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Anomalies in the History of Relativity

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Abstract — In November 1919 it was announced to the world that observations of a solar eclipse that occurred in May 1919 supported Albert Einstein's general theory of relativity. That announcement was one of the most influential events of 20th-century science, since Einstein's instant rise to enormous fame arose directly from it. In spite of the confidence with which the announcement was made, however, it was later realized that the accuracy of the observations was insufficient to constitute a reliable confirmation of the phenomenon that was predicted. Furthermore, another of the formulas published in the general theory, for the variation in the perihelion of the planet Mercury, had already been derived by another scientist several years earlier using another method. In spite of the fact that the experimental evidence for relativity seems to have been very flimsy in 1919, Einstein's enormous fame has remained intact and his theory has ever since been held to be one of the highest achievements of human thought. The resulting deification of Einstein has had some unfortunate effects: critics of his theory are often dismissed as cranks, and the search for better theories has been inhibited. It is suggested that the announcement of the eclipse observations in 1919 was not a triumph of science as it is often portrayed, but rather an obstacle to objective consideration of alternatives.

Keywords: Eddington — Einstein — general relativity — gravitation — light deflection — solar eclipses

Introduction

The generally accepted version of the history of relativity has been dominated for about eighty years by the famous announcement in November 1919 that the results of observations at a solar eclipse in May 1919 confirmed Albert Einstein's prediction from his general theory of relativity that starlight would be bent by a precise amount by the gravitational field of the Sun. The announcement caused such an enormous sensation, both among scientists and the general public, that Einstein and his theory became world famous, literally overnight. In spite of the confident announcement, however, it was later realized that the observations were not particularly accurate and that the results did not in fact provide decisive support for the theory. An interesting commentary on the situation is found in the first two paragraphs of an article by the Editor of *Nature* (Maddox, 1995), which are as follows:

> Historically, the most dramatic proof that Einstein was on the right track with his general theory of relativity came in 1919, when Eddington and his associate Crommelin took themselves off to the western Pacific and the track of the total eclipse of the Sun in that year. They were bent on measuring the predicted deflection of light from distant stars lying near the limb of the Sun. As the world knows, the prediction was confirmed within the limits of the accuracy of the observations, to general applause. Einstein became a celebrity overnight.
>
> What is not so well remembered is that the measurements in 1919 were not particularly accurate. The standard error of the measurements was roughly 30 percent, while the displacement of the images of different stars in the field near the eclipsed Sun ranged from half to twice the deflection Einstein had predicted. And later eclipse measurements have not been much better, largely because of the difficulties of making accurate measurements from the *ad hoc* observatories that have to be established.

That quotation shows the two conflicting sides of the problem: Einstein's enormous and enduring fame resulted directly from the announcement of the eclipse results, although the results were not particularly accurate. Another interesting description of the problem is given by Sciama (1967):

> The first attempt to test Einstein's prediction was made by British expeditions led by Sir Arthur Eddington just at the end of the Great War. At that time it was considered very striking that a group of British astronomers should attempt to test a theory proposed by a German. The results of the test were claimed as a triumph for Einstein, and his worldwide fame dates from this event. Ironically enough, attempts made at later eclipses suggest that Eddington underestimated the uncertainties inherent in such a difficult observation, and even today Einstein's prediction has not been tested with the precision one would wish.

The purpose of the present paper is to present more details of the history of the period in question and to attempt to assess the significance of the announcement of the 1919 eclipse observations.

The General Theory of Relativity

The general theory of relativity (Einstein, 1916) gave a formula that correctly matched the anomalous advance of the perihelion of the planet Mercury, which had already been observed by astronomers such as Le Verrier (Roseveare, 1982). The general theory also made two further predictions: that light would be bent by a gravitational field, and that spectral lines of light from stars (including the Sun) would appear displaced towards the red end of the spectrum because of the strong gravitational fields.

The predicted effects of gravitation on light were so small that they caused no measurable phenomena on the earth itself, but the theory predicted that light reaching the earth from a star would be bent by the gravitational field of the Sun if the light passed very close to the Sun. Since a star whose light passed very close to the Sun would usually be impossible to see, the deflection of starlight by the Sun is very difficult to observe. However, such deflections might be observable during a total eclipse of the Sun, and in March 1917 the Astronomer Royal of Britain, Sir Frank Watson Dyson, suggested that the total eclipse of the Sun that was to take place on May 29, 1919, would present an excellent opportunity to test this prediction of the general theory (Dyson, 1917). Although 1917 was at a dark period of the first world war, Dyson undertook the organization of the eclipse expeditions in the hope that the war would be over in time for the expeditions to take place. Even though the war did in fact end in time for the expeditions, Dyson did not go to observe the eclipse himself. Instead, Professor Arthur Stanley Eddington became the leader of the eclipse expeditions, and the subsequent rise of the general theory was strongly influenced by Eddington's work. (As a minor comment on the above quotation from Sciama, it should be pointed out that Eddington was knighted in 1930, so he was not Sir Arthur when he went on the eclipse expeditions.)

Although it appears that Dyson was originally somewhat skeptical about the possibility that Einstein's general theory would be confirmed (Eddington, 1939–1941), Eddington was a strong believer in the theory and was confident that it would be confirmed. Chandrasekhar (1987) has described Eddington's influence on Dyson by writing:

> Eddington's enthusiasm for the general theory of relativity must have succeeded in ensnaring into its magic his close friend and associate Sir Frank Dyson, the Astronomer Royal; for together, they were soon planning expeditions to observe the solar eclipse of 29 May 1919 if, to quote Jeans, "the state of civilization should permit when the time came." Eddington has described his own part in the planning and in the successful outcome of the expeditions "as the most exciting event, I recall, in my connection with astronomy."

Chandrasekhar went on to say that Eddington was so confident of the correctness of the theory that, if left to himself, he would not even have planned to go on the eclipse expedition. Chandrasekhar related how Eddington, a pacifist, was deferred from military service in the first world war "with the express stipulation that if the war should end by May 1919, then Eddington should undertake to lead an expedition for the purpose of verifying Einstein's prediction!"

The Eclipse Expeditions and their Observations

Two expeditions went from Britain to observe the total eclipse. Professor Eddington and Mr. E. T. Cottingham went to the island of Principe, in the Gulf of Guinea, West Africa, while Dr. A. C. D. Crommelin and Mr. C. Davidson went to Sobral in Brazil. Neither expedition went to the western Pacific as reported by Maddox (1995). Photographic plates of the stars close to the Sun, obtained during the eclipse, were compared with plates taken of the same portion of the sky at a different time of year when the Sun was elsewhere.

It should be noted that the deflection predicted by Einstein (1916) was very small, being about 1.7 seconds of arc for a ray of light at the edge of the Sun, and inversely proportional to the radial distance from the center of the Sun. Since no stars for which measurements were made during the 1919 eclipse were within two solar radii of the center, the largest deflection that would have occurred according to the theory would have been less than 0.8 seconds of arc, which, for the 343-cm focal length of the telescope used by Eddington, would have corresponded to about 0.01 mm on the photograph. In addition, there were other technical difficulties because of having to transport telescopes to remote locations. For example, light from the stars was reflected by a mirror into the telescope, so that the mirror could be rotated to compensate for the rotation of the earth during a time exposure, instead of rotating the telescope, which was not feasible under the conditions of the eclipse expeditions.

Possible sources of error in observations of this type at solar eclipses have been described by various authors such as von Klüber (1960) and Bertotti, Brill and Krotkov (1962). Some of these are:

1. Refraction of light in the Sun's corona and/or in the earth's atmosphere.
2. Distortions in the optical system caused by temperature changes during the eclipse.
3. Changes of scale between the eclipse plates and the comparison plates.
4. Distortions in the photographic emulsion while drying.
5. Errors in measurement of the images on the plates.

Although refraction in the Sun's corona was probably not significant, refraction in the earth's atmosphere may have been. For example, here is a recent estimate of the effect of the atmosphere on astronomical observations (MacRobert, 1995):

> Viewed at high power from the bottom of our ocean of air, a star is a living thing. It jumps, quivers, and ripples tirelessly, or swells into a ball of steady fuzz. Rare is the night (at most sites) when any telescope, no matter how large its aperture or perfect its optics, can resolve details finer than 1 arc second. More typical at ordinary locations is 2- or 3-arc-second seeing, or worse.

Although that article was in the "Backyard Astronomy" section of the journal, it seems reasonable to suggest that the conditions at Sobral and Principe in 1919 were probably not as good as the conditions obtainable in an American backyard observatory in 1995. The Sobral telescopes were set up on a racecourse, and the set-up is shown in photographs in various publications such as Eddington (1920). The author is not aware of any published photographs of the Principe set-up, but it may be noted that the baggage had to be transported about a kilometer through the wood by native carriers (Dyson *et al.*, 1920), so the set-up was unlikely to have been any better than at Sobral. In addition to the ordinary everyday effects of the "ocean of air," the darkening of the Sun during the eclipse may itself have worsened the refractive effects.

Distortion in the optical system may have been caused by the effect of the heating of the mirror by the Sun, and this was noted as a problem at Sobral (Dyson *et al.*, 1920).

In considering changes of scale between the eclipse plates and the comparison plates, von Klüber (1960) has pointed out that, for a telescope with a focal length of 343 cm, a change in the effective focal length or focal setting of only 0.1 mm, between the eclipse plates and the comparison plates (which are taken months apart), can cause scale errors of the same order as the Einstein effect for stars beyond about eight solar radii from the center of the Sun.

Distortion of the emulsion while drying was probably not very significant. However, measurements of the actual deflections on the plates may have introduced some errors in view of the extremely small displacements involved, especially when the x and y components of each displacement have to be measured.

In addition to all the above potential problems, the weather was cloudy at Principe at the time of the eclipse, and the photographs obtained were not as good as expected. In one of the earliest published references to the findings of the eclipse expeditions, the Astronomer Royal, Sir Frank Dyson, reported as follows at a meeting of the Royal Astronomical Society on July 11, 1919 (Fowler, 1919), before Eddington had returned from the expedition:

> I had a letter from Prof. Eddington two days ago. He is hoping to get good enough measures to determine the displacement definitely, but he obviously is greatly disappointed. He secured 16 photographs, but only for the last six was the sky clear enough to show any stars and on them he only got three, four, or five images; and, as the sky was generally only clear on one part of the plate at a time, the stars secured on the plates are badly distributed. From his best plate, however, he has some evidence of deflection in the Einstein sense, but the plate errors have yet to be fully determined.

It is interesting to compare that reference to Eddington's own later description of his measurement of that same plate. He describes how, although the weather was unfavorable because of cloud, sixteen photographs were taken. Of the twelve that were examined at Principe, most were useless, but "one plate was found showing fairly good images of five stars, which were suitable for a determination." (Eddington, 1920, p. 115) His description of this plate (p. 116) is as follows:

> The results from this plate gave a definite displacement, in good accordance with Einstein's theory and disagreeing with the Newtonian prediction. Although the material was very meagre compared with what had been hoped for, the writer (who it must be admitted was not altogether unbiassed) believed it convincing.

Eddington's biographer, Professor A. Vibert Douglas (1957), has also described his measurement of that plate, quoting from Eddington's Notebook. In the Notebook entry for June 3, after saying that he spent the whole day measuring, Eddington continued as follows:

> The cloudy weather upset my plans and I had to treat the measures in a different way from what I intended, consequently I have not been able to make any preliminary announcement of the result. But the one plate that I measured gave a result agreeing with Einstein.

Douglas's biography continued with the statement that "This was a moment which Eddington never forgot. On one occasion in later years he referred to it as the greatest moment of his life!"

It is interesting that the measurements on the same plate should have been viewed in such vastly different ways by Eddington at different stages of his study of the findings of the expedition: at first he was obviously greatly disappointed, then (although admittedly biased) he believed the evidence convincing in spite of the meager results, and later he saw it as the greatest moment of his life! Although he had at first suggested (in his letter to Dyson) that the stars on the plates were poorly distributed, he claimed in the published paper (Dyson *et al.*, 1920) that the five stars shown on the two plates from Principe "include all the most essential stars," and even suggested that the cloud may have been beneficial by preventing over-exposure of the brightest stars! In spite of that optimistic assessment, it appears from the published paper that the stars on the two usable plates obtained by the Principe expedition were indeed poorly distributed. Since the expected deflections, according to Einstein's theory, were radial, it seems obvious that a good test would include stars all around the Sun; but on the diagram showing the stars (Dyson *et al.*, 1920, p. 294), it is possible to draw a straight line through the center of the Sun in such a way that all the stars on both plates are well to one side of the line. In fact, it is possible to draw a straight line through the center of the Sun in such a way that, of the thirteen stars included in the combined records of both expeditions, only one (#2) is on the other side of the line. Furthermore, all five stars on one of the Principe plates (plate W, stars # 3, 4, 5, 6, 10), and four of the five on the other (plate X, the one measured by Eddington at Principe, stars # 3, 4, 5, 6, 11), are almost on a straight line. Since the measurements on the Principe plates were taken by placing a Principe plate film to film with an Oxford comparison plate so that the images of corresponding stars nearly coincided (which was possible because the Oxford plates were taken directly and the Principe plates by reflection in a mirror), the almost straight-line distribution of stars may have worsened the accuracy of fitting together the pairs of plates. (A diagram of the stars can also be found in Eddington (1920, p. 120.)

By contrast, the Sobral expedition had experienced fine weather; it had taken photographs using two telescopes, one similar to the Principe one and another having a different aperture and focal length. The photographs were also disappointing because the definition of the images had been spoiled.

When the photographs taken by the telescope similar to the Principe one were measured "The measures pointed with all too good agreement to the 'half-deflection,' that is to say, the Newtonian value which is one-half the amount required by Einstein's theory." (Eddington, 1920, p. 117)

Eddington (1920) went on to give a reason for preferring the Principe results: the astronomers at Principe had taken "check-plates" of another portion of the sky to check whether there was any effect of the 50-degree difference in temperature between Principe in May and England in January, when the photographs were taken for comparison, whereas no such check-plates were taken by the Sobral expedition. However, the Sobral expedition had stayed in Brazil for a further two months after the eclipse (Eddington, 1920, p. 117) to photograph the same region of the sky before dawn, so it would appear that the large temperature difference would not apply to the comparison of the photographs in that case. The published paper (Dyson *et al.*, 1920) states why it was not feasible for the Principe expedition to make a similar comparison:

> Unlike the Sobral expedition, we were not able to take comparison photographs of the eclipse field at Principe, because for us the eclipse occurred in the afternoon, and it would be many months before the field could be photographed in the same position in the sky before dawn. The check plates were therefore specially important for us.

In any event, it seems strange to state after the fact that the absence of check-plates of the Brazil expedition was a disadvantage. Either those check-plates were significant or they were not; if it was considered necessary to take them, that should have been decided *in advance* rather than being used *after the fact* to downgrade or reject a whole set of observations. A somewhat different opinion of the importance of the check-plates has been given by A. I. Miller (1996, pp. 88–89):

> Although the weather was far better at Sobral, Eddington insisted on emphasizing data from Principe. Clearly, a key part of this experiment is to use the same instruments at the same site for comparing data for the apparent and true star positions, but this was not done. The comparison photographs were taken several months later at Oxford.

There is also a puzzling discrepancy between the two accounts: although Eddington (1920, p. 117) states explicitly that "there were no check-plates taken at Sobral," the other account (Dyson *et al.*, 1920) states on p. 298, referring to the Sobral expedition, that "A few check plates of the field near Arcturus were taken, but have not been used."

Several authors have referred to the way in which Eddington emphasized some photographic plates rather than others. For example, Campbell (1923) wrote as follows:

Professor Eddington was inclined to assign considerable weight to the African determination, but, as the few images on his small number of astrographic plates were not as good as those on the astrographic plates secured in Brazil, and the results from the latter were given almost negligible weight, the logic of the situation does not seem entirely clear.

As a minor addition to the history of the eclipse expeditions, it may also be noted that, at the same meeting at which Dyson reported Eddington's disappointment (Fowler, 1919), Campbell reported on the results obtained at an earlier eclipse on June 8, 1918, by saying "It is my own opinion that Dr. Curtis's results preclude the larger Einstein effect, but not the smaller amount expected according to the original Einstein hypothesis." (This refers to an earlier version of Einstein's prediction of the displacement, which was half the amount predicted by the general theory.)

Even after the measurements have been made, there is still the mathematical problem of extrapolating back to the edge of the Sun, since no stars measured were within two solar radii of the center of the Sun and small changes in the observed deflections can cause a much greater change in the computed deflection at the limb of the Sun. This problem has been discussed by von Klüber (1960) and Bertotti, Brill, and Krotkov (1962); in particular, Figure 6 of von Klüber's paper shows extremely widely scattered results from various eclipse expeditions. A more recent eclipse expedition in 1973 has been described by Zirker (1995), who shows widely scattered measurements (Figure 9.4, p. 179) and states that the occurrence of a sandstorm limited the value of the results.

The problems of interpreting the results of the eclipse expeditions have been well summarized by Guggenheimer (1925), who made the following comment on the 1919 eclipse observations:

Any reader, though far from an expert astronomer or physicist, who will study the description of the apparatus used in these observations and the large margin of error possible by reason of defects therein, will readily comprehend that, in view of the required delicacy of measurement of the things observed and of the error allowances both for apparatus defects and other possible physical causes of the observed phenomena, the greatest caution in the analysis of the results is necessary. Some astronomers deny that the photographs of the eclipse observations, when compared with those taken of the same stars in the absence of the sun, show deflections approximating the amount or the direction predicted by Professor Einstein. An examination of the various tables of the deflections observed shows that many of them are far away from the quantities predicted. The quantity approximating the predicted one is obtained by averaging a selected few of the observations.

Announcement of the Eclipse Results

The results obtained by the British eclipse expeditions of May 1919 were announced at the famous joint meeting of the Royal Society and the Royal Astronomical Society held on November 6, 1919. In spite of the poor accuracy and the uncertainties surrounding the results, it was announced that the evidence was decisively in favor of the value of displacement that had been predicted by Einstein, and Sir Joseph Thomson, President of the Royal Society and Chair of the meeting, strongly endorsed the results (Thomson, 1919). The enthusiastic reception of the announcement at the meeting was matched by an enormously strong reaction by the general public and, according to one biography of Einstein (Clark, 1971), "Einstein awoke in Berlin on the morning of November 7, 1919, to find himself famous." Although Fölsing (1997, p. 448) has expressed disagreement with that particular interpretation of the event, there is little doubt that Einstein rose with spectacular rapidity to world-wide fame as a direct result of the announcement.

The atmosphere of excitement at the meeting has been described by Alfred North Whitehead (1926) in the following words:

> It was my good fortune to be present at the meeting of the Royal Society in London when the Astronomer Royal for England announced that the photographic plates of the famous eclipse, as measured by his colleagues in Greenwich Observatory, had verified the prediction of Einstein that rays of light are bent as they pass in the neighbourhood of the sun. The whole atmosphere of tense interest was exactly that of the Greek drama: we were the chorus commenting on the decree of destiny as disclosed in the development of a supreme incident. There was dramatic quality in the very staging: the traditional ceremonial, and in the background the picture of Newton to remind us that the greatest of scientific generalisations was now, after more than two centuries, to receive its first modification. Nor was the personal interest wanting: a great adventure in thought had at length come safe to shore.

The meeting has also been described in a very interesting way by Abraham Pais (1982), who identified the day of the joint meeting as "the day on which Einstein was canonized." Pais was obviously very pleased with his comparison of the meeting to a Congregation of Rites at which a candidate is considered for canonization in the Catholic Church, and compared various participants at the meeting to counterparts in the Congregation of Rites, using as his reference *The New Catholic Encyclopedia*. However, one of his comparisons was highly inappropriate, namely his comparison of Ludwik Silberstein to the *advocatus diaboli*, or Devil's advocate. Although Silberstein was critical of accepting that the results superseded Newton's theory, the circumstances in which his criticisms were expressed were totally different, in at least two important respects, from the circumstances in which the arguments of the Devil's advocate are expressed in a Congregation of Rites.

In the first place, it is imperative that the arguments of the Devil's advocate be heard *before* canonization is pronounced, and Pais's account gives the impression that Silberstein's criticisms had been heard before the results were endorsed by the President of the Royal Society. However, the generally accepted account of the meeting (Thomson, 1919) shows that Thomson had, to use

Pais's words, "pronounced the canonization" before Silberstein had had a chance to speak.

In the second place, it is the responsibility of the Devil's advocate to ensure that canonization does not occur undeservedly. In order to fulfill that responsibility, he must be given full access to all the relevant information required to make the case for the opposition to canonization. This condition was not fulfilled at the meeting at which the eclipse results were announced, because it was not possible for members of the audience to be sufficiently well-informed about the results that were being announced to make informed criticism of them. The paper that was eventually published (Dyson *et al.*, 1920), which is 43 pages long, with copious tables of results and mathematical analysis, carries the notation "Received October 30,—Read November 6, 1919." Also, the issue of *Nature* dated October 30 carried an announcement of the joint meeting, showing that the meeting had been arranged before the paper had been received by the Royal Society. It seems very unlikely, therefore, that the paper had received any critical review by independent referees before presentation, or that its contents were available to the audience early enough to be thoroughly studied. Just after the astronomers had presented their results, Thomson rose to call for discussion, but before the discussion actually started he strongly endorsed the confirmation of Einstein's prediction by saying (Thomson, 1919):

> It is difficult for the audience to weigh fully the meaning of the figures that have been put before us, but the Astronomer Royal and Prof. Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value of the displacement. This is the most important result obtained in connection with the theory of gravitation since Newton's day, and it is fitting that it should be announced at a meeting of the Society so closely connected with him. ...If it is sustained that Einstein's reasoning holds good — and it has survived two very severe tests in connection with the perihelion of Mercury and the present eclipse — then it is the result of one of the highest achievements of human thought.

It was that speech that Pais interpreted as the pronouncement of the canonization of Einstein; the remark about "one of the highest achievements of human thought" has been widely quoted and has obviously contributed enormously to the veneration of Einstein and relativity. If we pursue Pais's comparison of the joint meeting and a Congregation of Rites, we find another unfortunate feature of the comparison, namely that canonization of a saint by the Pope is infallible and irrevocable, so that subsequent criticism of the process is futile.

It is unfortunate that Pais, by identifying Silberstein as the Devil's advocate, gave the impression that a critical assessment of the eclipse results had been voiced at the joint meeting, and that the criticisms had been answered. Let us now consider what Silberstein did say in his contribution to the discussion at that meeting (Thomson, 1919). Although he could not find fault in the eclipse observations, for the reasons given above, he did point out the fact that the third main prediction of the general theory, the red-shift of light in a strong gravitational field, had not been observed. He said:

There is a deflection of the light rays, but it does not prove Einstein's theory; it cannot be logically deduced from his theory as a gravitational effect in the absence of the spectroscopic result. And, as far as we know from St. John's and Evershed's observations, the predicted shift of the spectrum lines, of the amount exceeding almost 100 times the probable error of the modern spectroscope (as Prof. Fowler has just told us), is not obtained. ...If the shift remains unproved as at present the whole theory collapses, and the phenomenon just observed by the astronomers remains a fact awaiting to be accounted for in a different way.

Eddington's reply to Silberstein was described by Earman and Glymour (1980) as being "mild and irrelevant." He said:

When a result that has been forecasted is obtained, we naturally ask what part of the theory exactly does it confirm. In this case it is Einstein's *law* of gravitation. It is not necessarily his theory that is confirmed, with the underlying assumption that *ds* is a quantity measurable by clock and scale. There still remains the question what the intermediary quantity *ds* is, which must be tested by the Fraunhofer lines. [Italics in the original.]

On the question of whether the eclipse results confirmed Einstein's general *theory* or only his *law* of gravitation, the published paper seems to be a little ambiguous; on p. 292 of the published paper (Dyson *et al.*, 1920) the following two statements occur within the course of a single paragraph (emphasis added):

The results of the observations here described appear to point quite definitely to the third alternative, and confirm Einstein's generalised relativity *theory*. But, whether or not changes are needed in other parts of the theory, it appears now to be established that Einstein's *law* of gravitation gives the true deviations from the Newtonian law both for the relatively slow-moving planet Mercury and for the fast-moving waves of light.

If there is to be a fair comparison between the critical assessment of a scientific paper and the critical assessment of a candidate for canonization, as performed by a Devil's advocate, all available facilities should be placed at the disposal of the assessor in each case. Although Silberstein did not have enough information about the eclipse observations at the meeting itself, he made the following criticisms of them at a meeting of the Royal Astronomical Society on 12 December, 1919 (Fowler, 1920):

They [the eclipse observations] indicate the presence of other factors modifying the displacement of the stars. These displacements are not radial. The deviations from the radial direction are marked running up to 15° for star No. 6 and to 35° for No. 11. Five of the stars near the Sun's axis show deviations from the radial displacement in the same sense; two stars near the Sun's equatorial plane show angular displacements in the opposite sense. If we had not the prejudice of Einstein's theory we should not say that the figures strongly indicated a radial law of displacement.

It seems reasonable to suggest that the prestige of Dyson and Eddington led to the arranging of the joint meeting before the paper had even been received, and to the rapid acceptance of the paper when it was received. In this connection, it is interesting to note the comment by Wali (1984, p. 115), about the influence of prestige on meetings of the Royal Astronomical Society, that "Papers had to be submitted a week before, by the first Friday of the month. Papers submitted by people like Eddington, Jeans and Milne were always read, and they always came first."

Although it seems obvious that no independent critical assessment of the results was made before the joint meeting, with information still available today it is possible to make some assessment of the results. As various authors such as von Klüber (1960) and Bertotti, Brill and Krotkov (1962) have pointed out, other scientists have re-assessed the results of the 1919 and other eclipse results, with a certain amount of variation from the results originally published. For example, Sciama (1969) refers to a table of results of eclipse observations from 1919 to 1952, and comments as follows:

It is hard to assess their significance, since other astronomers have derived different results from a re-discussion of the same material. Moreover, one might suspect that if the observers did not know what value they were "supposed" to obtain, their published results might vary over a greater range than they actually do; there are several cases in astronomy where knowing the "right" answer has led to observed results later shown to be beyond the power of the apparatus to detect.

Hermann Bondi, a strong supporter of relativity, makes the following comment (Bondi, 1960) on the subject of Einstein's prediction of the deflection of starlight by the Sun:

Einstein's prediction can therefore be checked only on the rare occasions when, at the moment of an eclipse, bright stars happen to be near the direction to the sun. The effect is difficult to study even when circumstances are most favourable. The indications are much in favour of Einstein's theory of relativity, but it would be premature to call the results conclusive.

If it was premature in 1960 to call the results conclusive, it was much more so in 1919.

Another interesting assessment of the significance of the 1919 eclipse results has been given by Calder (1979, p. 103), who wrote:

The eclipse results were a triumph. Newton's ideas about gravity had reigned unchallenged for more than two centuries, yet within four years of Einstein developing his the-

> ory it seemed confirmed, and Newton was dethroned. The deflection of light by gravity is, as I have stressed, central to Einstein's general relativity. But later measurements of the deflection of starlight at other eclipses gave a wide scatter of results. They departed from Einstein's prediction by anything up to sixty per cent. The difficulties of the observations were to blame, rather than any defect in the theory. While they did not allow any restoration of Newton they left, nevertheless, a little elbow room for alternative accounts of gravity. So, sixty years after the initial triumph, astronomers and relativists were decidedly cool about this way of checking up on Einstein.

With reasoning like that, the theory cannot lose: if the experiment supports the theory, the theory is confirmed; if the experiment does not support the theory, it is the difficulties of the observations that are to blame, not the theory.

From reading the many accounts of the way in which the results were obtained, it appears that the evidence for the bending of starlight by the sun was rather flimsy in 1919. Let us now consider another of the results of the general theory, namely its explanation of the movement of the perihelion of the planet Mercury.

The Perihelion of Mercury

As already mentioned, the general theory of relativity included a formula that correctly matched the variation in the perihelion of the planet Mercury. Pais (1982) has given the following interesting description of Einstein's excitement on discovering his explanation of that phenomenon:

> This discovery was, I believe, by far the strongest emotional experience in Einstein's scientific life, perhaps in all his life. Nature had spoken to him. He had to be right.

Polkinghorne (1998) quoted Pais's statement and made the following comment:

> It was a great triumph but, if the answer had not come out right, the aesthetic power of the equations of general relativity would have been quite unable in itself to save them from abandonment. It was indeed *nature* that had spoken. [Italics in the original.]

It should be pointed out that exactly the same formula as Einstein published in his general theory for the variation in the perihelion of Mercury (Einstein, 1916) had been derived by another scientist several years earlier using another method based on the assumption of a retarded gravitational potential (Gerber, 1898). Some of the reactions to Gerber's paper have been described by Fölsing (1997). In particular, Fölsing described the action of physicist Ernst Gehrcke, who had written articles against relativity since 1911, as follows:

> As no one believed his "refutations," Gehrcke in 1916 opened a second front. He unearthed some studies by the Pomeranian schoolmaster Paul Gerber, who, about the turn

of the century, had tried to explain the perihelion movement of Mercury by procedures which were entirely arbitrary and were rightly rejected by astronomers. By reprinting Gerber's paper along with his own comment in *Annalen* Gehrcke insinuated, first, that Einstein was a plagiarist and, second, that the problem of the perihelion precession had been solved before relativity theory.

According to Fölsing, Einstein did not publish a reply to Gehrcke's paper, but he did publish a brief note (Einstein, 1918) in reply to another paper by Gehrcke (1918); however, although in that paper Gehrcke had mentioned Gerber's formula, Einstein's reply, which Fölsing described as "concise and factual," did not mention Gerber. Einstein did, however, mention him in a later article (Einstein, 1920), in which, after agreeing that Gerber had given the correct formula for the perihelion motion of Mercury before he himself had done, Einstein continued as follows:

The experts are not only in agreement that Gerber's derivation is wrong through and through, but the formula cannot be obtained as a consequence of the main assumption made by Gerber. Mr. Gerber's work is therefore completely useless, an unsuccessful and erroneous theoretical attempt. I maintain that the theory of general relativity has provided the first real explanation of the perihelion motion of Mercury. I have not mentioned the work by Gerber originally, because I did not know it when I wrote my work on the perihelion motion of Mercury; even if I had been aware of it, I would not have had any reason to mention it.

In the same article, Einstein referred to criticism of the eclipse observations as follows:

Mr. Gehrke in his lecture has made the masterfully executed English measurements of the deflection of light by the sun appear in a bad light by mentioning the three independent groups only one of which, because of aberration of the heliograph mirror, gave erroneous results. He has suppressed the fact that the English astronomers themselves in their official report considered the results as a brilliant confirmation of the theory of general relativity.

In view of the many subsequent criticisms of the eclipse results, it appears that the confirmation of the theory may not have been as brilliant as those who obtained the results believed. It also seems unfortunate that Einstein based his claim of Gerber's errors on an appeal to the authority of "the experts" instead of indicating what was wrong with his derivation.

Another dismissal of Gerber's work is mentioned by Fölsing in the following words:

A sharper, crushing reproof came from the respected Munich astronomer Hugo Ritter von Seeliger, who pointed out that the Pomeranian schoolmaster had copied things which had long been known to every worker in the field, and that his so-called explanation was based on a crude mathematical mistake.

Unfortunately, Seeliger (1918) does not seems to have identified the crude mistake, and Roseveare (1982) has made the following comment on his reply: "Seeliger claimed that Gerber's calculation was based on an elementary mistake, though it is evident that it was Seeliger who was mistaken." Unfortunately, also, Fölsing does not say why Gerber's methods, as he put it, "were entirely arbitrary and were rightly rejected by astronomers." It is also unfortunate that Gerber, having died sometime between 1902 and 1917 (Roseveare, 1982), was not available to defend his work from Einstein and Seeliger.

Since Gerber's 1898 paper and Gehrcke's references to it are in German, British scientists may not even have been aware of them in 1919, so that Gerber's work was not taken into account in considering possible alternatives to Einstein's theory. Simply pointing out Gerber's earlier derivation of the formula in question is not, of course, accusing Einstein of plagiarism, since Gerber's derivation of the formula was different from Einstein's.

Although Roseveare (1982) gives a sympathetic description of Gerber's work, he states that his law was refuted, on two counts. The first count was the fact that Gerber's theory is a gravitational theory and says nothing outside that sphere; this problem is apparently avoided by general relativity because, according to Roseveare, "this theory already contains special relativity." That claim is debatable at least, and we shall argue below, in our discussion of special relativity, that special and general relativity are distinct theories; neither one contains the other. Roseveare continues his claim that Gerber's theory has been refuted by saying "The second count on which Gerber's law may be seen to be refuted is the deflection of light rays, though this was at times rather equivocal." In view of the above discussion of the accuracy of the light-deflection results, this reason does not appear to be decisive. Perhaps it might be interesting to make an objective reappraisal of Gerber's work.

The Special Theory of Relativity

Although the above discussion concentrates on the general theory of relativity, there are also some anomalies in the history of the special theory. When the special theory was first published (Einstein, 1905), it was taken to be similar to theories of others such as Lorentz, Poincaré and Larmor. For example, Whittaker (1953) mentions Einstein's special theory in passing in a chapter titled "The Relativity Theory of Poincaré and Lorentz," in the following words:

> In the autumn of the same year, in the same volume of the *Annalen der Physik* as his paper on the Brownian motion, Einstein published a paper which set forth the relativity theory of Poincaré and Lorentz with some amplifications, and which attracted much attention.

Although the special theory did not have a very large impact before the rapid rise of the general theory in 1919, the events of 1919 caused it to become famous also, even though it is a different theory. Here is what Cohen (1985,

p. 405) wrote about the way the events of 1919 contributed to the rise of the special theory:

In considering relativity, we must keep in mind that there are two different theories of relativity. ...But what brought the attention of the world to special relativity was the verification in 1919 of a prediction of the general theory, that starlight passing near the sun is bent by the sun's gravity. This verification, which occurred during a solar eclipse expedition, set into being the relativity craze that swept the world and overnight made Einstein a public figure.

It is time to realize that, even if the general theory were verified, that does not in itself verify the special theory. The two theories are different; they do not use the same set of axioms. The difference is shown by the following statement by Stachel (1995):

'Theory of relativity' is an umbrella term, covering two quite distinct theories, usually called 'the special' and 'the general' theory. The exact nature of the relation between the two is still a subject of controversy, and both might well not bear the common appellation 'relativity' if not so closely associated with the work of one man, Albert Einstein, who developed the second in attempting to extend the first to include gravitation.

Discussion

Various authors have tried to explain why the results of the eclipse observations were announced as being decisive when they were not. For example, Earman and Glymour (1980) write that

Eddington's overenthusiastic advocacy may perhaps be explained by his prior conviction that the theory was true and by his interest in saving something from the vast work of the Principe expedition. Dyson's position might be understood in part as the result of a reasonable evaluation of the evidence, and in part as the result of Eddington's advocacy. But one retains the suspicion that besides these reasons, there was, especially for Eddington, another: the hope that a British verification of Einstein's theory would force on British scientists a more open-minded and generous attitude towards their German colleagues.

The enormous public reaction to the scientific announcement appears to have been strongly influenced by the ending of the war with Germany, and it may be noted that another important historical event of 1919 was the signing of the Treaty of Versailles, over German protest, in June. Chandrasekhar (1987) has described how Rutherford attributed Einstein's great fame to Eddington and explained the public reaction in the following words:

The war had just ended; and the complacency of the Victorian and the Edwardian times had been shattered. The people felt that all their values and all their ideals had lost their bearings. Now, suddenly, they learnt that an astronomical prediction by a German scientist had been confirmed by expeditions to Brazil and West Africa and, indeed, prepared for already during the war, by British astronomers. Astronomy had always appealed to public imagination; and an astronomical discovery, transcending worldly strife, struck a responsive chord. The meeting of the Royal Society, at which the results of the British expeditions were reported, was headlined in all the British papers: and the typhoon of publicity crossed the Atlantic. From that point on, the American press played Einstein to the maximum.

It may be argued that the inaccuracies in the 1919 eclipse observations do not matter, since the same effect has supposedly been verified by more accurate measurements such as, for example, the measurements on quasars described by Maddox (1995). This point of view is somewhat guardedly espoused by the last paragraph of the critical discussion of the 1919 eclipse expeditions by Earman and Glymour (1980):

This curious sequence of reasons might be cause enough for despair on the part of those who see in science a model of objectivity and rationality. That mood should be lightened by the reflection that the theory in which Eddington placed his faith because he thought it beautiful and profound — and, possibly, because he thought that it would be best for the world if it were true — this theory, so far as we know, still holds the truth about space, time and gravity.

That is at least debatable. For example, Chandrasekhar (1990) made the following comments on a lecture in which P. A. M. Dirac had claimed that there was a long and impressive list of successes of Einstein's theory of gravitation:

It does not seems to me that the successes of Einstein's theory are either long or impressive. It is true that his prediction of the different rates of clocks in locations of differing gravity, his prediction of the deflection of light when traversing a gravitational field and resulting time delay, his prediction regarding the precession of the perihelion of Mercury, and finally, the slowing down of a binary star in an eccentric orbit by virtue of the emission of gravitational radiation, have been confirmed quantitatively. But all these relate to the departures from Newtonian theory by a few parts in a million; and of no more than three or four parameters in a post-Newtonian expansion of Einstein's field equations. And so far, no predictions of general relativity in the limit of strong gravitational fields have received any confirmation; nor are they likely in the foreseeable future.

It is also reasonable to ask whether the rapid and strong entrenchment of the general theory that occurred as a result of the eclipse announcement may have led experimenters to try to obtain the "right" answers from their observations, as suggested in the above quotation from Sciama (1969). For example, Collins and Pinch (1993) have commented on the measurements of the red-shift predicted by general relativity as follows:

> The experimental observations, conducted both before and after 1919, were even more inconclusive. Yet after the interpretation of the eclipse observations had come firmly down on the side of Einstein, scientists suddenly began to see confirmation of the redshift prediction where before they had seen only confusion.

Another unfortunate result of the announcement of the success of the eclipse observations has been an enormous hero-worship of Albert Einstein; Pais's statement that he was canonized has now been outmatched by Miller (1996, p. 90), who states that he was deified. A result of this deification is that the greatest scorn of the scientific community is reserved for those who would try to criticize either of Einstein's theories of relativity or to suggest alternative theories, and many mainstream scientific journals reject papers critical of either theory without review. The attitude of most journals is well described by Davies (1980) in the first sentence of his article "Why pick on Einstein?":

> Most editors of scientific journals make special provision for coping with the huge influx of papers and letters, many bearing private addresses in California, purporting to disprove or improve Albert Einstein's monumental work on the theory of relativity.

The assessment of scientific theories, and criticisms of them, should be based on the merits of each case, rather than on the addresses from which they come.

Although Maddox (1995) describes more modern ways of measuring the deflection of radiation by a gravitational field, he appears to be willing to consider the possibility that general relativity may not be the last word; the last paragraph of his article is as follows:

> The crying need remains what it has been for the past two decades, that of marrying together general relativity and quantum mechanics. As things are, they are like chalk and cheese. It will be a great surprise if general relativity survives that marriage unchanged.

As pointed out by Earman and Glymour (1980), it was Eddington's belief that confirmation of Einstein's prediction had a beneficial effect on scientific relations between England and Germany, and it is fair to ask whether that beneficial effect was sufficient to justify the announcement of the results as being decisively in favor of Einstein's theory when they were not. It is also fair to ask, today, whether continued belief in the decisiveness of the results as announced in November 1919 is beneficial to science and scientific objectivity, or whether scientific progress would be improved by a more open acknowledgment of the inaccuracies and uncertainties in the observations. Even if it is argued that Einstein's general theory has been supported by subsequent experimental observations, that does not alter the fact that, at an extremely important scientific meeting which had enormously far-reaching consequences, the audience was misinformed by eminent scientists about the phenomenon that was the main theme of the meeting. That historical fact is not wiped out by any subsequent experimental results, whatever they may be.

Because of the euphoric veneration of Einstein and relativity in November 1919, the objectivity with which science is supposed to act has been compromised, and the search for better theories has been inhibited. Canonization, deification, and claims of personal communication from Nature, should have no place in science. If the findings of the eclipse expeditions had been announced as being inconclusive instead of decisive in 1919, general relativity would have had to compete with other possible theories, such as Gerber's, to explain certain astronomical observations, and a better theory might eventually have been found. In the author's opinion, the confident announcement of the decisive confirmation of Einstein's general theory in November 1919 was not a triumph of science, as it is often portrayed, but one of the most unfortunate events in the history of 20th-century science.

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References

Bertotti, B., Brill, D., and Krotkov, R. (1962). Experiments on gravitation, in Witten, L. (Ed.) *Gravitation: An Introduction to Current Research*. New York: John Wiley, pp. 1–48.

Bondi, H. (1960). *The Universe at Large*. New York: Anchor Books.

Calder, N. (1979). *Einstein's Universe*. Harmondsworth, Middlesex: Penguin.

Campbell, W. W. (1923). The total eclipse of the Sun, September 21, 1922. *Publications of the Astronomical Society of the Pacific*, 35, 11.

Chandrasekhar, S. (1987). *Truth and Beauty: Aesthetics and Motivations in Science*. Chicago: University of Chicago Press.

Chandrasekhar, S. (1990). *Relativistic Astrophysics*, (Selected Papers: S. Chandrasekhar, Vol. 5). University of Chicago Press.

Clark, R. W. (1971). *Einstein: The Life and Times*. New York: World Publishing Company.

Cohen, I. B. (1985). *Revolution in Science*. Belknap Press of Harvard University Press.

Collins, H., and Pinch, T. (1993). *The Golem: What Everyone Should Know About Science*. Cambridge University Press.

Davies, P. (1980). Why pick on Einstein? *New Scientist*, 87, 463.

Douglas, A. V. (1957). *The Life of Arthur Stanley Eddington*. London: Nelson.

Dyson, F. W. (1917). On the opportunity afforded by the eclipse of 1919 May 29 of verifying Einstein's Theory of Gravitation. *Monthly Notices of the Royal Astronomical Society*, 77, 445.

Dyson, F. W., Eddington, A. S., and Davidson, C. A (1920). Determination of the deflection of light by the Sun's gravitational field, from observations made at the total eclipse of May 29, 1919. *Philosophical Transactions of the Royal Society of London*, Series A, 220, 291.

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

Eddington, A. S. (1920). *Space, Time and Gravitation: An Outline of the General Relativity Theory*. Cambridge University Press.

Eddington, A. S. (1939–1941). Sir Frank Watson Dyson. *Obituary Notices of Fellows of the Royal Society*, 3, 159–172.

Einstein, A. (1905). On the Electrodynamics of Moving Bodies. In Lorentz *et al.* (1923), pp. 37–65.

Einstein, A. (1916). The Foundation of the General Theory of Relativity. In Lorentz *et al.* (1923), pp. 111–164.

Einstein, A. (1918). Bemerkung zu E. Gehrckes Notiz "Über den Äther." V*erhandlungen der Deutschen Physikalischen Gesellschaft*, 20, 261.
Einstein, A. (1920). My answer to the Anti-Relativistic Company, Inc. *Berliner Tageblatt und Handels-Zeitung*, August 27. In G. E. Tauber (Ed.) (1979), *Albert Einstein's General Theory of Relativity*. New York: Crown Publishers, Inc.
Fölsing, A. (1997). *Albert Einstein: A Biography*. New York: Viking.
Fowler, A. (1919). (Chair of) Meeting of the Royal Astronomical Society, Friday, July 11, 1919. *The Observatory*, 42, 297.
Fowler, A. (1920). (Chair of) Meeting of the Royal Astronomical Society, Friday, December 12, 1919. *The Observatory*, 43, 33.
Gehrcke, E. (1918). Über den Äther. *Verhandlungen der Deutschen Physikalischen Gesellschaft*, 20, 165.
Gerber, P. (1898). Die raümliche und zeitliche Ausbreitung der Gravitation. *Zeitschrift für Mathematik und Physik*, 43, 93.
Guggenheimer, S. H. (1925). *The Einstein Theory: Explained and Analyzed.* New York: Macmillan, pp. 298–9.
Lorentz, H. A., Einstein, A., Minkowski, H., and Weyl, H. (1923). *The Principle of Relativity*. London: Methuen.
MacRobert, A. M. (1995). Beating the seeing. *Sky & Telescope*, 89, 40.
Maddox, J. (1995). More precise solar-limb light-bending. *Nature*, 377, 11.
Miller, A. I. (1996). *Insights of Genius: Imagery and Creativity in Science and Art*. New York: Springer-Verlag.
Pais, A. (1982). *'Subtle is the Lord...'*. Oxford: Clarendon Press.
Polkinghorne, J. (1998). *Belief in God in an Age of Science*. New Haven and London: Yale University Press.
Roseveare, N. T. (1982). *Mercury's Perihelion: from Le Verrier to Einstein.* Oxford: Clarendon Press.
Sciama, D. W. (1967). In *Einstein: The Man and His Achievement*, Edited by G.J. Whitrow, pp. 39–40. London: British Broadcasting Corporation.
Sciama, D. W. (1969). *The Physical Foundations of General Relativity*. New York: Doubleday.
Seeliger, H. (1918). Bemerkung zu dem Aufsatze des Herrn Gehrcke "über den Äther." *Verhandlungen der Deutschen Physikalischen Gesellschaft*, 20, 262.
Stachel, J. (1995). History of Relativity. In L. M. Brown, A. Pais, and B. Pippard, (Eds.) *Twentieth Century Physics*, Volume I, Chapter 4, pp. 249–356. Bristol: Institute of Physics Publishing.
Thomson, J. (1919). [Chair of] Joint Eclipse Meeting of the Royal Society and the Royal Astronomical Society. *The Observatory*, 42, 389.
von Klüber, H. (1960). The determination of Einstein's light-deflection in the gravitational field of the Sun. *Vistas in Astronomy*, 3, 47.
Wali, K. C. (1984). *Chandra: A Biography of S. Chandrasekhar*. University of Chicago Press.
Whitehead, A. N. (1926). *Science and the Modern World*. Cambridge University Press.
Whittaker, E. (1953). *A History of the Theories of Aether and Electricity: Volume two, The Modern Theories 1900–1926.* London: Nelson.
Zirker, J. B. (1995). *Total Eclipses of the Sun*. Princeton: Princeton University Press.