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FRESNEL, POISSON AND THE WHITE SPOT: THE ROLE OF SUCCESSFUL PREDICTIONS IN THE ACCEPTANCE OF SCIENTIFIC THEORIES

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INTRODUCTION: A PRIZE STORY

One of the most attractive stories in the whole history and philosophy of science folklore concerns the award of the French Academy's prize of 1819. As usually told, the story could almost provide a script for 20th Century Fox. A prestigious prize was at stake. The hero of the story was a comparatively young man from the provinces, with unorthodox views. Facing him was a prize commission whose members were very much part of the Parisian scientific establishment and a majority of whom were committed to the rival, entrenched orthodoxy. Most dramatically, there was a time when all seemed lost for the hero, but then came an amazing *volte face*, and his unorthodox views finally won through. Add to this the facts that there was constant concern for the frail hero's health and that the episode was played against the background of political turmoil – the hero himself rushing to join the royalist army which opposed the return of Napoleon from Elba – and the story looks like a sure-fire box-office winner.

Here are some of the details. The topic for the prize was the diffraction of light; the hero Augustin Jean Fresnel, in 1819 newly restored to his post as officer in the Engineering Corps, and just 30 years old. Fresnel sent to the Academy a long memoir[1] which attempted to show in detail how all diffraction phenomena could be explained on the supposition that light consists of wave-like disturbances transmitted through an all-pervading medium. This theory was, of course, not new in the early nineteenth century, but it had long been regarded as discredited. The prevailing view, invariably described as Newtonian, amounted (at any rate on a 'realistic' interpretation) to the theory that light consists of

1. Fresnel (1819).

material particles subject to short-range forces emanating from ordinary 'gross' matter. This emission theory formed a central plank of the Laplacian approach to physics which was dominant in France at the time. The commission of prize judges was a 'Who's Who' of contemporary French physics; it comprised Biot and Poisson, both committed Laplacians, as well as Laplace himself, plus Arago and Gay-Lussac. Arago was already known as a champion of Fresnel and his ideas. As for Gay-Lussac, while he, like the others, had been an active member of the Arceuil group around Berthollet and Laplace, he never seems to have taken a particular interest in optics and can probably be assumed to have been impartial. Verdet describes Gay-Lussac as 'little familiarised through his studies with the question at issue, but disposed by character to a wise impartiality'; Maurice Crosland asserts that 'Gay-Lussac . . . on this question could be relied upon to be impartial'.[2] But with or without Gay-Lussac, the corpuscularists held a clear majority on the commission. Indeed it is probable that the whole idea of a prize competition on diffraction had been conceived by the Laplacians, and principally Biot, as a stimulus to young scientists to provide a successful corpuscular account of this range of phenomena. Fresnel's chances, apparently quite slim at the outset, seemed to shrink to zero when, during the commission's deliberations, Poisson demonstrated that Fresnel's wave theory of diffraction has a patently absurd consequence: it implies that if a small opaque disc is held in the light emanating from a small hole, the centre of the disc's shadow will be bright, just as bright indeed as if no obstacle had been placed in the light's path. Arago tested this consequence; lo and behold, he found the white spot! The Academy's prize was promptly awarded to Fresnel. More importantly, the outcome of the competition proved a turning point in the history of science. Fresnel's success in 1819 signalled an end to the long domination of the corpuscular theory, and by the mid- to late 1820s the wave theory of light became generally accepted in the scientifically advanced countries. This successful novel prediction of a hitherto unsuspected, indeed hitherto *counter*expected, phenomenon had played a central, perhaps even crucial, role in the acceptance of the new theory from which it was drawn.

Something like this version of the story has penetrated the history and philosophy of science folklore. One of Kuhn's illustrations of a 'conversion' to a 'new paradigm' is, for example, the following:

2. Verdet (1866), p. xlii; Crosland (1967), p. 409.

In the case of the wave theory, one main source of professional conversions was even more dramatic [than any of the factors leading to the Copernican revolution]. French resistance collapsed suddenly and relatively completely when Fresnel was able to demonstrate the existence of a white spot at the center of the shadow of a circular disk. That was an effect that not even he had anticipated but that Poisson, initially one of his opponents, had shown to be a necessary if absurd consequence of Fresnel's theory.[3]

And Ronald Giere tells the story this way:

Fresnel's memoir was referred to a commission in which well-known advocates of particle models, Laplace, Poisson and Biot, held a majority. The commission was apparently not fully convinced by the evidence Fresnel had presented, and Poisson devised a further test. He applied Fresnel's model to the case of a shadow produced by a circular disk and deduced that the resulting diffraction pattern would have a bright spot at the centre of the shadow. Even from superficial accounts of this incident it seems clear that no one involved had ever seen such a spot. Moreover, it seems that Poisson and his fellow commissioners did not expect the spot to appear. It certainly was not a consequence of any current particle model that such a spot should exist. The experiment was performed by Francois Arago . . . The spot appeared as predicted and the commissioners yielded.[4]

Earlier, but essentially similar accounts are to be found in Jevons and Whewell. Most recent brief accounts of the episode refer to the following passage from Whittaker:

A champion was indeed needed when the [prize] memoir was submitted; for Laplace, Poisson and Biot, who constituted a majority of the Commission to which it was referred, were all zealous supporters of the corpuscular theory. During the examination, however, Fresnel was vindicated in a somewhat curious way. He had calculated in the memoir the diffraction-patterns of a straight edge, of a narrow opaque body bounded by parallel sides, and of a narrow opening bounded by parallel edges, and had shown that the results agreed excellently with his experimental measures. Poisson, when reading the manuscript, happened to notice that the analysis could be extended to other cases, and in particular that it would indicate the existence of a bright spot at the centre of the shadow of a circular screen. He suggested to Fresnel that this and some further consequences should be tested experimentally; this was done, and the results were found to confirm the new theory. The concordance of observation and calculation was so admirable in all cases where a comparison was possible that the prize was awarded to Fresnel without further hesitation.[5]

These accounts differ *slightly* in the exact claim made about the impact of the white spot episode: no doubt none of the authors, if pressed,

3. Kuhn (1962), p. 155. 4. Giere (1984), p. 280. 5. Whittaker (1951), p. 108.

would commit himself to the view that the episode was, in isolation, *the* crucial factor in winning the prize for Fresnel (and, more importantly, winning general acceptance for his wave theory). But several accounts come close to espousing this view, and certainly all agree in attributing some sort of special significance to the appearance of the white spot.

I shall argue in what follows that this story is historically incorrect. But my purpose is not simply to pour some factual cold water on an appealing story. The story has often been cited as an important illustration of a *general methodological thesis*: the thesis that favourable *novel* evidence – evidence first discovered only as a result of testing some already articulated theory – carries greater weight in support of that theory than does favourable, but already known, evidence.[6] The main aim of the present paper is to show that the real history of the reception of Fresnel's wave theory of diffraction supports, *not* this 'novel facts count more' view, but a rather different account of empirical support.

A MORE ACCURATE VIEW OF THE WHITE SPOT EPISODE

It is not usually easy to provide convincing evidence of what did and did not impress a particular group of scientists in their assessment of a particular scientific work. The case of Fresnel's prize memoir of 1819 is unusual in that there exist seemingly reliable public sources of exactly this information. These sources have been notably underexploited by those who have commented on the episode.

First and foremost there is the report of the prize commission itself.[7] This was read at the French Academy as part of the process of revealing the result of the competition, and was subsequently published, along with some additional notes, in the *Annales de chimie et de physique* in May 1819. I shall rely principally on this report in what follows. Some of the history of that report itself can, however, also be traced. Fresnel had submitted his first memoir on diffraction[8] to the Academy some three years before the prize competition. This earlier memoir contains several particular claims which Fresnel soon abandoned and replaced. None the less the overall theory presented there has a good deal in common with the 1819 theory. As was the custom, a commission was appointed (consisting in this instance just of Arago and Poinsot) to report on the merits of this 1816 memoir to a full meeting of the Academy, who then decided whether or not the paper should be pub-

6. For a discussion of, and references to, this debate see Worrall (1978).
7. Arago (1819). 8. Fresnel (1816).

lished in its *Recueil des Savants Étrangers.* Arago and Poinsot's report[9] was published in Fresnel's *Complete Works* and so is readily available today. More remarkably, so is a record of the reactions expressed at the meeting which heard Arago and Poinsot's report by various Academy members, including Biot, Poisson and Laplace. This record was taken by the secretary, kept by the Academy, and published in the early twentieth century as part of a *Procès-verbaux.* The final prize commission report of 1819 seems to represent a compromise between the 1816 report on Fresnel's first memoir and the rather less enthusiastic reactions of the Laplacians, recorded in volume 6 of the *Procès-verbaux* and, no doubt, expressed again during the prize commission's deliberations. These records, together with other papers and letters, permit a fairly complete reconstruction of the reaction of the French scientific elite to Fresnel's theory of diffraction and in particular to the white spot episode.

One common illusion about this episode can be immediately dispelled in the light of the prize commission's report. It is easy to form the impression from recent accounts that the prize was a highly competitive affair with a long list of entrants, that it was always unlikely that Fresnel would win and that he therefore needed something as dramatic as the unexpected success of the white spot prediction derived by Poisson. In fact there were just *two* competitors. There seems to be no record of who Fresnel's rival was – the candidates were officially anonymous, each memoir being identified by an epigram and being referred to by the commissioners as 'number one' and 'number two'. 'Number one's' memoir was not published. However, some judgment can be made of the scientific value of his paper from the fact that of the whole report of the commission, which in its published version covers some 18 printed pages, just one short paragraph at the end is devoted to poor old 'number one':

> The author of the Memoir marked number one is certainly a trained physicist; but, the means of observation which he employed not being sufficiently precise, some of the phenomena exhibited by light in passing through small apertures, or merely in the neighbourhood of opaque bodies have escaped his attention. The author seems to know neither the work for which we are indebted to Dr Young nor the memoir which M. Fresnel had published in 1816 in the *Annales de chimie et de physique*: also the part of his work which concerns the influences which rays of light exercise on one another in mixing, far from adding anything to what was already known, includes several obvious errors . . . [10]

9. Arago & Poinsot (1816). 10. Arago (1819), p. 236.

No wonder then, that the paragraph continues (and concludes): 'accordingly the commission has decided to award the prize to the memoir marked number two, and carrying as epigram: *Natura simplex et fecunda.*' As Verdet remarked '[Number one's] work was not for an instant put in the balance with that of Fresnel.'[11] The competition facing Fresnel could hardly have been less stiff.

A second common illusion about this episode is that the appearance of the white spot brought about not only the decision to award the prize to Fresnel, but also an immediate recognition on the part of the commissioners of the superiority of the wave theory championed by Fresnel over its previously entrenched rival. In fact, the report does its best to ignore basic questions concerning the nature of light: despite Fresnel's own strong emphasis on general theory, and his wholehearted commitment to the wave theory, the report manages to avoid any discussion of this and indeed the word 'wave' occurs nowhere in it. Fresnel's theory is given a rather severe positivistic reinterpretation. It is taken as consisting essentially of its mathematical expression – the famous 'Fresnel integrals'. The various different cases of diffraction (straightedge, single slit, etc.) are characterised by different limits of integration, and the resulting definite integrals yield expressions for the varying light intensities. Fresnel himself, had, of course, presented these integrals as *consequences* of the underlying wave nature of light. But this general theoretical aspect of his views is played down in the commission's report, being replaced only by a rather bloodless version of the Huygens' principle: a version which talks simply of the 'rays' of light (an *apparently* theory-neutral term), and makes no reference to any medium, let alone to any disturbances within a medium.

As for conversions to the wave account, here is Poisson, the very man who drew out the allegedly crucial prediction, writing to Fresnel in March 1823, four years after the prize competition:

> The theory of emission and that of waves both encounter great difficulties; time and the future work of physicists and mathematicians will perhaps end by settling all these doubts and entirely clarifying the question; but I believe that one can be sure at present that *if* the second theory [i.e., the wave theory] is true, it is certainly not for the reasons that have been given up to now . . . [12]

Poisson continued long after 1819 to point to difficulties (mostly of a basic mathematical kind) in Fresnel's theory; and, although one sometimes reads of Poisson being converted to waves sometime in the 1830s, convincing evidence of any full conversion is not easy to find.

11. Verdet (1866), p. xlii.
12. Poisson's letter was published in Fresnel (1866–70), II, p. 189.

Biot, another of the prize commissioners, also continued to resist the wave theory long after 1819. He remained a staunch Newtonian until *at least* the 1830s, attributing defects in the Newtonian theory largely to the fact that relatively few talented people were working on it. As for Laplace, he was already 70 years old in 1819 and published nothing on optics afterwards, but having been such a strong advocate of the Newtonian theory, any conversion on his part to the wave theory would surely have been reported somewhere. No such report seems to exist.

So there was no instant conversion to Fresnel's wave theory even among the prize commissioners, let alone of the scientific communities in Britain and France more generally. Still, even if it was much more gradual and less universal than the usual over-dramatised accounts imply, there was undoubtedly a general shift in scientific opinion away from a Newtonian theory of light (which was dominant in the eighteenth century) towards the wave theory (which became dominant by the 1830s). Moreover, focusing just on the prize competition, something must have convinced the commissioners of the high scientific merit of Fresnel's work – in some sense of high scientific merit compatible with an uncommitted attitude towards the truth of the underlying theory. (After all, the commissioners could always have decided to make no award at all.) Doesn't a slightly watered-down, more realistic, version of the story still stand? If the success of the white spot prediction was not *the crucial* factor (on reflection, we should no doubt not expect there to be any such thing), was not that predictive success at least the most important single factor in winning the prize for Fresnel, and in gradually winning his theory, if not always outright belief, at least a more and more sympathetic reception? This is something like the position adopted by Emile Verdet. Being much closer to the primary historical material, Verdet knew well enough that the white spot episode had *not* changed 'the basis of [the commissioners'] convictions'; but he none the less described it as a 'remarkable incident' which 'made a great impression on the minds of the judges' and 'probably determined the unanimity' of the decision.[13]

The question of what influenced the British and French scientific communities generally is, of course, a large one; but if we focus just on the prize commissioners then it is, I believe, clear from their report that even the watered-down claim about the white spot's impact is false.

First, so far as just the prize itself goes, it seems likely that Fresnel would have won even had his memoir contained no theory whatsoever. Fresnel's memoir after all records his invention of a new method of

13. Verdet (1866), p. xlii.

observing and measuring diffraction fringes. Earlier investigators had observed the fringes indirectly, either by casting them on a white screen or by viewing them from behind a plate of unpolished glass. Fresnel discovered that this was unnecessary and that the fringes could be viewed directly, in mid-air so to speak, using a simple magnifying glass.[14] This direct method led immediately to greatly enhanced visibility of the fringes and it allowed them to be observed much closer to the diffracting object itself than had hitherto been possible. Moreover, using an instrument of his own construction, which allowed the position of the lens to be nicely adjusted *via* a micrometer gauge, Fresnel had measured the distances between fringes with greatly increased precision. Almost one half of the prize commission's report is taken up with Fresnel's new observational method and its advantages – before any mention at all is made of any account which might codify the observational results achieved *via* the method.

The commissioners did, however, eventually come to 'the hypothesis on which the integral is founded which [Fresnel] presents as the general expression of all diffraction phenomena'[15] and to the question of what support this hypothesis receives from various experimental results. One interesting point, already mentioned, is that even though now concerned with theory, the commissioners present a bloodless, positivistically reduced version of Fresnel's views. They produce a brief (and in fact rather garbled) statement of the 'Huygens' principle' in terms, *not* of real mechanical disturbances travelling through a real mechanical medium, but of 'rays' spreading out from each point and interfering with one another. The central issue from the present point of view, however, is not how they interpreted the theory, but how they appraised the strength of various pieces of evidential support. Here there is no doubt that the commission placed the emphasis firmly on the *already known* straightedge diffraction cases, rather than on the white spot prediction.

The history of this central feature of the report can easily be traced. As I already remarked, Arago and Poinsot were charged with reporting on Fresnel's *first* memoir on diffraction, published in 1816. The feature of Fresnel's theory that chiefly impressed Arago and Poinsot was that it yields precise details of the diffraction fringes observable both inside and outside the geometrical shadow of a narrow opaque object, and that it yields these details in a straightforward way, one which needed to borrow only one quantity from observation. Here, for example, is

14. Fresnel (1819), pp. 262–3. 15. Arago (1819), p. 235.

what Arago and Poinsot said about Fresnel's theory and the interior fringes:

The simplest circumstances of the formation of the interior bands are inexplicable or at least unexplained on the usual [corpuscular] theory; that of M. Fresnel shows at the same time how they are propagated, what sizes they must have when the screen is placed at different distances, how these sizes are, for a given position of the body, in inverse proportion to its diameter; it explains also why the position of these fringes is independent of the distance of the point source . . . It determines finally when and how each interior fringe must leave the shadow to take its place among the visible exterior fringes . . . If we add that, in the same theory, the sizes and positions of the bands of different shades are deducible from the general formula, by replacing only [the parameter] *d* by corresponding values furnished by the observation of the coloured rings of the first order; that the fixity of the deviation, whatever the density or the refringent force of the body which produces the shadow, is not only an intelligible, but even a necessary consequence; that there is finally no diffraction experiment, known at present, which cannot, I shall not say be explained, but even calculated [from the theory]; one will not be able to avoid avowing, whatever opinion one holds on the basic question, that M. Fresnel's hypothesis deserves to be followed and to win the attention of physicists and geometers.[16]

When this early report was presented to the Academy, it met with a certain amount of criticism. This was mainly to the effect that many difficulties still faced the wave theory, that the corpuscular theory remained preferable in at least some respects, and that, therefore, more emphasis should be placed on the experimental laws discovered by Fresnel and less on the general theory which, he alleged, explained them.[17] This criticism, which emanated from the Laplacians, was no doubt repeated during the deliberations of the prize commission and is certainly reflected in their eventual report, as we saw earlier.

If the story about the white spot is at all correct, another important difference between the 1816 Arago–Poinsot report and the 1819 prize report is surely to be expected; for it was only during the 1819 commission's deliberations that the white spot episode occurred. If this predictive success really had the impact usually attributed to it, a major difference between the two reports would lie in the section dedicated to the empirical evidence for Fresnel's 'hypothesis': the white spot evidence should take pride of place in the 1819 report. In fact the 1816 and 1819 reports differ hardly at all in this respect: pride of place is definitely retained in 1819 by the straightedge cases – cases which had long been the subject of experimental scrutiny (beginning in the seventeenth century with Grimaldi, and then Newton).

16. Arago & Poinsot (1816), p. 86. 17. *Procès-verbaux*, 6, pp. 317–21.

The prize report emphasised, as before, that in these straightedge cases Fresnel's theory yields fringe sizes for all values of the distances between the source and diffracting object and between that object and the point of observation of the fringes. And it is again explicitly emphasised that the theory deals with these cases in a particularly straightforward way. The integral that Fresnel's theory yields as their solution is completely determined by that theory itself, except for one quantity which can be fixed independently of the straightedge diffraction experiments (for example, by doubling the numbers given by Newton for his 'intervals of fit'). These numbers had in turn been derived by Newton from his famous measurements of the diameters of 'Newton's Rings' – a phenomenon which does not at all involve diffraction as usually understood. The report recorded that Fresnel had made a series of 125 experimental measurements of the external fringes outside the shadow of a straightedge, and that in this whole series the difference between observation and the value provided by Fresnel's integral was only *once* as much as 5/100 mm, only *three* times 3/100 mm and *six* times 2/100 mm. In all the other 115 cases disagreement between theory and observation did not exceed 1/100 mm. Similar success was achieved by Fresnel in the case of a narrow slit and that of the fringes inside the geometrical shadow of a narrow object; again, both cases whose qualitative features had long been known, though admittedly Fresnel was now treating them with unprecedented precision.

It was only after this relatively long discussion of these already known cases that the prize commission's report turned to the white spot. The report devoted precisely *two sentences* to this phenomenon – two sentences in the penultimate paragraph of a report whose main text amounts to some nine printed pages. These two sentences read:

> One of your commissioners, M. Poisson, had deduced from the integrals reported by the author the singular result that the centre of the shadow of an opaque circular screen must, when the rays penetrate there at incidences which are only a little oblique, be just as illuminated as if the screen did not exist. This consequence has been submitted to the test of a direct experiment, and observation has perfectly confirmed the calculation.[18]

The consequence deduced by Poisson is indeed described as 'singular' and there is no doubt that its experimental confirmation was another impressive feather in Fresnel's cap. But there is no indication of any *exceptional* importance being attributed to this result, let alone any indi-

18. Arago (1819), p. 236.

cation of everything hanging on it. This new piece of evidence in favour of Fresnel's theory simply takes its place alongside other favourable evidence concerning already well-investigated cases.

So much for the main text of the commission's findings which contains the report actually made to the French Academy. The version published shortly afterwards in the *Annales de chimie et de physique* contained some extra material in the form of five appended notes. The last, and briefest of these (Note (E)) contains further reference to Poisson's deductions from Fresnel's theory. It records that Poisson had remarked to Fresnel, *some time after the commission had reported,* that his intensity integral could 'easily' be evaluated for two cases: the centre of the shadow of a small circular screen and points on the axis of a small circular aperture. The screen case having already been dealt with in the main text, nothing was added here. As for the circular aperture case, the note records that Fresnel himself had subsequently supplied the detailed calculation and found intensities varying between four times that expected in the case of an unobstructed wave and zero, depending on the distance of the point of observation from the centre of the aperture. Fresnel had gone on to confirm this prediction empirically. This is surely just as 'singular' a result as the screen case. Fresnel had found that his theory entails that if light from a point source is shone on an opaque screen in which there is a small circular *opening,* then at certain points beyond the screen but along the axis of the aperture (that is, the extension of the line from the source to the centre of the aperture) there is total darkness.

This 'black spot' prediction is 'complementary' to the white spot case – it is just as counterintuitive as the latter and its empirical success just as unexpected and remarkable. But again no special fuss was made of it and again no indication at all was given that any more weight was attributed to this new success of the theory than to the successes – both 'novel' and 'old' – already reported in the main text. In fact the most interesting aspect of this part of the story is that the consideration of this new prediction, and its empirical testing, were left until *after* the award of the prize. Poisson had seen, during the commission's deliberations, that the Fresnel integral could 'easily' be evaluated both for the small circular screen and the small circular aperture. He had performed the detailed calculation for the first case, and the results predicted had been empirically tested. But the second case was left uninvestigated until *after* the commission's decision. If everything hinged on predictive successes of this kind, or even if special weight was being given them, then this seems very strange behaviour. Under such circumstances one would surely expect the commissioners to urge Poisson to supply the detailed calculation and Arago to test the resulting prediction as soon as possible. One would also expect the commissioners to postpone their decision until they learned the result. Clearly they felt no such urgent need. To sum up: the original published documents provide no historical evidence that any special weight was given by the prize commission to Poisson's prediction of the white spot and its empirical confirmation. The emphasis in these documents is very much on Fresnel's success in dealing with the already long-known cases of straightedge diffraction.

DOES SUCCESSFUL PREDICTION CARRY ANY SPECIAL EPISTEMIC WEIGHT?

There is a long tradition in the *philosophy* of science which tries to articulate a logic of empirical support. It shows how theories *ought* to be appraised 'objectively' in the light of the empirical evidence and, in particular, when one theory is to be preferred to another in the light of the available empirical evidence. Mill and Whewell were distinguished nineteenth-century contributors to this tradition. In this century, Reichenbach, Carnap and other inductive logicians have contributed, as have Popper, Lakatos and others. Unshaken by Kuhn and the sociologists of science, the adherents of this tradition hold that there is an important element of rationality in the development of science which has not been present (or has not been present to anything like the same extent) in the development of say, art, or philosophy. Scientists generally have switched theories because the new theory was 'objectively superior' to the old according to clearly articulable, general principles. They claim that means that the new theory was *better empirically supported* than the old. 'Switching to' or 'preferring' a theory need not – in my view – involve believing it to be true. Einstein, for example, clearly thought that the quantum theory was overwhelmingly the best-supported theory in its field – but he also believed that it could not be true and that it would sooner or later be replaced by a theory which would be still more strongly supported, and also deterministic. A major problem for this 'logic of support' tradition is that of exactly how this *normative* enterprise is meant to mesh with the *descriptive* details of the history of science. This problem is the focus of a famous (in some circles, notorious) paper by Lakatos. I have tried to improve on Lakatos's solution.[19] Without going into details, it is clear that, just as a normative theory of goodness would be in bad trouble

19. See Lakatos (1971) and section 5 of Worrall (1976).

if a large number of generally recognised saints turned out to be evil according to its criterion, so a normative theory of science would be in bad trouble if such notables as Fresnel, Arago and Poisson turned out to be judging theories unscientifically. The aim then is to construct a 'logic of empirical support' which *both* seems *a priori* plausible *and* captures the judgments of most prestigious scientists. Or, if this logic fails to capture some particular judgment of that kind, it should provide a convincing and historically well-supported account of why the judgment went awry. As we shall see, one of the consequences of the account of support to be defended in this section is that Fresnel and his contemporaries *as a matter of fact* judged the theoretical import of the straightedge and circular screen diffraction results exactly as they *ought* to have done, according to that account.

Most recent contributors have recognised that the support an empirical result lends to a theory is not merely a question of whether or not the theory entails the result, nor merely a question of the logical strength of the result itself (conjunctions generally giving more support than disjunctions, etc.). These contributors have recognised that even among single, 'atomic' experimental results (each deductively implied by some theory plus initial conditions), some results support the theory more strongly than others. The white spot episode, as I indicated earlier, has often been cited as evidence for the claim that what characterises those results which yield greater support is their *temporal novelty*. Results which were first discovered only as a consequence of some theory's predicting their existence weigh especially heavily in that theory's favour, more heavily than any fact for which the theory successfully accounts but which was already known before the theory was articulated.

This claim of the special role of temporally novel predictions has a long and distinguished history; but it is false. Those who have argued for it have made a rather natural mistake. They have noted that there are outstanding cases in the history of science where a theory logically entails a certain empirical result but the theory was intuitively accorded little or no support from that result. They have correctly concluded that there is some extra factor involved in empirical support beyond mere deducibility. They have then remarked that in all such cases the empirical result concerned was already known at the time the theory concerned was first articulated, and conversely that there are no cases where a theory logically entailed some hitherto unknown result and was *not* regarded as impressively supported by it. They have therefore concluded – this time incorrectly – that the extra factor involved in genuine support is *novelty of the evidence*.

Zahar and I have argued[20] that the important extra factor in empirical support – beyond mere deducibility of the evidence from the theory – is concerned with the way in which the theory was developed or constructed. In all cases where scientists have *not* recognised evidence *e* as fully supporting theory *T*, despite *e*'s following from it, *T* had been modified or tinkered with or otherwise developed precisely so as to yield *e*. Nothing hangs on whether we say in such cases that the theory is *not at all* supported by the evidence *e*. The important judgment to capture is that any such support is *less than* that accruing to a theory which entails the result without any tinkering or modification. Suppose that there are two rival theories, *T* and *T'*, and that *T* entails *e* directly and without artifice. *T'*, on the other hand, was, say, developed out of a more general theory which contained a free parameter, a parameter whose value had been fixed using exactly the evidence *e*. In such a case scientists will generally regard *T* as receiving *more* support from *e* than *T'*, even though both theories now entail *e*. Philosophers of science have often dealt with this phenomenon as a problem concerning scientific *explanation*: *T'* will not generally be regarded as explaining *e*, because it was introduced *ad hoc*, relative to *e*. The point can, therefore, be stated in a second way: it is wrong to regard the downgrading of *ad hoc* explanations and the apparent upgrading of genuine predictions as two *separate* methodological phenomena – they are at root the *same* phenomenon.

The approach to empirical support developed by Zahar and myself has been called the 'heuristic account'.[21] One advantage it has over the temporal account is that it comes equipped with a rationale. If the time-order of theory and evidence *was* in itself significant for scientists then we should, I think, be reduced merely to recording this as a brute fact. For why on earth *should* it matter whether some evidence was discovered before or after the articulation of some theory? (This was John Stuart Mill's main point in this part of his argument with Whewell.) On the other hand, there is a clear reason why it matters whether or not some evidence was involved in the construction of a theory. If theory *T* straightforwardly entails some observationally decidable statement *e*, then checking on the truth of *e* will, in general, constitute a test of *T* – *T might* have got this observable state of affairs wrong and hence have been refuted. And this is quite independent of whether or not *e* was already known. However, if some particular feature of *T* was in fact tied down on the basis of *e*, so that *T* had been engineered to entail *e*, then checking *e* clearly constitutes no real test

20. See, in particular, Worrall (1985). 21. See Musgrave (1974).

of T. It is obviously no test of T to ask it to get right some result which had been explicitly incorporated into it in the first place. In such a case even though e follows from T and hence not-e is, in Popper's terminology, a potential falsifier of T – it wasn't *really* a potential falsifier of T, since T was, because of its method of construction, never at any risk from the facts described by e. This shows, I think, that the intuitions behind the notion of a genuine test cannot be captured in purely logical terms but must involve consideration of how the theory concerned was constructed. (It is interesting that, while certain informal remarks by Popper show that he is aware of the dependence of falsifiability on heuristic factors, his formal account explicitly excludes such factors.) This rationale for the heuristic theory of support no doubt raises further problems and needs careful handling, but at least it provides *a* rationale for the heuristic account, while the temporal account seems to have no rationale at all.

A second argument for the heuristic account is that, unlike the temporal account, it seems to capture judgments made by working scientists in particular cases.[22] The Fresnel–Poisson case provides further evidence. The heuristic account makes a clear-cut pronouncement about this case: the novel prediction about the circular screen case and the implications about the already investigated straightedge cases count equally in support of Fresnel's theory, *provided* these latter facts were not used in the construction of the theory. This proviso was *clearly* met. Fresnel produced a unified theory of diffraction, covering all cases in a general way. No special conditions needed to be made for special cases. The whole precise theory followed, in a natural way, from general and rather abstract theoretical considerations. Fresnel in effect argued as follows. Consider a series of waves emanating from a point source and spreading out through some medium. The waves necessarily arrive at points in the medium more distant from the source *via* points which are less distant. We should therefore be able to take the points on *any* wave front and reconstitute the disturbance at any point more distant from the source as a certain composite – as, in fact, a composite of the separate motions which *would* emanate from each point of the chosen wave front if the effects of the disturbance of that point could be isolated from all others. Every point on the chosen wave front would, in other words, be regarded as a 'secondary source' of disturbances. The process of recomposition of the 'primary' wave must take into account the differences of phase of the disturbances from the secondary sources as they arrive at the more distant point. This consideration was, of course,

22. For examples, see Worrall (1978).

dictated by the principle of interference. The interference principle itself was derived by Fresnel from the assumption that light is a wave motion in an elastic medium, together with various simplicity assumptions. Fresnel's knowledge of the history of his subject was not great. He named the whole assumption about the recomposablity of a light wave out of secondary waves, the 'Huygens Principle', although in fact its connection with any doctrine advanced by Huygens is remote. This is how Fresnel himself introduced his theory of diffraction in his prize memoir:

> I am now going to show that one can give a satisfactory explanation and a general theory [of diffraction] within the system of waves *without the aid of any secondary hypothesis*, by depending only on the Huygens principle and that of interferences, which are the one and the other consequences of the fundamental hypothesis.[23]

Here Fresnel referred to the Huygens principle as a straight consequence of the fundamental idea of light as waves in a medium. This is not, however, correct. Some aspects of his derivation of the principle from the 'fundamental hypothesis' – notably concerning the 'obliquity factor' – never really satisfied those with sensitive mathematical consciences. Fresnel does seem to have been aware of some difficulties on this score – in one place, for example, he referred to the Huygens principle as only 'an *almost obvious* consequence of the fundamental hypothesis'.[24] Nonetheless Fresnel did show that the principle is a 'natural' consequence of the fundamental hypothesis in the sense that, while not following just from the hypothesis alone, it does follow from it, together with some plausible, and *general*, considerations. Fresnel's reasoning amounts to an attempt to reconstitute the initial or 'primary' wave in the unobstructed case out of the mutual interference of the 'secondary' waves relying only on general mechanical considerations: *no special case was made and hence no observational result about the propagation of light in any particular circumstances was involved at any stage.*

Given the Huygens principle, the basic idea behind any case of 'Fresnel diffraction' is in principle straightforward. A wave front is taken tangent to, or intersected by, the diffracting object. The resultant disturbance at any point beyond the object is then found by integrating the effects at that point, not from *every* point of the wave front (as in the case of the unobstructed wave) but instead just from every point on the non-occluded part of the wave front. The theory is that the diffract-

23. Fresnel (1819), pp. 282–3; emphasis added.
24. Fresnel (1819), pp. 294; emphasis added.

ing object in fact performs no positive action on the light and plays no role beyond that of simply absorbing the portion of the incident light which happens to fall on it. Apart from details, it makes no difference what the shape of the diffracting object happens to be – straightedge, narrow strip, circular screen or circular aperture – the method is always the same: just omit from the integration those 'secondary sources' blocked by the object. The shape of the object simply supplies the limits of the integral which is otherwise completely determined by the *general* theory (aside, that is, from one parameter corresponding to the wavelength of the light employed and hence not adjustable for the different cases of diffraction).

Given all this, it is obvious that the heuristic account is correct. No objective weight should be carried by the time at which the diffraction patterns concerned were actually discovered. So far as Fresnel's theory is concerned it is *nothing more than an historical accident* that the straightedge cases happened to be known before the theory was articulated while the circular screen case was investigated only later. Prior knowledge of the straightedge fringe patterns did not help, indeed *could not have helped,* Fresnel; the particular theory of these phenomena was generated by his general wave-theoretical approach or research programme in a way which left no room for adjustment to detailed observation. Hence these straightedge results were just as much a test of the theory as was the novel case of the circular screen. Although as a matter of fact it didn't, Fresnel's theory *could have* predicted the straightedge cases in advance. It is clear that Fresnel himself saw the situation in precisely this way, more than once emphasising that the important feature of his theory of diffraction was that it yielded the right empirical results 'without the aid of any auxiliary [or sometimes 'secondary'] hypothesis'.

The equal theoretical impact of the straightedge and circular screen results not only seems reasonable, it accords with the reaction of the French Academy's commissioners to Fresnel's work. It also agrees perfectly with the explicit reactions of some of the later scientists who worked on the wave theory. Here, for example, is Stokes reviewing the case of Fresnel's theory and the straightedge diffraction results some 50 years after the event:

> The theoretical distances of the several fringes from the geometrical shadow were a matter of pure prediction; for the only unknown quantity involved in the theoretical expression, the length of a wave, had been determined by Fresnel by independent methods . . . so that not a single arbitrary constant was left to be determined by some measurement of a fringe in some one particular

case, whereby an at least partial accordance between theory and observation might have been brought about.'[25]

Stokes talks about the straightedge results as matters of 'pure prediction', even though they were already known when Fresnel developed his theory. And the reason he does so is exactly the one emphasised by the heuristic account, namely, the absence of prior empirical tinkering with theory, indeed the absence of any room for such tinkering in respect of the empirical results concerned: 'not a single arbitrary constant was left to be determined by some measurement of a fringe'. Many scientists follow Stokes in intuitively characterising 'prediction' in this way. By emphasising the special role of prediction they can appear to be supporting the temporal, when in fact they are supporting the heuristic account. Thus I do not, of course, deny the importance of predictions – re-emphasised by Allan Franklin in his chapter – so long as it is understood that already known phenomena can be 'predicted'.

There are two complicating factors in this historical episode which I have not so far mentioned. Neither in fact endangers my case, although each might appear to. I shall end this section by considering them. The first complicating factor is that Fresnel added a great deal of quantitative detail to the already known straightedge diffraction results. The straightedge cases had certainly been extensively studied before Fresnel, but never in anything like such minute detail. A defender of the temporal account might therefore accept my argument that the straightedge cases supported Fresnel's theory just as well as the circular screen case but attribute this to the *novelty* of some of the quantitative evidence about straightedge diffraction. But the question of quantitative evidence is not at issue between me and the temporalist: everyone agrees that quantitative, detailed agreement between theory and evidence is especially striking support for the theory. Where we disagree is over the impact of the success of even the qualitative predictions in the straightedge and circular screen cases. History supports my view of equality of impact and thereby refutes the opposing view of the temporalist: the fact that straightedge diffraction cases had long been known about just does not seem to have signified at all for the commissioners.

A second complicating factor is the *possibility* that the white spot might have been observed before 1819. Whittaker asserts, in an enigmatic footnote to his treatement of the Poisson prediction, that the 'bright spot in the centre of the shadow had been noticed in the early part of

25. Stokes (1884), p. 63.

the eighteenth century by J.N. Delisle'.[26] He gives no reference, no further detail, and no consideration of how this earlier discovery – if indeed it occurred – affects his main claims about the episode.

I have not been able to verify Whittaker's claim. It might appear to be vital to my methodological case that I show that Delisle did *not* make this discovery, since otherwise the case does not provide a 'crucial experiment' between myself and the temporalist. If the white spot was *not* unknown before Poisson's prediction, then the temporalist too accords the success of that prediction no special significance. This consideration exposes an ambiguity in the temporalist's position. In order to count as a 'novel fact', must that fact have been entirely unknown to *anyone* before its prediction by theory? Is it enough if the fact was not known to any of the scientists in the field at the time the theory was first articulated? It is easy to imagine an investigator making an experimental discovery, even publishing it in a distant corner of some out of the way periodical, and that discovery not becoming widely known. Some years later no one in the field might have any knowledge at all of the result. Indeed this may have happened in Delisle's case. Even if Delisle *did* discover the white spot, it seems that no one on the commission, nor so far as I can tell anyone in the wider scientific community in the early nineteenth century, knew anything of it. Thus the white spot was certainly *new* to the scientists of the time, even if it had in fact been observed before. It seems clear that the temporalist has no choice but to adopt the latter characterisation of 'novel fact' – that is, he must count as 'novel' any fact which was not consciously known at the time, which was not part of 'background knowledge' at the time. The other, more 'objective' characterisation might at first seem more appealing for the temporalist: a fact counts more if it was predicted by the theory and was hitherto totally unknown to any human being. But how could scientists actually *apply* (even implicitly) the temporalist's criterion in this form? If Arago, Poisson and the rest really were applying this form of the temporalist's criterion, they would have had to have exhibited some mysterious sixth sense which allowed them to intuit that someone had earlier observed the white spot even though they were consciously unaware of this. So the temporalist cannot use Delisle as an escape route. The temporalist's position has to be that an empirical result is 'novel' at time *t*, if that result was not part of background knowledge at *t*, and *not* that it is novel at *t*, if unknown to *anyone* before *t*. Even if Delisle did observe the white spot before 1819, the result was certainly not part of 'background knowledge' in

26. Whittaker (1951), p. 108.

1819. Hence the temporalist *is* committed, Delisle or no, to according special significance to the white spot – contrary, as we saw, to the historical record. This discussion surely underlines the complete untenability of the temporalist view: the objective support which a theory receives from a piece of evidence cannot rest on such purely accidental features of history as whether or not a particular empirical result is discovered, but fails to attract sufficient attention and is subsequently forgotten.

CLARIFICATION AND DEFENCE OF THE 'ZAHAR–WORRALL CRITERION'

The white spot case, so often cited in favour of the view that there is something special about successful (novel) predictions, in fact supports the heuristic account. In this section I shall try to use the historical case to clarify some further aspects of this latter account and to defend it against some recent criticisms.

One perennial criticism is that the account makes theory-confirmation unacceptably vague and unacceptably subjectivist. If we really did need to know how a theory was constructed in order to decide on its empirical support, we would seem to need access to the psyche of its inventor. But who really knows what went on in Fresnel's mind, or in Newton's or in Einstein's, when arriving at their great theories? Why should such subjective considerations matter at all once the theory is 'on the table'?[27]

The Fresnel case indicates that these fears of vagueness and subjectivism are groundless. We need know nothing about Fresnel's psyche and need attend only to the development of his theory of diffraction as set out in great detail and clarity in his prize memoir.

Although it is his integrals which do the work of yielding the observationally checkable details, Fresnel did not simply lay these integrals down. Instead he painstakingly derived them from the Huygens principle – a principle which he derived in turn from general mechanical considerations. It is an important, though underemphasised, aspect of science that scientists do not simply propose their theories as mere conjectures to be evaluated, only afterwards, in terms of their empirical consequences. Instead they *argue* for their specific, detailed theories – indicating how they can be arrived at from more general (and perhaps widely accepted) considerations. These arguments, as accounts of the construction of detailed theories, are just as much in the public domain

27. See, for example, Musgrave (1974, 1978).

as is the theory itself. It is this fact which saves the heuristic account of empirical confirmation from hopeless vagueness and subjectivism. In order to decide whether or not the straightedge diffraction results, say, were involved in the construction of Fresnel's theory, one need not look at his psyche but only at his published papers.

As for the question of *why* it should matter, once a theory has been produced, *how* it was produced, my answer in outline is this. Whether or not it can be given some further rationale, we *do* seem to regard a striking empirical success for a theory as telling us something about the theory's overall – what? Truth? Verisimilitude? Probable truth? General empirical adequacy? Closeness to a natural classification? Take your pick. The reasoning appears to be that it is unlikely that the theory would have got this phenomenon precisely right just 'by chance', without, that is, the theory's somehow or other 'reflecting' the blueprint of the Universe. The choice between the 'chance' explanation and the 'reflecting the blueprint' explanation of the theory's success is, however, exhaustive only if a third possibility has been ruled out – namely that the theory was engineered or 'cooked up' to entail the phenomenon in question. In this latter case, the 'success' of the theory clearly tells us nothing about the theory's likely fit with Nature, but only about its adaptability *and* the ingenuity of its proponents. There is, of course, no suggestion that it is scientifically unacceptable to adapt a theory so that it incorporates an originally refuting (or independent) phenomenon. Indeed from the point of view of empirical applicability it is clearly vital so to amend the theory. Classical physics, as amended to include the Lorentz–Fitzgerald contraction hypothesis, is clearly scientifically superior to the unamended form in terms of empirical adequacy – since the amended form, unlike its predecessor, entails the correct result of the Michelson–Morley experiment. The point is only that – assuming the Lorentz–Fitzgerald hypothesis *was* merely an *ad hoc* device aimed at accommodating the result – the Michelson–Morley result gives no support to the *general framework* of classical physics. The straightedge diffraction results, on the contrary, support not just Fresnel's *specific theory* of diffraction, but also the general framework of the wave theory of light. This is because of the intimate connection, not dependent on any experimental results, between the general framework and the specific theory.

Because the specific theory of straightedge diffraction and the specific theory of small circular disc diffraction both follow 'naturally' from the 'fundamental hypothesis' of waves, because Fresnel *could not have* used the already known straightedge diffraction results (owing to the logical structure of his general and more specific theoretical ideas), there is no reason to differentiate between the straightedge and the circular disc in the support they give to this theoretical ideas. As we saw from the investigation of the prize commission's deliberations, in this respect at least, *real* history and its *real* rational construction coincide.

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Some more detailed criticisms of the 'heuristic' account of empirical support have recently appeared.[28] I decided that it would be more appropriate to respond to these detailed points in a separate (and forthcoming) journal article.

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