Studies in History and Philosophy of Modern Physics 40 (2009) 57–67

Contents lists available at ScienceDirect

Studies in History and Philosophy of Modern Physics

journal homepage: www.elsevier.com/locate/shpsb

Trust in expert testimony: Eddington's 1919 eclipse expedition and the British response to general relativity

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ARTICLE INFO

Article history:
Received 11 March 2008
Received in revised form
29 June 2008

Keywords:
Eclipse
Eddington
Einstein
Expertise
Testimony
Relativity

ABSTRACT

The 1919 British astronomical expedition led by Arthur Stanley Eddington to observe the deflection of starlight by the sun, as predicted by Einstein's relativistic theory of gravitation, is a fascinating example of the importance of expert testimony in the social transmission of scientific knowledge. While Popper lauded the expedition as science at its best, accounts by Earman and Glymour, Collins and Pinch, and Waller are more critical of Eddington's work. Here I revisit the eclipse expedition to dispute the characterization of the British response to general relativity as the blind acceptance of a partisan's pro-relativity claims by colleagues incapable of criticism. Many factors served to make Eddington *the* trusted British expert on relativity in 1919, and his experimental results rested on debatable choices of data analysis, choices criticized widely since but apparently not widely by his British contemporaries. By attending to *how* and *to whom* Eddington presented his testimony and *how* and *by whom* this testimony was received, I suggest, we may recognize as evidentially significant corroborating testimony from those who were expert not in relativity but in observational astronomy. We are reminded that even extraordinary expert testimony is neither offered nor accepted entirely in an epistemic vacuum.

When citing this paper, please use the full journal title *Studies in History and Philosophy of Modern Physics*

1. Introduction

Epistemologists, sociologists, and philosophers of science are recognizing now more than ever the importance of issues of expertise when it comes to the social transmission of scientific knowledge.[1] For example, can non-experts make informed decisions in trusting experts, or does the epistemic inequality involved mean that non-expert acceptance of expert testimony is simply "blind" (Hardwig, 1991, p. 699) when it comes to testimony that non-experts cannot evaluate on its own terms? Given a speaker whose special expertise surpasses everyone else's, is even partial corroboration of his or her expert testimony possible, or must any epistemic justification to be had come entirely from one's own crude evaluations of the expert's claims?

In examining these questions, I consider a prominent case of expert testimony in physics: the 1919 British eclipse expedition led by Arthur Stanley Eddington to confirm Einstein's general theory of relativity, specifically his prediction of starlight deflection. Physicists, philosophers, and historians recognize Eddington's expedition as a major catalyst for Einstein's worldwide fame.[2] Chandrasekhar (1983), for example, recalls a 1933 fireside chat at which Ernest Rutherford remarked to Eddington, "*You* made Einstein famous" (p. 28). Pais (1982) reports that, while Einstein had not been mentioned in the *New York Times* prior to November 8, 1919, afterwards there was not a year until his death when his name did not appear there (p. 309). Einstein himself had done nothing special in November 1919. The reason for his recognition in the *New York Times* and newspapers worldwide was an announcement by three British scientists on November 6 at a joint London meeting of the Royal Society (RS) and the Royal Astronomical Society (RAS). Astronomer Royal Frank W. Dyson, A. C. D. Crommelin, and Eddington (the Plumian Professor of astronomy at Cambridge, RAS Secretary, and foremost British

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[1] Cf. Hardwig (1991), Goldman (2001), Selinger & Crease (2006), Collins & Evans (2007).

[2] Cf. Moyer (1979, p. 56), Stern (1982, p. 330), Hoffman (1983, p. 156), Friedman & Donley (1985, p. 67), Missner (1985, p. 269), Will (1986, p. 65), Calder (2005, p. 71), Crelinsten (2006). Buchwald, Schulmann, Illy, Kennefick, & Sauer (2004) note, "The eclipse of 22 May 1919 was the single most important event affecting Einstein's life in the period covered by this volume" (xxvi), which is quite a statement given that Einstein also married and his mother died during this period.

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doi:10.1016/j.shpsb.2008.08.003

authority on relativity) testified to the gathered British scientific community that expeditions to Africa and Brazil to observe the May 29, 1919 eclipse had confirmed Einstein's prediction and ruled out its Newtonian counterpart. Among those present was Peter Chalmers Mitchell, science writer for the *Times* of London and author of its November 7 article headlined "Revolution in Science; New Theory of the Universe; Newtonian Ideas Overthrown."[3] Scarcely a year since the end of World War I, experimentation by British astronomers propelled a German theoretical physicist to the RS Gold Medal and international fame.[4]

Eddington's eclipse expedition is often understood as a clear vindication of Einstein's bold prediction and by extension his general theory of relativity; most familiar to philosophers is the portrayal by Popper (1968) of the eclipse episode as science at its best, as exactly the sort of case on which falsificationism is built (pp. 36–37). In this laudatory treatment Popper is hardly alone, but recently more "warts and all" revisionist accounts have portrayed the eclipse episode as an exemplar of a different sort: see Earman and Glymour (1980) for a critical assessment of Eddington's work, Collins and Pinch (1993) for a depiction of Eddington as a typically fallible scientist-as-craftsman, and Waller (2002) for a portrait of Eddington as ruthlessly fraudulent. Waller (2002) makes the counterfactual claim that even if Einstein had predicted the classical deflection value Eddington would have spun the expedition results in his favor still: "Eddington knew precisely what he wanted and selected or rejected the results in strict accordance with one principle: whether or not it supported Einstein's theory. Those data sets that did were in; those that did not were out" (p. 58). With this damning assessment, Waller (2002) insists, "an impartial scientific observer of 1919 would almost certainly have agreed" (pp. 102–103).

Critical accounts of the eclipse episode are right to emphasize the role played by trust in Eddington's expert testimony in the contemporary British response to his alleged vindication of relativity. Critics are also right that it was not inevitable that Eddington's work be accepted as confirming relativity over classical mechanics, as the data alone does not force the revolutionary conclusion. Yet some retrospective criticisms of Eddington seem to regard trust in expertise as a source of epistemic impurity only and ignore its epistemic value. Waller's damning account, I suggest, shares with Popper's laudatory account a tendency to oversimplify the epistemological landscape.

Here I reconsider the details of the 1919 eclipse expedition and identify the relative social epistemic positions of Eddington and his colleagues. Did Eddington fudge his data and succeed because no one could recognize his fraud? It is true that WWI, British commitment to the ether, the difficulty of general relativity, and the practical constraints on measuring starlight deflection all served to make Eddington *the* unrivaled trusted expert on the general theory of relativity, and his eclipse experiment as the theory's crucial test, for Britain in the 1910s and early 1920s. Furthermore, Earman and Glymour's careful analysis has shown that Eddington's results rested on non-trivial choices, and the historical record does not find prominent British contemporaries publicly criticizing these choices. Yet emphasis on Eddington's epistemic and social authority as evidence of successful intellectual fraud fails to appreciate that it was specifically with regard to relativity and its observational implications that Eddington's expertise outstripped his colleagues, thus it was in these parts of the eclipse experiment where trust in his expert testimony was most essential. Meanwhile other British scientists could capably evaluate his experimental procedure and data analysis as those elements of the case rested on standard astronomical practice. In such matters Eddington was an expert but so were the colleagues to whom he presented his results, astronomers and physicists well positioned to identify and challenge dubious data analysis. Therefore, I argue, it is significant that those British physicists and astronomers who were slow to accept Eddington's testimony for GR did not blame his data analysis but instead articulated alternative explanations of the starlight deflection phenomenon. Thus we have reason to reject speculation that impartial scientific observers in 1919 would have judged Eddington's work to be ruthlessly untrustworthy, since despite the significant epistemic inequality involved, actual impartial scientific observers in 1919 capably judged otherwise.

2. Relativity and the war

Einstein (1905) came from obscurity to publish five papers, among which was his theory of relativity for non-accelerating reference frames. Yet Dirac (1982) recalls, having been a Bristol engineering student in the early 1910s, that "no one had heard about relativity or about Einstein previously, except for a few specialists at the universities" (p. 80). Special relativity was largely ignored in Britain compared to Germany, Goldberg (1970) argues, because of British physicists' commitment to the reality of the ether; eminent British physicists such as Lord Kelvin and Oliver Lodge opposed or ignored SR because it gave no priority to the ether's rest frame (p. 102). From 1905 to 1916, until Einstein's publication of and Eddington's engagement with GR, the only major British work on relativity was Ebenezer Cunningham's *Relativity and the Electron Theory* in 1915 and Cunningham's interest was mainly incidental as he was primarily interested in how SR fit with his own work in electrical theory and quasi-realist conception of the ether (Cunningham, 1915, pp. 87–94; Goldberg, 1970, pp. 105–107).

Moving from SR to GR only made relativity more forbidding for an already unsympathetic audience. To work out GR, Einstein (1915–1917) turned to complex tensor analysis, increasing his theoretical resources but also creating a more intellectually taxing theory. Articles in the *New York Times* (November 9, 1919) and *Washington Post* (April 3, 1921) propagated the legendary story that Einstein had warned publishers of his GR book that only a dozen people would understand it. According to another story, Eddington was praised at the RS–RAS joint meeting as one of only three people who understood relativity. Eddington hesitated in response (so the story goes) and when his interlocutor chided him not to be so modest, Eddington replied that he could not think of a third person (that is, besides Einstein and himself). Chandrasekhar (1983) reports that Eddington's interlocutor was Ludwig Silberstein, an eminent British physicist who considered himself something of an expert on relativity and one of the few people at the RS–RAS joint meeting to challenge Eddington's claim to have confirmed GR. Silberstein may have been implying that *he* was the third person, making Eddington's response all the more audacious.

The difficulty of GR was not the only impediment for British scientists; war was another. WWI severed communication and collaboration between Germans and Austro-Hungarians and their British, French, and American counterparts. Lodge had insisted on the eve of war, "Science is above all politics,"[5] but his optimism

[3] Revolution in Science (1919). The "Revolution in Science" article was published without a byline, but archival research by Sponsel (2002) identifies Mitchell as its author.

[4] Einstein was the finalist for the 1920 RS Gold Medal, but was rejected at the last stage of consideration, making 1920 an unusual year in which the Gold Medal was not awarded. Einstein finally won the award in 1926, immediately following Eddington in 1924 and Dyson in 1925.

[5] As quoted in From an Oxford notebook (1916a, p. 238).

was not universally maintained. As the war dragged on, scientists condemned those on the other side for insufficiently disassociating themselves from the war crimes of their nations' armies. Readers today may be surprised to find in *Nature* an essay explaining away the reputation of German science as the undeserved byproduct of good work done by foreigners in Germany and plagiarism by German scientists of the highest credentials.[6] In print British scientists frequently expressed their pessimism over German scientists' possible readmission to the international scientific community.[7] In a letter to the *Observatory*, Sampson (1916) expressed the common concern thusly: "My own feeling as to the difficulty of resuming relations upon anything like the old footing…arises from resentment—no matter at the moment against whom—that the base and bloody experiences of this war have destroyed the ground in which unguarded trust and friendship must grow" (p. 344). The war meant that few scientists on either side were exposed to one another's work in detail, and many had become unwilling to trust this work in the usual fashion. If British scientists were going to learn of Einstein's newest and quite intellectually challenging work on relativity, some sort of trusted conduit would be needed. Eddington, along with Willem de Sitter of the politically neutral Netherlands, would serve as this conduit.

For his part Einstein did not fit the stereotype of nationalist German scientists distrusted by and distrustful of British scientists. His pacifism had earlier led Einstein to renounce his German citizenship, and during the war he had released a statement with G. F. Nicolai and Willem Forster urging that "educated men in all countries not only should, but absolutely must, exert all their influence to prevent the conditions of peace being the source for all future wars."[8] This effort of internationalism was overshadowed, however, by the famous Manifesto to the Civilized World signed by many more German intellectuals and scientists supporting their military's actions. Among those British scientists who cared to distinguish pacifist internationalist German scientists from their pro-war colleagues was Eddington, himself a pacifist, internationalist, and Quaker. Having returned to Cambridge after several years at the Greenwich Observatory, in 1912 Eddington was appointed RAS Secretary, a post which put him into wider communication with scientists abroad than most of his colleagues enjoyed during the war. In this capacity he corresponded with de Sitter, who shared news of Einstein's recently published GR as well as his internationalist inclinations; Eddington was taken both by Einstein's work and the chance to promote the work of an "enemy" internationalist scientist. Stanley (2003) argues that as the Quaker response to war was to work to humanize the enemy, as a Quaker Eddington likely would have seen his promotion of Einstein's work in these terms (p. 64).[9] Eddington (1916) exhibits this humanizing approach in a letter to the *Observatory* urging his British colleagues to "think not of a symbolic German, but 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" (p. 271).

Eddington became relativity's most capable, connected, and impassioned advocate in Britain during and after WWI. Thomson, who as RS President presided over the November 1919 joint meeting, would later call Eddington "the greatest authority in England on that important, evasive, and difficult subject—relativity," while Lodge noted, "it must be said that Einstein without Eddington would be comparatively unknown in this country, whereas in fact the two have caught the imagination of the public to a surprising degree."[10] It was Eddington who solicited De Sitter's (1916a, b, 1917a, b) English-language GR explications in the *Observatory* and *RAS Monthly Notices*, and who soon thereafter published his own GR treatise; before the 1919 RS–RAS joint meeting, these papers were the only substantive works on GR available in English for a British audience. When the Physical Society commissioned Eddington to write his *Report on the Relativity Theory of Gravitation*, there was no one in Britain better suited for the task. In his review of Eddington's *Space, Time, and Gravitation,* Lindemann (1920) opined, "It is only right and proper that this, the first original work on the subject in our own language, should come from the pen of the man who not only was the first on this side of the Channel to study the theory and see its fundamental importance, but who also had such a large share in its triumphant verification at the last solar eclipse" (p. 329).

3. Background to the 1919 eclipse expedition

Among those sufficiently won over by Eddington's enthusiasm for GR was his colleague Frank Dyson, who as Astronomer Royal chaired the Joint Permanent Eclipse Committee (JPEC) organizing perennial eclipse expeditions. By March 1917, Dyson had decided that the May 29, 1919 eclipse would be used to test Einstein's prediction of starlight deflection of starlight and recruited Eddington to lead the expedition. It has been suggested that Dyson and Eddington sought to test this prediction not because they were particularly interested in the results but so that Eddington could avoid the WWI labor camps for conscientious objectors. Eddington (1917) testifies that he was motivated to accept relativity for reasons more metaphysical and aesthetic than empirical, and Chandrasekhar (1983) testifies that Eddington admitted to him a lack of interest in mounting the expedition "since he was fully convinced of the truth of general relativity" (p. 25). Furthermore the eclipse expedition did keep Eddington out of the labor camps. Eddington had long made clear that he objected to the war for religious reasons and was only awaiting official conscientious objector designation, which may have devastated his career.[11] What saved him was Dyson's July 1918 letter to the British government proposing that Eddington be tasked to lead the next eclipse expedition in lieu of military service. Dyson slyly appeals to nationalist pride, arguing that "Professor Eddington's researches in astronomy…maintain the high tradition of British science at a time when it is very desirable that it be upheld, particularly in view of the widely held but erroneous notion that the most important scientific researches are carried out in Germany." As an exemplary experimental astronomer, veteran of previous eclipse expeditions, and Britain's foremost expert on relativity, Dyson argued, "Professor Eddington is peculiarly qualified to make these observations" of an eclipse "of exceptional importance."[12]

Dyson's proposal was accepted. But the fact that Eddington avoided both military service and the labor camps does not prove

[6] Cf. America and German science (1919, pp. 446–447), Boccardi (1916).

[7] Cf. Larmor (1916), From an Oxford notebook (1916a), From an Oxford notebook (1916b).

[8] As quoted in Stanley (2003, p. 61).

[9] Stanley (2003) also reports that Eddington informed de Sitter that he was "interested to hear that so fine a thinker as Einstein was anti-Prussian," and that Einstein thanked de Sitter for his efforts with Eddington to "throw a bridge over the abyss of misunderstanding" (p. 69).

[10] As quoted in Douglas (1956, p. 104). One British scientist who may have grasped GR as early as Eddington was Henry Brose: physicist, POW in Germany, and translator of Freundlich (1920). Cf. Cunningham (1920).

[11] Cf. Stanley (2007, p. 148). Cunningham's career was not the same, for example, after years in the labor camps as a conscientious objector, though it should be noted that Cunningham (1919a–c) wrote a series for *Nature* explaining relativity theory and its observational implications for a general audience.

[12] As quoted in Douglas (1956, p. 94).

that this was *the whole point* of the expedition; it may have served multiple functions scientific and political. Eddington could have recognized that empirical results would matter for others even if not for himself; he could have recognized that for those less familiar with GR and for those with different aesthetic and metaphysical values, empirical verification would be evidentially weighty. After all, the case for GR in Britain after WWI would have to be built on empirical success.[13] Einstein offered three observational implications of GR: the deflection of light by gravitational fields, the shift in solar spectral lines, and the advance of perihelion for Mercury. Of these the third had been confirmed by Einstein before publishing GR, and while the accommodation of the perihelion of Mercury was cited as a reason to take GR seriously, alone it instigated no great "revolution" in scientific or popular opinion.[14] In 1919, evidence of a red shift in spectral lines was inconclusive at best, disconfirming of the relativistic prediction at worst; after the 1919 eclipse expedition, those British scientists slow to accept GR would cite the red shift problem among their major reasons for hesitation.[15] So if a broadly persuasive case was to be made, it would have to concern the deflection of light.

Einstein had predicted that light passing a massive body such as the sun would be deflected from its path such that, from the perspective of those observing from the far side of the massive body, the source of light would appear to have shifted from its usual position (that is, its apparent position when the massive body is positioned elsewhere). So a star observed when near the sun in the sky will be shifted some angular displacement α from its observed position when the sun is in a different part of the sky. Stars close to the sun usually cannot be observed, of course, since sunlight drowns out starlight, which is why a solar eclipse provides a unique opportunity to view stars close to the sun. These observations can be compared to stars' apparent positions at night; an observed displacement suggests that the sun has deflected the starlight from its original path. This angular displacement is terribly small, however, and drops off precipitously the farther the star is from the sun's limb; the closer the star is to the sun, the harder it will be to clearly observe the star and thus the more complete and longer in duration the eclipse totality must be to measure the star's apparent position relative to the sun. Given these constraints, replicating another team's results from an earlier expedition is easier said than done. Even with the required equipment, it is rare to find solar eclipses of sufficiently long duration and complete totality, high enough off the horizon to minimize atmospheric distortion, observable from a place on Earth easily accessible, with clear skies, when the sun is surrounded by a field of bright stars.

Eddington and his collaborators consistently presented the starlight-deflection question as a "trichotomy." The three acknowledged possibilities were the relativistic ("full"), classical ("half") and null deflection values. Under GR, Einstein predicted an angular deflection at the sun's limb of $1''74$. Newton himself had not weighed in on the starlight-deflection question, but if one assumes light is particular and subject to Newton's law of gravity, then light at the sun's limb will experience an angular deflection of $0''87$.[16] If one assumes no gravitational effect on light then of course one will expect no starlight deflection. While the null deflection is often assumed to be the Newtonian position, Eddington (1920) did not present the trichotomy this way, instead framing a null result as potentially confounding for both classical and relativistic theories (p. 58). Overthrowing Newton through the eclipse experiment, then, depended not just on the observation of starlight deflection but the *degree* of deflection observed. Hentschel (1992) argues that because the null prediction was advocated by some of Einstein's German colleagues, Einstein himself was most interested in the eclipse experiment to separate his theory not from Newtonian mechanics but from alternative theories of relativity predicting no starlight deflection, which explains why Einstein was less concerned with the precise quantity of observed angular deflection than the qualitative issue of deflection or not (pp. 597–598). For Eddington and his British colleagues, by contrast, the key comparison was between those possibilities identified as the relativistic and classical Newtonian predictions.

The May 29, 1919 solar eclipse was anticipated to be an especially good opportunity to test Einstein's prediction: the star field near the eclipsed sun was particularly rich, and the places at which totality would be fullest and longest included sites accessible to the British astronomers.[17] But the British 1919 expedition was neither the first nor last attempt to observe the deflection phenomenon. German astronomer Erwin Freundlich had sought to do so twice before, but war and weather conspired against him.[18] War also had kept Britain from observing the June 8, 1918 eclipse, but several US observatories sent expeditions, including Yerkes Observatory and Lick Observatory teams' attempts to test Einstein's prediction. While the Yerkes team was thwarted by cloudiness, the Lick team was more successful (Jones, 1918, p. 408). The Lick 1918 eclipse observations generated moderate interest in the United States and Britain; had these results been published swiftly, Lick team leaders William Campbell and H. D. Curtis may have undercut Eddington's work. Campbell did give a preliminary presentation at the RAS on July 11, 1919 speculating that these results actually ruled out Einstein's value.[19] Curtis and Campbell disagreed about the reliability of their results, however: Campbell was concerned about the observatory's reputation, while Curtis argued for publishing a "simple, frank statement" of the results. The dispute continued until Curtis left to become director of the Alleghany Observatory in July 1920, leaving Campbell to exercise his discretion not to publish.[20]

[13] As one writer for *Nature* put it, "The general processes by which Einstein derived this formula [relativity theory] carried no assurance that the results would describe Nature, and the theory must rest upon such tests as he himself proposed for it" (Societies and academies, 1920a, p. 459). A review of Eddington (1920) noted, "If the dream of complete relativity be true we are getting near the point at which it is so general as to lose touch with common experience" (Gravitation and relativity, 1919, p. 2).

[14] Cf. From an Oxford notebook (1917).

[15] Cf. Astronomy at the British Association (1919), Evershed (1918), Evershed (1919), St John (1916).

[16] Will (1986) reports that the half deflection was first derived from classical mechanics in 1804 by Johann Georg van Solder, but his work was soon forgotten, not to be unearthed until after the 1919 eclipse; in the interim, the half deflection value was independently derived and identified with Newton by Einstein in 1911 and Lodge in 1917. Cf. Gravitation and Light (1913, p. 231), Stanley (2003, p. 75), Will (1986, pp. 66–67).

[17] Cf. Proceedings of the British Astronomical Association, Wednesday 1918 November 27 (1919, pp. 77–78).

[18] Attempted observation in Brazil on October 10, 1912 was rained out, and just weeks before the August 21, 1914 eclipse in Russia, WWI broke out, leaving Freundlich interned in Russia as an enemy and his equipment impounded for the duration of the war (cf. Earman & Glymour, 1980, pp. 61–62). Freundlich and Einstein were close colleagues and maintained frequent correspondence throughout the 1910s. Particularly interesting is Eddington's December 1, 1919 letter that congratulated Einstein while expressed condolences that Freundlich had not been the one to confirm his colleague's work; Eddington then qualifies his condolence by noting the fortuitousness of British vindication of a German scientist's work (cf. Buchwald et al., 2004).

[19] Cf. Meeting of the Royal Astronomical Society, Friday 1919 July 11 (1919, pp. 298–299).

[20] Earman & Glymour (1980) give a detailed analysis of the Lick 1918 eclipse episode and the dispute between Campbell and Curtis over their results and the implications for Einstein's theory. The excerpted correspondence is fascinating, especially Curtis's December 29, 1920 letter arguing as follows: "A simple, frank

The November 1918 WWI armistice made the anticipated May 1919 expedition a reality. Dyson brought Eddington officially on board and scouting of observation sites determined that Sobral in Brazil and the island of Principe off the west coast of Africa were the best in terms of totality, weather, and accessibility (Dyson, Eddington, & Crommelin, 1920, p. 294). Travel was arranged and equipment secured: specifically a 13-in astrographic telescope from Oxford for Eddington and Cottingham at Principe and a 13-in astrographic telescope from Greenwich and 4-in aperture backup from the Royal Irish Academy for Crommelin and Davidson at Sobral (Dyson et al., 1920, p. 295). (These telescopes will be designated as P13, S13, and S4, respectively, though it should be noted P13 and S13 were reduced to 8-in for observation.) The two teams traveled together as far as Madiera, where Crommelin and Davidson continued to Brazil and Eddington and Cottingham departed for Africa. Both teams arrived on site weeks before the May 29 eclipse; journal entries and reports show that both teams enjoyed considerable local support in lodging, site preparation, and assistance building rudimentary structures to shelter their equipment (Dyson et al., 1920, pp. 312–313).

Skies were cloudy at both locations on the morning of May 29. Visibility improved at Sobral just before totality, so Crommelin and Davidson were able to observe several stars near the sun for the entirety of totality. In all, they exposed 18 S13 photographic plates at 5 and 10 s intervals and eight S4 plates at 28 s intervals (Dyson et al., 1920, p. 300). Seven S4 plates showed seven stars each, yielding an average observed angular displacement at the sun's limb of $\alpha_{S4} = 1''98$ with a probable error of $0''12$ (Dyson et al., 1920, pp. 302–306; Eddington, 1987, p. 118).[21] All S13 plates showed 7–12 stars, initially yielding an average displacement of approximately $\alpha_{S13} = 0''93$ (Dyson et al., 1920, pp. 310–312). However, preliminary on-site analysis immediately following the eclipse led Crommelin and Davidson to hypothesize that the 13-in astrographic telescope had undergone systematic error, as the following excerpt from their expedition journal demonstrates:

> May 30, 3am, four of the astrographic plates were developed, and when dry examined. It was found that there had been a serious change of focus, so that, while the stars were shown, the definition was spoilt. This change of focus can only be attributed to the unequal expansion of the mirror through the sun's heat. The readings of the focusing scale were checked next day, but were found unaltered at 11.0 mm. It seems doubtful whether much can be gotten from these plates. (Dyson et al., 1920, p. 309)

Correcting for the hypothesized aberration, Crommelin provides an average displacement value of $\alpha_{S13^*} = 1''52$. As with the uncorrected value $\alpha_{S13} = 0''93$, probable error measurements are not provided in the RS expedition report nor other published accounts, but Earman and Glymour (1980) provide standard deviations of $0''48$ and $0''178$ for the S13 and S4 results, respectively (p. 75).

At Principe thick cloud cover did not disperse until totality was nearly over; so although 16 plates were exposed, only two plates captured the minimum number of stars required to calculate an average displacement at the sun's limb, which Eddington provides as $\alpha_{P13} = 1''61$ with probable error of $0''30$ (Dyson et al., 1920, p. 326). Again, Earman and Glymour (1980) provide a standard deviation of $0''444$ (p. 75). Despite the large probable error, Eddington would opine of his results at the momentous RS–RAS joint meeting that "the accuracy seems sufficient to give a fairly trustworthy confirmation of Einstein's theory, and to render the half deflection at least very improbable" (Dyson et al., 1920, p. 328).

4. Expedition results in an unflattering light

For ease of reference, the predicted and observed angular displacements at the sun's limb with probable error measurements as available are as follows:

$$\alpha_{P13} = 1''61 \pm 0''30 \quad \alpha_{\text{full}} = 1''74$$

$$\alpha_{S4} = 1''98 \pm 0''12 \quad \alpha_{\text{half}} = 0''87$$

$$\alpha_{S13} = 0''93 \text{ or } \alpha_{S13^*} = 1''52 \quad \alpha_{\text{null}} = 0''$$

Given the brief description of events above one might expect the Principe results to have played a small role in the confirmation of Einstein's prediction, but this was not entirely the case. It is true that in both the RS *Philosophical Transactions* expedition report and Eddington's popular account in *Space, Time, and Gravitation*, the S4 result is accorded "the greatest weight" (Dyson et al., 1920, p. 330).[22] But Eddington has testified that his preliminary on-site analysis of the few good P13 plates was enough to convince him of Einstein's experimental vindication; reflecting on the expedition later, Eddington (1938) would report, "Three days after the eclipse I knew that Einstein's theory had stood the test and the new outlook of scientific thought must prevail" (p. 142). In the months between the eclipse and the RS–RAS joint meeting where Sobral and Principe results were presented side-by-side, Eddington's numerous professional and popular lectures provided only the preliminary results from Principe because those from Sobral were as yet unavailable. When Lorentz telegraphed Einstein on September 22, 1919 with the news that his prediction had been confirmed, it was on the basis of Eddington's September 12 presentation at the BAAS of the preliminary Principe results (Buchwald et al., 2004, xxxv). There Eddington claimed only that his initial measurements suggested an average deflection intermediate between the half and full predictions and that he hoped completed analysis would favor the latter. The BAAS audience was reminded by Silberstein that the red shift remained a problem for relativity and reminded by Cortie that the Lick 1918 photographs presented by Campbell at the RAS on July 11 had not shown any displacement.[23]

Also problematic is the matter of probable error. It is true that the S13 plates dismissed as erroneous were also those most

(*footnote continued*)
statement, as above, of indecisive results secured will, so far from hurting the L.O., increase its already great reputation for sanity and conservatism, and for not announcing theories till it can deliver the goods. When the Einstein theory goes into the discard, as I prophesy it will go within ten years, these negative or indecisive results will be more highly regarded than at present" (p. 66). Also noteworthy is that the Lick equipment best suited to testing Einstein's prediction was, like Freundlich's equipment, still impounded in Russia, so Curtis and Campbell were forced to use suboptimal data-collection and data-reduction techniques to measure the starlight deflection.

[21] Note that while the discussion to follow focuses on mean starlight displacement values, one might also quite usefully analyze the distribution of displacement values relative to stars' distances from the solar limb. (Recall that displacement is greatest closest to the limb.) My thanks to this journal's anonymous reviewer for pressing this point.

[22] Eddington (1987) addresses the S4 plates as follows: "From the first no one entertained any doubt that the final decision must rest with them, since the images were almost ideal, and they were on a larger scale than the other photographs. The use of the instrument must have presented considerable difficulties…but the observers achieved success, and the perfection of the negatives surpassed anything that could have been hoped for. These plates were now measured and they gave a final verdict definitely confirming Einstein's value of the deflection, in agreement with the results obtained at Principe" (p. 118).

[23] Cf. Physics at the British Association (1920, p. 454).

divergent from the relativistic deflection prediction, which some later critics have found suspicious given Eddington's commitment to relativity. Furthermore, even when the S4 result is identified as most reliable, the notion that Einstein's value is clearly confirmed and Newton's value clearly ruled out is only palatable when the P13 and S4 results are given together. Einstein's prediction of 1″74 is 0″24 below the S4 value of 1″98, which is to say, the theoretical value is fully twice the probable error of 0″12 off the experimental value and thus just within the "margin of safety" that Eddington (1987) claims is the standard practice (p. 118). On this "twice probable error" standard of safety, the S4 value *almost* rules out the relativistic prediction while the P13 value 1″61 ± 0″30 *barely* rules out the classical prediction 0″87″. Without the decidedly pro-relativity P13 value providing the necessary corroboration, it is a stretch to characterize the S4 value as providing "definitive" confirmation of GR (Dyson et al., 1920, p. 331).

One common criticism concerns the apparently arbitrary decision to dismiss the S13 results and retain the P13 results. Earman and Glymour (1980) put the point as follows:

> The dispersion of the measurements from the Principe astrographic is about the same as the dispersion from the Sobral astrographic. The latter's plates are slightly better than the former's and many more stars appear upon them. The Principe determination used check plates, the Sobral astrographic determination did not. In all, these sets of measurements seem of about equal weight, and it is hard to see decisive grounds for dismissing one set but not the other. [p. 75]

The seemingly unequal treatment of seemingly equally good data sets serves as Waller's primary evidence for his accusation of intellectual fraud against Eddington. The allegation is that, had Einstein predicted the half deflection, Eddington would have decided in favor of that prediction by making the opposite choice of keeping the S13 result of 0″93 while dismissing the P13 result of 1″61. For this allegation to stick, three points must be established: (i) uneven treatment must have been accorded the three telescopes' results, (ii) a defense of S13 over P13 comparably plausible to the actual defense of P13 over S13 must have been available, and (iii) S4 results must be satisfactorily accounted for. While the first point plausibly may be defended, the latter two may not. Consider the last point first. While the S13 result of 0″93 with a standard deviation of 0″444 would seem to confirm the half deflection prediction about as well as the P13 result of 1″61 with the same standard deviation confirms the full deflection prediction, the two results do not couple equally well with the S4 result 1″98 with standard deviation 0″178. As a pair, rather, 0″93 ± 0″444 and 1″98 ± 0″178 suggest an experiment gone awry. So Eddington only could only have argued for the half prediction had he also dismissed the S4 plates, which were clearly the strongest of the three sets of plates.

Eddington did not simply stipulate in his accounts of the eclipse expedition that the S13 data should be dismissed (though it must be acknowledged that with each new, more popular, and less fine-grained account that Eddington published, the S13 data is acknowledged less and the rationale for its dismissal is more abbreviated). Eddington and his collaborators repeatedly provided for public scrutiny their reasons for rejecting the S13 plates as errant. Let us remember that the initial assessment of S13 systematic error was made on site by Crommelin, who offers in his field journal, his part of the November 6 joint meeting announcement, and his November 13 report on the expedition for *Nature* the argument that the S13 results were skewed by uneven solar heating of the mirrors. Alongside this argument Crommelin provides the raw data and the calculated angular deflection value and acknowledges that this deflection value would seem to confirm the Newtonian prediction. Indeed, as required by RS policy, all of the expeditions' exposed plates were made available for examination by other RS fellows.

So while it is correct to characterize Eddington's attention to the S13, S4, and P13 results as emphasizing the latter two, still S13 results were not entirely hidden from interlocutors' scrutiny. S13 results were mentioned at the RS–RAS joint meeting, as demonstrated by the account published in the *Observatory* in November 1919 and the RS report published in *Philosophical Transactions* in January 1920. Yet Eddington was not entirely transparent. Earman and Glymour (1980) argue that "Eddington won the argument by the power of the reference work" (p. 79); it is true that the major English-language explications of GR and the eclipse experiment were written by Eddington himself. These accounts also, among all of the expedition team's major accounts, give the least explicit recognition to the troublesome S13 results. In the preface to the second edition of his *Report on the Relativity Theory of Gravitation*, reissued in 1920 to accommodate the increased interest in relativity, Eddington recounts in broad strokes the experiment and the underlying theory. He identifies Newton with the half deflection value and Einstein with the full deflection and provides the P13 result with a probable error estimate, the S4 result without probable error, and the S13 value not at all. The experiment receives more extensive treatment in Eddington's more widely read *Space, Time, and Gravitation*. The S4 and P13 deflection values are provided with probable errors for each; the author defends his data analysis and anticipates some common objections; the S13 telescope is acknowledged as indicating the half deflection but no exact measurement is provided and the now-familiar argument for S13 systematic error due to uneven solar heating of the mirrors is recounted in some detail (Eddington, 1987, p. 117). While those present at the RS–RAS joint meeting and those reading the reports of the meeting in *Nature*, the *Observatory*, the RS *Philosophical Transactions*, and the *RAS Monthly Notices* were provided the deviant S13 result and the rationale for its dismissal in detail, those learning about the eclipse experiment from Eddington's book-length treatments encountered more curtailed arguments.

Earman and Glymour (1980) are not unaware of Eddington's proffered reasons for giving greater emphasis to some telescopes' results than others, but just unconvinced by these reasons: if blurriness was reason enough to dismiss the Sobral astrographic plates, they argue, then the Principe astrographic plates should be dismissed for blurriness too (p. 79). Also criticized is Eddington's use of Greenwich check plates in analyzing the Principe plates. Recall that detecting a change in apparent star-position due to the sun requires comparing the star-position during the eclipse with its position when the sun is elsewhere in the sky. This might mean waiting on site for months until the star in question can be observed at night, which presents a huge logistical complication for researchers far from home. This factor also exposes the experiment to possible observational error should the telescope's positioning be altered in the slightest way during that long interim. The Sobral team took this approach. Crommelin and Davidson stayed in Brazil for several weeks to take the necessary comparison photographs in mid-July and so did not return to England until August 25, which is why their results were unavailable at the September 12 BAAS meeting. But since Africa is farther east then Brazil, the Principe team would have had to wait much longer to take on-site comparison photographs (Eddington, 1987, p. 115). Stanley (2003) reports that the team was also pressured to leave before an upcoming steamboat strike (p. 77). For these reasons Eddington elected to use check plates from England instead. The good news was that these plates were of better quality than on-site check plates because of the observatory's better observation conditions and equipment. The bad news was that since the check plates were taken from a different location than the eclipse plates, to use the check plates to determine the average angular deflection experienced at Principe, Eddington made a series of adjustments in his data reduction, adjustments which Earman and Glymour (1980) call into question.[24]

Such seemingly suspicious considerations might lend credence to Waller's allegations of intellectual dishonesty. But if we look carefully at the reasons Eddington and his colleagues gave in defense of their data analysis, where and to whom these reasons were presented, and the actual critical response from contemporary observers, I suggest, the case for this allegation is seriously undermined. In the next section I draw attention to the contexts in which Eddington presented his work, specifically his data-analytical decisions, to the larger British scientific community, and I attempt to show how the reception granted to Eddington's testimony by this expert community may have had indirect evidential significance. We may gauge the level of critical scrutiny to which the 1919 eclipse expedition results were subjected by examining the historical record: what was not said by critics in print and at professional gatherings is as significant as what was said. The fact that British physicists and astronomers were capable of critically engaging those parts of Eddington's work which have since been labeled contentious, and the fact that those who publicly disputed Eddington's claim to have confirmed relativity did so on different grounds, suggest that while Eddington's expertise played a crucial role in this case, the widespread acceptance of his expert testimony was not entirely without corroboration.

5. The presentation of expedition results

Sponsel (2002) reconstructs what he describes as the *public relations campaign* made by Eddington and Dyson to prepare Britain for the November 6, 1919 "Revolution in Science" moment. I offer here no criticism of Sponsel's thesis, but only note that several key elements of this PR campaign may also be characterized as opportunities to expose the expedition results to the bracing air of professional criticism. From August through December 1919 Eddington gave lectures and led discussions on relativity and the 1919 eclipse expedition at several professional meetings. Among the participants at these events were many of Britain's finest physicists and astronomers, and while few of them possessed the expertise sufficient to vouch for Eddington's explication of Einstein's theory and its purported observational implications for the deflection of light, they could nevertheless quite competently speak to the soundness of his astronomical experimental procedure and data analytical choices. Here I want to focus on two important meetings, their participants, and the justification their proceedings lent the eclipse expedition results presented there. The first is Eddington's presentation of results at the October 22, 1919 meeting of the $\nabla^2 V$ Club at Cambridge, after which Dyson set the date for the RS–RAS joint meeting two weeks later. The second is the momentous joint meeting itself.

The 83rd $\nabla^2 V$ Club meeting on October 22, 1919 warrants special attention. Sponsel (2002) argues that this meeting's events seem to have given Eddington and Dyson sufficient confidence in their project to pursue the RS–RAS joint announcement, which Astronomer Royal Dyson had some discretion in scheduling (p. 457). The $\nabla^2 V$ Club had been founded at Cambridge in 1899 as a forum for discussing matters of mathematical physics; standard practice was for one member to give a paper, followed by discussion and informal socializing.[25] Meetings were held every few months from 1900 through 1916. By October 1919, however, the club had not met for two years because of the war. The meeting at which Eddington presented his eclipse results was the first $\nabla^2 V$ Club meeting after a long hiatus, which suggests that the club may have been reinvigorated expressly to give Eddington a forum to present his work to a small, private group of colleagues before the larger, more public RS–RAS meeting. Furthermore, while the $\nabla^2 V$ Club continued to meet regularly after the October 22 meeting in its previous fashion, Eddington himself attended only the very next meeting before being dropped from the roster at the 88th meeting, as required by club by laws stripping membership of those who had missed four consecutive meetings.

Records of the October 22 meeting are fairly cursory. The minutes tell us which members were present, who was newly elected, and that Eddington spoke on the topic of the weight of light as concerned his May expedition. He is reported to have presented the underlying theory, experimental setup, data analysis, and a mean deflection of 1″60 at Principe; a Sobral deflection value was alluded to indirectly as roughly 1″75 with a six percent probable error. Eddington characterized the presented results as confirming Einstein's theory. The meeting was not declared social until after midnight (which suggests that there was much critical discussion), at which point club president Ebenezer Cunningham declared that the meeting had been an historic occasion and joked that after Eddington's presentation the name of the club would perhaps need to be changed to something more "barbaric."[26]

Eddington did not write about the October 22 $\nabla^2 V$ Club meeting in retrospective accounts of the eclipse expedition, but there are at least three reasons why he might have chosen this forum prior to the RS–RAS joint meeting. First, he was at this time speaking on GR and the eclipse expedition in many contexts popular and professional: the $\nabla^2 V$ Club meeting was one more opportunity to get the message out. Second, the informality and privacy of the $\nabla^2 V$ Club proceedings gave Eddington a safe place to expose himself to critical scrutiny. Meeting minutes were handwritten and unpublished in popular or scientific presses; as was the custom the presented paper was not entered in the minutes but only briefly summarized. Beyond that brief account Eddington's testimony at the meeting was carried outward by word of mouth alone, which had the effect of relieving Eddington (and other presenters at other meetings) of facing detailed scrutiny of his precise remarks by those who had not been in attendance.

Third, the $\nabla^2 V$ Club was a collection of those British scientists particularly well suited to evaluate and potentially corroborate Eddington's claim to have confirmed relativity. From its founding the club took mathematical physics as its focus. Members were physicists, astronomers, and applied mathematicians; new members were added when nominated by a current member and approved by a supermajority of members present at the nominating meeting, a process which served to maintain this focus. The esteemed RS, by contrast, drew its members from many scientific specializations. The considerable attention given to the November 6 RS–RAS joint meeting notwithstanding, many of those RS members in attendance would have been poorly

[24] Cf. Dyson et al. (1920, p. 317), Eddington (1987, p. 115).

[25] Minute Books of the $\nabla^2 V$ Club (1919). I am grateful to the Archive for the History of Quantum Physics (AHQP) for the opportunity to read these minute books on microfilm.

[26] $\nabla^2 V$ represents the Laplace operator, in some ways a symbol for classical mechanics—thus Cunningham's joke. That Einstein's vindication would warrant a more "barbaric" name is perhaps an indicator of strained postwar relations.

positioned to judge Eddington's work for themselves. This point should not be exaggerated, of course, as many RS and RAS members *would* have had professional expertise enough to follow and critically engage those parts of the presentation. But as assembled on October 22, 1919, the ∇^2V Club was considerably denser with relevant experts. Cunningham, the meeting's host and club president, was the preeminent British expert on relativity before 1914. He had authored *Relativity and the Electron Theory*, and while he was not nearly so engaged by the time Einstein published the general theory, he was still capable of authoring a series of papers (Cunningham, 1919a–c) for *Nature* in December 1919 explaining SR and GR and their observational consequences so as to better familiarize the British scientific community with Einstein's newly hot theory. Also attending was the astronomer G. W. Searle, a founding ∇^2V Club member and one of the few Brits to correspond with Einstein about relativity before 1919; Searle would have been quite capable of evaluating claims at the intersection of relativity and astronomy. The portions of Eddington's presentation concerning relativity specifically may also have been competently evaluated by attending ∇^2V Club member L. A. Pars, Eddington's first research student, who would go on to win the prestigious Smith prize for his own GR work in 1921. Other astronomers present included club members Harold Jeffreys, W. M. H. Greaves, and E. A. Milne, all of whom would later serve as RAS presidents. Greaves was at the time Dyson's chief assistant at Greenwich Observatory, Milne would soon take over as assistant director of the Solar Physics Observatory at Cambridge, and Jeffreys would assume the Plumian professorship in astronomy at Cambridge then occupied by Eddington. Also present were W. M. Smart and H. F. Baker, both of whom held endowed chairs in astronomy at Cambridge.

For those interested in the epistemic implications of the ∇^2V Club presentation, the fact that Eddington's testimony there was only part of the testimony given at the RS–RAS meeting cannot be ignored. Does the epistemic support provided by the approval of the experts at the ∇^2V Club transfer to those results presented at the momentous RS–RAS meeting supposedly precipitating a "Revolution in Science"? Here the overlap between ∇^2V Club membership and RS and RAS membership is relevant. Some ∇^2V Club members at the October 22 meeting were RS fellows; even more were RAS fellows. These ∇^2V Club members were probably at the momentous November 6 meeting, then, though we cannot be sure since there is no record of those attending beyond those who are reported as having made substantial commentary. ∇^2V Club members who were also RS or RAS members and present at both meetings would have been in position to recognize the difference in the results presented at the two meetings; the fact that they did not dispute this difference at the later meeting or afterwards in print would have suggested to others who were aware of these individuals' credentials that the presentations made at the two meetings were consistent with one another. ∇^2V Club members who were also RS or RAS members but who missed the October 22 meeting (such as RS president J. J. Thomson) may likely have learned of the general pro-Eddington tenor of the meeting's events if not the specifics of his presentation, as meeting minutes were unavailable until after the RS–RAS joint meeting. RS and RAS fellows at the joint meeting who were not ∇^2V Club members (such as Oliver Lodge) may have learned indirectly of Eddington's success at the earlier meeting, while remaining unaware of the specifics of his presentation and how this presentation compared to the full expedition report given at the RS–RAS joint meeting. These considerations suggest that, despite their differences, events of the earlier meeting may have been taken as lending credence (perhaps, for some, too much credence) to the later meeting's more widely known announcement.

Had the community of British scientists been incapable of critically engaging Eddington's claim to have established the reality of relativistic gravitation, we would expect to find some profession of this incapacity or at the least critical silence in the historical record. But in *Nature* and *Observatory* from 1919 through 1920 we find several attempts by Eddington's colleagues to defend alternate explanations of the observed phenomenon of starlight deflection by the eclipsed sun.[27] Lodge (1919a–d) wrote several letters to journal editors, contributed articles, and gave public lectures defending his hypothesis that increased refractivity due to denser ether near the sun caused the starlight deflection. But on this hypothesis, which had been first proposed by Jonckheere (1916), the increased ether density required to produce the observed deflection would have to be very great. Such an incredibly dense refractive ether should also reflect a great deal of light, so stars near the sun should appear significantly dimmer than usual, Eddington (1919a) and Lindemann (1918) argued, but this was not observed to be the case.

If not an ethereal effect, Nevill (1919) suggested, perhaps the region around the sun might deflect starlight for some other non-relativistic reason. Jonckheere (1916) suggested that starlight deflection at the sun's limb may be attributable to the solar atmosphere. But if this were the case, Lindemann (1918) argued, refraction by the solar atmosphere should be observable when comets pass close by the sun; since no such effect is observed, the solar atmosphere objection should be dismissed. Silberstein suggested at the December 1919 RAS meeting that the fact that star displacements on the S4 photographic plates were not uniformly radial was evidence that these displacements were not caused by gravity but some irregular refracting solar medium; this concern would be addressed by Bauer at a 1920 American Philosophy Society meeting in Philadelphia.[28]

In several letters to *Nature*, Anderson (1919a, b, 1920a, b) articulated, defended, and eventually abandoned the hypothesis that uneven cooling of the earth's upper atmosphere during the eclipse caused the observed starlight deflection. On December 4, Anderson suggested that the moon's shadow may cause a precipitous drop in atmospheric temperature during totality, so the temperature differential between cooled and uncooled atmosphere could account for the apparent change in star-position without appeal to relativity. Eddington (1919b) soon responded, arguing that any such effect would be tiny, at most one-twentieth the Einstein deflection. On Anderson's hypothesis, Eddington argued, an observer on site (as he had been at Principe) would have had to experience a temperature drop of 20 °C/min. Crommelin (1919) also responded, testifying that there had been little temperature change at Sobral and arguing that on Anderson's hypothesis the moving shadow of the moon through the atmosphere should have triggered asymmetric displacement-effects on the various stars across the different plates exposed at the beginning and end of eclipse totality, but no such asymmetry was observed. *Nature* also published letters from Dines (1919) and Richardson (1919) of Benson Observatory confirming that the eclipse had a minimal observed effect on atmospheric temperature. Anderson responded on December 18, accepting the objections to his hypothesis while insisting that air refraction during eclipses remained a difficult matter for analysis. He would write two more letters to *Nature* in January 1920, but in neither would he purport to answer the objection that the observed temperature change on the ground was far too small to account

[27] Cf. Joint eclipse meeting of the Royal Society and the Royal Astronomical Society, 1919 November 6 (1919).

[28] Cf. Societies and academies (1920b, p. 842).

for the observed starlight deflection. The objection died soon after that.[29]

Eddington spoke and led discussions on relativity and the eclipse results at the November 6 RS–RAS joint meeting, a packed public lecture at Cambridge on December 11, the December 12 RAS meeting, the February 5, 1920 RS meeting, and the March 26, 1920 meeting of the Physical Society. Each of these events saw spirited discussions of the merits of relativity. Einstein's work was defended at least in part by such renowned physicists and astronomers as Eddington, Dyson, Cunningham, Lindemann, Jeffreys, and Jeans, and criticized at least in part by such similarly renowned colleagues as Lodge, Silberstein, and Jonckheere. Accounts of these meetings report debate over the theory itself, the perihelion of Mercury, the red shift, and implications for the ether, but there is no recorded criticism of Eddington's data-analytical methodology.[30]

The British response to Eddington's work might be contrasted with the response by German astronomer Freundlich, whose 1931 criticism of the 1919 British expedition's results anticipated in part more recent criticisms.[31] As Hentschel (1994) has carefully documented, after several expeditions from 1914 through 1926 were doomed by weather and war, Freundlich finally was able to observe with some success the solar eclipse of May 9, 1929 in North Sumatra. Freundlich calculated a mean displacement at the sun's limb of $2''24\pm0''10$, considerably at odds with both relativity theory and Eddington's reported results. In addressing this discrepancy Freundlich and his collaborators criticized Eddington's data reduction procedure and posited a corrected 1919 mean displacement closer to $2''2$ (Hentschel, 1994, p. 182). Yet majority opinion among physicists, astrophysicists, and astronomers in 1931 was not with Freundlich on this matter (Hentschel, 1994, p. 191) and criticism of this sort seems not to have been part of the British response to GR in the late 1910s and 1920s.

6. The epistemic significance of colleagues' approval

I do not mean to suggest that Eddington was faultless in either his data analysis or public presentation of results. Nor were conditions ideal for critical evaluation of those parts of claims not completely grounded in his special expertise in relativity. But even in those epistemically impoverished circumstances, the British reception of GR was not the mere blind acceptance of a partisan researcher's biased claims.

Revisiting the 1919 eclipse expedition reminds us of the contingency of the outcome, of the way in which Eddington's testimony was presented, evaluated, and propagated in scientific and popular circles until Einstein's glorious success became well-credentialed conventional wisdom. Some of the contingencies were under Eddington's control, others were not. One factor largely beyond Eddington's control was the difficulty of replicating the results of this experiment. As a result corroborating and conflicting experimental evidence was scarce. Given this limitation, I offer with some confidence two counterfactual claims: had the contrary Lick results been widely available in published form prior to the RS–RAS joint meeting, the British reception of GR would have been seriously tempered, and had the May 29, 1919 eclipse not been taken as strong support for Einstein (whether due to weather, different data-analytical choices, or a more skeptical public), the British reception to GR would likely have remained cool for many years after. Consider again Eddington's consistent presentation of expected experimental outcomes as a trichotomy. This device framed the debate so that greater deflection was interpreted as evidence for GR, even when, as with the S4 data, the average deflection observed was twice the probable error beyond the relativistic prediction. Had a fourth or fifth potential outcome greater than $1.74''$ been included in the conceptual space, the consequent interpretation of the exposed photographic plates may have turned out differently.

The lesson is that *how* an experiment is framed affects *what* it is taken to show; with this comes the corollary that *who* gets to frame the conceptual space of expected outcomes then also controls in part what the rest of the epistemic community takes the experiment to show. I hasten to add, however, that in this particular case Eddington's unrivaled expertise in relativity did not also accord him unchecked cognitive authority in framing the starlight-deflection debate, as evidenced by opponents such as Lodge and Anderson attempting to undercut the pro-Einstein conclusion by challenging not the experimental results but what these results allegedly proved. Their ability to articulate and defend alternative non-relativistic explanations even in a losing effort suggests that the trichotomy was not a perfect framing device. Eddington's colleagues did not address the question "Newton or Einstein?" by his fiat but because of their own professional commitments and antagonism toward relativity on the ether issue. By contrast, on Hentschel's reading of Einstein's interest in the 1919 eclipse, the theoretical dispute important for Germans at the time concerned the qualitative issue of deflection and no deflection. For these reasons one might take issue with the characterization of Eddington's framing device as a false trichotomy: given the social context of postwar British science, Eddington was not solely responsible for the British neglect of other hypotheses.

Let us also return to the matter of Eddington's method of data reduction for the P13 results, specifically his use of check plates since criticized by Earman and Glymour. I do not claim that Eddington's chosen method is beyond criticism, but this method and the consequent P13 results were made explicit in Eddington's *Space, Time, and Gravitation*, his ∇^2V Club presentation, the RS–RAS joint meeting, the official expedition reports in the RS *Philosophical Transactions* and *RAS Monthly Notices*, and other talks and publications. Many physicists and astronomers capable of critically evaluating these choices were exposed to Eddington's work in person and in print, yet the historical record gives no indication that these experts raised objections against the use of check plates in the Principe data reduction. Similarly, those present at the RS–RAS joint meeting and those who read Crommelin's report in *Nature* and Eddington's account in *Space, Time, and Gravitation* were presented with the reasons for dismissing the S13 results as errant. (The ∇^2V Club presentation gave its audience no such reasons because the Sobral data was not presented there.) Issues of data-reduction and the attribution of error would have been familiar territory for contemporary professional astronomers; no special expertise in GR was needed. As Eddington (1987) wrote of his use of check plates, "In comparing two plates, various allowances have to be made for refraction, aberration, plate-orientation, etc.; but since these occur equally in determinations of stellar parallax, for which much greater accuracy is required, the necessary procedure is well known to astronomers" (pp. 115–116). Yet qualified contemporary British astronomers appear not to have registered public criticism on this matter.

[29] Cf. Orange (1920) for a proposed test of Anderson's hypothesis by comparing the moon's observed diameter at eclipse and when full. Crommelin (1920) responded that the photographs from May 29, 1919 could not be used for such a test due to their long exposures and ill-defined images of the moon, and warned against assuming that the moon's "dark" and "bright" diameters are the same.

[30] Notes (1920).

[31] My thanks to this journal's anonymous reviewer for pressing the importance of Freundlich's critique here.

To our eyes the fact that the relativistic prediction falls well beyond a standard deviation from the S4 average observed deflection may seem fairly damning. Eddington's identification of twice the probable error as the margin of safety for ruling out hypotheses may seem convenient, since this standard works to barely rule out the classical prediction against the P13 result and *not* rule out the relativistic prediction against the S4 result. But let us remember that Eddington's interlocutors, whether or not they understood relativity, surely had the necessary professional experience and access to the expedition's plates to check his estimates of probable error and challenge his chosen margin of safety. (Did any of his interlocutors actually check Eddington's math? I have found no historical evidence of this, but then checking another scientist's figures and finding them satisfactory is not the sort of result likely to be published.) The fact that we do not find challenges to Eddington's standard of twice the probable error as a margin of safety provides good indirect evidence, I think, that this standard was widely accepted in the relevant scientific community of astrophysicists.

One crucial area where his colleagues were forced to rely heavily on Eddington's unrivaled expertise was in the derivation of deflection predictions from a relativistic theory of gravitation. This derivation had been laid out by De Sitter (1916b, 1917a, b) and Eddington (1918), but few British scientists would have claimed to understand this work fully. Most readers even in physics and astronomy would have had to trust that Einstein, de Sitter, and Eddington carried out these derivations of observational implications from theoretical commitments correctly. Given the ravaging of international scientific trust by WWI, British readers would have had to trust that Eddington and de Sitter were competent and qualified in vouching for Einstein's work.

Attention to issues of trust can show how political, social, and moral facts can have genuine epistemic significance. Eddington trusted Einstein more than most Brits, and in turn they trusted their colleague Eddington as they would not have trusted a German scientist. Neither Eddington nor his colleagues were acting obviously epistemically irresponsible in these attitudes of trust and mistrust. Failure to disavow wartime actions of one's government was, for many, evidence against "enemy" scientists' trustworthiness. The difference between Eddington and his fellow British scientists was his desire and, as RAS Secretary, his ability to distinguish a trustworthy internationalist "enemy" such as Einstein from pro-war scientists.

7. Conclusions

My argument has been that, in these matters which have come to seem an acute professional embarrassment for Eddington, in fact his British colleagues in astronomy and physics were well equipped to draw upon their own expertise to make informed critical evaluations of his work. Consequently these colleagues' acceptance of Eddington's claim to have confirmed Einstein's revolutionary new theory would have been justified *in part*, though *not in full* by their trust in Eddington as the main British expert on relativity. A larger group of British scientists and the British public would have had good reason to believe these other experts could competently critically evaluate these parts of Eddington's testimony concerning experimental setup and data analysis. For this larger group, the fact that these parts of his testimony were not criticized constituted partial corroborating evidence of the reliability of the testimony, evidence provided by those experts capable of judging Eddington's claims. Thus even those without expertise in experimental astrophysics were not epistemically dependent on Eddington alone in coming to believe that there had been a revolution in science, that there was a new theory of the universe, and that Newtonian mechanics had at long last been overthrown.

If this account of the 1919 eclipse expedition convinces, an implication is that the epistemic resources and responsibilities of testifiers, listeners, and corroborators are brought to the fore. We are reminded that epistemic dependence and differential expertise can have both worrisome and liberating implications, and that expert testimony is rarely offered or accepted in a vacuum. We are reminded, as Collins and Evans (2007) have recently emphasized in their "periodic table" of expertise, that experts come in different dimensions and degrees. Even in cases of significant epistemic inequality, where one speaker's expertise stands out above the rest, corroboration of this expert's testimony by others with relevant partial expertise in the domain can provide crucial evidential support.

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