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# Constructing a 'revolution in science': the campaign to promote a favourable reception for the 1919 solar eclipse experiments

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> *A patriot fiddler-composer of Luton*
> *Wrote a funeral march which he played with the mute on,*
> *To record, as he said, that a Jewish-Swiss-Teuton*
> *Had partially scrapped the* Principia *of Newton.*
>
> *Punch*, 19 November 1919, p. 422

**Abstract.** When the results of experiments performed during the British solar eclipse expeditions of 1919 were announced at a joint meeting of the Royal Society and the Royal Astronomical Society, they were celebrated in the next day's *Times* of London with the famous headline 'Revolution in science'. This exemplified the general approbation with which A. S. Eddington and F. W. Dyson's results were received, the upshot of which was widespread approval for general relativity and worldwide fame for Albert Einstein. Perhaps because of Einstein's present reputation, there has been little historical analysis of why his theory should have been so celebrated on the basis of a single announcement of the results of one group's experiments. In this paper I argue that the remarkable public and professional success of the eclipse experiments was the direct result of a systematic and extended campaign by Eddington and Dyson and their associates to create interest in relativity theory, build an audience for the experiments, promote a favourable reception for the results and establish their work as a crucial experiment that would distinguish between the gravitation theories of Newton and Einstein. The campaign was motivated by Eddington's affection for Einstein's theory, and was successful largely because of Eddington's substantial credibility.

In November 1919 the attentions of the British popular and scientific press were aroused by the announcement, made at a joint meeting of the Royal Society (RS) and the Royal Astronomical Society (RAS), that Albert Einstein's law of gravity had been confirmed by observations made the previous May during a total solar eclipse.[1] *The Times* of London

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1 Very simply, the eclipse experiment was meant to test Albert Einstein's prediction, based on general relativity, that the path of starlight travelling to Earth would be deflected a specific amount by the Sun's gravitational field. The test was done during a total solar eclipse so that stars would be visible whose light passed near the perimeter of the Sun. Under normal circumstances, stars behind the sun are invisible because of the intensity of sunlight.

responded to the news from Astronomer Royal Frank Watson Dyson and the Plumian Professor of Astronomy at Cambridge, Arthur Stanley Eddington, with the now-famous headline, 'REVOLUTION IN SCIENCE. New theory of the universe. Newtonian ideas overthrown'.[2] Overall, the response in the public press ran the gamut from *The Times*' sensational banner to the *Daily Mail*'s light-hearted headline 'Light caught bending', and the commemorative verse at the head of this article. It is perhaps not surprising that an episode that is still considered epochal in the history of science should have been met with such immediate acclaim in the contemporary news media. Eddington's and Dyson's announcement set off such an extraordinary chain of events – culminating in the widespread acceptance of general relativity and the worldwide fame of Albert Einstein – that the 1919 eclipse expeditions' prominence in the history of science is unassailable. However, the veneration that relativity theory is now accorded in popular culture, and Einstein's present reputation as a transcendent genius, have served to excuse historians from explaining why the eclipse results should have been acclaimed so readily.

As John Earman and Clark Glymour persuasively demonstrated in their classic 1980 paper, the results themselves of the eclipse experiments were by no means an unequivocal confirmation of Einstein's theory.[3] Earman and Glymour argued that Eddington had been convinced by the aesthetic beauty of the theory that it must be correct. They also asserted that because he was a Quaker and a conscientious objector to the Great War, Eddington was hopeful that post-war hostility between British and German scientists would be eased if a British expedition confirmed the work of a Germanic physicist.[4] Ironically, after a masterful explanation of how 'Dyson and Eddington … threw out a good part of the data and ignored the discrepancies' in Einstein's favour, Earman and Glymour sanguinely concluded that the mood of those who might despair 'should be lightened by the reflection that the theory in which Eddington placed his faith … still holds the truth about space, time and gravity'.[5] With a reverence for the theory that seems to rival Eddington's own, it is unsurprising that Earman and Glymour gave only secondary attention to the question of why the announcement at the joint meeting (in their words) 'made Einstein an international celebrity, and put talk of relativity on the front page of the *Times* of London'.[6] This paper will build on their work in an effort to explain why scientists and members of the public would treat the results of first-time practitioners of a clearly difficult experiment – one that could not readily be replicated – as the crucial assessment of Einstein's theory. I will also address the issue of why Eddington's and Dyson's scientific audience offered so little dissent at the joint meeting where Eddington and Dyson announced the results.

In this paper I will argue that the extraordinary public and professional reaction was the direct result of a concerted effort by Eddington, Dyson and other members of the RAS

2 'Revolution in science', *The Times*, 7 November 1919, 12.

3 J. Earman and C. Glymour, 'Relativity and eclipses: the British eclipse expeditions of 1919 and their predecessors', *Historical Studies in the Physical Sciences* (1980), **11**, 49–85.

4 In fact, Earman and Glymour explain that the very reason Eddington was recruited into the eclipse expeditions was to help him avoid being treated as ignominiously as other conscientious objectors.

5 Earman and Glymour, op. cit. (3), 85.

6 Earman and Glymour, op. cit. (3), 49.

and RS's Joint Permanent Eclipse Committee (JPEC) to cultivate and define expectations for the experiments, and to promote a favourable reception for the results. I will argue that their efforts allowed them to construct an audience that had a specific understanding of their experiments, which would encourage its members to acknowledge the trials as the definitive test between the gravitational theories of Newton and Einstein. I will conclude that this episode of the history of science provides useful insight into the role of the audience in supposed crucial experiments, and will illuminate the way in which an experimenter must manipulate his audience in order for an experiment to be accepted as crucial.[7]

What concerns me in this article is not whether an experiment can actually be 'crucial' but, rather, what is required for an experiment to be perceived as crucial by those besides the investigator. Following Earman and Glymour, we can state with some confidence that Eddington was, like Einstein himself, sufficiently compelled by general relativity that he was convinced of its truth before it had been tested experimentally, and that he undertook the expeditions to get the evidence that he needed to prove Einstein correct. The question, then, was whether Eddington could make the experiments satisfactory and the results persuasive. In fact, he used his already ample (and to a great extent, unmatched) credibility with respect to relativity theory to present the experiments as rigorous and in favour of Einstein's predictions. The purpose of this paper, then, is to explain precisely how Eddington and others went about building this case and creating a receptive audience.

There is no reason to believe that these efforts were disingenuous or unnecessary, as history has shown that a potentially crucial experiment can fall at the hands of poor audience preparation. Isaac Newton argued that his self-proclaimed crucial experiment in 1666 proved the composite nature of white light. However, when he described the experiment and presented his conclusions to the Royal Society as self-evident, he was rudely disappointed by criticism from Robert Hooke. He had not revealed his conclusions using social and experimental protocol familiar to the RS, and he failed to take steps to prepare the audience to accept his experiment as a decisive test for the conclusions he wished to draw. Simply put, 'the difficulties with the reception of his theory that ensued in the following years derived from this mode of presentation'.[8]

7 The notion of a 'crucial instance', 'crucial test' or 'crucial experiment' that can distinguish definitively between two or more alternative theories or hypotheses, dates back to seventeenth-century natural philosophy. Though this concept was generally consented to through the nineteenth century, during the twentieth century the validity of crucial experiments was called into question in several branches of science studies. Pierre Duhem argued in 1906 that no physics experiment could be crucial in this sense because it would be impossible to 'enumerate completely the various hypotheses which may cover a determinate group of phenomena'. Building on the Kuhnian notion that scientific theories are only functional within the socially defined bounds of a given disciplinary matrix (paradigm), sociologists of scientific knowledge have argued that no experimental result is inherently meaningful, but can only be significant to those who share a common perception of the relation between observation and theory. Furthermore, of course, the perception of the degree of accuracy of empirical data is always subject to historical revision like that done by Earman and Glymour.

8 Rob Iliffe, 'Philosophical jostling: cavilling, flurting, and the perils of print', unpublished typescript. The issue of credibility was important in this instance as well. Simon Schaffer has argued that Newton's credibility as an experimenter was much higher in later years (1710s), when a wider audience did view Newton's experiment as crucial, than during the early years of the debate. Simon Schaffer, 'Glass works: Newton's prisms and the uses

Equally, there was precedent for strategy such as Eddington's proving successful and vital in the public's accepting experimental results as 'crucial'. Rob Iliffe has noted that, as the leader of 'a team of researchers who … devised a dramatic series of measurements to confirm theoretical doctrines [he] already considered unassailable', Eddington had a great deal in common with Pierre Moreau de Maupertuis.[9] Indeed, Maupertuis's expedition to Lapland in 1736 to 1737 to measure the curvature of the Earth is surprisingly comparable to the 1919 eclipse expeditions. Like the 1919 experiments to distinguish between the gravitational laws of Newton and Einstein, Maupertuis's work was represented as a crucial test of two hypotheses, the Cartesian theory of a vertically elongated globe versus the Newtonian prediction of a vertically flattened Earth. Iliffe argues that Maupertuis's evidence that the globe is flattened was made convincing only 'by means of the tactics and strategies that were deployed'. As we shall see, it was the public relations strategy used by Eddington and company that ensured that their work would be, like the Lapland expedition, a 'unique and dramatic *crucial* experiment'.[10]

## The 'false trichotomy'

In their account, Earman and Glymour did touch upon a vital aspect of Eddington's approach to represent the expeditions' work as a crucial experiment, namely that he 'repeatedly posed a false trichotomy for his deflection results'.[11] That is, he asserted that there were exactly three possible results of the experiments corresponding to three hypotheses that could be confirmed or disproved by measuring the degree that a ray of starlight passing tangentially to the Sun was deflected by the Sun's gravitational field: (1) if it were deflected 0.87″ this would confirm Newton's theory of gravitation by demonstrating that light is subject to gravity in the same way as any matter; (2) if the deflection were twice this, 1.74″, it would be proof of Einstein's theory of gravitation, which predicted that the speed of light would decrease in a gravitational field; and (3) if light were not deflected at all by gravity, then it would require a new understanding of the composition of light. Although there were, in fact, several other potential explanations for varying degrees of starlight deflection (such as the conjectured presence of a refractive atmosphere around the Sun), Eddington and company gave serious consideration only to the Newton and Einstein predictions. By eliminating other hypotheses, the 'false trichotomy' implied that any deflection greater than the Newton value (0.87″) must be due to the Einstein effect. Eddington in essence arranged a situation in which the greater the measured value of deflection, the higher the degree of proof that would be accorded to Einstein's theory. A story told later in life by Eddington suggests that this was a considered strategy: 'I remember Dyson explaining all this to my companion Cottingham,

of experiment', in *The Uses of Experiment* (ed. D. Gooding, T. Pinch and S. Schaffer), Cambridge, 1989, 67–104.

9 Rob Iliffe, '"Aplatisseur du Monde et de Cassini": Maupertuis, precision measurement, and the shape of the Earth in the 1730s', *History of Science* (1993), **31**, 336.

10 Iliffe, op. cit. (9), 365, 366. My italics.

11 Earman and Glymour, op. cit. (3), 84.

who gathered the main idea that the bigger the result the more exciting it would be.'[12] Earman and Glymour offer little clue as to when and where this trichotomy was publicized. It turns out to have been early, often, and everywhere possible. This article will show that Eddington and others drove this message home repeatedly to the public through the press and in person. Eddington used his position as the pre-eminent British authority on relativity to construct an audience that would be excited by the eclipse expeditions and primed to accept the results that he hoped the expeditions would produce. His work would be the crucial experiment to determine the validity of Einstein's theory, and his game plan played to Einstein's favour.

Eddington's efforts were reinforced by his extremely strong academic and institutional credibility. He occupied the prestigious Plumian chair of astronomy at Cambridge. As Secretary of the Royal Astronomical Society, he was the first man in Britain to be exposed to Einstein's general theory of relativity (via correspondence with Dutch astronomer Willem de Sitter during the Great War), and was the first British mathematician to comprehend it and begin contributing original work on the theory. He was also, just at the time of the joint meeting, pioneering British university education in the theory.[13] Given his credentials, Eddington was largely deferred to on explanations of both the theory and the practical aspects of the eclipse experiments. He was positioned perfectly to implement a strategy that would force the public to understand the experiments on his terms, which included an exclusive trio of possible results that vastly increased the likelihood that if Eddington found starlight deflection it would act as confirmation of general relativity.

## *The Times* before the 'revolution in science'

The link between newspaper reporting and Einstein's fame is well established in the secondary literature. Focusing on Einstein's renown in the USA, Marshall Missner has argued that 'the American press was *the* instrument that made Einstein into a celebrity'.[14] The conventional wisdom is that, heralded by *The Times*' 'Revolution in science' article, the joint meeting announcement precipitated public fascination with the creator of the important new theory of relativity. Indeed, Missner traces the diffusion of the 'Revolution in science' article to American newspapers after its appearance in *The Times* of London.[15] For all the notoriety accorded the 'Revolution in science' article, historians

12 A. S. Eddington, 'Forty years of astronomy', in *Background to Modern Science* (ed. Joseph Needham and Walter Pagel), Cambridge, 1938, 142.

13 Andrew Warwick, *Masters of Theory*, Chicago, forthcoming, Chapter 9.

14 Marshall Missner, 'Why Einstein became famous in America', *Social Studies of Science* (1985) **15**, 267–91 (original italics). On public fascination with Einstein, see also A. J. Friedman and C. C. Donley, *Einstein as Myth and Muse*, Cambridge, 1985.

15 Missner, op. cit. (14), 270. Missner's analysis of the role of newspapers in the spread of Einstein's fame is drawn largely from comparative readings of the journals themselves. Missner's paper does not mention that there are two secondary accounts of how *The New York Times* got hold of the joint meeting story from London. See Hillier Krieghbaum, 'American newspaper reporting of science news', *Kansas State College Bulletin* (15 August 1941), **XXV**, 1–73; and Meyer Berger, *The Story of* The New York Times *1851–1951*, New York, 1951. Taken in tandem, these reports are enigmatic since they disagree on a crucial point. (Both accounts are

have paid little attention to the publicity that preceded the eclipse announcements. Without such information, though, it is not entirely clear why a reporter from *The Times* would have been present at the joint meeting, or whether the general readership would have been concerned or even aware of the issue at stake. In fact the 'Revolution in science' article did not appear out of the blue. Rather, the eclipse experiments were a familiar topic by November 1919. The 'Revolution in science' article was the culmination of a series of ever more in-depth pieces in *The Times* about the eclipse expeditions and the forthcoming results. These articles appeared over the course of ten months prior to the joint meeting announcement. Crucially for my argument, there appears to have been extremely close contact between *The Times* and individuals related to the expeditions, with the newspaper serving as a valuable means by which the public could be informed and instructed how to understand the eclipse experiments.

The first mention in *The Times* of the eclipse itself or of the expeditions came on 13 January 1919 in a brief notice under the heading 'Coming eclipse of the Sun'. It read, 'The Astronomer Royal, Sir Frank Dyson, has arranged to send Dr. Crommelin and Mr. Davidson, of the Greenwich Observatory staff, to North Brazil to observe the eclipse of the sun which will take place at the end of May.'[16] Three months later readers were informed of the full scope of the upcoming venture in a substantial article 'Solar eclipse next month. British expeditions to Brazil and Principe'. The purpose of the eclipse experiments was explained under the subheading 'A physics problem'. Quoting from expedition member A. C. D. Crommelin's report of 6 February 1919 in *Nature*, the supposed trichotomy of possible results could not have been more explicitly stated: 'There are three possibilities: no shift, the half shift, or the full Einstein shift. The definite establishment of any one of the three as the truth would be an important addition to our knowledge of physics.'[17]

Readers of *The Times* were kept abreast of developments even as the expeditions were going on. On the day of the eclipse the newspaper carried an item on 'British observers' work', which covered the JPEC's observations in Principe and Sobral and a 'scheme of observation of the strength of radio-telegraphic signals during the eclipse [which was] arranged by a committee of the British Association'.[18] On 4 June *The Times* reported that the 'Astronomer Royal (Sir Frank Dyson) informs us that he received yesterday a cablegram from Professor A. S. Eddington and Mr. Cottingham from Princes Island, West Africa, stating that the eclipse of the sun was observed there through clouds, but

anecdotal and difficult to corroborate.) Berger makes the extraordinary claim that the impetus to cover the story came from New York in advance of the joint meeting, on the instruction of Carr Van Anda, a mathematics enthusiast who was the managing editor of *The New York Times*. I am hesitant to put much faith in this account, since Berger makes several factual errors such as placing the date of the joint meeting two days late on 8 November. Krieghbaum states that *The New York Times*' London correspondent (according to Berger, this man was a golf reporter named Henry Charles Crouch) took notice of the joint meeting coverage in London and wired a notice about it to Van Anda, who only then encouraged him to follow up on the story. It appears that Crouch's vigilance allowed *The New York Times* to cover the joint meeting on 9 November, one day earlier than other US newspapers (see Missner for the dates of the first US coverage). Many thanks to Matt Lavine for directing me to these sources.

16 'Coming eclipse of the Sun', *The Times*, 13 January 1919, 8.

17 'Solar eclipse next month', *The Times*, 22 April 1919, 16.

18 'Eclipse of the Sun to-day', *The Times*, 29 May 1919, 14.

they are hopeful of obtaining good results'.[19] The next day another notice ran stating that 'further telegrams from the British astronomers who observed the total eclipse of the sun last week report that the photographs taken at Sobral, Brazil, were quite successful, and the negatives already developed show all the stars that were expected to be recorded'.[20] Another longish article about the experiments appeared on 9 September, reporting on the quality of the expedition photographs and on the meticulous measurements that were presently being done on them. The purpose of the expeditions was reiterated, and readers were treated to several new details about the equipment used. They were also lured by the prospect of results in the near future:

> Asked whether the measurements so far made led to any fresh impressions concerning the constitution of light, Sir Frank Dyson limited himself to saying: – 'they disclose something, but what it is I am not prepared to state yet. It is a very curious position – but only one thing among many other things'.[21]

Rather than being an inexplicable case of editorial prescience, *The Times*' coverage of the joint meeting was to be expected given the nature of its previous reporting of the eclipse expeditions. Though the 'Revolution in science' article has generally been identified as a poignant symbol of the lively public response to the joint meeting announcement, it would be more accurate to view it as the logical conclusion to *The Times*' extensive coverage of the JPEC expeditions. As we have seen, this coverage was consonant with the publicity aims of the JPEC, as it placed the trichotomy of possible results into the public consciousness via the newspaper of record for British society. Any devout reader of *The Times* would, by 7 November 1919, have been very well informed about the purpose and implications of the expeditions, and would likely have been prepared to accept the announced results as decisive confirmation that 'Newton's ideas [had been] overthrown'.

I will now justify my earlier claim that the information within *The Times*' coverage of the expeditions appears to have been channelled directly from the JPEC members. As I have presented them so far (i.e. as they appeared to readers of *The Times*) the series of articles related to the JPEC expeditions and the eclipse experiments provide only a proximate cause for the 'Revolution in science' article. That is, while the 7 November article may be seen as the upshot of earlier eclipse coverage in the newspaper, we are left with the larger question of what or who may have prompted *The Times* to devote so much space to covering the expedition throughout 1919. One may imply from the articles' text that much of this impetus came from the astronomers. Evidence in *The Times*' archive supports the conclusion that both the impetus and much of the text itself came directly from JPEC members and their associates.

Within the pages of *The Times* none of the eclipse-related articles that I have cited was attributed to any author. However, *The Times*' Contributors Department maintained

19 'The eclipse', *The Times*, 4 June 1919, 12.
20 'The eclipse of the Sun', *The Times*, 5 June 1919, 16.
21 'Photographs of the eclipse', *The Times*, 9 September 1919, 12.

'marked copies' of the newspaper, which recorded the authorship or source of all editorial material published in *The Times*.[22] According to these records, one of the pieces (4 June) was attributed to Dyson himself, and two others (29 May, 5 June) were credited to Henry Park Hollis, who was Dyson's assistant at the Royal Observatory in Greenwich. Hollis was a past editor of the *Observatory* (1893–1912) and President of the RAS (1908–10), and had been working at Greenwich since 1881.[23] Perhaps not coincidentally, 1919 marked the beginning of his twenty-year association with *The Times* as astronomical correspondent. *The Times*' first mention of the eclipse expeditions (13 January) was taken from a syndicated report of the Press Association, which would have received its information from one of the participants, such as Dyson.[24] The other two articles were written by general reporters employed by *The Times*: John Griffiths Hartley (22 April) and Frederick William Long (9 September). As I have already mentioned, the content of Hartley's April article came from A. C. D. Crommelin's exposition of the physics problem under investigation. Long's information for the September article came from an interview he conducted with Dyson.[25] It appears that these reporters, who did not have an immediate connection with the expedition, found it largely necessary to transcribe information directly from JPEC members.

It is safe to conclude, therefore, that the JPEC members exerted considerable influence over the fashion in which news of the eclipse came to the public via *The Times*. The fact that *The Times* did not assign the story to a single correspondent to cover for the year suggests that the pressure to provide coverage was not generated within the paper's editorial office. *The Times*' archive cites five different sources for its coverage leading up to the joint meeting; what remains constant is a steady flow of information about the expeditions from the astronomers to the agents of the newspaper.[26]

22 Contributors Department's marked copies of *The Times*, TNL Archive, News International plc. I am deeply indebted to Nicholas Mays, deputy archivist of News International, for finding these records.

23 J. A. Venn, *Alumni Cantabrigienses* Part II, Vol. 5, Cambridge, 1953, 418.

24 According to Nicholas Mays, deputy archivist of News International,

> Press releases are a … recent invention. Those wishing to seek publication of information would have contacted newspapers or journalists directly by letter or telephone. The alternative was to contact news agencies such as [Press Association] or Reuters whose news service would have provided a general conduit for the spread of information.

Nicholas Mays, personal correspondence with the author.

25 In an article he wrote after the joint meeting (*The Times*, 8 November 1919, 12), Long refers to his having interviewed Dyson for the September article.

26 *The Times*' archivist Nicholas Mays supports this interpretation:

> I can confirm … that The Times did not have a correspondent with the expedition or use a correspondent based locally to the expedition's base. … As to how the information came to be published in the newspaper … the sponsors of the expedition would be the obvious source of the information which would no doubt have been provided to all interested editorial departments. Hollis would also have been an obvious conduit.

Nicholas Mays, personal correspondence with the author.

**Pre-eclipse publicity as popular science**

While the general reading public knew about the expeditions and their scientific goals through the information passed to *The Times*, those who read popular scientific publications or were members of scientific societies received even more information through the Eddington–Dyson publicity operation. I have already alluded to Crommelin's article in *Nature*, which was quoted in *The Times*. This lengthy article gave some general astronomical information about the coming eclipse as well as details of the JPEC's plans. Crommelin's explanation of the trichotomy went into greater depth than the excerpt added to *The Times* article revealed:

> Prof. Eddington has directed attention to the deduction that, since a ray of light carries energy, even apart from Einstein's theory, we should expect the same shift as would be produced by the sun's gravitation on a particle passing close to its surface with the speed of light; it is easy to show that this shift would be exactly half that predicted by Einstein, or 0.87″ at the sun's limb. There are thus three possibilities: no shift, the half shift, or the full Einstein shift. The definite establishment of any one of the three as the truth would be an important addition to our knowledge of physics. Should the decision be in favour of the Einstein shift, it would, in combination with the success of the latter in explaining the motion of the perihelion of Mercury, suffice to lead to its acceptance as the actual system of the universe. Its definite disproof would also be of service, since it would avoid the dissipation of further energy in its elaboration, though it would still deserve our admiration as an ingenious system of ideal geometry.[27]

It is interesting to note that *Nature*'s American counterpart, *Science* – sheltered as it was from the JPEC's efforts – contained no mention of the British expeditions or of the proposed relativity tests, either in the months leading up to the eclipse or in the interval between then and the announcement at the joint meeting. *Science* did, however, devote several columns of print in March 1919 to the worldwide magnetic observations that were organized for eclipse day by the Carnegie Institution.[28]

While Crommelin's article in *Nature* was more thorough than those in *The Times*, Eddington produced still more engaging and illuminating exposure for the JPEC expeditions through several public appearances, the contents of which were then reported in various publications. These often showed that Eddington was inventive and gifted at using analogies and visual aids to help those without a strong understanding of astronomy or mathematics to comprehend technical scientific concepts. He gave an excellent example of his skill in a lecture at the Royal Institution on 1 February 1918, which was recounted in detail in the weekly journal *Engineering*.[29] Eddington's talk was not solely about the forthcoming eclipse experiments; rather, it was an attempt to explain Einstein's general theory of relativity in a non-mathematical way. He first explained the Michelson–Morley experiment and Minkowski's space–time continuum using amusing anecdotes and a football bladder with a grid of lines drawn on it to illustrate the 'space–time world'. Having done so, he explained that the principle of relativity suggested shortcomings in Newton's law of gravity, since it did not hold up in instances of transformed space–time, so that 'we had to abandon either our assumption [about

27 A. C. D. Crommelin, 'The eclipse of the Sun on May 29', *Nature*, 6 February 1919, 444–6.
28 *Science*, 14 March 1919.
29 *Engineering: An Illustrated Weekly Journal*, 8 February 1918, 153–4.

relativity], or abandon Newton's law and find a new law of gravitation. That amended law – the only one possible to meet the requirements apparently – had been found by Einstein'. Again he illustrated his point, namely the inadequacy of Newtonian gravity and Euclidean geometry for instances of distorted space–time, with an analogy, this time of a circle drawn on a piece of paper with 'a heavy particle placed at the center … [which would give a non-Euclidean] ratio of circumference-to-diameter … slightly greater than pi'.[30]

He concluded his talk at the Royal Institution by discussing the ways in which Einstein's theory could be tested, giving particular emphasis to the method that he was himself to use the following year. Eddington used one of his favourite similes in explaining that according to Einstein's theory, 'A ray of light should be deflected like a bullet when passing an object exerting gravitational attraction.' He went on to say,

> With the enormous velocity of light only the sun would be able to produce an observable effect. The light ray from a star close to the sun should be slightly displaced; that might be tested next year when the totally eclipsed sun would be in a field of bright stars. The experiment might lead to one of three results: (1) A deflection amounting to 1.75 seconds of arc at the limb of the sun would confirm Einstein, but would be twice as great as otherwise expected; (2) a deflection of 0.83 second would prove that light had weight, but would overthrow Einstein's theory; (3) the absence of a deflection would show that light though possessing mass, had no weight, and hence the Newtonian law of proportionality between mass and weight would break down in another unexpected direction.[31]

Again, we can see that he missed no opportunity to expound the trichotomy of possibilities and, in so doing, to present the expeditions' work as a crucial experiment. As the pre-eminent British authority on relativity, Eddington's claim could hardly have been more credible. *Engineering* reported the existence of exactly three possible results as gospel truth, and there is no evidence that he was challenged during the lecture. The audience at the Royal Institution and the readers of *Engineering* consequently were primed to accept the outcome as dispositive proof of whichever theory's predicted deflection was closest to the announced result, and as such became part of the larger audience that Eddington had constructed.

Eddington made a similarly virtuosic performance at the meeting of the British Astronomical Association held on 27 November 1918, the large proportion of which was given over to his announcement that the JPEC was organizing two expeditions to observe the total solar eclipse. Once again, the substance of this appearance reached a wider audience, this time through the pages of the *Journal of the British Astronomical Association*. He explained that because the eclipse would occur before a field of bright stars, the usual programme of eclipse observation featuring varied goals and numerous instruments would be forgone. 'On this occasion everything is to be subordinated to the one purpose of *weighing Light*. I use the word "weigh" in its strictest sense, *i.e.*, measuring to what extent light is attracted by heavy bodies, in this case the Sun.'

30 These quotations come from the account in *Engineering* (hence the third-person references).

31 *Engineering: An Illustrated Weekly Journal*, op. cit. (29). The discrepancy between Newton values of 0.83″ and 0.87″ is based on the use of slightly different values for the constants in the equations from which the Newton value is derived. See Earman and Glymour, op. cit. (3), 52.

Eddington explained the difference between weight and mass, and described the planned experiment citing the Newtonian value (0.87″) as the predicted value of deflection. He continued,

> What I have said so far has nothing to do with the theory of Relativity; but there is another interesting point. According to Einstein's new theory of gravitation, the deflection will be twice as great as the amount I have mentioned ... So we have three possibilities to look out for.

He again explained the familiar trichotomy, but added,

> Of these, I think the first [no deflection] would really be much the most startling. I could not explain here precisely how the double deflection is reached on Einstein's theory; but the theory assigns a lower speed of light in a gravitational field, so that the Sun's field will act like a converging lens and thus cause a deflection.[32]

At this appearance, Eddington faced slight opposition to his claim that the experiment would be decisive. He was questioned about a potential difficulty in distinguishing between the gravitational effect and the refraction of the corona, and he conceded that this might be so, but evidently had success with the seemingly benign argument that the operative stars were so far from the Sun that the refraction would not be expected. Again, Eddington showed a penchant for using entertaining explanations to win over his audience:

> If light has weight in proportion to its mass like matter, I can give you some idea of what that weight is. There is an appalling amount of light in an ounce; in fact, the cost of light supplied by gas and electric light companies works out at something like £10,000,000 an ounce. This points the moral of Daylight Saving; the Sun showers down on us 160 tons of this valuable stuff every day; and yet we often neglect this free gift and prefer to pay £10,000,000 an ounce for a much inferior quality.[33]

Eddington was prolific in his employment of outlets besides technical journals and gatherings of professional astronomers as means whereby he could advocate the decisiveness of the eclipse experiments. Such use of diverse media was useful to Maupertuis in winning support for his Lapland measurements. Mary Terrall argues that 'Maupertuis devised a variety of legitimation tactics by addressing new audiences and multiplying the kinds of texts in which esoteric technical issues were discussed'.[34] Broad admiration for Eddington seems to flow through many of the accounts of his popular lectures on relativity and the eclipse experiments. He evidently possessed a highly developed skill for gauging the receptiveness of his various audiences towards the most notoriously 'esoteric

32 'Report on the meeting of the Association held on Wednesday, November 27, 1918, at Sion College, Victoria Embankment, E.C', *Journal of the British Astronomical Association* (1918–19), **29**, 35–9. Original italics.

33 Daylight saving was a controversial topic at this time. Although in 1907 London builder William Willett proposed advancing the clocks eighty minutes during summer months the first nations to institute daylight saving were Britain's enemies during the Great War. Germany and Austria advanced clocks one hour on 30 April 1916 as a wartime fuel-saving measure; other European nations including Britain followed suit within the next month. However, daylight saving time was abandoned after the war, only to be reintroduced in the 1920s.

34 Mary Terrall, 'Representing the Earth's shape: the polemics surrounding Maupertuis's expedition to Lapland', *Isis* (1992), **83**, 218–37.

technical issue', Einstein's relativity. Eddington's accomplishment at priming the popular-scientific audience is exemplary of the JPEC's overall success at bringing a particular understanding of the eclipse experiments to various sectors of the public.

## Priming the professional astronomers

Although Eddington was active before his departure in explaining relativity theory and the purpose of the JPEC expeditions in plain words to the interested public, he also strived to convince professional astronomers of the technical rigour of the crucial experiment. The pages of the monthly journal of astronomy, the *Observatory* (of which Eddington was an editor during this time), contained frequent references to the eclipse expeditions throughout 1919. These consisted largely of a continuing epistolary debate over details of the experiments, such as the ideal type of location for the observation positions. For example, John Evershed, an eclipse expedition veteran in his own right, recommended that the telescope lenses would perform most reliably at a sea-level site near a body of water, where daytime temperatures would be most consistent.[35] The letters to the *Observatory* also included criticisms of the experiment, such as that of Robert Jonckheere, who drew attention to three possible sources of refraction of a ray of light near the Sun (the Sun's atmosphere, the particles that may form the zodiacal light, and a possible effect due to more condensed ether in circumsolar regions), which he said 'need to be considered and definitely settled, especially as to their laws of refraction with regard to the distance from the Sun, before the test which will be afforded by the eclipse of May 1919'.[36]

Eddington responded in print to the objections of Jonckheere and others in a relatively long paper in the March 1919 issue of the *Observatory*. Among the pre-eclipse discourse that I have mentioned, this contained the most detailed and technical explication of the theory and practice of the planned experiments. Eddington discussed the equipment that had been chosen to obtain photographs of the star field behind the eclipse and the methods that he intended to use to evaluate them. He also made his case for the theoretical adequacy of the experiment to settle decisively the question of whether light was impervious to gravity, behaved like a particle according to Newtonian gravitation, or had decreased velocity in a gravitation field per Einstein's theory. Interestingly Eddington's tack had changed from that taken when he spoke at the British Astronomical Association some months prior. At the BAA he implied that the Newton value (0.87″ of deflection) was the most likely result, and only subsequently, and somewhat cursorily, discussed relativity and the Einstein value. In March 1919, on the eve of his departure for the observing station at Principe, he was unabashed in his favour for the Einstein value, as evidenced by the second paragraph of the *Observatory* paper:

> The prediction which it is hoped to confirm or disprove is that a ray of light passing near the sun will be bent, the path being concave to the Sun. It is easily seen that if this is so the star will appear

35 J. Evershed, Letter to the Editors, 'The Einstein effect and the eclipse of 1919 May 29', *Observatory* (1917), **515**, 269–70.

36 Robert Jonckheere, Letter to the Editors, 'The apparent deflection of stars near the sun as a proof of Einstein's theory of gravitation', *Observatory* (1918), **526**, 215–16.

displaced away from the Sun. More precisely a star whose true position is just on the Sun's limb will appear displaced 1″.75 from the limb; and for other stars the displacement is inversely proportional to the distance from the Sun's centre. This is the value according to Einstein's theory, but another less revolutionary hypothesis suggests a deflection of half this amount. It is important that the deflection, if it exists, should be measured sufficiently accurately to discriminate between the different theories.[37]

Eddington was also prepared to defend both the experimental protocol and Einstein's theory in responding to Jonckheere's criticism:

> Presumably the suggestion is that the condensation is due to the presence of the massive Sun, and that its effect is to modify the velocity of light. But a modification of the velocity of light in the neighbourhood of a massive body is just the effect we are looking for; so the suggestion amounts to a hypothetical explanation or illustration of the Einstein effect, and is not to be regarded as an alternative to it.[38]

He was now ranging, even before the expedition, into an approach that Rob Iliffe has recognized in Maupertuis's post-expedition strategy for credibility, when 'the work whose verification had been the expedition's goal now constituted its most significant support'.[39]

Even as Eddington's public messages about the eclipse expeditions and Einstein's theory became more self-assured, he took a pre-emptive strike against the possibility that he might come home with a null result, or no result at all. There were good odds that the latter circumstance would transpire. Indeed, prior attempts to test for the Einstein effect during eclipses in 1912 and 1914 had fallen prey to the exigencies of weather and war respectively. As recently as 1918, H. D. Curtis from the Lick Observatory in California had photographed an eclipse in hopes of measuring the deflection, but had been forced to do so through borrowed lenses, which were less than ideal, because the shipment of their own equipment had been stalled due to the demands of war. When Eddington and his colleagues departed on the eclipse expeditions, no results had yet been made public from the 1918 observations.[40] Eddington was doubtless acutely aware that his own venture was subject to many potential pitfalls (in 1912 he had travelled to Brazil to observe a solar eclipse but suffered heavy rain on eclipse day, which blocked the sun altogether[41]). The lingering effects of the Great War had already compromised their efforts since technicians had been unavailable to refurbish coelostat drive mechanisms for the telescopes until very shortly before departure. Eddington maintained that 'the chance of unfavourable weather is the chief but by no means the only apprehension. Nor can we ignore the possibility that some unknown cause of complication will obscure the plain answer to the question propounded'.[42]

37 A. S. Eddington, 'The total eclipse of 1919 May 29 and the influence of gravitation on light', *Observatory* (1919), **537**, 119.

38 A. S. Eddington, op. cit. (37), 122.

39 Iliffe, op. cit. (9), 367.

40 For a concise review of prior attempts to measure the Einstein effect, see Earman and Glymour, op. cit. (3), 60–71.

41 For an account of Eddington's first solar eclipse expedition (to view the eclipse of 10 October 1912) see Chapter 4 of A. V. Douglas, *The Life of Arthur Stanley Eddington*, London, 1956.

42 A. S. Eddington, op. cit. (37).

Eddington was contending with a classic problem for scientific expeditions: that of producing credible results in compromised conditions. In terms of producing a conclusive result, the prime technical challenge to the 1919 experiment was comparable to that faced by Maupertuis's expedition in the eighteenth century. The difficulty was that of making precise measurements with delicate instruments that had been transported great distances and were being operated in compromising conditions (heat for the telescope lenses; cold for Maupertuis's instruments). When the results were announced in both cases, the strategy used to counteract possible criticism was the delicate manoeuvre of broaching the topic in order to explain pre-emptively how these difficulties had been overcome. Rob Iliffe has astutely noted that these demanding conditions may come with corollary benefits: '[Maupertuis's expedition] functioned like an Archimedean lever … far enough away to be unchallengeable and authoritative. From the freezing North Maupertuis forced the burden of proof on to the French cartographers as if to say: do my measurements over again or agree with me'.[43] Eddington departed in the spring of 1919 with strong bona fides as an academic and with the rare opportunity to bolster his credibility by meeting the challenge of the expedition. Like Maupertuis before him, Eddington would control the decisive measurements. He could also be secure in the knowledge that he had done everything possible to prepare the audience to accept some degree of deflection greater than 0.87″ as crucial evidence in favour of his valued theory.

## Post-eclipse public relations

Once the concept of the trichotomy had been established, the vital question was whether the expeditions would produce reliable results. From the commencement of the expeditions until the results were announced, the publicity surrounding the JPEC's venture centred on the potential quality of the results, and what they might be. While the expeditions were under way, Eddington's heretofore dominant role as the public face of the venture necessarily ceased. It is also at this time that the publicity message was least consistent, as the potential quality of the Principe photographs came under question. Dyson, who remained in London, stepped to the forefront to keep astronomers and the general public alike informed of events taking place in the eclipse zone.

I have already described the way in which Dyson and Hollis fed information to *The Times*; Dyson also passed the correspondence that he received from the expedition members to the *Observatory* for publication. The May 1919 issue related that 'cablegrams have been received at the Royal Observatory, Greenwich, announcing the safe arrival of Messrs. Crommelin and Davidson at Para, Brazil, on April 3 and of Prof. Eddington and Mr. Cottingham at Principe on April 26'.[44] The June issue contained 'Stop press news', that cablegrams had been received from Principe and Sobral. It reported that 'the Sobral observations were "splendid" and at Principe the eclipse was observed through cloud, but with hopes of success'.[45] Dyson also gave updates to largely

43 Iliffe, op. cit. (9), 366.
44 'The solar eclipse of 1919 May 29', *Observatory* (1919), **539**, 217.
45 'STOP PRESS NEWS', *Observatory* (1919), **540**, 256.

the same extent at the May and June meetings of the RAS. However, at the June meeting he revealed that the JPEC had established codes to be used in the cablegrams sent after the eclipse observations had been made. While Crommelin and Davidson stuck to the code – 'splendid' meant that the eclipse had been unobscured by cloud – Eddington had broken the code to explain that he was still optimistic that his images would provide valid evidence despite the cloud cover. Notwithstanding Eddington's optimism, the fate of the Principe photographic plates immediately became an object of speculation. The *Observatory* published an article about all the investigations that had been carried out on May 29, including the Carnegie magnetic observations and the JPEC expeditions. It reported that 'news has been received at the Royal Observatory that Prof. Eddington and Mr. Cottingham are on their way back to England. Until they reach this country, no further information will be available as to whether the clouds at the time of totality were sufficiently thin to enable satisfactory photographs to be obtained'.[46]

Dyson contradicted this news at a remarkable July meeting of the RAS. It was the first time the society had met in July in over sixty years, and the occasion was a visit to London by the director of the Lick Observatory, W. W. Campbell, and several other American astronomers on their way to a conference in Brussels. Campbell discussed the results of Curtis's observations of light-deflection at the 8 June 1918 solar eclipse. Campbell gave an extremely detailed account of Curtis's data reduction, and stated that, in his opinion, Curtis's preliminary results precluded the 'larger Einstein effect, but not the smaller amount expected according to the original … hypothesis'.[47] Perhaps spurred by Campbell's presence, Dyson responded to RAS President Alfred Fowler's request for news from Eddington by revealing more details than he had released initially. This time the news came in a more pessimistic tone:

> [Eddington] obviously is greatly disappointed. He secured 16 photographs, but only for the last six was the sky clear enough to show any stars and on them he only got three, four, or five images; and as the sky was generally only clear on one part of the plate at a time, the stars secured on the plates are badly distributed. From his best plate, however, he has some evidence of deflection in the Einstein sense, but the plate errors have yet to be fully determined. The sky was clear ten minutes after totality.[48]

It is rather interesting to note that the joint meeting on 6 November was actually the second special gathering in London in 1919 held specifically so that the results of Einstein versus Newton eclipse experiments could be announced. The November meeting was reported widely by the news and scientific media and has gained a prominent place in the history of physics. In contrast, the RAS meeting of July 1919 was not mentioned in *The Times* and is barely an afterthought in the history of relativity. This lack of notoriety for the July meeting might be attributed in part to the fact that Campbell announced

46 'Eclipse news', *Observatory* (1919), **541**, 290.

47 For a detailed discussion of the Lick results, which were never published, see Earman and Glymour, op. cit. (3), 60–71.

48 'Meeting of the Royal Astronomical Society', *Observatory* (1919), **542**, 297–9. Dyson's interpretation of Eddington's mood was different than Eddington's own. By this time Eddington had experienced what he recalled as the greatest moment of his life, when 'the one plate that I measured [while still in Principe] gave a result agreeing with Einstein'. (Quoted from Eddington's notebook in Douglas, op. cit. (41), 40–1.)

preliminary results that seemed to reinforce the status quo. Of course, Campbell's announcement also differed from the famed joint meeting in that it was not preceded by any systematic publicity aimed at constructing a receptive lay and scientific audience.

When Eddington returned he completed the final measurements and resumed his position as the mouthpiece of the JPEC. Even as he wrangled with the measurements in Cambridge, carrying out the curious process (described by Earman and Glymour) of emphasizing some data and rejecting others, Eddington continued making public appearances to discuss the expeditions. He spoke at the 12 September meeting of the British Association for the Advancement of Science (BAAS) in Bournemouth, where his assessment of the situation belied the conviction with which he would make the final announcement of results in seven weeks' time. At the BAAS Eddington announced that the measurements that had been made so far suggested a value of deflection 'intermediate between the two theoretically possible values 0.87″ and 1.75″'.[49] However, he was not loath to admit that 'he hoped that when the measurements were completed the latter figure would prove to be verified'.[50] After giving news of the update, Eddington led a discussion on relativity theory, at which point Oliver Lodge stated that he regarded special relativity 'as a supplement of Newtonian dynamics … [and did not see it] as entailing any revolutionary changes of our ideas of space and time, or as rendering necessary the further complexities of 1915 [general relativity]'.[51] He further stated that 'he hoped the results obtained would be definite and show a displacement of 0.87″'.[52] Ludwig Silberstein, who considered himself Eddington's peer with respect to his understanding of relativity, 'drew attention to the fact that St. John's results in 1917 [examining another prediction of general relativity, the red shift] showed no shift of the spectral lines, a fact which in itself would overthrow the theory in question'.[53]

Given this type of opposition, and the seeming ambiguity of the measurements, why were so many prepared to believe that Einstein's theory had been confirmed when Dyson and Eddington announced it at the joint meeting? I contend that the answer must lie in the JPEC members' superior public relations. Eddington's talk was recounted in both the *Observatory* and the *Report of the British Association*, the former of which barely mentioned the opposition,[54] and the latter of which endorsed the authority of Eddington's anticipated announcement by noting that to 'establish the existence of such an effect [displacement] and the determination of its magnitude gives, *as is well known,* a *crucial test* of the theory of gravitation enunciated by Einstein'.[55] Here was a correspondent citing the crucial experiment claim as common knowledge despite witnessing Eddington's two vocal rivals contesting that very assertion. This is strong evidence that by this time, less than two months before the joint meeting, Eddington's prolificacy was being rewarded as his message was reaching its target.

49 *Report of the British Association*, 1919, 156.

50 *Report of the British Association*, 1919, 156.

51 *Report of the British Association*, 1919, 156.

52 'Astronomy at the British Association', *Observatory* (October 1919), **544**, 365.

53 *Report of the British Association*, 1919, 156.

54 'Astronomy at the British Association', op. cit. (52), 365. It receives six lines of mention in nearly two pages addressed to Eddington's talk and discussion.

55 *Report of the British Association*, 1919, 156. My italics.

## Announcing the results

Having made a concerted effort to create a receptive audience for their experimental results, the question remained of how and when Dyson and Eddington should announce the results. Actually, this ought to have been largely predetermined. Although Eddington and Dyson had revealed selected details of the ongoing data analysis, there was an established protocol by which the results were supposed to be made public. Indeed, though the 1919 joint meeting is often described as an exceptional episode in the history of science, the meeting itself was by no means unprecedented. Joint meetings of the Royal Society and the Royal Astronomical Society were held on 28 June 1900, 31 October 1901 and 19 October 1905 for the express purpose of announcing the results of solar eclipse observations.[56] All of these meetings, including the momentous one on 6 November 1919, were held in accordance with regulations that had been established in 1899 to govern the affairs of the JPEC. In that year, a 'Committee appointed on Dec. 8, 1898 to consider the relations of the Royal Society and the Royal Astronomical Society in the Constitution of the Joint Permanent Eclipse Committee' set up the ground rules by which that collaborative venture would operate. At the meeting of the Council of the Royal Society on 16 February 1899 the committee proposed a series of regulations, of which Number Six outlined the procedure by which the JPEC would publicize its results:

> As soon as practicable after the return of an Eclipse Expedition, a joint meeting of the Royal and Royal Astronomical Societies shall be held, at which the observers shall give the first and preliminary accounts of their observations. The meeting shall be held under the general rules of a Discussion Meeting of the Royal Society and the proceedings thereat shall be published identically in the 'Proceedings of the Royal Society' and the 'Monthly Notices of the Royal Astronomical Society'. … Should any observer be unable to prepare his preliminary account in time for the meeting, opportunity shall be afforded for its publication in a like manner in some subsequent number of the 'Proceedings of the Royal Society' and the 'Monthly Notices of the Royal Astronomical Society'.[57]

The regulations were approved at the following RS council meeting (held on 16 March 1899) upon receipt of a letter of approval from the secretaries of the RAS.[58]

There was no question, then, of the form in which the JPEC members would make the official first announcement of the results. I have already shown that non-committal reports that could well be construed as 'the first and preliminary accounts of their observations' had actually been made weeks before the joint meeting held on 6 November, for example at the BA meeting on 12 September. Such fragments of information may have served to intensify the public anticipation for the joint meeting, where the conclusive results would finally be announced, ostensibly for the first time. In actuality, as I will show, by the time of the joint meeting even the final results had been announced to a select group.

56 British astronomers made over two dozen eclipse expeditions between 1860 and 1914. For an excellent account of this phenomenon, see Alex Pang, 'The social event of the season: solar eclipse expeditions and Victorian culture', *Isis* (1993), **84**, 252–77.

57 *Royal Society Minutes of Council 1898–1903*, Vol. 8, Royal Society Library, London.

58 *Royal Society Minutes of Council 1898–1903*, op. cit. (57).

On 22 October 1919, more than two weeks before the joint meeting, Eddington attended the 83rd meeting of the $\nabla^2V$ Club at Cambridge University, a limited-membership society for the 'discussion of questions in Mathematical Physics'. In Ebenezer Cunningham's rooms, Eddington prepared to tell the elite mathematical physicists at Britain's premier centre for that discipline that the results had confirmed Einstein's theory. As it was recorded in the club's minute books,

> The President [Cunningham] called on Professor Eddington to read his paper on 'The Weight of Light'. The paper dealt with the attempt to verify experimentally Einstein's Theory of Gravitation based on the Principle of Relativity. The practical problem consisted in determining the effect of the Sun's gravitational field on the path of a ray of light of a star. The Eclipse of 1919 May 29 was extremely favourable, the eclipse field being situated in the Hyades. There were 3 possibilities:
>
> (a) No observable deflection of a ray of light at the Sun's Limb.
> (b) Deflection of 0″.87 at Sun's Limb, this being the amount calculated from the older gravitational theory.
> (c) Deflection of 1″.75 calculated from Einstein's Theory of Relativity.
>
> Professor Eddington described the difficulties attending the measures of such small quantities and how the differences due to the scales of the Eclipse and comparison plates were allowed for. Of 16 plates exposed, 6 showed images of 5 stars. From the measures of the plates, he deduced a deflection of 1″.60 at the Sun's Limb the P.E. [probable error] being +/−0″.30. He announced that the deflection observed by the Greenwich astronomers at Sobral pointed to the value 1.75 with a p.e. of 6%. These results confirm Einstein's Theory.
>
> A general discussion followed; the President remarked that the 83rd meeting was historic and that the results announced there would probably involve the changing of the name of the club from $\nabla^2V$ to something more barbaric. The meeting was declared social a little before midnight.[59]

There is a great deal of circumstantial evidence to suggest that the preliminary announcement at the $\nabla^2V$ Club was a key component of Eddington's and Dyson's decision to make a public disclosure of the results on 6 November. The eighty-third meeting of the club was the first in three and a half years, since May 1916. Following its reunion, regular meetings were held during the rest of 1919 and through 1920, but Eddington was not present at any of those sessions. At the eighty-seventh meeting (11 May 1920), Eddington 'ceased to remain [a member] under rule 14', by which resident members were ousted for non-attendance at four consecutive meetings. Apparently in 1919 his interest in the club did not extend beyond the meeting at which he spoke. Though there is no unequivocal evidence that Eddington himself acted to reunite the club (of which he had been a member since 1904), circumstances suggest that he may have convened the meeting to serve

59 Minute Books of the $\nabla^2V$ Club, University of Cambridge Department of Applied Mathematics and Theoretical Physics. I am grateful to the Department of Applied Mathematics and Theoretical Physics for permission to publish these extracts. The contents of these minute books are reproduced on microfilm in the *Archive for the History of Quantum Physics*. $\nabla^2V$ is the Laplace operator, developed in Pierre Simon Laplace's masterpiece of Newtonian analysis *Méchanique céleste* and ubiquitous in mathematical physics. Cunningham's comment about changing the name of the club likely referred both to the fact that Einstein's complex mathematics had now superseded the Newtonian mechanics of the elegant Laplace operator, and (in the wake of the Second World War) to Einstein's German origin. Eddington's $\nabla^2V$ Club announcement is not common knowledge in the secondary literature; it is mentioned in the context of Eddington's relativity teaching at Cambridge in Warwick, op. cit. (13), Chapter 9, the source to which I owe my knowledge of the event.

as a private forum where he could run through his announcement of the results. The evidence that does exist indicates that the reception Eddington received at the $\nabla^2$V Club meeting prompted a defining moment in his own assessment of the results' readiness for publication.

The joint meeting had not even been scheduled when Eddington presented the results in Cambridge on 22 October. However, Dyson took action the following day at a meeting of the Council of the Royal Society. On 23 October,

> The Astronomer Royal drew attention to the results obtained by expeditions sent out to observe the recent eclipse of the sun, and on his motion it was Resolved – That Thursday afternoon, November 6, be set apart for a joint meeting of the Royal and Royal Astronomical Societies to Receive and discuss the first and preliminary accounts of the observations, in accordance with the arrangement agreed to between the two Societies in 1899.[60]

Thanks to ongoing research by Matthew Stanley, it is clear that Dyson was in close contact with Eddington at this time, so he was not acting unilaterally when he scheduled the joint meeting.[61] Eddington wrote to Dyson on 21 October 1919, the day before the $\nabla^2$V Club meeting, to discuss some details of his progress in preparing the results for publication. He did not advise Dyson to schedule a public announcement of the results, but he confirmed that he wanted to meet Dyson in London. Earlier in the month they had arranged to meet in London on 23 October, which we know to be the day after the $\nabla^2$V Club meeting.[62] If they did meet in person on 23 October before the Royal Society's afternoon meeting, then perhaps Eddington told Dyson that he was now satisfied with the results. The fact that Eddington claimed the paper needed some final revisions when he wrote to Dyson on 21 October suggests that the $\nabla^2$V Club meeting coincided to the day with Eddington's decision that the results were ready to be publicized.

Why might Eddington have placed so much stock in the reception he was accorded at the $\nabla^2$V Club meeting? A closer look at his audience proves to be revealing. The men to whom Eddington addressed his $\nabla^2$V Club announcement comprised a remarkable collection of mathematical physicists, some of whom had substantial experience with Einsteinian relativity by British standards of the 1910s.[63] Ebenezer Cunningham, who hosted the meeting and acted as president, 'is widely remembered as the man who introduced relativity theory into Britain and acted as its chief spokesman prior to World

60 *Royal Society Minutes of Council*, Vol. II, 1914–20, Royal Society Library, London.

61 Matthew Stanley, personal correspondence with the author, and Matthew Stanley, '"An expedition to heal the wounds and desolation of war": British astronomy, the Great War and the 1919 eclipse', *Isis*, forthcoming. Stanley sheds a great deal of light on the source of Eddington's public relations savvy and offers new insight into the process by which Eddington reduced the expeditions' data.

62 The meeting between the two was arranged at least two weeks earlier, as it is mentioned in a letter from Eddington to Dyson written on 7 October 1919. Thus there would have been ample time for Eddington to schedule a meeting of the $\nabla^2$V Club in advance of his meeting with Dyson. Full citations for the letters of 7 October and 21 October may be found in Stanley, op. cit. (61). Thanks to Matt Stanley and Adam Perkins of the RGO archives for passing along information about these letters.

63 There were seventeen attendees at the 22 October meeting: H. F. Baker, A. S. Eddington, J. A. Crowther, G. F. C. Searle, E. Cunningham, C. T. R. Wilson, H. Jeffreys, W. M. H. Greaves, J. E. P. Wagstaff, F. P. White, G. H. Henderson, A. L. McAulay, W. M. Smart, E. A. Milne, S. Lees, A. R. McLeod and L. A. Pars.

War I'.[64] He wrote the first English book on relativity,[65] although by 1919 he had been surpassed by Eddington as the pre-eminent British expositor of the theory. G. F. C. Searle of the Cavendish Laboratory was, by virtue of his close acquaintance with several German physicists, the only British scientist with whom Einstein corresponded about relativity before the watershed year of 1919.[66] L. A. Pars was Eddington's first research student; he became proficient in general relativity and won the Smith's Prize in 1921 for a two-part paper on 'Geometrical vector theory and the restricted principle of relativity' (Part I) and 'On the general theory of relativity' (Part II).[67] As other historians have noted, few British mathematical physicists had even attempted to grasp relativity prior to the notoriety it received in the wake of the joint meeting announcement. That several members of the $\nabla^2V$ Club had first-hand experience with relativity should be considered exceptional, and indicative of the high calibre of physicists in Eddington's audience.[68]

The $\nabla^2V$ audience was qualified not only to evaluate the theoretical points of Eddington's announcement, but also to judge the merit of the astronomical work that went into producing his result. The $\nabla^2V$ Club members included some of the nation's most distinguished astronomers. H. F. Baker was the University of Cambridge Lowndean Professor of Astronomy from 1914 to 1936. W. M. Smart, who acted as secretary during

64 Andrew Warwick, 'Cambridge mathematics and Cavendish physics: Cunningham, Campbell and Einstein's relativity 1905–1911. Part I: The uses of theory', *Studies in History and Philosophy of Science* (1992) **23**, 625–56, 626.

65 Ebenezer Cunningham, *Principle of Relativity*, Cambridge, 1914.

66 Andrew Warwick, 'Cambridge mathematics and Cavendish physics: Cunningham, Campbell and Einstein's relativity 1905–1911. Part II: Comparing traditions in Cambridge physics', *Studies in History and Philosophy of Science* (1993), **24**, 1–25. Although Searle received an unsolicited copy of a paper on relativity directly from Einstein in 1909, Warwick shows that at the time Searle 'found the work unimportant and impenetrable' (ibid., 14). However, he may have passed the paper to Norman Campbell, his colleague at the Cavendish Laboratory whose publications began to reveal his familiarity with Einstein's work beginning in September 1909 (ibid., 18).

67 D. R. Taunt, 'L A Pars', *Bulletin of the London Mathematical Society* (1986), **18**, 505–8. Warwick, op. cit. (13), Chapter 9.

68 Andrew Warwick has made an extremely close examination of the transfer of both special and general relativity to Cambridge, which was Britain's leading centre for mathematical physics training and research. In Warwick, op. cit. (64), he argues that Cambridge mathematical physicists (exemplified by Cunningham) interpreted Einstein's 1905 paper on special relativity in a manner uniquely conditioned by local training in and research on the electronic theory of matter. In Warwick, op. cit. (66), he compares the Cambridge mathematical-physicist reading of Einstein to that of Cambridge experimental physicists (exemplified by Norman Campbell) who viewed special relativity as a new theory of measurement. Warwick's study of the ways in which various Cambridge physicists interpreted Einstein's paper in ways that were useful to their own work is shaped by his contention that immediately after 1905 there was no essential Einsteinian theory of special relativity for physicists to 'receive'. For an account of 'the complex process through which in Germany … many relativities with many histories could become singular … and through which the work of Einstein came to be sharply distinguished from that of others' see Richard Staley, 'On the histories of relativity: the propagation and elaboration of relativity theory in participant histories in Germany, 1905–1911', *Isis* (1998), **89**, 263–99. In Warwick, op. cit. (13), Chapter 9, he extends his treatment to incorporate general relativity, focusing on Eddington. Warwick claims that among similarly trained Cambridge mathematical physicists Eddington's unusual affinity for and success with general relativity can be attributed largely to his perspective as an astronomer. This allowed him to ignore general relativity's ominous implication for the ether, to which many other Cambridge mathematical physicists were committed.

the meeting, was John Couch Adams Astronomer at Cambridge. W. M. H. Greaves became Dyson's chief assistant at Greenwich and, later, President of the RAS. Harold Jeffreys was another Fellow (and future President) of the RAS who would follow in Eddington's footsteps as Plumian Professor of Astronomy. E. A. Milne was on the verge of becoming Assistant Director of the Solar Physics Observatory at Cambridge, which he did in 1920, and he too was later President of the RAS.[69] Milne also went on to study relativity in some detail, and in the 1940s he made a noted, if not successful, critique of Einstein's general relativity theory, introducing his own theory of 'kinematic relativity'.[70]

In light of his $\nabla^2V$ Club colleagues' expertise, it is not difficult to comprehend why Eddington would have announced the eclipse results to them before doing so in public. It is also easy to see why after their endorsement he was convinced that the results were ready for publication. By coming to the $\nabla^2V$ Club first Eddington could gauge the type of reaction that his momentous claim would produce. If necessary he could answer the members' queries and address their possible objections in private, rather than doing so in the glare of the spotlight in London. In view of the fact that the $\nabla^2V$ Club meeting was not declared social until nearly midnight, we can suppose that there was a considerable amount of discussion following Eddington's presentation. Based on Cunningham's very congratulatory benediction it is clear that Eddington won over his audience. Thus he had convinced many of the nation's pre-eminent mathematical physicists and astronomers that the eclipse expeditions had confirmed Einstein's theory. Any criticisms that were raised along the way were likely to be valuable to Eddington in preparing to make the public announcement. He now also knew that he could count on his announcement in London receiving no resistance from the members of the $\nabla^2V$ Club, several of whom were Fellows of the RS or RAS and might be expected to attend the joint meeting.[71] Furthermore, those at the joint meeting who might protest the decisiveness of the experiment would stand at theoretical odds with the Cambridge network and its unrivalled credibility.[72] Significantly, this network included J. J. Thomson, who in his capacity as President of the Royal Society would chair the joint meeting. He was Cavendish Professor of Experimental Physics up to 1919 and remained Master of Trinity College, Cambridge until 1940. Although Thomson was not a member of the $\nabla^2V$ Club, he would almost certainly have been informed of the proceedings given that Milne, the club secretary Smart, and Eddington himself, were all Trinity men. It is noteworthy that Thomson did throw his weight behind Eddington's conclusions even as he presided over the joint meeting.

69 Thanks to Peter Hingley of the RAS for helping to confirm details on the astronomers at the $\nabla^2V$ Club.

70 W. H. McCrea, 'Edward Arthur Milne', *Obituary Notices of Fellows of the Royal Society of London* (1950–1), 7, 421–43.

71 As I will discuss below, there is unfortunately no record of which I am aware that lists the identity or even the number of attendees of the joint meeting.

72 Cambridge links many actors in this paper. The three stalwarts of the JPEC, Dyson, Crommelin and Eddington, were all graduates of Cambridge. Henry Park Hollis, who conveyed news of their work to the public via *The Times*, was also a Cambridge graduate.

## Reaction to the announcements at the joint meeting

The full impact of Eddington's and Dyson's publicity campaign and audience preparation is revealed in the events surrounding the joint meeting of 6 November 1919. Their efforts in the previous months had produced public and scientific audiences that apparently were eager to learn the results of the eclipse expeditions. Their astute portrayal of the JPEC expeditions as a crucial experiment to distinguish between three possibilities placed them in a position to present results that had been difficult to derive and in fact pointed towards no single value for deflection as decisively in Einstein's favour. Finally, Eddington's omnipresence publicizing the experiments had reinforced his well-deserved credibility as Britain's expert on Einstein's theory, to the extent that it seems many observers were conditioned to defer to Eddington's authority. If Eddington did have any private doubt about the esteem with which his judgement would be received, I have argued that it was probably assuaged two weeks earlier when he had won over Cunningham and the rest of the $\nabla^2 V$ Club members. (Cunningham's own authority was sufficient that in the aftermath of the joint meeting he was able to respond to curiosity in scientific circles by writing a three-part paper for *Nature* on 'Einstein's relativity theory of gravitation'.[73]) These factors render comprehensible the collective acclamation bestowed upon Einstein and the JPEC members by the audience at the joint meeting.

It seems that little has been written about precisely who made up the audience at the meeting besides those who merited mention in first-hand accounts of the proceedings. This may be attributed to the fact that neither the RS nor the RAS has a record of attendance. Even so, it is possible to estimate the number of attendees at about a hundred to 150 persons.[74] Every account with which I am familiar confirms my argument that the publicity surrounding the experiments had succeeded in creating an audience with 'the greatest possible interest' in hearing the final results.[75] The best testament to the audience's eagerness comes from a first-hand account by A. N. Whitehead:

> It was my good fortune to be present at the meeting of the Royal Society in London when the Astronomer Royal for England announced that the photographic plates of the famous eclipse, as measured by his colleagues in Greenwich Observatory, had verified the prediction of Einstein that rays of light are bent as they pass in the neighbourhood of the sun. The whole atmosphere of

73 Ebenezer Cunningham, 'Einstein's relativity theory of gravitation', 3 parts, *Nature*, 4 December 1919, 354–6; 11 December 1919, 374–6; 18 December 1919, 394–5.

74 The meeting took place 'in the rooms of the Royal Society', which were in Burlington House, Piccadilly, from 1857 to 1967. A paper by D. C. Martin on 'Former homes of the Royal Society', *Notes and Records of the Royal Society* (September 1967), **22**, 12–19 shows a floor-plan giving dimensions of the 'Meeting Room' (45 feet by 36 feet) and two photographs taken from the rear of the room showing that seating was in pews. Because the room had columns along either side that appear on the floor-plan and in the photographs, one can be confident that the photos show the full seating capacity of eight pews on either side of a central aisle. If the room was crowded, as it apparently was at the joint meeting (see, for example, R. W. Clark, *Einstein: The Life and Times*, New York, 1971, 232), the pews could have held about one hundred people. There was room for perhaps thirty or forty people to stand, if necessary, plus a large (29 feet by 36 feet) anteroom adjacent to the meeting room where overflow capacity could have stood. Thanks to Andy Warwick for suggesting this estimation method, and many thanks to the ever-helpful Peter Hingley of the RAS and to Clara Anderson of the RS who directed me toward the Martin article.

75 This quotation is from *The Times*' account. *The Times*, 7 November 1919, 12.

> tense interest was exactly that of the Greek drama: we were the chorus commenting on the decree of destiny as disclosed in the development of a supreme incident. There was dramatic quality in the very staging: the traditional ceremonial, in the background the picture of Newton to remind us that the greatest of scientific generalisations was now, after more than two centuries, to receive its first modification. Nor was the personal interest wanting: a great adventure in thought had at length come safe to shore.[76]

Evidently months of publicity plus the post-expedition drama over the results had built the audience's anticipation to a crescendo.

Dyson, Crommelin and Eddington spoke (in that order) for the JPEC. They announced the final values for the deflection as 1.98″ from Sobral and 1.61″ from Principe, and Dyson stated that

> after a careful study of the plates I am prepared to say that there can be no doubt that they confirm Einstein's prediction. A very definite result has been obtained that light is deflected in accordance with Einstein's law of gravitation.[77]

Eddington's attitude was one of complete confidence in the results. He even stated that the now infamous cloud-cover at Principe was a blessing in disguise, since 'the Sun's rays could not seriously affect the mirror by heating it'. Most importantly he emphasized the decisiveness of his crucial experiment, regardless of difficulties that might arise in other trials of general relativity:

> for the full effect which has been obtained we must assume that gravity obeys the new law proposed by Einstein. This is one of the most crucial tests between Newton's law and the proposed new law. This effect may be taken as proving Einstein's *law* rather than his *theory*. It is not affected by the failure to detect the displacement of Fraunhofer lines on the Sun. If this latter failure is confirmed it will not affect Einstein's law of gravitation, but it will affect the views on which the law was arrived at. The law is right, though the fundamental ideas underlying it may yet be questioned.[78]

As Earman and Glymour have demonstrated in great detail, the initially 'disappointing' results from Principe were given great weight while results from one of the two telescopes at Sobral (where the weather was clear), which had initially given a value of 0.97″, were discarded altogether and not mentioned at the meeting.[79] If the audience had been willing to consider the multitude of possible explanations for light deflection, widely varying results may not have been particularly convincing. However, because Eddington and Dyson had established the trial as a crucial experiment in which one of the three possibilities must come out the winner at the expense of the others, nearly every member of the well-prepared audience seems to have accepted that findings of ample deflection, no matter how imprecisely distributed around 1.74″, constituted a 'Revolution in science'.

76 A. N. Whitehead, *Science and the Modern World*, Cambridge, 1926, 15.

77 'Joint eclipse meeting of the Royal Society and the Royal Astronomical Society', *The Observatory* (1919), **545**, 391.

78 'Joint eclipse meeting', op. cit. (77), 389–98, 393. Original italics.

79 Earman and Glymour, op. cit. (3), 76–81.

Eddington's credibility was vital to the successful reception granted the JPEC members' presentation. J. J. Thomson emphasized this point from the chair of the joint meeting:

> It is difficult for the audience to weigh fully the meaning of the figures that have been put before us, but the Astronomer Royal and Prof. Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value for the displacement. This is the most important result obtained in connection with the theory of gravitation since Newton's day.[80]

Though one might expect Thomson to have reserved such judgement until independent authorities could scrutinize the figures, recall that he was almost certainly aware that Cunningham and his colleagues had approved them after discussing the figures with Eddington at the $\nabla^2$V Club meeting. The observers were praised by every participant in the discussion, and in general the scene was unlike that at the BAAS meeting in September. Oliver Lodge attended, but did not dissent. He left the joint meeting early (purportedly for a previous engagement) and wrote to *The Times* the next day that although he cautioned against 'a strengthening of great and complicated generalisations … on the strength of the splendid result', he admitted that the

> eclipse result is a great triumph for Einstein; the quantitative agreement is too close to allow much room for doubt, and from every point of view the whole thing is of intense interest … I heartily congratulate Professor Einstein and also the skilled and painstaking observers who have so admirably verified his striking and original prediction.[81]

The only speaker at the joint meeting who was somewhat belligerent regarding the experimenters was Ludwig Silberstein. As it was recounted in the *Observatory*, Silberstein argued that 'in spite of what the President said, I believe this result to be essentially an *isolated* fact. … We owe it to that great man (pointing to Newton's portrait) to proceed very carefully in modifying or retouching his Law of Gravitation.'[82] Eddington responded with mild derision: 'When a result that has been forecasted is obtained, we naturally ask what part of the theory exactly does it confirm. In this case it is Einstein's *law* of gravitation.'[83] Having been singled out by Silberstein, Thomson gave a further nod to Eddington's authority as he closed the meeting: 'We must thank the Astronomer Royal and Professor Eddington for bringing this enormously important discovery before us, and for taking such pains to make clear to us exactly where the problem stands.'[84] Silberstein's indignity was not over when the meeting adjourned. His most notable contribution to the history of the meeting was not as an eloquent antagonist, but as the butt of Eddington's most famous joke. As S. Chandrasekhar related it,

> Eddington recalled … that, as the joint meeting of the Royal Society and the Royal Astronomical Society was dispersing, Ludwig Silberstein came up to him and said, 'Professor Eddington, you must be one of three persons in the world [meaning Einstein, Eddington and Silberstein himself] who understands general relativity.' On Eddington's demurring to this

80 'Joint eclipse meeting', op. cit. (77), 394.
81 'Sir Oliver Lodge's caution. To the editor of *The Times*', *The Times*, 8 November 1919, 12.
82 'Joint eclipse meeting', op. cit. (77), 396–7. Original italics.
83 'Joint eclipse meeting', op. cit. (77), 398. Original italics.
84 'Joint eclipse meeting', op. cit. (77), 398.

statement, Silberstein responded 'Don't be so modest, Eddington', and Eddington replied that 'On the contrary, I am trying to think who the third person is.'[85]

The joint meeting closed on a high note for Eddington, and the meeting as a whole was a tour de force for the JPEC. Careful preparation led to success with the scientific audience. Among this audience, one correspondent for *The Times* carried the triumphal mood to the public audience.

The identity of the correspondent who wrote the 'Revolution in science' article is recorded in the 'marked copies' of *The Times*. To my knowledge this has not previously been disclosed.[86] It might be expected, given the authorship of earlier *Times* coverage, that Hollis or even Dyson himself would have contributed an article on the joint meeting. In fact, the author was not an astronomer. Nor was he a general reporter who would be likely, in the fashion of Long and Hartley, simply to transcribe information channelled by one of the JPEC members. In fact, the piece was written by the noted zoologist Peter Chalmers Mitchell, who had been a Fellow of the RS since 1906. Mitchell made contributions to *The Times* over several decades and from 1918 to 1922 he served on the newspaper staff.[87] Though it may not be immediately clear why Mitchell should have been the one to cover the joint meeting, some details of his life may be relevant.

Mitchell's father-in-law was a veteran of the 1860 solar eclipse expedition, the late Charles Pritchard, Savilian Professor of Astronomy at Oxford.[88] Mitchell was fascinated by modern physics, which he applied to his work on biology. According to D. P. Crook,

> Mitchell lived in the age of Ernst Mach, Henri Poincaré and Albert Einstein, F. H. Bradley and Alfred Whitehead – men whose scientific philosophies had demolished the old Newtonian certainties. ... Mitchell absorbed the epistemological misgivings arising out of the 'new physics', and with some originality applied them in the biological world to enhance the 'restrictionist' approach.[89]

This is an interesting observation in light of Mitchell's own role in Einstein's popular victory over Newton. By Crook's characterization, Mitchell appears to be the type of person who would have been very intrigued by the pre-eclipse publicity describing a crucial test between Einstein and Newton.

Mitchell may also have been attracted by Eddington's use of the experiment as a means to transcend international conflict. In 1915 Mitchell published the influential book *Evolution and the War*, in which he opposed hawkish social Darwinism and advocated

85 S. Chandrasekhar, *Eddington: The Most Distinguished Astrophysicist of His Time*, Cambridge, 1983, 30.

86 As I mentioned above, it was only through the generous help of Nicholas Mays that I learned of the existence of *The Times* 'marked copies', op. cit. (22).

87 Mitchell's obituary may be found in *The Times*, 3 July 1945, 7. After 1922 he maintained a relation with *The Times* as scientific correspondent.

88 On Pritchard, see Alex Pang, op. cit. (56). On his relation to Mitchell, see any *Who's Who* entry on Mitchell.

89 D. P. Crook, 'Peter Chalmers Mitchell and antiwar evolutionism in Britain during the Great War', *Journal of the History of Biology* (1989), **22**, 325–56. According to Mitchell's 'restrictionist' approach to evolutionary theory, biological 'laws' should not be extrapolated to determine rules of human conduct (thus war should not be justified as a nature-mandated struggle for existence).

cooperationism based on his reading of evolution. Mitchell also opposed the war on a personal level, since he had trained at the universities of Leipzig and Berlin and had many acquaintances among German scientists.[90] In this sense he shared the sentiment of Eddington, who made well-known pleas for international cooperation to continue between scientists during the war.[91] During the Great War Mitchell contributed to the drafting and publicization of the British peace terms. In fact, his appointment to the staff of *The Times* came as a result of his personal acquaintance with the proprietor of the newspaper, Lord Northcliffe, under whom he worked in the Department of Enemy Propaganda.[92] I know of no direct evidence of a personal connection between Mitchell and Eddington (though the two had been Fellows of the Royal Society together since Eddington's election in 1914), but Oliver Lodge of all people drew a comparison between their work.[93]

In light of these details, an exploration of Mitchell's participation in the joint meeting publicity would be a promising avenue for future research. This much is clear: he had a strong connection to the proprietor of *The Times*; as a Fellow of the Royal Society he had a right to attend the joint meeting; and he was a dedicated internationalist, so he had reason to share Eddington's desire to publicize and celebrate the British confirmation of a German theory.

Though it is difficult to gauge precisely the extent of the general public's response to Mitchell's article, there is no doubt that the joint meeting announcement quickly became the object of popular fascination. Eddington followed up the announcement with a public lecture in Cambridge and 'hundreds were turned away unable to get near the room'.[94] It seems that the notion of Newton's being overthrown (as *The Times*' sub-headline declared) caused a particular stir. *Punch* joked poetically that a 'Jewish-Swiss-Teuton had partially scrapped the *Principia* of Newton'.[95] *The Times* noted that

> wide interest in popular as well as scientific circles has been created by the discussion which took place at the rooms of the Royal Society on Thursday afternoon. … The subject was a lively topic of conversation in the House of Commons yesterday, and Sir Joseph Larmor, F.R.S., M.P. for Cambridge University, on arriving at a lecture … said he had been besieged by inquiries as to whether Newton had been cast down and Cambridge 'done in'.[96]

90 Crook, op. cit. (89), 343.

91 In a famous passage, Eddington urged his colleagues to think 'not of a symbolic German, but of your former friend Prof. X, for instance – call him Hun, pirate, baby-killer, and try to work up a little fury. The attempt breaks down ludicrously'. Eddington, 'The future of international science', *Observatory* (1916), **501**, 271.

92 Crook, op. cit. (89).

93 In his autobiography Lodge noted that they were both appealing writers. He found their work in '*Nature* or *The Times* [which he read] with the intention of devouring Jeans or Eddington or Chalmers Mitchell, or whatever the attractive article is'. Oliver Lodge, *Past Years*, New York, 1932, 112.

94 These are Eddington's own words in a widely quoted letter to Einstein (Eddington to Einstein, 1 December 1919). See Abraham Pais, '*Subtle is the Lord …*'*: The Science and the Life of Albert Einstein*, New York, 1982, 306.

95 *Punch*, 19 November 1919, 422.

96 'Revolution in science', *The Times*, 8 November 1919.

It would certainly appear that the JPEC was successful in its campaign to convince the public that the eclipse experiments were a crucial test between Einstein and Newton.

*We cheered the Eclipse Observers' start,*
*We welcome them returned, Sir;*
*Right gallantly they played their part,*
*And much from them we've learned, Sir.*
*No pains nor toil they thought too great,*
*Nor left ein stein unturned, Sir,*
*Right heartily we asseverate*
*Their bottle a day they've earned, Sir.*[97]

## Conclusion

I have argued that the flood of notoriety for Einstein and the eclipse experiments that followed the joint meeting ought not to be taken for granted simply because of our present view of Einstein's work, and have shown that this acclaim did not emerge from a vacuum as some historical accounts might suggest. Rather it was the culmination of a swell of publicity, which has received very little scrutiny by historians, that began in the months leading up to the expeditions themselves and was initiated and sustained by the members of the JPEC, including Eddington, Dyson and Crommelin. These three men and their associates collaborated in the creation of an audience that would eagerly await the experimental results, and was prepared to consider generally high deflection values as confirmation of Einstein's theory. Clearly, the experiment did not stand on its own: the edifice of the crucial experiment had to be constructed by Eddington and his associates. I have argued that the JPEC's success must be explained in terms of the credibility that Eddington established and the manner in which audiences were primed by information that was disseminated to *The Times* and elsewhere.

Eddington's integration of the roles of experimentalist and publicist is by no means unique in the history of science. As Bruno Latour describes it, Louis Pasteur's demonstration of the anthrax vaccine at Pouilly-le-Fort was a similar triumph of strategy. The demonstration was acclaimed in the press and in the annals of history with rhetoric not dissimilar to that applied to the eclipse experiments. It was called 'the wonderful experiment of Pouilly le Fort', and described in the *Revue scientifique* with an ancient Greek conceit that A. N. Whitehead could have borrowed: 'Pouilly-le-Fort is as famous today as any other battlefield. Monsieur Pasteur, a new Apollo, was … more certain of success than that child of poetry would be.'[98] Latour argues that the achievement at Pouilly-le-Fort was not a miracle, but rather the culmination of Pasteur's strategy to translate the achievements of the laboratory into the public eye. Though seeking experimental confirmation and public approbation put both Pasteur's and Eddington's predictions at risk of falsification, each man took every step to maximize the likelihood

97 This is one of several 'newly uncovered' verses of the 'Astronomer's drinking song' that were published in the *Observatory* following the joint meeting. The rest can be found in 'From an Oxford note-book', *Observatory* (1919), **546**, 25.

98 Bruno Latour, *The Pasteurization of France*, Cambridge, MA, 1988.

that his result would be favourable. Like Pasteur in Pouilly-le-Fort, where Latour notes 'certain elements of the laboratory were taken to the field', Eddington was able to mediate between his craftwork as an experimenter and the way in which this activity was presented to the public. Eddington's exposing photographic plates on an Atlantic island on 29 May 1919 did not confirm Einstein's theory. Inasmuch as Eddington did confirm general relativity, he did so in the autumn of 1919 by bringing the desk-skills of measurement and data manipulation to bear on the photographic plates in his study at the Cambridge Observatory, and then by delivering this work judiciously to various audiences.

Eddington helped to construct British audiences for relativity at four levels. As the initial agent of general relativity in Britain, he introduced technical papers on the subject to the *Monthly Notices* of the RAS. Through specialized lectures at Cambridge he trained students in an understanding of relativity theory.[99] As a popularizer of relativity theory, he expanded general interest in the theory and in the eclipse expeditions. Finally, his specific preparations for the joint meeting, such as the briefing at the $\nabla^2 V$ Club, helped to ensure that the literal audience occupying the seats at the meeting would be ready to receive 'confirmation' of general relativity theory. He was an unreserved advocate of Einstein's relativity theory, he devoted several months of his life to testing one of its predictions, and he worked hard to achieve public acclaim for his efforts. His endeavours as a publicist served not only to convince scientists and society that the JPEC members would be performing a crucial experiment, but also benefited his cause specifically by reinforcing his credibility as the practitioner of a challenging experiment.

I am not attempting, in this paper, to condemn Eddington's publicity actions as disingenuous or inappropriate. Instead, recognizing the importance of social dynamics in the transmission of scientific knowledge, I would argue that this behaviour exemplified components of Eddington's skill as an experimental scientist. Scientists and the public construed his work in 1919 to be valid and significant because he showed talent for influencing his audience's comprehension of relevant theory and experimental practice, and skill at portraying his experiments as decisive. I shall not attempt to settle the issue of where such presumably tacit knowledge may have been acquired.[100] With respect to Eddington's manipulation of the data, I have noted Earman and Glymour's relief that at least he backed a winning horse in Einstein. Eddington himself admitted that he was 'not altogether unbiased'.[101] Again, though, I am hesitant to condemn Eddington's actions. Eddington, like Einstein, believed genuinely that general relativity theory was too good not to be true, and he felt that the evidence must support the theory.[102] When the raw data did not do so, it is reasonable to assume that Eddington found the fault in his own ability

99 For a discussion of Eddington's teaching of general relativity, of reasons why Eddington may have been sympathetic to Einstein's theory, and of the elements of his training that allowed him to master relativity, see Warwick, op. cit. (13), Chapter 9.

100 Stanley, op. cit. (61), makes a convincing argument that Eddington's portrayal of the eclipse expeditions was conditioned by his familiarity with Quaker anti-war publicity.

101 Eddington, *Space, Time, and Gravitation*, Cambridge, 1923, 127–8.

102 When asked in 1919 what he would have said if the eclipse experiments had failed to show the light deflection he predicted, Einstein reputedly answered, 'Then I would have to pity the dear Lord. The theory is correct anyway.' Pais, op. cit. (94), 30.

as an experimenter. In this light, it is not surprising that the data that Eddington discarded were those that disagreed with the Einstein prediction.[103] Again, the question of Eddington's experimental skill is pertinent; the pages of history of science are filled with figures whose experiments were seemingly unreplicable (Newton) or insurmountably counter-intuitive to others (Barbara McClintock), but whose intuition or skill has been borne out. Though Eddington's data analysis did not fit with what Earman and Glymour called the scientific 'model of objectivity and rationality', it produced a result that has yet to be overturned. If this too was skill, rather than caprice or an unlikely case of wanton falsification of evidence, it also was of the tacit sort. Dyson may have offered the most insightful assessment of how such 'skills' were manifested. He predicted to Cottingham what might happen if their test of the theory that so compelled Eddington failed to produce a satisfactory result: 'Eddington will go mad, and you will have to go home alone.'[104]

103 Again Eddington and Pasteur may be likened here with respect to Pasteur's experiments on spontaneous generation, which he was convinced did not take place. Pasteur defined seemingly spontaneous growth as contamination of his experiments. In a manner that seems comparable to Eddington, 'Pasteur was so committed in his opposition to spontaneous generation that he preferred to believe there was some unknown flaw in his work than to publish the results'. H. Collins and T. Pinch, *The Golem: What You Should Know about Science*, 2nd edn., Cambridge, 1998. This raises the problem of the 'experimenter's regress'. For an excellent discussion of that phenomenon in relation to Newton's prism experiments, see Simon Schaffer, op. cit. (8).

104 Eddington recounted this quip in 'Forty years of astronomy', op. cit. (12), 142.