendicular to the bounding planes and a function only of the distance from either plane.

[10] *Principia,* Proposition XCIV, Theorem XLVII of Section XIV. For more detailed analysis of Newton's demonstration, see Sabra, *op. cit.,* and D. T. Whiteside's editorial notes in *Mathematical Papers of Isaac Newton,* 6, pp. 428–436. As Sabra and Whiteside indicate, Newton's demonstration (which depends crucially on the assumption that the refracting force has no horizontal component) has much in common with Descartes' account of the phenomenon. The chief difference formally is that Newton derived the proportionality of the velocities of the 'rays' in the two media from his assumption about the acting forces, whilst Descartes simply assumed this proportionality. From the more general point of view, the difference is much more striking. Descartes' explanation is completely at odds with his general theory of light. Newton's is perfectly consistent with the general theory that light consists of material particles acted on by attractive and repulsive forces — a theory which Newton's successors generally attributed to him. Hence the impact of Newton's demonstration. See below.

[11] P. Prévost, 'Quelques remarques d'optique, principalement relatives à la réflexibilité des rayons de la lumière', *Philosophical Transactions of the Royal Society* **88** (1798).

[12] J. B. Biot and F. Arago, 'Upon the Affinities of Bodies for Light . . .', *Philosophical Magazine* **26** (1806).

> It would be extremely interesting to reduce [the law of double refraction], as Newton has reduced the law of ordinary refraction, to the action of attractive and repulsive forces, of which the effects are sensible only at insensible distances. . . .[13]

However, as Newton scholars have emphasized, Newton's own optical views were a good deal more complicated and subtle. For example, Newton entertained the hypothesis of an ether which played an important role in optical effects: the passage through the ether of the light particles causes wave-like disturbances which overtake the particles and successively put them into 'fits of easy reflection' and of 'easy refraction'.[14] Moreover, at least in some places, Newton inclined to the view that the apparent action-at-a-distance forces which emanate from gross matter and affect the light particles might, in fact, be reducible to density gradients in the ether.[15]

Also Newton, as is well known, vigorously asserted that he was by no means committed to the real existence of the light corpuscles. For example, his famous (and frosty) response to Hooke's criticism of his 'First Paper on Light and Colours' of 1671 – 1672 (where Hooke had characterized the corpuscular hypothesis as Newton's 'first proposition') had included the avowal that:

> 'Tis true, that from my Theory I argue the corporeity of Light; but I do it without any absolute positiveness, as the word *perhaps* intimates; and make it at most but a very plausible *consequence* of the Doctrine, and not a fundamental Supposition. . . .[16]

And, even when giving the account of refraction which proved to be so influential in establishing the corpuscular theory, Newton had insisted that he was 'not at all considering the nature of the rays of light, or inquiring whether they are bodies or not', but rather was merely demonstrating 'the analogy there is between the propagation of the rays of light and the motion of bodies'.[17]

The fascinating details of Newton's beliefs need not, however, detain us here. Whether or not Newton believed in corpuscles, his method of proceeding — particularly in his treatment of refraction — involves the idea that light comes in discrete parts which move under the action of forces in accordance with the laws of mechanics.[18] His results and speculations in the *Opticks*

[13]P. S. de Laplace, 'Sur la loi de la réfraction extraordinaire dans les cristaux diaphanes', *Journal de physique* (January 1809).

[14]See particularly Newton's *Opticks*, 7th edn, pp. 278 – 88 of the Dover paperback edition.

[15]See for example his 1675 paper, 'An Hypothesis Explaining the Properties of Light', *Philosophical Transactions of the Royal Society*, reprinted in I. B. Cohen (ed.), *op. cit.*, pp. 177 – 235.

[16]'Mr. Isaac Newton's Answer to Some Considerations on His Doctrine of Light and Colours', *Philosophical Transactions of the Royal Society* (1672), p. 5086; reprinted in Newton's *Correspondence* I, and in I. B. Cohen (ed), *Isaac Newton's Papers and Letters on Natural Philosophy*.

[17]*Principia*, pp. 230 – 231 (Motte – Cajori translation).

[18]Sabra too agrees with this view: 'It was particularly obvious from [Newton's] explanation of refraction that he was relying on a corpuscular view . . .' (*op. cit.*, p. 314). Also: 'Newton's conception of rays was, from the beginning, that of the corpuscular theory . . .' (*op. cit.*, p. 294).

suggest that prismatic dispersion may be due to the particles corresponding to the different spectral colours having different masses and therefore being deviated by different amounts by the same forces, and they suggest too that all the phenomena of 'inflection' are caused by real inflecting forces. Whether or not these forces can be further reduced to ether density gradients, and whether or not the ether needs to be invoked to explain other effects, are questions that can be held in abeyance until it has been seen how much can be achieved by the attempt essentially to reduce optics to particle mechanics.

It was certainly this programme which many of his eighteenth-century successors took from Newton. As already mentioned, Laplace attempted to develop an account of double refraction precisely on these lines. He also speculated on the effects of gravity on the light corpuscles and even suggested the possibility of 'black holes'. A variety of scientists (Malus, Brougham, Jordan among them) tried to develop the ideas about 'inflection' in the *Opticks* explicitly appealing to 'inflecting forces' which affect the light corpuscles as they pass the edges of opaque objects. J. B. Biot's was perhaps the most systematic attempt to develop the corpuscular theory: he tried to explain diffraction, double refraction and polarization effects in terms of polarized (and revolving) particles. (Hence Biot could ascribe a periodicity to light particles without invoking any ether-produced 'fits'.) Finally, in this by no means complete list, the Scottish physicists Robison, Playfair and Wilson worked on the optics of moving media, assuming (if rather tentatively) a corpuscular theory which Robison himself described as the view that

> light may perhaps consist of small particles emitted by the shining body with prodigious velocity, which are afterwards acted upon by other bodies, with attracting or repelling forces like gravity, which deflect them from their rectilineal courses. . . .[19]

Newton invented this 'research programme' whether or not he believed in its 'hard core'.

This whole approach clearly involves the assumption that the light corpuscles have inertial mass. Again Newton (and others) *may* have had certain qualms about this assumption, and insisted on speaking non-committally in terms of 'Rays' of light. But the whole programme makes no sense without the assumption: what effect would forces have on mass-less particles?[20] That light has inertia was clearly realized to be an implication of

[19]John Robison, 'On the Motions of Light, as Affected by Refracting and Reflecting Substances which are also in Motion', *Transactions of the Royal Society of Edinburgh* 2 (1788), 96 – 97. For more details of this corpuscular optics research programme see my 'Thomas Young and the "Refutation" of Newtonian Optics', in C. Howson (ed.), *Method and Appraisal in the Physical Sciences* (1976).

[20]Of course, a Bosvichean reduction of *all* matter to immaterial force fields would still leave matter, and hence the light corpuscles, with inertia.

Newton's treatment of refraction. For example, Adam Walker wrote:

> That light is matter, or material, cannot be doubted when we observe the inflections it suffers in passing out of one medium into another . . . .[21]

But if the light particles have mass (though no doubt very little of it) then a beam of light consists, on the corpuscular view, of a large number of high-velocity material particles and hence must carry a momentum (found simply by summing the momenta of the individual particles). This momentum should manifest itself in any reflecting or absorbing body on which the beam falls. *Light should exert a pressure.*[22]

(iii) *Eighteenth-century experiments on light pressure*

The corpuscular theory's prediction that light exerts a pressure was not a new one. Kepler did not hold a corpuscular theory but did believe in light pressure and he cited the fact that comets' tails are always directed away from the sun as evidence of the existence of such a pressure. Kepler's guess that the direction of the comets' tails is explained by pressure of solar radiation on the gases making up the tails is nowadays regarded as (at least partially) correct. But the phenomenon could be given alternative explanations: Newton, for example, was inclined to the view that the vapours in the comet's tail are heated by the sun, are rarefied and hence move away from the sun, in the same way that smoke rises up a chimney away from a fire.[23] Some more down-to-earth manifestation of light's alleged momentum would have to be found if it was to become an accepted phenomenon.

Nothing seems to present itself as an obvious embodiment of light pressure — we do not, for example, feel any 'solar wind' on our faces. And many eighteenth-century scientists[24] spent much time calculating how small the particles of light must be, given that delicate flowers and the like were not destroyed by their impact. Benjamin Franklin for one clearly thought that this lack of any obvious effect constituted a difficulty for the whole corpuscular

[21]Adam Walker, *A System of Familiar Philosophy in Twelve Lectures* (London, 1796), p. 2. (I was guided to Walker's book by Schagrin's article, *op. cit.*) Aside from this theoretical reason, there were several more empirical reasons which persuaded eighteenth-century scientists of the materiality of light — for example the phenomenon of phosphorescence.

[22]'It would certainly go a great way towards proving the *materiality* of the rays of light, if it could be observed that they had any *momentum* so as, by their impulse, to give motion to light bodies.' (Priestley, *op. cit.*, p. 385.)

[23]'The ascent of smoke in a chimney is due to the impulse of the air with which it is entangled. The air rarefied by heat ascends, because its specific gravity is diminished, and in its ascent carries along with it the smoke which floats in it; and why may not the tail of a comet rise from the sun after the same manner.' *Mathematical Principles of Natural Philosophy*, Book III, Proposition 41 (p. 528 of the Motte – Cajori translation).

[24]Pieter van Musschenbroek and Joseph Priestley were two such. See below, p. 141 and p. 144.

approach. Writing to his friend Cadwallader Colden in 1752, he confessed himself

> much in the dark about light.[25] I am not satisfied with the doctrine that supposes particles of matter called light continually driven off from the sun's surface with a swiftness so prodigious. Must not the smallest particle conceivable have, with such a motion, a force exceeding that of a twenty-four pounder discharged from a cannon?. . .Yet these particles with this amazing motion will not drive before them, or remove, the least and lightest dust they meet with. . . .

One obvious reply to Franklin is that his powers of conception seem rather weak: clearly it *is* quite conceivable that the mass of a light particle might be so minute that, despite its enormous velocity, its momentum is too small to be detected easily. This was essentially the reply made to Franklin by Bishop Samuel Horsley in 1770 — though Horsley was rather less succinct. Making various assumptions which he justifies at some length but which remain rather arbitrary (such as that the diameter of each light particle is of the order of 10^{-12} inches and that its density is of the order of three times that of iron), Horsley calculated that the momentum of a light particle will be less than that of an iron ball of a quarter of an inch diameter moving at a velocity of less than an inch in 1.2×10^{10} Egyptian years.[26]

However, it was not even clear at the time Franklin wrote that he was correct in thinking that light cannot 'drive before [it] the least and lightest dust'. For there had already been several claims in the literature to have detected light's momentum.

Indeed as early as 1696 the Dutchman Nicolaas Hartsoeker had claimed that the effects of the impulse of light are many, varied and large-scale. In his famous *Principes de physique,* Hartsoeker wrote:

> if one exposes a small spring to the focus of a burning glass, one will see that spring make quite noticeable vibrations. The rays of the sun chase smoke from the top to the bottom of a chimney. Travellers assert that the Danube is much less rapid in the morning, when the rays of light oppose its course, than it is in the afternoon, when it aids that course. Everybody knows that the Meuse has a rather large tide at the northeast of its mouth; and as the river usually swells around half

[25]This *may* have been the first time this joke was cracked (though I doubt it) — it most certainly was not the last. Franklin's letter is quoted from I. B. Cohen (ed), *Benjamin Franklin's Experiments. A New Edition of Franklin's Experiments and Observations on Electricity* (Harvard University Press, 1941).

[26]Reverend Samuel Horsley, 'Difficulties in the Newtonian Theory of Light, Considered and Removed', *Philosophical Transactions of the Royal Society* (1770). The 'charm' which Schagrin (*op. cit.*) sees in Horsley's calculations has, I'm afraid, eluded me. Priestley also pointed out that 'Admitting the materiality of light, it must however be acknowledged, that the particles of which it consists are extremely minute, and, notwithstanding its amazing velocity, that its *momentum* is very small.' (*Op. cit.*, p. 381.)

a foot more at night than in the day. . . it seems that one can attribute this phenomenon to the sun's rays, which, for the greater part of the day, chase the sea from the land, towards which it however approaches at night when the sun is set and its rays chase it no longer.[27]

And, in his *Cours de physique,* published posthumously in 1730, Hartsoeker claimed that

when a handful of sand is exposed to the focus of a burning glass, that sand is driven away and immediately dissipated, as if by a gust of wind. . . .[28]

Hartsoeker's speculations about tidal effects and the like may now seem rather quaint (indeed, as early as 1734 de Mairan was to have great fun at their expense), and even the smaller 'effects' with the spring and sand were no doubt due to some mechanical or perhaps thermal disturbance. But the German Wilhelm Homberg made very similar experimental claims in 1708. Homberg was employed by the Duke of Orleans in Paris and therefore had at his disposal the Duke's formidable three-foot concave burning mirror made of polished copper.[29] It may have been this which he used in the experiments reported by his friend de Fontenelle in the French Academy's *Histoire* of 1708; Fontenelle being then the Academy's current Permanent Secretary. In Homberg's first experiment, particles of amianthus (a form of asbestos) were allegedly disturbed by exposing them to the sun's rays focused by a burning glass; and Homberg's second experiment (remarkably similar to Hartsoeker's 1696 'result') involved attaching one end of a spring to a piece of wood and 'striking' the free end with a beam focused by a lens — the result being that the spring 'vibrated very sensibly as if it had been hit with a stick'.[30]

Homberg's experiments proved indeed quite influential. For example, in a sort of early textbook which appeared in 1736 and was translated into French as *Cours de physique experimentale et mathématique,* 1769, Pieter van Musschenbroek cited Homberg's experiments as *proving* the materiality of light:

light is material and solid, because light rays collected and condensed at the focus of a burning glass, and directed on little pieces of amianthus move them and turn them about themselves, directed on a watchspring, fixed at one end to a piece of wood, set the other end of the spring in vibration as *Homberg* observed.[31]

[27]Nicolaas Hartsoeker, *Principes de physique* (1696), p. 137; my translation. The section of Hartsoeker's book which contains this passage is headed 'Experiments which show that the rays of light have some force to give movement to bodies which they meet in their path'.

[28]Hartsoeker, *Cours de physique* (1730), p. 85; my translation.

[29]I obtained this information from J. R. Partington, *A History of Chemistry,* Vol. III.

[30]*Histoire de l'Académie Royale des Sciences* (1708), p. 21; my translation.

[31]Musschenbroek, *Cours de physique experimentale et mathématique* (1769).

Indeed the only interesting question for Musschenbroek was the further one of whether ordinary gross matter, if sufficiently subdivided and attenuated, could be converted into light, or if, instead, light is 'a body of a peculiar kind, which differs from all other bodies in virtue of qualities which come together in it alone'.[32] Musschenbroek seems to have been unaware of some further experimental research which had already been conducted by the time he wrote. This was made by J. J. Dortous de Mairan and reported in a paper delivered to the French Academy in 1731. (The paper was published in the *Histoire de l'Académie Royale des Sciences* for 1747.) Mairan analysed Homberg's results and concluded that they exhibited

> only chance and irregular disturbances, sudden jumps excited by the heat, by the rarefaction and sudden explosions of the air which surrounds these objects, and not at all that constant and sustained movement to which the flux of the rays at the focus of the mirror should give rise [according to the idea that light exerts a pressure].[33]

Mairan, therefore, seems to have been the first to raise the problem which was to bedevil all serious attempts to detect the pressure of light: the alternative possibility that any effects might be caused by the heat produced by the light.

Mairan, in his attempt to test Homberg's conclusion more severely, seems also to have been the first to hit on the idea of trying to detect the pressure of light by focusing a beam on some sort of pivoted apparatus of low friction (like a compass needle or a torsion balance). The vast majority of all subsequent attempts to detect light pressure have simply been repetitions of essentially this same experiment.

Mairan could find no unambiguous light pressure:

> I . . . wanted to test the alleged impulse of solar rays focussed by a glass of 6 inches diameter on compass needles, of whatever declination and inclination, of 4 and 6 inches in length. There resulted only equivocal flutterings *(tremoussemens equivoques).* M. du Fay and I constructed a kind of mill of copper, extremely mobile; we trained on it the focus of a lens. . . and drew out only the same uncertainty. I have since procured a similar machine — lighter and more skilfully suspended. It is a horizontal iron wheel of around 3 inches diameter, having 6 radii at the end of each of which is a little oblique vane; the wheel's axis which is also of iron is held at its top only at the end of a magnetic iron rod. The wheel and its axis hardly weighed 30 grains in all. Nothing is more mobile than that wheel; but, at the same time, nothing is less certain than the induction one might like to draw in favour of the impulse of the rays.[34]

[32]*Op. cit.*

[33]J. J. Dortous de Mairan, 'Eclaircissemens sur le traité physique et historique de l'aurore boréale', *Histoire de l'Académie Royale des Sciences* (1747), pp. 363 – 435; the passage quoted is from p. 426 — my translation.

[34]*Op. cit.*, p. 427.

Indeed as the light was focused on one of the vanes, the 'machine turned sometimes to one side, sometimes to the other'. And Mairan concluded that the best explanation of these results was that they were due to the heating of the air around the machine:

> . . . the explosion of a mass of air, suddenly and unequally heated, around the vane on which the light was focused seems to me to yield a sufficient reason for these effects.[35]

Mairan also realised that the 'natural' next step was to remove '[t]he perpetual obstacle of this air' by performing the same experiment in a vacuum. Interestingly, the fact that theories are always involved both in pointing to which experiments are important and which are possible is again illustrated here; this time with results which were, in view of later developments, unfortunate. Mairan decided that he 'need not give [himself] the trouble' of repeating the experiment in a vacuum because he was

> persuaded that there is in our atmosphere amongst that more gross air which we breathe and which does not at all penetrate glass, another more subtle air or some other fluid which penetrates glass.[36]

And the possibility of convection currents in this more subtle air would mean that this modified experiment would not be any more revealing. Mairan seems here illicitly to be assuming, independently of experiment, that light does indeed exert a pressure — a fact in which, as a corpuscularist, he firmly believed.[37] For, had he reperformed the experiment *in vacuo* and found the erstwhile movement *quelled* then it would follow that this 'more subtle air' could play no role in the phenomenon.

Be all this as it may, Mairan had certainly reversed Homberg's experimental verdict. Not many years later, however, this reversal seemed itself to have been overturned. The Reverend John Michell (famous for his role in inspiring Cavendish's measurement of the gravitation constant) performed experiments which seemed to establish a light pressure. Michell did not himself publish any account of his experiments, and the only account we have of them is through his friend Joseph Priestley's famous 1772 book *The History and Present State of Discoveries Relating to Vision, Light and Colours.*

According to Priestley, Michell suspended a ten-inch long piece of harpsichord wire on a sharply-pointed needle. A very thin plate of copper was

[35] *Op. cit.*, p. 428.

[36] *Ibid.*

[37] 'Light is certainly a body . . .. It thus exerts an impulsive force against bodies which it encounters in its path, if it moves, and it does move, since it comes to us from the sun.' *Op. cit.*, p. 424.

fastened to the end of the wire, and a 'middling sized shot corn' as balance, to the other. Across the middle of the wire, and at right angles to it, there was a short magnetized needle, by means of which the wire could be oriented at will once the apparatus (which weighed in all only 'about ten grains') had been encased in glass. The experiment consisted of focusing a light beam on the copper plate and the result, according to Priestley, was that

> [i]n consequence of this, the copper plate began to move with a slow motion, of about an inch in a second of time, till it had moved through a space of about two inches and a half, when it struck against the back of the box.[38]

The experiment was repeated successfully several times. Moreover, by shining the light on the other side of the copper plate the apparatus was made to revolve equally successfully in the reverse direction. Priestley advocated the repetition of the experiment by others. Nevertheless, even as it stood,

> There seems to be no doubt, however, but that the motion. . . is to be ascribed to the impulse of the rays of light.[39]

And Priestley went on to use Michell's result to ground estimates of the size of the light particles (and of the wasting away of the sun caused by its emission of light).

What of the possibility that the movement of Michell's apparatus was caused by convection currents? Priestley *did* allow that these play a role in the phenomenon but *only* once the copper plate had become distorted by the heat of the focused rays. Once distorted, the plate did indeed begin

> to act in the same manner as the sail of a windmill being impelled by the stream of heated air, which moved upwards, with a force sufficient to drive it in opposition to the impulse of the rays of light.[40]

But Priestley implicitly discounts the possibility that the heating of the air around the apparatus played a role in its movement right from the start — certainly he mentions no precautions taken to exclude this possibility.[41]

This seems a strange omission especially in view of Priestley's earlier reference to Mairan's memoir. And in fact it was not very long before a new experiment was performed which supported Mairan's view that any observed

[38]Priestley, *op. cit.*, p. 389.

[39]*Ibid.*

[40]*Ibid.*

[41]Priestley does say that the point of encasing the apparatus in 'a box the lid and front of which were of glass' was to 'prevent its being disturbed by any motion of the air' (*op. cit.*, p. 388). But, since the air was not pumped from the box, Michell and Priestley clearly had in mind only to shield the apparatus from extraneous air currents.

movement in these experiments was to be attributed to the heating of the air. The Reverend Abraham Bennet published a paper in the *Philosophical Transactions of the Royal Society* for 1792, which included the following very brief description of an experiment of his:

> To the end of a fine gold wire, three inches long, and suspended by a spider's thread in a cylindrical glass, was fastened a small circular bit of writing paper: light was admitted through a small hole and also the focus of a large lens was thrown upon the paper, with the intention of observing whether it would be moved by the impulse of light: but though these experiments were often repeated, and once with the paper suspended in an exhausted receiver, yet I could not perceive any motion distinguishable from the effects of heat.[42]

Bennet's result certainly appears to be an improvement over its predecessors, since the experiment was performed, 'once' at any rate, in an 'exhausted receiver'. His report of it is, however, extremely unsatisfactory: he gives no further details at all either about the exact air pressure or about the actually observed movements of his apparatus or about what he had expected the 'effects of heat' to be. Despite this, Bennet's result was often quoted during the early nineteenth century as strong support for the non-existence of a pressure of light.[43]

As for the theoretical relevance of his result, Bennet explicitly suggested that his failure to detect a pressure supports the wave-theoretic view and undermines its corpuscle-based rival:

> Perhaps sensible heat and light may not be caused by the influx or rectilinear projection of fine particles: but by the vibrations made in the universally diffused *caloric* or matter of heat, or fluid of light.[44]

(iv) *Does the wave theory of light predict a light pressure?*

Bennet's remark raises the question of whether or not the wave theory of light predicts the existence of a light pressure. This question turns out to be surprisingly complex. The prediction the corpuscular theory makes here is entirely straightforward — a light beam consists of particles in motion, the particles possess inertia and hence the beam carries a momentum. Is Bennet right that the rival wave-theoretical view predicts no pressure, so that the pressure question does indeed afford, at least in principle, the opportunity of a crucial experiment between the two theories?

[42]Bennet, 'Experiments on a New Suspension of the Magnetic Needle', *Philosophical Transactions of the Royal Society* (1796), p. 87.

[43]See, for example, below, p. 146.

[44]Bennet, *op. cit.*, pp. 87 – 88.

I shall be analysing the logical aspects of this question at some length below, but if, for the moment, we restrict ourselves to the sociological aspects of the matter, then there is no doubt that Bennet was expressing the near-unanimous view of eighteenth- and early-nineteenth-century scientists. We have seen, for example, that Musschenbroek held that Homberg had shown the existence of light pressure and that this actually *proved* the materialist hypothesis — a position he could hardly have adopted had he believed that the wave theory too predicted a light pressure. Thomas Young, the famous champion of the wave theory, writing in 1802, was at pains to discount Michell's results which he clearly saw as a threat to his theory. And Young enthusiastically endorsed both Bennet's experimental result and his view that the result favoured the wave theory:

> [Bennet] very justly infers from [his] total failure [to detect a pressure] an argument in favour of the undulatory system of light.[45]

Humphrey Lloyd (the Irish experimental physicist who first discovered the phenomena of conical refraction which provided such important support for Fresnel's wave theory) in 1834 wrote an excellent 'Report on the Progress and Present State of Physical Optics'. In his report, Lloyd referred to the pressure issue as seeming to afford a 'criterion of truth' of the emission theory (and hence presumably of the falsity of its wave rival).[46]

By the time Lloyd wrote, the great wave revolution in optics was already taking place. Due almost entirely to developments of it made by Fresnel, the wave theory became, and was for long to remain, the dominant theory in optics. Fresnel's achievements turned the spotlight onto quite different questions than that of light pressure.[47] The general attitude to this question in the first half of the nineteenth century seems, however, to have been that, because the results of the latest experiment (by Bennet) were negative, the light pressure issue formed another confirmation of the wave theory — an attitude which, of course, presumes that the wave theory predicts no light pressure. This attitude is clearly reflected, for example, in Lloyd's report and in his later *Elementary Treatise on the Wave-Theory of Light*. Lloyd had no doubt that Bennet's is the correct result:

> it is now universally conceded that no sensible effect of the *impulse* of light has ever been perceived. The experiments of Mr. Bennet seem to be decisive on this point.[48]

[45]Thomas Young, 'On the Theory of Light and Colours', *Philosophical Transactions of the Royal Society* (1802); reprinted in *Miscellaneous Works of the Late Thomas Young*, Vol. I., George Peacock (ed.) (1855), p. 167.

[46]Humphrey Lloyd, 'Report on the Progress and Present State of Physical Optics', *British Association for the Advancement of Science Reports* 4 (1833), 300.

[47]Although Fresnel *did* attempt to detect a light pressure and thought he had succeeded in detecting the pressure of radiant heat — see below, p. 148.

[48]Lloyd, *Elementary Treatise on the Wave-Theory of Light* (London: Longmans, 1857), p. 10.

Nor did Lloyd have any doubt that the wave theory predicted precisely this result. Indeed Lloyd's main concern (for he was exceptionally sophisticated methodologically) was to warn the wave-theorists not to regard this result *too easily* as a crucial experiment. The corpuscular theory had undoubtedly been defeated but (as always) the defeated theory could be modified in an *ad hoc* way so as to avoid the seemingly conclusive refutation:

> it is easy [for the adherents of the corpuscular theory] to attribute to the molecules of light a minuteness sufficient to evade any means that we possess of detecting their inertia by their effects on other bodies. . . .[49]

The view that Bennet's result supported the wave theory continued to be widespread far into the nineteenth century. Balfour Stewart, for example, in his *Elementary Treatise on Heat* of 1866 remarked:

> . . . the experiments of Mr. Bennet showed all absence of momentum when the concentrated light of the sun was made to strike a piece of paper. . . [from which] we must conclude that light particles do not give a blow, and hence that the emission theory of light is not true; and if this theory be not true, we must have recourse to the undulatory or some similar theory which assumes the existence of a medium pervading space.[50]

(Stewart did not actually say so, but his argument clearly presupposes that the undulatory theory predicts that light beams carry no momentum.)

This assessment of the theoretical situation was itself soon to be reversed. But before coming to this reversal, there are some further about-faces on the experimental side to report.

(v) *William Crookes and the 'light-mill'*

In the 1870s William Crookes (noted amongst other things for his discovery of thallium) became firmly convinced that he had detected experimentally the pressure of radiation in general and of light in particular. Crookes first reported his discoveries in 1874.[51] He clearly shared the opinion (which I have just been arguing was general) that the wave theory predicted no light pressure, for he regarded his results as constituting a definite difficulty for that theory. He did, however, fully recognize that the wave theory was so well established by 1874 that his discovery was bound to be treated as what we might now call a

[49]Lloyds, *Report*, p. 300.
[50]Stewart, *op. cit.*, p. 76.
[51]William Crookes, 'On Attraction and Repulsion Resulting from Radiation', *Philosophical Transactions of the Royal Society* **164** (1874), 501 – 527.

Kuhnian anomaly rather than as an out-and-out falsification. Commenting on Bennet's experiment and Young's remarks about it, Crookes wrote:

> Bearing in mind the overwhelming proofs we now possess that the undulatory theory more nearly expresses the truth than does the emissive theory, it is not likely that the very different results I have succeeded in obtaining. . . will have any weight in modifying the accepted theories of light and heat.[52]

The background to Crookes's experiments was, briefly, this.[53] He had arrived at the conjecture that light and radiant heat exert pressure whilst making his chemical experiments — in fact he decided that some of his very delicate weight measurements were being disturbed by the action of light. He went on to attempt some direct experiments on this alleged action, inspired in part by a paper of Fresnel's which he had come across during his research.

Fresnel's paper 'Note sur la répulsion que les corps échauffés exercent les uns sur les autres à des distances sensibles', had been published in the *Annales de chimie et de physique* for May 1825 (and, later in the same year, in slightly revised form in the *Bulletin de la Société philomathique).* It contained reports of experiments which had convinced Fresnel that heated bodies repel nearby objects through a void. Although the heat in these experiments was supplied by focused sunlight, Fresnel seems not to have thought that the sunlight had any direct role in the repulsion. Indeed Fresnel reported that he had earlier tried to detect a light pressure and *failed:*

> To test certain hypotheses, I tried a long time ago *and unsuccessfully* to displace in the void, by the action of the rays of the sun collected at the focus of a lens, a small silvered disc attached to the end of a very light horizontal rod suspended by a silk thread.[54]

(Fresnel gave no clear indication of whether or not this failure to detect a light pressure ran counter to what he would have expected on his general theory of light.) His experiments with heat were successful, however — a fixed body heated by focused sunlight *did* repel his silvered disc mounted in the way described. Fresnel's experiments were performed in the best vacuum he could create — the air pressure being only one or two millimetres of mercury — and this convinced him that the movement could not be attributed to any gas action, particularly since a gradual increase of the air pressure, far from

[52] *Op. cit.*, p. 503.

[53] For more details of Crookes's experiments, his development of the radiometer and of the subsequent debate about its theoretical import, see A. E. Woodruff, 'William Crookes and the Radiometer', *Isis* 57 (1966), 188 – 198; and S. G. Brush and C. W. F. Everitt, 'Maxwell, Osborne Reynolds, and the Radiometer', in R. MacCormach (ed.), *Historical Studies in the Physical Sciences,* I.

[54] Fresnel, *op. cit.*, reprinted in his *Oeuvres complètes,* Vol. 3, p. 668; translation and emphasis mine.

increasing the repulsive effect, reduced it:

> To assure myself that these phenomena were not produced by the small amount of air or vapour remaining under the bell-jar, I allowed the air gradually to re-enter, and, on repeating the experiment when the interior air had become fifteen or twenty times more dense than originally, I found that the repulsion had not sensibly increased in energy, as would have happened if it had been produced by the movement of the heated air; there were even certain positions of the mobile disc relative to the fixed disc [the one that was heated] for which one could not produce movements as great as in the void.[55]

These experiments are important but seem to have created little interest until Crookes repeated and extended them. Crookes, too, brought sources of heat and light close to torsion balances or similar apparatuses and looked for movement. He soon convinced himself that, *contra* Fresnel, light radiation could cause repulsion just as well as radiant heat could. In fact Crookes at first claimed that both forms of radiation could exercise an *attractive* as well as a repulsive effect — depending on the air pressure in the case in which the torsion balance is kept:

> . . .I was enabled to show attraction or repulsion when radiation acted on a mass at one end of a beam, according as the glass tube contained air at the normal pressure, or was perfectly exhausted. At an intermediate internal pressure the action of radiation appeared *nil.*[56]

Crookes entitled his original 1874 paper: 'On Attraction and Repulsion Resulting from Radiation'. This turned out to be the first of a series. In the rest of the papers however the word 'attraction' was dropped from the title. They are all entitled simply 'On Repulsion Resulting from Radiation' — indicating Crookes's increasing concentration on the repulsive effect achieved at the lowest pressures he could produce.[57]

In the course of his investigations, Crookes invented a 'light-mill' or *radiometer* — now to be found in the 'executive toys' department of any large store. This consists of a pivoted rod on the end of which are vanes with alternately silvered and blackened faces — the whole thing being encased in a near-vacuum. As Crookes found, when light is shone on the 'light-mill' the rod revolves — the blackened faces of the vanes receding from the light.

The question, as always, was whether this movement was the direct result of a radiation pressure or was, instead, the effect of heating the gas surrounding

[55]Fresnel, *loc. cit.*, p. 669.

[56]Crookes, 'On Repulsion Resulting from Radiation, Part II', *Philosophical Transactions of the Royal Society* **165** (1875), 519.

[57]These papers are given part numbers — 'Part I' is implicitly the 1874 paper with the slightly different title mentioned above (footnote 51).

the apparatus. Crookes fully realized that even the best vacuum contains residual gas. Nonetheless, he was quite convinced, when starting out on these investigations in 1874, that the observed movements were too large to be accounted for by the action of gas, and hence that they were indeed the direct result of the action of radiation. He admitted that several authors had produced quite plausible-sounding reasons for the involvement of the residual gas. *But* —

> However strong may be the reasons in favour of the air-current explanations, they are, I think, answered irrefragably by the phenomena themselves. If a current of air within 7 millims. of a vacuum cannot move a piece of pith, certainly the residual air in a Sprengel vacuum should not do so. . . . It is, however, abundantly demonstrated that in all cases after [the] critical point is reached, the repulsion by radiation is most apparent, and it increases in energy as the vacuum approaches perfection.[58]

Crookes rested his case, not unreasonably, on the fact, already noted by Fresnel, that the observed movement *diminishes* as the pressure is increased from near zero up to a 'critical point' at which no movement at all is observed. Surely if the effects were due to gas action they would *increase* as the amount of gas increased? As indeed they do beyond that 'critical point', in which region the effects can, Crookes maintained, plausibly be attributed to air currents. And in 1875 he was still convinced that, in the near-vacuum case, he had observed radiation pressure rather than any gas action:

> It was impossible to conceive that in these experiments sufficient condensable gas or vapour was present to produce the effects. . . .[59]

It seemed, then, that Crookes's radiometer had at last definitely shown that light exerts a pressure — to the discomfort of the prevailing wave-theoretical view of light. However, Crookes's interpretation of his results was not unchallenged. The rival view that it was some action of the residual gas which caused the radiometer's movement still had its champions: notably Osborne Reynolds and his younger colleague at Manchester, Arthur Schuster.

Reynolds's theory was not, of course, that the radiometer's movement was caused by convection currents in the gas — otherwise it would have run foul of Fresnel's and Crookes's results. Reynolds's theory was rather that this was a thermal, kinetic effect — due, somehow or other, to the differential action of the gas molecules on the blackened and silvered faces of the vanes. This action

[58]Crookes, 'On Attraction and Repulsion Resulting from Radiation', *Philosophical Transactions of the Royal Society* **165** (1875), 519.

[59]Crookes, 'On Repulsion Resulting from Radiation, Part II', *Philosophical Transactions of the Royal Society* (1875), p. 547.

was noticeable only when a relatively small number of gas molecules was present and evened out as the number of molecules increased. Reynolds's own account of the molecular action was not very satisfactory and indeed the problem of providing an acceptable precise account remained unsolved for some time to come. But Reynolds and Schuster soon hit on a crucial experiment, to decide between the two general accounts of the radiometer's movements, which did not require any detailed theory of the alleged molecular action.

Schuster's own account of the importance and outcome of this experiment was admirably clear:

> Whenever we observe a force tending to drive a body in a certain direction we are sure to find a force equal in amount acting in the opposite direction on the body. . . from which the force emanates. . . .
>
> If the force [causing movement of the radiometer arms] is due directly to radiation, the reaction will be on the radiating body; if, on the other hand, it is due to any interior action, such as the one suggested by Professor Reynolds, the reaction will be on the exhausted vessel enclosing the bodies on which the force acts. I have been able to test this by experiment, and have found that the action and reaction are entirely between the light bodies suspended *in vacuo* [*i.e.* the radiometer arms] and the exhausted vessel.[60]

In fact, Schuster simply suspended a radiometer by a fine thread so that the whole apparatus could rotate fairly freely. If the rotation of the vanes were due to an interaction between the vanes and the gas, then by Newton's Third Law, the gas and hence the radiometer case should be pushed in the opposite direction. If, on the other hand, the effect were due to the light pressure, then, since the reaction would be on the light source *outside* the radiometer, there would be no effect on the gas and, in fact, the slight friction at the support of the axle would lead to a slight tendency of the case to revolve in the same direction as the radiometer arms. Schuster observed that the case revolves in the opposite direction to that of the fly.

The result convinced Crookes (and also, it seems, the rest of the scientific world) that he had been wrong to assume that radiation pressure drives the radiometer. And, indeed, several people now pointed out that, quite aside from any action on the case, the revolution of the radiometer *fly* is opposite to what it should be if driven by light pressure. For, as Crookes had noted, the effect is greater on the *blackened* sides of the vanes. However (assuming, to simplify matters, that light is totally absorbed by the black sides and totally reflected by the silvered sides), a light pulse carrying a momentum p will transfer that momentum to the blackened side, but a similar pulse will, in

[60]Arthur Schuster, 'On the Nature of the Force Producing the Motion of a Body Exposed to the Rays of Heat and Light', *Philosophical Transactions of the Royal Society* **166** (1876), 715.

being reflected, transfer momentum $2p$ to the silvered side. And so the silvered sides ought to be repelled more strongly than the blackened ones, contrary to observation.[61]

The result of Schuster's work was, therefore to discredit the claim that the radiometer is powered by light pressure, and hence once again to throw open the experimental question of whether light exerts a pressure. He had, moreover, shown how difficult it would be unambiguously to detect a light pressure if indeed it existed. For even in quite a high vacuum the gas action was non-negligible. It also seemed that the wave theory could take comfort from Schuster's experiment which had indirectly reinstated Bennet's result of no pressure. However, any such complacent attitude was rapidly being overtaken by changing events on the theoretical front.

(vi) *A complete reversal on the theoretical front*

The wave approach to optics had been more or less dominant since the 1830s — largely due, initially at any rate, to the epoch-making work of Augustin Fresnel. As work on the wave theory progressed, various more specific problems were highlighted and various more specific techniques were developed to deal with them, but at the level of the most basic theory the wave approach consisted in trying to work out the mechanics of the light-carrying medium, the 'luminiferous ether'. This medium had at first been assumed to be an elastic *fluid*. However, as the discovery of the various phenomena of polarized light proved again and again that a light beam could be 'sided' (*i.e.* exhibit different properties in different planes through its direction of propagation), it seemed inevitable that light waves had to be endowed with a *transverse* component. (After all, a longitudinal wave is, by definition, symmetrical about its direction of propagation.) The possibility of a transverse component was raised, in very tentative fashion, by Thomas Young and it was Fresnel who, independently of Young, finally and firmly grasped the nettle — doing away with the longitudinal component altogether and adopting the theory that the disturbance in a light wave is *exclusively* at right angles to the direction of propagation.[62] This new view was indeed prickly. It has the consequence, described by Young himself as 'perfectly appalling', that the ether is a *solid* — for no fluids but only elastic solids can transmit transverse waves. However, the empirical successes of the theories which Fresnel based on this transverse assumption was so impressive that scientists became

[61]Crookes had already noted this as a difficulty when he still believed the radiometer to be driven by radiation pressure and he had given the effect an extremely confused explanation in terms of such a pressure. (See his 'On Repulsion Resulting from Radiation, Parts III and IV', *Philosophical Transactions of the Royal Society* **166** (1876), 350.)

[62]For more details, see Young's *Collected Papers*, I, pp. 333, 383, and Fresnel's *De La Lumière* (1822).

convinced that this view of the ether had to be made sense of, no matter what the difficulties.[63]

The elastic solid theory of the ether held sway from the 1830s to around the 1860s when Maxwell began to develop his theory of electromagnetism. He showed that disturbances in his electromagnetic field were transmitted in the 'free ether' with a velocity equal to that of light, and he hypothesized that visible light is in fact just a small part of the electromagnetic spectrum. Fresnel's optical equations for the transmission, reflection and refraction of light were taken over wholesale into Maxwell's theory — becoming just special cases of Maxwell's equations. Maxwell always sought a reduction of the electromagnetic field to the properties of a mechanical ether but the various attempted reductions met well-known difficulties.

However, quite apart from these attempts at reduction, the aspect of the incorporation of wave optics into electromagnetism which is central from our present point of view was already clear. Maxwell had created a *wave* theory of light which unambiguously predicts that a light beam exerts a pressure. As he himself remarked in his 1873 *Treatise,* it follows from his equations that

> in a medium in which waves are propagated there is a pressure in the direction normal to the waves and numerically equal to the energy in unit volume.[64]

In general the light pressure can be regarded as arising from stresses in the ether created by the passage of light. The Maxwellian account becomes more concrete, and particularly clear, in the case of a light beam falling on a metallic reflecting surface. Here the rapidly-alternating magnetic field associated with the light creates, by induction, electric currents in the surface layers of the metal, but a metal carrying a current in a magnetic field is acted on by a force which is at right angles both to the magnetic field and to the direction of the current; if the light is incident normally, this force, the light pressure, is itself normal to the reflecting surface.

It is not a straightforward problem whether or not this pressure is naturally interpretable as a *momentum* carried by the beam — as an 'electromagnetic momentum'. (On this point see the especially clear treatment in Chapter 1 of Lorentz' *The Theory of Electrons.)* However, in Lorentz' own electron theory it does become natural to regard a light beam as carrying a momentum and, of course, Einstein's relativity theory contains the principle that *all* energy possesses inertia and hence that any energy flux has an associated momentum.

The Maxwell prediction (also, as noted, yielded by relativity theory) that a

[63]For the development of the elastic solid approach in the nineteenth century, see Kenneth Schaffner (ed.), *Nineteenth-Century Aether Theories* (1972).

[64]J. C. Maxwell, *A Treatise on Electricity and Magnetism,* 1st edn, Vol. 2 (1873), p. 391.

flash of light of energy E carries a momentum E/c (where c is, as usual, the velocity of light) was arrived at, around the same time, by an entirely independent line of reasoning. The Italian scientist A. Bartoli argued that it follows from the Second Law of Thermodynamics that, quite generally, any stream of energy in space must carry with it a momentum — the value of this momentum in the case of a light beam being, as before, E/c. (Bartoli's argument is contained in his 1876 book, *Sopri i movementi prodotti della luce e dal calorie.)*

A still more important link between radiation pressure and thermodynamics forged itself in the mind of Einstein. Starting from Boltzmann's *statistical* version of the second law, Einstein inferred that, in the case of black-body radiation in particular, although Maxwell's theory gave the correct *time-average* for the radiation pressure, there had to be *fluctuations* in this pressure for which Maxwell's theory could *not* account. This was no mere incidental result for Einstein but, as Gerald Holton has pointed out, it provided a starting point for, and the common link between, Einstein's three great papers of 1905.

As Holton remarked:

> While the three epochal papers of 1905 — sent to the *Annalen der Physik* at intervals of less than eight weeks — seem to be in entirely different fields, closer study shows that they arose in fact from the same general problem, namely, the fluctuations in the pressure of radiation.[65]

One of these papers, of course, contained Einstein's new photon theory of light: a theory which, in a sense, combines the wave and corpuscular views.

But instead of pursuing these later developments (which raised major problems of their own), let us return to Maxwell's version of the wave theory and its firm prediction of the existence of radiation pressure. This had certainly revolutionized the situation. It meant that Bennet's 1792 experimental result, which had long been regarded as corroborating the wave theory of light, was now turned into an anomaly for that theory. This in turn led to further experimental research which I shall describe below (Section I. viii). However, this feature of Maxwell's work also raises again very sharply the question of what prediction the wave theory 'really' makes about light pressure. Might eighteenth-century corpuscularists and early-nineteenth-century wave-theorists simply have been mistaken in regarding the wave theory as entailing no 'impulse' of light?

[65]Gerald Holton, 'On the Origins of the Special Theory of Relativity', reprinted in his *Thematic Origins of Scientific Thought: Kepler to Einstein,* p. 167. (I should like to thank Professor Holton for drawing my attention to this sequel to my main story.) The common origin of these three Einstein papers and the role of the fluctuations is further investigated by Martin Klein in his 'Einstein's First Paper on Quanta' and (especially) 'Einstein and the Wave-Particle Duality', in *The Natural Philosopher* 2 (1963) and 3 (1964), respectively.

(vii) *The wave theory and light pressure*

According to at least one historian, E. T. Whittaker, these earlier scientists did indeed make a straightforward error. According to Whittaker, Bennet's and Young's assumption, that the experimental absence of light pressure supports the wave theory, was 'remarkable'. Indeed, wrote Whittaker, Young's

> attitude is all the more remarkable because Euler many years before had expressed the opinion that light-pressure might be expected just as reasonably on the undulatory as on the corpuscular hypothesis.[66]

Whittaker thus implies that Bennet and Young (and therefore later scientists like Humphrey Lloyd and Balfour Stewart) were making a logical error in holding that the wave theory predicted no light pressure; an error, moreover, that Euler's optical work ought to have helped them to avoid.

It seems to me, however, that the error is in fact Whittaker's and is the old (and, admittedly, not easily avoidable) error of writing history with the up-to-date textbook too firmly in mind. It is certainly true, as has just been seen, that Maxwell's version of the wave theory straightforwardly entails the existence of light pressure; and it is also true that the development of physics after Maxwell made it increasingly natural to associate inertia, and hence momentum, with radiation energy. Whittaker seems to imply that it ought *always* to have been obvious that light waves carry momentum. But surely, on the contrary, a rather radical shift in our conceptual framework was required in order to accommodate the association of a momentum with any energy flux. (This is explained with great clarity by Lorentz in his *The Theory of Electrons.*[67])

More importantly from the present point of view, the question of whether or not a pressure is developed by a light wave is not one with a univocal answer. The development of wave optics in the nineteenth century did not consist of gradually teasing out the consequences of 'the' wave theory. This theory was, in fact, a series of theories, all of which made certain shared 'core' assumptions, but each of which differed from the others over more specific assumptions. Once this is seen, it becomes obvious that we cannot infer from the undeniable fact that later wave theories entail a light pressure that the earlier wave theories of Young and Fresnel must 'really' likewise have had this consequence. So let us subject Young's and Fresnel's theories to a separate analysis.

Both Young and Fresnel, of course, viewed the ether as a mechanical

[66] E. T. Whittaker, *A History of Theories of the Aether and Electricity: The Classicial Theories*, p. 274.

[67] For an excellent non-technical account of the gradual conceptual shift see also Max Born, *Einstein's Theory of Relativity* (Dover, 1962).

medium. Now, modern analyses of mechanical radiation (such as sound waves in air) *do* in fact associate a pressure with such radiation. However, the existence of this pressure depends essentially on the medium's *not* being perfectly elastic. If, on the contrary, the medium which carries the radiation is perfectly elastic (and if certain other 'natural' assumptions are made, notably that the *mean* density of the medium remains constant whilst the radiation passes through it) then *no* radiation pressure is possible.

Consider the case, for example, of a longitudinal wave in a perfectly elastic fluid. This case is the most revealing since, not only did both Young and Fresnel originally assume that light waves are indeed longitudinal waves in an elastic fluid, but it is also easier to see how a mechanical radiation pressure might arise in the case of a longitudinal or *pressure* wave than it is in the case of a transverse wave. The assumption that the medium is perfectly elastic means that Hooke's law — that the restoring force generated by the disturbance of part of the medium from its equilibrium position is directly proportional to the amount of the disturbance — is *rigorously* obeyed. This means, restricting ourselves to waves in one dimension, that the wave equation

$$\frac{\partial^2 \varrho(x, t)}{\partial t^2} = \frac{v^2 \partial^2 \varrho(x, t)}{\partial x^2}$$

is exactly valid (ϱ is the disturbance, or excess pressure, at the point x and time t, and v is the velocity of propagation). Fresnel, in developing his theory, assumed that the expression for the disturbance at a given spatial point created by a monochromatic light wave was the 'simplest' possible one satisfying this equation, *i.e.* $\varrho_x(t) = A \cos(\omega t - \phi)$, where A is the amplitude of the wave, ω the angular frequency and ϕ the 'phase constant'. (In fact any arbitrary function $f(\omega t - \phi)$ satisfies the above wave equation, and taking this special form involves the — quite natural — assumption that the *mean* density of the medium remains constant despite the passage of the wave.) Presumably what would be measured in an attempt to detect a radiation pressure experimentally would be some time average of this excess pressure.[68] But on Fresnel's assumptions, this time average is clearly zero: $\cos(\omega t - \phi)$ being 'as often' negative as positive. (A full analysis of this, surprisingly tricky, question would take into account the way the pressure is to be detected: absorbing wall, reflecting wall or combination of both. Such an analysis leads, however, to the same conclusion: that for a perfectly elastic fluid, whose mean overall density remains constant, there is *no* radiation pressure.)[69]

[68]Even this is not as straightforward as it might appear. For the general question of mechanical radiation pressure, see R. B. Lindsay, *Mechanical Radiation* (New York: McGraw-Hill, 1960); and, for a particularly clear account, R. T. Beyer, 'Radiation Pressure in a Sound Wave', *American Journal of Physics* **18** (1950), 25.

[69]See Beyer, *op. cit.*

Young here made exactly the same assumptions as Fresnel; hence the fair conclusion seems to be that, given the version of the theory he actually held, Young was quite correct in regarding the absence of light pressure *in vacuo* as a consequence of the wave theory and hence justified in taking Bennet's result as favouring it.[70] As we shall shortly see, it is nowadays accepted that light does exert a pressure which can be experimentally detected. So we might equally well say that had these later experimental results (notably of Lebedew and of Nichols and Hull; see below, and footnotes 80 and 81) been available in the 1800s then this would have been as good a refutation of the wave theory as could be imagined. This was certainly the opinion of J. H. Poynting writing in 1905:

> A hundred years ago, when the corpuscular theory held almost universal sway, it would have been much more easy to account for and explain the pressure of light than it is today, when we are all certain that light is a form of wave-motion.[71]

Moreover,

> had these Eighteenth Century philosophers been able to command the more refined methods of today, and been able to carry out the great experiments of Lebedew and of Nichols and Hull. . . there can be little doubt that Young and Fresnel would have had much greater difficulty in dethroning the corpuscular theory and setting up the wave theory in its place.[72]

While the assumption of the perfect elasticity of the ether continued to be made, the prediction of no light pressure continued to hold. Very often, of course, such assumptions are made as 'first approximations', which are essential to allow mathematical deduction of empirical consequences but which are acknowledged to be, strictly speaking, false, and hence are to be removed in the 'final analysis'. Young's and Fresnel's assumptions should not automatically be held to fit this pattern. No doubt the assumption that any *ordinary* medium is perfectly elastic is bound to be, at best, an approximation; but there was nothing to indicate that this had to be true of the very *extraordinary* ether.

What then of Euler's theory of light? Had not Euler already shown in the mid eighteenth century that pressure was to be expected just as much on the wave theory as on the corpuscular theory? Certainly Euler had stated that

[70]I do not of course assert that either Fresnel or (especially) Young had thought the light pressure question through as fully as in the text. No doubt Young 'sleepwalked' to the correct conclusion about his theory's consequences in this regard.

[71]J. H. Poynting, 'Radiation Pressure', *Philosophical Magazine,* Series 6 (1905), p. 393.

[72]*Ibid.*

> [J]ust as a vehement sound excites not only a vibratory movement of the particles of the air, but one also observes a real movement of small, very light particles of dust, one cannot doubt that the vibratory movement caused by light produces similar effects.[73]

Two claims need to be established, however, if the case of Euler is to show that Young made a logical mistake concerning radiation. One needs to show, first, that Euler had a general theory of light which is unambiguously a wave theory and which unambiguously predicts the existence of pressure; second, that Euler's version of the wave theory is essentially the same as the later 'classical' wave theory of Young and Fresnel. The second claim is definitely false. As just seen, no deduction of light pressure goes through on the basic Young–Fresnel theory. So if indeed Euler's theory predicts pressure, the obvious conclusion to draw is that it differs radically from the classical, mainstream view. The Euler problem arises from again taking an overly monolithic view of wave optics.

The first of the above claims also appears rather doubtful. I am no Euler expert and must leave to others a full assessment of his optical views. But it does seem to me very difficult to extract any sort of coherent general theory of light from the work of the man who is, after all, often called the 'great eclectic'. Euler was especially impressed by the analogy of light with sound. (In the case of sound, the transmitting medium, air, is of course, *not* perfectly elastic and hence a radiation pressure is to be expected.) This wave aspect of his theory is however matched by his explicitly corpuscular–theoretic treatment of refraction. (A treatment which has led D. T. Whiteside to refer to Euler's 'own variant formulation of the emission theory'.[74]) He was on occasion very scathing about the emission theory,[75] but the details of his own basic theory are very sketchy. One of his principal objections to the corpuscular theory was that it has the absurd consequence that the sun, by continually emitting these particles, is gradually diminishing. But if the sun would lose mass on the view he opposed, wouldn't it equally lose energy on the view he proposed? (Not, of course, that Euler would yet have been in a position quite to express it this way.) Not at all: 'the luminous quality alone

[73]Euler, *Histoire de l'Académie de Berlin,* ii (1748), quoted from Whittaker, *op. cit.*, p. 274.

[74]See Whiteside's editorial notes on p. 435 of his *Isaac Newton's Mathematical Papers,* 6.

[75]Euler believed that the emission theory was absurd because (a) it would mean the sun's mass would soon be exhausted, (b) the corpuscles would continually bang into one another and yet light rays are straight lines, (c) ordinary transparent bodies would have to be amazingly porous and (d) the corpuscles would have to enter the eye. He remarked: 'All these difficulties, taken together, will, I doubt not, sufficiently convince you that the system of emanation has in no respect a foundation in nature; and you will certainly be astonished that it could have been conceived by so great a man, and embraced by so many enlightened philosophers. But it is long since Cicero remarked, that nothing so absurd can be imagined as to find no supporter among philosophers. For my part, I am too little a philosopher to adopt the opinion in question.' (*Letters . . . to a German Princess,* p. 70.)

would occasion no expenditure. . . .'[76] It is remarks like this which seem to have led Schagrin to describe Euler as holding a

> 'vibratory-wave theory' in which there is a vibration, a back and forth displacement of the medium, with no net transfer of forward motion.[77]

Whether such a view is one in which a momentum can genuinely be associated with a light beam is not at all clear. Certainly de Mairan, at the time of the publication of Euler's views, challenged the idea that they provided a real basis for any light pressure. According to de Mairan, Euler could allow for a gross body's being carried along backward and forward with the etherial vibrations, but *not* for any *net* force on the body giving it an undirectional motion.[78]

So, of the two claims needing to be established to show that the case of Euler demonstrates that Young made a logical error, the first is definitely false and the second not clearly true.

(viii) *The 'final' experimental detection of light pressure*

Returning now to Maxwell, his revision of the wave theory had completely transformed the already much-reversed situation: instead of that theory predicting no light pressure and being confirmed by Bennet's failure to detect one, the theory now predicted pressure and this negative result became an anomaly for it. The theory, however, was well confirmed in other areas and, moreover, made not only a qualitative prediction about the existence of light pressure but an exact quantitative prediction about its size — the pressure exerted by a beam is, according to Maxwell, numerically equal to the energy in unit volume of the beam. It became, therefore, a challenge to experimentalists to detect this elusive pressure which was 'known' to exist.

This challenge was, of course, taken up. The standard story is that Zöllner scored a near miss,[79] that Lebedew scored the first qualitative success,[80] and that the definitive experiments were performed in the first years of the present century by the Americans Nichols and Hull who not only definitely detected light pressure but whose results were also quantitatively correct to within a few per cent.[81] For example, Max Born wrote in his *Einstein's Theory of Relativity* (revised edition, 1962) that the fact that a short flash of light carries a momentum equal to E/c where E is the energy of the flash and c the velocity of light 'was confirmed experimentally by Lebedew (1890) and again later with

[76]Euler, *op. cit.*, p. 79.
[77]Schagrin, *op. cit.*, p. 929.
[78]de Mairan, *op. cit.*
[79]F. Zöllner, *Poggenodorfs Annalen* **160** (1877).
[80]P. Lebedew, *Astrophysical Journal* **14** (1902).
[81]See, for example, E. F. Nichols and G. F. Hull, 'The Pressure due to Radiation', *Proceedings of the American Academy* **38** (1904), 562.

greater accuracy by Nichols and Hull (1901). . . .'[82] Again, Hecht and Zajac report in their 1974 textbook on *Optics:*

> Even though it is quite small. . . the pressure exerted by light was actually measured as long ago as 1901 by the Russian experimenter Pytor Nikolaievich Lebedev. . . and independently by the Americans Edward Leamington Nichols. . . and Gordon Ferrie Hull. . . .

There is, however, still a final twist to the plot — the validity of Nichols and Hull's results came to be questioned. Several faults — some of them subsequently acknowledged by Hull himself — were found in their results by two London scientists, Mary Bell and S. E. Green.[83] The main experimental problem remained that of gas action: no matter how good the vacuum around the torsion balance whose movement was to betray the radiation pressure, the inevitable heating of the gas always had some effect. Nichols and Hull decided to perform the experiment at a comparatively high air pressure, sixteen millimetres of mercury, at which radiometer action seemed to be a minimum. This, however, means that not only does the movement of the torsion balance rapidly become damped by the air, but also convection currents again come into play. Nichols and Hull attempted to circumvent this latter problem by using a 'semi-ballistic' method. This involved shining light on the pivoted vanes for only brief periods (six seconds) — the reasoning being that, whereas the radiation pressure acts instantaneously, the gas action takes time to develop.[84] This, however, made the experiment still more delicate and its delicacy was enhanced by a complicated apparatus — involving thermocouple systems and galvanometers — aimed at measuring energy density and compensating for the inevitable small changes in the intensity of the beam of light. All of this meant that various detailed calculations were required to transform Nichols and Hull's observed data into results about the radiation pressure *vis-à-vis* the energy density. These calculations were shown by Bell and Green to be faulty in several respects: a wrong value of the mechanical equivalent of heat had been used in the computation of the energy densities (Hull had earlier admitted this himself independently of Bell and Green), logarithms had been taken to the base ten instead of to the base e at one point and at another a mistake had been made over units; also the wrong value for

[82] Born, *op. cit.*, p. 283.

[83] Mary Bell and S. E. Green, 'On Radiometer Action and the Pressure of Radiation', *Proceedings of the Physical Society* **45** (1933), 320. This was followed by a paper 'Notes on the Pressure of Radiation', *Proceedings of the Physical Society* **46** (1934), 589, which contains a section written by G. F. Hull in which he comments on the mistakes Bell and Green had found in his earlier work with Nichols.

[84] 'Radiation pressure, from its nature, must reach its maximum value instantly, while observation has shown that gas action begins at zero and increases with length of exposure. . . .' Nichols and Hull, *op. cit.*, p. 563.

the conversion factor used to transform the 'ballistic throws' of the vane to static conditions had been taken. Once allowance for these errors was made, Nichols and Hull's results, far from agreeing with Maxwell's quantitative prediction within one per cent as they had claimed, showed at least ten per cent divergence between the light pressure and the energy density. Bell and Green concluded:

> It is thus seen that Nichols and Hull's results, when correctly calculated, show the pressure of radiation to be in excess of the energy density by some 10 per cent. Hence they cannot be regarded as furnishing conclusive quantitative evidence of the validity of the relationship of equality deducible from the theory.[85]

Moreover, Bell and Green make it clear that what surprised them about these corrected results is not how far off, but, on the contrary, how *close* to, the 'real' value they were. This is not only because of the small deflections of the torsion balance actually observed, but also of the 'indirectness' of Nichols and Hull's methods — which meant that so many 'corrections' had to be made based on empirical formulae (*e.g.* about the 'sensitivities' of their galvanometers).[86]

I do not believe that any of this should be taken to support a cynical attitude towards such pressure experiments. After all, no-one has seriously claimed that Nichols and Hull's results were qualitatively unreliable. No-one seems to doubt that they (and earlier experimenters like Lebedew) 'observed' radiation pressure; it is just that additional effects were involved in the movements of their balances. Nonetheless, the clear impression which emerges from Bell and Green's papers is that it is the experimenter's skill rather than the underlying theory which is being tested. That there *is* a radiation pressure which is equal to the energy density is taken for granted, the problem is to devise an experiment 'sensitive' enough to show this: and Bell and Green themselves, claimed to have made significant progress towards this aim.[87] The significance of this attitude towards experiment is one of the interesting methodological problems which emerge from this whole historical episode. I now turn to these problems.

II. Some Methodological Morals

One general lesson to be gleaned from this episode is simply that real history of science can be much more intricate (and fascinating) than a naive

[85] Mary Bell and S. E. Green, 'On Radiometer Action and the Pressure of Radiation', *Proceedings of the Physical Society* **45** (1933), 350.

[86] See for example S. E. Green's remarks in G. F. Hull and Mary Bell and S. E. Green, 'Notes on the Pressure of Radiation', *Proceedings of the Physical Society* **46** (1934) (especially p. 596).

[87] See Mary Bell and S. E. Green, *op. cit.*

conjectures-and-refutations view might suggest. Certainly the old view that 'Man proposes, Nature disposes' is very far from an accurate reflection of this particular historical episode, which highlights the fact that it may be clear *neither* quite what man has proposed about some particular aspect of reality (does the wave theory predict light pressure or not?), *nor* what Nature's disposition is in that regard (do the vanes move exclusively because of heat effects or do those effects mask a genuine light pressure?). This is by now, however, rather a well-worn theme and there are more particular lessons to be drawn from the history — both about theories and about experiments.

The episode makes it clear, I think, that single theories are not the best units in terms of which to describe the history of science. There is no such thing as *the* wave theory of light; the mistake of operating as if there were seems to be the source of Whittaker's misplaced remark about Young's reaction to Bennet's experimental result. A certain penumbra of vagueness often surrounds the precise practical predictions of even well-developed theories — this is why some confusion about the existence of a mechanical radiation pressure existed even into the present century[88] — but it certainly seems to be the case that Young's waves of light carry no momentum, whilst later light waves do. It is wrong to think that there is one wave theory which makes one or the other of these mutually incompatible predictions and hence that Young made a logical error in regarding his theory as confirmed by Bennet.

One way of absorbing this lesson is to talk of 'developing' or 'evolving' theories, recognizing that a theory at one stage of its evolution may predict one result, and, at a later stage, a different one. This way of talking may, perhaps, do no harm if not taken too seriously. It is easy, however, to become carried away and feel that a whole new logic is required for these evolving entities.[89] In fact nothing half so radical need be inferred. Surely the clearest method of analysing historical episodes like this one is that already indicated. Rather than one 'evolving' wave theory there was a series of such theories. Each shared certain central components with others in the series (in particular, the 'core' component that light is some sort of disturbance transmitted through some 'luminiferous medium'), but differed from others over 'more specific' assumptions. Since the theories in this series may be inconsistent with one another, there is no mystery over how they can have different, indeed conflicting, observational consequences.

This method of analysis — already implicit in Duhem's methodological writings — is an important component of Lakatos's 'methodology of scientific research programmes'.[90] Lakatos's programmes issue in a series of different

[88]See particularly Beyer, *op. cit.*

[89]This line is, for example, taken rather programmatically in various papers by C. A. Hooker.

[90]See Lakatos, *op. cit.* Although Lakatos himself seems occasionally confused here — given this way of analysing a scientific development, there is, for example, no need to talk of refutations being 'shelved' and, still less, 'ignored'.

theories. Hence we have one wave optics research programme but several different wave theories — a description which, I believe, fits the historical case very well.[91]

What role did the experiments on light pressure play in the appraisal of the scientific merits of this wave optics programme compared with its corpuscular optics rival? In order to answer this question properly, a clear analysis is required of what is to count as an 'observation statement'.

A good deal of the confusion surrounding the current debate on the alleged theory-ladenness and fallibility of all observation statements has arisen from an ambiguity over which statements can count as 'observational'. It is, of course, very easy to forget that theoretical assumptions are being made in 'interpreting' experimental results once these theoretical assumptions have become firmly entrenched and so become 'second nature' to the experimentalist. It would be a *very* pedantic experimentalist who insisted that he was not observing electrons but rather a certain kind of cloud chamber track, a very pedantic astronomer who insisted that he was not observing planetary positions but only the inclinations of his telescopes when certain characteristic spots of light are sighted on their axes. Hence we get 'observation statements' about paths of electrons and paths of planets. But such statements are *clearly* theory-laden and the chance of their being 'corrected' is no mere abstract possibility but one which must be taken seriously. Indeed, actual examples can of course be given.

Newton's theory of gravitation, as is well known, ran into some early difficulties with Flamsteed's 'observational results' about planetary positions. These results were directly inconsistent with Newton's theory of mechanics plus gravitation, together with plausible assumptions about the forces acting on the planets. Newton, holding on to his theory, calmly insisted that Flamsteed's results needed to be 'recalculated'. He performed the recalculation and removed the inconsistency.[92] Newton, it seems, held on to his theory hoping that 'the facts would accommodate themselves to the theory rather than the other way round',[93] and he was vindicated — his theory 'surviving a refutation'. Given this way of analysing the episode we have theories facing fallible *and corrigible* observation statements, and we have the methodological problems of specifying when it is good scientific practice to give up the theory and when to give up the 'facts'.

There is nothing actually incorrect in this method of analysis: indeed, as I admitted above, once observational theories have become entrenched it is only natural to interpret results in their light; moreover, Newton's theory is about

[91]For more details of the wave optics research programme and its rivalry with the corpuscular programme see my contribution to F. Gil and C. Giorello (eds), *Controverse Scientifiche* (Einaudi, 1982).

[92]See p. 431 of *The Mathematical Papers of Isaac Newton,* 6.

[93]J. Agassi, 'Sensationalism', *Mind* 75 (1966), 1 – 24.

the motion of massive bodies under the action of forces, and not about spots of light on the axes of suitably inclined telescopes. Present-day theories of matter are about subatomic particles not tracks in cloud chambers. Nonetheless, this method of analysis obscures an important point and hence has sanctioned much too radical an inference from historical episodes like this. The important point is simply that there is, in such cases, always a level at which the observations are taken as fixed and incorrigible. There is no suggestion that Flamsteed crudely 'misobserved' — on the contrary his *basic* data about 'sightings' of the planets were taken as read. Indeed Newton calculated the 'new' planetary positions by applying a new account of atmospheric refraction (with its consequent 'corrections') to the *old* raw data. As he himself told Flamsteed: 'I have computed [a *Tabula Refractionum*] by applying a certain Theorem to your Observations.'[94] Similarly, in the equally celebrated case of the overthrow of Stas's refutation of Prout's hypothesis, there is no suggestion that Stas's basic gravimetric and volumetric operations were faulty. Again what happened was that it was pointed out that Stas's 'calculation' of the atomic weight of chlorine from his basic data inevitably relied on certain *assumptions* — assumptions which were subsequently challenged.

All this points to the fact that cases of alleged 'data correction' can all be told another way. If, for example, we take the 'observation statements' sanctioned by Flamsteed to be about *sightings* of planets, then nothing in the story suggests that observation statements (properly arrived at, of course, and independently checked) can seriously be challenged. The problem then is that Newton's theory, even in conjunction with an assumption about total forces, has no such observation statements as consequences. The unit from which such consequences are derivable is a much wider one, incorporating not only Newton's theory but also many other assumptions, including optical ones about the transmission of light between the planet and the telescope, say, and instrumental ones about the working of the telescope. On this account there is certainly no need to speak of refutations being 'fallible' or 'overthrown': Flamsteed produced a 'hard' refutation, but not of Newton's theory; what he refuted was only a wider system including Newton's theory. The way that Newton dealt with this refutation was to replace a 'peripheral' assumption about atmospheric refraction, leaving his own 'central' theory intact. This similarity between the pre- and post-refutation system (*i.e.* the fact that they have the same 'central' theory) should not, of course, blind us to the fact that we have, as a result of the refutation, switched to a new theoretical system, quite different from, indeed inconsistent with, the old.

I do not, of course, deny that even at the 'lowest' observational level (meter readings, digital displays and the like) mistakes can be made: trivial errors,

[94] Newton, *op. cit.* (footnote 92).

minor hallucinations caused by overwork, *etc.* The experimenters might even be lying. But surely *such* fallibility can easily be removed (at least in principle[95]) by repeating the experiment or observation and by insisting on *independent* checks. Arguing for the fallibility and corrigibility of low-level observation statements on this sort of ground seems to me analogous to arguing for the fallibility and corrigibility of equation arithmetic on the grounds that everyone can make an error in calculation. There remains the (boring) possibility that we are *systematically* deceived about the real state of affairs even at this low-level stage — perhaps by courtesy of malicious demons, constant mass hallucination or the like. Obviously, these cannot be denied as *possibilities,* and so all observation statements — even the lowest-level ones — must be regarded as *fallible in principle;* but I see no reason to regard them as seriously *corrigible in practice.* Certainly no episode in the history of science of which I am aware indicates that we must take seriously the possibility of a repeated and independently-checked meter reading, say, turning out to be 'false'. Optical illusions, and all the fascinating effects revealed by the psychology of perception, likewise in the end fail, I believe, to have here any relevance. It would take us too far afield to argue this in any detail, so I shall confine myself to two remarks. First, what evidence is there that any optical illusion effects come into play when, say, clear, constant, digital displays are being read in normal, full lighting with no artificial distractions? Secondly, the evidence for the claims of physiology or psychology of perception must itself be in the form of *accepted* (lower-level) observation statements. (How do we 'know' that certain people are colour-blind, for instance? Because they fail certain tests. How do we 'know' that there is a real difference between two areas of a test figure which colour-blind people are failing to spot? Because if we analyse light reflected from the two different areas we can produce two meter readings which everyone, colour-blind or not, will agree are different.)[96]

With Duhem we may agree, then, that the deductive structure of any empirical test of a theory will, if fully articulated, reveal many extra assumptions which need to be invoked, implying that what is really tested is a *group* of assumptions.[97] I claim that this methodological lesson can be absorbed in one of two ways. The extra assumptions (or, at any rate, some of them) can be left implicit — sanctioning assignments of truth values to 'high-level' observation statements. Or they can be incorporated as extra explicit premises in the test, hence creating a theoretical system which is testable at the level of meter readings and the like. I see no argument why, despite their

[95]It may be more difficult in practice — especially if (as in the cases of Homberg, Hartsoeker and Bennet) full details of the experimental arrangement are not given.

[96]See Elie Zahar, 'The Popper – Lakatos Controversy', to be published.

[97]P. Duhem, *The Aim and Structure of Physical Theory,* Part II, Chapter VI.

admitted fallibility in principle, repeated and independently-checked observation statements *of this kind* cannot safely be taken as the empirical bedrock on which science rests.[98] This means that this second way of absorbing the Duhemian lesson is bound to lead to an increase in clarity. For, in any case of a clash between theory and experiment, the scientist can always in this way be represented as facing the same problem — which of the whole group of theoretical assumptions, central, auxiliary or 'instrumental', should be rejected and replaced in view of this clash?

Let us now see how these methodological considerations relate to the particular historical episode under review. Certainly if we are liberal with observational status and allow statements about the existence or non-existence of light pressure to count as observational, then this episode is bound to appear as a paradigm case for the necessity of treating observation statements as revisable. We have, then, the opportunity of a direct crucial experiment between the rival wave and corpuscular theories since they make conflicting predictions about this 'observable' fact. But unfortunately the experimental verdict kept on being reversed. Refuted in 1708, the wave theory 'staged a comeback' in 1730, only to be refuted again by 1770. In 1792 Bennet vindicated the wave theory by showing experimentally that there is no light pressure. However, we now know that Bennet got the experimental result wrong. If only the 'precise' experimental techniques of Nichols and Hull had been available at the beginning of the nineteenth century then the wave theory of light would have begun its career of acceptance unambiguously empirically refuted. This might lead some people to argue that this is a case in which too much experimental precision might have had deleterious effects. But then we also know, with hindsight, that the 'fully-developed' wave theory really predicts the right result.

This makes an exciting and dramatic story. But it also, I claim, makes the whole episode much more mysterious than it need be; and it leaves out several important factors. It seems reasonable to insist that, although these experiments were indeed aimed at deciding whether or not light exerts a pressure, and although they were generally interpreted either as revealing such a pressure or revealing that no such pressure exists, what was *in fact* observed in these experiments was whether or not certain highly mobile objects were noticeably and uniformly displaced. Provided we specify what, in the particular circumstances, counts as a noticeable movement, then surely we need not take seriously the corrigibility of any unambiguous experimental result, when taken at this level. (There may be borderline cases but in those we should simply say that the experiment yielded no unambiguous result.)

[98] I argue this thesis in more detail and against the opposing view of Paul Feyerabend in 'Against Too Much Method', *Erkenntnis* **13**, and in 'Facts and Feyerabend', in H. P. Duerr (ed.), *Versuchungen: Aufsätze zur Philosophie Paul Feyerabends* (Suhrkamp, 1981).

Hartsoeker and Homberg may have been mistaken when they claimed that they saw a small spring move when 'struck' at its unattached end by a beam of light — they may even have been lying. But any such mistakes or lies can be checked simply by repeating the experiment. We need not question that Michell observed movement of his pivoted apparatus — we would simply confidently claim that any real effects were due to convection currents, not to light pressure. Similarly we need not doubt that Bennet saw no movement of his torsion balance; we would simply be confident that his apparatus was not 'sensitive' enough to detect the pressure which was really operating. There is no doubt that Crookes observed movement of his light-mill, the question is only whether or not this undoubted movement can correctly be attributed to radiation pressure. Finally, and perhaps most convincingly, although Mary Bell and S. E. Green in a sense certainly challenged the results of Nichols and Hull, they were quite explicit about there being no suggestion that Nichols and Hull simply mistook the measured movements of their balance or that they misread their galvanometers and the like.[99] The mistakes which Green and Bell point to are all concerned with the *'calculation'* of the values for radiation pressure and energy density — which calculations involve various auxiliary or observational theories and assumptions (as well as, of course, some mathematics). Indeed Bell and Green take Nichols and Hull's *basic data* (measured deflections, galvanometer readings and the like) for granted and see how well they agree with the theoretical predictions when 'correctly calculated'[100] or submitted to a 'correct evaluation'.[101]

The problem with observed deflections of torsion balances and the like is not that they are seriously corrigible but that no predictions about them follow either from the corpuscular theory or from the wave theory of light. Obviously the corpuscular theory, for example, does *not* entail that if a light beam is trained on a screen attached to a pivoted needle then the needle will move away from the light source. It does entail that the needle will be subject to a pressure in that direction but whether this is translated into an actual movement will, of course, depend on the size of this pressure compared with the friction at the pivot and any contrary pressures. Similarly Young's wave theory cannot be directly refuted by an observation that such a needle moves when light shines on it. It does entail, as we saw *above,* that no pressure is exerted on the needle by the light, and hence that the needle will not move because of such a pressure — but it may, of course, move because of other factors.

This does not mean that these experiments were irrelevant for Young's wave theory. Suppose that Michell's result — that his pivoted apparatus moved in a certain way in certain circumstances — were repeated, checked and found to

[99]Bell and Green and Hull, *op. cit.*
[100]*Ibid.*, p. 596.
[101]*Ibid.*

be correct. Then this result could surely safely be taken as conclusively refuting the conjuction of Young's wave theory and the assumption that no significant thermal or other effects are operative. One cannot hold on to *that* conjunction hoping that 'the facts will accommodate themselves to theory'. Something has to give: 'anything' does not 'go'. But given that he has been presented with no independent reason for accepting the auxiliary, the defender of the wave theory will not think twice about blaming that auxiliary for the refutation rather than his own 'central' theory. Indeed this gives him a chance for a great success: he can conjecture that it is convection currents in the air caused by the heat associated with the light beam which account for the movement; and hence can predict that as the air pressure surrounding the pivoted apparatus is reduced, so is the movement when light is shone on it. This prediction can be regarded as confirmed by Bennet's experiment.

The boot is now firmly on the other foot. The corpuscular theory is certainly not directly refuted by Bennet's experiment, but no defender of that theory can regard the result as irrelevant. After all, the way forward for the corpuscular theory was bound to involve specifying — at least roughly, and preferably precisely — the *mass* of the light particles and the *number* of particles in unit volume of a beam of given intensity. Given the sensitivity of Bennet's apparatus (which could be independently measured by subjecting it to a 'known' mechanical force) and given the natural assumption that there is no contrary pressure on the vane which just happens to arise when light is shone on it, then the result of Bennet's experiment does directly refute a whole range of possible theoretical systems built around the corpuscular theory which specify particular values for the mass and number of the light particles.

Indeed this analysis exposes a major flaw in the whole corpuscular programme: despite many attempts, no-one really succeeded in obtaining anything other than the most qualitative estimates of the mass of the light particles and of their number in relation to observed intensities. The free movement of the planets through the solar system pervaded by the sun's light indicated that both values were extremely small; and the small density per unit volume of beam was confirmed by the fact that one light beam could be made to cross another without any noticeable scattering.[102] But no-one could successfully go further than this; which meant that corpuscularians were embarrassed by too many free parameters: not only were the forces emanating from 'gross' matter unspecified (though Malus made a strong attempt at specification), but so also were the light particles' mass and number (the lack of success in tying down these parameters is one indication of the degeneration of the programme).

[102]On this point see, for example, J. F. W. Herschel, 'Treatise on Light', in the *Encyclopaedia Metropolitana* (1827). (According to Priestley — *op. cit.* — the first scientist to explain how light beams could cross without interference by assuming enormous distances between light particles was Canton in *The Philosophical Transactions of The Royal Society* **58**.)

This analysis also explains the attitudes of some corpuscularians (notably the most sophisticated of them, de Mairan, though also Horsley and Priestley) to these pressure experiments. They tended to regard them *not* as testing their theory but as supplying upper bounds for the values of certain parameters in their theory. The only acceptable theory available was the corpuscular theory; its prediction of the existence of light pressure had to be correct; the role of experiment was to measure it, at least roughly. If, as in de Mairan's experiment, a particularly mobile mill is unmoved by an intense beam of light then this simply places a still lower upper bound on the mass of a light particle (already known to be small because of its causing no distress to 'delicate flowers', and the like).

Experiment is here being used as an instrument for developing a theory which has already been accepted for other reasons. This is surely a reasonable procedure and part of what Kuhn characterized as 'normal science'. We simply have an already accepted theory which in order to be fully testable by a certain experiment requires an auxiliary of a known kind. The problem is that we have no reason to specify one *particular* auxiliary rather than others. Experiment can help to supply such a reason and hence can result, it is to be hoped, in a more contentful theory that can be genuinely tested elsewhere.

So long as no claim is made that the experimental result actually *supports* the theory together with the new auxiliary, so long, that is, as experimental support for the system is sought elsewhere, then this procedure is surely quite legitimate methodologically. Kuhn is right in stressing the importance and prevalence of this sort of experiment — not to be dismissed, as some would like to do, as 'hack' science. Even the best scientific theories use *some* experiments in order to 'fill in' and develop, rather than to test, the theoretical systems built around themselves. (For example, there were no means of arriving at values of the wavelengths of light of different colours in the classical wave theory except by 'reading off' these values from some experimental result.) The problem with the corpuscular programme was not, then, its use of experimental results to estimate parameters, but rather the fact that no theoretical system arrived at in this way went on to make new, testable predictions about different phenomena. (This is, of course, in complete contrast to the nineteenth-century wave theory. There, once wavelengths have been 'read off' from one experiment — say from distances between two of 'Newton's rings' — the theory produces different, testable and successful predictions using those values.)

It seems that these eighteenth-century pressure experiments did not really test the prevailing corpuscular theory but rather provided it with opportunities for expansion which it was unable to take — not surprisingly, it might be thought, since the theory is false. But surely the *later* pressure experiments do test and indeed support the opposing wave-theoretical view — a view which we anyway know to be superior for all sorts of other reasons?

Certainly there is here no question of auxiliary assumptions *from within the central theory itself* requiring 'filling-in' or 'sharpening-up'. Maxwell's theory, pre- or post- the Einsteinian revolution, makes a completely clear prediction about light pressure: that it exists and that the amount of momentum carried by a pulse of energy E is E/c. Experimental tests of this prediction are, however, still by no means straightforward. The problem now is mainly with what might be called 'external auxiliaries'. What action does the residual gas have? How constant is the intensity of light in the particular experiment? How accurate are the measurements of the energy flux? How 'sensitive' is the apparatus measuring the pressure?

There is enormous confidence within the scientific community concerning the theoretical prediction — more confidence, indeed, than in any set of auxiliary assumptions made in any particular experiment. No one seriously doubts that light pressure exists and the job of experimentalists seems to be *not*, in fact, to test the theoretical prediction but rather to *reveal* this pressure. This is particularly clear from the papers by Mary Bell and S. E. Green, referred to above. Bell and Green take it for granted that light pressure is a 'phenomenon'.[103] The faults in Nichols and Hull's experiments are *never for a moment* taken to mean that the Maxwell/Einstein prediction is less than fully secure; these faults simply mean that the extent of the agreement between theory and experiment which they revealed, and which had been regarded as one or two per cent, could only in fact be regarded as nine to eleven per cent, at best. The aim of later experimenters was to perform 'more precise' experiments which agree more closely with theory — an aim in which Bell and Green felt they had succeeded:

> Our work has shown that the high-vacuum method with the use of metal is capable of disentangling radiation pressure from gas action with much greater certainty than any other method. With the advantages enumerated we naturally were able to obtain somewhat better agreement between pressure and energy values, than could have been obtained from the unavoidably circuitous methods adopted by Nichols and Hull.[104]

All this is, again, very much in accordance with Kuhn's conception of normal science and with his remarks about it often being the experimenters' skill rather than the theoretical paradigm which is regarded as tested in a particular experiment. It would be easy to draw cynical, relativistic conclusions: theory seems to have created for itself a position of 'heads I win, tails you lose' — if the experiment agrees with theory, the theory is right, but if experiment disagrees the theory is not wrong. Is this procedure, however, quite

[103]Bell and Green and Hull, *op. cit.*, p. 589.
[104]*Ibid.*, p. 603.

as circular as it might appear?

The first, and perhaps most important, remark is that the confidence in the theory underwriting the pressure prediction is no mere sociological phenomenon: its results from confirmation of the theory in severe tests where the necessary auxiliaries are more clear-cut and themselves better tested than in the pressure case. The theory has, as it were, earned itself the right to favourable treatment in less clear-cut cases. Moreover, this favourable treatment by no means amounts to being given *carte blanche*. The auxiliary assumptions about gas action and the rest are not separately testable independently of *any other* theoretical assumptions (here Duhem was right), but they *are* testable independently of the main theory under test (here Duhem was, I think, wrong).[105] If one were allowed to make just any assumption about gas action, the energy flux and so on, then the procedure I have highlighted would indeed be circular. But claims about the gas action, although intimately linked to claims about radiation pressure, can be separately tested (using other assumptions from the kinetic theory); testing claims about the energy flux and its measurement do indeed require other assumptions (theory of the thermocouple, or whatever), but these assumptions do not include any about radiation pressure. Of course, all these other theories and assumptions *might* be challenged — but a successful scientific challenge requires a new theory which explains its predecessor's success and makes new and successful experimental predictions. This is no easy task. The fact is that, given the best conjectures we can make about the auxiliary and experimental phenomena together with the theoretical prediction about radiation pressure, we obtain results which are not far from those which are experimentally observed. Since, if the predictions were wildly off the mark, this whole set of assumptions would come to be in serious doubt, it seems reasonable to count the fact that they are not so far off the mark as being in their favour.

These later pressure experiments are not, then, to be dismissed as unimportant for the appraisal of the merits of our present theories of light — they do provide them with some support. Nonetheless the relationship between theory and experiment has turned out to be much more complicated and subtle than might have been expected. Certainly the full analysis of these experiments has taken us a long way from the initial, naive idea that they could operate as 'crucial experiments', straightforwardly supplying a criterion of 'truth' or of 'falsity' for basic theories of the constitution of light.

[105]To clarify a little; I am sure that Duhem was wrong *if* he held this position, but, while some passages in his book suggest he did hold it, others propound a weaker and less exceptionable view.