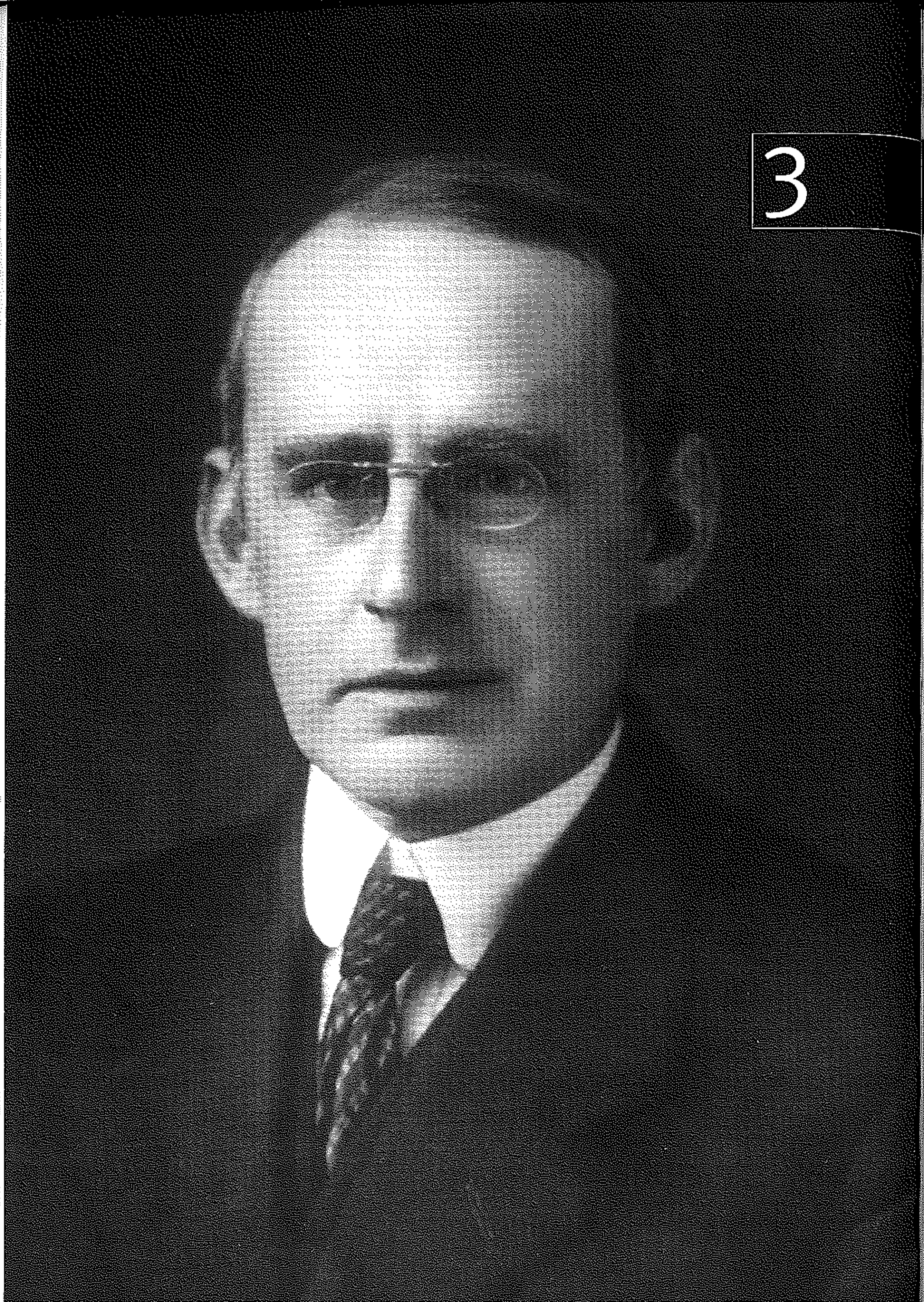

3

The eclipse of Isaac Newton

Arthur Eddington's 'proof' of general relativity

> In 1919 [Eddington] led an expedition to Principe Island (West Africa) that provided the first confirmation of Einstein's theory that gravity will bend the path of light when it passes near a massive star. During the total eclipse of the sun, it was found that the positions of stars seen just beyond the eclipsed solar disk were, as the general theory of relativity had predicted, slightly displaced away from the centre of the solar disk.
>
> 'Arthur Eddington', *Encyclopaedia Britannica* (1992).

> The expeditions despatched to Brazil and the island of Principe on the occasion of the total eclipse of the Sun on 29th May, 1919 found that the effect which had been predicted by Einstein did in fact exist. Quantitatively, too, the agreement is a good one.
>
> W. Pauli, *Theory of Relativity* (1958).

Imagine that having used an exceptionally powerful telescope to determine very accurately the distances between the stars of a constellation, you repeat the process on another night. On the second occasion it happens that on its way to you, light from one of the stars is passing very close to an intervening star or black hole. If unaware of the effect large heavenly bodies have on light, you would find to your surprise that this particular star has shifted in relation to its companions in the constellation. If repeated a third time when, as in the first case, light from all the stars in the constellation passes nowhere near any stars or black holes, the seemingly errant star would be back where it started. Such apparent movements of fixed stars presents a puzzle, but it is not the stars that create it. The real cause is the capacity of gravitational fields to warp space-time and thereby alter the direction in which light beams travel. The degree

Left: Sir Arthur Stanley Eddington (1882–1944).

of distortion depends on the mass generating the gravitational field and how close to it the light beam passes. Neither our brains nor our cameras, however, are configured to take account of such gravitational effects. Instead, when a star beam reaches us after passing very close to a large heavenly body, we instinctively locate the light source by assuming that the light has travelled to us in a straight line. Thus we mislocate the source star.

Unnatural though light bent by gravity may seem, it is not a new idea to science. The possibility that a ray of light is made up of a stream of tiny packages has had supporters for millennia. Once Newton's gravitational theories were accepted, it was recognized that they could have important implications for these hypothesized units of light. On the reasonable assumption that each unit has to have some mass—albeit unimaginably small—then they would be as much affected by gravity as would any other object in the Universe. (To argue otherwise would be to deny the central point that Galileo supposedly demonstrated from atop the Leaning Tower of Pisa.) In 1801, the Bavarian scientist Johann von Soldner calculated just how much deflection one would expect to see. Looked at from a Newtonian perspective, what von Soldner said can be thought of in terms of an imaginary tube through which the beam of light passes on its way to us. Viewed from Earth, this tube can be seen to have three co-ordinates by which to locate the position of any given unit of light at any given stage on its journey: two spatial ones (left/right and up/down) and time. Thus, as our unit of light travels down this imaginary tube, the gravitational pull of any nearby stars or planets can be factored in and the whereabouts of the light unit in space-time calculated with great accuracy.

Or so it seemed until the second decade of the twentieth century. Then Albert Einstein published his ideas on relativity and fundamentally challenged the simplicity of this picture. According to Einstein, it is not the light units that are affected by gravity, but the very time/space co-ordinates hitherto used as absolutes to track their path. Our tube can no longer be imagined as having standard units of space and time throughout its length. Rather, it is as though the reference grid on a map ceases to be an external imposition and becomes, instead, part of the landscape. Like the landscape, it becomes itself subject to the great forces of nature. This is because, Einstein argued, large gravitational fields warp the space-time continuum and, consequently, alter the course of light passing through them.

Even knowing that there is hardly a physicist alive who does not believe in general relativity, these are exceptionally difficult ideas to grasp. But in the first decades of the twentieth century the status of general relativity was that of a clever speculation garlanded by just a few ambiguous observations. Therefore the obstacles that Einstein's supporters faced were immense. Nevertheless, even if Einstein's theory was highly speculative, the history of human thought had rarely encountered such a superbly inventive and reason-defying concept. Before long, physicists on both sides of the fence were racking every available neuron trying to devise methods of testing general relativity against Newtonian mechanics. In 1916, the ante was suddenly raised when Einstein used his theory to calculate that the degree of light distortion caused by general relativity would be roughly twice that predicted by Newtonian physics. To his supporters, this calculation raised the exciting possibility of producing an experimental vindication of Einstein's controversial new theory.

Their opportunity lay in the 1919 solar eclipse. And the challenge they faced was that of measuring a very small effect with sufficient precision to distinguish between Einstein's predictions and the Newtonian alternative. Given the available technology, the Sun was the only body likely to create an effect large enough to be measured from Earth with the necessary accuracy. Usually there was an insuperable difficulty with this. When star beams travel close to the Sun they are completely obscured by its overwhelming luminosity. During a solar eclipse, however, this problem disappears. Because the Moon temporarily obscures the Sun, these star beams briefly enable their source stars to be observed. To take scientific advantage of this, in 1918 two separate British scientific expeditions set out for the tropics. Their plans were to make observations of suitable stars during the eclipse of 29 May 1919 and subsequently to repeat the exercise in the night sky. The expeditions were very well publicized and the scientific community awaited their results at a high pitch of excitement. Towards the end of 1919, a packed meeting of the Royal Society in London finally learned that Albert Einstein's predictions had been fully vindicated. His ascent to scientific pre-eminence was assured and physics would never be the same again.

In the years that followed, it quickly became accepted dogma that these two studies of the eclipse had fully supported general relativity theory. Doubts were occasionally raised, but they were quickly silenced. Now more than 80 years on, '1919' is as symbolic a date for physicists as 1859 (the year of the publication of *On the Origin of Species*) is for biologists and 1776 for constitutional historians. Thus, the British physicist Paul Davies wrote in his 1977 book *Space and Time in the Modern Universe*:

> The bending of light rays by a gravitational field was a central prediction of Einstein's theory, and was triumphantly verified observationally by Sir Arthur Eddington (British 1882–1944) during an eclipse of the sun in 1919, when the bending of starlight by the sun was measured and found to agree with the theoretical value calculated by Einstein.

No doubt the enduring inspirational qualities of these expeditions owe much to their seeming to show the scientific method at its best. First, an innovative theory is developed that challenges an existing paradigm. Second, different predictions based on the same event are derived from the competing theories. Third, exact data are collected and one of the theories justly triumphs over the other. Beyond this, Eddington's story is even more attractive because the 'duel in the sun' he managed to set up refined some very complex physics down to a seemingly simple matter of the degree of deflection. Throw in the exotic locations, the struggle to reach them, and, in counterpoint, the extreme savagery of the First World War, and you have the scientific Odyssey par excellence.

Delve a little deeper, however, and one begins to see that the solar-eclipse expeditions of 1918–19 were no more successful than thousands of lesser experiments—past and present—in satisfying these model criteria. The chief reason that these studies retain their popularity is that Einstein's ideas ultimately triumphed. Looking back on the solar-eclipse expeditions our presentist sensibilities incline us to think that the researchers of 1919 *must* have produced accurate and compelling data. But this, as we have seen in the past two chapters, need not be true at all. Indeed, here again it's clear that the scientists involved were very lucky to be accepted by their posterity as having proved their point. For at the time, as the science historians John Earman and Clark Glymour have shown, the evidence they presented was unquestionably inadequate. This leads on to the further question of why the scientific community embraced with such alacrity an experimental 'proof' that was really nothing of the sort.

Meet the teams

Both 1918–19 eclipse expeditions comprised British physicists. The first team, which observed the eclipse from Sobral in Brazil, was led by A. Crommelin and C. Davidson. The other, headed by Arthur Eddington and his assistant E. Cottingham, made its observations from the island of Principe, which lies off the coast of West Africa. Eddington, born in the English Lake District, was already an eminent Cambridge physicist and it was his interpretation of both teams' data-sets that would serve to vindicate Einstein. For this reason it is noteworthy that even before departing for Principe he was well known for his Einsteinian sympathies. As the most important expositor of general relativity within Britain, most of his colleagues knew that he was undertaking the eclipse expedition in the fervent hope of confirming his radical intuition that Einstein was right.

To understand the difficulties the teams faced we need first to consider the sorts of equipment they used for the task in hand. The Sobral team took with it an 'astrographic telescope' and a 4-inch telescope. Eddington's team took just an astrographic instrument. Their plans, however, were identical. Photograph the star beams close to the edge of the eclipse and then photograph the same stars later in the year in other parts of the sky as a baseline. Crommelin would remain in Brazil to do this, whereas Eddington would return to England and make use of facilities at the University of Oxford.

The teams also took with them the same theoretical predictions. Depending on how great were the displacements found, either Einstein or Newton would be vindicated. They were prepared to endorse Newton if the displacement was in the region of 0.8 second of arc, and Einstein if it was close to 1.7 seconds of arc. This difference is so small that it amounts to measuring less than the width of a penny as seen from over a mile away! This was a tall order indeed. In the event, because there were no stars aligned tightly to the edge of the Sun during the eclipse, they had to settle for ones appreciably further out. As this meant a much weaker gravitational effect, measurement would be proportionately harder. So it is easy to understand why, when the exceptionally accomplished Eddington calculated an arc of displacement close to that predicted by Einstein, he described it as the most 'exciting event I recall in my . . . connection with astronomy'.

The problems

Quite apart from the smallness of the measurements to be made, the technical difficulties facing the two teams were simply immense. The most fundamental problems stemmed from the fact that a comparison was being made between the apparent locations of stars photographed in different parts of the sky in different seasons. Unavoidably, therefore, ambient temperatures were going to differ from one occasion to the other. This is important because the disparity in focal length between a warm and a cold telescope can easily produce a distortion equivalent to that which the experimenters were expecting to observe. A similar effect may be produced by the fact that the solar-eclipse photographs were to be taken during the day and the remaining photographs during the night. Aside from ambient temperature, both studies were also hampered by different degrees of 'atmospheric turbulence'. (This is the distortion to background images, mainly caused by convection currents, that can be seen when looking across the top of a hot barbecue; in tropical locations atmospheric turbulence would have been a very serious problem.) On top of this, both parties faced the unavoidable problem of inclement weather. In the event, clouds were partially to obscure exposures taken by both groups.

Add to these hazards the possible mechanical changes to the telescopes caused by their having to be transported to sites so far from England, when even the slightest damage affecting the angle of the photographic plates would have had disastrous results. Exacerbating this problem, the eclipses had to be observed in remote areas where large state-of-the-art equipment could not be transported. Both teams had to rely on smaller models that required a long exposure time. As such, their telescopes had constantly to be counter-rotated so that the Earth's rotation did not alter the point in the sky at which they were aimed. The mechanisms for rotation that the two teams constructed introduced yet another potential source of error.

Some of these difficulties could be controlled for and taken into account at the calculation stage. This generally involved determining the displacement of stars whose altered position could only have been caused by mechanical changes with the telescopes and photographic equipment. The measure of their displacement could serve as a reliable index to the amount of experimental distortion involved. Once these effects had been quantified, the behaviour of the target star beams could be isolated. But making these adjustments accurately required a minimum of six undisplaced stars in each photographic frame; otherwise there was insufficient data for the statistical procedures to be performed. Additionally, neither team could deny that their experimental method was likely to involve errors that had not been identified and would therefore pass unrecognized.

To give a sense of just how serious these difficulties were, it's worth mentioning that in 1962 a much-better equipped British party tried to reproduce Eddington's findings. At the end of a frustrating attempt to do so they concluded that the method was much too difficult and could not be implemented successfully. In view of the obstacles considered above, this seems far from surprising. The Sri Lankan Nobel laureate, Subrahmanyan Chandrasekhar, with whom Eddington had a long and highly personalized academic dispute, later claimed that science was only one reason for the 1918–19 expeditions. He suggested that Eddington's overall leadership was used to obviate his need either to enlist or declare himself a conscientious objector during the First World War. The implication appears to be that this consideration was allowed to out-weigh the known impracticality of the expeditions' objectives. To date, however, there is no independent verification of Chandrasekhar's claim.

The results stage

On the long-awaited night of the eclipse, the Sobral team managed to obtain 19 plates from their astrographic telescope and 8 plates from their 4-inch telescope. Eddington's Principe team was hampered by cloud cover and took away just 16 plates, but only two of these, each showing only five stars, were actually usable. The Sobral team managed to take the clearest photographs with its 4-inch telescope. These suggested a deflection of star beams grazing the Sun at between 1.86 and 2.1 seconds of arc, averaging out at 1.98 seconds. (Note that Einstein's prediction was 1.7 seconds.) The Sobral team's astrograph shots were of a lower quality, but 18 of them were used to calculate an average of 0.86 seconds. In other words, one set of photographs was close to Einstein's prediction, the other was very close to the Newtonian value of 0.8. Unfortunately the first score was too high to be strictly compatible with general relativity and the score in the second set was based on low-quality exposures. In addition, each set of photographs involved very large standard errors. This should have immediately prompted doubts as to the reliability of the averages themselves.

From the usable Principe plates, Eddington calculated a star-beam displacement of between 1.31 and 1.91 seconds. But even these plates were of embarrassingly poor quality and it has been suggested that the mathematical formula he used to reach these figures was in itself biased. Be this as it may, Eddington's two poor plates gave a mean score of 1.62 seconds, marginally below the Einsteinian prediction.

Self-evidently, with such poor and contradictory evidence, attempting a resolution of the controversy on the basis of these figures was an extremely risky affair. Take just one of the hazards mentioned above: atmospheric turbulence. In the hot environments in which both teams were working it was likely that all but the largest displacements would be cancelled out by this phenomenon. Had the teams been measuring star beams just clipping the Sun's edge, their displacement might have been large enough to eliminate atmospheric turbulence as the sole cause. In 1919, however, with the star beams closest to the Sun obliterated by the corona, those that could be observed were some way from the Sun's rim. Consequently the displacements were so small that the entire effect could quite easily have been caused by atmospheric turbulence alone. At some level, the teams were aware of this. Thus, in discussions after the announcement of the eclipse results, Eddington and his assistants admitted that calculations of small displacements were unreliable. Yet, they refused to let this effect their presentation of the measurements. As we have seen, within a few months Einstein's ideas were being adjudged victorious from the pulpit of the Astronomer Royal.

The interpretation stage

The Sobral and Principe expeditions most certainly did not produce measurements that could definitively confirm either Newtonian or Einsteinian theory. In his book *The Physical Foundations of General Relativity* (1972), the British astronomer Dennis Sciama explained that eclipse observations are notoriously 'hard to assess . . . since other astronomers have derived different results from a re-discussion of the same material'. In this case, there can be no doubt at all that both theories could potentially have been declared victorious, although it may have appeared to the Sobral team that the most likely verdict was a tie. But this is not what happened. Under Eddington's hand, the eclipse results were subjected to extensive cosmetic surgery until they matched Einstein's prediction. Without this treatment Einstein could not have been vindicated in 1919.

Eddington began by casting doubt on the scores obtained by the Sobral team. He claimed that their astrographic results were not randomly distributed around the mean score as one would expect with normal data points. Instead, they were mostly beneath it, suggesting that a 'systematic error' had occurred that had artificially lowered the mean score itself. Without this error, he implied, their results would also have approximated to the higher Einsteinian prediction. This was a reasonable argument. The problem was Eddington's abject inability to show that the same error had not occurred in the other data-sets. When challenged, he produced not a single piece of unambiguous evidence to demonstrate that the measurements he accepted were unaffected by the same error. Even more seriously, Eddington conveniently ignored the fact that the Sobral team's astrographic photographs were visually far superior to his own two hazy plates. There may have been valid concerns about the reliability of Crommelin and Davidson's photographs. But one thing should have been clear: Eddington's were very much worse. As the American commentator W. Campbell wrote in 1923:

> 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.

This was an understatement of which any Briton would have been proud. Note also that Eddington's two plates contained an insufficient number of undisplaced stars from which to make the necessary adjustments for error (five rather than six). Then factor in the large standard deviation in the results that he accepted (rendering most of the results either too high or too low), and one can understand why Earman and Glymour concluded in their 1980 article, 'the eclipse expeditions confirmed the theory [of Einstein] only if part of the observations were thrown out and the discrepancies in the remainder ignored'. In short, they didn't.

A core principle of the standard model of the scientific method is that theoretical predictions should not be allowed to influence which results are used and which are discarded. In Eddington's approach, however, as with Louis Pasteur and Robert Millikan, predictions and data interpretation became mutually confirming. Eddington evaluated his results according to how they conformed to his preferred theoretical predictions. On one hand, inordinate value was attached to photographs that approximated Einstein's 1.7 seconds of arc deflection; on the other, dubious ad hoc reasons were invented for jettisoning any that disagreed. 'Einstein's prediction had not been verified as decisively as was once believed', Sciama gently pointed out in 1972. Reflecting on eclipse expeditions in general, he added, 'one might suspect that if the observers did not know what value they were "supposed" to obtain, their published results might vary over a greater range than they actually do'. Or, as the Polish-American physicist Ludwik Silberstein said at a meeting of the Royal Astronomical Society in 1919, 'If we had not the prejudice of Einstein's theory we should not say that the figures strongly indicated a radial law of displacement'. So serious were Eddington's manipulations that one strongly suspects that had the predictions of the rival theories been the reverse—Newton high, Einstein low—Eddington would have discarded his own photographs as too hazy and accepted with alacrity the Sobral party's astrographic pictures.

Most of Eddington's contemporaries were either less incisive or less cynical than Silberstein and Sciama. As a result, after careful massaging, Eddington's judiciously selected data-set could be presented as unequivocally supporting his candidate's theoretical predictions. Having discarded a full 18 plates on very specious grounds, he set about writing the official accounts of the expeditions. In these he routinely referred to only two sets of prints: the four 4-inch telescope photographs obtained by the Sobral team and his own very poor two photographs. As these images gave mean scores of 1.98 and 1.671 respectively, few scientific readers could avoid concluding that Newton had been decisively beaten: the reigning champion for over 200 years had fallen at last.

Once the eighteen astrographic plates had been rejected and forgotten, concerns about the quality of the Principe photographs quickly evaporated. The complexities of the issue receded from view, and the controversy between Einstein and the Newtonians suddenly—but falsely—appeared to be a one-horse race. This is clear from the account of the eclipse expeditions in James A. Coleman's best-selling *Relativity for the Layman* (1969):

> The Sobral group found that their stars had moved an average of 1.98 seconds of arc, and the Principe group's had moved 1.6 seconds of arc. This nearness to the 1.74 seconds of arc predicted by Einstein was sufficient to verify the effect.

But in many cases unwittingly, Coleman and dozens of other scientific commentators skate over the fact that among astronomers Eddington's account did not win immediate assent. Already, in 1918, an American expedition had travelled to Washington state to observe an eclipse. They had reported that the 1.7-second light deflection was 'non-existent'. Ten further eclipse observations were made between 1922 and 1952. Only one of these produced seemingly high-quality data, and that suggested a displacement arc of 2.24 seconds—substantially higher than predicted by Einstein. In fact, virtually every eclipse observation was either unreliable or, in most cases, both unreliable and higher than the Eddington scores. In light of these results, many of those at the cutting-edge of research into general relativity sensibly deferred judgement for rather longer than the accepted view implies. Some embraced general relativity only when evidence of an entirely different type became available.

Status and trust

In overwhelming his critics, Eddington used the Royal Society of London to great effect. This body was set up in the late seventeenth century amidst a nation recoiling from a regicide and years of civil war. Against such a background, the peaceful and mannerly resolution of controversies was given a very high priority. Scientists were no exception. The focal point of the Royal Society was a large lecture theatre in which the cream of the scientific establishment could gather to watch experiments being performed. The idea was that members would reserve judgement on any given topic until the relevant experiments had been carried out in front of them. Then, having personally seen the unvarnished facts, the scientific community could democratically arrive at a consensus and thereby avoid protracted conflict. In many ways, the same principles are alive and well today. Journal articles require the inclusion of detailed methodologies that should allow experiments to be repeated in other laboratories. Witnessing and consensus-forming no longer take place within one location on one occasion, but they can nevertheless be achieved.

But there have always been problems with achieving agreement on what an experiment does or does not prove. Today, science takes place on such a vast scale that it is not always convenient to replicate every important experiment performed. Further, as the British sociologist Harry Collins and his American collaborator Trevor Pinch have shown, some experiments require specialized training, highly recondite knowledge, and technical expertise that may take months or years for another laboratory to acquire. This means that scientists sometimes just have to take their colleagues' word for it. In the case of the Sobral and Principe expeditions, quite apart from the tremendous difficulty in understanding general relativity and performing the appropriate calculations, the experiments themselves were exceptionally difficult to perform, extremely expensive, and totally reliant on eclipses of the Sun. Thus, few astronomers were inclined to try to replicate Eddington's results. In these circumstances, most astronomers were more than happy to accept his interpretations without demur. Whatever else it may be, this case is a powerful demonstration of the role of trust in the advancement of science.

Yet, however high Eddington's personal reputation stood in 1919, there were still major challenges facing him. Success required that the scientific community sin by omission by colluding, first, with his suppression of well over two-thirds of the photographs from the Sobral and

Principe expeditions, and, second, with his ignoring the much more equivocal evidence advanced by other eclipse expeditions.

In understanding why the scientific rank and file placed so much confidence in Eddington it has first to be appreciated that, to many, Einstein was already the greatest modern physicist. In addition, Eddington was not only an extremely accomplished astronomer in his own right, but he was British at a time when this counted for a great deal. Taking full advantage of his esteemed status, Eddington had the clout to secure the ascendancy of his own interpretation by enshrining it within a series of seminal papers and books that he himself authored. By 1919, Eddington had also acquired enormous credibility because he was such a fine expositor of general relativity. He grasped its implications with a flair that could not but inspire confidence. Such was his standing in this new scientific area that the following apocryphal story had wide currency. Eddington's fellow physicist Ludwig Silberstein remarks, 'Professor Eddington, you must be one of three persons in the world who understands general relativity'. After a longish pause, he continues, 'Don't be modest Eddington', to which the latter replies, 'On the contrary, I am trying to think who the third person is!' The story is entirely mythical, but it is as illuminating as it is amusing.

Arthur Eddington's apparent vindication of Einstein's ideas also gained rapid credence because of the status of many of its earliest converts. On 6 November 1919, Sir Joseph J. Thomson, the President of the Royal Society, announced to the assembled ranks of the scientific elite, '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 Professor Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value for the displacement.' With the weight of the President of the Royal Society and the Astronomer Royal on his side, Eddington could hardly have been surprised to read the following banner headlines in *The Times* the following morning:

Revolution in Science
New Theory of the Universe
Newtonian Ideas Overthrown

'It was generally accepted', *The Times* report went on, 'that the observations [of the eclipse] were decisive in the verifying of the prediction of the famous physicist Einstein.' Over the next few weeks *The Times* carried several letters from respected scientists in support of relativity and even one from Einstein himself on the 28 November. The contributions of detractors, in contrast, were invariably scorned. Indeed, if we return once more to J. J. Thomson's announcement, we see that he was as determined to browbeat the scientific community as was *The Times* the general reader. His concluding remarks included the observation that, 'It is difficult for the audience to weigh fully the meaning of the figures that have been put before us'. It seems not unreasonable to paraphrase this as 'It's beyond your competence to judge in this matter so take our word for it'. Thus, if anybody present had challenged Eddington's conclusions, the challenger would have been up against more than the weight of the evidence. With the three-line whip imposed by a seemingly holy alliance of the Astronomer Royal, the President of the Royal Society, and Eddington himself, none saw serious merit in disagreeing.

Once Thomson's decree had been issued, the scientific community accepted the party line virtually en masse. And for the most part they did so despite lacking a proper understanding of the expeditionary data. Clearly, then, in this case much of the scientific community was prepared to endorse interpretations without being able to justify their decision on empirical grounds. Furthermore, most scientists subsequently stood by this position irrespective of the later publication of eclipse data that did not corroborate Eddington's figures. It is extraordinary how little these later critics managed to influence the debate after 1919. Cutting-edge researchers were the only scientists prepared to dispute the Eddington figures, but even though their results were published they did not have the strength to overturn the interpretations of 6 November 1919. After that date, they were battling against what can fairly be called a cultural consensus. Quite rationally, where non-astronomers reached the limits of their knowledge of astronomical science, they followed their instincts and backed their most accomplished and highly regarded colleagues. At least in the short run, what is perceived to be scientific truth is usually to be found on the side of the big battalions.

In matters of gravity, weight counts

The standard story of the eclipse expeditions carries all the hallmarks of presentist history of science. There is a crucial experiment that vindicates a novel and brilliant theory; one man whose foresight and determination permits it to become established fact; and it has the added spice of the experiments being formed in the exotic jungles of Principe and Brazil. (Only a cabal of jealous rivals and an obdurate Church are wanting to make it a classic.) Looked at in the light of modern knowledge, it is so hard to suspend awareness of 'what happened next' that we tend to assume that the results presented in November 1919 amounted to the very best of cutting-edge science. Yet what has now been revealed by historians shows just how lucky Eddington was. Had he not been later vindicated on the basis of much better results, his posthumous reputation would have been severely tarnished and the eclipse expeditions would long since have ceased to inspire undergraduate physicists.

This analysis of the 1919 experiments shows that Eddington fell far short of the canonical rules of the scientific method. More interestingly, it also reveals that there is an inexact correspondence between how closely these procedures are followed and the persuasiveness of the theories that emerge. In 1919, general relativity won the debate because it had the best public relations available. But this was not a new phenomenon. Indeed, there is a certain poetic justice in Sir Isaac Newton having been eclipsed in this way. After all, several recent biographies have shown that it was partly Newton's power-play tactics as President of the Royal Society that managed to win unusually rapid assent for his own ideas two centuries earlier.