Thomas Young and the 'refutation' of Newtonian optics: a case-study in the interaction of philosophy of science and history of science*

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Introduction

A great scientific revolution occurred in optics in the early part of the nineteenth century when the Newtonian corpuscular theory of light was rejected in favour of the wave theory. The majority of scientists became convinced of the superiority of the wave theory in the late 1820s to early 1830s, yet many historians and commentators have claimed that the superiority of the wave theory had already been established by the work of Thomas Young published in the first years of the 1800s. Hence there arises a famous and much discussed historical problem – why was Young's work ignored for so long?

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My paper consists of five parts. In the *first* part, I discuss briefly the reasons usually given for regarding Young as having established the wave theory's superiority. In the *second*, I argue that the usual explanations given by historians and commentators on science of why Young's work was ignored for a time are false. In the *third* (and by far the longest) part, I produce some arguments which challenge the fundamental assumption that Young did establish the superiority of the wave theory. In the *fourth* part, I consider how the (descriptive) historical problems are shifted by my (normative) methodological reappraisal of the merits of Young's theory and of the import of his experimental results. In the *fifth* part, I try to draw some of the general morals about the interaction of history of science and philosophy of science which seem to be illustrated by my case study.

1 *Young's alleged achievement*

The most widespread (and least sophisticated) account of Young's achievement is that his work decided the issue between the corpuscular and wave theories of light. By a series of experiments, of which perhaps the most conclusive was the two-slit experiment, Young, according to this account, established that beams of light are capable of interfering and, in particular, of interfering destructively. In other words, Young experimentally established the remarkable fact that light added to light may produce darkness. This possibility, as Young himself had pointed out, is a consequence of the wave theory of light: if at a given point the trough of one wave meets the crest of another equal and parallel wave, the net effect on the medium at that point will be zero. (Indeed, this is the central consequence of Young's famous 'principle of interference'.) Destructive interference is, however, not a possibility according to the Newtonian corpuscular theory – for how can the effect of two parallel streams of particles be less than the effect of either stream separately? Thus Young's experiments were crucial in deciding between the two rival theories of light.

This account is often given. R. W. Wood, for example, in his famous optics textbook reports that 'true interference was first observed by Dr. Young...whose justly celebrated experiments established almost beyond question the validity of the wave theory'.[1] Magie writes: 'Young's great importance for physics rests upon his discovery of the interference of light by which he established the undulatory theory.'[2] The story of the crucial nature of Young's experiments goes back to the mid-nineteenth century. Young's first biographer, George Peacock, Dean of Ely and a celebrated Cambridge mathematician, writes of one of Young's experiments: 'This was a *crucial* experiment and may be considered as having constituted an important epoch in the history of the undulatory theory.'[3]

[1] Wood [1905], p. 2. Similar statements are made in almost all optics textbooks.
[2] Magie [1935], p. 59.
[3] Peacock [1855], p. 162.

And Peacock's friend Whewell suggests that 'the man whose name must occupy the most distinguished place in the history of Physical Optics, in consequence of what he did in reviving and establishing the undulatory theory, is Dr. Thomas Young'.[4]

Now even if we weaken the claim that Young established the truth of the wave theory to the less obviously false claim that Young at least established the superiority of the wave theory by refuting its corpuscularist rival, it is strange that, both in Britain and in France, most physicists interested in optics adhered to the emission theory well into the 1820s. If Young's experimental results (all of which had been published by 1807) were crucial, why this irrational delay?[5]

2 *Young's work allegedly ignored: the 'Newton-worship', 'poor presentation' and 'character assassination' explanations of the 'neglect' of Young's work*

One natural answer would be that Young's work was not known to his scientific contemporaries. This answer is, however, far from being true. His three major papers on the wave theory all appeared not in some obscure and unread journal but in the *Philosophical Transactions* – two of these papers had originally been delivered as Bakerian lectures at the invitation of the Council of the Royal Society. The fact that both Young and his work were known to major scientists of the time is well documented. He corresponded with Brewster, Herschel, and Wollaston[6] in England. In France, Laplace knew of, and refers to, Young's work; as did Biot and Arago. Indeed, Fresnel in his work consistently refers to 'the celebrated Dr. Young'.[7]

But if the corpuscular theory was not only refuted, but even *known* to be refuted (since scientists knew of Young's work), why did scientists continue to accept it?

Whewell in his *History of the Inductive Sciences* provided not just one answer to this question but three.[8] These answers were repeated and

[4] Whewell [1837], p. 21.

[5] Whewell puts the problem in almost as many words when he writes that Young's experiments, together with the theoretical results in Young's [1802*a*], 'certainly *ought* to have convinced all scientific men of the truth of the doctrine thus urged [i.e. the wave theory]'. (Whewell [1837], p. 324; my italics.) Cf. Jenkins and White [1957], p. 203: '[Young's] early work proved the wave nature of light but was not taken seriously by others until it was corroborated by Fresnel.'

[6] Wollaston actually wrote to Young: 'I like your Bakerian very much, but I cannot say that I have yet inserted the undulatory doctrine into my creed and it may be some time before I repeat it with fluency.' See Young [1855], p. 261.

[7] See, for example, Laplace [1813], volume 4 of Biot's enormous [1816], and Fresnel [1822], p. 1.

[8] Two of Whewell's three explanations (those which I call below the 'obscure style' and the 'character assassination' explanations) are to be found still earlier in Arago's 1832 '*Éloge*' on Young (see his [1859]). The actual source of the 'Newton-worship' and 'character assassination' explanations seems to have been Young's own ([1804*b*]) bitter reply to Brougham's reviews of his papers.

elaborated by Peacock (who admits that he had 'nothing to correct and very little to add' to Whewell's 'judicious treatment' of Young[9]) and eventually became almost as much a part of the scientific folklore as the crucial nature of Young's experiments. I shall call the three answers, in turn, the '*Newton-worship*' answer, the '*poor presentation*' answer, and the '*character assassination*' answer.

(i) According to Whewell's first explanation Young's theory was ignored because of the general hero-worship of Newton in the early nineteenth century. Whewell writes:

When Young, in 1800, published his assertion of the Principle of Interference, as the true theory of optical phenomena, the condition of England was not very favourable to a fair appreciation of the value of the new opinion. The men of science were strongly preoccupied in favour of the doctrine of emission, not only from a national interest in Newton's glory, and a natural reverence for his authority, but also from a deference towards the geometers of France, who were looked upon as our masters in the application of mathematics to physics, and who were understood to be Newtonians in this as in other subjects.[10]

Peacock copies Whewell:

The reverence...attached in this country to whatever was sanctioned by the authority of Newton, operated not a little to retard...the acceptance of any conclusion or theory which he had repudiated [as he had, of course, the wave theory of light[11]].[12]

The Newton-worship claim is repeated by recent historians. I. B. Cohen for example, in his preface to the Dover edition of Newton's *Opticks*, writes (p. xiii):

Had not Fresnel and Arago in France become interested in the work of Young, it seems probable that the great name of Newton would effectively have blocked any pursuit of Young's ideas...and any further development of the wave theory of light.

(ii) Whewell adds a mitigating circumstance which, in turn, becomes his second explanation for the lack of recognition of Young's achievements: namely that Young presented his theoretical and empirical results poorly so that their content and importance was obscured.

Whewell writes:

We may add, however, that Young's mode of presenting his opinions was [because of his 'style of writing'] not the most likely to win them favour.[13]

[9] Peacock [1855], p. 138.
[10] Whewell [1837], p. 346.
[11] Cf. below, p. 131.
[12] Peacock [1855], p. 182.
[13] Whewell [1837], p. 148. In 1832 Arago had written 'I should be wanting in frankness, I should be the panegyrist not the historian, if I did not avow, that in general Young did not sufficiently accommodate himself to the capacity of his readers; that the greater part of the writings for which the sciences are indebted to him, are justly chargeable with a certain obscurity.' (Quoted from the reprint in Arago [1859], p. 512.)

Peacock copies Whewell:

It is but an act of justice, however, both to those who neglected as well as to those who opposed the conclusions of these Memoirs[14] to admit that there is much in the form which they assumed which made it very difficult to appreciate their value. Like all Young's early scientific writings they were extremely obscure.[15]

So scientists from 1802 to the 1820s refused to allow that Newton might have been wrong. And they were unable to comprehend Young's papers. But, as if this were not enough, they were also, as we shall now see, taken in by a nasty character assassination.

(iii) Young's 1801 Bakerian lecture and his 1802 paper[16] were attacked by an anonymous reviewer in the *Edinburgh Review* of January 1803. The author was known to be Henry Brougham – later to be Lord Chancellor. Brougham also attacked Young's [1804*a*] paper, which had again been delivered as a Bakerian lecture (in 1803). This second attack appeared in the October 1804 issue of the *Edinburgh Review*. Brougham's comments cannot be construed as favourable. Brougham writes, for example, that Young's first lecture 'contains nothing which deserves the name either of experiment or of discovery and...is, in fact, destitute of every species of merit'.[17] Brougham finds that Young shifts his ground so often as to force him to ask if 'the world of science is to be as changeable in its modes, as the world of taste, which is directed by the nod of a silly woman, or a pampered fop'.[18] Brougham finds Young's law of interference 'one of the most incomprehensible suppositions we remember to have met with in the history of human hypotheses',[19] and he admits his final feelings to be ones of 'regret at the abuse of that time and opportunity which no greater share of talents than Dr Young's are sufficient to render fruitful, by mere diligence and moderation'.[20]

According to Whewell, these two reviews amount to an assassination of Young's scientific character and they retarded the growth of knowledge: 'We can hardly doubt that these Edinburgh reviews had their effect in confirming the general disposition to reject the undulatory theory.'

[14] These are Young's [1802*a*], [1802*b*] and [1804*a*].

[15] Peacock [1855], p. 185. Peacock obviously added the qualification 'early' (in 'Young's early scientific writings') so as not to have a problem of why people eventually came to understand Young. That Young's style became clearer as he got older is an interesting hypothesis. If true it would make Young a notable exception to the general rule. Unfortunately I can find no evidential basis for the hypothesis in Young's writings.

Pettigrew in his [1840], p. 23, also favours the 'poor presentation' hypothesis: 'Perhaps one reason for this apparent neglect is to be found in the object and style of his writings. In his mathematical...researches, he has presumed upon his readers being more acquainted with science than is really the case...' Crowther in his [1968] makes the poor presentation of his results the central reason for Young's neglect.

[16] Published as Young's [1802*a*] and [1802*b*].

[17] Brougham [1803], p. 450.

[18] *Ibid.*, p. 452.

[19] Brougham [1804], p. 97.

[20] *Ibid.*, p. 103.

Peacock elevates Brougham's reviews into the major cause of Young's neglect. He writes rather melodramatically:

The effect which these powerful and repeated attacks produced upon the estimate of Dr. Young's scientific character was remarkable. The poison sank deep into the public mind, and found no antidote in reclamations of other journals of co-ordinate influence and authority. We consequently [*sic*] find that the subject of Dr. Young's researches remained absolutely unnoticed by men of science for many years.[21]

Young's later biographer Wood copies Peacock – according to Wood, Brougham's 'bitter and virulent attacks on Young's work certainly did much to delay the acceptance of the wave theory.'[22]

Ernst Mach joins in:

In spite of his great work, Young was not to rejoice in the fruits of his labours immediately, for a scientific reactionary who felt himself called to uphold Newton's theory even to an iota, commenced an unparalleled attack upon him...Young's reputation was actually marred by it for many years...[23]

According to Whittaker, although Young's papers showed 'the superior power of the wave theory' their publication 'occasioned a fierce attack... from the pen of Henry Brougham...[T]here can be no doubt that Brougham for the time being achieved his object of discrediting the wave theory.'[24]

Let us examine each of these three explanations.

The claim that there existed in the late eighteenth and early nineteenth centuries a Newton-worshipping establishment ready to pounce on any unfortunate who was foolhardy enough to suggest the falsity of some theory proposed by Newton seems to be refuted by the historical facts.[25] Several scientists were able to state quite openly that a Newtonian theory is false without either forfeiting their good name, or preventing serious consideration of their subsequent work. Let me list a few of these scientists.

[21] Peacock [1855], p. 173. Peacock writes elsewhere (footnote on p. 192 of Young [1855]) that Brougham's reviews 'Not only seriously damaged, for a time, the estimation of the scientific character of Dr. Young, but diverted public examination of the truth of his theories...for nearly twenty years'.

[22] Wood [1954], p. 157. Young produced and had published a reply to Brougham's attacks (reprinted in Young [1855], pp. 192–215). It is alleged that only one single copy of this reply was sold. If so it was a well-thumbed copy, for there is documentary evidence that several people read it.

[23] Mach [1926], p. 156.

[24] Whittaker [1910], p. 108.

[25] Indeed, almost the only evidence in favour of the claim is to be found in Young's own [1802*a*], where he tries to defend his own theory by showing it to be not so very different from Newton's. Brougham did not let Young get away with this exercise in public relations (which, if the rest of my evidence is anything to go by, would anyway have had little effect on his fellow scientists). Brougham writes (it is an aspect of his attack which historians have tended to overlook): 'We hold the highest authority to be of no weight whatever in the court of Reason; and we view the attempt to shelter this puny theory under the sanction of great names, as a desperate effort in its defence, and a most unwarrantable appeal to popular prejudice.' (Brougham [1803], p. 454.)

Priestley wrote in 1772 of Newton's researches into the colours of thin plates that 'in no subject to which he gave his attention does [Newton] seem to have overlooked more important circumstances in the appearances he observed, or to have been more mistaken with respect to their causes.'[26]

G. W. Jordan produced, in 1799 and 1800, two tracts[27] on the 'inflection' of light, in which he is especially critical of Newton's hypotheses of 'fits of easy reflection and transmission', which, Jordan claims, 'are inconsistent with the actual condition of things, and the general phenomena of light and of nature...they obstruct all discovery concerning them; they interrupt the general progress of philosophy'.[28] Far from being dismissed as obvious rubbish since inconsistent with Newtonian doctrine, this work seems to have been given serious attention, and was certainly favourably received at least by the reviewer for the *Philosophical Magazine*. This reviewer records that Jordan

> has carefully repeated those experiments by which Sir Isaac Newton effected his analysis of light. The experiments have produced to his observation, phenomena materially different from those which appeared to Newton...this author infers from his observations...that...the Newtonian theory of light and colours is not fundamentally true.[29]

And the reviewer gives the following scrupulously fair and balanced summary:

> The apparent accuracy of these observations; the logical fairness of the induction; the literary composition of the essay, deserve every praise. Without having ourselves repeated the experiments, and without knowing them to have been repeated, with similar results, by others, we would not presume to decide concerning the truth of the doctrine.[30]

No sign here of any irrational desire to preserve Newton at all costs against the charge of having uttered a falsehood. (And this was written in 1800, only two years before Young's first major paper.)

In 1802 itself, Wollaston published the results of some experiments in crystal optics.[31] These results are stated to be inconsistent with Newton's law of the position of the extraordinary ray in extraordinary refraction; they are explicitly stated to confirm the law of Huygens. Wollaston maintained a high reputation amongst his fellow scientists (even to the extent of being nicknamed 'The Pope' because of his alleged infallibility) and his results (independently arrived at by Malus) were accepted as correct, were frequently quoted, and formed the basis of Laplace's theoretical work on the subject.

Young's senior contemporary William Herschel, in a series of papers on

[26] Priestley [1772], p. 498. This book, by the way, has a chapter entitled 'The Opposition which Newton's Doctrine of Light Met With'.

[27] Jordan [1799*b*] and [1800].

[28] Jordan [1799*b*], p. 126.

[29] *Philosophical Magazine*, **7**, 1800, p. 365.

[30] *Loc. cit.*, p. 366.

[31] Wollaston [1802].

'Newton's rings',[32] was able, without incurring the wrath of his contemporaries, to state explicitly that Newton's theory of fits 'cannot account for the phenomena'. Whilst even Brougham, Mach's 'scientific reactionary who felt himself called to uphold Newton's theory even to an iota', developed a theory of light which, while based on corpuscles, was openly different from Newton's theory. Newton regarded his theory of fits as experimentally proven. Brougham calls it, in print, a 'degrading' speculation, which hides the simple facts behind 'the smoke of unintelligible theory'.[33]

Moreover, as later commentators demonstrated, it is not too difficult to reconcile the view that the corpuscular theory is false with the views that it is a scientific sin to espouse a theory which turns out to be false, but that Newton was free from (scientific) sin. This is because it is not difficult to defend Newton against the 'charge' of having espoused the corpuscular theory. Although it is clear, I think, that Newton basically was a corpuscularist (he saw the problems in optics that a corpuscularist would see and tried to solve them in a corpuscularist way[34]) he did not explicitly adopt the corpuscular hypothesis. Indeed there are passages in his writings in which he is explicitly non-committal;[35] moreover, Newton endowed rays of light with periodic properties ('fits'), properties which he was inclined to attribute to periodic disturbances in the ether overtaking the rays of light.[36] Anyone wanting to espouse the wave theory could easily have argued that Newton was not *really* a proponent of the opposing theory. Indeed, this became standard practice amongst commentators once the wave theory had been accepted. One example of this occurs in the translators' footnotes to Arago's '*Éloge*' on Young: 'Upon the whole it appears that the name of Newton can in no way be legitimately claimed as a partisan of either theory.'[37]

Apart from all this, one wonders why, if criticism of Newton was not allowed during the period from 1800 to 1820, the Newton-worshipping scientific establishment crumbled so quickly afterwards. For by about 1830 all major scientists in Britain and France accepted the wave theory as superior to its corpuscular rival.[38]

[32] William Herschel [1807], [1809*a*], [1809*b*].

[33] Brougham [1796], p. 272.

[34] See below, p. 159.

[35] For example, [1672], p. 5086 (reprinted in Cohen [1958], p. 116): 'Tis true that from 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, nor so much as any part of it.' See also [1675], p. 249 (reprinted in Cohen [1958], p. 179).

[36] See, e.g., Newton [1730], Query 17, and [1675], p. 251 (reprinted in Cohen [1958], p. 181).

[37] Arago [1859], p. 515.

[38] This does not mean that all major scientists in 1830 *believed* the wave theory. Many, in part put off by the all-pervading but invisible ether required by the wave theory, felt that the wave theory could not constitute the truth about the universe. Biot, Brewster, Herschel, Potter and even Arago seem to have put themselves into this category with

Next, the 'poor presentation' explanation of the neglect of Young's theory.

No doubt some parts of Young's work are rather obscure. This is particularly true of the mathematical sections. This is seized upon by Whewell, Peacock and some other commentators. Whewell for example writes:

> [Young's] mathematical reasonings placed them out of reach of popular readers, while the want of symmetry and system in his symbolic calculations, deprived them of attractiveness for the mathematician.[39]

This will, however, hardly do as an explanation of the neglect of Young's theory of light, for of the three early papers which are alleged to constitute Young's major contribution to the wave theory (and which amount to some 50 pages of the *Philosophical Transactions*), precisely *one paragraph*[40] contains any 'symbolical calculations'. The vocabulary of the rest is restricted to that of ordinary English, plus the natural, rational and real numbers, the equality sign and a few variables which are always given an explicit physical interpretation.

In fact, those aspects of his work which are generally alleged to constitute his primary achievement and to have established the wave theory's superiority – namely the qualitative aspects of his 'crucial' experimental results and his principle of interference – are on the whole presented with admirable clarity.[41] And in fact Young's accounts of the qualitative aspects of his experimental results (namely the appearance of interference fringes in certain circumstances and their disappearance in others) seem to have been clearly understood by corpuscularists like Biot and Brougham, both of whom report them accurately.[42]

varying degrees of firmness. Some scientists (like Potter; see for example his [1833]) continued to work in an effort to discomfit the wave theory. Yet no serious scientist of 1830 denied that, *as things stood*, the wave theory was superior to the corpuscular theory. This is nicely exemplified by the attitude of Sir David Brewster. It is clear from his work (though he never makes it explicit) that he was very attached to the corpuscular theory, and Moon (amongst others) classified Brewster as an 'anti-undulatist' (see Moon [1849], p. 58). Yet Brewster admits that 'the theory of undulations has made great progress in modern times, and derives such powerful support from an extensive class of phenomena, that it has been received by many of our most distinguished philosophers' (Brewster [1831], p. 135), and that 'The inability of the undulatory theory to explain the phenomena of inequal refrangibility, is almost the only exception to its universal application in accounting for the most complicated phenomena of light.' (Brewster [1832], p. 317.) All of which illustrates the importance of sharply distinguishing between scientists' beliefs about the universe, their choice of which of the various competing theories to try to develop, and their appraisals of the merits and demerits of these theories. See below, §5, pp. 161–3.

[39] Whewell [1837], p. 348.

[40] This occurs on p. 165 of Young [1802*a*]. (All page references to Young's work – except for his [1807] lectures – are to the reprints in the [1855] Collected Works.)

[41] This applies to all the different versions of the interference principle; see below, pp. 138–42. (For an important qualification in the case of the two slit experiment, however, see below, pp. 152–6.) But it does not mean that these clear statements were clearly problem-free.

[42] See Biot and Pouillet's appendix to volume 4 of Biot [1816] and Brougham [1803] and [1804]. Admittedly Brougham makes one or two minor mistakes but these clearly arise from simple misreading and not from fundamental misunderstanding.

This ought then to have been enough. As long as these allegedly crucial aspects of his work were clearly presented, any obscurity in the rest of Young's work is unimportant and certainly cannot be used to explain other scientists' failure to accept the wave theory of light as superior to its rival.[43]

Finally the 'character assassination' explanation of the neglect of Young's theory. The claim here, remember, is that Young's reputation was so marred by Brougham's reviews that no one took him seriously.[44]

This claim is – at least *prima facie* – rather implausible. First of all, even if the claim were true it would only explain Young's theory's lack of success in England, and not in France where the *Edinburgh Review* was not at all widely read. But is the claim true? If it is, there must have been very few counter-suggestible people around at the beginning of the nineteenth century. For one would have thought that Brougham, by attacking Young so loudly and aggressively, gave Young so much (adverse) publicity as to make it extremely *unlikely* that his researches would remain 'absolutely unnoticed by men of science for many years'.

Peacock himself in the end admits that the story is implausible, although he still expects us to believe it. He tries to make it more credible by referring to the great authority of the *Edinburgh Review*. Peacock writes that the influence of Brougham's reviews 'upon public opinion was *more remarkable than could reasonably be expected*, even from *the great authority of the publication in which they appeared*...'[45] Young's second biographer Wood also tries to make the 'character assassination' claim more plausible by telling us how authoritative the *Edinburgh Review* was.[46]

[43] This already indicates one way in which methodological considerations can shift historical problems. If a methodology were to pronounce Young's work as having established the wave theory's superiority, but having established it for reasons different from those normally alleged, then different aspects of Young's work may have to be looked at and these may indeed turn out to be obscure. (See below, p. 120.)

[44] Some of my arguments for the untenability of this claim were anticipated by David Hargreave of the University of Wisconsin in an excellent short paper entitled 'A New Look at the Young–Brougham Controversy', which was read before the Mid-West Junto meeting of the History of Science Society (University of Oklahoma, Norman, Oklahoma, April 4, 1969), and of which Professor Hargreave kindly allowed me to read a copy. (Hargreave's paper unfortunately remains unpublished.)

[45] Footnote in Young [1855], p. 192 (my italics). As we have seen above, pp. 111–12, most commentators report the hypothesis of Brougham's influence as a fact. Larmor (in his [1934] essay on Young in *Nature*) is one of the few who admit the implausibility of the hypothesis, though even he does so in a half-hearted way. Larmor writes that Brougham's 'satire [!] is commonly held to have diverted men from any attentive consideration of the new discoveries [of interference effects], by discrediting their author, and so as is said managed to postpone the progress of optical science for twenty years. *But that is possibly ascribing to him too much credit*...' (p. 277, my italics). Larmor goes on to mention that Young's personal reputation does not seem to have suffered at Brougham's hands – cf. below p. 118.

[46] Wood [1954], pp. 168–9. This Peacockian explanation which Wood supports is, however poor, an improvement on Arago's earlier attempt to make the story more

But Brougham's first two articles appeared in the *very first* volume (second number) of the *Edinburgh Review*.[47] It seems unlikely that the *Review* achieved its authority instantly; unless for some reason the British educated world was awaiting the launching of the new literary enterprise with bated breath.[48] This *may* have been the case, but it is certain that those who thus awaited its inception did not include its editor-writers – at least if the attitude of one of them, Francis (later Lord) Jeffrey, is representative.[49] Five months before the first issue actually appeared Jeffrey wrote to his friend Morehead:

> Our Review has been postponed till September, and I am afraid will not go on with much spirit even then. Perhaps we have omitted the tide that was in our favour. We are bound for a year to the booksellers, and shall drag through that I suppose for our indemnification.[50]

And a month later, writing to his brother John, Jeffrey was even gloomier:

> Our review is still at a stand. However, I have completely abandoned the idea of taking any permanent share in it, and shall probably desert after fulfilling my engagements, which only extend to a certain contribution for the first four numbers. I suspect that the work itself will not have a much longer life. I believe we shall come out in October, and have no sort of doubt of making a respectable appearance, though we may not perhaps either obtain popularity, or deserve it.[51]

Of course, Jeffrey's gloom turned out to be unjustified and the *Review* was a success.[52] However, (*a*) its success was almost entirely based on its social and political content and in particular on its great reforming campaigns (e.g. against slavery) – campaigns which had scarcely started even in 1804 when Brougham's second and final attack on Young appeared; and (*b*) as one might have expected, in the course of these campaigns it

plausible by referring not to the eminence of the *Edinburgh Review*, but to the eminence of Henry Brougham. He claims (see his [1859], p. 516) that we cannot blame the public for adopting in this case the views of the journalist Brougham for 'The journalist, in fact, was not one of those unfledged critics whose mission is not justified by any previous study of the subject. Several good papers, received by the Royal Society, had attested his mathematical knowledge...the profession of the bar in London had acknowledged him one of its shining luminaries: the Whig section of the House of Commons saw in him an efficient orator, who in parliamentary struggles was often the happy antagonist of Canning: this was the future President of the House of Peers – the present Lord Chancellor. How could opposition be offered to unjust criticisms proceeding from so high a quarter?' Arago conveniently forgets that, although this was all true when he wrote in 1832, when Brougham wrote against Young he was not an experienced and revered Lord Chancellor but only a young man of 23.

[47] Hargreave made this same point before me (in the paper quoted above, note 44).

[48] Paul Feyerabend drew my attention to this possibility.

[49] The instigators and writers-cum-editors of the early editions of the *Edinburgh Review* were Sidney Smith, Jeffrey, Horner and Brougham. Each of these was to achieve national fame, but in 1802 (the first number of the *Review* appeared in October of that year) they were relatively unknown young men. (Their respective ages in 1802 were 31, 29, 24 and 23.) Jeffrey soon became its first full-time editor. Brougham, in particular, seems then to have had only a limited reputation (as a rather unpredictable up-and-coming young man).

[50] Quoted in Cockburn [1852], p. 129.

[51] *Ibid.*

[52] For the full story of this success, see Clive [1957].

built up a substantial band of devoted enemies – these people would if anything have been predisposed *in favour of* anyone who had been attacked in the *Review*.[53]

Before leaving this 'character assassination' claim, I should like to point out that none of the historians who have written on the Young–Brougham controversy have considered it worthwhile to inquire in detail into the intellectual merits and demerits of Brougham's arguments. Did Brougham have any good arguments, for example, for his claim that Young's wave theory 'teaches no truth, reconciles no contradictions, arranges no anomalous facts, suggests no new experiments, and leads to no new inquiries'?[54] All these historians wrote after the wave theory had triumphed, and so perhaps they felt that the fact that Brougham supported the corpuscular theory meant that his arguments could not be worth considering.[55] I shall not follow this tradition. Indeed, we shall see as we go along that although Brougham certainly made mistakes in these attacks (which must have been very hurriedly written), some of his arguments are quite perceptive.

Apart from all this, the three socio-psychological explanations of the lack of recognition of Young's work would, if true, explain too much. We should expect someone who had contradicted the holy word of Newton, who wrote so obscurely that no one could understand him, and whose reputation had been savaged by a highly reputable journal to meet with little personal success. And yet Young was highly successful.[56] Well before the acceptance of the wave theory (an event which Young lived to see and profit by[57]) Young had become part of the scientific establishment. He was

[53] I should add that Brougham is attributed with attacks on other scientists in the *Review* almost as vituperative as the attacks on Young. In particular he attacked Count Rumford and W. H. Wollaston, neither of whom can exactly be said to have had his scientific career or scientific credibility destroyed!

[54] Brougham [1803].

[55] G. N. Cantor is a partial exception to this. In his scholarly and interesting [1971] (one of a series of interesting papers by Cantor connected with Young's optical researches) he does mention some of Brougham's arguments and even mentions that Young in his 1804 *Reply* 'failed to appreciate many of Brougham's more pertinent remarks, when they did not coincide with his conceptual scheme' (*op. cit.*, p. 88). (Unfortunately Dr Cantor does not go on to substantiate this remark.) But even Cantor largely concentrates on the question of the *motives* behind Brougham's attack and (usually) fails to go into the question of the scientific merits and demerits of the criticisms Brougham puts forward. Cantor mentions the usual explanation (based on personal animosity) of the ferocity of Brougham's attack, but instead traces the differences between Young and Brougham to differences of a '*methodological* character'. By this he means that Brougham had certain views about science which might have led him to find any hypothesis unacceptable if it involved the 'unobservable' ether. I don't think that this will do. Positivistic attitudes, like the one attributed to Brougham, tend to disappear if the theories involving the disputed entity are good enough. After all, Brougham certainly accepted 'unobservable' forces. Anyway why may not Brougham's main motive have been scientific rather than methodological: perhaps he just wanted to get at the truth; perhaps he was genuinely convinced that Young's theory was hopelessly false?

[56] Hargreave too (see note 44, above) points out that 'there is little evidence that Young's reputation within the scientific community was dimmed by his encounter with Brougham'.

[57] Young died in 1829.

on three separate occasions invited to give the Bakerian lecture to the Royal Society.[58] He was appointed Foreign Secretary of the Royal Society in 1802 and he also became (at the invitation of Rumford) Davy's professorial colleague at the Royal Institution. In 1812 he was offered the secretaryship of the Royal Society. What more could a full-time physician expect? Moreover, Young during his career had no less than 16 papers published in the *Philosophical Transactions* (11 of them after Brougham's attacks, which had allegedly destroyed his scientific reputation and in which the Council of the Royal Society had been rebuked for admitting his and others' 'paltry and unsubstantial papers into its Transactions'). Young was also invited to contribute well over 50 articles on a variety of subjects to the *Encyclopaedia Brittanica,* as well as a supplement to an article by the great Arago.

Something then is wrong with each of the three time-honoured explanations of why Young's fellow scientists failed to recognise the superiority of the wave theory and pushed on with Newtonian optics, despite all Young's counter-arguments and crucial experiments. Are we then to look for other non-intellectual, 'external' factors to explain the action of Young's contemporaries? This is certainly a possible course of action (and may possibly be successful). It is not, however, the only such possibility. But these other possibilities will not be visible until it is recognised that the reception of Young's work appears noteworthy and in need of explanation only in view of the assumption that his work *ought* to have been taken seriously because it established the superiority of the wave theory. The problem of what features a theory must possess in order to be superior to a rival theory is, however, a (normative) methodological problem – and different methodologies may specify different features. Our historical problem thus depends on the application of methodological considerations. Once this is realised new avenues of approach to the problem are opened.

The highly favourable appraisal of Young's work is, no doubt, usually taken for granted by historians, or, perhaps, argued for (as I indicated earlier) in rather uncritical falsificationist terms. It would be interesting to sharpen up these implicit methodological assumptions, and indeed to look at Young's work through the eyes of *various* explicit methodological criteria. We could then see, in each case, how much of this historical episode the methodology could explain 'internally' (i.e. in terms of the intellectual merits of Young's work, as it sees them) and how much was left to be explained with the help of 'external' factors.[59] This would,

[58] The information that these were probably three separate invitations was kindly supplied by the Librarian to the Royal Society.

[59] I use the terms 'internal' and 'external' in the redefined (and methodology dependent) sense given them in Lakatos [1971], reprinted above. Note in particular that in Lakatos's sense not all external factors need be social, though most methodologies will characterise social factors as external.

however, be a very protracted exercise.[60] Instead I intend to apply to Young's work the explicit criteria of appraisal supplied by one methodology – the methodology of scientific research programmes.[61]

This restricted exercise will in any case highlight the two main ways in which historical problems like that of Young's 'neglect' may be shifted via methodological considerations (and it will provide an illustration of one of the ways).

The first main way is this. Suppose that explicit methodological analysis of Young's work reveals that it did indeed contain the vital intellectual turning-point in the wave–corpuscle debate, but that this analysis locates the turning point in a place other than those generally pointed to. In this case the problem of Young's neglect will still exist and will still be in need of an external solution. Now, however, the search for external factors will have been redirected, historians will have to explain why Young's fellow scientists failed to spot the importance of this *new* aspect of Young's work.

The second way in which this historical problem (and others like it) may be shifted by methodological analysis is as follows. Suppose that the methodological analysis of Young's work reveals that it did *not* establish the wave theory's superiority. In that case there will be no reason to invoke external factors to explain the failure of Young's contemporaries to see the superiority of the theory. If, moreover, the methodology can also point out the factor which did eventually turn the debate in favour of the wave theory, then the whole episode of the nineteenth-century revolution in optics (both its failure to occur in the 1800s and its occurrence in the 1820s) will be capable of an internal explanation in terms of the differing objective merits of the (developing) theories involved.[62]

3 *What did Young really achieve? A reappraisal of Young's work*

My intention is to use the methodology of research programmes to provide a new answer to the question of what scientific conclusions Young's contemporaries ought to have drawn from his work. This reappraisal of Young's work turns out to provide a solution to many of the problems associated with its immediate reception, by showing the reactions of

[60] The exercise would hardly be complete unless it included a look at the whole episode of the early nineteenth century revolution in optics. For a methodology may pronounce Young's work non-crucial, thus avoiding the need to invoke external factors to explain its 'neglect'; but it may also be unable to point to anyone else's work as establishing the wave theory's superiority – thus incurring the need to explain the eventual *acceptance* of the wave theory in external terms.

[61] For the methodology of scientific research programmes, see especially Lakatos [1970], and Zahar [1973].

[62] This, I shall argue, is precisely what does happen when Young's work is reappraised using the criteria supplied by the methodology of scientific research programmes. The question of how far this ought to be regarded as a success for that methodology is a difficult one which I shall leave until §5.

Young's contemporaries to be allied much more closely than is usually believed to the intellectual situation in early nineteenth-century optics.

3(*a*) *Two expectations based on the methodology of scientific research programmes*

Some experiments come out in favour of one theory and against another. As Duhem showed,[63] however, these are not crucial experiments in any conclusive sense, for, in drawing from the two theories the consequences which are compared with experiment, extra assumptions must be made, and it may be these assumptions which are false. The proponent of a theory may therefore defend it against an apparent refutation, an apparent 'crucial experiment', by modifying not the theory itself, but one of the requisite auxiliary assumptions. According to Lakatos, this is typically what happens in the development of science. In other words, the rivalry between two theories (or rather programmes) is not considered settled at a single blow by 'crucial experiments'. Rather the title of crucial experiment is an honorific one (mistakenly) conferred by *later* commentators on one of the many 'anomalies' which the eventually defeated theory never managed to solve except in an *ad hoc* way. Indeed, on the basis of the methodology of research programmes one would expect[64] that, for any famous crucial experiment in the history of science, the protagonists of the theory allegedly defeated by the crucial result could, and did, develop their theory so as to explain this result – the trouble was that such explanations, though certainly constructible, were unacceptable because in various senses *ad hoc*. I shall argue that this first expectation is satisfied in the case of Young's allegedly crucial experiments in §3(*a*1) below. The methodology of scientific research programmes also leads one to expect that in the case of any crucial experiment there were, at the time, other experiments whose results were just as anomalous[65] for the eventually successful theory as the 'crucial' result was for the eventually defeated theory, and that it was only in view of later developments (i.e. the 'progressive' explanation of some of these results by the successful theory) that the title 'crucial' was not conferred on *these* experiments. I shall show that this expectation too is satisfied in the special case of Young's 'crucial' experiments (§3(*a*2)).

[63] See Duhem [1906].

[64] The status of these 'expectations' is considered in §5, pp. 173–6.

[65] I talk here as though there are *degrees* of anomalousness. It surely is the case that either a theory conjoined with accepted statements of initial conditions is inconsistent with an experimental result, or it is not. This is a straightforward black and white situation. The grey enters when we consider the real historical facts rather than methodologists' fairytales. In fact theories are not normally made sufficiently precise for them to make predictions in certain areas. In order for these theories to be capable of being inconsistent with results in these areas certain auxiliary assumptions have to be added. The range of auxiliary assumptions which would render the basic theory inconsistent with these results and their 'naturalness' within the programme of which the theory is a part are then questions of degree (which moreover may be characterisable only intuitively).

3(*a*1) *Corpuscularist explanations of Young's 'crucial refutations'*

The results of two of Young's experiments are alleged by various authors to have provided crucial evidence in favour of the wave theory of light and to have crucially refuted the corpuscular theory. These experiments are the 'two slit' experiment, reported in Young's *Lectures in Natural Philosophy*, delivered in 1802–3 but published only in 1807, and a special kind of diffraction experiment, described in a paper delivered to the Royal Society in 1803 (and published as Young [1804*a*]).

The two slit experiment consists in allowing homogeneous light, diverging from a single slit in a first screen, to fall on a second opaque screen in which there are two narrow and closely adjoining slits (symmetrically placed with respect to the first slit); the effects are observed on a third screen some distance from the second. The result of the experiment is that the observation screen is covered with a series of bands alternately light and dark, the centre of the pattern being light. (If the experiment is performed with white light a series of coloured bands will be observed, only the central band being white.[66]) I shall consider below what positive role this experiment plays in supporting the wave theory.[67] Let us for the moment assume that Young's wave theoretic explanation of these bands, as due to the alternately constructive and destructive interference of light coming from the two slits, is acceptable and unproblematic. Did the experimental result refute the corpuscular theory?

Certainly no one, so far as I know, produced a corpuscular theory of light which could explain all the details of the fringes and their positions on the observation screen. Also, precise versions of the corpuscular theory no doubt could be constructed which, together with accepted initial conditions, would be inconsistent with the result of the two slit experiment. Thus there is no denying that the two slit result presented a problem for the corpuscular theory. It was, however, not Young who first presented corpuscularists with this problem, for it was of a kind that they had known about since 1665.

From the point of view of the corpuscular theory, Young's two slit experiment was merely a more complicated form of the diffraction experiments first recorded by Grimaldi in 1665. Grimaldi had reported that the real shadow of a narrow opaque object in sunlight is wider than its 'geometrical' shadow,[68] and that coloured fringes are observable both inside and outside the shadow.

[66] Young discusses only the case of homogeneous light. (See below p. 153.)

[67] See pp. 143–6.

[68] This whole situation is somewhat confused. What one counts as the actual, as opposed to the geometrical, shadow of an opaque object was later shown (by Fresnel) to be arbitrary. However, for all but the smallest opaque objects, Fresnel's theory predicts that while the intensity on the observation screen will tend to zero as one goes into the object's geometrical shadow on either side, the intensity will nevertheless be quite high

The results of Grimaldi's experiments and of similar ones performed by Hooke, Newton[69] and others had convinced corpuscularists that the assumption that light is propagated in straight lines had to be amended. The diffraction effects[70] had to be explained, however, as small-scale *exceptions* to the general rule of rectilinear propagation, for facts like those of eclipse phenomena and our inability to see through a bent tube were taken on all sides as conclusive evidence that at least most of the light is propagated rectilinearly most of the time.[71]

some distance *into* the geometrical shadow – i.e. that what one might naturally take as the 'apparent real shadow' will be *smaller* than the geometrical one (altough there will of course be dark bands *outside* the geometrical shadow). Grimaldi, however, categorically states that the 'shadow...appears considerably larger in fact that it ought to be, if the whole thing is supposed to act by straight lines...' (Grimaldi [1665]; see Magie [1935]). Most modern textbooks nevertheless inform us that Grimaldi discovered that real shadows are smaller than geometrical ones. This is true even of the 'popular textbook' by Holton and Roller which has at least one eye on history. Holton and Roller write: 'Grimaldi put a tiny obstacle in the path of the beam and found its shadow...to be smaller than that to be expected...' (Holton and Roller [1958], p. 545). Young, like Newton, accepted that Grimaldi had shown that real shadows are enlarged relative to their geometrical counterparts.

[69] Some interesting problems arise from Newton's various accounts of fringes inside and outside shadows. In his 1675 'Second Paper on Light and Colours', Newton has a diagram (p. 199 of the reprint in Cohen [1958]) in which he drew three fringes *inside* the shadow of a *large* wedge. Now obviously Newton could not have performed this experiment, since it is a 'diffraction by a straight edge' experiment, and as is now well known, no fringes are observed inside the geometrical shadow in such a situation. (Historians have failed to spot this point, so far as I can see.) In Part I of the third book of the *Opticks*, however, no mention is made of fringes inside the geometrical shadow, and all the diagrams, even of the shadows of hairs, show only external fringes. This fact has been much commented upon. Mach, in his [1926], for example, writes of this investigation in the *Opticks*: 'Strange to say, Newton completely ignored the inner fringes observed by Grimaldi; he even explicitly asserted that diffraction only took place in an outward direction from the edge of the shadow-forming body, and all his diagrams give effect to this opinion.' (p. 143). This mystery was cleared up by Professor Stuewer who, in what seems to me an exemplary piece of work (Stuewer [1970]), repeated Newton's experiments, using Newton's own descriptions and other historical evidence to produce experimental equipment which he conjectured was very similar to the equipment Newton used. The result of the experiment was that no fringes were observed inside the shadow. Since Newton does not claim to have performed the experiment he describes in his [1675] but only to be reporting Grimaldi, Stuewer's conjecture is that Newton at first accepted Grimaldi's results (though what I say above about Newton's diagram suggests that he did not understand Grimaldi) but then repeated the experiments himself and decided on the basis of his own results that only external fringes exist.

[70] I use '*diffraction*' (a term introduced by Grimaldi) in a theory-neutral sense. As we shall immediately see, Hooke, and following him Newton, explained diffraction effects as due to the '*inflection*' of light by material bodies. Young (and at first Fresnel) described them, rather confusingly, as due to the interference of inflected rays. Fresnel eventually satisfactorily explained them on the basis simply of his theory of the propagation of light.

[71] This was Young's position too. His theory faced two problems here, however (those of explaining both rectilinear propagation and the exceptions to it), while the corpuscular theory faced only one (rectilinear propagation being taken care of, as we shall immediately see, by Newton's first law of motion). Young's rather unsuccessful attempt to solve these two problems of the wave theory will be discussed in detail below pp. 144–6. *Fresnel completely transformed this situation by progressively explaining (near) rectilinear propagation as a special case of diffraction.*

Corpuscularists solved this problem by assuming that light, in passing by a material body, is acted upon by short-range forces which emanate from the body and which deflect the light corpuscles from their naturally rectilinear path. A fundamental assumption of the corpuscular theory (it is better described as part of the hard core of the corpuscular programme[72]) was that light corpuscles obey the ordinary laws of particle mechanics. Any deviation, therefore, from a straight path indicates the presence of external forces acting upon the light corpuscles.[73]

Thus the protagonists of the corpuscular theory had a clearly defined way of explaining Grimaldi's diffraction fringes. The existence of fringes both inside and outside the geometrical shadow showed that both attractive and repulsive forces were active, which had anyway already been 'established' by the fact that transparent bodies could both reflect and refract light. The fact that in sunlight the fringes are variously coloured showed that the same forces have different effects on the particles corresponding to the different colours. But this had already been 'established' by the fact of prismatic dispersion.[74] When homogenous light is used, the corpuscular theory could explain the light fringes inside the geometrical shadow as due to the short-range forces deviating the corpuscles towards places they would not otherwise reach; and it could explain the dark fringes outside the geometrical shadow as due to the forces deviating the corpuscles away from places they *would* otherwise reach.

By assuming that the action on the light is alternately attractive and repulsive at different distances from the body, any given set of fringes could, given sufficient ingenuity, be explained. Thus, so could Young's two slit 'interference' pattern. But given that corpuscularists had not so far dealt (in quantitative fashion) with the simpler cases of diffraction, they were unlikely to bother much with Young's two slit case, which seems from this point of view a complicated affair involving three sets of fringes – those produced by the narrow section of screen between the two slits and those produced by larger sections of the screen on either side.[75]

[72] I shall characterise the corpuscular and wave *programmes* more explicitly below, pp. 156–7 and 136.

[73] The aim of the corpuscular programme was thus to explain all such deviations, whether by reflection, refraction or diffraction as due to one single set of forces emanating from matter.

[74] The assignment to the corpuscles of a mechanical parameter, the different values of which would explain this different effect, constituted a major problem for Newton and the whole corpuscular programme. For Newton's difficulties on this point, see Bechler [1974]. Bechler does not remark that Laplace provided a solution of the problem within the corpuscular programme; see below, p. 128.

[75] The complexity of the two slit situation (the apparent simplicity of the account in most modern textbooks is, of course, only achieved by forgetting about the diffraction pattern produced by each slit separately) was pointed out even after the wave theory had been accepted. For example, a translators' footnote in the 1859 English edition of Arago's *Biographies of Distinguished Scientific Men* (p. 493) tells us that 'some of the earliest of Young's researches were complicated by unnecessary conditions. Thus, to exhibit the

It may be objected that although corpuscularists like Brougham,[76] Jordan[77] (and later Biot[78]) might succeed through a suitable assignment of short range forces in explaining the two slit interference pattern, they could not, at the same time, possibly succeed in explaining – even in an *ad hoc* way – the change in pattern when one of the slits is closed. For, as we now know, points on the observation screen which had been dark when both slits were open, are illuminated when one slit is closed. Surely, this establishes that light can be subtracted from darkness to produce light and hence establishes Young's fundamental claim that light added to light can produce darkness. Is not *this* the final crucial blow to the corpuscular theory?

It may seem strange that Young never records having performed this simple and apparently vital modification of his experiment.[79] It is, however,

effect of *two* rays interfering, he at first not unnaturally transmitted the narrow beam of light through *two* small apertures near together. In point of fact, though the real effect is here seen, it is mixed up with others of a more complex kind.'

By the way, the possibility of corpuscularists appealing to the effect of the screen on the light in order to account for the result of the two slit experiment, is often pointed out by textbook writers. (I am indebted to M. L. G. Redhead for pointing out to me just how widespread this comment on Young's result is.) For example, Hardy and Perrin write ([1932], p. 569): 'Young's explanation of the phenomenon was not immediately accepted because of the possibility that light undergoes some modification in passing through the pin-holes.' And Jenkins and White write ([1957], p. 239): 'Soon after the double-slit experiment was performed by Young, the objection was raised that the bright fringes he observed were probably due to some complicated modification of the light by the edges of the slits and not to true interference. Thus the wave theory of light was still questioned.' Unfortunately these remarks are usually introduced as an afterthought to highlight the importance of experiments like Fresnel's biprism and two mirror experiments in which the interfering beams are created from one beam without the use of slits. The remarks are usually inconsistent with others made about Young's experiments. This is certainly the case, to take an example, with Jenkins and White. A mere eight pages before their remark just quoted they write: 'The first man successfully to demonstrate the interference of light, and thus establish its wave character, was Thomas Young. In order to understand his crucial experiment performed in 1801, we must first...' (*op. cit.*, p. 225). They go on to describe the two slit experiment. And 30 pages earlier than that they state '[Young's] early work proved the wave nature of light...' (p. 203). Of course one can rescue them from the charge of inconsistency by assuming that they meant that *given what we now know* Young established the wave theory of light, but people at the time did not know enough about the nature of diffracted light to realise it. This position, however, is surely untenable. One may as well say that the wave theory of light was established by the first person to observe what we now know to be diffraction fringes. This was probably the first creature that evolution endowed with eyelashes, who would see diffraction bands when looking at the sun through half closed eyes.

[76] See Brougham [1796]. Some of Brougham's theories were criticised and developed in Prévost [1798]. [77] See Jordan [1799*a*], [1799*b*] and [1800].

[78] See especially Biot [1816], vol. 4 and appendix.

[79] Holton and Roller decided that this is *so* strange that it cannot be true and wrote 'Young, the first to demonstrate the "double-slit experiment", also pointed out that this interference pattern is immediately destroyed when either of the two slits is covered up.' (Holton and Roller [1958], p. 557.) Humphrey Lloyd had, much earlier, made the same mistake: 'That these alternations of light and darkness [in the two slit experiment] are produced by the mutual actions of the two pencils, Young proved by the fact that when one of the beams is intercepted, the whole system of fringes instantly disappears.' (Lloyd [1833], p. 303.) Mach ascribes the true situation to luck, reporting that Young 'did not happen to chance' on this modification of his two slit experiment (Mach [1926], p. 147).

not so strange. For suppose Young *had* closed one of the two slits and observed the one slit diffraction pattern, how could he possibly have explained it himself? For where now are the two portions of light which are to interfere to produce the fringes? Light 'diffracted in all directions' by the now *single* slit is of no use, since two 'portions' of light have to coincide in direction in order to interfere. This difficulty is symptomatic of a much more fundamental defect in Young's theory which I shall discuss later;[80] for the moment I return to Young's experiments. Although Young does not record having performed this modification of the two slit experiment, he does record having performed what we should now regard as an analogous experiment.[81]

This is the 1803 diffraction experiment – the second of the experiments alleged to be crucial by commentators on Young's work. Young himself, his early commentators Whewell and Peacock and some later writers like Whittaker all record this as *the* experiment which established Young's principle of interference and hence refuted the corpuscular theory.[82]

Young's experiment consists, like Grimaldi's,[83] in observing the fringes inside and outside the 'shadow' of a narrow opaque object. Young then modified Grimaldi's experiment by screening off the light passing one side only of the diffracting object. The result is that the fringes previously visible inside the shadow disappeared.[84] Young at the time claimed this to be a 'simple and...demonstrative proof' that the two portions passing on either side of the object are concerned in the production of these inner fringes. Peacock, following Whewell, writes of this result: 'This was a *crucial* experiment, and may be considered as having constituted an important epoch in the history of the undulatory theory.'[85]

Now it would be strange if this were the case, for then the 'epoch' ought

[80] See below, pp. 144–7. I do not, of course, deny that Young could have developed an *ad hoc* explanation of the single slit diffraction pattern on the basis of his theories. In fact this pattern would no doubt be (for Young) a combination of the 'external fringes' produced by the two sections of screen making up the slit. These external fringes Young variously ascribed to interference between direct (rectilinearly propagated) rays and light inflected by the side of the screen, and to interference between direct rays and light reflected by the side of the screen. Neither explanation works; either explanation involves four portions of light – illustrating the arbitrariness pointed to below, p. 140.

[81] Which pairs of experiments are 'analogous' depends on theory. These two experiments are analogous according to the wave theory as later developed by Fresnel. Interestingly enough the two experiments are not analogous on the 'natural' interpretation of Young's theory. In the single slit case there is, apparently, only one 'portion' of light and one would expect, on Young's theory, no interference. In the narrow diffracting object case Young *can* see two portions of light; one passing either side of the object. (But cf. footnote 80 above.)

[82] See Whewell [1837], p. 326; Peacock [1855], p. 162; Whittaker [1910], p. 108.

[83] See above, p. 122.

[84] It is this qualitative aspect of the experimental outcome which normally is considered important. There are, nevertheless, other aspects of this result which turn out to be important in the light of the methodology of research programmes. See below, pp. 147–56.

[85] Peacock [1855], p. 162.

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already to have been 'constituted' in 1665 when Grimaldi recorded that, while fringes are visible inside the shadows of narrow objects, there are no fringes visible inside the shadows of large opaque objects. Young's narrow object plus screen combination amounts simply to a large opaque object.[86] But let us forget this problem, and again assume for the time being that Young's wave theory provides an acceptable and unproblematic explanation of this experimental result, and merely ask whether the result refutes the corpuscular theory.

As in the case of the two slit experiment there doubtless are precise versions of the corpuscular theory which contradict this 1803 experimental result. This is not, however, to deny the possibility of constructing assumptions consistent with the fundamental tenets of the corpuscular theory which, together with these tenets, explain Young's result. This possibility was, moreover, actually explored.

There were, in fact, two major ways of developing the corpuscular theory so that it explained Young's results. The first involves admitting that the joint effect of two beams of light may be darkness; this, however, is given a physiological rather than a straightforwardly physical explanation. This first explanation (which was only *in fact* resorted to when many more results concerning what we now call interference effects had been established) shows that the folklore is quite trivially wrong in claiming that Young's experiments crucially refuted the corpuscular theory. Some corpuscularists were willing to admit that particles of light *can*, in a sense, destructively interfere. This corpuscular explanation makes the apparent lack of illumination the result of two sets of particles arriving at the eye in a fixed 'phase' relation. There are two possibilities. It could be claimed that in the case of destructive interference the two sets of particles arrive in such a way that particles of the first set having stimulated vibrations in the eye, particles of the second set produce vibrations which interfere destructively with the first at the retina.[87] Alternatively it could be claimed that no light is experienced when the elements of the two streams of particles arrive in *unison* so that they, as for example Potter put it, 'cease by their combined bulks being too large to create that sensation which we call light.'[88]

These explanations cannot be dismissed *automatically* as cheap *ad hoc*

[86] This simple point must have been well known. To my knowledge the point was first made in print by Brewster (see Brewster [1831], p. 130). Later commentators seem to have missed it altogether; Mach is an exception, but he simply asserts that despite Young's result being well-known it was important. Mach writes: 'This experiment would, as Brewster rightly notes, have been really unnecessary, if Young had considered that wide shadow-forming objects show no fringes in the shadow. Nevertheless, the result is an important one.' (Mach [1926], p. 145.)

[87] See e.g. Young [1817], p. 328 and Fresnel [1822], p. 25. (It is often a sign that a programme is getting ahead of its rival, as the wave programme was beginning to in 1817 and definitely was in 1822, when its protagonists start to give their rivals ideas for new auxiliary assumptions!)

[88] Potter [1833], p. 33.

manoeuvres simply because they invoke physiological considerations.[89] Fresnel's wave theory also used physiological factors to explain certain phenomena. For example, the non-appearance of interference patterns in some circumstances is explained by Fresnel as due, not to the *non-occurrence* of interference, but rather to the *invisibility* of the interference fringes caused by limitations in our visual apparatus.

Nor was this use of physiological considerations an isolated event in the development of the corpuscular programme. For example, Laplace held that various experiments had shown that 'the relative velocity of a homogeneous light ray is constantly the same',[90] that is, that the observed velocity of light is always the same no matter what the relative velocity of the observer and the light source. He conjectured that this constancy was 'determined by the nature of the fluid which it [the light particle] puts in movement in our organs, to produce the sensation of light'.[91] More exactly, Laplace's hypothesis was that 'luminous bodies launch an infinity of rays subject to different velocities' but that the eye is capable of registering only those rays whose speed, relative to it, is comprised between certain very narrow limits.[92] This conjecture was regarded as confirmed by the discovery, made by Ritter and Herschel, of the 'invisible rays' emitted by the sun. And indeed Laplace's theory is independently testable (it was in fact eventually independently refuted), for it predicts that ultra-violet and infra-red rays have velocities different from that of light.

The proponents of the corpuscular programme, however, also had a second, rather less drastic, method of accounting for Young's 1803 diffraction experiments; a method which did not invoke any physiology at all. This method of explanation (already available to corpuscularists *before* Young produced his result) involved assuming that the forces, which were 'known' to emanate from the sides of bodies and to act on light, were capable of 'interfering'. This method was never (as far as I know) actually used in any published account to explain Young's result, but that it could be so used is clear from corpuscularists' explanations of other experiments.

Brougham, for example, published a long paper principally on diffraction effects in 1796 in which he explains the result of a kind of one slit experiment by assuming that the light-bending forces emanating from the sides of material bodies 'interfere'. He speaks of light being 'inflected' by

[89] Although as a matter of fact these particular explanations are *ad hoc* at least in the empirical sense, i.e. they predict no novel facts. (One can, as usual, envisage ways in which they might have been developed so as to become independently testable.)

[90] Laplace [1813], p. 327. Since, by the way, Laplace regarded this as empirically established and came to a theory very different from the special theory of relativity, this seems to provide a 'constructive' disproof of the (once popular) idea that Einstein induced his theory from the 'facts' about the propagation of light.

[91] *Ibid.* [92] *Ibid.*

one side of the slit and then, when the slit is narrow enough, being 'deflected' by the other side.[93]

A second example of a corpuscularian who used the interference of the light-bending forces to explain diffraction effects is G. W. Jordan. Jordan published in 1799 and 1800 a series of three monographs which record carefully the results of a great number of painstaking experiments on diffraction.[94] These experiments include the observation of the shadows of pieces of lead of steadily decreasing width. Jordan observed that 'By reduction of the breadth of the piece of lead... thin slender streaks of white light, with dark intervals began to make their appearance [within the shadow of the piece of lead]... By further reduction... these white streaks became broader and more distinct and approached the central dividing line of the shadow...'[95] This is substantially Young's 1803 diffraction experiment (at least in its qualitative aspects[96]) only in reverse. Young first observed the shadow of a narrow object and then saw the internal fringes disappear as the object was widened; whereas Jordan observed the internal fringes appear as the diffracting object was slimmed down. Jordan does not follow the above description of his experiment with an explicit explanation of its outcome. He does, however, explain why the fringe pattern in the one slit experiment changes as one narrows the slit. The changed pattern, according to him, is caused by the fact that 'by their [i.e. the sides of the slit] mutual approach *their various actions upon different portions of the intervening light begin to intermingle with each other*...'[97]

The change in diffraction pattern in the Grimaldi–Young experiment when light passing one side of the narrow object is screened off could thus be given a corpuscular–theoretic explanation without allowing that light beams mutually interfere and without invoking physiology. One has only to assume that in the absence of the screen, light passing one side of a narrow opaque object is acted on, not just by forces emanating from that side, but also by forces emanating from the other. However, screening off the light passing one side also screens off the forces emanating

[93] The experiment is the one first performed by Newton in which a beam of a light passes through a small gap between two knife blades which meet at a very small angle (see Newton [1730], p. 329). Brougham writes of the fringes produced in this experiment that they were 'formed by the *inflexion* of one knife and were moved into its shadow and separated and dilated by the *deflection* of the other' ([1796], p. 235).

[94] These monographs are Jordan [1799*a*], [1799*b*] and [1800]. Jordan's experiments were, in fact, too painstaking. He had no clear theories about what conditions were important for the production of diffraction effects and so he modified *all* the experimental conditions he could think of. The result is that Jordan's work provides a beautiful illustration of the anti-inductivist thesis that experimentation undirected by theory is worse than useless and leads nowhere.

[95] Jordan [1799*b*], p. 78.

[96] I consider below, pp. 147–56, the role of the quantitative aspects of Young's experiment in the wave-corpuscle debate.

[97] Jordan [1799*a*], p. 81; emphasis supplied.

from that side, and hence the path of the light not screened off is also affected.[98]

I do not claim that either of these two corpuscular explanations of Young's experimental results was unproblematic. The second explanation in terms of the 'inference of forces', for example, seems to require the light-bending forces to be sensible at small but nonetheless sensible distances, whereas other phenomena seem to require these forces to be insensible at any sensible distance. Furthermore, no one succeeded, in anything but an *ad hoc* way which required modification with each new experimental result, in assigning to opaque bodies specific light-bending forces obeying specific laws.[99] But no matter what difficulties these explanations run into elsewhere, they were possible corpuscularist explanations of Young's results. Hence neither of these now famous results crucially refuted the corpuscular theory in the usual sense.[100]

[98] My account here is oversimplified in at least one respect. The explanation in the text had, I think, struck Young himself as possibility. This is suggested by a further modification of his experiment, which he reports, but whose importance has not been commented on in the subsequent literature. Young, apart from placing the screen adjoining one side of the narrow diffracting object, also places it some distance behind the object, still screening off the light passing by one side only. In this case too the fringes inside the shadow disappear, despite the fact that, as Young significantly adds, the light passing the unobstructed edge of the object '*must have undergone any modification that the proximity of the other edge of the* [*object*] *might have been capable of occasioning*' (Young [1804*a*], p. 180; my italics). I did not include this modification in the text since it only complicates matters without affecting my claim that a corpuscular explanation of the experiment is possible. For the corpuscularist will now say that when the screen is held some distance from the diffracting object, the light, in passing the object, is again acted on by forces emanating from both sides, but the fringes thus produced are shifted and modified by the subsequent effect of the forces emanating from the screen.

[99] In previous drafts of this paper, written before I had found Jordan's little-known work, I had said that my account was simplified in a respect other than the one mentioned in footnote 98, viz. that no corpuscularist ever actually published the explanation I give in the text. Jordan's explanation certainly does not seem to have been applied to Young's 1803 diffraction result by other corpuscularists. Biot, for example, records Young's result in his [1816] (appendix to volume 4) as a so far unresolved anomaly. Brougham, in his second review, challenges the accuracy of the result itself (see Brougham [1804], p. 99). Nevertheless, Jordan's explanation came as no surprise for it is (in some sense) suggested by the corpuscular theory (or is within the spirit of the corpuscular programme). Of course, we should not expect the claim that it is generally easy for a programme to produce *ad hoc* explanations even of refutations of its theories always to be supported by the actual production and publication of such explanations by scientists at the time. Indeed, scientists will often fail to publish such explanations precisely because they intuitively recognize them to be *ad hoc*.

[100] Still a third corpuscular explanation might be possible in terms of the light-molecules' fits of easy transmission and reflection, since these were interpreted as dispositions of the molecule to be acted on by the attractive and repulsive forces respectively. (This might also indicate how the coloured fringes were formed in diffracted light since the molecules corresponding to different colours have different intervals of fit.) See below, p. 157. No one seems, however, even to have started to develop this as an explanation of the diffraction fringes. Brougham, as I pointed out, regarded Newton's fits hypothesis as 'unintelligible' (see above, p. 114). Biot, who developed the theory of fits, steered clear of any explicit attempt to *explain* diffraction effects (although he recorded lots of diffraction experiments).

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I now turn to the second expectation based on the methodology of research programmes: that the successful theory in any allegedly crucial experiment (in this case the wave theory) will have faced (at the time of the discovery of the 'crucial' result) anomalies which posed for it problems just as big as those posed for the defeated theory by the 'crucial' result. This expectation too is satisfied.

3(*a*2) '*Crucial refutations*' *of the wave theory*

Newton had long ago claimed that there was 'both Demonstration and Experiment' against the wave theory. The demonstration concerns the rectilinear propagation of light. I shall argue later that Young provided no acceptable reply to Newton's objection on this point.[101] But as well as this 'Demonstration' there were two distinct experiments which, in Newton's view, crucially falsified the wave theory of light.

The first of these experiments concerns the phenomenon of dispersion. Briefly, Newton held that his famous experiments on prismatic dispersion established that the splitting of a ray of sunlight into a spectrum of coloured rays was the result not of a modification of the ray of sunlight during its passage through the prism, but rather of the separation of the rays of different refrangibilities already present in the original ray.[102] Newton alleged that such a selection process would be impossible if light were a wave motion.[103] Newton thus regarded these researches (and especially his '*Experimentum Crucis*' in which the differently coloured rays of a spectrum are allowed to pass separately through a second prism) as especially damning for the wave theory.[104] Any wave theory, it seemed, would have to explain the creation of coloured light out of white light as due to a modification of the light by the prism – but then why did not the same modification occur when one of the coloured rays thus created crossed the second prism?[105]

These experimental results were not actually inconsistent with any specific version of the wave theory up to and including Young's. Huygens, for example, in his famous *Treatise on light* never mentioned, let alone explained, any colour phenomena, and thus *without the addition of extra auxiliary assumptions* Huygens's theory has no implications about dispersion. Similar considerations apply in Young's case. Although Young does make the odd suggestion about dispersion,[106] he makes no attempt to give a systematic account of it, and most of the time appears to be unaware of

[101] See below, pp. 144–7.

[102] 'Colours are not *Qualifications of light*, derived from Refractions, or Reflections of natural Bodies (as 'tis generally believed), but *Original* and *connate* properties...' (Newton [1671–2]; quoted from the reprint in Cohen [1958], p. 53).

[103] See, for example, Newton [1730], Queries 27 and 28.

[104] Because it hits at the whole wave *programme*. See below, pp. 136–8.

[105] The coloured rays in fact cross the second prism without any further modification of their colour.

[106] See, for example, his [1807], p. 463.

the problems it poses for his theory.[107] But this failure explicitly to predict the wrong results is to be regarded neither as a virtue of the wave theory nor as a criticism of Newton's argument. Indeed, scientists are not likely to oblige the methodologist–historian by developing their theories in an attempt to explain some group of phenomena if they guess that any specific theory to which this attempt will lead will be refuted by the facts. They are *not*, however, to get credit for simply failing to predict the wrong result (as was explained by Popper). What Newton was claiming was that any foreseeable addition of auxiliary assumptions to the basic wave assumptions which would produce a theory which made predictions about dispersion *would* be inconsistent with his experimental results.[108]

The second experiment which, Newton held, refuted the wave theory concerns double refraction and was first discussed by Huygens. Indeed, the full title of Huygens' famous book on light is: *Treatise on Light. In which are explained the cause of that which occurs in Reflexion, and Refraction. And particularly in the strange refraction of Iceland Crystal.* This title is, however, a fraud. The causes of 'the strange refraction of Iceland Crystal' are not explained in this work.

Bartholinus, in 1669, became the first to record that when a small object is viewed through two opposite faces of a crystal of calcite ('Iceland spar') it appears double. When a ray is incident on such a crystal it is, in general, split into *two* refracted rays. One of these rays, called the *ordinary ray*, obeys the usual law of refraction, the other does not and hence is called the *extraordinary ray*. Huygens's attempted wave theoretic explanation of this

[107] For example, Young often cites the fact that wave theories predict uniform velocity of propagation of light in a given homogeneous medium (since homogeneous elastic substances transmit all disturbances no matter what their size and frequency with the same velocity) as a great advantage of such theories over their corpuscularist rivals (see, e.g., his [1810], p. 253: 'Among the facts which appear favourable to the Huygenian theory we must first enumerate the uniformity of the velocity of light, in any one medium...'). But in both theories different refrangibilities are associated with different velocities, and so it ought to follow on the wave theory from the fact of prismatic dispersion that one disturbance (emanating from the sun, say) can be split into many different disturbances each crossing a single homogeneous medium (glass, say) with a velocity *different* from the others. This was indeed regarded as a major difficulty for the wave theory even after the revolution had occurred.

[108] This is a further argument for speaking of the wave *programme*. The situation can then be characterised more precisely as follows: no explanation of dispersion in line with the heuristic of the wave programme was possible either in Newton's or in Young's time. It also constitutes a further argument against what Kuhn and Lakatos call naive falsificationism. Kuhn, Feyerabend and Lakatos all point out that scientists often disregard inconsistences between accepted theories and accepted observational results in the hope that the theory concerned will eventually be so modified that this inconsistency is removed. But, as this example shows, 'problems' or 'anomalies' for a programme in actual scientific practice need not be clear cut inconsistencies with fully articulated theories. (No account of both prismatic dispersion and the propagation of white light in a vacuum was developed within the wave theory, until Gouy, in his [1886]. Even this account was far from being uncontroversial. For the controversy over Gouy's solution see Wood [1905], chapter VI. The account of the controversy was dropped from later editions of Wood's book after the new photon theory of light became known.)

phenomenon was that when a wave front impinges on such a crystal surface it generates two waves which cross the crystal simultaneously. The first yields the 'ordinary' ray and travels through the ether alone, the 'wave fronts' corresponding to it being spherical. The other (which yields the extraordinary ray) travels through both the particles of the ether *and those of the crystal itself.* It has different velocities in different directions, being transmitted by spheroidal surfaces. Huygens's sphere/spheroid construction gives the correct paths of the rays for various angles of incidence, as was later confirmed by Malus and Wollaston. As an explanation of the phenomenon, however, it is refuted by a simple experiment performed and reported by Huygens himself.[109]

There is one direction through a calcite crystal in which no double refraction occurs. This direction is called the optic axis. If two crystals placed one behind the other are similarly oriented with respect to their optic axes, a ray doubly refracted by the first will not be refracted doubly by the second – the ordinary ray produced by the first crystal will in fact be propagated as an ordinary ray by the second, and the extraordinary ray produced by the first as an extraordinary ray in the second.

Undeterred, Huygens explained the results of this experiment by assuming that a ray incident upon a calcite crystal is modified in such a way that the emergent ordinary ray consists of a disturbance which no longer has the capability of travelling through matter. (He has explained, remember, the extraordinary ray as yielded by a disturbance which travels through the particles both of the ether and of the crystal.) This explanation is immediately refuted by altering the inclination of the second crystal. If the principal sections (planes passing through the optic axis normal to a crystal surface) of the two crystals are perpendicular, the ordinary ray emergent from the first crystal is refracted extraordinarily by the second, and *vice versa.* At all relative inclinations of the principal sections apart from parallel and perpendicular, both rays emerging from the first crystal are doubly refracted by the second – i.e. four rays are produced. At this stage Huygens admits himself at a loss: 'But to tell how this occurs, I have hitherto found nothing which satisfies me.'[110] And so 'leaving then to others this research',[111] Huygens dropped his central problem. He should have also dropped the title!

Young was unable to offer any precise way out of Huygens's *cul-de-sac.* The only mention he makes of double refraction in his early papers is when he states that Huygens's *experimental* law, giving the position of the extraordinary ray as a function of the angle of incidence and the inclination of the optic axis, is more accurate than the one suggested by Newton.[112] In

[109] See Huygens [1690], pp. 92–4.

[110] Huygens [1690], p. 94. [111] *Ibid.*, p. 95.

[112] See his [1802*a*], pp. 166–7. Young was, of course, correct, but Huygens's experimental law was not dependent on the wave theory and could, as Laplace showed (see, e.g., Laplace [1809]), be explained within the corpuscular theory.

his later work,[113] he does present one or two suggestions on how one might construct a wave theoretical account of double refraction. He presents in fact the conjecture that double refraction might be explained by assuming that the ether inside birefringent crystals has different elasticities in different directions. This is, of course, as one would expect from Young, a highly intelligent suggestion, but as in other cases, Young never developed his idea into anything more than a suggestion. Suggestions have no influence on the appraisal of the existing merits of two theories.[114]

On the other hand, Newtonian optics could at least offer a *post hoc* explanation of these double refraction effects. It explained how the action of the second crystal on the rays transmitted by the first crystal changes as we turn the second crystal around the rays. Newton endowed his light corpuscles with sides – two opposite sides which have the property on which double refraction depends and two other sides which do not have this property. Whether any particle is transmitted as part of an ordinary or of an extraordinary ray was then made to depend on the inclination of its sides relative to the optic axis of the crystal. No such explanation of the results of the two crystal experiments could be given by either Huygens's or Young's wave theory. For the disturbance in a longitudinal wave is, by definition, *in* the direction of its propagation, and thus such a wave motion, unlike a Newtonian different-sided corpuscle, could not have different properties in different planes through the direction of its propagation.[115] The situation for the wave theory became, in this respect, much worse in 1810 when Malus recorded that light beams could be made to show this 'sidedness' (or could be 'polarised'), not just by passage through birefringent crystals, but by simple reflection at certain angles.[116]

The only major optical discovery made in the period between Newton and Young also turned out to be anomalous for the wave theory. This was the phenomenon of aberration, an apparent periodic movement of the fixed stars discovered by Bradley in 1728. Aberration received a simple explanation by the corpuscular theory in terms of the addition of the velocity of light particles and that of the earth. No such explanation could be given within the wave theory unless, as Young was to suggest, the earth

[113] See especially Young [1809] and [1817].

[114] Commentators generally, in fact, give Young great credit for this *aperçu*, regarding it as a remarkable anticipation of the 'Truth'. But since Young never turned it into a real explanation he should presumably be given credit only if his suggestion influenced the man who eventually produced the explanation – in this case, Fresnel. Young's work in this respect (as in others) seems to have had precisely no effect on Fresnel.

[115] Thus Newton had written: 'Are not all Hypotheses erroneous, in which light is supposed to consist in Pression or Motion, propagated through a fluid Medium? For Pressions or Motions propagated...through a uniform Medium, must be on all sides alike; whereas by those [double refraction] Experiments it appears that the rays of light have different properties in their different sides.' ([1730], Query 28.) Newton never fills in the sketch of his own theory.

[116] See below, p. 141.

moves freely through the ether.[117] This was, however, inconsistent with wave theoretic explanations of other phenomena,[118] and it even predicts that *all* bodies are transparent![119]

3(*b*) *The wave optics research programme and its eventual degeneration at Young's hands*

The standard account of the situation in optics in the first years of the nineteenth century is then inaccurate in both the ways the methodology of scientific research programmes would have led us to expect. Corpuscularists could deal with Young's alleged crucial refutations; and the wave theory faced empirical difficulties which seemed, at the time, just as intransigent as those faced by the corpuscular theory.[120] This shows that if we take into account the historical facts about the state of the two theories and the problems they faced, the decision on which one was better is not as straightforward as it is usually made to appear. The attitude of those scientists like Laplace, who preferred the corpuscular theory, cannot be dismissed as entirely intellectually baseless. Indeed, given the two theories as they stood at the time and the experimental evidence, Laplace's attitude seems rather reasonable. He wrote:

> The phenomena of double refraction and of the aberration of the stars, seem to me to give to the system of the emission of light, if not complete certainty, at least an extreme probability. These phenomena are inexplicable by the hypothesis of the undulations of a fluid ether. The singular property of a ray polarised by a crystal, of no longer dividing in passing into a second crystal parallel to the first, evidently indicates different actions of a single crystal on the different faces of a molecule of light, whose movements are...subject to the general laws of the movements of projectiles. [121]

Of course, as I stressed at the beginning, the investigation of the relative merits of two theories involves the application of some set of *norms*. I now intend to apply the set of explicit norms provided by the methodology of scientific research programmes to the wave and corpuscular theories as

[117] See Young [1804*a*], p. 188. Young, as usual (cf. above, p. 134), did not work out the consequences of his suggestion.

[118] Specifically in Young's case with his original explanation of the coloured diffraction fringes – a phenomenon he made depend on the presence of an 'inflecting atmosphere' of higher density ether around opaque bodies (see Young [1802*a*]). See also Cantor [1970–1] for an account of the various hypotheses Young made about the ether during his career.

[119] This rather striking refutation of the wave theory with its hypothesis of an all-pervading ether was apparently first pointed out by Halley.

[120] Of course the fact that a fact can be dealt with by a theory (i.e. that assumptions can be introduced which together with the theory imply a statement describing the fact) does not mean that it poses the theory no problem. The problem is not simply to produce any old explanation of a fact, but to produce a specific kind of explanation: a non-*ad hoc* one which is testable independently of the fact concerned.

[121] Laplace [1813], I IV, chapter XVIII; my translation. See also below, p. 160. The English translation by H. H. Harte has 'seemed' instead of (the correct) 'seem' – perhaps Harte, writing after the wave theory had been accepted, thought that Laplace was too great a scientist not to have come to realise that his appraisal was 'mistaken'?

they had been developed up to and including the first few years of the nineteenth century. (They are, in fact, better described as the wave and corpuscular programmes, as we shall immediately see.)

Various aspects of my account so far have suggested that there was no such thing as *the* wave theory. There was rather a *series* of such theories. Each theory was built around (and therefore logically implies) the fundamental assumption that light is some sort of disturbance in an all-pervading elastic medium. But this assumption alone does not account for specific optical phenomena – for this, extra assumptions have to be invoked. A sequence of different such additions to the fundamental assumption were made. But the shift from one set of assumptions to the next was not made in a random trial-and-error fashion. These shifts were occasionally spurred by empirical refutations (although refutations always remained). But more importantly, these shifts were guided by the aim of the whole wave optics enterprise – the union of optics with the mechanics of elastic media. This aim both guided the extension of the programme (via the accretion of suitable additional assumptions so that the whole theory made predictions about fields it had not hitherto covered), and it ruled out as unacceptable (and suggested replacements for) certain auxiliary assumptions which were actually made – and it ruled them out *independently of whether or not they were part of an empirically successful theoretical system.*

There was therefore a wave optics *research programme* complete with 'hard core' ('Light is a disturbance in an all-pervading elastic medium') and 'positive heuristic' ('Reduce all optical phenomena to the mechanics of elastic media, without invoking any force not already made available by theoretical mechanics'). In this programme mechanics, as we shall see more clearly as we go along, played a dual role. Newton's laws of mechanics were used (often implicitly) in deducing consequences from testable versions of the wave programme and thus played a part in explaining the phenomena. But mechanics also played a second, prescriptive role – all optical phenomena were to be explained as consequences of ethereal disturbances obeying the ordinary laws of mechanics without invoking 'special' or 'abnormal' forces not amongst those already made available by mechanics. There were several testable versions of the wave programme which, independently of any particular empirical difficulties, were pronounced unacceptable by this heuristic principle.[122]

[122] This heuristic principle plays a largely negative role as far as Young's work is concerned, pointing to certain inadequacies in his theories. It really begins to show its fertility in a positive way at the hands of Fresnel. Let me cite one very clear example. The law of reflection (that the angle of incidence equals the angle of reflection) was certainly amongst the best corroborated empirical laws in optics. Huygens's wave theoretical account of reflection has this law as a consequence (hardly surprisingly, since Huygens constructed the theory *ad hoc* precisely so as to account for the law!). Huygens's theoretical account can hardly then be said to have been in empirical difficulties. Yet the account is unacceptable from the point of view of mechanics since it requires certain point centres of disturbance in the ether to produce an *actual* disturbance only in one direction (the

The wave optics programme existed long before Young. Indeed it existed long before Huygens. As Verdet remarks in his preface to Fresnel's collected works: 'Neither Huygens, nor any of the other authors who, in the seventeenth century, considered light as a motion, present this idea as a personal invention. They treat it as one of those current hypotheses which do not belong to anyone, but which everyone is required to discuss.'[123]

The major contributor to the wave optics programme prior to the nineteenth century was undoubtedly Newton. It was Newton who had 'discovered' the periodicity of light,[124] and he who had suggested that Huygens's theory, though false, would have had greater verisimilitude and would have dealt with a greater range of phenomena had it characterised light as a *periodic* motion and the light of different colours as having different periods.[125] Newton also was the first to provide an analysis of harmonic motion;[126] and the first to base an explanation of a physical phenomenon (the peculiar tides at the East Indian port of Batsha) on a principle of interference or superposition.[127]

We see already from this the heuristic role of mechanics in Young's major 'theoretical discovery' – the principle of interference. All wave theorists prior to the latter half of the nineteenth century (Huygens included) assumed that the medium in which light is propagated obeys the laws of mechanics. Newton had produced an explanation of certain wave effects on the basis of an interference principle (which principle, it is interesting to note, Newton presents not as a new discovery but rather as a simple consequence of the mechanics of fluid media).[128] It was therefore not too much of a step to 'invent' the hypothesis that light waves interfere, given mechanics and given Newton's work both on periodicity and on interference. The only difference between Newton's principle of interference and Young's is that in Newton's tidal case the restoring force, which brings the particles back towards the equilibrium position from

disturbance being 'infinitely feeble' in all other directions). Fresnel set out to replace this account with a new one in line with mechanics, in which the disturbance was real in more than one direction. He produced in this way a theory which both explains the success of the previous theory and *corrects* it in certain cases (where the reflecting surface is very narrow).

[123] Verdet [1866], p. xv; my translation.

[124] That is, he conjectured that light was periodic and he supported his conjecture with his experiments on the colours of thin plates ('Newton's rings').

[125] It is not always realised that Huygens, usually regarded as the founder of the modern wave theory, made light consist not of nice smooth periodic wave-like disturbances, but rather of irregular pulses. Newton calls the assumption that the disturbances alleged to make up light are periodic, 'the most free and natural application of this [light is motion] hypothesis to the solution of the phenomena'. (Newton [1672].) Verdet remarks ([1866], p. xvii) that 'The first one who (after Newton) would dare to return to the wave theory, could not fail to consider the light waves succeeding each other periodically at regular intervals, depending on their colour....'

[126] Newton [1729], Book II.

[127] Newton [1729], System of the World, p. 586.

[128] *Ibid.*

which they have been disturbed, is supplied by gravity, whereas in Young's optical case the restoring force is supplied by the elasticity of the ether.[129]

Thus Young was not conjuring theories out of the blue, but was pursuing an already existing research programme. The question is how Young developed the programme – whether it progressed or degenerated at his hands.

There are several significantly different formulations of the law of interference to be found in Young's work. Together they form a classic case of a 'degenerating problemshift'.[130] I shall first chart this degeneration and then investigate the (independent) question of whether or not the original principle of interference constituted progress.

Young's first version of the law of interference states:

> Whenever two Undulations, from different origins, coincide either perfectly or very nearly in Direction, their joint effect is a Combination of the Motions belonging to each.[131]

This proposition is very different from the interference or superposition principle which appears in modern textbooks, which is suggested by mechanics and which states that, whenever several waves are propagated simultaneously, the disturbance at a given point is the sum of the disturbances originating from the individual waves. This modern principle explains both the interference of light in certain circumstances (if the two light beams act at the same point their effect is the vector sum of the disturbances belonging to each) *and* the failure of light beams to 'interfere' in the sense that a light beam carries with it beyond the point of crossing no sign of having crossed another beam. According to this modern principle (developed by Fresnel), light disturbances emitted from any number of sources and acting at the same point always interfere or superpose there. The circumstances in which observable interference fringes are produced are, however, limited by a variety of factors.

Young's claim differs from this modern principle in claiming only that the disturbances (positive or negative) sum when the two waves 'coincide either perfectly or very nearly in Direction'. This restriction of his principle to only parallel (or near parallel) light beams constitutes, I believe, an immediate indication of Young's lack of that tenacity which is generally necessary if important scientific discoveries are to be made. It marks the beginning of Young's policy of (too readily) modifying his wave theoretic assumptions in the face of empirical difficulties (even when these assumptions were dictated by the heuristic of the wave optics programme). Moreover, the modifications Young makes are always *ad hoc* – they are always made precisely so as to remove the difficulty in question without making any extra predictions about other situations.

No doubt Young produced this first version of his interference principle

[129] This is not said in an attempt to belittle Young's achievement, but to point to the power (and autonomy) of the programme.

[130] See Lakatos [1970], p. 118.

[131] Young [1802*a*], p. 157.

to take account of the admitted experimental fact that interference fringes are not in general *observable* unless the interfering beams are close to parallelism. This does not mean, however, that two non-parallel beams do not interfere or superpose. Indeed Fresnel gave a wave theoretic explanation of the non-appearance of interference fringes in these cases which presupposes that the two beams do superpose, but that the interference pattern produced changes so rapidly that the illusion of uniform illumination is created.[132] But rather than attempt to explain the refutation of the general superposition principle in the case of non-parallel beams as only an 'apparent refutation', Young restricts the principle to apply to only parallel or near parallel beams.[133]

In his second major paper on light published later in 1802 Young writes:

> The law [of interference] is, that 'Wherever two portions of the same light arrive at the eye by different routes, either exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any multiple of a certain length, and least intense in the intermediate state of the interfering portions; and this length is different for light of different colours.'[134]

Young clearly wants to create the impression that he is quoting himself. This is, however, far from being the case. Apart from the rather problema-

[132] See below, footnote 137.

[133] This is the method which Lakatos in his [1963–4] calls 'exception-barring'. There may well, by the way, have been a further difficulty (this time of a conceptual nature) behind Young's presentation of the principle in this restricted form. (That this, and the difficulty mentioned in the text, did play a role in Young's thought is, of course, a conjecture on my part – Young gives no explicit indication of what lay behind this formulation.) The conceptual difficulty is as follows. As we shall see, Young wants light to be propagated rectilinearly (with just a little fuzziness around the edges of beams). He also describes light as a disturbance in a fluid medium – the disturbances as in the case of sound being in the direction of propagation. Simple mechanical considerations suggest that he cannot then have *general* interference of light. Consider for example the following situation. Two rays of light (from coherent sources) are made to cross one another at an angle of 90°. If, as Young assumes, the disturbances are in the direction of the ray's propagation, then the resultant disturbance at the point of the medium where the rays cross will produce an *elliptical motion*, the exact form depending on the phase relation between light from the two sources (assumed constant) and on the relative intensities. If the two disturbances are of equal intensity and are either in phase or 180° out of phase, then the resultant disturbance will be in a straight line at an angle of 45° with both rays. Thus the disturbance here is in *neither* direction of propagation. (I should add that Fresnel too had some difficulty early on with the question of the interference of non-parallel beams, though this conceptual difficulty was completely solved when he adopted – and provided independent evidence for – the transverse wave hypothesis.)

The associated problem of the apparent failure of light rays to 'interfere' when they cross was by the way a standard problem for all theories of light. It is now often presented as an objection to the corpuscular theory: when two streams of light corpuscles cross, should not at least some of them collide and fly off in directions other than those of the two rays? But given that the speed of light is about 200000 miles per second, then even if one particle in a ray were separated from its predecessor by 1000 miles, around 200 particles would arrive at the eye in a second, easily creating what would on this view be the *illusion* of continuous emission. With particles so far apart the probability of a collision is of course negligibly small. This explanation of 'non-interference' seems to have been well known in the nineteenth century. See, for example, Herschel [1827].

[134] Young [1802*b*], p. 170.

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tical extension of the principle to light of different colours,[135] whereas previously light from any two origins could interfere, now all that is claimed is that two portions of the 'same' light (by which he presumably means light from the same origin) will interfere. In this passage Young's requirement that the two portions be of the 'same' light appears as a sufficient condition, but in the published version of his Royal Institution lectures it appears as a necessary condition. There Young writes:

> In order that the effects of the two portions of light may be thus combined [to produce 'the alternate union and extinction of colours'] it is *necessary* that they be derived from the same origin...[136]

So the 1807 version of Young's principle states that two 'portions' of light interfere if and only if they have a joint origin (and if and only if they are parallel or nearly so). By 1807 Young had clearly become aware of an obvious objection to his 'law of interference': by allowing any two sources of light (for example, two candles) to illuminate a screen we should cover the screen with interference fringes, which is contrary to experience.[137]

This 1807 version of the law is, however, open to a new and devastating objection of a conceptual kind already hinted at in Brougham's second attack. What is to count as two 'portions' of the 'same' light? If we screen off an arbitrary part of the light coming from a single source, ought we, according to Young's law, to create interference fringes in the light reflected by some convenient wall simply by removing the screen? For the light now coming from the two arbitrarily selected parts of the source

[135] Why should light of a specific wavelength be preserved as a separate entity when mixed with light of other wavelengths? This problem was not solved until Fourier analysis was applied to it in the late nineteenth century.

[136] Young [1807], p. 464; my italics.

[137] It is not, however, according to Fresnel's theory, contrary to fact! Fresnel, sticking to the heuristic of the wave programme, upheld the principle that any two optical disturbances acting at the same point superpose or interfere, and he explained the *invisibility* of the interference pattern in circumstances like the 'two candle' case as due to the fact that the pattern changes position so rapidly (because there is no constant relation between the phases of the various disturbances from the various parts of each source) as to create the *illusion* of uniform illumination.

This is in fact a typical example of Young's failure to stick to his (theoretical) guns when faced by empirical difficulties. In all his later published material (with one exception) he restricts the domain of application of the law of interference instead of trying to explain the refutation as only apparent. The exception is in his reply to Brougham (who makes something like this objection). There Young does half-heartedly try to produce such an explanation. He writes: 'Let us suppose the assertion [that his theory predicts interference fringes whenever two sources of light illuminate an area] true – what will be the consequence? In all common cases the fringes will demonstrably be invisible...' (Young [1804*a*], p. 203). His explanation of their invisibility is however, not very good: 'if we calculate the length and breadth of each fringe, we shall find that a hundred such fringes would not cover the point of a needle...' (*ibid.*). But it is a consequence of Young's theory (together with a few simplifying assumptions) that the breadth of the fringes is proportional to D/d where d is the length of the line joining the two sources, and D is the distance from the mid-point of that line to the point of observation. For a given separation of the sources, the size of the fringes can thus in principle be increased at will by increasing the distance between the sources and the observation screen.

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travels different distances before it reaches the wall. We can in fact regard the light emitted from, say, a single candle flame, as coming from any number of sources. If *we* decide to regard it as from two sources, since it is two sources of the 'same' light, Young's law of interference tells us it will produce interference fringes. If we decide to regard it as from one source, Young's law of interference tells us it will not produce fringes. Brougham rightly objects to this degree of 'observer interference':

It must follow [from Young's theory] that, by doubling the quantity of light on any place, we can cover it with coloured fringes; *or, which is the same thing, that coloured fringes are nothing absolute, but a mere relative idea*...[138]

The next version of the interference principle appeared in Young's 1817 article on 'Chromatics'. By then Young's theory had run into new empirical difficulties. Apart from the fact that half an undulation had to be assumed lost in various and rather ill-defined circumstances,[139] the discovery of polarised light by Malus in 1810 finally convinced Young that there was something fundamentally wrong with the wave theory.[140] Young in fact wrote of the state of the debate between the corpuscular and wave theories in the year of the publication of Malus's discovery (1810):

All the satisfaction we have derived from an attentive consideration of the accumulated evidence, which has been brought forward...on both sides of the question, is that of being convinced that much more evidence is still wanting before it can be positively decided.[141]

Young was even less happy with his theory in 1815 when he wrote to Brewster:

With respect to my own fundamental hypotheses respecting the nature of light, I become less and less fond of dwelling on them as I learn more and more facts like those which Mr. Malus discovered...[142]

In view of all this, it is not surprising that Young's 1817 formulation of his law is full of cautious qualifications. The central part of it reads:

...When two equal portions of light, in circumstances exactly similar, have been separated and coincide again, in nearly the same direction, they will either

138 Brougham [1804], p. 99; my italics. (This, by the way, is far from being the only correct argument against Young's theory that Brougham had.) This objection to the wave theory was again dealt with by Fresnel, who showed that light coming from a single light source (a candle say) could indeed be regarded as coming from any number of sources – no interference fringes are observed, however, because there is no constant phase relation between these different sources.

139 This lost half wavelength, although Young gave it the beginnings of dynamical explanation (see e.g. his [1817], p. 330), remained a bugbear for the wave theory for some time. See also below, p. 149, footnote 173.

140 Young was not here being a good falsificationist (as might at first appear). Malus discovered only that there are ways of 'polarising' light other than by passing it through a birefringent crystal. (The ordinary and extraordinary rays in double refraction are, of course, each polarised in different planes.) But polarised light was as old as Bartholinus's experiments of 1665. Cf. above, p. 132.

141 Young [1810], p. 249.

142 Letter to Brewster of 13 April 1815, published in Young [1855], pp. 360–4.

co-operate, or destroy each other, accordingly as the difference of the times, occupied in their separate paths, is an even or an odd multiple of a certain half interval...[143]

The two 'portions' of light have now for some reason to be 'equal' (though admittedly only if they are of equal intensity is *complete* destruction possible). They also have to be in 'circumstances exactly similar'.[144] Young also states that

in reflections at the surface of a rarer medium, and of some metals, in all very oblique reflections, in diffractions, and in some extraordinary refractions, a half interval appears to be lost.[145]

Moreover,

it is said that according to some late observations of Mr. Arago, two portions of light, polarised in transverse directions, do not interfere with each other.[146]

By 1817, in fact, Young had been reduced to defending 'the general law of interference' for which he had found in 1803 'so simple and so demonstrative a proof' as nothing more than 'a temporary expedient for assisting the memory and the judgement' to be employed only 'hypothetically'.[147]

Thus at each stage Young modified his claims precisely so as to reconcile his principle of interference with known refutations, without providing the principle with extra content which would be exposed to possible refutation. Some modifications were actual logical weakenings;[148] others consisted essentially of weakening the previous version so that it no longer said anything about those cases in which it had been refuted and then adding to it *correct* descriptions of the previously refuting instances.[149] The various versions of Young's principle do then form a classic case of a 'degenerating problemshift'.

[143] Young [1817], p. 287.

[144] This seems to amount to a declaration by Young that he is no longer going to bother rewriting the law in the event of any further refutations, but will simply assert that the law was only apparently refuted because the circumstances of the two beams were not *exactly* similar.

[145] *Ibid.*

[146] It was of course 'said' correctly. As Fresnel was to explain, however, the beams do superpose. They fail to interfere only in the sense that no fringes are produced. (Not this time through lack of coherence, but because the disturbances in the two beams are in different planes.)

[147] Young [1810], p. 249. Young seems then to have been an instance of the psychological law (to which no doubt many counter-instances also exist) that degree of belief in a programme decreases as the programme degenerates. (This psychological law is not, of course, a consequence of the methodology of scientific research programmes.)

[148] This applies to the switch from the [1802*a*] version to the [1802*b*] version (ignoring the addition about coloured light). Such moves are, in the jargon, '*ad hoc*$_1$': see Lakatos [1970], p. 125, footnote 1.

[149] This applies to the switch from the [1807] to the [1817] version. Such moves produce theories whose content is *not* a subset of the content of their predecessor theory, but such extra content as they have consists merely of a substitution for the consequences of their predecessor known to be false of the corresponding 'correct' results yielded by experiment.

3(*c*) *Did the wave optics programme ever progress at Young's hands?*

But, given what we now know about the power and verisimilitude of the wave theory, it would be very surprising if Young's development of it produced no novel predictions whatsoever. I now turn to the question of whether the wave optics programme always degenerated at Young's hands.

According to the standards laid down by the methodology of scientific research programmes, the shift from one theory to the next constitutes scientific progress if the new theory not only explains the facts it was introduced to explain but makes extra predictions as well, some of which are empirically confirmed.[150] The positive weight such confirmed predictions lend to the programme is not at all diminished if some other programme eventually succeeds in explaining in an *ad hoc* way the facts originally predicted by its rival. Thus the fact that, as we saw earlier,[151] the corpuscularists could explain the two results, now usually alleged to have crucially refuted their theory, does not, according to this methodology, preclude the possibility of these results constituting vital positive evidence for the rival wave theory.

According to the methodology of research programmes, a given factual proposition 'supports' a programme (by showing that it is progressive) if

(i) it was predicted by the latest theory produced by the programme (in conjunction with appropriate and experimentally accepted initial conditions);

(ii) the factual proposition is accepted as empirically accurate according to available experimental techniques;

(iii) the factual proposition was not used in conjunction with some previous theory in the programme to *construct* the theory which entails it.

Did either of the two experimental results cited by Young's commentators as crucial contributions to the wave–corpuscle debate satisfy these three requirements?

There is in fact some doubt whether the results as usually described satisfy even the basic requirement (i) – i.e. there is some doubt whether they could be predicted on the basis of Young's theory. This doubt stems from some vagueness in Young's account of the basic propagation of light. In the case of the two slit experiment, for example, although Young's theory predicts what would happen *if* two nearly parallel beams of light affect the same portion of the observation screen, it is not clear whether the theory predicts that any portion of the observation screen *is* affected by two beams of light. Whether it does or not depends on the account Young's theory gives of how light is propagated after being admitted through a single slit or aperture. Let us look into this.

[150] See Lakatos [1970], p. 118. This formulation actually incorporates a modification of Lakatos's original proposal due to Zahar; see Zahar [1973]; reprinted below pp. 211–75.
[151] Above, pp. 127–30.

As I mentioned earlier,[152] one of the fundamental objections against the wave theory urged by Newton was its difficulty in explaining the rectilinear propagation of light. Newton wrote in 1672:

To me the fundamental supposition itself seems impossible, namely, that the waves or vibrations of any fluid can like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way into the quiescent medium where they are terminated by it.[153]

And in the *Opticks* he asked:

Are not all Hypotheses erroneous, in which Light is supposed to consist in Pression or Motion, propagated through a fluid Medium?...if it consisted in Pression or Motion...it would bend into the Shadow. For Pression or Motion cannot be propagated in a Fluid in right Lines, beyond an Obstacle which stops part of the Motion, but will bend and spread every way into the quiescent Medium which lies beyond the Obstacle.[154]

Huygens had tried to produce a wave theoretical explanation of rectilinear propagation,[155] but this was completely *ad hoc* (it was in fact completely circular) and involved assumptions about the ether which were mechanically totally unacceptable.[156] Newton justifiably ignored it. Young did almost nothing to improve the wave theory's plight in this respect.

Young shared the basic position of the corpuscularists on the propagation of light. Light was basically rectilinearly propagated, and any deviation from a rectilinear path had to be explained as an exception to the general rule. Thus, for example, when Young needed two 'portions' of light whose interference would explain the fringes outside the shadows of opaque objects, he plumped for one 'portion' propagated rectilinearly past the object and another 'portion' passing nearer the object and 'inflected' from its naturally rectilinear path.[157] In fact, Young's basic method in explaining interference and diffraction effects (this was also the heuristic principle followed by Fresnel in his very early work) was to forget about the wave character of light except where two light beams crossed. Light coming from two sources was treated as consisting of rectilinear rays (left theoretically uninterpreted, as it were) but then, when the 'rays' crossed, their wave character was remembered and thus their interference explained.[158] Nevertheless Young did in at least one place in his three

[152] Above, p. 131. [153] Newton [1672]; see the reprint in Cohen [1958], p. 121.

[154] Newton [1730], p. 362. [155] See Huygens [1690], pp. 19–21.

[156] This is a rather controversial assertion, for which I intend to argue elsewhere.

[157] See for example Young [1804*a*], p. 180.

[158] This heuristic principle has lived on as a pedagogical device. In most modern textbooks on optics, the chapter on interference precedes the chapter(s) on diffraction. In the interference chapter, light is considered as consisting of rectilinearly propagated rays up till the point where two or more rays interfere, when the ray suddenly becomes wavelike. The justification for this procedure then comes in the diffraction chapter where it is shown that, although all light is diffracted, in most circumstances the assumption that light is rectilinearly propagated is a good enough approximation. This justification was of course not provided by Young, but only later by Fresnel.

major early papers seem to be attempting a wave theoretical justification of this procedure, i.e. to be attempting a wave theoretical explanation of the rectilinear propagation of light. He argues there that for an undulation in an elastic medium to

continue its progress to any considerable distance, there must be in each part of it a tendency to preserve its own motion in a right line from the centre; for if the excess of force at any part were communicated to the neighbouring particles, there can be no reason why it should not very soon be equalized throughout, or, in other words, become wholly extinct, since the motions in contrary directions would naturally destroy each other.[159]

As far as I can understand this passage, the explanation it provides of rectilinear propagation is unacceptable (it is *ad hoc* in the main sense specified by the methodology of research programmes);[160] for it argues *back from* the 'fact' of rectilinear propagation to the (mechanically unrealistic) assumption that the particles of an elastic medium, when agitated, do not pass on some of their momentum to all particles in contact with them, but only (or in great part only) to those particles on the extension of the straight line joining the agitated particle to the original source of the disturbance.[161]

Notice that Young talks only of a 'tendency' for the light disturbance to maintain a rectilinear course. Young decided that a slight divergence from rectilinear propagation could be a quite useful explanatory instrument.[162] Thus he gives in his [1802*a*] paper the following account of what happens when light is admitted through an aperture. (This is in fact the only detailed statement on this part of the theory in his three major papers on light. It occurs as 'Proposition III' immediately after the passage just quoted.)

A Portion of a spherical Undulation, admitted through an Aperture into a quiescent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.[163]

There is no danger of this divergence ruling out a wave theoretic explanation of rectilinear propagation because the divergence is 'weak

[159] Young [1802*a*], p. 149.

[160] See particularly Zahar [1973], p. 102.

[161] Young in effect immediately admits that this is a mechanically (or 'mathematically') unsound assumption, but simply states that we can hardly expect any more, given the contemporary state of hydrodynamics: 'It may be difficult to show mathematically the mode in which this inequality of force is preserved, *but the inference from the matter of fact* appears to be unavoidable; and while the science of hydrodynamics is so imperfect that we cannot even solve the simple problem of the time required to empty a vessel by a given aperture, it cannot be expected that we should be able to account perfectly for so complicated a series of phenomena as those of elastic fluids.' (Young [1802*a*], pp. 149–50; emphasis supplied.) Perhaps in view of this Young would have been better not to insist so loudly that he had solved the problem of reconciling the wave theory and light's (apparent) rectilinear propagation.

[162] This divergence is, however, to be in addition to, and *not* a replacement for, the 'inflection' light undergoes in the neighbourhood of opaque bodies.

[163] Young [1802*a*], p. 151.

and irregular'. But how does this divergence operate? In the text to the above 'Proposition', Young uses the 'weak and irregular...diverging Undulations' to explain the apparent narrowing of the beam of light, and hence the apparent enlargement of the shadows of the two sides of the aperture, reported by Newton, following Grimaldi.[164] This is alleged to depend on the 'divergence...diminish[ing] the force' of the 'principal' (rectilinearly propagated) rays, particularly near the edges of the beam. The divergent disturbance does not create sensible light (Young says it does not 'add materially' to the 'force' of the 'dissipated light') and thus the beam is narrowed.[165] Young immediately admits, however, that 'in other circumstances the lateral divergence might appear to increase, instead of diminishing, the breadth of the beam'.[166] Thus in these 'other circumstances' – the diverging light *is* 'sensible', i.e. sensible light does get into the geometrical shadow. But there is not a word about what these other 'circumstances' are.

Young is therefore in the happy position of being able to account, without further effort, for *two quite different* possible results of the two slit experiment – *which the actual result will be, he can tell only by performing the experiment.*

(i) He can assume that the divergent disturbance produces no sensible light. In this case his theory predicts that the two slit pattern will consist of exactly two illuminated bands, adjoined and separated by unilluminated bands, one illuminated band being due to light emerging from one slit, the other illuminated band being due to light coming through the other slit. The light from both slits is propagated more or less rectilinearly, the two illuminated bands being in fact slightly narrower than they would be were light propagated in *strictly* rectilinear fashion. No point of the observation screen will be illuminated by sensible light from both slits and so no interference will occur.

(ii) Young can assume that the two slit arrangement constitutes one of those 'circumstances' in which light diverging into the geometrical shadow is sensible. In this case various points of the observation screen may be illuminated by light from both slits. Thus, according to the interference principle, a series of interference fringes may be produced.[167]

Thus Young's theory explains the existence of fringes in the two slit experiment only in an *ad hoc* way – Young reads off from the result of the experiment that this is an instance of case (ii). In other words, Young's theory does not predict the result of the two slit experiment. All Young can justifiably claim of the result is that it establishes which of his two kinds of exception to rectilinear propagation operates in these experimental circumstances.

[164] See above, p. 122, footnote 68. [165] Young [1802*a*], p. 152. [166] *Ibid.*

[167] I say 'may be produced' because, as we have seen (above pp. 138–42), Young very soon gave up the claim that *any two* overlapping beams interfere.

The case of the 1803 diffraction experiment is similar. Again it is a question of first observing fringes and then deciding which two 'portions' of light interfered to produce them, rather than of predicting the existence of fringes. For example, the well-known fringes outside the geometrical shadow of an opaque object are alleged by Young (after the event, of course) to be produced by the interference of two 'portions' of light. One of these is the 'portion' passing rectilinearly by the object. The other 'portion' Young had different conjectures about in different papers – he sometimes alleged it was 'inflected' by the object and sometimes that it was reflected from its sides. As for the fringes *inside* the shadow of a narrow object held in a diverging cone of light, neither of the two required portions of light could, in this case, be rectilinearly propagated. Both portions would, on Young's assumptions, somehow have to be bent. Young in fact assumes (and I stress again that this is only *after* observing the fringes) that this is one of those 'circumstances' in which the divergence from rectilinear propagation (on both sides of the object) produces 'sensible' light. It is these two 'divergent' portions which interfere to create the internal fringes.

It cannot then be the frequently cited qualitative features of either of Young's two famous experimental results which provide the 'novel facts' which the methodology of research programmes requires Young's theory to predict if it is to constitute progress. For not only were these features already known (as I stressed earlier), they were not predicted by Young's original theory and subsequent versions only dealt with them in an *ad hoc* way (i.e. in a way which violates requirement (iii), above, p. 143).

A further possibility remains open, however.[168] While I have shown that Young's theory did not characterise the circumstances in which two 'portions' of light would overlap so as to produce fringes, and hence that that theory did not predict the existence of fringes, the theory may still predict what happens *if there is overlap*.[169]

There is, of course, a quantitative aspect to Young's principle of interference. Two 'portions' of the 'same' homogeneous light (i.e. light from a single source and of a given wavelength, λ) will destructively interfere if their path difference is an odd number of half wavelengths. So, while Young must, in various circumstances, wait to observe the fringes before being able to say that interference is occurring, once he knows that there is interference, and once he has identified the two 'portions' of light which are interfering, he ought to be able to predict some of the quantitative features of the fringe pattern, such as the spacing between fringes. For example, the first dark fringe on either side of the central fringe ought to

[168] I should like to thank Paul Feyerabend who, in criticising an earlier draft of this paper, pointed out that I had omitted to consider this possibility.

[169] This would be very much in line with the heuristic procedure outlined above, p. 144, of considering beams of light as consisting of rectilinearly propagated rays until they cross, at which point they assume wave characteristics.

occur at points where the difference of the paths of the two 'portions' of light which produce it is $\frac{1}{2}\lambda$. Geometry will then give the distance between the centre of the pattern and the centre of the first dark fringe as a function of λ. Provided that Young had an independent method of arriving at values of λ for various kinds of homogeneous light, it seems that this fringe spacing forms a prediction of Young's theory.[170] It is, moreover, a prediction which, at least in the case of the two slit experiment, is not matched either by the corpuscular theory or by 'background knowledge'.

In fact Young did arrive at values of λ for the various spectral colours independently of these diffraction results through his reinterpretation of Newton's results on the colours of thin plates. [I should perhaps digress a little here and make a few remarks on Young's theory of thin plates, for this is sometimes considered one of Young's important achievements. Newton's theory of this phenomenon implied that, in thin plates of varying thickness, the first dark band occurs where the distance from the first surface of the plate to the second is just sufficient for the light particle to change from a fit of easy transmission (in which it must have been at the first surface in order to cross it) into a fit of easy reflection and back into a fit of easy transmission so that it is transmitted rather than reflected at the second surface. Thus no light is reflected back to the eye where the thickness of the plate is D, where D is the 'interval' between two successive fits of the same nature. (Similarly the first bright band occurs where the thickness of the plate is $D/2$.) Young, on the other hand, ascribed the dark bands to the destructive interference of the light reflected at the first surface and the light which was transmitted at the first surface, reflected at the second and then re-transmitted at the first.[171] This meant that for Young the first dark band on a plate of varying thickness occurs where the thickness is $\lambda/4$ – for then the second beam will have travelled a total distance of $\lambda/2$ further than the first beam, and beams originally in phase but with a path difference of $\lambda/2$ will destructively interfere. I would say that the scores which these two theories chalked up for their respective programmes were about equal. On the one hand, Newton had produced the *first* decent explanation of the phenomenon (and his theory can predict, for example, all the radii of the successive rings in the 'Newton's rings' experiment[172] having measured the radius of just one), whereas

[170] Alternatively, one could regard this as a prediction of the value of λ (which can then be checked against values arrived at by other means) on the basis of the measured distances between the fringes. (It is, in fact, very often the case that the values of the free parameters of a theory can be 'read off' from any one of a group of n experimental results and then the theory predicts the other $n-1$ results.)

[171] See e.g. his [1802*a*], pp. 160–2.

[172] This consists of allowing light to fall at (approximately) normal incidence on a plano-convex lens lying convex side down on a glass plate. This provides a plate (of air) of varying thickness. It produces a series of concentric coloured rings – the thickness of the air plate corresponding to each ring can then easily be calculated (it clearly depends on the radius of curvature of the lens).

Young simply re-interpreted Newton's results and theory (his values of the various wavelengths are obtained from Newton's values for the various intervals of fits by a linear transformation). On the other hand, Young was able successfully to resolve the major problem which this re-interpretation presented to him.[173] Each theory also faced one rather obvious major conceptual difficulty. On the one hand, it looked as if Newton would have to make the interval of fits dependent on the obliquity of the incident rays (a very unnatural assumption) to account for the fact that the radii of the rings increase with increasing obliquity. On the other hand, it was clear that Young's theory could not account for the (more or less perfect) blackness of the dark rings, for the two beams whose interference he alleged caused them were of *very* unequal intensities; and it was anyway clear that a full wave theory of the phenomenon would have to take account of many more beams than two (beams 3 times, 5 times, etc. internally reflected and then transmitted at the first surface), but it was not clear how taking these extra beams into account would affect the prediction.]

Returning to the main problem, Young could obtain values of λ for various kinds of light from the thin plates experiments. If Young did make quantitative predictions, based on these values of λ, about the fringe positions in his diffraction and interference experiments, and if he experimentally confirmed them, then he would, according to the standards of the methodology of research programmes, have provided the wave programme with new and dramatic confirmation. In view of the apparent fact that the corpuscular programme had no 'novel facts' of its own with which to match this, it would then seem that the methodology of research programmes, if not exactly leaving us with the same historical problems as before,[174] would not have led to much significant historical advance. For even when we analyse the intellectual situation in optics in the early

[173] This problem was that the centre of the ring system viewed by reflection was, according to Newton, black. But there the plates were assumed to be in contact and one would expect no path difference between the two (?) beams and therefore *constructive* interference. Young was forced to assume *ad hoc* that the phase of one of the beams was altered by half a wavelength (Young actually speaks of one of them *losing* half a wavelength) in being reflected. He assumed that this was the beam reflected at the first surface, which underwent a dense-to-rare reflection, and that in fact a half wavelength is 'lost' in all dense-to-rare reflections, but not in rare-to-dense ones. (There is obviously some conceptual difficulty here, for the two surfaces are supposed *in contact* and so it is not clear that it makes sense to speak of *two* reflected beams. Also, it is hard to know how Newton and Young could be so certain about what happened at the centre of the pattern. For even in the best experimental conditions the two plates will not really be in contact, there will always be dust particles or something similar interposed between them.) Anyway, Young based on this explanation the conjecture that if the lens and the plate were of different optical densities, and if a substance of intermediate density were interposed, both reflections would be of the same nature and so there would be no loss of half a wavelength, and the centre of the pattern would thus be white. Young claimed to have confirmed this prediction.

[174] As I shall show later, the corpuscular programme had certain advantages (in terms of heuristic strength) over the wave programme. See also p. 152, footnote 181 below.

1800s in its terms, Young's contemporaries would still seem to be displaying an embarrassing lack of interest in his achievements.

I shall now reconsider Young's two famous experiments in an attempt to decide whether Young's theory did indeed make such novel predictions, and whether Young experimentally confirmed them.

First, a methodological preliminary. It is never difficult to dress up what are in fact *ad hoc* manoeuvres as genuine predictions of a theory. Indeed, one cannot tell merely from looking at the deductive structure of a test of a theory, whether or not one is dealing with a genuine prediction. For the scientist has only to disguise (or omit to mention) the fact that some initial condition or 'auxiliary hypothesis' assumed in the test, rather than having been independently ascertained, was 'read off' from the experimental result. In such cases the initial condition or auxiliary hypothesis concerned is arrived at, not on the basis of some *independent* experimental technique, but by first looking at the experimental result and then working backwards to find some assumption which when fed into the theory gives the already known result. This is a consistency proof rather than a prediction![175]

Let me give a very simple example. Suppose our theory is that all *A*s are *B*s. If we have an independent method of deciding whether some given object x is an A, then we can use the theory to *predict* that x is a B, which prediction we can then check against experiment. It is possible, however, that our only evidence for x being an A was the *observed* fact that x is a B (together with our theory that all *A*s are *B*s). In that case, although we could dress up the situation in predictive form 'All *A*s are *B*s; x is an A, therefore x is a B', we intuitively would not count the theory as having made a genuine prediction.

A second, slightly less simple example is, I claim, provided by Young's procedure in the case of his 1803 diffraction experiment. Young could have predicted the spacing of the fringes in this experiment had he had an independent method of identifying the 'portions' of light which do the interfering at the various points of the observation screen. But Young had no such method; rather he *worked back* from the observed fringe spacings (and the known value of the wavelength) to an identification of the 'portions' of light whose interference produced the fringes and the paths these 'portions' took. In other words Young was *not* in a position to say: 'These fringes are caused by the interference of these two portions of light; at this point the path difference of the two portions is an odd multiple of $\lambda/2$; therefore a dark fringe must be observed at precisely this point.' His method is rather: 'Here is a dark fringe. It must have been caused by the interference of two portions of light which have crossed

[175] That these are the important kinds of 'predictions' which ought not to count as giving genuine support to a theory was originally pointed out by Zahar in his [1973].

paths which differ by an odd multiple of $\lambda/2$. Which two portions and which paths might they be?'

It is clear that this *was* Young's method of proceeding, for he writes, in his account of this 1803 experiment:

If we now proceed to examine the dimensions of the fringes under different circumstances, we may calculate the differences of the lengths of the paths described by the portions of light...concerned in producing these fringes; ...[176]

Had Young been able independently to identify the sources and paths of the two interfering 'portions', then the path differences for various points on the observation screen would, of course, have been given simply by geometry. It seems, then, that the quantitative aspects of this 1803 diffraction experiment cannot provide the required 'novel fact'. We are left then with the two slit experiment.

Here there is a 'natural' conjecture about the sources and paths of the two beams interfering at any point. This conjecture is that the sources are the centres of the two slits and the paths are straight lines from the sources to the point of interference. There seems little doubt that Young does make this conjecture, for he writes that the two apertures 'may be considered as centres of divergence, from whence the light is diffracted in every direction'.[177] This 'natural' conjecture together with the interference principle yields, as we now know, predictions about the fringe spacing in the two slit experiment which are correct, at least as first approximations.[178]

Now had Young used this conjecture together with his interference principle to predict the quantitative features of the two slit result, and had he confirmed this prediction, it would still seem a debatable issue whether this prediction should count as a success for Young's fully fledged wave theory. For however 'natural' the conjecture about the paths of light may be, it is inconsistent with Young's wave theory. As we have just seen,[179] what Young says happens when light passes through a single aperture is that it is propagated essentially in a beam terminated by straight lines, but that there is a 'divergence' especially near the edges of the beam. Given that the two slit case is one in which interference fringes are produced, it follows from Young's theory that this is a 'circumstance' in which this 'divergence' produces sensible light. But this 'divergence' emanates from at least every point on the edge of the beam – presumably in a variety of directions. This means that the total light arriving at any

[176] Young [1804*a*], p. 181.

[177] Young [1807], p. 464.

[178] This differentiates the two slit case from that of the 1803 experiment where the analogous 'natural' conjecture – that the sources of the two 'portions' which are to interfere to produce the inner fringes are the two sides of the diffracting object – leads to predictions which are rather far from being correct. Fresnel later explained the narrow object diffraction pattern as caused by the 'interference' not just of waves emanating from the edges of the object but from all unobstructed parts of the original wave front.

[179] Above, p. 145.

point of the observation screen is a rather complicated compound of light rays which have taken a variety of paths in arriving there.

This raises rather a tricky methodological problem. Let us assume that the methodology of research programmes is correct in claiming that it is a theory's dramatic unexpected predictive successes which constitute the vital evidence in its favour. Are such predictions still to count as important successes for a theory when the derivation of these predictions from the theory requires auxiliary assumptions which are *inconsistent with other parts of the theory*? Fortunately we can decide whether or not Young demonstrated the superiority of the wave theory without answering this question. For there are, I claim, sufficiently many suspicious aspects of Young's account of the two slit case to support the belief that he never performed the experiment!

It is now known that, together with some simplifying assumptions, the interference principle yields, by some very elementary considerations, the prediction that the two slit interference pattern will consist of a series of equally spaced fringes – the centre of one bright fringe being a distance $\lambda D/d$ from the centre of the next bright fringe (where D is the distance of the screen with the two slits from the observation screen, d is the distance between the two slits and λ is, as usual, the wavelength of the light involved). This is more or less what is observed, at least near the centre of the pattern.

Now Young himself never makes any attempt to *derive* any prediction about the two slit case, but he does produce, out of the blue, some predictions which conform to the above approximations. He writes that the light on the observation screen is 'divided by dark stripes into portions nearly equal' and that the fringes become wider 'as the surface is more remote from the apertures (i.e. as D increases) and 'wider in the same proportion as the apertures are closer to each other'[180] (i.e. as d decreases). Assuming, as we are, that Young could independently support all the required premisses,[181] these predictions would, if confirmed, be good

[180] Young [1807], p. 464.

[181] If we further assume for a moment that Young did derive and confirm explicit predictions about the fringe spacing, then he would have shown the wave programme to be progressive. In that case we might again have to resort to 'external' considerations to explain Young's theory's lack of success; but the methodological appraisal in which we have indulged still would not have been historiographically pointless. For it would have shifted and more closely characterised the problem, which would now be the very specific one of why Young's fellow scientists failed to spot the importance of these *particular* aspects of this *particular* experimental result. Funnily enough this problem (which I am about to show does not in fact arise) has a rather plausible solution: Young's fellow scientists may not have known of the result. Unlike his 1803 diffraction experiment which was recorded in his [1804*a*] paper, published in the *Philosophical Transactions* and originally given as a Bakerian lecture, the account of the two slit experiment is rather hidden away in Young's [1807] *Lectures* delivered to an 'audience of fashionable ladies' (Brougham's phrase) in 1802–6. Although his [1807] book did eventually achieve a certain popularity, it seems hardly to have been read at the time. Furthermore, it was deliberately aimed at a popular audience (see Wood [1954], chapter vi) and so was perhaps unlikely to be read

enough to show the empirical superiority of the wave theory according to norms supplied by the methodology of scientific research programmes. However, as I said, there is evidence that Young never performed the experiment.

The first piece of evidence is that Young never explicitly claims to have performed the experiment. His usual practice is to head his accounts of experiments with the word '*Experiment*' or '*Observation*' and to give a full account of how he carried out the experiment.[182] He never describes the two slit case as an experiment, however, referring to it as merely 'the simplest case' of interference.[183]

The most worrying feature of Young's account of this 'simplest case' of interference is that he describes it as involving 'a screen in which there are *two very small holes or slits*...'[184] Assuming, as seems reasonable, and as do those few commentators who remark on this fact, that Young means by holes, round pinholes, then great doubt is thrown on whether Young ever performed the experiment. For the result he describes (dark and light *stripes*) is by no means obtained if the two apertures in the screen are round pinholes.[185] Furthermore it is more difficult to observe any interference effects when circular apertures are used.[186]

But perhaps still more worrying than this detail which Young includes are the many details he leaves out. First of all, and again contrary to his usual practice, he gives no numerical details whatsoever either of the experimental set-up or of the result. Also, he talks simply of allowing a 'beam of homogeneous light' to fall on the screen with the 'two slits or holes' without remarking at all on the source of this beam or the distance of the source from the double slit. Neither does he give any indication of

by scientists. Young himself never stressed the importance of the result: he never returned to it in his later writings (as he did to many of his other results) and even in his *Lectures* it is not given any emphasis. (This would be explained if my conjecture below about his performance of the experiment is correct.) None of his immediate commentators, Pettigrew, Whewell and Peacock, as much as mention the two slit experiment.

[182] See for example his [1804*a*], p. 179, where he heads the account of an experiment '*Exper. 1*' and begins his account 'I made a small hole in a window shutter, and covered it with a piece of paper which I perforated with a fine needle...'.

[183] Young [1807], p. 464.

[184] *Ibid.*

[185] Rather, what one gets is interference between the two circular diffraction patterns centred on the so called 'Airy disks' produced by each aperture separately. (This assumes that the original source of light was also circular.)

[186] Mach, in his [1926], pp. 147–8, pointed out the fact that the visibility of the interference effects is greatly enhanced when slits are used. He failed to see any difficulty in Young's account, however, by curiously misreading it. Mach refers to the account in Young's [1807] but somehow omitted to read there the 'or slits'. He in fact attributes the discovery of the two slit experiment to Fresnel. Mach writes: 'Young...illuminated two small apertures in close proximity by light from a single opening in a window shutter [in fact Young does *not* mention this window shutter]...Fresnel however replaced the two round apertures by two narrow vertical slits, which effected a great improvement.' Young had managed thoroughly to confuse Mach, by the way, for Young states (*ibid.*) that he is considering the case of a 'beam of homogeneous light' which he could hardly have obtained directly through a hole in a window shutter!

the separation and dimensions of the 'holes or slits', nor of the distance of the double slit from the observation screen.

Yet had he performed the experiment (*and still more had he wanted others to be able to repeat it*) he could reasonably be expected to have mentioned each of these factors. For instance, had he successfully performed it he must surely have remarked that for the experiment to succeed the original source of light has to be very small and its distance from the double slit very large. (We know in fact that if the difference of the paths from the two edges of the source to either of the slits is greater than about $\lambda/4$ the fringes will not in general be observable![187]) The width of the slits and the relation between this width and the width of the screen separating the two slits are also not specified by Young and yet are vital to the successful performance of the experiment. Only if the slits are very narrow and the distance between them very small will fringes be observable. Moreover, there are opposing tendencies here – as the slit separation increases, the *number* of dark fringes increases, but the *sharpness* of the fringes decreases.[188]

Furthermore, neither Young's theory of light nor the analogy between light and water waves, which Young always had very much in mind, gave him any reason to suspect that any of the features I just mentioned are important for the successful performance of the experiment. For example, as we saw earlier,[189] Young had no clear ideas on what was later called coherence which (had he had them) would have both pointed to, and explained the necessity for, a small source at a great distance from the double slit. Also his theory gave him no reason to suspect that narrowing the slits would lead to a greater 'spreading' of light from each of the slits and hence to greater definition of the fringes, for, according to Young's single slit theory, 'weak and irregular' undulations diverge from the sides of *any* beam.[190]

As for the interference of water waves, Young, we know, gave 'ripple tank' demonstrations of the pattern produced when a train of water waves meets a barrier in which there are two apertures. He always had water waves very much in mind when considering light, and he in fact prefaces his account of the optical two slit case with an account of the analogous water wave case. But, with water waves, interference is observable no

[187] This is explained in the 'classical' wave theory as due to the fact that each point of the slit source produces its own interference pattern. These patterns are displaced slightly relative to each other and hence their superposition may mask the individual interference effects.

[188] To give an example, if the double slit is separated from the observation screen by 70 cm, interference fringes will not normally be visible if the slits are separated by as much as ½mm. Without the aid of lenses the two slit interference effects are never very sharp since where the overlap occurs the light supplied by each slit separately is not very great. *Remember also that Young was observing the effects on a screen, a method which renders the effects much less easily observable than the later method (invented by Fresnel) of observing the fringes directly with the aid of an eye-glass.*

[189] See above, p. 140.

[190] See above, pp. 145–6.

matter what the form of the original disturbance or its distance from the double slit, and no matter what (within wide limits) the distance between the two slits.

Thus, there were no theoretical considerations which would have directed Young to the successful performance of this experiment. Moreover, the dearth of details in Young's account make it seem unlikely that Young ever did successfully perform it, and certain that he did not give sufficient information about the conditions of the experiment to ensure its repeatability by others.[191] My claim is supported by the otherwise rather strange (and certainly unargued) assertion by Houston in his text book on optics that 'the real obstacle [to the acceptance by Young's contemporaries of the crucial nature of the two slit experiment] was the difficulty of repeating the experiment'.[192]

Houston's remark suggests that other scientists actually attempted to 'repeat' the experiment and failed. Unfortunately (for it would obviously strengthen my case) I have not been able to find any evidence for this.[193] It is clear, however, that people were confused about Young's claims. Certainly anyone attempting to repeat it (or rather 'repeat' it, since it is likely Young never performed the experiment) following the account by Arago, probably the most famous optical scientist of the time, would scarcely have succeeded. For Arago writes that Young's experiment involves introducing 'sunlight into a dark room through two little holes not very far apart'.[194] But, of course, no interference fringes would be produced in these circumstances because of the lack of a constant phase relation between disturbances emanating from the two holes. For the experiment to stand any chance of success the light must be introduced first through a single slit (or small hole) before impinging on the double slit.

If my claim that Young never successfully performed the two slit experiment is correct, then it explains Young's reticence about the experiment. He never gave the two slit 'case' any prominence, hiding it away in his *Lectures* of 1807 and never returning to it in later works (unlike many of

[191] In a very similar context, Arago, in discussing Grimaldi's claim to have observed interference effects, makes it clear that we ought not to be at all surprised if scientists are not convinced that some experimental discovery has been made, if the result is not very clear and if the conditions for repeating the experiment are not given. He writes (in his [1859], p. 420): 'Grimaldi had long ago (before 1665) formed some notion of the action which one beam of light may exercise upon another: but in the experiment which he cites the action was but obscurely manifested; and besides this, the *conditions* which were essential to its production had not been pointed out, and thus no other experimenter followed up the inquiry.' What I am suggesting is that precisely similar considerations apply to Young and his two slit experiment.

[192] Houston [1938], p. 135.

[193] It seems unlikely that I shall find any such evidence. For only in the relatively short period between 1807 and the 1820s is an account of such a failure to obtain interference fringes likely to have been published. For after the acceptance of Fresnel's work, it was 'known' that interference fringes were the 'correct' result.

[194] Arago [1819]. The passage occurs on pp. 232–3 of the reprint in Fresnel's *Oeuvres Complètes*.

his other experiments, including the 1803 diffraction experiment). Indeed, it should be noted that, although it is probably the two slit experiment for which Young is now most famous, Young himself made so little fuss of it that none of his early commentators, Whewell, Peacock or Pettigrew, give it the slightest prominence.

4 *Was Young's work really ignored? The heuristic superiority of the corpuscular programme* circa *1810*

I think I have shown then that, when the intellectual background to the reception of Young's work is appraised using the criteria supplied by the methodology of research programmes, the reactions of Young's fellow scientists do not seem so strange. First of all, by this appraisal, instant rationality is exposed for the myth it is[195] – the corpuscular programme was certainly not instantly and once and for all made untenable by Young's experimental results (though the programme had so far failed to produce anything but *ad hoc* explanations of these and other, already well known problematical results). Secondly, the wave programme also faced some experimental difficulties which it had so far failed to resolve except in an *ad hoc* way.

Moreover, the reception of Young's work appears even less strange in view of the fact that from the point of view of heuristic strength (or perhaps heuristic definiteness) it was the corpuscular programme which seemed the stronger at the time Young wrote. It is this fact that I intend to establish next.

It is clear from what has been said that (as with 'the' wave theory) there was no such thing as the corpuscular theory of light, there was rather a series of such theories. Each of them implied that light consists of corpuscles emitted from luminous objects. The various corpuscular theories of light were related by a heuristic principle which was supplied by particle mechanics. A fundamental assumption behind all corpuscular theories was that the corpuscles of light obey the ordinary (*and already known*) laws of particle mechanics. This assumption played a wider programmatic role in addition to its descriptive one. For it both specified the problems the corpuscular programme faced and guided their solution. Thus, given that the particles of light are to obey the same laws as ordinary material particles, it follows from Newton's first law that any deviation from rectilinear propagation on the part of the light corpuscles was caused by some net external force (or forces) acting upon the light.[196] The fact

[195] See Lakatos [1970], pp. 154ff.

[196] The corpuscular programme thus seems to be one of those interesting cases (for other alleged examples, see Urbach [1974]) in which the heuristic is more important in characterising the programme than the 'hard core'. The natural 'hard core' assumption of the corpuscular programme would be that light consists of bits of *matter* thrown out by light sources. Yet many of the protagonists of the programme would have been

that the corpuscles producing different colours are refracted by different amounts meant, given the basic corpuscular assumptions, and given that only one set of forces could presumably be operative at the surface of a refracting body, that the corpuscles had to have distinguishing features which explained their different refractions.[197]

Moreover, the phenomenon of partial reflection showed that even particles incident on a transparent body at the same angle and velocity are acted upon differently by the same set of forces, some particles being refracted (by the attractive forces) and some reflected (by the repulsive forces). Newton explained partial reflection using his famous theory of fits of easy reflection and easy transmission, which he had previously used to explain the phenomenon of 'Newton's rings'.[198] Biot in turn explained these 'fits' as due to the light particles having repulsive and attractive 'poles', the particles being in uniform rotation (hence one could talk of a particle's 'phase').[199] The polarity of the corpuscles could also be used to explain polarisation effects.

Thus, by the early years of the nineteenth century, the corpuscularists had built up for themselves a quite complicated conceptual apparatus. Once, however, the relevant parameters had been assigned to the particles and the relevant forces to the opaque or transparent bodies, the path of any given ray of light was to be completely specified by known and simple particle mechanics.[200]

sceptical about this assumption. But Newton (and later corpuscularists) applied particle mechanics to optical phenomena (Newton did this, for example, for refraction). But, if mechanics is to apply, the corpuscles must have mass. Thus one can pursue a programme without believing in its hard core.

[197] The 'natural' candidate for this job was *mass*. Newton seems, however, to have been aware of the difficulties to which assigning the particles different masses leads. The principal difficulty is that, given that *in vacuo* the velocity of any kind of light is the same, the force which launches the light particles from a luminous source would have to be different according to the particle's mass: see Bechler [1974]. Bechler does not mention that this implausible assumption about a differential emissive force is made more plausible by Laplace's suggestion that lots of particles are thrown off by a luminous body at *different velocities*, but that our senses register only those having velocities within a certain range (cf. above, p. 128). Thus there may be several emissive forces each acting constantly on the particles, *no matter what their mass*, but only those particles to which a given force gives velocities within this range will be registered by the eye as light. Which kind of particles these are will differ with different emissive forces. Whatever may be the case, as late as 1827 Sir John Herschel records that it is part of the corpuscular theory that the 'particles differ from each other...in their actual masses, or inertia' (Herschel [1827], p. 439). Many attempts were made to detect light's mass (e.g. by Michell and by Bennett). Quite sophisticated attempts to work out the consequences of light's possessing inertia were also made by various corpuscularists (e.g. by Soldner who was concerned with the effect on light of the gravity of highly massive bodies, and by Laplace who worked out the conditions under which 'black holes' might exist). See also footnote 201.

[198] For the theory of fits see, e.g., Newton [1730], pp. 278–88.

[199] See Biot [1816], vols. 3 and 4.

[200] There is no doubt that it was Newton who almost singlehandedly invented the corpuscular programme, despite the fact that, as is well known, he deliberately refrained in the *Opticks* from explicitly asserting that light consists of corpuscles. It was Newton who

The problem with the corpuscular programme was that it lagged behind the facts: none of the modifications the corpuscularists made to their assumptions led to new predictions which were subsequently empirically confirmed. Each new diffraction effect, for example, far from being predicted by corpuscularists, required a rearrangement of the light-bending forces they assigned to matter. This, however, does not detract from the fact that, faced with any experimental result, the corpuscularist had a clear idea of how to go about explaining it (even, and indeed especially, if the result was anomalous). The presence or absence of light in any particular region, for instance, was to be explained as the result either of direct (rectilinear) propagation, or of some net force emanating from material bodies and acting on the light corpuscles.[201]

As I remarked earlier,[202] there was also, in 1810, such a thing as the wave optics programme. As in the case of the corpuscular programme, the wave programme's aim – the explanation of all optical phenomena in terms of the mechanics of an all-pervading elastic medium – also provided its heuristic. *Because of the comparatively less developed state of the mechanics of elastic media relative to the mechanics of rigid particles, the heuristic of the corpuscular programme was, however, rather more definite than that of the wave programme.*[203]

suggested that reflection, refraction and 'inflection' are all referrable to one set of forces emanating from matter and acting on light. Proposition IX of Book II, Part III of his [1730], for example, reads: '*Bodies reflect and refract light by one and the same power, variously exercised in various circumstances.*' (Notice, by the way, that this is a '*Proposition*' not a '*Query*'.) As mentioned above, Newton regarded '*inflection*' as but a special kind of refraction (this was also Hooke's position). It was also at Newton's hands that the corpuscular programme achieved one of its major heuristic successes, cf. below p. 159.

[201] See above, p. 156. Professor Erwin Hiebert, having heard a shortened version of this paper at the Nafplion conference, argued that I had not established the existence of a serious corpuscular optics research programme after Newton. Now a programme once created may exist (in Frege's and Popper's 'World 3' – see Popper [1972]) without anyone at all working on it. But in fact the corpuscular programme made many appearances in 'World 2' (the world of human consciousness) in the period between Newton and Young, as is evinced by its being quoted by a variety of authors. Let me give two examples. The Scots physicist John Robison writes that Newton's theory is '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...' (Robison [1788], pp. 96–7). The programme is even more fully articulated in Herschel [1827] – the first optics textbook. Moreover, the scientists who worked on this programme in this period cannot all be regarded as lightweights. They include: Boscovich, the Scottish physicists Robison, Wilson and Playfair (they were particularly concerned with the optics of moving media), Michell and Bennett (they were particularly concerned with testing for light's inertia), Soldner (he was concerned with the question of massive bodies bending the rays of light-particles), Malus (who with Berthollet investigated, as many other scientists had done, the effect on the fringe patterns of varying the form and substance of the diffracting objects), Laplace, Biot and Brewster.

[202] See above, p. 136–8.

[203] This was admitted by Fresnel who agreed that the 'system of undulations' was 'more difficult to follow in its mechanical consequences than the emission hypothesis' (Fresnel [1822], p. 1; my translation). Lloyd in 1833 also admits that the corpuscular programme had an advantage since: 'The nature and laws of projectile movement are

Neither the corpuscular nor the wave programme had had anything in the way of *predictive empirical success* up to this time: both programmes were in the business of *post hoc* explanations. However, as regards giving explanations of optical phenomena which (whether or not *post hoc*) were in line with the heuristic of the programme, there is no doubt that the corpuscular programme was way ahead of its rival.

The first great heuristic boost to the corpuscular programme had been given by Newton in the *Principia*. He had there demonstrated that, if a moving particle were subject to no net external force, except during its passage through a narrow region bounded by two parallel planes, in which region the force on it satisfied certain conditions,[204] then (*a*) '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',[205] (*b*) the incident velocity of the particle will be to its emergent velocity as the sine of the angle of emergence from the 'active region' is to the sine of incidence,[206] and (*c*) if the particle is slower on emergence, then there is a critical angle at which it will be reflected from the 'active region' at an angle equal to the angle of incidence.[207]

Although Newton is at pains here to insist that he is 'not at all considering the nature of the rays of light, or inquiring whether they are bodies or not',[208] but rather merely remarking on 'the analogy there is between the propagation of the rays of light and the motion of bodies',[209] it is clear that both he and later scientists were very impressed by the fact that, as he had demonstrated, if a light ray *were* a stream of particles subject to no external force except in the neighbourhood of the interface between two different media, then the laws of mechanics imply that light would be refracted according to the known laws.

In fact this result of Newton's was the most quoted result in optics in the

far more familiar to every lover of mechanical philosophy than those of vibratory propagation; and the triumphant career of the former branch of this science, in its application to the movements of the heavenly bodies, is in itself sufficient to induce every one to lean to a theory which proposes to account for the phenomena of light on similar principles.' (Lloyd [1833], p. 296.) Some important results in continuum mechanics *were* available, thanks largely to Newton (see above p. 137) and Euler. (The wave programme is rather exceptional in that its heuristic shifted in the course of its development. The attempt to explain optical phenomena in terms of the mechanics of elastic *fluids* was, as is well known, replaced by the attempt to explain optical phenomena in terms of the mechanics of elastic *solids*.)

[204] These conditions are that the force be perpendicular to the two bounding planes and that the force is a function only of the distance from either of the planes.

[205] Newton [1729], p. 226. This is in fact the content of Proposition XCIV, Theorem XLVII of Section XIV.

[206] *Loc. cit.*, p. 228. This is the content of Proposition XCV, Theorem XLIX.

[207] *Ibid.* This is the content of Proposition XCVI, Theorem L.

[208] *Loc. cit.*, pp. 230–1. He also writes here: 'These attractions bear a great resemblance to the reflections and refractions of light...' (*loc. cit.*, p. 229).

[209] *Loc. cit.*, p. 230. Newton's *beliefs* are unimportant as far as methodological appraisal is concerned. The important fact is that his heuristic reflects the assumption that light consists of particles. Cf. above p. 156, footnote 196.

period from the *Principia* up to 1810. Thus, for example, Biot and Arago in a paper written prior to the latter's 'conversion' to the wave theory wrote: 'Newton *proved* that [the] change of direction [in refraction] was owing to an attraction which bodies exercise upon the elements of light...'[210] And Prévost 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 (Newton. Princip. *1.* 1 *prop.* 94)'.[211]

A second example of a development within the corpuscular programme which was an advance from the heuristic point of view is Laplace's demonstration that Huygens's law for the position of the refracted rays in double refraction is reducible to the principle of least action. Laplace himself was clearly aware wherein the significance of this result lay, for he frequently remarks that the most important aspect of the result is that the principle of least action is one of the general principles of the action of 'intermolecular attractive and repulsive forces'.[212]

On the other hand, up until 1810, the protagonists of the wave programme had, at every sign of empirical difficulties, perverted their mechanical assumptions in the interests of explanatory expediency. Thus, for example, Huygens pointed out that it is a consequence of mechanics that each disturbed particle in an elastic medium may be regarded as a source of the disturbances beyond it.[213] But then faced with the 'fact' of rectilinear propagation, Huygens spoiled matters by assuming that each 'secondary' disturbance 'is infinitely feeble'.[214] As we have seen, Young hardly improved things in this respect. Indeed the story of the various modifications he made to the principle of interference is the story of how

[210] Biot and Arago [1806]; emphasis supplied.

[211] Prévost [1798].

[212] See e.g. Laplace [1813] pp. 320–2. Laplace was clear about the analogy between his demonstration and Newton's. Laplace writes (e.g. in his [1809]) that he had decided 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 or repulsive forces, of which the effects are sensible only at insensible distances...'. This shows that Young was missing the point in his [1809] review of Laplace's paper when he complains that Laplace overestimated his achievement. Young points out that it was known that Huygens's theory assumed Fermat's principles of least time and that this is the same as Maupertuis's principle of least action 'supposing only the proportion of the velocities [in the two media, in this case a vacuum or air and the birefringent crystal], inverted'. But, Young goes on to point out, it was also well known that the proportion assigned to these two velocities by the corpuscular theory ($v_r = nv_i$ where v_r is the velocity of the refracted ray, v_i the velocity of the incident ray and n is the index of refraction) *is* inverted by the wave theory (which gives $v_r = v_i/n$). Young was obviously correct in all this, but the point he misses is that, while the principle of least time is assumed by Huygens, it was by no means clear that it is a fundamental law of disturbances in elastic media, whereas the principle of least action *was* known to be a fundamental law of particle mechanics. Laplace himself concluded that his demonstration 'leaves no doubt' that double refraction is due to such attractive and repulsive forces. What he ought, more modestly, to have concluded is that he had shown the heuristic power of the corpuscular programme.

[213] Huygens [1690], p. 19. [214] *Ibid.*

he allowed empirical difficulties to bully him into describing optical disturbances as increasingly different from disturbances in an elastic medium obeying the ordinary laws of mechanics.

Comparing the achievements of the two programmes in, say, 1810, it thus seems that there was nothing to choose between them in terms of empirical progress, but that, as regards heuristic power, it was the corpuscular programme which had so far shown itself the better. This situation did change, but only at the hands of Fresnel.

The analysis provided by the methodology of research programmes thus yields, I claim, a completely 'internal' explanation of the reception of Young's work. Despite the impression most historians try to give, Young's work was received with some interest, but it did not 'persuade scientists of the truth' of the wave theory, as Whewell, for example, said it ought to have done.[215] This, if my analysis is correct, is hardly surprising, for although there is no denying that Young had some intelligent and suggestive ideas, his work neither established the truth of the wave theory nor its superiority over its rival. Thus there is, on this account, no need to invoke external factors like 'Newton worship' to explain Young's alleged neglect. What I should now like to do is to consider how far this should be regarded as a success for the methodology of scientific research programmes. This will also involve looking at the general lessons about the interaction of philosophy of science and history of science which are illustrated by my case study.

5 *The interaction between history of science and philosophy of science*

I shall show that there is an acceptable, general method by which methodologies may be tested against the history of science and shall then apply this method to see if my solution of the problem of Young's neglect counts as a confirmation of the methodology of scientific research programmes.

If there is a general way in which methodologies may be tested against history, then, since there is a well-known logical gap between normative and descriptive statements, there must be some principle which bridges this gap, and which methodologists are prepared to (and ought to) accept. I shall claim that there is such a principle. But the character of this principle depends on which aspects of the development of science methodologists are entitled to pass normative judgements; and so it is to this question that I turn first.

Methodologies, or philosophies of science, ought to provide, amongst other things, general criteria for appraising scientific theories.[216] Such

[215] Cf. above, p. 109, footnote 5.

[216] I shall use the term 'theory' to denote the unit of scientific discovery specified by a methodology, whether or not this is a theory in the intuitive sense.

a criterion will provide an ordering of the competing theories available at any time.[217] Assume that two theories, *A* and *B*, were rivals at some stage in the development of science, and that some methodology unequivocally pronounces theory *A* better than theory *B*.[218] This methodological pronouncement presumably has consequences about *some* of the decisions and actions of those scientists who were actually confronted with the two theories. But which decisions and actions?

First, the methodology certainly tells such a scientist that he ought to accept *A* as currently a better scientific theory than *B*, since, according to the methodology, *A is* the better theory.

Suppose that some of these scientists had previously been trying to develop theory *B*: does the methodology tell them that they ought to stop doing so and work instead on theory *A*?

Many methodologists assume that the answer to this question is 'yes'. Or at least, they seem to think that the methodology's judgement that *A* is better than *B* should carry with it *some* consequences about which theory a scientist should work on – perhaps the consequence that if working on *B* does not improve it relative to *A*, within a certain time period, then the scientist ought to switch to working on *A*. For example, Feyerabend argues that the standards provided by the methodology of research programmes are empty unless some time period is specified such that, if a programme has consistently degenerated throughout that period, then the methodology says that the programme should be abandoned, and that further work on it is 'irrational'.[219] Indeed, Feyerabend claims that if a methodology does not advise scientists on which theory to work, then its standards are mere 'verbal ornament[s]', 'a memorial to happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple and "rational" rules'.[220]

[217] This ordering need not be a total ordering. The criterion of scientific merit provided by some methodology may, for example, have two parts, and there may be theories *A* and *B* such that *A* is better than *B* according to the first part, but *B* better than *A* according to the second part. Unless there is still a third part to the criterion for resolving such conflicts, *A* and *B* would then be incomparable according to this methodology.

[218] I am assuming throughout that the methodologies I deal with aim to provide *the* criterion of scientific merit; so that if *A* comes out better than *B* on this criterion, it would follow simply that *A* is a better scientific theory than *B*. (Some methodologists, e.g. some inductive logicians, make the more modest claim that the criterion they provide is only one possible way of judging theories, but that there may be others, equally good, which rank theories differently. This modesty is usually discovered only after the methodology concerned has run into difficulties.)

[219] Feyerabend writes: 'it is easy to see that [the] standards [of the methodology of scientific research programmes]...have practical force only if they are combined with a *time limit* (what looks like a degenerating problem shift may be the beginning of a much longer period of advance). But introduce the time limit and the argument against naive falsificationism reappears with only a minor modification (if you are permitted to wait, why not wait a little longer?) Thus the standards which Lakatos wants to defend are either *vacuous*...or they can be *criticized* on grounds very similar to those which led to them in the first place.' Feyerabend [1970], p. 215.

[220] *Ibid.*

But why should a methodology advise a scientist always to work on (i.e. to try to develop) only the theory it characterises as presently the best available? Say that A is better, according to some methodology, than B. It is possible that a scientist developing the ideas behind (the inferior) theory B will produce a theory C, which, *according to the same methodology*, is even better than A. Indeed most major innovations in science consist precisely of some great scientist developing an idea which had so far led to inferior scientific theories, and producing out of it a superior scientific theory. For example (and reverting to the terminology of the methodology of research programmes), Fresnel adopted the wave programme which before him had degenerated and was (at least in some senses) inferior to the corpuscular programme. He made the wave programme progress, and eventually made it much superior to its corpuscle-based rival. Hence, assuming that we do not want methodologies to yield advice which if followed would, according to its own canons, retard the development of science, it ought not to advise scientists to work on only the best available theory.[221]

There seems to be no difference on this score between Popperianism and the methodology of research programmes (which thus is not, as both Feyerabend and Kuhn imply, an intellectual retreat in this respect from the austere Popperian standards). Why should one not try to develop a refuted or even irrefutable theory in competition with a refutable and corroborated theory? Indeed, Popper says in many places that many scientific breakthroughs have been achieved through the development of originally irrefutable ideas.[222]

I reject Feyerabend's suggestion that if a methodology does not imply advice to scientists about which theories they should work on, then it is

[221] Some attempts have been made to weaken the advice which methodologies ought to yield so as to take account of this argument. Musgrave, for instance, in his [1976], wants to make methodological advice, advice to the scientific community rather than to individual scientists – the scientific community should invest *most of* its (manpower and financial) resources into the best available programme. Grünbaum had earlier also pointed out that the 'appraisals imply advice' thesis can be saved from the above difficulty by making the advice, advice to the majority of scientists – the 'gifted minority' is excused from following the advice (see Grünbaum [1975]). But to adopt this position seems to me really to give up the game. The main reason for adopting it seems to be the idea that methodologies without advice are empty, an idea I show to be false below. The other arguments for the position are dealt with more satisfactorily by my position. I argue that the methodology of research programmes can *explain* why most scientists work for most of the time in the most progressive programme available to them, but it does not *advise* scientists to do so (see below, p. 175).

[222] See, for example, Popper [1963], chapter 2. In fact the 'adviceless' nature of falsificationism is, if anything, clearer than that of the methodology of research programmes, which includes in its appraisal of the *present state* of a programme an appraisal of its heuristic power, and hence of the likelihood (in some intuitive sense) of its making *further* progress, *without the injection of major new ideas*. This does not, however, refute what I have said above. Some scientist may decide to work in a degenerating programme and, in doing so, endow it with powerful new heuristic principles. (This is what, for example, Fresnel did.) Such decisions must not, therefore, be advised against by the methodology.

empty. Such a methodology will still appraise and rank theories in terms of their present scientific merits, and it will tell a scientist what general features a new theory must have if it is to be even better than any of the existing theories. The fact that these requirements do not indicate how to go about constructing a theory which satisfies them does *not* mean that these requirements are empty (just as the notion of a proof in a formal system is not empty despite the fact that it does not – in general – indicate how to go about constructing a proof of any particular proposition).

Returning to my main concern – the problem of whether or not methodologies are testable against the history of science – our considerations suggest that a methodology pronounces directly only on the 'rationality' or 'irrationality' ('correctness' or 'incorrectness' would be better) of scientists' appraisals of existing theories, and not on the 'rationality' or 'irrationality' of working on one theory rather than another.[223] Thus, in the special case of Young, the directly embarrassing aspect of the affair for those who claim that Young established the wave theory's superiority is not that scientists did not stop working on corpuscular ideas, but rather that most seem to have continued to regard the corpuscular theory as superior.

But, the naturalistic fallacy being genuine, even a methodology's pronouncements on scientists' intuitive judgements of the theories which confronted them cannot be *directly* tested against history. A methodology that tells us what scientists *ought* to prefer will not be directly refuted if scientists' actual preferences are different. Nevertheless, a methodology which has to admit that scientists have often preferred the 'wrong' theory would certainly be embarrassed by the enormous increase (at least since Newton) in science's predictive power and practical, technological success. Also, the recent upsurge in the use by philosophers of science of historical examples to illustrate their own position and to undermine the positions of other philosophers, would be inexplicable unless these philosophers suppose that their appraisals have to reflect accurately the intuitive appraisals made by those scientists who were actually confronted with these theories.

My claim is, in other words, that we should regard the following proposition, which connects the methodologist's normative claims with descriptive ones, as an implicit adjunct to any methodology, M:

∗ Other things being equal, working scientists have accepted theory A as better than theory B if, and only if, A was better than B; moreover,

[223] But, of course, a methodology may indirectly say something on this point, as I show below, pp. 174–5. The point is simply that in order to make predictions about these aspects of the development of science a methodology will have to invoke extra premisses on whose truth the methodology is not obliged to insist. Thus if these predictions turn out to be historically false, there is no need to regard the methodology as disconfirmed – it may be these extra premisses which are false.

we can tell whether *A* was better than *B* by applying the criterion of scientific merit supplied by the methodology *M*.[224]

And indeed some contemporary philosophers do explicitly subscribe to something like *. Popper, for example, writes:

[My] view of scientific method is corroborated by the history of science, which shows that scientific theories are often overthrown by experiments, and that the overthrow of theories is indeed the vehicle of scientific progress.[225]

The conjunction of a particular methodology and its version of * (which is a propositional schema) does have descriptive consequences. If theories *A* and *B* were once rivals and the methodology makes *A* better than *B*, then this appraisal, plus *, entails that, unless things were unequal, scientists of the time did regard *A* as better than *B*. In order to make consequences of this kind historically testable, we must further require that any attempt to save such a prediction by an appeal to the inequality of things must result in a *specific* conjecture about why things were unequal. But methodologists will, in fact, when faced with a case in which scientists' intuitive appraisals differed from the appraisals provided by their methodology, feel obliged to attempt a specific explanation. We have seen this, for instance, in the case of Whewell's account of the reception of Young's work.

Given all this, there is one way in which a historical case study may disconfirm a methodology, and there are two ways in which a historical case study may confirm a methodology. I shall illustrate these three possibilities using the example of (naive) falsificationism.

According to naive falsificationism, theory *A* is better than theory *B* if *A* and *B* explain a certain range of phenomena equally well except that, in one instance, they give conflicting predictions and only *A*'s prediction survives experimental test: *A*'s superiority has been established by a 'crucial experiment' between the two. In the case of the early nineteenth-century revolution in optics, two of the most popular contenders for the

[224] Peter Urbach and others, confronted with *, suggested that it may be circular, and that it may not fulfil the role of plugging the gap between descriptive and normative factors because of the mention of scientists on its left hand (descriptive) side. But, for any methodology, * becomes circular only if one decides who is, and who is not, a scientist on the basis of the methodology concerned. But one can instead decide this on the basis of 'general informed opinion'. Now admittedly, (*a*) general opinion will be informed not by purely descriptive, empirical considerations, but by a mixture of empirical and normative considerations (this is argued in full below, pp. 169–70); and (*b*) the set of people whom 'general opinion' makes scientists will be 'fuzzy'. But admission (*a*) would affect my proposals only if the normative considerations which inform 'general opinion' were systematically taken from some explicit methodology (which they are not). As for (*b*), we can afford to be liberal about admission to the set of scientists provided that we included amongst the external factors, which may be invoked to explain divergences between their judgements and the judgements prescribed by the methodology, factors like a lack of the requisite intelligence or mathematical ability on the part of these scientists.

[225] Popper [1945], vol. 2, p. 260. See also Magee [1973], p. 28: 'Popper's view of science slides onto its history like a glove.'

title of crucial experiment have been Young's two slit experiment and his 1803 diffraction experiment. As we have seen, however, neither of these experiments did, as a matter of fact, convince scientists at the time at which they were (allegedly[226]) performed of the superiority of the wave theory. Thus, given either of these identifications of the crucial experiment, and given *, naive falsificationism can be saved only by an appeal to the inequality of things. This methodology is thus disconfirmed by my case study unless it can come up with a specific suggestion, backed by independent evidence, about what made things unequal.[227]

Could a falsificationist turn this disconfirmation into a confirmation? There are, as stated above, two ways in which he might do this. He could come up with a different contender for the role of crucial experiment between the two theories of light. Indeed several other contenders are already to be found in the literature. The most often mentioned of these is Foucault's experiment of 1850, which is taken as having established that the velocity of light in media denser than air is smaller than its velocity in air, as predicted by the wave theory, not larger than in air, as predicted by the corpuscular theory.

Given this re-allocation of the title of crucial experiment, falsificationism would say that the objective superiority of the wave theory was established only in 1850, and thus would no longer come into conflict (*via* *) with the attitudes of Young's contemporaries. Falsificationism would even be confirmed by the facts about the nineteenth-century revolution in optics if it turned out that scientists began to regard the wave theory as superior after the Foucault experiment had come out in its favour. For this is precisely what the methodology, given this identification of the crucial experiment, and given *, predicts. (Funnily enough, this descriptive prediction runs into the opposite difficulty. We have seen that if Young's experiments were crucial then, from the point of view of falsificationism, the revolution – in the sense of the time at which scientists started to regard the wave theory as superior – occurred twenty years too late. If the Foucault experiment (which was performed in 1850) was crucial then the revolution occurred twenty years *too early*. In the one case we should have to introduce external factors to explain scientists' irrational delay, in the other to explain their undue haste.[228])

Alternatively, the falsificationist may attempt to turn this historical

[226] As I pointed out earlier (§3, pp. 152–6), there is some doubt whether Young actually performed the two slit experiment.

[227] As I suggested earlier, we must not allow a methodology to get away with simply claiming that other things were not equal without making its claims historically testable by *specifying* the inequality. We must also require that the specific inequality-of-things hypothesis receive *independent* support – the falsificationist cannot be allowed to avoid disconfirmation by claiming, say, that Young's contemporaries *must* have been Newton-worshippers because they did not see that Young had crucially refuted Newtonian optics.

[228] Amongst the problems that someone who identified the Foucault experiment as the crucial one would face is that posed by the fact that the result Foucault is generally

disconfirmation into a confirmation by looking for a new 'external' factor, for whose obfuscating role there is independent historical evidence. Imagine, for example, that he decides that the really crucial experiment between the two theories of light was Young's two slit experiment; and that he conjectures that external factors intervened to prevent Young's contemporaries from knowing about this experiment. He finds that an account of this experiment was published *only* in Young's [1807] *Lectures*. Let us further imagine that historical research reveals that, because of some printing error, the copies of this book available up until the 1820s were produced with the very pages which ought to have contained the account of this experiment missing. This, it seems to me, would form a stunning confirmation of this falsificationist account.

These considerations apply quite generally to any methodology which ranks scientific theories. Any such methodology will, given $*$, be *disconfirmed* if it claims that theory A was better than theory B, yet theory B was accepted historically as better than A, and there is no independent support for the conjecture that external factors distorted scientists' judgement at that time. The methodology is *confirmed* either if its appraisals and scientists' intuitive appraisals go hand in hand, or if, in the case of divergence, independent evidence is produced for the existence of misjudgement-provoking external factors. Such confirmations may be particularly significant, if the same historical cases disconfirm other methodologies.[229]

On this account, the methodology of research programmes is confirmed by my historical case study. This methodology, conjoined with $*$ plus the fact that Young's wave theory was not preferred by the scientific community of the 1800s and 1810s, predicts that unless other things were

supposed to have established only in 1850 was already recognised as demonstrated beyond all serious doubt in the 1830s. Humphrey Lloyd, for example, writes in his [1833] of 'the *established fact* that the velocity of light is less in transparent bodies [than it is in air]...' (p. 392, emphasis supplied).

[229] Perhaps a more formal account will increase clarity. Writing 'CP' for the (*ceteris paribus*) assumption that other things are equal, '$A >_M B$' for the assertion that A is better than B according to the methodology M, and '$P(A, B)$' for the assertion that A was historically preferred to B, I claim that any methodology M should be regarded as asserting that

$$(*_M)\ CP \rightarrow (A >_M B \leftrightarrow P(A, B))$$

The first, straightforward kind of confirmation of M is where the 'initial condition' that $A >_M B$ is fed into the 'law' $*_M$, and the *ceteris paribus* assumption made, $*_M$ then yields $P(A, B)$, an assertion which may be confirmed by historical research.

$*_M$ implies:

$$(*'_M)\ (A >_M B \wedge \neg P(A, B)) \rightarrow \neg CP$$

This gives us the second kind of confirmation of M. Here the 'initial conditions' are '$A >_M B$' and '$\neg P(A, B)$' which, when fed into $*'_M$, yield $\neg CP$, an assertion which may be made specific and then independently historically tested.

If, however, all the historical evidence is that there were no particular perturbing factors at the time of the rivalry of A and B, then $\neg\neg CP$ should be regarded as historically confirmed and thus $*'_M$, and hence $*_M$ and with it the methodology M, are 'refuted'.

unequal, Young's theory was not better than its rival. And in fact Young's theory was not better than its rival according to the methodology of scientific research programmes. The successive versions of the wave theory developed by Young form a classic case of a degenerating problem shift: each new version did no more than deal *ad hoc* with (some of) its predecessor's refutations. Moreover Young was not really able, despite a few intelligent hints and gropings, to develop the heuristic machinery of the wave programme so as to provide clear guidelines for the further development of the programme and for how it should deal with the many problems (both theoretical and empirical) it was known to face. No wonder then that the majority of Young's fellow scientists preferred the (admittedly empirically degenerating but) heuristically powerful corpuscular programme.[230]

My account of how methodologies can be tested using history of science is, I hope, both a clarification of, and an improvement on, the account already given by Lakatos.[231] Many of Lakatos's readers have taken him as asserting that a methodology is the better, the more of the history of science it pronounces 'rational' or that it can explain 'internally'.[232] This would, however, make the Hegelian view that whatever is real is rational the supreme methodology. On my account, on the contrary, one methodology may be better confirmed than another even if it explains fewer historical developments 'internally'. For a methodology may be confirmed by historical cases of which it gives an *external* explanation, *provided* it can give independent historical evidence for the existence of the external factors it invokes.

I have assumed so far that there is such a thing as purely descriptive history against which methodologies can be tested. Several methodologists (notably Agassi[233] and Lakatos[234]) have claimed, however, that the writing of history is, whether or not historians realise it, informed by methodological considerations. This clearly raises the suspicion that my account

[230] Indeed, when the intellectual merits of the competing theories are assessed using the methodology of research programmes, this whole historical episode is explicable internally. Not only was the rejection of the wave programme, as Young had developed it, correct according to this methodology, so also, as I hope to show in a forthcoming paper, was the eventual acceptance of the programme as Fresnel had developed it. For not only did Fresnel make the wave programme progressive (it predicted new phenomena like conical refraction), he also revealed its heuristic power.

[231] See his [1971], reprinted in this volume.

[232] That is, in terms of the intellectual merits and demerits (as the methodology sees them) of the competing theories. (Perhaps I ought to stress again here that which factors are internal is, in the Lakatosian sense, methodology-dependent.) One of Lakatos's readers who interprets him as expounding the Hegelian view is R. J. Hall (see his [1971], p. 151). I should add that Lakatos explicitly denies that this is his position – see his [1971], p. 118 (reprinted above, p. 32).

[233] Agassi [1963].

[234] Lakatos [1971]; reprinted in this volume, pp. 1–39.

of the testing of methodologies against history involves a kind of circularity which may be vicious.[235] In order to remove this suspicion I shall now turn to the question of what history of science can learn from the philosophy of science, and, in particular, of how the writing of history of science may be affected by philosophical considerations. (This will also enable me to rebut the suggestion – made to me by intellectual friend and foe alike – that historical case studies undertaken for the purposes of investigating the merits of some methodology are likely to confirm that methodology; and to clear up some prevalent misunderstandings of Lakatos's account of 'rational reconstuctions'.)

A part of Lakatos's striking paraphrase of Kant's dictum holds that 'history of science without philosophy of science is blind'.[236] In arguing for this claim, Lakatos in fact argues for a pair of theses. The first thesis is that even history of science written without any explicit recourse to any methodology will in fact involve, or have implicit in it, some normative views. This thesis is susceptible of various interpretations (not always clearly distinguished by Lakatos). Given the thesis in some of these interpretations, my account of the testing of methodologies would indeed be vitiated by circularity. Fortunately under these interpretations the thesis is false. The second thesis (with which I unequivocally agree) is that by taking methodological considerations into *explicit* account, the historian will become aware of certain interesting problems (and, perhaps even more importantly, certain interesting shifts in problems) to which otherwise he would be blind. This second thesis does not imply any circularity problems for my account of the testing of methodologies.

What seems to me true about the first of these two theses is that a historian's selection procedures (and a historian may select only unconsciously although he *must* select) and the set of problems he finds interesting are circumscribed by normative considerations. Indeed, in simply deciding which people and what work to study (i.e. in deciding which people to regard as scientists, and which pieces of work to regard as scientific) the historian of science is implicitly relying on some normative methodological criteria. Of course, everyone will allow that the historian himself may be ignorant of any explicit philosophical or methodological views, and that he may simply follow 'general opinion' in deciding who was, and who was not, a scientist, what was, and what was not, scientific. But 'general opinion' on this point is not informed simply by empirical considerations – not everyone who was ever called, or whoever called himself, a scientist is held to be a scientist by general opinion,[237]

[235] Kuhn in his [1971] explicitly accuses Lakatos's account of the testing of methodologies of circularity.

[236] This paraphrase is, in full: 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind.' (Above, p. 1.)

[237] For example, Lafayette Ron Hubbard (founder of 'Scientology') is not generally regarded as a scientist, although he and many others claim him to be one.

and similarly not every piece of work that was called scientific is so considered by general opinion.[238]

'General opinion' and with it the writing of history of science are informed by some (often rather uncritically accepted) normative views about science. But this, as I briefly indicated earlier,[239] does not introduce a circularity into the process of testing methodologies against history. Only if 'general opinion' were consistently and exclusively informed by a specific methodology so that, in doing history of science, one was, in fact, selecting as 'scientists' and as 'scientific advances' just those that conform with this methodology, would history be useless as a test of that methodology. But this is not the case and thus we have a set of scientists and scientific advances specified *independently* of any specific methodology.

Indeed, the situation here is analogous to that within science itself. The fact that our observations are made 'in the light of', or are directed by, theoretical considerations (a fact which Popper and others describe by saying that observation statements are 'theory-laden') does not mean that they cannot be used to test theories. Only if the observations were interpreted purely in the light of the theory would no real test of it by them be possible.

This is enough to deal with the circularity charge, but let us investigate the more general question of how history of science is informed by philosophical considerations by further pursuing this analogy with theory testing. Those philosophers who have argued that observation statements are 'theory-laden' were surely correct to stress that all observations are *directed by* theoretical expectations and considerations.[240] They were also surely correct in claiming that which observations and experiments a scientist makes will depend on which theories he seriously entertains. It does not, however, follow from any of this (as some of these methodologists may have thought) that our observations are 'theory-impregnated'.

This second claim *may* also be true, but its truth is not established by the argument that all our observations and experiments are made 'in the light' of theory; moreover, its analogue in the case of testing methodologies seems to me clearly false. The admission that normative considerations are, willy-nilly, involved in the historian's selection of material and of problems does *not* imply that all statements in history of science books are normative or have implicit in them normative views.

For example, the reasons historians of science have chosen to study Thomas Young and to read, say, his paper 'On the Theory of Light and Colours' are based on general considerations about what constitutes

[238] For example, as Paul Feyerabend likes to point out, witchcraft was once widely considered to be a paradigm of experimental science.

[239] Above, p. 165, footnote 224.

[240] The theory of perception indicates that this is true even of our non-systematic, everyday observations.

genuine science. But the statement: 'Thomas Young wrote a paper called "On the Theory of Light and Colours" which was published in the *Philosophical Transactions* for 1802' is obviously not normative. Coming to a decision on its truth value would involve only descriptive, and no methodological considerations.[241] More importantly, the (true) statement that Young's work did not evoke much immediate interest is factual and not normative, although it was methodological considerations which led historians to emphasise and comment on this fact – they assumed that Young was a great scientist and that his theory was great science and hence they found the neglect of his 'achievement' remarkable.[242]

As well as directing interest, methodological considerations may also penetrate descriptive history by providing certain terms of the historian's language. The historiography of science is littered with terms like 'crucial experiment', 'hard fact', 'verification', 'inductive method', 'simplicity' and the like. It is (partly) this which leads Lakatos to speak of 'normatively interpreted history'.[243] But again the fact that some of the historian's terms are imported from methodology does *not* mean that any history is bound to be 'normatively interpreted'. It may sometimes be difficult to know if a historian uses these terms whether he is making a descriptive or a normative claim, but that these are two quite separate kinds of claims is always clear. Say, for example, that a historian writes: 'Young performed a crucial experiment between the corpuscular and wave theories of light'. It may not be clear whether he means 'Young performed an experiment, the result of which was correctly predicted by the currect version of the wave theory, but incorrectly predicted by the current version of the corpuscular theory', or whether he means in addition that this experiment 'ought to have convinced scientific men of the truth of' the wave theory.[244] But it *is* clear that the first of these is a straightforwardly descriptive claim (though it involves claims about logical relations) whilst the additional claim is normative in character. So long as we can separate these two kinds of claims (and here, it seems to me, we can be completely 'operationalist', asking whether in arriving at a decision on the truth value of a sentence, we should need to see if some methodological criterion was satisfied), and so long as we use only

[241] This is not to deny that some statements which the historian makes will be normative. There is nothing to stop a historian making a statement like 'Thomas Young produced a theory of light and colours which was, in 1802, the best available theory'. It is uncontroversial that whether such a statement is true or false depends on what the correct criteria of scientific merit are. The issue is whether *all* history of science statements have implicit methodological commitments.

[242] This is beautifully illustrated by Crowther, who writes: '...it is essential to know why *so great a scientist as Young* had so little general impact' (Crowther [1973], p. 671).

[243] Lakatos [1971], p. 91, reprinted in this volume p. 1, ('normatively interpreted' occurs in brackets in the original).

[244] The last quote is from Whewell; see above, p. 109.

descriptive claims in testing a methodology, then again no question of circularity arises.[245]

I now come to the second of the pair of theses implicit in Lakatos's paraphrase of Kant. This is that history of science can be improved in various ways by an explicit use of a critically defensible methodology. That some of the historian's descriptive terms are supplied by methodology does not imply that all history of science is 'normatively interpreted' or 'soaked in methodology'; however, it does point to an important way in which history may be influenced and improved by an explicit appeal to methodology. For one methodology may supply a set of descriptive terms which enables the historian to express more concisely and more faithfully more of the facts about some historical episode. Indeed my case study suggests, I claim, that the methodology of scientific research programmes is an improvement on other methodologies in this respect. Other methodologies, for example, speak in terms of scientific theories, but there were no such things as *the* corpuscular theory of light, and *the* wave theory of light. There were instead a series of wave theories and a series of corpuscular theories. Moreover, the terms of these series were related in various clear ways. These are not methodology-induced facts, but straightforward historical facts. However, the notion of a research programme and those of positive and negative heuristics which provide the connecting links between the various theories issuing from the programme do direct the historian to these facts and enable him to describe them accurately and in detail.

A sort of corollary to this point is that problems may be generated for a historian simply by the lack of descriptive power in his language. For example, if the historian's language allows him to speak only in terms of theories, then he may well find it difficult not to consider Young as hard done by. After all, did Young not propound in the early 1800s the theory that light is a wave motion in an all-pervading medium? And was not this the theory for which Fresnel was much later given great credit? Why then did the scientists of the time have to wait for Fresnel to 'rediscover' the wave theory before accepting it? Once, however, the historian's language is enriched with the new descriptive notion of a programme, then there *need* be no mystery about this. It may have been (and, indeed, I claim

[245] In view of all this, Lakatos's claim that all histories ('histories$_2$' – sets of statements) are rational reconstructions (of 'history$_1$' – the set of historical events) is false, unless by rationally reconstructed history is meant simply history selected from some point of view. Lakatos uses the term 'rational reconstruction' in a variety of senses. His failure consistently to distinguish between the various senses has caused confusion (many have even taken him to be asking for licence to falsify history to fit his methodology!). In view of this, it is perhaps better to drop the term altogether, or to restrict it to its original Lakatosian use as a pedagogical device. (The discussion in his essay 'Proofs and Refutations' consists of 'rationally reconstructed history' from which the wrinkles have been ironed out and the less essential features omitted. These reappear in the footnotes where 'the *real history*... chime[s] in.' (Lakatos [1963–4], p. 7; my italics.))

that this is how it was) that the wave programme as it had developed up to Young, was not superior to the corpuscular programme as it then stood; whereas Fresnel developed the *same* wave programme into much the better of the two rivals.

This second Lakatosian thesis can be pursued further. Given that parts of the historian's terminology and many of his problems are supplied by (possibly unconscious) methodological considerations, it is clearly better if he can make these considerations explicit and so expose them to criticism. This will, at least, sharpen the problems the historian faces and will, very often, show him that the real problems are different from what he originally thought. For instance, I think I have shown in preceding sections that the problem of Young's neglect by his contemporaries is created by normative considerations – namely, the usual high appraisal of the merits and importance of Young's work. So long as the historian is not even conscious that this is an *assumption* that he is making, he is bound to continue to look for an explanation of Young's treatment in (what for us will be) 'external' terms. On the other hand, once this appraisal of Young's work and the methodological assumptions on which it rests are made explicit, then a new possibility appears. This is that this appraisal is mistaken; and if it is mistaken (as indeed I have argued it is) then the descriptive historical problem is (as we have also seen) radically shifted. The historian is bound to be blind even to this possibility if he insists that he can ply his trade without recourse to any methodology.[246]

There is a second way in which a historian's perceptiveness may be improved by making *explicit* use of methodology. A methodology may supply the historian with a heuristic – not only with a set of problems, but also with a stock of conjectured solutions of them. I shall illustrate this point with some examples.

The standard pattern of scientific development according to the methodology of scientific research programmes is of protracted rivalry between research programmes – a rivalry from which one eventually emerges victorious, not at a single blow ('crucial experiment' style), but rather after a sustained period of progress on its part and degeneration on its rival's part. The methodology does *not* predict that all (major and minor) scientific revolutions take this form: it is perfectly conceivable, for instance, that some programmes were never pursued into their degenerating phases. Nevetherless, the methodology does point to the possibility of such developments and was indeed developed to deal with

[246] Entirely similar considerations apply to sociologists. Any analysis of any of the social aspects of science is likely to be trivial unless account is taken of the intellectual merits and demerits of what scientists produce; but in order to assess these satisfactorily a critically defensible methodology is required. Moreover, very different sociological theories are likely to be produced when different methodologies are accepted as guides – what one says, e.g., about the 'authority structure' in early nineteenth-century physics is likely to depend very heavily on how one appraises Young's real intellectual achievement.

them. Thus, this methodology does not *predict* that where the secondary sources say there was a crucial experiment, the primary sources will reveal protracted rivalry and several alternative explanations of the experimental results based on the allegedly crucially refuted theory. It does, however, encourage the historian to look for these things in the primary sources. And (as we have seen in §3(*a*1)) they are, in fact, to be found in the case of Young's two slit and diffraction experiments.

Similarly, falsificationism does not logically imply that no experimental discoveries were made by chance. It does, however, suggest (as was pointed out by Agassi[247]) that most discoveries were arrived at as refutations of known and seriously entertained theories (especially where the discovery concerned was considered by scientists to be of major importance). Thus falsificationism provides a heuristic for the historian of science: take any famous 'chance discovery' and see if the discoverer did not in fact have a theory in mind, the testing (and refutation) of which led to the discovery.[248]

To take a third and final example, historical conjectures about scientists' decisions to work on one theory rather than another may also be suggested by systematic methodological appraisals. For although, as I argued above,[249] methodologies must not instruct the scientist to work on only the best available theory (and do not predict that all scientists will do so), there is no doubt that a scientist's intuitive appraisal of the present state of a theory and its competitors will often play a big role in his decision whether or not to try to develop that theory. Consider again the methodology of scientific research programmes (which is stronger than most other methodologies in this respect). It includes in its appraisal of the present state of a programme an appraisal of its present heuristic power. And it points out that in a progressive programme with a strong heuristic there will be plenty of new problems and well-articulated methods for attempting to solve them.[250] The creative leap required to invent a new progressive theory within such a programme is, in a clear, if intuitive sense, smaller than it is in less powerful programmes, where entirely new ideas may be needed. Hence this methodology *suggests* (but does not entail) testable historical conjectures of the following kind: if there was

[247] See his [1963].

[248] Agassi applies this heuristic to several interesting cases in his [1963]. Notice, by the way, that this heuristic could prove successful even if no new (i.e. hitherto unknown) historical facts were turned up in the process. For old material may be newly (and more plausibly) interpreted. For example, what was before considered a stray remark on which the scientist concerned, for some strange reason, laid a great deal of stress, may now become a central remark; what was before considered an entirely fortuitous change of conditions of some experiment may now be explained as made in a deliberate effort to refute a previously held theory. (This seems to me to deal with an argument for the inutility of philosophy in history of science presented by Pearce Williams in his stimulating [1975].)

[249] Pp. 162–4.

[250] For this, see especially Zahar's account of the relativity programme in his [1973]; reprinted below, pp. 211–75.

at some particular time a programme which was progressive and heuristically more powerful than other available programmes, most scientists will have worked on it.

(It may seem that in this third example of philosophy supplying a historical heuristic, I am taking back what I said earlier about appraisal and advice, and about a methodology having direct consequences only for scientists' appraisals of the merits of theories. I am not. To turn these suggestions about what scientists do into genuine *predictions*, into genuine deductive consequences of the methodology, extra historical assumptions have to be made. The methodology is by no means committed to truth of these extra assumptions in particular cases – as it *is*, I have argued, to the truth of $*$. Assume, for example, that there was, at some particular time, a programme which the methodology of research programmes appraises as more progressive and heuristically more powerful than any other available programme. If this appraisal is to yield the prediction that most scientists worked on this 'best' programme, we should have to add to it not only the $*$ assumption that these scientists did intuitively make this appraisal, but also an extra premiss. This extra premiss would state roughly that the majority of scientists at that time were sufficiently enthusiastic about the progressive programme to want to devote their time to it, and that they were not self-confident enough – and not sufficiently motivated, say, by metaphysically-induced preferences – to try to instill new life into what they recognised to be a degenerating programme. But there seems no reason to regard the methodology as committed to these extra assumptions.[251] Thus, if some particular instance of this prediction turned out to be false, there would be no need for the methodology to invoke external factors which induced scientists to misjudge the intellectual situation. For, as I have pointed out, scientists may quite consistently try to develop a theory A, even while agreeing that there already exists a theory B, which *as things stand*, is better than A.[252] Whereas, as I argued above, if some particular instance of a prediction (based on a methodology and the relevant instance of $*$) about scientists' intuitive rankings of theories' merits turned out to be false, then the supporters of that methodology *would* be obliged to look for misjudgement-provoking external factors).

Finally, let me summarise this section. First, I tried to show that a methodology can be rendered historically testable by adding the relevant

[251] The fact that these extra assumptions do, however, seem to hold for most historical periods (i.e. that scientists do generally get excited enough about sufficiently powerful programmes, however 'absurd' their metaphysical presuppositions) provides the *explanation* (promised above, p. 163, footnote 221) of why most of the scientists work for most of the time in the most progressive programme available to them.

[252] For example, several scientists in the first few decades of the nineteenth century (Sir John Herschel and Sir David Brewster amongst them) seem to have been inclined to try to revitalise the corpuscular optics programme, although everyone agreed (Herschel and Brewster are on record as agreeing) that, as things stood, the wave optics programme had been put well ahead of its rival by Fresnel.

instantiation of *. Secondly, I tried to show that the writing of history of science is guided, for the most part implicitly, by normative considerations, but that this does not entail that all statements about the history of science are normative, nor that the attempt to test philosophies against history is circular. Lastly, I tried to indicate how explicit recourse to (normative) methodological considerations can improve history of science.

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