

Testing relativity from the 1919 eclipse—a question of bias

Daniel Kennefick

When interpreting experimental results, context is everything. The researchers who took and analyzed the most important eclipse data had good reasons for judging the experiment a victory for Albert Einstein.

Daniel Kennefick is an assistant professor of physics at the University of Arkansas at Fayetteville.

One of the most famous measurements in the history of 20th-century astronomy was made over the course of several months in 1919. Teams of observers from the Greenwich and Cambridge observatories in the UK traveled to Brazil and western Africa to observe a total solar eclipse that took place on 29 May 1919. Their aim was to establish whether the paths of light rays were deflected in passing through the strong gravitational field of the Sun. Their observations were subsequently presented as establishing the soundness of general relativity; that is, the observations were more consistent with the predictions of the new gravitational theory developed by Albert Einstein than with the traditional Newtonian theory.

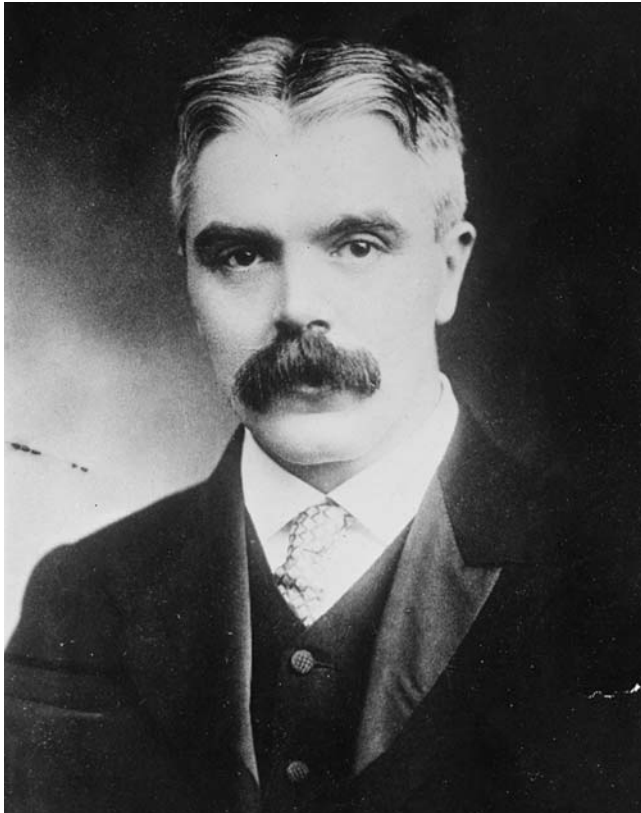
In recent decades many physicists and historians of science have cast doubt on the soundness of the famous experiment. They claim that the measurements made in 1919 were not sufficiently accurate to decide between the Einsteinian and Newtonian theories of gravity. It has been further alleged, especially by some philosophers of science, that the conclusion in favor of Einstein was motivated by bias on the part of the expeditions' most famous member, Arthur Stanley Eddington. Eddington was known to be an enthusiastic proponent of general relativity and is said to have been anxious to make a gesture toward reconciliation between the UK and Germany in the aftermath of World War I by verifying the theory of one of Germany's leading men of science, who, like Eddington himself, was a pacifist.¹ Thus the 1919 eclipse is nowadays sometimes given as a prime example of experimenters fitting their data to the expected result—the so-called predictor effect.

The story that the 1919 eclipse was not the decisive experiment it was cracked up to be has two versions. One, common among physicists since at least the 1970s, goes to accuracy: The experimenters were simply lucky to get reasonably close to one of the two predictions, so the experiment does not constitute a really viable test of the theories. The other story, common among philosophers and historians of science but beginning to find a popular audience, originates in a 1980 paper by philosophers John Earman and Clark Glymour.² They specifically charge Eddington and his collaborators with throwing out data that appeared to support Isaac Newton rather than Einstein. Some modern critics have charged that such action was not justifiable on scientific grounds and was more likely motivated by Eddington's theoretical and political bias.

Of course, it's not possible to be certain about any reconstruction of nearly century-old experimental decisions, but I argue that the balance of evidence lies heavily in favor of the view that the leaders of the 1919 expedition, Frank Watson Dyson and Eddington, had reasonable grounds for judging that their results were inconsistent with the prediction of



Arthur Stanley Eddington (1882–1944). In 1919 Eddington had already acquired a major reputation as a result of his work on the internal structure of stars. His enthusiasm for general relativity has led some historians to accuse him of bias in the analysis of the 1919 eclipse data. (Courtesy of the AIP Emilio Segrè Visual Archives.)



Frank Watson Dyson (1868–1939). The astronomer royal for England and director of the Royal Greenwich Observatory, Dyson was principally responsible for organizing the expedition of 1919. Experienced both in techniques of astrometry and in eclipse expeditions, he took charge of the data analysis of the Greenwich expedition to Sobral, Brazil. (Courtesy of the Library of Congress, George Grantham Bain Collection.)

Newtonian theory. Indeed, their treatment of the data appears to be vindicated by a subsequent 1979 reanalysis of their plates using modern astrometric data-reduction methods. Still, the two researchers did not believe they had said the last word. Indeed, Dyson and his collaborators went to great lengths to try to replicate the experiment at the total eclipse of 1922.

The expedition

In a 1911 paper, Einstein first predicted that light would fall in a gravitational field, so starlight passing close to the limb of the Sun would be deflected from its path.³ He calculated that the observed position of a star whose light passed near the Sun would change by 0.87 arcsecond (0.87"). His analysis was based on his understanding of basic features a relativistic theory of gravity must include, in particular the equivalence principle. The equivalence principle demands that all masses must fall at the same rate in a gravitational field.

Eddington and Dyson labeled the value Einstein calculated in 1911 as the "Newtonian" value, a label justified by the subsequent discovery that a similar value based only on Newtonian physics had been published in 1804 by the German astronomer Johann Georg von Soldner.⁴ In 1916, after he had developed the final version of his theory of general relativity, Einstein realized that there was an additional component to the light-deflection effect caused by the way that the Sun's mass curves spacetime around itself. Thus a straight path, or geodesic, near the Sun is curved, compared with a

path through flat space. The extra deflection caused by that curvature is comparable to the deflection due solely to falling, so that the general relativistic prediction calls for twice as great a shift in stellar positions—about 1.75" at the limb of the Sun—as does the Newtonian theory.⁵

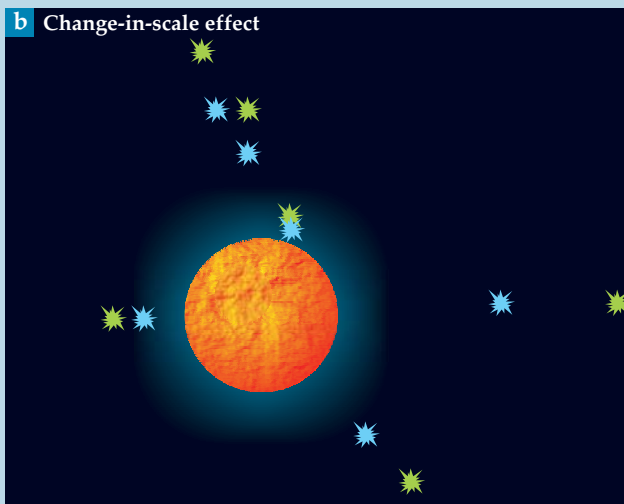
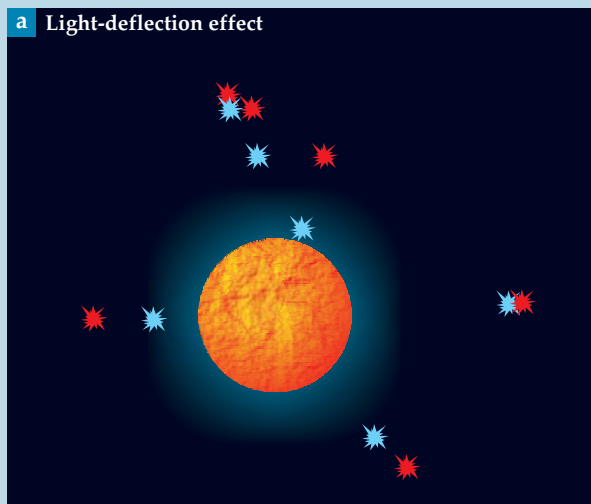
As early as 1913, Einstein wrote to leading astronomers, trying to interest them in making a measurement of the effect he had predicted. Stars are not normally visible close to the Sun, though, a problem that required astronomers to take pictures of a field of stars around the Sun during a total solar eclipse. That meant laborious travel to regions where an eclipse was predicted to take place. Before 1919, several attempts to measure the effect were foiled by a combination of bad weather and World War I.⁶ Given that Einstein changed his prediction in 1916, it was perhaps fortunate that expeditions before that date had not been successful.

The 1919 eclipse was recognized as a particularly favorable opportunity because of the presence of unusually bright stars belonging to the Hyades cluster close to the Sun during the eclipse. Moreover, by that time Einstein's theory had gained considerable prominence because of its success in explaining Mercury's anomalous perihelion advance as a perturbation in its orbit caused by the bending of spacetime by the Sun.

The man who recognized the significance of the 1919 eclipse was Dyson (not related to the physicist Freeman Dyson), England's astronomer royal and director of the Royal Greenwich Observatory.⁷ The man who had pointed out to Dyson the importance of Einstein's new theory was Eddington, director of the Cambridge University Observatory. Dyson, as chairman of the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society, appointed Eddington to a subcommittee formed to prepare for an expedition to observe the 1919 eclipse. Although it seemed that war might frustrate their efforts, the abrupt end of hostilities in November 1918 occurred just in time to make the expedition possible. Eddington, taking with him a Northamptonshire clockmaker named Edwin Turner Cottingham, traveled to a station on the island of Principe just off the coast of western Africa, close to the equator. Dyson sent two of his Greenwich assistants, Charles Davidson and Andrew Crommelin, to a station at Sobral in northern Brazil.

Probably the most famous illustration of Eddington's alleged bias in favor of Einstein's theory is a story subsequently repeated by Eddington himself in which Dyson, in explaining the experiment to Cottingham before departure, told the clockmaker that there were three theoretically plausible results: no deflection; half deflection, which would show that light had mass, and vindicate Newton; and full deflection, which would vindicate Einstein. Gathering that the greater the deflection the more theoretically exciting and novel the result, Cottingham asked what would happen if they obtained twice the Einstein deflection. "Then," replied Dyson, "Eddington will go mad, and you will have to come home alone."⁸

The two expeditions left the UK in March and arrived at their stations in good time for the eclipse. On the day of the eclipse, 29 May, Eddington was disappointed by heavy clouds, but they thinned sufficiently over the course of the eclipse for him to obtain images of the brightest stars on the last few exposures he took. Meanwhile, the Greenwich team in Sobral was favored by fine weather during the eclipse but troubled by the failure of its main instrument, an astrographic lens. Created for use in photographic all-sky surveys, astrographic lenses were designed to have an unusually wide field of view. The backup instrument, a 4-inch lens, performed well. But with its narrower field of view, the 4-inch



An unusual number of bright stars from the Hyades cluster conveniently filled the sky on all sides of the Sun during the 1919 eclipse. **(a)** In this artist's rendering, deflection of starlight from the Sun's gravity shifts the original stellar positions (blue) radially away from the center of the Sun; those closest to the center suffer the greatest shift. **(b)** A change in scale between one exposure and another shifts stellar positions radially away from the center of the image; those far from the center show the greatest shifts.

lens showed fewer stars on its plates than the astrographic one would have.

Data analysis

The fortunate circumstance that the Sun would be in a field containing relatively bright stars gave the astronomers an excellent chance of acquiring good quality images of stars close to the Sun, where fainter stars would be drowned out by the light of the solar corona. The predicted amount of the apparent shift in star positions was, they believed, within the level of accuracy achievable by contemporary astrometric techniques, even allowing for the technical difficulty imposed by transporting delicate equipment to remote locations before installation.

The parsec is defined as the distance at which a star, seen from Earth, will undergo apparent motion, due to parallax, of one arcsecond over the course of a year. No stars are within a parsec of our solar system; therefore, all stellar parallax work is subarcsecond in nature, much of it well below the arcsecond. Dyson had considerable experience in working on stellar parallaxes. In fact, both Dyson and Eddington began their careers working on problems of astrometry, including parallax and proper motion of stars and other bodies.

The method used to determine the apparent shifts was to expose pictures of the star field during the eclipse and then take comparison exposures of the same star field at night, without the Sun present. Obviously, the comparison plates had to be taken at a different time of the year, because it would take some time for the Sun to move out of the Hyades star field. It was also desirable that they be taken at a time when the star field was at the same position in the sky, which meant waiting at the eclipse site until the Sun had moved far enough along the ecliptic for the star field to rise to that same height in the sky before sunrise.

For the Sobral team, for whom the eclipse took place in the morning, that meant waiting only a couple of months, which is what it did. But the Principe team, for whom totality occurred in the middle of the day, would have had to wait almost half a year to take comparison plates on site, which

Eddington did not do. Instead, comparison exposures were taken in the UK before departure.

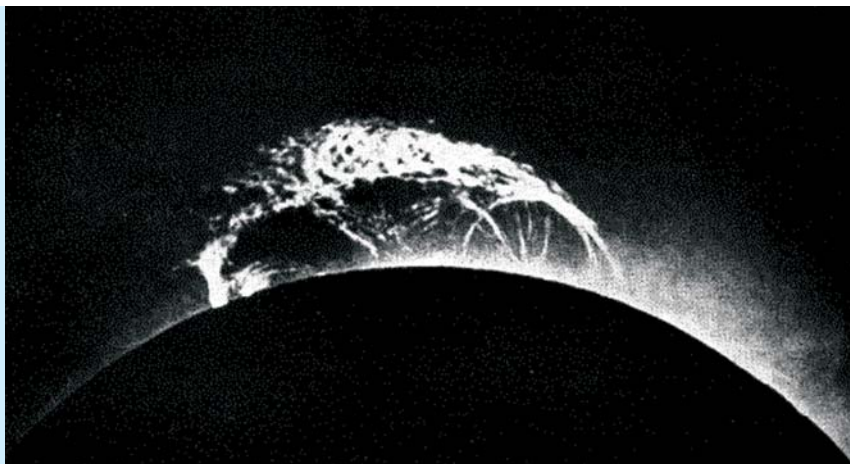
Since problematic changes of scale and other complications might arise from comparing exposures at different times and locations and with different installations of the same equipment, the Cambridge team also took so-called check plates of a different star field both in the UK and on Principe. Those plates would alert the team about any unexpectedly large change of scale between the eclipse and comparison plates. And because the Sun never appeared in either set of check plates, they constituted a control on Eddington's experiment. Indeed, that passive role seems to have been Eddington's original plan for them.

When comparing two different images of the same star field taken at different times, one must account for certain shifts in stellar position caused by predictable astronomical and atmospheric effects. Even when taken with the same instrument, two images of the same field may be rotated slightly with respect to each other or, worst of all, may vary in magnification, which introduces a relative change of scale between the two images. That change of scale is the most pernicious effect from the point of view of someone interested in measuring light deflection, because it most closely mimics the light-deflection effect. Light deflection moves stars radially away from the Sun on the image. A change of scale moves stars radially away from the center of the image, where the Sun is best placed in order to get a symmetrical field of stars close to it.

Fortunately, there is one characteristic difference between the two effects. Light deflection is greatest for those stars closest to the limb of the Sun and minimal for those stars far from it. The reverse is true under a change of scale: Stars far from the center of the plate suffer the greatest change in position, while stars near the plate's center are affected least. Thus straightforward comparison of the positions of a number of stars on the two plates can, in principle, disentangle the effects.

Eddington's difficulty with his eclipse plates taken on Principe was that only the brightest stars were visible on a

A remarkable solar prominence that occurred during the 1919 eclipse, photographed from Principe, an island off the coast of western Africa. (Courtesy of the Royal Astronomical Society, London.)



couple of plates taken through thinning clouds at the end of totality. With only a few stars to work with, his chances of disentangling shifts caused by scale changes from those caused by light deflection were much reduced. Effectively half of his precious information would be devoted to measuring a number, the scale change, which was of no intrinsic interest.

Eddington therefore turned to an alternative method of data reduction, in which he measured the change in scale between the check plates taken in Oxford and those taken on Principe. He assumed that the change of scale was the same as the one between the comparison plates taken in Oxford and the eclipse plates taken on Principe. He could then plug that number into his equations for those plates. Thus all of the measurement information he had available would be going toward establishing the numbers he really cared about—the light deflection for each star.

As Eddington himself acknowledged, that method had not been his original plan. Both teams intended to forestall any arguments against their data reduction by directly measuring the scale change between their eclipse and comparison plates. After all, Eddington's check plates on Principe were taken at night, and the eclipse plates were taken during the day of a star field in a different part of the sky. It was impossible to really know whether changes in temperature and other environmental conditions might have changed the scale in the two sets of plates.

In response to the uncertainty, Eddington emphasized the stability in temperature of the humid tropical air at both stations. A well-known feature of eclipses is the sudden drop in temperature of several degrees during totality, as the shadow of the Moon, on the order of a hundred miles wide, sweeps across the land. But on Principe, with its humid tropical conditions and cloudy weather, the temperature barely changed during totality, according to Eddington; the variation was less than a degree between the daytime temperature at the time of the eclipse and the nighttime temperature while the check plates were taken.⁹ Thus Eddington felt confident that the change of scale measured from the check plates could be applied successfully to the eclipse-comparison plates.

While Eddington was sitting in Cambridge working on his revised data analysis scheme, Crommelin and Davidson in Sobral were taking their comparison plates and then sailing back to the UK, which they reached by 25 August. In September in Greenwich, Davidson and another Greenwich assistant, Herbert Henry Furner, began measuring star positions on the plates under the supervision of Dyson himself. Little survives of the Cambridge data, but nearly all of

the plates and dozens of sheets of the data reduction are extant in the Greenwich archives. The material offers a good picture of what transpired there. Most significantly, no evidence exists that Eddington was ever present for, or participated in, any of the Sobral data reduction. Dyson's handwriting appears in the Sobral data-reduction notes at many key points, but Eddington's does not appear anywhere. Furthermore, Eddington's side of an exchange of letters between the two men is preserved in the archives. Consider this 3 October 1919 reply to a lost letter from Dyson:

Dear Dyson,

I was very glad to have your letter & measures. I am glad the Cortie plates gave the full deflection not only because of theory, but because I had been worrying over the Principe plates and could not see any possible way of reconciling them with the half deflection. I thought perhaps I had been rash in adopting my scale from few measures. I have now completed my definite determination of A (5 different Principe v. 5 different Oxford plates), it is not greatly different from the provisional though it reduces my values of the deflection a little. (Arthur S. Eddington to Frank W. Dyson, 3 October 1919, MS.RGO.8/150, Cambridge University Library)

The quote certainly showcases Eddington's theory-centric approach to the data analysis. But it also makes clear that his first knowledge of the reduction of data from the 4-inch plates (referred to here as the Cortie plates, after Aloysius Cortie, the Jesuit priest who loaned the 4-inch lens to the expedition) was in a letter from Dyson. Apparently, Eddington had been previously informed of the results of the Sobral astrographic data reduction, but his response was not to intervene in the analysis of the Greenwich plates. Rather, he reviewed his own plates in an effort to reconcile them with the reported result from Sobral that favored the half-deflection Newtonian result.

Accordingly, we must believe that it was Dyson who made the decision to ignore the results from the astrographic plates and rely instead on those from the 4-inch-lens exposures. Therefore, it seems more relevant to inquire into Dyson's bias for or against either theory than to worry about Eddington's.

In fact, like the vast majority of astronomers at the time, Dyson was moderately skeptical of general relativity. In an 18 March 1920 letter to Frank Schlesinger, director of the

Yale Observatory, he stated,

The result was contrary to my expectations, but since we obtained it I have tried to understand the Relativity business, & it is certainly very *comprehensive*, though elusive and difficult. (MS.RGO.8/123, Cambridge University Library)

Eddington wrote similarly to the mathematician Hermann Weyl on 18 August 1920:

It was Dyson's enthusiasm that got the eclipse expeditions ready to start in spite of very great difficulties. He was at that time very skeptical about the theory though deeply interested in it; and he realized its very great importance. (Hermann Weyl Nachlass, Hs 91:523, ETH-Bibliothek Zürich)

Regarding the question of reconciliation with Germany, often cited as a further motivation for Eddington's bias toward Einstein's theory, it is probable that in that case, too, Dyson held more mainstream views. Dyson's obituary (published in 1939) states that he helped further postwar reconciliation, but that should be set in context. For the first few years of its existence, the International Astronomical Union, an organization in whose formation he played a key role after World War I, did not permit Germany or its allies membership. A good example of the typical English astronomer's view of both relativity theory and German science is given by a letter between two astronomers preserved in the Royal Greenwich Observatory archives with the eclipse material:

The second theory of Einstein [general relativity] ... is far more speculative and I fear only accord with observations will make me accept it. Besides the analysis is too beastly for words. I can well understand the compatriots of Riemann and Christoffel burning Louvain and sinking the Lusitania. (Rudolph Moritz to Philip H. Cowell, 1 March 1918, MS.RGO.8/123, Cambridge University Library)

But even if Dyson was not biased toward relativity at the outset, might he not have been swayed by the visionary certainty of his younger and more theoretically up-to-the-minute colleague Eddington? There is little reason to think so. Dyson was the senior man in British astronomy, and although Eddington's fame is nowadays much greater, the

two were on a roughly equal footing in terms of their public fame and scientific reputation at the time of the eclipse. Furthermore, there are good grounds for believing that Dyson made the scientifically correct decision in choosing to ignore the astrographic data.

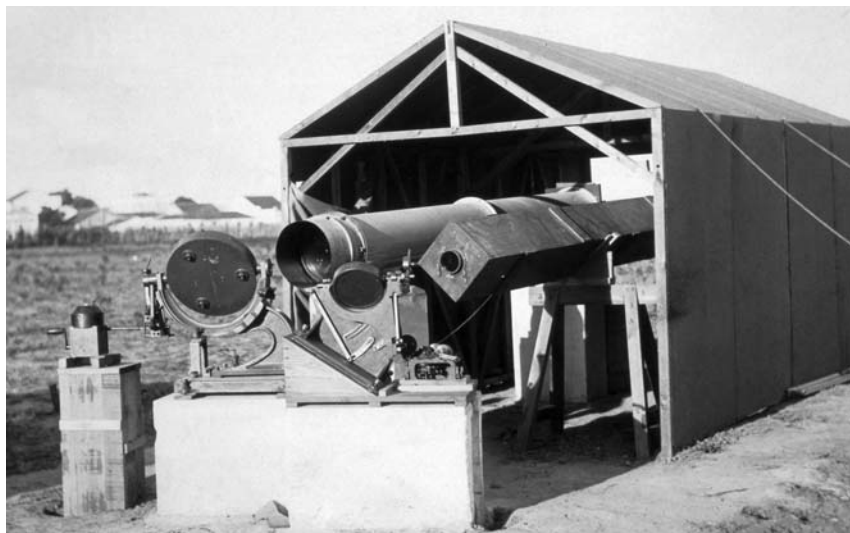
1979 reanalysis

The Greenwich team had planned from the beginning to make its astrographic lens its main instrument. But that lens had never been used at an eclipse, and fears of problems with the mirror and its driving mechanism encouraged the Sobral team to bring a backup instrument based on the Cortie 4-inch lens. In the immediate aftermath of the eclipse, onsite development of some plates alerted Crommelin and Davidson that the astrographic setup had lost focus during the eclipse. The stars were noticeably streaky, a problem reported by Dyson at a meeting of the Royal Astronomical Society as early as 13 June.¹⁰ Disturbingly, when the comparison plates were taken two months later, the instrument was once again in focus.¹¹

Despite any worries about the quality of the plates, Dyson and his team went ahead and reduced the astrographic data first. Nevertheless, they encountered significant difficulties in measuring the plates. Because of the imprecision of the streaky and out-of-focus images, they measured the star positions on the astrographic eclipse plate in only one coordinate. Having thus thrown away half their data at the outset, they pressed on and recovered the controversial result of $0.93''$, which they reported to Eddington sometime before 3 October. Once they had reduced the 4-inch-lens data, aided by the sharp focus obtained on the eclipse plates, they were confronted with the problem that their two instruments had produced measurements in profound disagreement with each other.

Interestingly, the Greenwich team tried an alternative method of analysis with the astrographic data. In Dyson's section of the joint report, he states that an alternative method of analysis recovered a result of $1.52''$ from the Sobral astrographic data. A later 1921 paper essentially repeats the remark (quoting slightly different figures for unstated reasons):

If it is assumed that the scale has changed, then the Einstein deflection from the series of plates is $0.90''$; if it is assumed that no real change of focus occurred, but merely a blurring of the images, the result is $1.56''$; little weight is, however, attached to this series of photographs.¹¹



Instruments at Sobral, Brazil.

The 4-inch lens is in the square tube on the right, and the astrographic lens, chosen for its wide field of view, is in the circular tube on the left. In front of the tubes are mirrors that are driven by a mechanism that keeps the stellar images at the same position on the plates during an exposure. The mirror on the left was the chief suspect in the poor-quality astrographic-lens images produced during the 1919 eclipse. (Courtesy of the Science Museum, London.)

What that means is that Dyson and his colleagues made an attempt at something close to Eddington's method of analysis. They did not have check plates, as he did, but they did take the comparison plates at the same site and with the same equipment as the eclipse plates. If Eddington could assume that no change of scale occurred between daytime eclipse plates and nighttime check plates on Principe, might the same trick work for Sobral, where the tropical conditions also meant little change in temperature? They calculated the change in scale due to known astronomical effects and applied the change to differences in star positions between eclipse and comparison plates. The result was a value greater than 1.5" for the light deflection, not far off Eddington's Principe result. The implication, then, was that the data analysis of the astrographic plates had uncovered an unexpectedly large change of scale that was due to some defect in the instrumentation.

Dyson and his collaborators probably argued along the following lines. If their calculation of a large change of scale in the astrographic plates was correct, then the instrument must have undergone a significant change in magnification due to the temperature change during the eclipse. That would mean that the deflection value measured was consistent with Newtonian theory. Alternatively, if one argued that the instrument might have simply lost focus, with no problematic change of scale having taken place, then the implied result was more consistent with the Einsteinian theory and with the results obtained by the Sobral 4-inch and Principe astrographic lenses. Support for the Newtonian theory was thus, in some sense, logically incompatible with the instruments having behaved in the intended manner. I suspect that line of argument strongly influenced the Greenwich team's decision to exclude the astrographic data from their final report.

Interestingly, a modern 1979 reanalysis of the data undertaken at the Royal Greenwich Observatory supports that view. At the behest of then director Francis Graham Smith and Andrew Murray, the observatory's astrometry expert, Geoffrey Harvey and E. D. Clements took out the 1919 plates from the two Sobral instruments and measured star positions using a modern plate-measuring machine. Data were then analyzed by astrometric data-reduction software written by Murray. The table shown here compares Harvey's results with those of the original 1919 team (all quantities in arcseconds).¹²

A comparison of data

Instrument	1919 result	1979 result
4-inch lens	$1.98'' \pm 0.18''$	$1.90'' \pm 0.11''$
Astrographic lens	0.93"	$1.55'' \pm 0.34''$

Recall that Dyson's alternative result for the astrographic data was 1.52" (with no error given).

The results of the 4-inch-lens instrument agree rather well with the original measures. What is most striking is the close agreement between the result for the astrographic lens and the alternative value given by Dyson and Crommelin in 1919. Although it could be coincidence, the reanalysis provides after-the-fact justification for the view that the real problem with the Sobral astrographic data was the difficulty, with the limited means available in 1919, of separating the scale change from the light deflection.

Ironically, however, the 1979 paper had no impact on the emerging story that something was fishy about the 1919 experiment. Indeed, so far as I can tell, the paper has never been cited by anyone except for a brief, vague reference in Stephen

Hawking's *A Brief History of Time*.¹³ Hawking, however, recalled the reanalysis as showing that the original measurement could not have achieved the accuracy it claimed for itself, which prompted a member of the 1979 team to issue a clarification letter.¹⁴

Gaining perspective

When interpreting experimental results, context is everything. The last professional eclipse expedition to perform the light-bending experiment was in 1973, led by a University of Texas team that was motivated in part by a desire to test Einstein's theory against the Jordan-Fierz-Brans-Dicke scalar-tensor theory. Discriminating between the predictions of those theories demanded far greater precision than could have been achieved in 1919 and may have fostered a more critical evaluation of the earlier experiment. Since 1973, radio astronomers have been able to perform the measurement more accurately by observing quasars being occulted by the Sun.¹⁵

Similarly, philosophers like Earman and Glymour are grappling with the epistemological issue of whether it is really possible for theories to be overthrown by individual experiments. The 1919 measurements were not sufficient, by themselves, to overthrow Newton. Unfortunately, that rather subtle point has become coarsened by repetition into a charge that Eddington and Dyson were prejudiced by virtue of simply being incapable of making measurements of the required accuracy. I argue that they had reasonable grounds for making their central claim that their results were not compatible with Newton's theory but were broadly compatible with Einstein's. In that sense their efforts were as important in the replacement of Newtonian gravity with general relativity as any single experiment ever can be.

Dyson and his collaborators were anxious to repeat their measurements and employed methods based on the use of check plates to provide an independent measure of scale change at the 1922 eclipse.¹⁶ They were foiled by bad weather, although a group from the Lick Observatory did provide new measures that agreed with the 1919 results. Although they did not have the last word on the light-bending experiment, and however blessed by fortune they may have been to obtain the data they did, the men of 1919 should be given credit for conducting a difficult experiment with skill, insight, and honesty under extraordinarily difficult circumstances. Their work was a major contribution to the emergence of general relativity as one of the leading theories of modern physics.

References

1. M. Stanley, *Practical Mystic: Religion, Science, and A. S. Eddington*, U. Chicago Press, Chicago (2007).
2. J. Earman, C. Glymour, *Hist. Stud. Phys. Sci.* **11**, 49 (1980).
3. A. Einstein, *Ann. Phys. (Leipzig)* **35**, 898 (1911).
4. J. G. von Soldner, *Berl. Astron. Jahrb.*, 161 (1804).
5. A. Einstein, *Ann. Phys. (Leipzig)* **49**, 769 (1916).
6. J. Crellin, *Einstein's Jury: The Race to Test Relativity*, Princeton U. Press, Princeton, NJ (2006).
7. F. W. Dyson, *Mon. Not. R. Astron. Soc.* **77**, 445 (1917).
8. S. Chandrasekhar, *Notes Rec. R. Soc. London* **30**, 249 (1976).
9. F. W. Dyson, A. S. Eddington, C. R. Davidson, *Philos. Trans. R. Soc. London, Ser. A*, **220**, 291 (1920).
10. A. Fowler, *Observatory* **42**, 261 (1919).
11. F. Dyson, *Nature* **106**, 786 (1921).
12. G. M. Harvey, *Observatory* **99**, 195 (1979).
13. S. W. Hawking, *A Brief History of Time*, Bantam Press, New York (1988).
14. C. A. Murray, P. A. Wayman, *Observatory* **109**, 189 (1989).
15. C. M. Will, *Was Einstein Right? Putting General Relativity to the Test*, Basic Books, New York (1993).
16. C. R. Davidson, *Observatory* **45**, 224 (1922).