

as an anachronism.¹⁷⁸ However, the reforms of 1848 and the establishment of the Board of Mathematical Studies brought the Tripos under the control of the University, reducing somewhat the power of the Examiners and Moderators. In this instance, increased University control did not make the Tripos more relevant to contemporary developments in mathematics or society. Rather it stabilized the Tripos and helped to preserve it as a part of a liberal education. If greater University influence was not always progressive, Whewell was not always anachronistic. He urged that engineering be included in the curriculum and helped to establish the Natural Science Tripos, a typical "progressive" measure. The reforms of 1848, moreover, made it easier for those wishing to go on to the other Triposes, including the Natural Science, to compete successfully in the mathematical. A compromise allowing a degree of specialization and the maintenance of a liberal education was achieved.

The Mathematical Tripos grew by a series of accretions adopted for utility and tested in practical experience by conscientious men challenged to reconcile the needs of a traditional liberal education with an explosion in mathematical knowledge. By 1848 the impossibility of satisfying all interests could not be denied. The mixed mathematicians won. Calculus, the tool of the mixed mathematician, and mixed mathematics itself, remained intertwined and mutually reinforcing disciplines founded on geometry and intuition and overlaid by a façade of formalistic manipulation. Cambridge mathematics, with the exception of algebraic analysis, was a mixture of physical mathematics and mathematical physics, of geometric intuition and analytic manipulation. Problem solution, not abstract rigor, dominated. The business was not that of the pure mathematician or even the theoretical physicist, but that of the applied mathematician.

178. Rothblatt, *Revolution* (ref. 8), 200-213; Sviedrys, "Victorian Cambridge" (ref. 169), 127.

Relativity and eclipses: The British eclipse expeditions of 1919 and their predecessors

EVERYONE WHO KNOWS a little of the history of physics recalls that Einstein predicted in 1916, on the basis of his newly presented general theory of relativity, that starlight is bent by the gravitational field of the sun, and that in 1919 British expeditions obtained confirmation of this prediction from photographs taken during a total eclipse. The report of the expedition made Einstein an international celebrity, and put talk of relativity on the front page of the *Times* of London. The eclipse expedition report drove scientific opinion in Einstein's favor and won general relativity the admiration and interest of many physicists. As far as science is concerned, no eclipse before or since has been so important. The British eclipse results and the British discussion and assessment of relativity that preceded and followed them have been offered as an example of a scientific revolution carried out rationally, without nationalist prejudice or obscurantism,¹ an example the more remarkable because it occurred during and immediately after a bitter war with Germany, Einstein's home. Perhaps the example is too remarkable. The apparently sudden and dispassionate willingness of many British physicists to give

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The following abbreviations are used: CP, H. D. Curtis Papers, Hillman Archive, University of Pittsburgh; EP, Albert Einstein Papers, Princeton University Library; RAS, *MN*, Royal Astronomical Society, *Monthly notices*.

1. See D. Moyer, "Revolution in science: The 1919 eclipse test of general relativity," in A. Perlmutter and L. F. Scott, eds., *On the path of Albert Einstein* (New York, 1979), 55-101. We have found this paper of great help in studying the English eclipse expeditions.

their sympathy to Einstein's theory appears anomalous, and requires some explanation.

The initial reception of special relativity in English-speaking countries was almost uniformly hostile or disdainful. English physicists did not work on a theory so removed from the ether mechanics on which they had been reared.² General relativity was, before 1919, even less known in England than the special theory, because the war had interrupted receipt of German scientific journals. Arthur Stanley Eddington, as Secretary of the Royal Astronomical Society, had received from Willem de Sitter, who was living in Holland, the only copy of Einstein's paper on the gravitational field equations of general relativity to reach England during the war.³ Eddington arranged for a series of papers by de Sitter, expounding the new theory, to appear in the *Monthly notices* of the Royal Astronomical Society and in 1918 Eddington himself wrote for the Physical Society a persuasive review of the new theory and its applications.⁴ Yet there is no evidence that before 1919 many British physicists had warmed to general relativity. What little literature appeared on the theory was generally critical, or concerned with presenting alternatives. Before 1919 there did not appear in British scientific journals a single article, other than de Sitter's, that applied or extended the new theory. British physicists had to take cognizance of the theory, primarily because Eddington and de Sitter had made them aware that Einstein had succeeded in explaining the long-standing anomaly in the motion of Mercury, but prior to the eclipse expedition they were not disposed to accept the new relativistic account of gravitation or even to trouble much to understand it.⁵

One may imagine that in order to turn the tide of opinion the eclipse results must have been unequivocal. They were not. Early in 1919 the Royal Astronomical Society received from W. Campbell, Director of the Lick Observatory, a report of an American expedition to measure the deflection of starlight during the total eclipse of 1918. Although Campbell was very cautious about the value of these measurements, he reported that in his opinion they definitely ruled out the value for the deflection predicted by Einstein's theory.⁶ The Brit-

2. See S. Goldberg, "In defense of the ether: The British response to Einstein's special theory of relativity, 1905-1911," *HSPS*, 2 (1970), 89-125.

3. Einstein, "Die Feldgleichungen der Gravitation," Akademie der Wissenschaften, Berlin, *Sitzungsberichte* (1915), 844-847; S. Chandrasekhar, "Verifying the theory of relativity," *Bulletin of atomic scientists*, 31 (Jun. 1975), 17-22.

4. W. de Sitter, "On Einstein's theory of gravitation and its astronomical consequences," *RAS, MN*, 76 (1916), 699-728, 77 (1916), 155-184, 78 (1917), 3-28; A. S. Eddington, *Report on the relativity theory of gravitation* (London, 1918).

5. "Astronomy at the British Association," *Observatory*, 42 (1919), 365.

6. "Meeting of the Royal Astronomical Society, Friday, July 11, 1919," *ibid.*, 298-299.

ish results, taken at face value, were conflicting, and could be held to confirm Einstein's theory only if many of the measurements were ignored. Even then, the value of the deflection obtained was significantly higher than the value Einstein predicted. The contrast with the red-shift is interesting. In 1911 Einstein had predicted that the spectrum of sunlight received on Earth should be shifted towards the red in comparison with light from terrestrial sources. Many observers did not find the shift Einstein predicted; others did, at least for certain spectral lines, and earlier observations had shown a few lines for which the shift was as Einstein predicted. If one were willing to throw out most of the data, one could argue that Einstein's prediction was confirmed, but no one made such an argument for the red-shift until 1919.⁷ Why was the willingness to dismiss contradictory data from the eclipses so much greater?

There is a prior question, almost equally puzzling: why did the British set out to measure the deflection at all? Total eclipses are not to be wasted, and in 1919 the British devoted nearly all of their resources to two expeditions to test the Einstein effect. To take place in 1919 the eclipse expeditions had to be planned earlier. Plans were announced in 1917. But in 1917 the British were at war with Germany, and bitterly at war. The pages of *Nature* and *Observatory* were then occasionally disfigured with articles on the decadence and inferiority of German science, and on the derivative and imitative character of the German mind. British scientists talked about boycotting German science after the war: German publications were not to be referred to, Germans were not to be invited to international scientific meetings, and so on. To some extent these recommendations were implemented.⁸ Although he retained Swiss citizenship, throughout World War I Einstein, of German birth, was a member of the Berlin Academy, living in Berlin. Add to this the general British disinterest in things relativistic. Why then, to the almost complete exclusion of other solar physics, did the British plan to mount expensive and troublesome expeditions to test a German's theory?

In what follows, we describe the scientific circumstances surrounding the tests of Einstein's prediction, and suggest some possible answers to the questions just posed. Great changes in scientific opinion do not occur without the advocacy of influential persons. In the case before us those persons were Frank Watson Dyson and

7. Einstein, "Über den Einfluss der Schwerkraft auf die Ausbreitung des Lichtes," *Annalen der Physik*, 35 (1911), 898-908; K. Schwarzschild, "Über die Verschiebungen der Bänder bei 3883 Å in Sonnenspektrum," Akademie der Wissenschaften, Berlin, *Sitzungsberichte* (1914), 1201-1213; J. Earman and C. Glymour, "The gravitational red shift as a test of general relativity: History and analysis," *Studies in history and philosophy of science*, in press.

8. J. Haberer, "Politicalization in science," *Science*, 178 (1972), 713-724.

Arthur Stanley Eddington, who were instrumental in arranging for the eclipse expeditions and in enforcing the conclusion that the eclipse results confirmed Einstein's theory. The data helped, but Dyson and Eddington were also essential. By 1919 measurement of the "Einstein effect" had become a challenge; the failure of repeated attempts, before 1919, to obtain satisfactory measurements had given the project the special allure of an experimental task widely recognized to be difficult, just possible, and certainly significant.

1. DERIVATIONS OF THE DEFLECTION OF LIGHT

Einstein gave two arguments for the deflection of light passing near a massive body such as the sun; one argument, given in 1911 before the general theory was in hand, relied on his "principle of equivalence," while the other, given in 1916, used Einstein's own approximate solution to his gravitational field equations together with Huygens' principle from classical optics.⁹ The former derivation gave a value for the deflection at the limb of the sun of 0.83" of arc, the latter 1.7" of arc. Better values for the constants involved give 0.87" and 1.74"; these are the values usually cited in the literature for the deflection at the limb of the sun according, respectively, to the equivalence principle alone and to the field equations of the general theory. Einstein's derivations, which persuaded most of his contemporaries, are still sometimes given in textbooks. From a modern point of view, however, they are both problematic.

Einstein had not by 1911 yet absorbed the four-dimensional geometrical way of viewing space-time urged by Minkowski. In certain respects his thinking about space-time was still classical. In his paper of 1911 Einstein argued that the frequency ν of light received on Earth is related to the frequency ν_0 of that same light when emitted from the surface of the sun by

$$\nu = \nu_0(1 + \phi/c^2), \quad (1)$$

where ϕ is the negative difference of gravitational potential between the sun and the Earth. Equation (1) offers a paradox. The frequency is just the number of periods per second, but, according to (1), the number emitted is different from the number received; this seems absurd, since the process in question is supposed to be stationary. Einstein's solution to this "absurdity" is that the same measure of time must not be used in the two places: "If we measure time in S_1

9. Einstein (ref. 7), and "Die Grundlage der allgemeinen Relativitätstheorie," *Annalen der Physik*, 49 (1916), 769-822.

with the clock U, then we must measure time in S_2 with a clock which goes $1 + \phi/c^2$ times more slowly than the clock U when compared with U at one and the same place."¹⁰ Because differently constructed clocks must be used at places having different gravitational potentials, the measured velocity of light will be different in different places, and Einstein claims that the velocity of light at a place with gravitational potential ϕ will be given by

$$c = c_0(1 + \phi/c^2), \quad (2)$$

where c_0 is the velocity of light at the origin of coordinates, that is, at the location of the source of the static gravitational field.

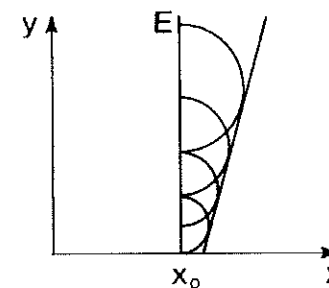


Fig. 1 Angular shift according to Huygens' principle of the wavefront in a region of varying velocity of light.

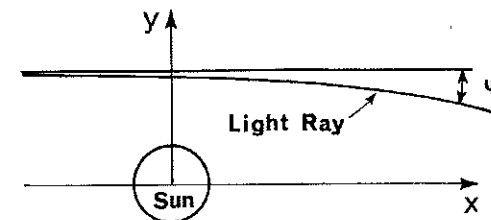


Fig. 2 Deflection of a light beam passing by the limb of the sun.

Consider now the plane wave moving from left to right in Figure 1, where Ex_0 is the wave front at some moment t . According to Huygens' principle, the wave front at time $t + dt$ is the envelope of the curves obtained by drawing at each point of the circle of radius $c(y)dt$, where $c(y)$ is the velocity of light at point (x_0, y) . Thus if the velocity of light varies as Einstein proposes in (2), the normal to

10. Einstein (ref. 7), translation from W. Perrett and G. B. Jeffery, *The principle of relativity* (New York, 1952), 106.

the wave front is turned towards the source, and the trajectory of the light wave looks as shown in Figure 2. The angle of deflection per unit of path is

$$\frac{d\psi}{dx} = \frac{1}{c} \frac{\partial c'}{\partial y}, \quad (3)$$

where c' is the velocity of light in the direction of motion. Using equation (2) and integrating Einstein obtains for the total deflection of a ray of light passing at distance Δ from a mass M the expression

$$\psi = 2kM/c^2\Delta. \quad (4)$$

The argument Einstein gave in 1911 is purely heuristic, and in its assumption that differently constructed clocks must be used to measure time at different places in the gravitational field is quite alien to the general theory of relativity in its final form. What is more surprising is that the derivation of the deflection that Einstein gave in 1916, on the basis of the completed general theory, is almost as contrary to the spirit of the theory. Einstein first obtained an approximate solution to his field equations for the exterior field of a static, spherically symmetric mass. The components of the metric tensor are

$$\left. \begin{aligned} g_{\rho\sigma} &= -\delta_{\rho\sigma} - \alpha \frac{x_\rho x_\sigma}{r^3} \quad (\rho, \sigma = 1, 2, 3), \\ g_{\rho 4} &= g_{4\rho} = 0, \quad g_{44} = 1 - \frac{\alpha}{r} \end{aligned} \right\}, \quad (5)$$

where r is $(x_1^2 + x_2^2 + x_3^2)^{1/2}$ and α is $kM/4\pi$. Einstein thought of a light ray as given by a set of coordinate intervals dx^i , i.e., a vector subject to the constraint

$$ds^2 = g_{ik} dx^i dx^k = 0 \quad (i, k = 1, 2, 3, 4). \quad (6)$$

If the direction of the vector in three-space is given, then the ratios $dx^1 : dx^2 : dx^3$ are determined, and so, in virtue of equation (6), is the quantity

$$\gamma^2 = \left(\frac{dx^1}{dx^4} \right)^2 + \left(\frac{dx^2}{dx^4} \right)^2 + \left(\frac{dx^3}{dx^4} \right)^2. \quad (7)$$

According to Einstein, γ is the velocity of the light ray "defined in the sense of Euclidean geometry."¹¹ For example, if the light ray is moving in the x^2 direction, then the velocity of light will be

$$\gamma = \frac{dx^2}{dx^4} = (-g_{44}/g_{22})^{1/2} \approx 1 - \frac{\alpha}{2r} \left[1 + (x^2/r)^2 \right]. \quad (8)$$

11. Einstein (ref. 9), translation from *The principle of relativity* (ref. 10), 162.

Now applying Huygens' principle, just as he had in 1911 but with the new expression (8) for the velocity of light, Einstein found a value for the deflection of light twice that obtained earlier.

The most immediately disconcerting feature of Einstein's procedure is that the formulas (7) and (8) represent only an artifact of the coordinate system. In general relativity, coordinate intervals do not represent distances or time periods, and their ratios do not represent velocities. Thus, for example, a different choice of coordinates would lead to a different "velocity" of light if equation (7) were applied, but all of the physically significant quantities of the theory are independent of the coordinates chosen. The application of Huygens' principle is especially inappropriate here because according to the general theory the velocity of light does have an invariant significance, and in fact while the application of Huygens' principle depends on the variation of the velocity of light from point to point, in general relativity the *proper* velocity of light as measured by standard clocks is always and everywhere a constant (unity in the units assumed in our notation).

Why then does Einstein's procedure yield the correct result? The Cartesian coordinate system (5) displays three crucial features of the metric g_{ik} : that it is static; that it is spherically symmetric; and that it is asymptotically flat. Thus in a natural way a flat Minkowski metric η_{ik} can be associated with g_{ik} , namely, one whose components in the special Cartesian coordinates are $\text{diag}(-1, -1, -1, +1)$. Einstein's procedure is essentially this: the trajectories of light rays passing through the solar system are the trajectories of null curves of the metric g_{ik} given by (5); that is, they are curves whose tangent vectors have zero space-time length according to g_{ik} . These vectors do *not* have zero space-time length according to the Minkowski metric η_{ik} ; furthermore, the proper velocity, according to η_{ik} , of a null trajectory of g_{ik} is not constant but is in fact the quantity γ of (7). The flat background metric is thus used to apply Huygens' principle to the variable light velocity γ . At distances from the sun as great as the Earth's, η_{ik} will be indistinguishable from g_{ik} to first order in α/r , and the value for the light deflection, calculated from Huygens' principle, will be numerically close to the actual value.

While the introduction of the background metric η_{ik} may help to rationalize Einstein's procedure, it goes against the thrust of general relativity by encouraging the practice of assigning a metrical significance to coordinate differences. Such a practice, unfortunately promoted by some of Einstein's own calculations, caused confusion among those less adept than he at getting the right answer.¹²

12. For details, see our paper (ref. 7).

Other contemporary derivations of the deflection of light were not any better than Einstein's. Schwarzschild and Droste independently found the same exact solution to Einstein's field equations (now known as "the Schwarzschild solution"), and both re-derived Einstein's formula for Mercury's perihelion advance.¹³ Neither attempted a derivation of the deflection of light. De Sitter repeated Einstein's derivation of 1916 with few changes. Eddington's derivation in his *Report on the relativity theory of gravitation* is especially curious. He noted that Einstein's "velocity" of light was merely a coordinate artifact and that the true velocity is constant. After giving essentially Einstein's equation (7) for the velocity of light, Eddington wrote: "The velocity thus depends on the direction; but it must be remembered that this coordinate velocity is not the velocity found directly from measures at the point considered. When we determine the velocity by measures made in a small region, and use natural measure (i.e., $g_{uv} \dots$), the measured velocity is necessarily unity."¹⁴ Then, having pointed to the essential flaw in the argument from Huygens' principle, Eddington proceeded to give that very argument, and no other, for the deflection.

A sound general relativistic derivation of the deflection of light proceeds by integration of the equations of null geodesics of the Schwarzschild metric. Such a treatment was begun in an appendix of Eddington's book, *Space, time and gravitation* (1921); but the treatment given there and in his later work, *The mathematical theory of relativity* (1923), was not entirely rigorous because the equation of null geodesics was obtained by discarding, on heuristic grounds, a term in the equation for timelike geodesics. Eddington obtained the equation for timelike geodesics,

$$d^2u/d\phi^2 + u = m/h^2 + 3\mu^2, \quad (9)$$

where $u = 1/r$, ϕ is an angular coordinate, and h is a constant equal to $r^2 d\phi/ds$. Eddington then argued that since for a null geodesic $ds = 0$, it follows that $h = \infty$, and hence that $m/h^2 = 0$, making the first term on the right hand side of (9) disappear. Although the resulting equation is correct for null geodesics in the Schwarzschild solution, the argument for it is questionable, since the correctness of the conclusion depends on a judicious choice of the form of the equations of motion. If, for example, the same reasoning were

13. K. Schwarzschild, "Über das Gravitationsfeld eines Massenpunktes nach der Einsteinschen Theorie," Akademie der Wissenschaften, Berlin, *Sitzungsberichte* (1916), 189-196; J. Droste, "The field of a single center in Einstein's theory of gravitation, and the motion of a particle in that field," Akademie van Wetenschappen, Amsterdam, *Proceedings*, 19 (1917), 197-215.

14. Eddington (ref. 4), 53.

applied directly to the standard form of the general relativistic geodesic equation all the terms would become infinite if ds were set equal to zero. A correct treatment of null geodesics requires the use of an affine parameter in the equations of motion, which, unlike s , is not a measure of arc length. In any case, these derivations were given after Einstein's theory had won a wide following, thanks largely to Eddington's claim to have verified Einstein's light-deflection prediction.

In the case of Einstein's red-shift prediction, we know that the most prominent observers working on the problem had very little understanding of how the prediction was obtained, and even mathematical physicists were, for a long while, confused about the connection between the theory and the prediction. There is reason to think that some participants in the early attempts to measure the gravitational deflection of light had no firmer grip on the theory. Thus C. A. Chant of the University of Toronto, an astronomer who directed the Canadian expedition to measure the deflection at the solar eclipse of 1922, wrote in April of 1923 to H. D. Curtis, then Director of the Allegheny Observatory in Pittsburgh:¹⁵

Just a few minutes ago I was called to the telephone to speak to Dayton C. Miller, of Cleveland, who is on his way to Montreal to speak on the Michelson & M. exp.: he says in the January Phil. Mag. Larmor says Einstein's result is twice as large as it should be. I have not seen the paper & suppose it will be involved and difficult to read, but I shall look into it to-morrow. 'Who shall decide if doctors disagree?' Almost daily I am called on to explain Einstein, and have to plead ignorance of the way in which the results are obtained.

By 1916 Einstein had obtained two different expressions for the angular deflection of a light ray by a massive gravitational source, both giving the angle as a hyperbolic function of distance of closest approach to the massive body. The two expressions, one from the principle of equivalence and the other from the general theory, differ only by a factor of two. Eddington argued that the smaller expression, that of equation (4), is predicted by Newtonian theory. In his *Report* Eddington obtained this "Newtonian" value for the deflection by assuming that light of energy E has mass E/c^2 and is subject to Newton's law of gravity.¹⁶ Later, in his report to the famous joint meeting of the Royal Society of London and the Royal Astronomical Society, gathered to hear the results of the eclipse expeditions, Eddington suggested with uncharacteristic hesitancy that the

15. C. A. Chant to H. D. Curtis, 15 Apr 1923 (CP).

16. Eddington (ref. 4), 56.

Newtonian hypothesis is represented by supposing that light follows null geodesics and massive particles follow time-like geodesics of the metric

$$ds^2 = -dr^2 - r^2 d\theta^2 + (1 - 2M/r) dt^2. \quad (10)$$

This expression, Eddington said, "may be accepted as corresponding to Newton's law—at any rate, it gives no motion of perihelion of Mercury and the half-deflection of light."¹⁷

Nothing about the motion of light in a gravitational field follows from Newtonian theory unless some assumption about the coupling of gravity and electromagnetic radiation is made. Laplace and J. Soldner had calculated gravitational action on light assuming a particle theory of light; Laplace had determined the mass density requisite for light to be gravitationally captured—that is, for a Newtonian black-hole—and Soldner had calculated the deflection of a light ray grazing the limb of the sun.¹⁸ Neither piece of work was familiar to physicists in 1918, and there is no reference to them until, in 1921, Lenard republished Soldner's work as part of an attempt to discredit the novelty of Einstein's prediction. One could quite as consistently with Newtonian theory assume that gravity acts on radiation according to some other law than that for matter, but the simplest assumption is that no action occurs or that it takes place according to the law for matter. One thus obtains a prediction of no deflection, or of the deflection given by equation (4). Eddington repeatedly claimed that these and Einstein's second prediction were the only possibilities.

2. MEASUREMENT OF THE DEFLECTION

Einstein suggested in his paper of 1911 that his prediction might be tested at eclipses. Figure 2 implies that if the line parallel to the x-axis represents the course of a light ray when the sun is not nearly between the star and the observer on earth, then when the sun is nearly between—when it is at the origin of coordinates—the light from the star should be bent and the star should appear displaced in comparison to the first case. The displacement should be radially

17. "Joint eclipse meeting of the Royal Society and the Royal Astronomical Society," *Observatory*, 42 (1919), 389-398.

18. Laplace, translated in S. Hawking and G. Ellis, *The large-scale structure of space-time* (Cambridge, 1973), Appendix; J. Soldner, "Über die Ablenkung eines Lichtstrahls von seiner geradlinigen Bewegung durch die Attraktion eines Weltkörpers," *Annalen der Physik*, 65 (1921), 593-604, reprinted from *Berliner astronomisches Jahrbuch* for 1804.

away from the sun. In principle, then, to determine the deflection one should photograph a field of stars at a season when the sun is not between them and the Earth, and photograph the same field during a total solar eclipse when the sun is so located that light going from the stars to the earth must pass near the sun's limb. Superposing the two photographs, one should be able to measure the displacement of each star image in fractions of millimeters and, knowing the scale of the photograph, calculate the displacement in seconds of arc.

In practice, the slightest mechanical change in the telescope between the taking of the two sets of photographs will alter the scale: one millimeter on the eclipse photographs will correspond to a different number of seconds of arc than will a millimeter on the comparison photographs. A displacement will occur because of the change of scale. Since the eclipse and comparison photographs are ordinarily taken months apart, one set during the day and the other at night, significant scale differences are always to be expected. In addition, small rotational and translational shifts of star images occur in the course of superposing the eclipse photograph on the comparison plate. Further, besides the displacement due to scale differences (traceable chiefly to a change in focal length of the telescope) there are displacements due to changes in the orientation of the photographic plates to the optical axis. Altogether, modern treatments of the deflection involve at least a dozen parameters (six for displacement in the direction of each of two orthogonal axes) that require the images of at least six stars for their determination.¹⁹ The number of parameters required is sometimes reduced by the use of check plates, photographs of an arbitrary field of stars taken usually the night before or the night after the eclipse and again when the comparison photographs are taken. Assuming that the focus has not changed between the day of the eclipse and the night before or the night after, or between the comparison exposures and those of their check plates, the scale factor between eclipse and comparison plates can be determined from the check plates. The assumption is not always a good one. The eclipse photographs are taken during the day and the accompanying check plates at night, when different temperatures prevail; and the telescope must usually be moved to obtain check plates with the comparison plates. Since changes in the effective focal length as small as a hundredth of a millimeter in a standard astrographic telescope can have significant effects on the scale value determined from check plates, temperature changes and mechanical deformations can be serious sources of error.

19. H. von Klüber, "The determination of Einstein's light-deflection in the gravitational field of the sun," in A. Beer, ed., *Visitas on astronomy*, 3 (1960), 47-77.

What one has after clamping the eclipse and comparison photographs together and carefully measuring the displacement of the star images are displacements Δx and Δy for each star image on the eclipse photograph. These displacements will be different for different stars. The measured displacements are due to the action of several different causes, the most significant of which are those just described. The different causes have differing dependencies on the distance of the star image from the solar image. The Einstein deflection (or the Newtonian one) decreases hyperbolically with increasing angular distance of the star from the sun, whereas the displacement due to a change of scale should increase linearly with angular distance from the sun. One can write an equation for the x displacement of a star image and another for the y displacement. Each equation will have, say, six parameters; with six stars the values of the parameters will be uniquely determined, but with more than six stars no single assignment of values to the parameters will exactly fit the data. Typically, values of the parameters are chosen that satisfy the criterion of least squares.

Attempts prior to 1919

Many failures can be lost in the light of a great success. Most of the attempts before 1919 to measure the deflection of light have been forgotten, even by historians. Prior to 1919 there were at least four eclipse expeditions intent on measuring the deflection of starlight. Einstein knew of all of them. Because of bad weather or war, three of the expeditions failed to obtain any results. The results of the fourth never saw print. The British eclipse expeditions were not novel in their aim but only in their success.

The key figure in the early attempts to measure the deflection of light was Erwin Freundlich. Freundlich had studied at Göttingen under Felix Klein, and then taken a post at the Berlin observatory. Learning that Einstein was disappointed that professional astronomers had taken no interest in testing his ideas, Freundlich wrote to him offering his collaboration. A regular correspondence ensued; Einstein and Freundlich discussed not only eclipse plans, but also the possibility of photographing stars near the sun in daylight (a suggestion that was also made by F. A. Lindemann) and the possibility of detecting the effect of Jupiter's gravitational field on starlight.²⁰ Freundlich received the proof sheets of Einstein's paper of 1911 on

20. Einstein's correspondence with Freundlich (EP); ref. 17, 395; A. Hermann, ed., *Albert Einstein/Arnold Sommerfeld Briefwechsel* (Basel, 1968), 36.

the influence of gravitation on light, and from then on devoted himself to the task of finding empirical evidence for or against Einstein's predictions.²¹

Freundlich first thought that eclipse photographs taken for other purposes might betray the deflection, and he was especially interested in photographs taken by the Lick Observatory. In October of 1911 astronomers interested in Ephemeris work had met in Paris. Following the Congress, C. D. Perrine, of the Argentine National Observatory, stopped over briefly in Berlin, and Freundlich called on him. Perrine had used the 40-foot camera of the Lick Observatory to photograph the eclipse of May, 1900; the sun had then been almost precisely in the same field of stars that it would occupy at the eclipse of May, 1919. The field was extremely good for detecting the Einstein deflection if there was one.²² Freundlich had both a question and a request. The question was whether the Lick photographs would be satisfactory for detecting the deflection if one existed. Perrine was doubtful. The photographs had been taken to study the solar corona and to detect a sub-Mercurial planet. The solar photographs, Perrine thought, would be unsatisfactory because of the small field and brief exposure time, so that few stars would appear, and the photographs taken in search of Vulcan would be unsatisfactory because of the eccentric position of the sun on the plates. Freundlich's request was that the Argentine Observatory undertake to obtain photographs suitable for detecting the deflection of light during a planned expedition to Brazil to photograph the eclipse of October 10, 1912. Perrine agreed and was able to obtain the Lick Observatory's "intermercurial" camera lenses which had been used in the search for Vulcan. Alas, at the expedition's station in Christina it rained before, during and after the eclipse. Perrine wrote that "we...suffered a total eclipse instead of observing one."²³

On August 21, 1914, there was a total eclipse of the sun whose path of totality passed through southern Russia. On July 28 of that year, a month after the shooting of Archduke Ferdinand, Austria-Hungary invaded Serbia, and on the first of August Germany declared war on Russia. It was as though an omen had arrived three weeks too late. Eclipse expeditions from at least three observatories found themselves in Russia as the war began and the scientific pro-

21. L. Pyenson, "The Göttingen reception of general relativity," (Ph.D. thesis, Johns Hopkins University, 1974), 316.

22. W. Campbell, "The total eclipse of the sun, September 21, 1922," *Astronomical Society of the Pacific, Publications*, 35 (1923), 11-44, on 18.

23. C. Perrine, "Contribution to the history of attempts to test the theory of relativity by means of astronomical observations," *Astronomische Nachrichten*, 219 (1923), 281-284.

grams of all three suffered. The key figure, again, was Freundlich. He had planned to mount an expedition to test Einstein's prediction, and had discussed the plans in correspondence with Einstein. The Berlin Observatory and more particularly its director, H. Struve, refused to provide funds. Einstein wrote Freundlich that if official funds were not forthcoming he would obtain them from private sources, and eventually funds did come from Emil Fisher, the distinguished chemist, and from Krupp.²⁴

Freundlich had borrowed parts of telescopes from the Argentine Observatory and had assembled four cameras altogether. He was to meet an expedition from Argentina in Feodosia, in southern Russia, where the two expeditions would join up and take their photographs. Freundlich never made it. Instantly converted on the first of August from visiting astronomer to enemy alien, Freundlich was interned and his instruments impounded. He was soon exchanged for Russians in similar circumstances, and returned to Berlin in September of 1914. Meanwhile, the group from the Argentine Observatory found themselves helpless to take over Freundlich's program, partly because they had loaned instruments to Freundlich, and partly because their own instruments arrived late.

The Lick Observatory had also sent an expedition to Russia, under the direction of Campbell. They too hoped to measure the deflection of starlight, but they were foiled by bad weather. The members of the expedition returned to California but their instruments had to be left in Russia for the duration of the war, a circumstance that was to be important for a subsequent attempt to measure the deflection.²⁵

In 1916 there was an eclipse whose path of totality passed through Venezuela. Most of the world was busy with something else. The Argentine Observatory sent a small expedition but did not attempt to take photographs that would show the deflection.²⁶ In 1918 there was, at last, a convenient eclipse, if not as to time then at least as to place: the path of totality passed through the United States. The Lick Observatory people scarcely had to move in order to photograph the event. Unfortunately, they had nothing to photograph it with. The Lick Observatory's instruments, stored in Petrograd at the Pulkovo Observatory, were not shipped until August of 1917. Eventually they reached Vladivostok, where they stopped. In March of 1918 they had reached Kobe, Japan, but they were again stalled because war materials took precedence. The eclipse was to

24. Einstein to Freundlich, 7 Dec 1913 (EP); Pyenson (ref. 21), 325.

25. Campbell (ref. 22), 17.

26. Perrine (ref. 23).

occur on June 8 of 1918, and about five weeks before the event Campbell and his associates gave up hope of receiving their instruments in time. They did succeed, however, in borrowing two lenses from an Oakland observatory, which they took to Goldendale, Washington, the eclipse station, and mounted with plate holders and cameras. The lenses were not designed for astrographic work; with a sharp focus the field of stars was very small. Accordingly, the astronomer in charge of the instruments, H. D. Curtis, placed the plates "in a compromise focal position." The weather was not good, but a hole in the clouds allowed one photographic plate to be obtained with each instrument. Comparison plates were exposed at the Lick Observatory in January of 1919.²⁷ Thus were obtained the first photographs designed to detect the gravitational deflection of sunlight. What happened to them afterwards is both remarkable and obscure.

The force behind the Lick Observatory's initial interest in Einstein's prediction was not Campbell but Curtis. It was Curtis who brought Einstein's prediction to Campbell's attention, Curtis who set up the instruments and took the photographs in 1918, and Curtis who was to do the measurements.²⁸ Curtis was apparently on leave from war duty during the eclipse. He developed the plates and then returned to government service for another ten months; apparently no one at the observatory did anything with them until he returned. Then an instrument had to be constructed for measuring the plates, and so it was in fact May or June of 1919 before measurements of the plates began. They showed 50 stars, down to the ninth magnitude. Within seven weeks of Curtis' return they had all been measured for displacement; in July of 1919, before the British eclipse expeditions had returned, Campbell was in England, where he attended the Royal Astronomical Society meeting of July 11 and briefly reported Curtis' results. Curtis did not test Einstein's prediction in the manner we have described, apparently because the star images were not sufficiently sharp to make the procedure seem reliable. Instead, Curtis divided the stars into an inner and outer group, which, according to Einstein's hypothesis, should have different mean displacements. Campbell reported that "the differential displacement between the two groups should have been 0".08 or 0".15, according to which of Einstein's hypotheses was adopted. The mean of the results came out at 0".05 and of the right sign." It is not clear from Campbell's oral report how the scale factor was determined if

27. Campbell (ref. 22), 18, and "The Crocker eclipse expedition from the Lick Observatory, June 8, 1918," American Philosophical Society, *Proceedings*, 58 (1919), 241-254, on 253.

28. Campbell (ref. 22), 15.

at all. Campbell concluded: "It is my opinion that Dr. Curtis' results preclude the larger Einstein effect, but not the smaller amount expected according to the original Einstein hypothesis."²⁹

One expects that the particulars of these results would have been promptly reported in print. Instead, nothing happened. According to Campbell, Curtis continued to make measurements on the plates and to discuss their reduction over the course of the next year. In July of 1920 a report of the results still had not appeared, and Curtis left the Lick to become director of the Allegheny Observatory. Campbell and an assistant now re-measured all of the plates, and still a report of the results did not appear. To the best of our knowledge, the results of the Crocker expedition of 1918 have never been published and in consequence they had virtually no effect on the reception of general relativity.³⁰ Something of the nature of the expedition's results, and an explanation of the failure to publish, are suggested by Curtis' surviving correspondence with Campbell.

In early 1920 when Curtis moved to Pittsburgh, the only information that had been published regarding the outcome of the measurements of the 1918 plates was the very guarded statement by Campbell to the Royal Astronomical Society that the measurements definitely ruled out the larger deflection. Late in 1919 the results of the British expedition were reported to a joint meeting the Royal Astronomical Society and the Royal Society of London, but the published report of the expedition did not appear until 1920. In 1920 the results of the Goldendale expedition were still very much of interest.

On July 15, 1920 Campbell wrote to Curtis reporting the results of the computations of one of his assistants, Miss Adelaide Hobe.³¹ Campbell thought that the numbers disagreed with Einstein's theory. Curtis wrote back on July 22 regretting that the results were not definite enough to be announced. Campbell replied:³²

Just a few lines to suggest, in response to yours of July 22nd, that you say little or nothing about the Einstein results yet because I think some errors, possibly serious, are showing up in the recent work, partly through errors in transferring data from note books to computation sheets. In a few days I hope to go over the computations pretty carefully and see what the next step should be.

29. Campbell (ref. 6), 299.

30. Dr. Donald Osterbrock informs us that to his knowledge the astrometric measurements of the "Einstein-Vulcan plates" (as they were then called) were never published; see Osterbrock, "Lick Observatory solar eclipse expeditions," *Astronomy quarterly*, 3 (1980), 67-79.

31. CP.

32. Campbell to Curtis, 26 Jul (CP).

By September of 1920, Campbell had decided to remeasure the eclipse plates, only to conclude that the comparison plates were unsatisfactory. He located the difficulty in the mounting of the eclipse lenses (Chabot lenses), which resulted in blurred images. In October he attempted to get better comparison plates by mounting the lenses on the side of the large reflector at the Lick. The resulting images were overexposed and elongated. On December 20, 1920 Campbell wrote to Curtis:³³

My provisional opinion, based upon these overexposed plates, is that the Chabot lenses cannot be depended upon to answer the Einstein question either positively or negatively. If, after securing the shorter exposures, I find that the images at a considerable distance from the center are too poor to permit accurate measurement—and that I am really expecting—I think it will be desirable to publish a note to that effect, calling attention to the unfortunate facts that the L. O. eclipse lenses were expected to return from Russia almost up to the last minute, that we had to give them up, borrow other lenses, and construct the camera mountings at the eclipse station in the fortnight immediately preceding the eclipse, that the driving clock was an inferior one, etc.

Thus, by the end of 1920, Campbell was disposed to discard a year's work. Curtis was not pleased, and he wrote back proposing a draft that is the closest thing we have to a report of the 1918 expedition:³⁴

The matter is entirely in your hands. It may be best, as you suggest, simply to state that, as a result of extensive measurements and tests, - "we are forced to the conclusion that no sufficiently definite result can be secured from the Goldendale plates to warrant their publication as a trustworthy authority either for or against the existence of a deflection effect".

My own strong preference, however, would be to append some such statement as that given above to a brief description of the plates, the measurements and the results.

"The simple mean of the measurements made on the Goldendale plates in both coordinates and of six plates of the 1900 eclipse in declination is $0.69 \pm .02$ for the deflection at the Sun's limb. When weighted in accordance with the values assigned in the following short table, the value of the deflection at the Sun's limb becomes $0.74 \pm .03$.

33. Same to same, 20 Dec 20 (CP).

34. Curtis to Campbell, 29 Dec (CP).

Plate		Defl. at limb	No. stars	Weight
Goldendale	A, R.A.	+1".55 ±".05	35	3
"	A, Decl.	+0.53 ± .14	35	5
"	B, R.A.	+1.76 ± .16	31	3
"	B, Decl.	+0.22 ± .14	31	5
Chabot No.	2, Decl.	+0.38 ± .08	8	1
"	4, "	+0.51 ± .08	10	1
"	4, "	+0.29 ± .11	8	1
"	10, "	+0.79 ± .05	9	1
"	3, "	+0.57 ± .13	8	1/2
"	7, "	-.12 ± .07	8	1
"	9, "	+0.65 ± .06	10	1
"	9, "	+1.13 ± .07	6	1

"The mean of the above results suggests the gravitational deflection of 0.87 at the Sun's limb. From the data given earlier as to the character of the plates employed, from the probable errors of the separate plates, and from the serious lack of agreement in the individual results, we do not believe that these results permit a decision for or against the Einstein or other deflection hypothesis, and these indecisive results are published simply as a matter of record." etc., etc.

A simple, frank statement, as above, of indecisive results secured will, so far from hurting the L. O., increase its already great reputation for sanity and conservatism, and for not announcing theories till it can deliver the goods. When the Einstein theory goes into the discard, as I prophesy it will go within ten years, these negative or indecisive results will be more highly regarded than at present.

Curtis' error terms are likely probable errors, in keeping with the statistics of the times; we have no idea how they were determined, or the basis for the weights assigned. The entries in the table for the Chabots were taken from the plates exposed by the Lick Observatory expedition to Georgia at the eclipse of 1900 for the purpose of studying the solar corona and locating sub-Mercurial planets. A striking feature of Curtis' values for the Goldendale plates is that the right ascensions agree well and the declinations conflict badly with the deflection predicted by general relativity. This raises the question whether the images were blurred in such a way as to suggest systematic error in the declinations. The subsequent correspondence between Curtis and Campbell answers the question affirmatively, but it also casts doubt on the reliability of Curtis' reduction of the measurements.

Although Campbell had proposed giving the whole thing up at the end of 1920, in the Spring of 1921 he was still at work on the reduction of the data. On March 31, 1921 Campbell wrote to Curtis that "We... shall soon have something to communicate as to the

results. We are applying a few more checks before daring to make any announcements of results."³⁵ On April 13 he wrote that he would not attend meetings in Washington because he did not want to take the time away from the Einstein work: "If I attend one meeting in Washington, and thus say A, I could not avoid saying B, C, D, etc., while on the Atlantic Coast, and that would mean an absence of at least three weeks, an intolerable period under present conditions. The subject has been on my mind every day, and I can truthfully say every night, for a good many months, and I do not want to stop striking while the irons are hot." With the assistance of Joseph Moore, Robert Trumpler and Miss Hobe, Campbell had devised an illumination system for the plates, consisting essentially of an electric light "about 32 inches directly behind each star image," and had obtained new comparison plates that were not overexposed. On May 4 he wrote again: "I am still giving much of my time to the Einstein measures and reductions, but I think we are within ten days of the end."

The last informative piece of correspondence from Campbell to Curtis is dated May 19, 1921:

I am expecting to write up our Einstein results about the latter half of next week, as Dr. Trumpler began this morning on what I think is the last section of the computational work. Very soon thereafter I shall tell you all about it. In the meantime I would like your comments on the cause or causes leading to the peculiar form of the star images recorded at Goldendale. The four cameras in the bunch all gave the same form of image at those points on the plates where the images were in best focus. Your focal settings were, as you know, a compromise between the central and the circumferential foci. The images in focus, greatly enlarged, are like this:

They are plainly doubled



and in addition dis-

torted chiefly by unfortunate motions of the cameras as a whole, and the images of the outer parts of the plates are further complicated by radial distortions. What could have caused the motions of the cameras? The principal image is the upper one, but it has a prolongation or tail extending *down* to the *right* with which irregularities in clock running could have had very little to do. The fainter image is below and to the left, but an extension or tail extends to the right and a little upward, which may have been due partly to rapid changes in the clock rate, but not entirely so.

Curtis replied on May 24, suggesting that the trouble was with

35. This and the following letters quoted are in CP except for those of 22 Jul 20 and 3 Jul 22, which are in the Lick Observatory Archives.

the mounting: "It is my opinion that the wooden mounting and tubes were not sufficiently rigid and could "give" a little as the instrument took different hour angles, certainly in declination and probably a little also in R. A."

Campbell never did send Curtis the results of his new measurements and calculations, and the report, if written, was never published. The correspondence indicates the quality of the Goldendale plates and of the measured deflections. It also suggests why the results were never published. Curtis, who did the initial work and originally brought the problem of measuring the gravitational deflection of light to Campbell's attention, was a convinced anti-relativist, and remained so at least through the twenties.³⁶ He was willing, perhaps even anxious, to publish the results of the Goldendale expedition, as well as other results that conflicted with general relativity. In other correspondence he touted measurements of red-shifts made at the Allegheny Observatory as decisively refuting Einstein's predicted gravitational red-shift. Even after the Lick Observatory expedition to the 1922 eclipse produced results in good agreement with Einstein's value for the deflection Curtis wrote to C. A. Chant:

I see that Campbell has likewise announced that his plates show a deflection of about 1."5 at the limb. Well, there may be a deflection, but I do not feel that I shall be ready to swallow the Einstein theory for a long time to come, if ever. I'm a heretic.

But Curtis could not do anything about publication of the Goldendale results without Campbell, and Campbell was a man of a quite different temperament. He would rather be right than first.

Campbell was very loath to let anything out of his hands if it might contain an error. Contemporaries complained about his hoarding of data on at least two occasions. One concerned the velocities of stars and nebulae. Campbell had directed a program of measurements of velocities at the Lick Observatory for many years. In addition, in 1903, he had arranged for the construction of an observatory with a large reflector and spectrograph near Santiago, Chile, in order to determine radial velocities of stars in the Southern Hemisphere. At the time, Campbell's measurements were the only ones being made of Southern Hemisphere stars. Many astronomers were anxious to learn the results, especially when, in the 1920s, such data

36. Curtis met Einstein in Washington in 1921, and reported his observations to Campbell (letter of 11 May [CPI]): "He surely looks like the fourth dimension! Face is somewhat sallow and yellowish, redeemed by very keen bright eyes. But wears his hair à la Paderewski in narrow greasy curls of small diameter and four or five inches long."

became essential to the resolution of the dispute about the distances of stars and nebulae. Although Campbell's colleague, W. H. Wright, thought that the Lick had been generous in publishing "items of particular interest, such as variable radial velocities, observations of novae, and the like," in fact Campbell did not publish the majority of the data until 1928.³⁷

The other occasion had to do with a charge by Campbell that Curtis had made important errors in reducing the measurements he had made of the eclipse plates. "The sheets contained so many errors," Campbell wrote on July 3, 1922, "that we were led to regard your final results as fairly representative of your original measures, because the computational errors were so numerous as to be themselves subject to the law of accidental errors!"

Curtis replied on July 11, 1922:

It is now two years since I left. I have felt a bit hurt at times that you have never written me a line as to the *results* of the improved methods of measuring used by you and Miss Hobe, with more carefully checked computations. I have figured that you were perhaps saving these 'till after the coming eclipse, but you ought to know me well enough to realize that I would keep any such figures confidential, if you wished it so. I put considerable energy in on that proposition, enough, even if it is now regarded as valueless, to earn the right to know how things came out when no error was made. It impresses me as not quite fair.

Campbell's great caution, his reluctance to publish values that were not correct, the considerable variations in individual values obtained from the Goldendale plates, and the accumulating conviction that the declination measures were subject to systematic error doubtless suffice to explain why he did not publish any results from the 1918 eclipse. Even so, the plates from that eclipse, the endless measuring and remeasuring of them, the calculating and recalculating of the deflection and of the sources of error, were of considerable value to the confirmation of general relativity. They provided Campbell and his staff at the Lick Observatory with the enormous body of special knowledge needed to carry out the measurements of light deflection at the 1922 eclipse, measurements that were for some time the best available indicators of gravity's hold on light.

The popular image of Einstein waiting tranquilly or even indifferently for the reports of observers is an unlikely one. However certain Einstein may have been of his ideas (and his certainty, and

37. W. Wright, "William Wallace Campbell," *National Academy of Sciences, Biographical memoirs*, 25 (1941), 35-74, on 46; W. Campbell, J. Moore, et al., "Radial velocities of stars brighter than visual magnitude 5.51, as determined at Mount Hamilton and Santiago," *Lick Observatory, Publications*, 16 (1928), 1-399.

his ideas, wavered between 1914 and 1919),³⁸ he took an active role in arranging for their experimental confirmation. It was Einstein who communicated Schwarzschild's work on the gravitational red-shift to the Berlin Academy, Einstein who cultivated and encouraged Freundlich, and Einstein who arranged for Freundlich to loan equipment to Grebe and Bachem, who were among the first solar spectroscopists to claim to have observed the red-shift Einstein predicted.³⁹ Einstein certainly knew about the various attempts to measure the gravitational deflection of light; as the years wore on, with every attempt defeated by one circumstance or another, he must have felt considerable frustration. In the same letter in which he first described to Arnold Sommerfeld the equations of the general theory of relativity, obtained only days before, Einstein reported Freundlich's progress in a tone uncharacteristically wrathful: "Freundlich has a method to measure the deflection of light by Jupiter. Only the intrigues of wretched people are preventing this last important test of the theory from being carried out."⁴⁰ He added that he was not really deeply concerned because the theory was secure enough already! In September 1919, he wrote to Ehrenfest asking about news from the English eclipse expeditions.⁴¹

Einstein's frustration, and Freundlich's, are perfectly understandable. Expedition after expedition had returned empty handed. The one that did obtain photographs did not publish the results. Freundlich, who had learned of Campbell's preliminary views about the results of the expedition of 1918, wrote Einstein that the Americans had not been able to demonstrate the light-deflection effect.⁴² The eclipse of 1919 was the next hope, but the Lick Observatory could not mount an expedition because of the war. The Argentine Observatory had hoped to send an expedition to Brazil to test Einstein's prediction in 1919, but the government refused to finance it. Freundlich planned an expedition but did not carry it out, probably because of lack of funding.⁴³ Everything hung on the British, who seem to have won the privilege of photographing the 1919 eclipse almost as a spoil of war. While scientists in nearly every other

38. See our papers "Lost in the tensors: Einstein's struggles with covariance principles," *Studies in history and philosophy of science*, 9 (1978), 251-278, and "Einstein and Hilbert: Two months in the history of general relativity," *Archive for history of exact sciences*, 19 (1978), 291-307.

39. See ref. 7 for details.

40. Letter of 28 Nov 1915 (ref. 20), 36.

41. Letter of 12 Sep 1919 (EP); cf. Einstein to Besso, Mar 1914, in P. Speziali, ed., *Albert Einstein/Michele Besso correspondence* (Paris, 1972), 53.

42. Letter of 15 Sep 1919 (EP).

43. Freundlich to Einstein, 17 Jun 1917 (EP).

civilized nation were picking up the pieces from the Great War, the British set sail to test the general theory of relativity.

The background to the British eclipse expeditions

In 1917 Sir Frank Watson Dyson, the Astronomer Royal, was the most influential figure in British astronomy. Dyson, a man of solid but not brilliant scientific accomplishments, was one of those people, familiar in every discipline, who exercise enormous personal authority well beyond the influence of their published work. As chairman of the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society, it was Dyson who first proposed in print an expedition to test Einstein's prediction at the eclipse of 1919.⁴⁴ In November of 1919 Dyson announced that the expeditions had confirmed Einstein's prediction. Dyson himself was no relativist; his acquaintance with the theory and his interest in it doubtless came from Eddington. Eddington had succeeded Dyson as Chief Assistant at the Greenwich Observatory in 1905, after Dyson was named Astronomer Royal of Scotland. This appointment developed or initiated their acquaintance, for as Chief Assistant one of Eddington's first tasks was to complete the reduction of a determination of solar parallax that Dyson had undertaken with the help of C. R. Davidson. Eddington became Plumian Professor in 1913, Director of the Cambridge Observatory in 1914, and Secretary of the Royal Astronomical Society in 1916. He was the bright young light in British astronomy, and Dyson may have viewed him as a protégé.

Eddington and Dyson shared more than their interests in astronomy. Both came from nonconformist religious backgrounds; Dyson's father was a Baptist Minister and Eddington's father proprietor and headmaster of a Quaker school. Both had studied at Trinity College, Cambridge and their careers there were nearly as alike as could be: Dyson was second wrangler in the Mathematical Tripos of 1889, Eddington senior wrangler in 1904. Each was a Smith's Prizeman. They became friends and no doubt Eddington shared with Dyson his growing interest in general relativity.⁴⁵

In Eddington, the general theory of relativity found an influential and energetic champion. By 1918 he was a quasi-official spokesman, frequently publishing brief notes to clarify points about the theory or to defend it against criticism. His *Report on the relativity*

44. Dyson, "On the opportunity afforded by the eclipse of 1919 May 29 of verifying Einstein's theory of gravitation," *RAS, MN*, 77 (1917), 445-447.

45. H. Plummer, "Arthur Stanley Eddington, 1882-1944," *Royal Society, Obituary notices of Fellows*, 5 (1945), 113-125.

theory of gravitation, first published in 1918, was a careful and plausible presentation that widened the theory's audience in Great Britain. Eddington's advocacy and the conceptual novelty of the theory persuaded Dyson to propose expeditions to test it. According to S. Chandrasekhar, the distinguished astrophysicist and student of Eddington's, there was a further reason for Dyson's efforts.

In 1917 the British government made all able-bodied men subject to conscription. Eddington, a Quaker, was determined to refuse on grounds of conscientious objection. Various Cambridge dons, anxious to save Cambridge the embarrassment of having one of its professors refuse military service, attempted to arrange Eddington's deferment on scientific grounds. They were nearly successful. In Chandrasekhar's words: "A letter from the Home Office was sent to Eddington; and all he had to do was to sign his name and return it. But Eddington added a postscript to the effect that, if he were not deferred on the stated grounds, he would claim it on conscientious objection any way." Since policy was to send objectors to camps, Eddington's postscript had spoiled the arrangements. Dyson intervened with the Home Office and again won Eddington's deferment on scientific grounds, but apparently only by promising that Eddington would undertake an arduous scientific task. "Eddington was deferred with the express stipulation that if the war should end by 1919, then he should lead one of the two expeditions that were being planned for the purpose of verifying Einstein's prediction with regard to the gravitational deflection of light."⁴⁶

With the war's end in 1918, instrument makers once again became free to work on scientific projects. Materials for the expeditions were gathered with great haste, and, in March of 1919, they set sail. A. Crommelin and C. Davidson went to Sobral, about fifty miles inland from the coast of Brazil, and Eddington and E. Cottingham went to Principe, an island off the coast of West Africa. The intrinsic drama and adventure of these expeditions, coming on the heels of the Great War, Eddington's skillful and influential advocacy of the new theory, a perhaps vague general understanding that Einstein's new concepts were revolutionary, the sense of belligerency turning to peaceful collaboration, and, even, Campbell's appearance at the Royal Astronomical Society while the expeditions were still out, combined to create considerable anticipation among British physicists and astronomers over the expeditions' results.

46. Chandrasekhar (ref. 3), 18.

The expeditions and their results⁴⁷

The Sobral group took with them two instruments, one an astrographic telescope of 3.43 meters focal length from the Greenwich observatory, the other a telescope of 19 feet focal length and 4-inch aperture borrowed from the Royal Irish Academy. The astrographic instrument used photographic plates 16"x16" while the 4-inch lens used smaller plates 10"x8". Because telescope mountings could not be carried to the stations, the expeditions brought for each instrument a coelostat—essentially a mirror driven by a falling weight—to reflect the starlight into the telescopes. The 16-inch coelostat for the astrographic telescope was troublesome. Photographs taken before the eclipse showed that the mirror had an astigmatism, and to avoid distortion an 8-inch stop was introduced. It was also found that the drive on the large coelostat did not run evenly; to avoid blurring images during the eclipse it was decided to take very brief exposures with the instrument. In the event, alternative 5-second and 10-second exposures were taken. Both instruments were focused by aiming at Arcturus, and check plates were taken of the field about it. The morning of the eclipse was cloudy, but the cloud cover cleared before totality, and the observers obtained many photographs with each instrument, 19 plates with the astrographic telescope and 8 with the 4-inch instrument. All of the latter had an exposure time of 28 seconds. Most of the astrographic plates showed 12 stars, and 7 stars showed on all but three. The smaller field of the 4-inch lens showed only seven stars, except for one of the eight plates, which, because of cloud, showed no stars at all. Davidson and Crommelin left Sobral on June 7, returning to their station a month later to take comparison plates of the eclipse field. They took many exposures over several nights with each instrument. They left Sobral towards the end of July and returned to Greenwich on the 25th of August.

Eddington and Cottingham at Principe had a single instrument, an astrographic telescope from Oxford, in dimensions quite like the Cambridge astrographic used at Sobral. As at Sobral, check plates were taken of the field of Arcturus. The day of the eclipse was cloudy, and the observers took their photographs through cloud, in the hope that at least some of the images would be usable. They obtained 16 plates, most showing no star images; only two plates, each showing but 5 stars, were usable. Eddington and Cottingham

47. The following account is taken from F. W. Dyson, A. S. Eddington, and C. Davidson, "A determination of the deflection of light by the sun's gravitational field, from observations made at the total eclipse of May 29, 1919," Royal Society, *Philosophical transactions*, 220 (1920), 291-333.

were not able to take comparison plates at Principe because of transportation difficulties and they therefore arranged to have comparison plates taken at Oxford, and also check plates of the Arcturus field.

Of the plates taken with the three instruments, those obtained from the 4-inch lens at Sobral were unequivocally the best, although the focus of the instrument was not perfect. The deflection was calculated from superposition of the eclipse plates and a scale plate and of the comparison plates in the manner earlier described. From the seven photographs of seven stars, Crommelin and Davidson calculated the deflection at the limb of the sun to be 1.98" with a probable error of only 0.12". The astrographic plates, although showing more stars, had much poorer images. Crommelin and Davidson wrote:⁴⁸

The images were diffused and apparently out of focus, although on the night of May 27 the focus was good. Worse still, this change was temporary, for without any change in the adjustments, the instrument had returned to focus when the comparison plates were taken in July.

These changes must be attributed to the effect of the sun's heat on the mirror, but it is difficult to say whether this caused a real change of scale in the resulting photographs or merely blurred the images.

Crommelin and Davidson calculated in the same way as before the deflection from 18 of the astrographic eclipse plates and obtained a mean deflection of 0.86" at the limb of the sun, almost exactly the Newtonian value. They did not report a probable error.

The blurred and dumbbell-shaped images of the two usable plates from the Oxford astrographic at Principe were worst of all. Eddington's procedure for reducing these measurements differed from that used by the Sobral group. Instead of superimposing the comparison and eclipse plates, measuring the displacement, and then calculating the scale factor, translation, rotation and deflection coefficients from the measurements by least squares, Eddington got the scale factor indirectly from check plates. Five pairs of check plates, one plate of each pair from Oxford and one from Principe, were superimposed, the displacement measured and the difference of scale between Oxford and Principe determined by least squares. (No part of the displacement of the images in the check plates could be due to the gravitational deflection of light.) With the scale factor thus determined extraneously, Eddington took the measurements of the displacement of the star images on the eclipse plates and determined, for each pairing of eclipse plate and comparison plate, the translational and rotational displacement by *assuming* that the gravita-

48. *Ibid.*, 309.

tional deflection was half way between the "Newtonian" value and the value predicted by general relativity. With the translation and rotation components of the displacement thus determined, Eddington calculated the value of the gravitational deflection. Finding that it came much closer to Einstein's full deflection than to the Newtonian half-deflection, he calculated the rotational displacement of the images assuming the full Einstein deflection, and from this second-order estimate calculated a final value for the gravitational deflection. It is possible that in assuming the full Einstein deflection in calculating the rotational displacement between the eclipse and comparison plates Eddington reached a final estimate of the gravitational deflection slightly different from what a straightforward least-squares determination of translation, rotation and gravitational deflection would have given. He obtained a mean value of 1.61 with a probable error, including contributions from the error in scale determination from the check plates, of 0.30" of arc.

The error estimates given in terms of probable errors, a statistic no longer in use, can readily be converted to standard deviations, assuming normal distributions. No error estimate was given for the Sobral astrographic, but a standard deviation can be calculated from the value of the deflection obtained from each of its plates. The standard deviations in seconds of arc are approximately: Principe astrographic, 0.444"; Sobral astrographic, 0.48"; Sobral 4-inch, 0.178".

The dispersion of the measurements from the Principe astrographic is about the same as the dispersion from the Sobral astrographic. The latter's plates are slightly better than the former's and many more stars appear upon them. The Principe determination used check plates, the Sobral astrographic determination did not. In all, these sets of measurement seem of about equal weight, and it is hard to see decisive grounds for dismissing one set but not the other. The Sobral 4-inch results were much more impressive, both in quality of image and dispersion of measurement. But the very quality of the Sobral 4-inch results prevent them from constituting an unequivocal confirmation of Einstein's predicted deflection of 1.74": the mean value is too high and the dispersion too small. The mean of the 4-inch measurements is about 1.3 standard deviations from the Einstein value: if Einstein's value is the true one and the errors are random, about one chance in ten existed of obtaining a mean value as high as that given by the measurements. Put another way, the evidence that the Sobral 1.98" mean with 0.178" standard deviation provides for the *Einstein value* of 1.74" is not enormously better than the evidence that the Principe astrographic 1.61" mean and 0.444"

standard deviation provides for the *Newtonian value* of 0.87". The 1.98" mean is about 1.3 standard deviations away from 1.74" ; the 1.61" mean is about 1.7 standard deviations away from 0.87".

The natural conclusion from these results is that gravity definitely affects light, and that the gravitational deflection at the limb of the sun is somewhere between a little below 0.87" and a little above 2.0". If one kept the data from all three instruments, the best estimate of the deflection would have to be somewhere between the Newtonian value and the Einstein value. If one kept only the results of the Sobral 4-inch instrument, the best estimate of the deflection would be 1.98", significantly above even Einstein's value. The conclusion that the Astronomer Royal announced to the extraordinary joint meeting at the astronomical and royal societies on November 6, 1919, was stronger: Einstein's prediction, Dyson announced, had been confirmed.

3. THE ARGUMENTS FOR EINSTEIN

From telegrams and letters he received, Dyson kept the members of the Royal Astronomical Society informed of the progress of the expeditions. On July 11, 1919, when Campbell gave his report of the 1918 Lick expedition, Dyson said that Eddington was "hoping to get good enough measures to determine the displacement definitely, but he obviously is greatly disappointed."⁴⁹ At the September meeting of the British Association Eddington himself presented a preliminary report. He stressed the difficulty in getting good plates, and the paucity of stars discernible on the Principe plates. He deferred to the Sobral expedition: "The expedition to Brazil for the same purpose, undertaken by the Greenwich observers, may be described as completely successful, though they had some clouds also. The plates obtained should settle the question."⁵⁰

Sir Joseph Thomson, President of the Royal Society, was in the Chair at the joint meeting on November 6, 1919. He called upon the Astronomer Royal, who gave the Sobral results as follows:⁵¹

The astrographic plates gave 0".97 for the displacement at the limb when the scale-value was determined from the plates themselves, and 1".40 when the scale-value was assumed from the check-plates. But the much better plates gave for the displacement at the limb 1".98, Einstein's predicted value being 1".75. Further, for these plates the agreement between individual stars was all that could be expected . . .

49. Ref. 6, 299.

50. Ref. 5, 364.

51. Ref. 17, 391.

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

Dyson spoke as the voice of both expeditions, and it is striking that he described the Principe expedition but did not mention its results. It appears that Dyson had decided to discount the results of both the Sobral and the Principe astrographics. The Sobral 4-inch results remained alone, and they constituted confirmation of Einstein's prediction. Crommelin followed Dyson. He seemed inclined to give some weight to the astrographic results, but, of course, greater weight to the 4-inch plates.

Then Eddington spoke:

Of the 16 plates taken during the five minutes of totality the first 10 showed no stars at all; of the later plates two showed five stars each, from which a result could be obtained. Comparing them with the check-plates secured at Oxford before we went out, we obtained as the final result from the two plates for the value of the displacement at the limb $1".6 \pm 0".3$. The p.e. was determined from the residuals of individual stars. This result supports the figures obtained at Sobral.

Eddington's position was that the Principe results were to be given some weight, and that they supported the Sobral 4-inch results.

In the discussion that followed, the weight of Dyson's and Eddington's authority was evident. Thomson observed: "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 favor of the larger value for the displacement."⁵² Thomson virtually endorsed the general theory. Fowler, the President of the Royal Astronomical Society, spoke in its favor as well, but with reservations because of the negative red-shift results. F. A. Lindemann spoke against the possibility, suggested by Newall, of interpreting the results as due to refraction of coronal matter. Ludwig Silberstein maintained that the eclipse results could not confirm Einstein's *theory* in the face of the negative evidence from red-shift measurements. He emphasized the greater precision of the spectral measurements, and proposed to give them greater significance than the eclipse results:⁵³

The spectral shift required is perhaps 100 times, but certainly not less than 40 or 50 times, the error of modern measurement. The solar spec-

52. *Ibid.*, 394.

53. *Ibid.*, 397.

trum can, even in this country, be observed many times a year, and the matter can thus be decided without our having to wait years or centuries for another equally advantageous eclipse. If the shift remains unproved as at present the whole theory collapses, and the phenomenon just observed by the astronomers remains a fact waiting to be accounted for in a different way.

Eddington's mild and irrelevant reply was that, because of the red-shift difficulty, the eclipse results did not confirm Einstein's theory "with the underlying assumption that [the metric interval] ds is a quantity measurable by clock and scale." What was confirmed, according to Eddington, was "Einstein's law of gravitation," that is, Einstein's equations for the trajectory of light in a gravitational field.⁵⁴ Silberstein could not have been satisfied, for his very point was that if there was no theory on which to base the equations for the trajectory, there could be no warrant for attributing the deflection to the action of gravity. As his reference to English weather may suggest, Silberstein could be irritating. A perhaps apocryphal story told us by Prof. Chandrasekhar relates that after Eddington had given a lecture on general relativity, Silberstein, who regarded himself as an expert on the theory, congratulated Eddington for being one of the three people in the world who really understood it. When Eddington made no reply, Silberstein told him not to pretend modesty. Eddington rejoined: "On the contrary, I was trying to imagine who the third person could be."

The account of the proceedings records no queries about the probable error of the Sobral 4-inch determination, or about the warrant for giving some weight to the Principe results but none to those of the Sobral astrographic. The issue was posed directly by Campbell in 1923, who wrote with typical American understatement:⁵⁵

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 so 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.

Eddington had addressed the question in his part of the official expedition report. He argued that the Sobral astrographic results were largely vitiated by *systematic* error. He admitted that the Sobral images were "probably superior to the Principe images" and that the

54. *Ibid.*, 398. Eddington later changed his opinion on this matter; *Space, time, and gravitation* (Cambridge, 1923), 127-128.
55. Campbell (ref. 22), 19.

many more Sobral plates showed many more stars; but he argued that the number of plates and images did not recommend the Sobral images because they appeared to suffer from systematic rather than random error.⁵⁶ Further, he argued, the use of check plates to determine the scale for the Principe plates, and the presence on the eclipse plates of the stars most crucial in calculating a deflection, gave the Principe results greater weight than the meager number of stars might suggest.

Campbell must have read this defense before writing his criticism, and must have been unconvinced. Eddington's use of the check field was an important point, for fixing both the deflection coefficient and the scale factor from the eclipse plates would certainly have lowered the weight of each resulting determination of deflection. Although check plates had been taken at Sobral, they could not be used to determine the scale factor in comparing eclipse and comparison plates because the focus of the astrographic instrument had changed between the exposure of the check plates and the eclipse. Still, whatever systematic error there was in the Sobral determinations must have manifested itself by an effective change of focus. Since the change of focus with the Sobral astrographic resulted in images less blurred than those from the Oxford astrographic used at Principe it was hard to see the ground for supposing that systematic error made the Sobral determination less reliable than the Principe value. Eddington won the argument by the power of the reference work. His books were for some years the most authoritative and widely read of the English language monographs on the theory. In them, beginning with the preface to the second edition of the *Report on the relativity theory of gravitation* (1920), Eddington gave as the results of the expeditions 1.98" from Sobral and 1.61" from Principe. He never mentioned the Sobral astrographic results. It is not surprising that the great majority of English-speaking physicists who cared for the subject believed that the results had been unequivocally in favor of Einstein's prediction.

In almost every discussion of the eclipse program that the expedition members and the Astronomer Royal published before the expeditions set out, in the presentation to the joint meeting, in the published report of the expeditions, and in relevant subsequent publications, it was maintained that only three results were possible: light suffers no deflection, or the Newtonian half-deflection, or the full Einstein deflection. The trichotomy of possible outcomes dominated British discussions of the eclipse, and gave the expeditions

56. Ref. 48, 329.

heightened drama and the status of a crucial test. It also made it more likely that the outcome would be an endorsement of Einstein's prediction. And in fact if the only possibilities allowed were these three, the results had to be viewed as confirmation of Einstein's prediction. But if values both higher than Einstein's and between Einstein's and Newton's were admitted, then the evidence for Einstein's prediction would have been greatly weakened. Dyson and Eddington did not let that happen.

Neither Dyson nor Eddington ever argued in print for the trichotomy of possibilities they repeatedly presented. They let the presentations carry the weight of persuasion. The trichotomy perhaps seemed plausible to a British audience because the habit of toying with alternatives to Newton's law of gravitation did not have anything like so firm a hold there as on the continent. Not only was there a continental tradition of alternative gravitational hypotheses, taken from electrodynamic analogues and extending well back into the 19th century, but also a continental literature on relativistic gravitational theories and on gravitational theories founded on the electrical theory of matter. English writers contributed relatively little to this body of work. In Britain, even one alternative to Newton's theory was almost too many. Both Dyson and Eddington recognized that the deflection could be different from any of the three alternatives they entertained, but they seem to have dismissed the possibility as unworthy of serious consideration.⁵⁷

As the problem then presented itself to us, there were three possibilities. There might be no deflection at all; that is to say, light might not be subject to gravitation. There might be a "half-deflection", signifying that light was subject to gravitation, as Newton had suggested, and obeyed the simple Newtonian law. Or there might be a "full deflection", confirming Einstein's instead of Newton's law. I remember Dyson explaining all this to my companion Cottingham, who gathered the main idea that the bigger the result the more exciting it would be. "What will it mean if we get double the deflection?" "Then," said Dyson, "Eddington will go mad, and you will have to come home alone".

Alternative hypotheses that would fit the data better than Einstein's were readily produced after the fact by physicists of no great distinction. G. Bertrand, for example, showed that a modification of Riemann's law containing a single arbitrary parameter would fit the orbit of Mercury, and, assuming the anomalous

57. Eddington, "Forty years of astronomy," in J. Needham and W. Pagel, ed., *Background to modern science* (Cambridge, 1940), 117-142.

advance to be 38" of arc per century, also give a gravitational deflection of 2.0". Any number of suggestions were made attributing the deflection either to refraction by matter in the region of the sun or to "ether condensation," but none of these artificial alternatives attracted a following.⁵⁸

The English scientific discussions of the eclipse results largely turned on side issues and red herrings. It was objected that the displacement of the star images was due to refraction in the Earth's atmosphere, caused by the atmospheric temperature differences the eclipse shadow produced.⁵⁹ The objection was refuted by Arthur Schuster and others.⁶⁰ Silberstein promoted the idea that the displacements were due to the fixing process used on the photographic plates, but the suggestion was easily disproved. It was objected that the Sobral 4-inch results were not purely radial as the theory required. Henry Russell, at Princeton, argued persuasively that this effect probably arose from a slight curvature in the mirror of the coelostat.⁶¹

4. AFTERMATH

The war years had seen widespread bitterness towards German scientists on the part of their colleagues in Allied countries. In 1914 a manifesto signed by ninety-three distinguished German scholars, artists, and scientists was published defending German war policy and action. Twenty of the signatories were prominent scientists, including Haber, Haeckel, Nernst, Ostwald, Planck, Roentgen, and Wien. English and French scientists were shocked and angered by the manifesto, and replied with calumnies proving the moral and substantive inferiority of German science.⁶² Many scientists in Allied

58. G. Bertrand, "La loi de Riemann, le périhélie de Mercure et la déviation de la lumière," *Académie des Sciences, Paris, Comptes-rendus*, 174 (1922), 1687-1689; L. Silberstein, "The recent eclipse results and the Stokes-Planck ether," *Philosophical magazine*, 39 (1920), 161-170.

59. A. Anderson, "The displacement of light rays passing near the sun," *Nature*, 104 (1919), 354. Eddington suggested that Anderson's idea might help account for the "rather high value found at Sobral," and so admitted that the Sobral value was indeed high; Eddington, "The deflection of light during a solar eclipse," *Nature*, 104 (1919), 372.

60. Schuster, "The influence of small changes of temperature on atmospheric refraction," *Physical Society of London, Proceedings*, 32 (1920), 135-140.

61. Silberstein, "The eclipse results and the contraction of photographic images," *RAS, MN*, 80 (1920), 630-631; F. Slocum, "Photographic distortion on eclipse plates and the Einstein effect," *Popular astronomy*, 29 (1921), 273-274; Russell, "Note on the Sobral eclipse photographs," *RAS, MN*, 81 (1920), 154-164.

62. D. Harris, "The Germans and scientific discovery," *Nature*, 98 (1916), 168-169; G. Petit and M. Leudet, ed., *Les allemands et la science* (Paris, 1916). Cf. B. Schroeder-Gudchus, *Les scientifiques et la paix* (Montreal, 1978).

countries believed that their colleagues in Germany and Austria had made science the willing tool of a brutal militarism; in consequence, the sentiment for international science that had prevailed before the war was replaced by the conviction that scientists of the Central Powers must be excluded from formal and even personal relations with scientific workers in Allied nations. The pre-war International Association of Academies, whose purpose had been to coordinate the activities of international learning, and whose center at the outbreak of the war had been the Berlin Academy, was a victim of the hostilities. In its place arose in October of 1918 the International Research Council, whose executive committee stood adamant against relations with German and Austrian scientists. When W. W. Campbell stopped in England to attend a meeting of the Royal Astronomical Society and to report on the American eclipse expedition of 1918, he was on his way to Brussels to represent the United States at a meeting of the International Research Council. Because of the efforts of the Council and related causes, German scientists were excluded from nearly three fourths of the scholarly meetings held between the end of the war and 1925 in countries that had not been among the Central Powers.⁶³

Einstein and Eddington stood apart from this subordination of science to nationalist prejudice. Einstein had not signed the Manifesto of German Professors and Men of Science, and indeed had signed a letter opposing it. For his part, while Eddington may have shared the British belief in the wrongness of the German cause, he would not subordinate human or scientific perceptions to nationalist ones. On Schwarzschild's death, Eddington wrote a moving tribute to a man who was both a scientific luminary and an enemy soldier.⁶⁴

The war exacts its heavy toll of human life, and science is not spared. On our side we have not forgotten the loss of the physicist Moseley, at the threshold of a great career; now, from the enemy, comes news of the death of Schwarzschild in the prime of his powers. His end is a sad story of long suffering from a terrible illness contracted in the field, borne with great courage and patience. The world loses an astronomer of exceptional genius, who was one of the leaders in recent advances both in observational methods and theoretical researches.

It would be paying an ill-service to the memory of one who gave the final proof of devotion to his country, to seek by these traits to dissociate him from the rest of his nation. We would rather say that through him a new spirit was arising in German astronomy from within, raising, broadening and humanizing its outlook.

63. Schroeder-Gudehus (ref. 64), chapter 4; D. J. Kevles, "Into hostile political camps": The reorganization of international science in World War I, *Isis*, 62 (1970), 47-60.

64. Eddington, "Karl Schwarzschild," *Observatory*, 39 (1916), 336-339.

This is not the tone that most British men of science would have struck. Eddington reasserted his views more broadly when the Oxford correspondent of *The observatory* wrote suggesting that future relations with German science should be few and far between. Eddington reminded his readers that five of the most distinguished of the signatories of the manifesto had publicly expressed regret at its wording, and he went on:⁶⁵

It is not any personal attitude of the German scientists that presents a difficulty, but the feeling that they are involved in the general condemnation of their nation. But the indictment of a nation takes an entirely different aspect when applied to the individuals composing it. Fortunately, most of us know fairly intimately some of the men with whom, it is suggested, we can no longer associate. Think, not of a symbolic German, but of your former friend, Prof. X., for instance—call him Hun, pirate, baby-killer, and try to work up a little fury. The attempt breaks down ludicrously. No doubt, he is a most ardent supporter of his fatherland, passionately convinced of the righteousness of its cause. Call this wrong-headed, if you will, but surely not morally debased. Far be it from me to deny his individual responsibility for his country's share in the evil that has befallen. The worship of force, love of empire, a narrow patriotism, and the perversion of science have brought the world to disaster. But how can we expect him to look at these things with our eyes—to see his country as we see it; with us even the attempt to view the conduct of our government from the German's standpoint is discouraged as harmful to the state.

This last sentence brought a harsh response from Sir Joseph Larmor.⁶⁶ Evidently Eddington was not unwilling to provoke the powers of British science in his fight against British self-righteousness and nationalist, anti-German scientific perspective.

For Eddington, one of the chief benefits to be derived from the eclipse results was a rapprochement between German and British scientists and an end to talk of boycotting German science. It may be that Dyson, too, hoped that the expeditions would promote this end. In November Einstein wrote to Eddington from Holland, expressing his pleasure at the expeditions' results and his gratitude for Eddington's efforts. Eddington replied: "It is the best possible thing that could have happened between England and Germany. I do not anticipate rapid progress towards official reunion, but there is a big advance towards a more reasonable frame of mind among scientific men, and that is even more important than the renewal of formal associations."⁶⁷ Fowler expressed a similar sentiment in closing the December meeting of the Royal Astronomical Society: "We may take

65. Eddington, "The future of international science," *ibid.*, 271-272.

66. Larmor, "The future of international science," *ibid.*, 313-314.

67. Letter of 1 Dec 1919 (EP).

a reasonable pride in the contribution which our Society has made to the development of this subject through its representatives on the Eclipse Committee, and we may hope that there will be general satisfaction in the knowledge that our national prejudice did not prevent us from doing anything that we could to forward the progress of science." The Society was not, however, prepared to accept Eddington's proposal to award its gold medal to Einstein.⁶⁸

In later years, Eddington emphasized the value of the eclipse in subduing scientific jingoism. In his obituary notice for Dyson he wrote:⁶⁹

The announcement of the results aroused immense public interest, and the theory of relativity which had been for some years the preserve of a few specialists suddenly leapt into fame. Moreover, it was not without international significance, for it opportunely put an end to wild talk of boycotting German science. By standing foremost in testing, and ultimately verifying, the "enemy" theory, our national Observatory kept alive the finest traditions of science; and the lesson is perhaps still needed in the world today.

In truth, while some aspects of Eddington's handling of the deflection of light were in the finest tradition of science, others were not. As he confessed in *Space, time and gravitation*, he was "not altogether unbiased."⁷⁰ The bias showed in his treatment of the evidence: he repeatedly posed a false trichotomy for the deflection results, claimed the superiority of the qualitatively inferior Principe data, and suppressed reference to the negative Sobral results. (His discussion of the red-shift was sometimes no better; in *Space, time and gravitation*, for example, he held that observed shifts different from those Einstein predicted did not tell against the general theory as long as they were not absolutely nil.) Dyson was more judicious; he refrained from adducing the Principe results as evidence for general relativity and called for measurements of the deflection at the next two eclipses. But Dyson, too, joined in advocating the trichotomy and certainly did not emphasize the discrepancy between the observations and Einstein's prediction.

Eddington was committed to the theory before the expeditions were proposed; as he put it to Chandrasekhar, if the matter had been left to him, he "would not have planned the [eclipse] expeditions since he was fully convinced of the truth of the general theory of relativity." Dyson, on the other hand, did not expect the theory to be

68. RAS, *MN*, 80 (1919), 118; Eddington to Einstein, 21 Jan 1920 (EP).

69. Eddington, "Sir Frank Dyson, 1868-1939," *Royal Society, Obituary notices of Fellows*, 3 (1940), 159-172, on 167.

70. Ref. 54, 113.

confirmed; it was "too good to be true."⁷¹ 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.

There were other eclipse expeditions after 1919, and they obtained a variety of results, most of them a bit higher than the deflection required by the general theory. It mattered very little. The reputation of the general theory of relativity, established by the British eclipse expeditions, was not to be undone. Its force was sufficient to overwhelm objections to the theory founded on the negative solar red-shift results. There had always been a few spectral lines that could be regarded as shifted as much as Einstein required; all that was necessary to establish the red-shift prediction was a willingness to throw out most of the evidence and the ingenuity to contrive arguments that would justify doing so. The eclipse results gave solar spectroscopists the will. Before 1919 no one claimed to have obtained spectral shifts of the required size; but within a year of the announcement of the eclipse results several researchers reported finding the Einstein effect. The red-shift was confirmed because reputable people agreed to throw out a good part of the observations. They did so in part because they believed the theory; and they believed the theory, again at least in part, because they believed that the British eclipse expeditions had confirmed it. Now the eclipse expeditions confirmed the theory only if part of the observations were thrown out and the discrepancies in the remainder ignored; Dyson and Eddington, who presented the results to the scientific world, threw out a good part of the data and ignored the discrepancies.

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.

71. Ref. 3, 18; Eddington (ref. 69), 167.