William Wallace Campbell and the "Einstein Problem": An Observational Astronomer Confronts the Theory of Relativity

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# William Wallace Campbell and the "Einstein Problem": An observational astronomer confronts the theory of relativity

THERE ARE THREE classical tests of general relativity, all astronomical in character: an excess advance of the perihelion of mercury of about 43 seconds of arc per century beyond what Newtonian theory predicts; a shift toward the red of spectral lines emitted by large gravitating bodies; and the bending of the path of light in a gravitational field, observable as an outward displacement of the stars in the vicinity of the sun. It was the British verification of the last of these predictions that launched Einstein and his theory to international fame immediately after the first world war. Ever since, astronomers' participation in the reception of general relativity has been viewed almost exclusively as an effort to prove Einstein right.

The dramatic nature of the British eclipse results and the ensuing burst of publicity have captured the attention of the writers and historians interested in Einstein and relativity. All the major recent Einstein biographies focus on the British eclipse expeditions and ignore or pass quickly over other attempts to test light bending.[1] Historians and

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1. E.g., Ronald W. Clarke, *Einstein. The life and times* (New York, 1971), chaps. 8–9; Banesh Hoffmann and Helen Dukas, *Albert Einstein. Creator and rebel* (New York, 1972), chap. 8; Jeremy Bernstein, *Einstein* (New York, 1973), chap. 11; Abraham Pais, *'Subtle is the Lord'... The science and the life of Albert Einstein* (New York, 1982), chap. 16.

scientists interested in the larger impact of Einstein and his general theory of relativity have also been fascinated by the effect of the British announcements.[2]

In order to gain a wider perspective on the historical circumstances of the theory's early reception among astronomers, I have studied American astronomers' work during the 1910s and 1920s.[3] A comparison of national characteristics of astronomy in the United States and England brought out clearly a special circumstance about the British eclipse episode. Astronomers in England were generally well trained in mathematical physics; most of them had passed through the Cambridge mathematics curriculum with high honors. One of the main protagonists in the eclipse expeditions, Arthur Stanley Eddington, had a deep understanding of general relativity. His interest had evolved from attempts to modify the theory of gravitation to incorporate electromagnetism; and he viewed the light-bending tests as checking on the general idea of linking electromagnetism (light) and gravitation.[4] In this theoretical climate, the British eclipse expeditions were conceived, interpreted, and publicized. By contrast, astronomers in the United States were renowned as observers, and, like their colleagues in physics, acknowledged weakness on the theoretical side.

I concentrate in this paper on the efforts of William Wallace Campbell, director of the Lick Observatory on Mount Hamilton in northern California (fig. 1), to determine whether starlight bends in passing through the gravitational field of the sun, as predicted by Einstein's theory of relativity. Unlike Eddington, Campbell did not conceive his research program in terms of theoretical questions being elaborated at the time. Rather, the program was initiated from outside the

2. Subramanyan Chandrasekhar, "Verifying the theory of relativity," *Bulletin of the atomic scientists, 31:6* (1975), 17–22; William H. McCrea, "Einstein's relationship with the Royal Astronomical Society," *Observatory, 99* (1979), 105–107; Don F. Moyer, "Revolution in science: The 1919 eclipse test of general relativity," in B. Dursonogly, A. Perlmutter, and L. F. Scott, eds., *On the path of Albert Einstein* (New York, 1979), 55–101; John Earman and Clark Glymour, "Relativity and eclipses: The British eclipse expeditions and their predecessors," *HSPS, 11* (1980), 49–85. Earman and Glymour devote more attention to attempts before the British to measure the bending of light at an eclipse, but they discount the role that these attempts played in the reception of Einstein's theory.

3. Jeffrey Crelinsten, "The reception of Einstein's general theory of relativity among American astronomers, 1910–1930 (Ph.D. thesis, Université de Montréal, 1981).

4. Arthur Stanley Eddington, "Some problems of astronomy. XIX. Gravitation," *Observatory, 38* (1915), 93–98, "Einstein's theory of gravitation," *MNRAS, 77* (1916–17), 377–383; *Report on the relativity theory of gravitation for the Physical Society of London* (London, 1918), esp. 55–56, "The total eclipse of 1919 May 29 and the influence of gravitation on light," *Observatory, 42* (1919), 119–122. Eddington later wrote two very influential

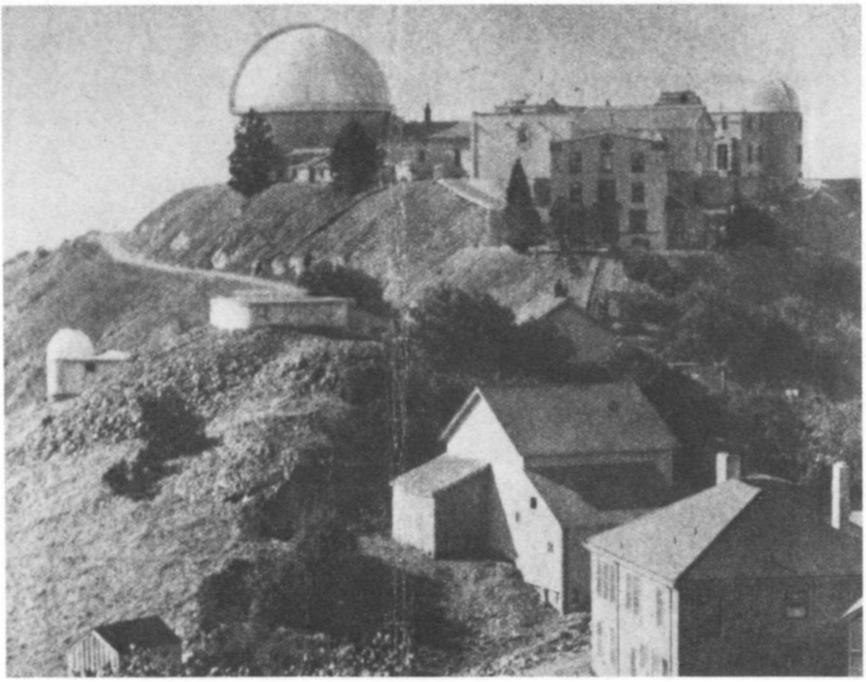

FIG. 1 View of Lick Observatory on the summit of Mt. Hamilton, California, *ca.* 1923. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

observatory and developed as an adaptation of a specialized eclipse problem that had been investigated at Lick Observatory since the turn of the century. I will demonstrate that Campbell's early involvement implied no judgment on the merits of Einstein's theory itself, and that although he came to express opinions on the matter, he had no real understanding of the theory, relying on his colleague Heber Doust Curtis, and later on Robert Trumpler, to monitor the theoretical details. Campbell resisted being drawn into public debates regarding the validity

books on general relativity: *Space, time and gravitation* (Cambridge, 1920) and *The mathematical theory of relativity* (Cambridge, 1923).

of a theory that he did not feel capable of properly assessing. Yet extra-scientific considerations eventually led him to alter his position, and during the 1920s he was forced to take a stand on the theory itself.

The story divides naturally into four periods. The first covers attempts to measure the bending of light before the appearance of Einstein's full-fledged general theory of relativity. The second treats reactions to the general theory and results of American observations during the world war. The third concerns events in 1919 leading up to the British announcement of results favoring relativity. The fourth describes research conducted after Einstein and his theory became world famous. Though important events occurred throughout the 1920s, I will end the story around 1923, when Campbell and his colleagues were forced to abandon their role of neutral technical specialists.

## 1. THE EARLY INVOLVEMENT: 1911–1914

### Lick eclipse expeditions, the "Vulcan problem" and the principle of relativity

Campbell had been appointed to the Lick staff in 1891, where he had the use of the Lick 36-inch refracting telescope (figs 2a, 2b), then the largest instrument of its kind in the world and later second only to the 40-inch refractor of the Yerkes Observatory.[5] Campbell designed a new and powerful spectrograph for use with the refractor. With it he set new standards of precision in stellar spectroscopy.[6]

When Lick Director James Keeler died suddenly and unexpectedly at the turn of the century, the Lick trustees consulted twelve leading astronomers for advice about a successor. They all chose Campbell.[7] As director of Lick Observatory, Campbell embarked on a systematic program to determine spectroscopically the radial velocities of stars

5. Henry C. King, *The history of the telescope* (London, 1955), 308–318. The Yerkes 40-inch refractor went into operation in May 1897. Helen Wright, *Explorer of the universe. A biography of George Ellery Hale* (New York, 1966), chaps. 4–5.

6. King (ref. 5), 311–312; William Hammond Wright, "William Wallace Campbell," *BMNAS, 25* (1941), 35–74; Joseph Haines Moore, "William Wallace Campbell," *ApJ, 89* (1939), 143–151.

7. Robert Trumpler, "William Wallace Campbell 1862–1938," *The sky* (Dec. 1938), p. 18.

brighter than a specified value in order to determine statistically the structure of the stellar system and the motion of the sun through it. With the financial assistance of Darius Ogden Mills, a friend of the observatory who had financed Campbell's spectrograph, the new Lick director had a second spectrograph built, which was set up at a station in Chile so that the southern part of the sky could be included in his massive observing program.[8]

In 1911, the year that Einstein's prediction of the gravitational bending of light first came to the attention of astronomers, Campbell published preliminary results of the Lick radial velocity program, which caused a small stir. Campbell showed that there was a systematic shift in the spectra of certain types of stars that if interpreted as a velocity, meant that they were receding from the sun by as much as 4 km per second. The cause of this so-called K-term became the object of much research. More than a decade later, Campbell's K-term would be interpreted and misinterpreted in terms,of Einstein's general theory of relativity.[9] The farthest reaching result of Campbell's accurate and systematic program on radial velocity was that it inspired many other observatory directors to put similar programs on their research agendas. Campbell's technical and organizational experience in the area put him in demand as a consultant, and his reputation and that of his Observatory spread rapidly.

Another area of research that Campbell domesticated at the Lick was eclipse photography and spectroscopy. The development of astronomical photography in the later nineteenth century had revolutionized eclipse observations, since the photographic plate offered an objective record of various phenomena previously only reported in the descriptions of observers. Photography was first used on a large scale at the total solar eclipse of 1860. Results obtained there confirmed the solar origin of the prominences, previously thought to be emanations from

8. W. W. Campbell, "The D. O. Mills expedition to the southern hemisphere," *PASP, 13* (1901), 28–29, "A brief account of the D. O. Mills expedition to Chile," *PASP, 15* (1903), 70–75, "Organization and history of the D. O. Mills expedition to the southern hemisphere," Lick Observatory, *Publications* (1907), 5–12. For Campbell's program on radial velocity, see Wright, (ref. 5), 44–47 and Moore (ref. 5), 145–7.

9. W. W. Campbell, "On the motions of the brighter class B stars," *LOB, 6* (1911), 101–124, "Some peculiarities in the motions of the stars," ibid., 125–135, *Stellar motions: With special reference to motions determined by means of the spectrograph* (New Haven, 1913); R. Trumpler, "Observational evidence of a relativity red shift in class O stars," *PASP, 47* (1935), 249–256; W. de Sitter, "On Einstein's theory of gravitation and its astronomical consequences. Third Paper," *MNRAS, 78* (1917–18), 3–28, on 26–27.

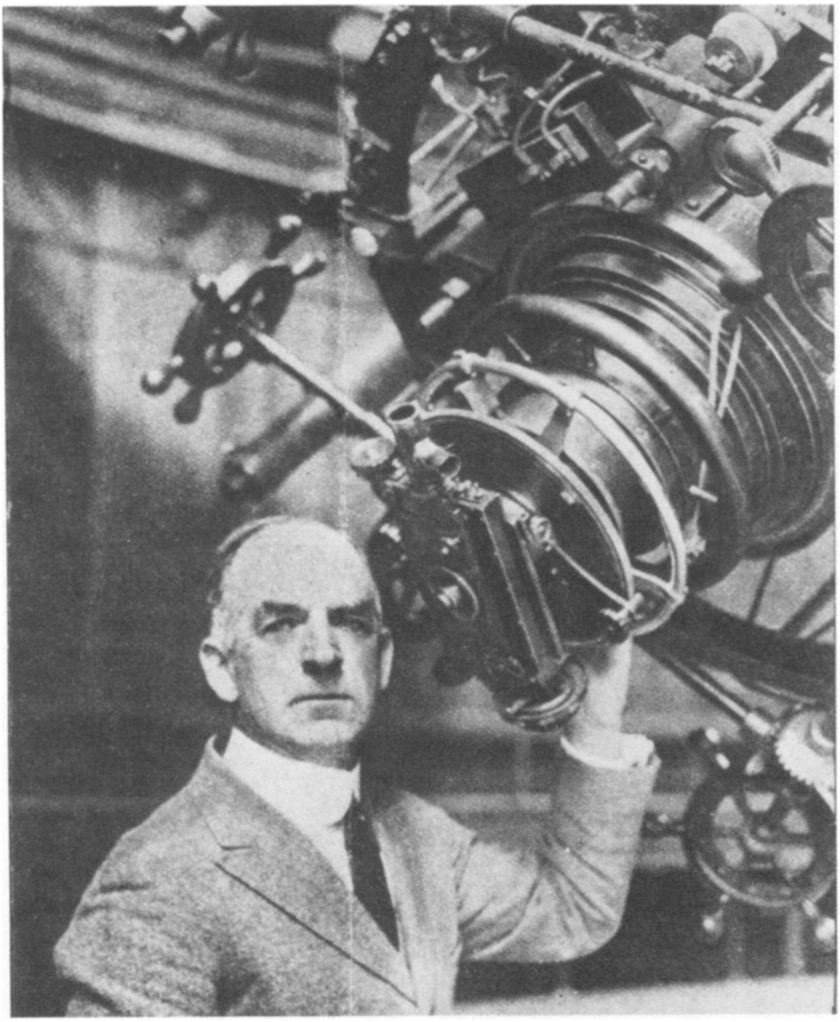

FIG. 2a W. W. Campbell in 1893 at the eye end of the Lick 36-inch refractor. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

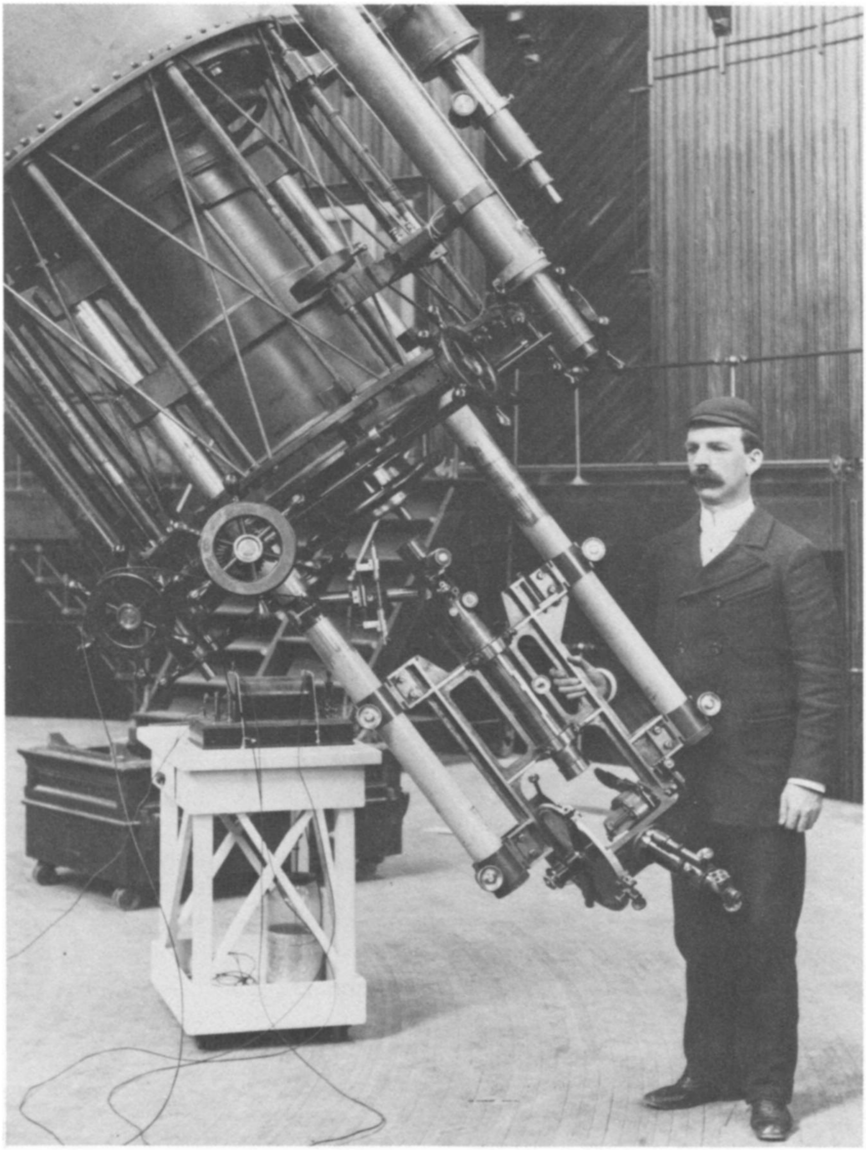

FIG. 2b Campbell thirty years later, at the same telescope, several months before he announced results of eclipse observations made in Australia in 1922. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

the lunar atmosphere. It marked an epoch in the study of eclipses.[10]

In 1894 British astronomers centralized their administrative apparatus for planning eclipses by establishing a permanent eclipse committee, run jointly by the Royal Society and the Royal Astronomical Society. This Joint Permanent Eclipse Committee (JPEC) organized all the main British eclipse expeditions and coordinated the securing of government funds to finance them.[11] Most other countries relied on individual observatories to finance and send out parties to observe eclipses. In the United States the Lick early took a prominent role in eclipse hunting. Less than one year after the observatory had gone into operation, its director, Edward S. Holden, sent an expedition to observe the total solar eclipse of January 1, 1889, which was visible in California. The well-publicized expedition boosted public interest in astronomy and so helped launch the Astronomical Society of the Pacific.[12] After Campbell became director in 1901, he headed a series of expeditions that placed the Lick among the leading institutions specializing in eclipse work. As in Campbell's large radial-velocity program, what characterized his contribution to the eclipse field was his advantageous use of the new technologies of spectroscopy and photography.

One purely photographic problem that Campbell included in the overall Lick eclipse program was the search for the hypothetical planet "Vulcan," whose existence inside the orbit of Mercury had been suggested in 1859 by the French astronomer, Urbain Jean LeVerrier. The planet Neptune had been dramatically discovered in 1846, based upon LeVerrier's analysis of the observed motion of the planet Uranus; yet even taking this new member of the solar family into account, LeVerrier could not fully account for observed planetary motions using the Newtonian laws of gravitation. The largest discrepancy was for Mercury: the perihelion of its elliptical orbit advances more rapidly than could be accounted for by gravitational forces from the other planets. In 1859 a French amateur astronomer reported a black point in transit across the face of the sun. LeVerrier assumed that the planet he had just invented to account for the excess advance had been discovered, and he christened it "Vulcan." LeVerrier calculated its orbit and predicted other transits, which were never observed, although 200 spurious sightings of it were reported between 1859 and 1878.[13]

10. Cf. Antonie Pannekoek, *A history of astronomy* (London, 1961), 406; Reginald L. Waterfield, *A hundred years of astronomy* (London, 1938), p. 20ff. Samuel Alfred Mitchell, *Eclipses of the sun,* 3rd ed. (New York, 1932), 130–136.

11. A. J. Meadows, *Greenwich Observatory..., 1836–1975* (London, 1975), 90.

12. Donald E. Osterbrock, "Lick Observatory solar eclipse expeditions," *The astronomical quarterly, 3* (1979–80), 67–79, on 67.

13. Pannekoek (ref. 10), 359–363; Waterfield (ref 10), 31; Warren Zachary Watson, *A historical analysis of the theoretical solution to the problem of the advance of the perihelion of Mercury* (Ph.D. thesis, University of Wisconsin, 1969), ix, 94.

At an eclipse in 1878 two American astronomers, Lewis Swift and James Craig Watson, reported two bright star-like objects near the sun which could not be identified with any of the fixed stars. Watson and Swift were careful observers, and their colleagues assumed that they had found intramercurial planets. Interest in the search for Vulcan was rekindled, yet it was never found again. Years later, the astronomer Samuel Alfred Mitchell complained: "The reputations of these two astronomers for careful observing were so great that it cost the science of astronomy a quarter of a century of eclipse observations before it was finally decided that no intra-Mercurial planets exist which are as large or as bright as the objects supposed to have been seen."[14]

To resolve the issue, Campbell had special lenses designed for a photographic search of the solar vicinity during an eclipse. He put Charles Dillon Perrine in charge of what he called the "Vulcan Problem." Perrine worked three eclipses, in 1901, 1905, and 1908, and found only well-known stars, 300 or 400 of them at least on the plates of 1908. Later that year, Campbell reported his final conclusions on the Vulcan matter: "It is felt that the Lick Observatory observations of 1901, 1905 and 1908 bring definitely to a conclusion the observational side of this problem, famous for half a century."[15] Campbell left open the possibility that instead of planets, matter too small to detect directly might be spread uniformly around the sun. This possibility had been developed systematically by Hugo von Seeliger of Berlin, who appealed for observational support to the existence of Zodiacal light which he attributed to reflection of sunlight by his dust ring.[16] Campbell wrote Seeliger after Perrine finished working on the 1908 eclipse plates:

> During the past year I have been very much interested in your paper demonstrating that the outstanding residuals in the motions of Mercury and the other smaller planets are due to attractions by the material responsible for the zodiacal light. I have read your paper carefully and can make no adverse criticism. Neither have I seen criticism by others.... It seems to me that our observations and your theoretical deductions lend renewed interest to the zodiacal light as a subject for investigation.

14. Mitchell (ref. 10), 153.

15. Campbell to Mitchell, 19 Mar 1908 (LO); W. W. Campbell, "Observations of the total solar eclipse of January 3, 1908," *PASP, 1* (1910), 312.

16. Hugo von Seeliger, "Kosmische Staubmassen und das Zodiakallicht," Akademie der Wissenschaften, Berlin, Math.-phys. Klasse, *Sitzungsberichte, 26* (1901), 265–292, "Das Zodiakallicht und die empirschen Glieder in der Bewegung der innern Planeten," ibid., *36* (1906), 595–622, "Die empirischen Glieder in der Theorie der Bewegungen der Planeten Merkur, Venus, Erde und Mars," *Vierteljahrschrift, 41* (1906), 234ff.; cf. Alexander Wilkens, *Hugo von Seeligers wissenschaftliches Werk* (Munich, 1927); Lewis Pyenson, "Einstein's early scientific collaboration," *HSPS, 7* (1976), 83–124, on 115.

Seeliger answered that as far as he knew, his work had not been criticized, but largely ignored, especially in England. He was delighted that it had aroused interest in America.[17]

Campbell and Seeliger did not correspond further on the subject, but Campbell remained interested. In April 1911 he attended a meeting in which the Yale theoretician Ernest William Brown discussed Seeliger's dust hypothesis. Brown pointed out that LeVerrier and the American astronomer Simon Newcomb had suggested similar solutions before Seeliger, and that none was satisfactory. In correspondence he told Campbell that his main criticism of Seeliger's ideas was the "attempt to explain *several* of the anomalies by means of *several* [dust] hypotheses.... The existence of so much matter round the sun is [not] by any means certain on account of the large amount of light which it would reflect." In deference to his observational colleague, he added "However, I am not an expert in that matter."[18]

Hearing that Campbell had "been interested in the elusive 'Vulcan'," Brown asked him to help check if the Vulcan hypothesis might "account for the great inequality in the moon's motion," a subject on which Brown was an acknowledged expert. Campbell replied that earlier searches for Vulcan, done by visual methods, had led to "purely negative results, and to much wrangling," and that the "photographic method of search" in Perrine's hands had come out negative. Though Campbell offered to search the plates where Brown might direct, Brown answered that the Lick investigations "put the non-existence of such intra mercurial planets as I have postulated beyond much doubt. I hardly think that it would be worth while to re-examine the plates with such a faint hope."[19]

About this time astronomers' discussions of the "principle of relativity" began to impinge on anomalous perihelion motion. In 1911 the Dutch astronomer Willem de Sitter referred to Seeliger's work in the context of calculating the astronomical consequences of the principle of relativity. The motions of planetary perihelia so determined were of the order predicted by Seeliger's hypothesis, and de Sitter showed that a

17. Campbell to Seeliger, 6 Mar 1908, and Seeliger to Campbell, 8 Apr 1908 (LO): "Meine arbeit ist von anders Seite *nicht* kritissiert worden und ich habe vorläufig nichts hinzuzufügen....dass man meine Arbeiten in England z.B. überhaupt zu ignorieren pflegt. Ich kann mir das nicht erklären. Dagegen freut es mir, dass die amerikanischen Collegen, besonders die vom Lick Observatory anders denken."

18. Brown to Campbell, 5 Mar 1912; Campbell to Brown, 13 Mar 1912; E. W. B[rown], "The secular motions of the elements of the inner planets," *MNRAS, 70* (1910), 342–344.

19. Campbell to Brown, 13 Mar 1912; Brown to Campbell, 22 Mar 1912 (LO).

combination of the principle of relativity and two out of three of Seeliger's suggestions would account for many of the discordances observed.[20] Two years later de Sitter expanded his work on planetary perihelia in an article in a series on "Some problems in astronomy." Campbell monitored these developments and published a short note, summarizing attempts to account for discrepancies between the observed positions of bodies in the solar system and their predicted positions on Newtonian gravitation theory. He mentioned that "[Fritz] Wacker, [Henrick Antoon] Lorentz, and de Sitter have discussed the principle of relativity as a possible important factor in explaining existing residuals in the motions of planetary perihelia."[21]

By the time Campbell published these remarks, he had taken an active interest in the observational side of an entirely new kind of research being done on the "principle of relativity." The "magnificent"[22] collection of eclipse plates that had convinced Brown to drop the Vulcan idea in relation to the theory of the moon's motion, and the lenses that had been used to obtain these plates, were the tools for this research; but the project had nothing to do with having disproved the Vulcan hypothesis as an explanation of anomalies in motions of planetary perihelia. The eclipse plates and lenses were used in the first search for the bending of light in a gravitational field. This work gradually evolved into the inclusion of a new problem, the "Einstein Problem," on the research agenda of Lick eclipse expeditions after 1912.

## The role of Erwin Freundlich

In 1911 Einstein published a paper on the influence of gravitation on the propagation of light. "According to our view," he wrote, "the spectral lines of sunlight, as compared with the corresponding spectral lines of terrestrial sources of light, must be somewhat displaced toward the red." This redshift could be measured, he said, if the conditions under which the solar bands arise were exactly known; but as other influences such as pressure and temperature affect the position of the spectral line, "it is difficult," he concluded, "to discover whether the inferred influence of the gravitational potential really exists."[23]

20. W. de Sitter, "On the bearing of the principle of relativity on gravitational astronomy," *MNRAS, 77* (1911), 388–415.

21. W. de Sitter, "The secular variations of the elements of the four inner planets," *Observatory, 36* (1913), 296–303; W. W. Campbell, "Concerning some forces affecting cosmical motion," *PASP, 25* (1913), 164–166; F. Wacker, "Ueber Gravitation und Elektromagnetismus," *Physikalische Zeitschrift, 7* (1906), 300–302. For Wacker's ideas, see Lewis Pyenson, *The Göttingen reception of Einstein's general theory of relativity* (Ph.D. thesis, Johns Hopkins University, 1974), 47–50.

22. Campbell to Brown, 13 Mar 1912 (LO).

23. Albert Einstein, "Ueber den Einfluss der Schwerkraft auf die Ausbreitung des

In this paper, Einstein also predicted that light should be deflected in a gravitational field. He calculated that the deflection could be detected by observing stars seen near the sun during a solar eclipse. Einstein derived a formula for the angular deviation $a$ (in seconds of arc):

$$a = 2kM/c^2D, \tag{1}$$

where $k$ is the gravitation constant, $M$ the mass of the sun (or any gravitating object), $c$ the velocity of light, and $D$ the distance from the path of the ray to the center of attraction. For a ray grazing the limb of the sun, $a = 0''.86$. At the end of the paper Einstein wrote:[24]

> It is greatly to be desired that astronomers should undertake this investigation, although the foregoing reasoning may prove to be insufficiently founded or even entirely illusory. For, aside from any theory, the question must be considered, whether with our present resources an influence of the gravitation field on the propagation of light can be established.

Erwin Finlay-Freundlich was a junior observer at the Berlin Observatory. He heard of Einstein's new work from a visitor, Leo Wentzel Pollak, a *Demonstrator* at the Institute für kosmische Physik at the German university in Prague, where Einstein was professor of theoretical physics. Pollak said that Einstein was very interested in having astronomers test implications of his new theory. Freundlich wrote Einstein that same night, and Pollak, upon his return to Prague, sent him proofs of Einstein's article. This common interest focused on the light-bending. Einstein thought that the sun would offer the only practicable possibility for a test, and he wanted to know whether the sun's atmosphere would affect measurements. Freundlich thought that it might be possible to use the planet Jupiter, but Einstein disagreed.[25]

During November Perrine, who had left Lick in 1909 to become director of the southern hemisphere observatory in Cordoba, visited the Berlin Observatory. When Freundlich told him about the light-bending test, Perrine suggested that he write to various astronomers who might have old eclipse plates on which star images might be measured for deflection. Naturally he mentioned the Lick Vulcan plates. Freundlich sent a circular letter to several observatories, including Lick, asking for "support of astronomers, who possess eclipse-plates" to test Einstein's

Lichtes," *Annalen der Physik, 35* (1911), 898–908, translated in Einstein et al., *The principle of relativity* (New York, 1952), 99–108, on 105.

24. I have used the translation by Campbell's colleague Heber Doust Curtis, as quoted in his "The influence of gravitation on light," *PASP, 25* (1913), 77–80. The translation in *Principle* (ref. 23) leaves out the conditional clause in the first sentence.

25. Pyenson (ref. 16), 105–106.

predicted deflection of light by the sun.[26]

The first response came from Edward Charles Pickering, director of Harvard College Observatory, who noted that his brother William had proposed the "best method of photographing the eclipse of the Sun and stars on the same plate," trying the method during the eclipse of May 28, 1900. Pickering's method had obliterated images of faint stars, but as improved by Samuel Pierpont Langley of the Smithsonian Institution and "with still better results," by Campbell at Lick, it had perhaps recorded the information Freundlich wanted. The Harvard collection of eclipse plates appeared to contain nothing useful, but it had many photographs of stars "passing very near to Jupiter," which, he supposed, might also show the effect.[27]

Freundlich had already sent his circular letter to the Smithsonian Astrophysical Observatory, but he sent another copy to the Institution's secretary, which brought him copies of Langley's old plates. The U.S. Naval Observatory supplied two photographs taken at the eclipse of August 30, 1905.[28] Freundlich was delighted with the response from the American observatories, since "in Europe there hardly exist any [such plates] at all."[29]

Freundlich's circular letter never reached the appropriate desk at Lick. He sent another, mentioning Perrine's advice "to apply especially to you," and asking for glass copies of all eclipse plates that showed the sun and stars in the same field. "I need not assure you that the result of my whole investigation depends to the greatest part from your kind support and you can imagine how much I shall be obliged to you." He told Campbell that he had already heard from Pickering, Abbot, "the Naval Observatory, and the English Observatory," who had promised "to support my investigation."[30] Campbell promised to send positives on glass of all Perrine's intramercurial plates that seemed applicable and of the corresponding chart plates of the same stellar regions taken several

26. Freundlich to "Sir," 25 Nov 1911 (LO; HA; US; USNO); C. D. Perrine, "Contribution to the history of attempts to test the theory of relativity by means of astronomical observations," *AN, 219* (1923), 281–284.

27. E. C. Pickering to Freundlich, 12 Dec 1911 (HA). Freundlich answered that the effect for Jupiter would be too small to measure. Freundlich to Pickering, 22 Dec 1911, 20 Jan 1912; Pickering to Freundlich, 5 and 8 Jan 1912.

28. Freundlich to "Sir," 25 Nov 1911, and C. G. Abbot to Freundlich, 2 Mar 1912 (SA, Record Unit 45, Box 22); Captain Jayne, Superintendent Naval Observatory, to Freundlich, 9 Jan and 29 Mar 1912; and Freundlich to "Sir," 26 Jan 1912 (US, Naval Observatory general correspondence).

29. Freundlich to Pickering, 20 Jan 1912 (HA).

30. Freundlich to Campbell, 24 Feb 1912 (LO). The earliest correspondence between Freundlich and the Greenwich Observatory preserved there dates from 1913 and pertains to the eclipse of 1914.

months before the eclipses with the same instruments; but he had reservations about the suitability of the plates since none had the sun's image in the center of the field.[31]

In view of the likely unsuitability of the Vulcan plates for the task at hand, Campbell offered to lend the cameras he had designed for the Vulcan problem to Perrine to try Freundlich's problem at the eclipse in Brazil on October 9–10, 1912. The photographs taken would have the sun's image in the center of the field. Campbell advised Freundlich to write Perrine and he, too, wrote Perrine the same day, urging him to go and suggesting the procedure to use.[32]

Freundlich hoped to get something from the old Vulcan plates. "Fortunately [he wrote Campbell] the effect pronounced by Mr. Einstein decreases so rapidly with increasing distance from the Sun, that by measuring the distance of two stars more or less distant from the Sun, I am able to measure at least the simple effect nearly quite undiminished. Perhaps there will be found also a few stars both north and south from the Sun."[33] As for the coming eclipse, Freundlich accepted Campbell's offer with thanks from himself and from his director, Hermann Struve, who "also would be pleased, if one could get good plates for my purposes at the future eclipses." Struve later turned sour on Freundlich's project.

Perrine agreed to include Freundlich's problem on his research agenda, and Campbell sent the lenses down via the astronomer William Joseph Hussey. The eclipse took place on October 10, 1912. A few days later Campbell received a telegram from Edward C. Pickering of Harvard, the communication center for American astronomy: "Perrine cables from Brazil, rain."[34] Meanwhile the measuring of the old Vulcan plates got off to a slow start. Plates sensitive enough for copying had to be shipped from New York. They arrived at Lick on April 30, 1912, and Campbell put Heber Doust Curtis on producing the copies.[35] Curtis had followed Einstein's work and knew the formula for light bending by the sun. He calculated values for the deviation with angular distance, given in table 1. "It should be possible," he noted, "to get this with some certainty from several hundred stars." The paper containing these calculations is pinned to another with Curtis' assessments of the plates (table 2). Curtis concluded: "*For the purpose of Freundlichs*

31. Campbell to Freundlich, 13 Mar 1912 (LO).
32. Campbell to Perrine, 13 Mar 1912 (LO).
33. Freundlich to Campbell, 3 Apr 1912, Freundlich's English.
34. Perrine to Campbell, 23 Apr and 26 Aug 1912; Campbell to Perrine, 9 May and 31 May 1912; E. C. Pickering to Campbell, 14 Oct 1912; Perrine to Campbell, 25 Feb 1913 (LO). Cf. Perrine (ref. 26).
35. Campbell to Freundlich, 30 Apr 1912 (LO).

*investigation* doubt if more than 6 or 8 [plates] would be really useable."[36]

TABLE 1

| | |
|---|---|
| at the limb | 0".83 |
| 1° from the limb | 0".28 |
| 6° from the limb | 0".06 |

TABLE 2

| | |
|---|---|
| Vulcan plates with Sun on plate | |
| 4 Spain 18 × 22 | Many fair images |
| 3 Aswan 16 × 20 | Images poor |
| 4 unmarked 14 × 17 | Images fair |
| 1 16 × 20 Flint | 1 star only |
| 2 18 × 22 | Many stars but images poor |
| At distance from Sun | |
| 4 Spain 18 × 22 | Many good images |
| 3 Aswan 16 × 20 | |
| 2 " 14 × 17 | |
| 6 unmarked 14 × 17 | |
| 3 16 × 20 | no stars ? |
| 1 18 × 22 | many stars |
| 2 " | many stars but poor |
| Also Chart plates for all these regions | |

Curtis selected some of the plates from the Spain and Flint Island eclipses, rejecting those taken at Aswan in Egypt since "only a few stars were shown on these plates and the images had trailed to such an extent as to make them useless" for Freundlich's measures. On June 6, 1912 the plates were sent to Berlin via the Smithsonian Institution's Bureau of International Exchange. Curtis warned Freundlich that the measures would be difficult: "Even on the original negatives many of the star images are excessively faint, and can be made out only with the greatest difficulty; I fear that you will not be able to make them out at all on the copy in many cases, though I have purposely made the positives rather thin, so as not to blot out these exceedingly faint images by over-exposure." Curtis reiterated that for an adequate treatment of the problem "plates should be taken with the Sun central, and the cameras should be rated to a stellar rate, instead of to the solar rate, as was the

36. Undated fragment (LO, Curtis folder). Cf. H. D. Curtis, "The theory of relativity," *PASP, 23* (1911), 219–229.

case in all these eclipse plates."[37]

The day after Perrine's eclipse expedition had been rained out, an optimistic Freundlich wrote Campbell that the Vulcan plates had arrived. The plates from the Smithsonian Institution had come several months earlier, and Freundlich had been measuring the coordinates of all the stars on one of the plates, using apparatus borrowed from Karl Schwarzschild of Potsdam. The plates from the Naval Observatory had been splintered in transit. Freundlich, sanguine about the possibilities with the Smithsonian and Lick plates, had not requested replacements.[38]

The old eclipse plates proved as disappointing as the news from Brazil. On all the plates Freundlich received, including "the very valuable ones of the Lick Observatory," the insufficiently sharp star images made a "successful measuring of the plates illusory." Karl Friedrich Küstner of Bonn had examined one of the better Lick plates and concurred that their having been taken to find intramercurial planets made them useless for the light-bending problem.[39]

It appears that Freundlich conceived the technical problem of measuring the eclipse plates quite differently from what later came to be the accepted procedure. Freundlich had mentioned that he measured the rectilinear coordinates of each star on an eclipse plate, which suggests that he calculated deflection from the difference between the measured coordinates of the eclipse stars and those of the comparison stars. Although the trailing of the images and the reliance on copies aborted Freundlich's enterprise, his absolute method of measurement would in any case have produced intolerably large errors.[40] Curtis also favored the absolute method, but Campbell preferred a differential approach, where the positive of an eclipse plate is superimposed upon the negative of a comparison plate, and only differences between star images are measured. This issue was to become critically important in subsequent attacks on the problem.

37. Curtis to Freundlich, 6 Jun 1912 (LO).

38. Freundlich to Campbell, 11 Oct 1912 (LO); Freundlich to C. G. Abbot, 30 Apr 1912 and F. W. True to Freundlich, 15 May 1912 (SA); Freundlich to USNO, 22 Apr 1912 (US); E. F. Freundlich, "Ueber einen Versuch, die von A. Einstein vermutete Ablenkung des Lichtes in Gravitationsfeldern zu prüfen," *AN, 193* (1913), 369–372.

39. Freundlich to Campbell, 6 Feb 1913 (LO). Freundlich (ref. 38).

40. That Freundlich intended an absolute measurement is confirmed by the Toepfer machine that Freundlich borrowed from Schwarzschild. Karl Kamper, an astrometrist at the David Dunlap Observatory, informs me that Toepfer machines of this period were usually designed for absolute measurement.

**The Russian eclipse of 1914**

Freundlich's hopes now turned to the eclipse of 1914, which would be visible from Russia. He announced his willingness to collaborate with anyone wishing to take photographs at the eclipse and detailed requirements for taking adequate observations. He asked Campbell whether he would join in. "I would be most thankful for your kind support." Campbell promised that if, as he hoped, an expedition were sent from Lick, then "we shall plan to obtain photographs meeting the requirements of your problem; that is, with the solar image in the center of the field and with the driving clock adjusted to follow the stars."[41] He attached as a condition to the collaboration that "any results obtained by you might be announced by you in a preliminary way in the Nachrichten, or otherwise, and your full paper on the subject be published as a Lick Observatory Bulletin."

Freundlich also tried to enlist the Greenwich Observatory in his project. Its director, Frank Dyson, declined: "It would be an extremely delicate research to undertake at an eclipse, if not quite beyond present possibilities, and so I cannot venture to promise to take photographs specially for this purpose." Dyson did refer Freundlich's appeal to the JPEC, of which he, as Astronomer Royal, was chairman. The committee decided "that a special equipment would be required to carry out this work satisfactorily and...that no suitable instruments were at its disposal."[42] In fact the JPEC financed more than one expedition to Russia, and instruments could have been adapted to Freundlich's problem, if someone had wanted to do so.

Meanwhile, the Lick had received funding for an expedition to Russia from one of its long-time benefactors, William H. Crocker, a trustee of the University of California. Since Campbell attended the International Solar Union meeting in Bonn in August 1913, and the meeting of the Astronomical Gesellschaft in Hamburg, he had an opportunity to visit the Berlin Observatory, and to discuss the coming eclipse observations with Freundlich "so that they will best meet your requirements."[43] Campbell's visit to Germany coincided with a high point in international relations among astronomers. The Berlin Observatory which had originally declined to join the Solar Union, accepted election on the proposal of British and American members.[44] The German astronomers

41. Freundlich (ref. 38), 372; Freundlich to Campbell, 6 Feb 1913; and Campbell to Freundlich, 6 Mar 1913 (LO).

42. Freundlich to Dyson, 7 Feb 1913, and Dyson to Freundlich, 16 Feb 1913 (RGO); JPEC, Minutes, 13 Dec 1913 (RAS). Cf. McCrea (ref. 2), 252.

43. Campbell to Freundlich, 27 May 1913, and Freundlich to Campbell, 2 July 1913 (LO); W. W. Campbell, "The total eclipse of the sun, September 21, 1922," *PASP, 35* (1922), 10–44, on 16.

44. Campbell to Hale, 27 Aug 1913, and Hale to Campbell, 19 Sep 1913 (HM). Cf.

were extraordinarily hospitable at the Bonn and Hamburg meetings. "Not only were the scientific sessions successful," Campbell remarked in his report to Hale, "but the social arrangements were on a very extensive scale and extremely happy. The hosts at later meetings are going to have hard work to live up to the high standard set at Bonn." On his return to the United States, Campbell published an extensive, glowing account of the meetings. Herbert Hall Turner wrote a similar account for the British.[45]

Campbell's attitude toward the eclipse collaboration reflected this international goodwill as well as his own favorable feelings toward Freundlich and German astronomers in general. When Freundlich, who hoped that several expeditions would join his project, asked whether Campbell would mind a full treatment of the problem being published in German as well as in English in the Lick Bulletin, Campbell acceded.[46] He was perfectly content to contribute observations, having his German colleague to measure and write up the results.

Toward the end of 1913 Curtis published an article summarizing the status of the Einstein problem. With physicists "divided into two warring camps on the subject of the theory of relativity," the light-bending test gave astronomers a chance to contribute to a controversial subject. Relating Freundlich's abortive attempt to measure the Lick Vulcan plates ("Probably the only observational material at present available to test the truth or falsity of this hypothesis"), Curtis went on to detail how observations might be made at the coming total eclipse of the sun. Using the intramercurial plates taken at Flint Island in 1908, he estimated the limiting magnitudes of stars at different distances from the sun's limb. He discussed the star field that would be visible around the eclipsed sun and suggested that the bright star Regulus in the constellation Leo could be used by observers to guide their telescopes in right ascension.[47] Curtis' publication gave all parties going to Russia the necessary information to make the Einstein test themselves. Except for Freundlich and Campbell, none did.

Meanwhile Einstein and Freundlich had been toying with the possibility of measuring the displacement of stars near the uneclipsed sun. When Einstein took up a professorship in Zurich in 1912, he told his

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Wright (ref. 5), 260–261, 300–301; David DeVorkin, "Community and spectral classification in astrophysics: The acceptance of E. C. Pickering's system in 1910," *Isis, 72* (1981), 29–49, esp. 36–40.

45. Campbell to Hale, 27 Aug 1913 (HM); W. W. Campbell, "International meetings of astronomers in Germany," *PASP, 25* (1913), 244–255; H. H. Turner, "Oxford notebook," *Observatory, 36* (1913), 356–366, 382–386, 413–417.

46. Freundlich to Campbell, 28 May 1913, and Campbell to Freundlich, 14 Jun 1913 (LO).

47. Curtis (ref. 24).

colleague Julius Maurer about the idea. Maurer was discouraging. But he suggested that Einstein consult Hale. Einstein did, in a letter now well known; and Maurer graciously added an appeal that the famous American astronomer consider Einstein's question about the possibility of testing light bending with daylight observations of stars in the vicinity of the sun.[48]

Hale saw no hope for the endeavour and referred the problem to Campbell, who, as he knew from Paul Epstein, took an interest in the eclipse method. Campbell wrote both Hale and Einstein of his plans to obtain photographs for Freundlich using the intramercurial lenses. Meanwhile, Freundlich was encountering difficulty in raising funds for his own expedition. Struve refused to support the project financially, so Freundlich applied to the Berlin Academy for funds, which, despite Einstein's urging, also refused. In the end, Freundlich obtained sufficient funds from other sources to launch an expedition. He borrowed some equipment for the Einstein problem from Perrine, who was also sending an expedition from Cordoba, but would not be attempting the Einstein test himself.[49]

At the Lick Observatory, detailed plans for the eclipse expedition went forward. Curtis would accompany the instruments from New York to the eclipse station, about 15 miles from Kiev. Campbell and his family planned to reach Kiev about 20 July. After the eclipse, Campbell wanted to attend the Astronomische Gesellschaft meeting, set to take place in St. Petersburg and at the Russian National Observatory at Pulkova; then to return home via Berlin, Paris, and London, to give his three sons a chance "to see the important observatories in those cities." The outbreak of war blighted the fruition of these plans. At first it was not certain how the eclipse party would be affected. Richard Hawley Tucker, acting director of Lick during Campbell's absence, wrote: "We are imagining that the threatened war will make things exciting for your eclipse party, but we hope on every account that Russia will keep out of the complications."[50] A week after their arrival in Kiev, Campbell's party was oblivious to the growing tensions. Curtis had arrived several days earlier and had found "a splendid location for the observing station, had rented the place, including a fine

48. Pyenson (ref. 16), 108; Einstein to Hale, 14 Oct 1913 (HM), printed in Hoffmann (ref. 1), 113, and in Helen Wright, Joan Warnow, and Charles Weiner, eds., *The legacy of George Ellery Hale* (Cambridge, Ma., 1972), 66–67.

49. Hale to Campbell, 1 Nov 1913; Campbell to Hale, 4 Nov 1913; Hale to Einstein, 4 Nov 1913; all in HM. The last letter is in Wright et al. (ref. 48), 68–69. Campbell to Einstein, 4 Nov 1913 (LO); Pyenson (ref. 16), 109; Perrine (ref. 26), 283–284.

50. Campbell to Hale, 3 Mar 1914 (HM); Campbell to Byron S. Hurlbut, 15 Jan 1914 (LO, Campbell folder 5); R. H. Tucker to Campbell, 20 Jul 1914 (LO).

house, had installed a cook, etc." Campbell was delighted, especially as the work was going ahead of schedule; but though the clouds of war were not yet apparent, those of nature were not encouraging. "The weather has been pretty unpromising," Campbell wrote, "raining nearly every day, and cloudy the most of the time. We hope for clear sky on the afternoon of the 21st. But nothing will surprise us."[51]

Curtis and Mrs. Campbell visited Kiev for supplies and found out that on July 30 Austria had declared war against Serbia and that Russia had begun mobilizing. The next day the reserve at the town of Brovary, where the eclipse camp was set up, mobilized. On August 2 the astronomer Robert Filopowitch Foghel, of the University of Kiev, called on Campbell with the news that Germany had declared war against Russia. Campbell wrote immediately to the British Consul in Kiev to ask for protection for the expedition; he requested "a detail of two policemen in uniform here at our station, from August 20th to August 25th, to guard the expedition against the acts of ignorant or excited people who may be inclined to connect the eclipse with events occurring around them." The American Embassy in St. Petersburg advised him that, "as England, France, Germany, Austria, Russia and Belgium are all at war and nearly all other countries mobilizing, the neutral ships leaving neutral ports are naturally few"; and that, consequently, the U.S. government would probably charter ships to take Americans back home who could not otherwise return.[52]

The Russian authorities allowed the American party to observe the eclipse but arrested Freundlich's. The older members were deported immediately; Freundlich and his younger colleagues were held as prisoners of war, and later exchanged for Russians caught in Germany when hostilities broke out. Perrine had not arrived early enough to take over Freundlich's light-bending test, but it would have been useless in any case. Clouds as well as the moon eclipsed the sun. Mrs. Campbell's diary notes for the day speak eloquently: "Total failure. Thick gray cloud at eclipse time and lovely clear sunshine afterwards."[53]

Campbell's party returned via the Baltic to London, threatened by German mines and submarines. "The war is a terrible thing," Campbell remarked to Hale. "I fear that we, in this country, do not appreciate how very real the war is to people in the countries involved." He worried that the war would inject "intense international hatreds" into science. Hale agreed that the situation was grave. "The war grows worse

51. Campbell to Mr. Bruce, 28 Jul 1914 (LO).

52. Mrs. Campbell's diary notes; Campbell to Consul for Great Britain and Ireland, 3 Aug 1914; Charles Wilson (American Chargé d'Affaires) to Campbell, 11 Aug 1914 (LO).

53. Perrine (ref. 26); Mrs. Campbell (ref. 52).

and worse," he answered, "and I greatly fear from what I hear from Germany that all international relations may be seriously interrupted, perhaps for years. I agree with you in thinking that we should make unusual efforts to assist the next Solar Union meeting, and only hope it may not prove necessary to postpone it."[54] In the event all activities of the Union were suspended for the duration of the war.

The scientific failure of the expedition also weighed heavily on Campbell. He wrote Hale:[55]

> If the critical two minutes had been clear so that we could have brought valuable results home with us we would have thought nothing of the inconveniences which later greeted us at various points. I never knew before how keenly an eclipse astronomer feels his disappointment through clouds. Eclipse preparations mean hard work and intense application, and I must confess that I never before seriously faced the situation of having everything spoiled by clouds. One wishes that he could come home by the back door and see nobody.

From then on, Campbell applied himself single-mindedly to the Einstein problem independent of his former colleague from Germany.

Fortunately solar astronomers inclined to Einstein testing did not have to put all their chances on the weather on eclipse day. Since the turn of the century they had been trying to determine possible causes for a systematic redshifting of line in the solar spectrum. The accepted mechanism had been pressure shifts in the solar atmosphere; and Einstein had remarked in his 1911 paper that although reported measurements of solar line shifts were roughly what his principle of equivalence predicted, observers had ascribed their results to pressure in the sun's absorbing layer.[56] But by then other evidence, particularly observations by John Evershed, director of the Kodaikanal Observatory in southern India, indicated a pressure in the sun's atmosphere far too low to account for the line shifts.[57] Freundlich pointed out that Evershed's values for solar line shifts agreed with those demanded by relativity.[58] By 1914 solar spectroscopists included the relativistic gravitational redshift as one of several possible mechanisms producing solar line

54. Hale to Campbell, 13 Oct and 5 Nov 1914; Campbell to Hale, 16 Oct 1914 (HM).

55. Campbell to Hale (ref. 54).

56. Einstein (ref. 23), 105; cf. Eric Gray Forbes, "A history of the solar red shift problem," *Annals of Science, 17* (1961), 129–164.

57. J. Evershed, "Pressure in the reversing layer," Kodaikanal Observatory, *Bulletin, 18* (1909), 131–134; "A new interpretation of the general displacement of the lines of the solar spectrum towards the red," ibid., *36* (1913), 45–52.

58. E. Freundlich, "Ueber die Verschiebung der Sonnenlinien nach den roten Ende auf Grund der Hypothesen von Einstein und Nordström," *Physikalische Zeitschrift, 15* (1914), 369; J. Evershed and T. Royds, "On the displacements of the spectrum lines at the sun's limb," Kodaikanal Observatory, *Bulletin, 39* (1914), 71–81, on 80–81.

shifts.[59]

2. THE WAR PERIOD: 1914–1918

When general relativity came out, its mathematics were novel and complex to most physical scientists. Yet its exact accounting for the excess advance of Mercury's perihelion could not be ignored. As the theory slowly attracted attention during the war years, the gravitational redshift and the bending of light began to be viewed as crucial tests. The perceived difficulty of the mathematics focused attention on the few observational tests, the results of which would determine whether or not the theory need be taken seriously. The difficulty of the required observations and reductions promised rewards to the institution that first successfully carried them out, provided, of course the results were definitively pro or con. The "Einstein problem" evolved into a high profile eclipse program as well as a difficult and intractable one.

### The spreading of the news to English-speaking astronomers

It was via a small network of theorists that news of general relativity spread from wartime Berlin to American astronomers across the Atlantic (fig. 3). Willem de Sitter, director of Leiden Observatory, followed the development of general relativity closely. His colleagues in theoretical physics, Lorentz and Paul Ehrenfest, were intimate friends of Einstein; he sent them proofs of his articles, which they shared with de Sitter, who also had the advantage of conversation with Einstein in the fall of 1916.[60] Knowing that German periodicals were not reaching Britain, de Sitter wrote to Arthur Stanley Eddington, then a secretary of the Royal Astronomical Society, telling him about Einstein's new theory, and enclosing a copy of Einstein's comprehensive paper on general relativity published in May 1916. Realizing that it could not very well be reprinted in an English journal during wartime, de Sitter offered to write an article on the theory himself for publication in the Society's *Memoirs*. Eddington was "immensely interested in what you [de Sitter] tell me about Einstein's theory....Hitherto I had only heard vague

59. This aspect of astronomers' involvement in the testing will not be elaborated here. See Crelinsten (ref. 3), 84–94, 112–116, 183–213, 361–381; Forbes (ref. 56); and John Earman and Clark Glymour, "The gravitational red shift as a test of general relativity: History and analysis," *Studies in history and philosophy of science, 11* (1980), 175–214.

60. Martin J. Klein, *Paul Ehrenfest;* vol. 1, *The making of a theoretical physicist* (New York, 1970), 302. Karla and Franz Kahn, "Letters from Einstein to de Sitter on the nature of the universe," *Nature, 257* (1975), 451–454.

rumors of Einstein's new work. I do not think anyone in England knows the details of his paper."[61]

Eddington approved de Sitter's plan but advised publication in the RAS's frequently published and widely distributed *Monthly notices,* rather than in its slow *Memoirs*. With Eddington's intervention, the first of a series of three papers by de Sitter, of which the manuscript was received at the RAS in August 1916, went immediately to the printer and appeared in a planned supplement to the *Notices* for October.[62] The next two came slower, owing to a fire at the printer. Eddington undertook to master de Sitter's and Einstein's papers, and soon he too was interpreting and elaborating general relativity.[63] American astronomers had no one within their ranks who did the same. They learned Einstein, whose papers did not reach them owing to the British blockade, from de Sitter and Eddington.[64]

Nor did de Sitter and Eddington neglect more general audiences. Writing in the semipopular *Observatory*, de Sitter emphasized the requirement that the laws of nature should be independent of the choice of coordinates, introduced the concept of an invariant tensor, and related Riemann's metrical tensor to Minkowski's four-dimensional time-space. Using the latter notion, he introduced the picture of the world-line of a material particle. Einstein's theory of gravitation stated that world-lines in this time-space are geodesics, de Sitter explained, lines traced by particles subject to no forces. Gravitation was a property of space: the coefficients determining the metric properties of time-space also determine the gravitational field. Einstein's equations for the metric and gravitational field give in first approximation Newton's law

61. Eddington to de Sitter, 11 Jun 1916 (LC); A. Einstein, "Die Grundlage der allgemeinen Relativitätstheorie," *Annalen der Physik, 49* (1916), 769–822, issued separately as *Grundlage der allgemeinen Relativitätstheorie,* (Leipzig, 1916) and translated in *Principle* (ref. 23), 111–164. That this is the paper de Sitter sent appears from Eddington to Dyson, 9 Dec 1916 (RGO).

62. W. de Sitter, "On Einstein's theory of gravitation and its astronomical consequences," *MNRAS, 76* (1915–16), 699–728, *77* (1916–17), 155–184, and *78* (1917–18), 3–28. Karla and Franz Kahn (ref. 60), 454, misrepresent Eddington's initial reaction to Einstein's theory and to de Sitter's desire to advertise it as "somewhat cool"; but Eddington's letter emphasizes the difficulties about publishing in the RAS's *Memoirs* only to recommend the alternative of the *Notices,* not to discourage de Sitter. Cf. Eddington to Wesley, RAS Secretary, 24 Aug 1916 (RAS), recommending de Sitter's paper as one of "exceptional importance."

63. Eddington to de Sitter, 11 Jun and 31 Dec 1916 (LC); A. S. Eddington, "Gravitation and the principle of relativity," *Nature, 98* (1916), 328–330; "Einstein's theory" (ref. 4); *Report* (ref. 4); W. de Sitter, "Space, time, and gravitation," *Observatory, 39* (1916), 412–419.

64. E. g., E. B. Wilson, "Generalized co-ordinates, relativity, and gravitation," *ApJ, 45* (1917), 244–253.

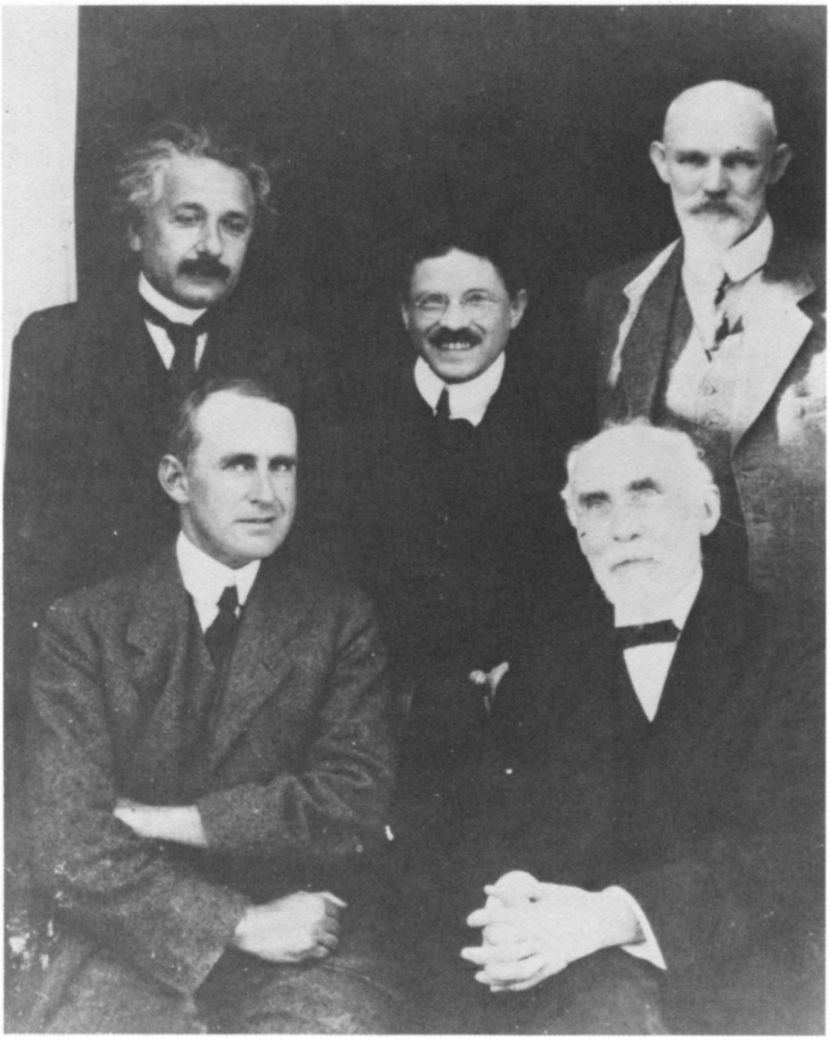

FIG. 3 European relativity theorists: in the rear, left to right, Einstein, Paul Ehrenfest, Willem de Sitter; in front, Arthur Eddington and Hendrik Antoon Lorentz. Courtesy of the Neils Bohr Library, AIP.

of gravitation. "If the approximation is pushed one order further [de Sitter wrote] the well-known anomaly in the motion of the perihelion of Mercury is exactly explained, and no other effects at present within the reach of observation are produced in the motions of the planets or the Moon."[65]

At least one reader of *Observatory* did not think that Einstein had done anything new and true. In a letter to the editor, Thomas Jefferson Jackson See, a pompous plagiarizer employed as an astronomer at the U.S. Naval Station on Mare Island near San Francisco, objected to the "metaphysical" foundations of general relativity. He complained that while de Sitter "carefully discusses the *analysis* of Einstein's treatment, he so completely passes over all *physical considerations* as actually to convey the impression that gravitation is not a *physical problem,* but only an *analytical one.*" See expressed amazement that de Sitter "actually states that under this theory gravitation is not a 'force,' but 'a property of space.'"[66] See quoted Sir Oliver Lodge's calculations of the equivalent forces of attraction of the earth on the moon, and the sun on the earth, in terms of the breaking strength of enormous number of steel pillars. Evidently, he concluded, gravitation is "an influence exerted by *matter,*" and Einstein et al had as their unique merit a demonstration of "the extent to which purely mathematical reasoning may be misapplied by those who ignore appropriate physical considerations."

See's comments drew a response from James Jeans, who worriedly pointed out that See's letter "gives rise to a fear that Einstein's Theory may meet with an unfavourable reception on account of the somewhat metaphysical—one might almost say mystical—form in which his results have been expressed."[67] He offered assurances that "the more concrete part of Einstein's work is quite independent of the metaphysical garment in which it has been clad." Say astronomers on Jupiter had determined an empirical law of refraction in the Jovian atmosphere by observing Zenith transits of the Jovian satellites. Jeans likened the changes wrought by Einstein's new theory of gravitation to what the Jovian astronomers would have to do to their law of refraction should a revolutionary mathematician in their midst demand a new one incorporating observations outside the vertical. The revolutionary mathematician, if a relativist, would assert that "the true laws of refraction can have no reference to chosen directions in space; technically speaking,

65. Eddington to de Sitter, 4 Jul 1916 (LC); de Sitter (ref. 63), 418.

66. T. J. J. See, "Einstein's theory of gravitation," *Observatory, 39* (1916), 511–512. For See's reputation see Crelinsten (ref. 3), 102–103, 273–275; Helen Wright (ref. 5), 118–119; and William Graves Hoyt, *Lowell and Mars* (Tucson, 1976), 119–123.

67. J. H. Jeans, "Einstein's theory of gravitation," *Observatory, 40* (1917), 57–58.

they must be invariant for all changes of axes." Jeans insisted that if the new laws of refraction were verified, the Jovian mathematician might suggest that the result "proved that refraction was a 'property of space,' or that 'horizontal and vertical had separately vanished to shadows.' These suggestions, however interesting they might be to metaphysicians, might conceivably prejudice the scientific acceptance of the new laws of refraction, but we can see that such prejudice would be unwarranted." Jeans claimed that Einstein's "crumpling up of his four-dimensional space" may likewise be considered fictitious. Though the theory brought the true laws of gravitation, it did not explain the nature of gravitation. "Gravitational theory has, so far, been trying to fit round pegs into elliptical holes: the new theory finds a new round hole into which the pegs will fit perfectly, but this does not explain the physical nature of the pegs."

"[It] is very well put," Eddington wrote de Sitter of Jeans' reply, "though he rather understates (probably not in his own mind but in the wording of the note) the so called metaphysical aspects of the theory." To readers of *Observatory* Eddington warned that Jeans' "excellent analogy" might lead to the "new conceptions of space and time being underrated," and he tried to defend "the 'metaphysical' garb in which the theory is usually clothed." Building on Jeans' analogy, Eddington pointed out that Jovians who had developed a law of refraction having special reference to the vertical might believe in a flat Jupiter. They would then hold the vertical to be a fundamental concept of geometry, with "*up* and *down* as different as right and wrong." In contrast with Jeans, Eddington took the notion of a curved space as a physical insight. "It is not a mystical theory," he asserted, "but a mere matter of obtaining sufficiently delicate measurements, to show that the space we have hitherto supposed we were measuring gets crumpled in a gravitational field." Eddington pointed out that the new law of gravitation could be compared only with difficulty to the old theory. To decide whether the new law conforms with the old idea of a force proportional to the inverse square of the distance, he noted, it is necessary to know what is meant by distance. "Neither the new conceptions nor the old suggest any one value from the various possibilities as being the true distance," Eddington emphasized. "Thus we are led to the metaphysical question immediately—What is distance? What is space?"[68]

The exchange between See, Jeans, and Eddington anticipated a major line in subsequent discussions of general relativity. The questions raised by See were not silly. The geometrization of physics that

68. Eddington to de Sitter, 31 Dec 1916 (LC); A. S. Eddington, "Einstein's theory of gravitation," *Observatory, 40* (1917), 93–95.

general relativity called for, while one of its transcendental beauties for de Sitter and Eddington, was metaphysical nonsense for others. Jeans urged an instrumentalist interpretation, perhaps more as a tactic than as a conviction. As he wrote Oliver Lodge, "I fear de Sitter has rather prejudiced the reception of Einstein's theory by a too abstruse presentation."[69] Discussion of these points of view, as well as attempts at classical explanations of relativistic effects, occupied British astronomers and physicists for several years.[70]

The discussion in Britain was taken up in the United States by the ever-vigilant Curtis, who noted that "the theory as originally put forward by Einstein has been subjected to so many alterations as now to be referred to as the 'old' theory of relativity." He referred to the replacement of "ordinary three-dimensional Euclidean space by a four-dimensional time-space." It was much to swallow:

> Many will feel that the idea of a four-dimensional time-space is fully as difficult of comprehension as was the mystery of gravitation, all-pervading, inexplicable, in our classical physical theories. While the mathematician is willing to admit that many other forms of space or geometries of space would satisfy physical science as well as the Euclidean, we must confess that we are still of the point of view of the mathematician who stated that, while it would be possible, in a four-dimensional universe, to turn an egg inside out without breaking its shell, still he realized that there were many practical difficulties in the way of the accomplishment of this feat.

Although in this context Curtis felt inclined to "sympathize somewhat with Professor See's point of view," he was mathematician or philosopher enough to find general relativity attractive because of "its unification of all matter and all forces in a single simple and homogeneous system."[71] In the end, however, he came out with See, rejected the geometricization of gravitation, and sought mechanical explanations for relativistic effects in the astronomical domain.[72] Two aspects of Curtis' later position characterize the climate of opinion regarding relativity among American astronomers around 1920: mistrust of the geometrical notions of the theory and avoidance of its details.

69. Jeans to Lodge, 14 Aug 1917 (UC).

70. See Moyer (ref. 2), esp. 56–70, who, however, sometimes reads history backward (cf. Crelinsten [ref. 3], 29–63, 100–101, 107–108; and Stanley Goldberg, "In defense of ether: The British response to Einstein's special theory of relativity: 1905-1911," *HSPS, 2* [1970], 89–126.)

71. H. D. Curtis, "Space, time, and gravitation," *PASP, 19* (1917), 63–64.

72. Crelinsten (ref. 3), 291–300, 325–360.

## The challenge of American observations

Einstein's full general theory of relativity called for a gravitational redshift the same in essence as that of the theory of 1911, and light bending double the amount predicted by the equivalence principle. The doubling arises from a combination time curvature and space curvature. The first, which dominates at small velocities, is equivalent in first approximation to Newton's law of gravitation; it emerges from any joining of special relativity and Newtonian theory, and so appeared in Einstein's theory of 1911. The second part of the the light bending came new with the general theory, and cannot be derived on Newtonian theories.

While Campbell and Curtis pursued light bending at Lick, their colleagues to the south at the Mt. Wilson Solar Observatory looked for the gravitational redshift in the sun. The spectroscopist Charles Edward St. John had begun an investigation of redshifts of 43 cyanogen band lines in the solar spectrum, which were known to be unaffected by pressure. At a meeting of the National Academy of Sciences on June 5, 1917, Hale read a preliminary announcement of results from St. John's investigations that would have a profound effect on opinions and attitudes toward Einstein's theory. St. John declared on the authority of Einstein's work of 1911 that "the equivalence principle of generalized relativity" led to predictions of redshift and light bending, the amount of the latter for a star near the limb of the sun amounting to 1".75. This mixing of the mechanism of 1911 with the prediction of 1916 indicates the tendency of the Mt. Wilson astronomers to gloss over theoretical details and concentrate on the results to be measured.[73] "The general conclusion to the investigation," Hale read out, "is that within the limits of error the measurements show no evidence of an effect of the order deduced from the equivalence relativity principle." St. John's report was widely published, and his general result—that the cyanogen lines do not show the red shift—was confirmed by Evershed.[74]

The news about cyanogen distressed Eddington, who was busy preparing his extensive *Report* on the relativity theory of gravitation for the Physical Society of London. "St. John's latest paper has been giving me sleepless nights—chasing mare's nests to reconcile the relativity theory with the results, or vice versa. I cannot make any headway."

73. C. E. St. John, "A search for an Einstein relativity-gravitational effect in the sun," NAS, *Proceedings, 3* (1917), 450–452; St. John obtained numerical values from Eddington, "Einstein's theory" (ref. 4).

74. C. E. St. John, "The principle of generalized relativity and the displacement of Fraunhofer lines toward the red," *ApJ, 46* (1917), 249–263, and St. John (ref. 73); J. Evershed, "The displacement of the cyanogen bands in the solar spectrum," *Observatory, 41* (1918), 371–375. Cf. Crelinsten (ref. 3), 114–116.

Nor did he. He wrote in his *Report*: "The difficulties of the test [the measurement of the predicted redshift in the solar spectrum] are so great that we may perhaps suspend judgement; but it would be idle to deny the seriousness of this apparent break-down of Einstein's theory."[75] The prestige of the Mt. Wilson group lent a great deal of weight to St. John's conclusions. But the observations were difficult, and threw into relief the importance of the search for gravitational bending of light.

Campbell had been continuing to pursue the problem well before news of general relativity reached the United States. A solar eclipse was visible from Colombia and Venezuela on February 3, 1916. Financial difficulties then plagued Lick and Campbell hoped that Perrine would send an expedition and make the test. Perrine did go, but not to attempt the Einstein test, since narrow finances prevented him from taking the necessary equipment.[76] Campbell's next chance would come in the summer of 1918, in the United States. Early in January 1917 his colleague at Lick, Robert Grant Aitken, called attention to the Einstein test and the old Vulcan problem in relation to the coming eclipse. He pointed out that telescopes of the type previously employed to search for intramercurial planets could be used to look for light bending, which he estimated at $0\overset{''}{.}9$, as in Einstein's theory of 1911.[77] By 1918 many U.S. scientists had been mobilized for war work, and little money was available for attending eclipses. Nonetheless, by pooling equipment and manpower, more than a dozen parties were organized.[78] Several of them looked for light bending during the eclipse, but only Campbell and Curtis got results. They were made known in a climate charged by St. John's negative results and by the international situation immediately after the Great War.

### The Lick expedition to Goldendale Washington

When war precipitated the Lick party's hasty departure from Russia, they left their instruments at the Pulkovo Observatory. Russian colleagues had promised to return them after the war, or as soon as transportation allowed. It was not until August 1917, under the Kerensky regime, that the instruments began their long journey East to Vladivostok. They reached the Pacific Ocean in December, weeks after the

75. Eddington to W. S. Adams, 28 Jan 1918 (HM); Eddington (ref. 4), 57.

76. Campbell to Perrine, 6 Aug and 20 Sep 1915 (LO); C. D. Perrine, "The total eclipse of February 3, 1916," *PASP, 28* (1916), 247.

77. R. G. Aitken, "A total eclipse of the sun," *PASP, 29* (1917), 25–40.

78. Katherine Bracher, "The famous eclipse of June 8, 1918," *Sky and Telescope, 58* (1979), 411–413.

Bolsheviks seized state power in western Russia. At Vladivostok, "the government, or the opposite of government," as Campbell wrote to a colleague, "put a business boycott in force there and nothing could be moved."[79] The boycott ended in April 1918, and the instruments eventually set sail for Kobe, Japan, whence it was hoped that a steamer would bring them to the west coast of the United States.

The line of totality for the eclipse was to pass diagonally across the United States from Washington in the Northwest to Florida in the Southeast. Campbell chose as his site the town of Goldendale, Washington, since it was the "westernmost" of all suitable stations, having "quick connection with Pacific Ocean ports" and his traveling instruments. Campbell was not sanguine, however, that they would arrive in time and arranged an alternate procedure:[80]

> Fearing that the special equipment would not be available, such portable equipment as the Lick Observatory possessed has been made ready in the past month, and several instruments have been generously loaned by the Students' Observatory and the Department of Physics at Berkeley. The expedition will be on a more modest scale than had previously been hoped, but the equipment will nevertheless be well worth while.

The equipment from Russia did not arrive in time, and the auxiliary instruments had to be used. Campbell's improvised inventory included several items. For the Einstein test he used a 4.5-inch photographic lens (focal length 15 feet), and a 3-inch Vulcan lens (focal length 11 feet 4 inches), both from the existing Lick equipment. He borrowed two 4-inch, 15-foot focal length photographic lenses from the Chabot Observatory in Oakland, California. These "Chabot lenses" were mounted on wooden tubes constructed at the eclipse station (see figs. 4a, 4b).[81]

Curtis, placed in charge of the Vulcan and Einstein cameras, went with the rest of the Lick party to Goldendale about two weeks before the eclipse. After about a week at the site, Curtis wrote Hale at Mt. Wilson: "We have had a fair average of weather to date and are hoping for good luck:—the most disquieting feature noticed to date lies in the fact that we are running *thirteen* instruments and the house we have rented possesses *three* black cats and a dog named '*Shadow*'!!!" The night before the eclipse was to occur, clouds moved in and it looked like the black cats would have their way. At the crucial moment,

79. Campbell to William M. Cake, 30 Mar 1918 (LO, Goldendale eclipse box).

80. Undated ms., likely a news release, late April 1918 (ibid.).

81. W. W. Campbell, "Return of eclipse instruments from Russia," *PASP, 30* (1918), 312, and "The Crocker eclipse expedition from the Lick Observatory, University of California, June 8, 1918," *PASP, 30* (1918), 220–240, on 229.

however, a break in the clouds moved before the sun. As Campbell described the drama: "The clouds uncovered the Sun and its immediate surroundings less than a minute before totality became complete, and the clouds again covered the Sun less than one minute after the total phase had passed." Ethel Crocker, the wife of Regent Crocker who paid for the expedition, witnessed the surprise clearing as a religious experience: "It *was* a miracle that little lake of blue sky in the centre of which was the phenomenon we had all gathered to see—God is very good to people that believe in his power to perform miracles."[82]

The *New York Times* ran a story by Campbell under the headlines "CLOUDS FALL AWAY FOR SOLAR ECLIPSE" and, "LICK EXPEDITION FAVORED."[83] Campbell explained the purpose favored by the weather:

> It is hoped that the measured positions of the recorded stars will serve as a test of correctness or falsity of the so-called Einstein theory of relativity, a subject which has occupied a foremost position in the speculation of physicists and others during the last decade....The test as an eclipse problem has never been made before, and it may be the only satisfactory test known to physicists, but whether our work will contribute evidence of value remains to be seen.

The dramatic event of an eclipse, always of interest to the lay public, was thus linked in the public mind with a controversial theory called relativity and with the name of Einstein, probably the first time either was mentioned in the North American press.

Curtis had the job of measuring the plates. A hasty examination revealed that stars fainter than the eighth magnitude had been recorded, although clouds had interfered somewhat with the stars farthest from the sun. Thereupon Curtis left Mt. Hamilton for Berkeley to help train navigators for the Navy. He had begun this work, supervised by Armin O. Leuschner of the Students' Observatory at Berkeley, in the summer of 1917.[84] His eldest son was a wireless operator in the Navy, and he himself wished to serve where he could. As Curtis explained to Hale: "[I] find it impossible to be content at astronomical work these days if I can be doing war work instead."[85]

82. Curtis to Hale, 2 Jun 1918 (HM); W. W. Campbell, "The total solar eclipse of June 8, 1918," *LOB, 10,* no. 318 (1918), 1–3; Ethel Crocker to Mrs. Campbell, 6 Jul 1918 (LO, Goldendale eclipse box).

83. "Clouds fall away for solar eclipse," *New York Times,* 10 Jun 1918.

84. Campbell (ref 81), 230; Campbell to Hale, 29 Aug 1917 and 22 Jul 1918, and Curtis to Hale, 2 Jun 1918 (HM); Campbell to Secretary of the Navy, 8 May 1918, Alfred E. Burton to A. O. Leuschner, 18 Jul 1917, and S. H. Levy to Campbell, 3 Aug 1917 (LO, Curtis folder no. 2).

85. Curtis to Hale, 27 Jul 1918 (HM).

FIG. 4a  Group photo of observers and guests assembled in front of the Einstein cameras at Goldendale, Washington, 1918. From left to right: Joseph H. Moore (Lick), his daughter Kathryn, A. H. Babcock (Southern Pacific Company), Warner Swasey, D. Campbell, Mrs. Campbell, E. P. Lewis (Berkeley), William H. Crocker, J. E. Hoover (Lick foreman), Mrs. Crocker, F. S. Bradley (San Francisco), W. W. Campbell, Samuel L. Boothroyd (University of Washington), Mrs. Plaskett, Heber Curtis (Lick), Estelle Glancey (Cordoba), J. S. Plaskett (Dominion Astrophysical Observatory, Victoria, B.C.), John Brashear, C. A. Young (Victoria), Mrs. Moore, Edward E. Fath (former Lick graduate), Mrs. Morgan (proprieter of lodgings at site). (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

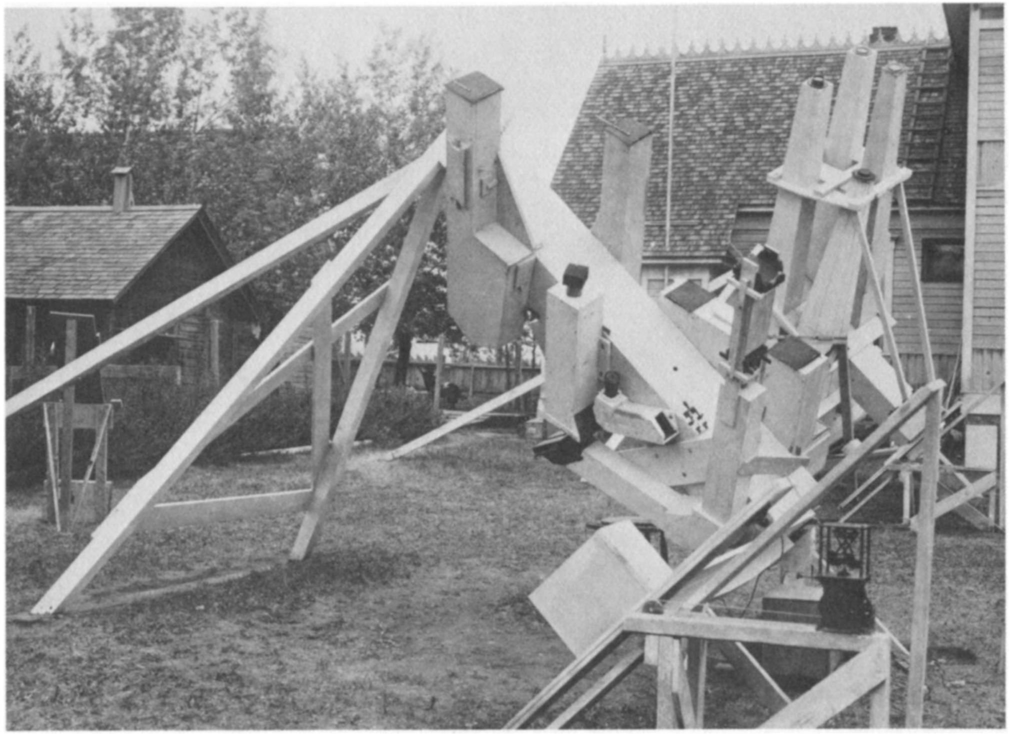

FIG. 4b Einstein cameras and spectrographs set up at Goldendale, Washington. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

There was also a technical reason for delay. Normally, the comparison plates of the region of the sky where the sun was during the eclipse would have been taken at Mt. Hamilton by night several months before the eclipse. Hoping for the arrival of instruments from Russia, Campbell and Curtis had not made comparison plates with the auxiliary equipment they finally used and had now to wait for the sun to vacate the region of sky of interest so that they could photograph it at night with the cameras used at Goldendale. This bit of work would be possible in the late fall or early winter. In August, however, Curtis left the West Coast for war-related work at the Bureau of Standards in Washington, D.C.[86] Campbell decided that nothing would be done with the Einstein plates until the comparison plates could be taken the next winter.[87] When the war ended in November 1918, Curtis decided to remain at the bureau until the summer of 1919. The Einstein problem was put on ice, despite requests from colleagues like Samuel L. Boothroyd, who was "anxious to get the latest evidence regarding the Einstein effect."[88]

## 3. 1919: A YEAR OF DRAMATIC ANNOUNCEMENTS

### Technical problems and a race for results

Early in 1919 Campbell consulted Curtis about taking the comparison plates. The star images on the eclipse plates were not clearly defined, and Curtis responded that for comparison purposes, the "double image and the little 'jump' will be very troublesome." At this early stage, neither astronomer knew that the poor definition of the star images on the Goldendale plates came largely from movement of the telescope mounting during exposure, which caused doubling and tailing of the stellar images. Curtis was inclined to blame the clocks and the lenses for the poor images. In taking the comparison plates, he advised, an auxiliary lens should be used "to guide in R.A. so as to get a good plate the first time." "With our good clocks in Russia," he reminded Campbell, "all our clocks at Goldendale were 'seconds'."[89]

At the end of February Campbell reported to Curtis that he and his colleagues had got comparison plates after some hard struggle. Three nights had started out beautifully, but each time the wind had made

86. Campbell (ref. 81), 230; Curtis to Campbell, 8 Jul and 5 Aug 1918, and Campbell to Curtis, 10 Jul 1918 (LO).

87. Campbell to Pickering, 13 Aug 1918 (LO).

88. Campbell to Curtis, 14, 27, and 30 Nov 1918; Curtis to Campbell, 8 Dec 1918; Boothroyd to Campbell, 31 Dec 1918 (LO).

89. Campbell to Curtis, 6 Jan 1919, and Curtis to Campbell, 13 Jan 1919 (LO).

trouble for them. They had used a guiding telescope as Curtis had suggested, "and that helped immensely."[90] Campbell decided that "at the 1923 eclipse the Vulcan mounting, etc., must be designed anew, and to include a guiding telescope and other conveniences and necessities." The eclipse of 1923 would be visible from Southern California and Mexico. Evidently in February 1919 Campbell had no intention of observing the eclipse of the following May, or that forecast for 1922, which would be visible from Australia.

With the comparison plates in hand, Campbell proceeded to design a differential measuring apparatus. He described his plan to Curtis:

> It seemed to me that we could construct, solidly, a framework to hold the positive of a Goldendale plate and the corresponding Mount Hamilton negative face to face vertically in front of a north or west window, with a large ground glass between them and the window; that a sort of double slide, of strong design and accurately constructed in wood, could hold a micrometer eyepiece in front of the plates, so that the eyepiece could be shifted by finite jumps over the whole area searching for any Goldendale objects not duplicated at Mount Hamilton, and measuring the distances between the corresponding images.

Curtis doubted whether the fainter stars would be made out as well on a positive as on the original negative, and preferred a procedure that would not require positives at all. Curtis wanted to have two glass scales, 16 inches long, ruled at the Bureau of Standards to use instead of a steel or wooden scale. Since these would be highly accurate, he hoped they would enable him to measure the negatives of both sets of plates (Goldendale and Mt. Hamilton) absolutely in rectangular coordinates, "and get rid of scale and orientation error by actual solution over the entire plate." He conceded that the differential scheme "would probably work all right, though I am a little afraid of the difficulty of allowing adequately for scale and orientation differences in such a method." Curtis enclosed a sketch of the apparatus he had in mind, "with suggestions as to the design,"[91] shown in figure 5. Campbell grew increasingly anxious about Curtis' return to Lick to work on the Einstein plates. That suited Curtis, who had grown tired of peacetime Washington, and it was agreed that he would be back on the mountain by May 1.[92] What fired up Campbell was the news that the British intended to send two expeditions to observe the May eclipse, and that they would concentrate on the Einstein problem. Campbell wanted to be first in announcing definite results:[93]

90. Campbell to Curtis, 28 Feb 1919 (LO).
91. Curtis to Campbell, 9 Mar 1919 (LO).
92. Campbell to Curtis, 13 and 28 Feb, 3 Mar 1919 (LO); Curtis to Folks, 22 Feb 1919 (LO copy from MA).
93. Campbell to Curtis, 3 Mar 1919 (LO).

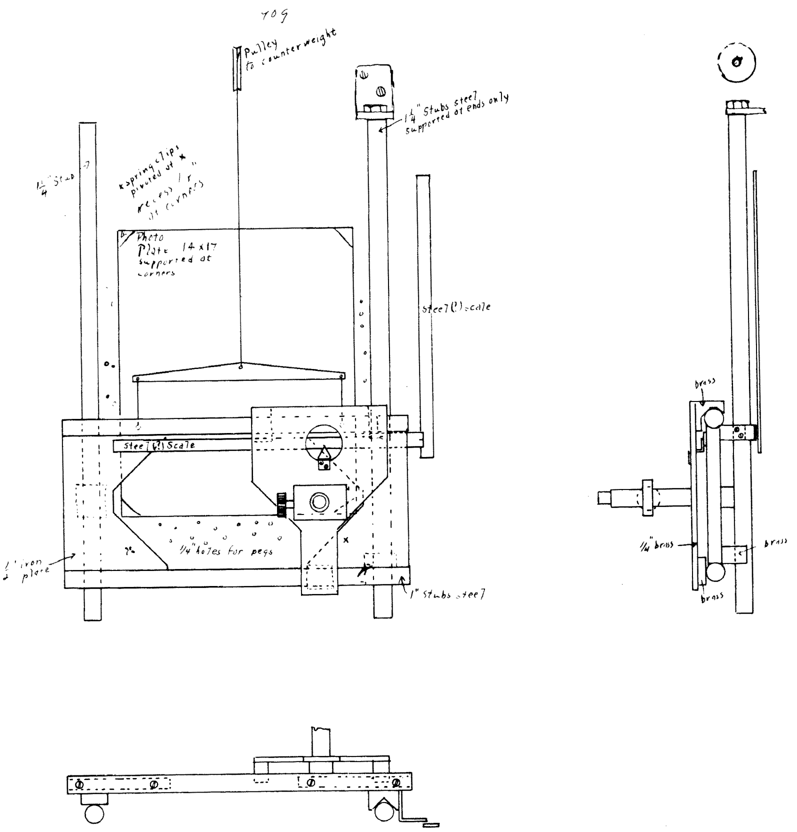


FIG. 5 Curtis' sketch of March 1919 for constructing machine to measure the Goldendale eclipse plates. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

> I am really anxious to have you get out of the Vulcan-Einstein photographs whatever may be in them very promptly, not only on general principles, but because our friends across the water are sending expeditions to Brazil and Africa for the eclipse of this year, devoting themselves intensively to the Einstein problem. It behooves us to get what there is coming to us, quite promptly. So I am deciding to count pretty heavily on your advice as to what comparing and measuring apparatus to get ready for your arrival.

With the pressure on, Campbell deferred to his colleague's advice on the matter of the measuring machine. He authorized Curtis to order steel rods for use as traveling guides upon which the optical part of the measuring apparatus would ride. He urged Curtis to proceed as quickly as possible with the working drawings, as he would like to put the Lick shop men onto the project in early April. "That may rush you a little," he admitted, "but I am anxious to have the apparatus well along to completion when you arrive, so that you may have something to report pro and con at the Pasadena meeting of the Astronomical Society of the Pacific in connection with the Pacific Division of the 3 A.S. [AAAS] about June 19–20."[94] So Campbell was pushing for an announcement of preliminary results by the middle of June, two weeks after the May eclipse and well in advance of any likely substantive statement by the British expeditions.

In late March Curtis was working feverishly on the project. Although he agreed to try Campbell's differential method first, he was having the glass scales ruled up at the Bureau "as an 'anchor to windward' in case that scheme does not work." His plan was "to put the plates on in the same position, so that I shall use the same division of the glass scales for the eclipse plate and the comparison plate." Absolute measure was then the standard practice, the differential method being Campbell's innovation to suit the problem at hand. If it didn't work, Curtis remarked that "we shall have the real glass scales to fall back on in case we have to measure the plates in the ordinary way."[95]

The steel rods Curtis ordered were not perfectly straight. Rather than lose time in an uncertain attempt to obtain straighter ones, Campbell decided to use them. Their fault ruled out Curtis' absolute method. "I think we are going to be obliged to depend upon differential measures," Campbell decided. "We cannot hope for accuracy of the slides except in an instrument costing a thousand or two dollars, which would let us use absolute values of the coordinates."[96] Differential measures were used, but with reference to the glass scales, rather than by using a micrometer to measure the actual distance between each eclipse star image and its companion image on the comparison plate. This hybrid technique would lead to problems.

By about the middle of April all was ready for Curtis' return. Yet Campbell could hardly have been pleased with the situation. The Goldendale eclipse plates, taken with improvised equipment, were not of first quality; and rival Einstein testers in the form of two British expeditions were waiting at separate locations to photograph the star field around the sun during an especially favorable eclipse, with over five minutes totality and thirteen bright stars of the Hyades in the field.[97]

94. Campbell to Curtis, 17 Mar 1919 (LO).

95. Curtis to Campbell, 22 Mar 1919 and cf. same to same, 24 and 25 Mar 1919 (LO).

96. Campbell to Curtis, 8 Apr 1919 (LO). When two of the larger steel rods were placed side by side with two ends in contact, the other ends stood as much as a quarter of an inch apart.

97. W. W. Campbell, "Total solar eclipses of the near future," *PASP, 30* (1918), 256–257; F. W. Dyson, "On the opportunity afforded by the eclipse of 1919, May 29, of

Moreover the entry of the British raised the stakes of Einstein testing. Eddington had convinced British astronomers that the theory of relativity had fundamental implications not only for gravitation theory, but for basic concepts of physics and astronomy. Campbell had been interested in the Einstein test partly as an instrumental challenge, and partly as a judgment about a prediction of a controversial theory. The British were looking into a possible revolution in science. Their excitement gave a new urgency to the project Campbell had been engaged on for over five years.

### Campbell's attitudes and Curtis' results

Campbell's attitude toward relativity and the eclipse test around 1919 appears clearly from his correspondence with Arthur Hinks, formerly chief assistant at the University Observatory, Cambridge, who had resigned in 1914 rather than serve under Eddington. Hinks opened the exchange: "Now that Peace is in sight," he wrote, "I find my thoughts reverting to astronomy a little, and I hope eventually to finish off some things I had to leave incomplete in 1913." He did not expect to take up newer developments. "The statistical stuff with its integral equations was bad enough. But relativity is much further beyond the limits of my comprehension, and I shall find when I start to make up my two years arrears of reading that I am hopelessly outclassed." Campbell replied that "most astronomers could conscientiously make the same confession." As for himself, he said, "I have not yet made up my mind what to think of 'relativity' as applied to our subject. I have not attempted to go through the mathematics, but the applications have interested me very much in a general way. Eddington is rendering valuable service in keeping us posted on the applications and implications."[98]

The application of greatest interest to Campbell concerned Curtis' measurement of the 1918 eclipse plates. Writing Hinks on June 2, 1919, he anticipated results in three or four days. "We hope that a week of intensive computing may [then] give us at least a hint as to what the final results of his work will be." Campbell favored a negative result. "I must confess that I am still a skeptic as to the reality of the Einstein effect in question, but I would not be willing to undertake a technical defense of my skepticism. I am quite ready to welcome a positive result, though I am looking for a negative one." Campbell

98. LO. Hinks folder. Hinks to Campbell, 1 Jan 1919, and Campbell to Hinks, 2 Jun 1919 (LO).

retained this open-minded skepticism until working through results from the Australian eclipse of 1922.

Campbell left California for a meeting in Washington before Curtis had anything definite. But by the middle of June Curtis could speak, as he did to Charles Burkhalter of the Chabot Observatory. "You will be interested to know that the plates taken at the Goldendale station for the Einstein effect have given good results on measurement; these were taken with the two Chabot lenses which you loaned to us." As an extra line of evidence, Curtis had measured up plates taken at the eclipse of May 1900, in Georgia. The results of both sets of measures came out negative. "I expect to do some more measuring before publishing, but the conclusions from these plates and from the 40-foot plates taken in 1900 is very definite to the effect that the Einstein effect does not exist, and that there is no deflection of the light ray when passing through a strong gravitational field."[99]

The additional measures Curtis contemplated included ones on plates taken by Burckhalter with the Chabot lenses. "You have some valuable material, as it turns out, bearing on this same problem, taken with these same lenses at the 1900 eclipse." Curtis thought it "probable that these may prove considerably more valuable than the Goldendale plates, because of favourable arrangement of relatively bright stars." Curtis prepared an extensive paper on the results of his measures of the Goldendale plates and the 1900 Vulcan plates for the Astronomical Society of the Pacific meeting in Pasadena. He kept on, measuring and remeasuring, always with the same result. "Recent measures only corroborate the results sent in my paper, [he wrote Hale] namely, that there is no Einstein effect."[100]

In his talk for the Pasadena meeting (which, as he did not attend, was read for him), Curtis considered both the values for light bending discussed by Einstein. He observed that the value of 1911 applied "quite apart from any theory of relativity, if light is acted upon by a gravitational field in the same way as ordinary matter. The problem may then be described [in Eddington's phrase] as an attempt to weigh the light ray."[101] As for the full general relativistic value, Curtis merely stated it without attempting interpretation: "In Einstein's later theory,

99. Curtis to Burckhalter, 16 Jun 1919 (LO); cf. W. W. Campbell, "The total eclipse of the sun, September 21, 1922," *PASP, 35* (1923), 11–44, on 18.

100. Curtis, "The Einstein effect: Eclipse of June 8, 1919 (Preliminary paper)," 9pp. (LO, Curtis file 3); Curtis to Campbell, 23 Jun 1919 (LO); Curtis to Hale, 23 Jun 1919 (HM).

101. Curtis, *Einstein effect* (ref. 100). The uninformative published abstract reads, in toto, "The result of measures on plates taken with the Vulcan cameras at Goldendale, Washington, at the eclipse of June 8, 1918." H. D. Curtis, "The Einstein Effect: Eclipse of June 8, 1918," *PASP, 31* (1919), 197.

as published in 1915, he postulates a relativity deflection of twice the former amount, or 1".75 for a star at the sun's limb." Curtis considered his measurements as arbiters not only between Einstein's values, but also between "the numerous physical theories bearing on the nature of light, the ether, and the structure of matter" for which "data bearing on the behavior of light in a strong gravitational field will be of great value." The arbitration had proceeded as follows. Two cameras had been used at Goldendale, with a clock drive that proved slightly defective. "The resulting star images were not of first quality, showing a slight elongation, but are fairly satisfactory for measurement." A total of fifty-five stars were found on the plates, some too faint to examine quantitatively; 43 to 49 images on each plate could be measured. The region immediately around the sun was "rather poorly represented," with stars 40′ to 2°30′ from the sun being picked up.

Curtis compared all images by a differential method so that "slight errors in the straightness of the ground steel ways, departure from rectangularity, scale error, etc., might have no effect." Although he used the differential method to solve for scale and orientation errors in the usual way, he determined star positions by reference to the glass scales he had had made at the Bureau of Standards. Let $X$, $Y$ be the measured coordinates of a star, and $n_1$ the difference between the $X$ coordinates for the eclipse and the corresponding comparison plate, and $n_2$ the difference between the $Y$ coordinates for the same pair. According to a method of reduction devised by Herbert Hall Turner,

$$\left.\begin{aligned} aX + bY + c &= n_1 \\ dX + eY + f &= n_2 \end{aligned}\right\}, \qquad (2)$$

where $a$, $b$, $c$, $d$, $e$, $f$ are constants due to the effects of orientation of the plates, scale difference, refraction, etc. Curtis got a set of equations for each star, measuring the plates both direct and reversed, and solved the equations by least squares.

The effect of refraction had been minimized by taking the comparison plates at such an hour angle that the refraction, estimated under Mt. Hamilton winter conditions, should be the same as for the eclipsed region photographed at Goldendale. In the reduction, Curtis pointed out, "we have to consider in the method employed only the difference in the differential refraction. As the refraction for the originals and the comparison plates was practically identical, this second differential is linear over the entire plate far within the limit of accuracy required." Earlier Curtis had worried that "the increase of altitude above sea level of the instrument... will completely change the refraction conditions, and the effects will have to be determined separately from the plates themselves for each set."[102] He now felt that the refraction conditions

102. Curtis to Campbell, 13 Jan 1919 (LO).

had not differed as radically as he had expected. He estimated that the probable error of a single star place ranged from 0".4 to 0".6, and thought that further analysis might reduce it further.

In the final procedure, the differential displacements between stars on the eclipse and comparison plates, or "residuals" as Curtis called them, were corrected by Turner's method and "projected on the line joining the center of the sun and the star." In this way, Curtis obtained the radial components of the "corrected residuals" for each star in the eclipse field. He could then present his results in two ways. For one, he arranged the stars in order of distance from the sun's center. If the Einstein effect was real, then there should have been a systematic decrease of displacement from the nearer stars out to the further ones. Curtis reported that "no regular run of differences can be made out between residuals of the closer and more distant stars." Alternatively, Curtis divided the stars into two sets: "an inner group of twenty stars, mean distance 68'.9, and an outer group of from twenty-three to twenty-nine stars at a mean distance of 124'.6." The results, projected as a slide at the meeting, are given in table 3.

TABLE 3

| | INNER GROUP 20 STARS AVER. DIST. 68'.9 | OUTER GROUP 23 TO 29 STARS AVER. DIST. 124'.6 | EXCESS OF INNER GROUP (+ = EXPANS.) |
|---|---|---|---|
| Plate #2, direct | −0".03 | −0".22 | +0".19 |
| Plate #2, reversed | +0.09 | −0.09 | +0.18 |
| Plate #3, direct | −0.09 | +0.01 | −0.10 |
| Plate #3, reversed | +0.04 | +0.17 | −0.08 |
| Average expansion of innner group of 20 stars on four plates | | | +0".05 |
| Predicted expansion from Einstein's later theory (1".75) | | | +0".18 |
| Predicted expansion from Einstein's first theory (0".87) | | | +0".09 |

The results from Plate 2 were in excellent agreement with the prediction based on general relativity. Curtis only considered the mean from the two plates, however, which was proper scientific procedure, and he concluded that the results "indicate no expansion of the inner group of stars such as is called for in Einstein's later theory, and, less definitely, pronounce against the smaller value predicted from his earlier theory." This outcome was confirmed by analysis of the plates from the eclipse of 1900. Six bright stars were found on the 16-second and 8-second exposures, and the images were better than those obtained at Goldendale. On the 8-second exposure plates they were "particularly fine," Curtis reported, "with small, round sharp images susceptible of very accurate measurement." The stars were also closer to the sun than any on the Goldendale plates. Since a 40-foot focal length camera had been used, the accuracy was increased manyfold, "as on these 40-foot telescope plates 1mm. = 16."9." The Chabot lenses were less than 15-foot focal length each, and the scale value on the Einstein plates was approximately 1mm. = 45″. Of course, for the 1900 eclipse, no comparison plates had been taken. Curtis therefore measured the six stars and compared the measures "with the rectilinear coordinates given for the region in the Paris zone of the Carte Photographique du Ciel,"[103] being forced in this case to use the absolute method of measuring corrected by Turner's method.

Curtis divided the six stars into two groups, furthest (stars A,D,E,F) and closest (B,C) to the sun, and gave their displacements (table 4). The displacements were an order of magnitude less than what Einstein's theory predicted. In sum: "It is not believed that the proposed further working over of the Lick material will change the conclusion derived from this present investigation, namely, that there is no deflection of the light ray produced when the ray passes through a strong gravitational field, and that the Einstein effect is non-existent."

## Spreading the news

Although attendance at the Pasadena meeting was fairly light because postwar conditions minimized travel and many men were still abroad or winding down war work, two circumstances ensured that many astronomers heard about Curtis' results. First, the meeting took place at Mt. Wilson, and Hale, who had decided to send others to the IAU in Brussels, attended. He was very pleased with Curtis' results: "Accept my hearty congratulations on the results of your eclipse work [he wrote]. I confess that I am much pleased to hear that you find no evidence of the existence of the Einstein effect. I listened to your paper at the A.S.P. meeting with a great deal of interest, and was really delighted with the results obtained."[104] Hale was the nerve-center of much of American astronomy and science, and he was sure to spread such excellent news, particularly as the negative results concurred with St. John's. Campbell had missed the Pasadena meeting to go East to head a delegation representing the American section of the IAU and to sail abroad for the meeting in Brussels. From Washington he cabled Curtis for results suitable to present at a special meeting at the Royal Astronomical Society in London. He asked for "Number, limiting magnitudes stars, probable errors," and added a caution, "careful about

103. Cf. Herbert Hall Turner, *The great star map, being a brief general account of the international project known as the astrographic chart* (London, 1912).

104. Hale to Curtis, 24 Jun 1919 (HM).

TABLE 4

Results, 1900 Georgia eclipse; 40-foot telescope.
1mm. = 16".9

| | LATER THEORY (1".75) | EARLIER (0".87) |
|---|---|---|
| Average deflection predicted by theory for outer group, stars A,D,E,F | +0".54 | +0".27 |
| Average deflection predicted by theory for inner group, stars B and C | +0.84 | +0.42 |
| Excess of B, C, by theories | +0".30 | +0".15 |
| Sum of deflections, from measures A,D,E,F | +0".059 | |
| " " " " " B,C | +0.054 | |
| Average deflection, from measures A,D,E,F | +0".015 | |
| " " " " B,C | +0.027 | |
| Excess of B, C, by measures | +0".012 | |

proper motions 40-foot plates." The positions of the stars on the Georgia eclipse plates had been compared with their positions 3½ years earlier, as determined from photographs taken on November 30, 1896 for use in the *Carte du ciel.* Campbell wanted to be sure that the stars had not moved in the interim. Curtis telegraphed that the proper motions would be negligible; he reported a probable error of 0″.4 to 0″.6 for the Goldendale plates and 0″.05 for the Georgia plates. Curtis assured Campbell: "I am confident that there is no Einstein effect whatsoever; have gone over results again and you can make it as strong as you like."[105]

Campbell sailed for Europe on June 30 in company with seven colleagues: Walter Sydney Adams, Frederick H. Seares, and Charles Edward St. John of Mt. Wilson; Lewis Boss of Dudley Observatory; Frank Schlesinger of Allegheny Observatory; Samuel Alfred Mitchell of Leander-McCormick Observatory; and Joel Stebbins of the University of Illinois. On board ship, the eight American delegates had a series of meetings to discuss the approaching session of the IAU. As part of the proceedings, Campbell presented Curtis' results from the 1918 eclipse in a formal session entitled the "Einstein effect."[106]

The special meeting of the Royal Astronomical Society took place on July 11, four days after the Americans arrived. All of them spoke at the meeting.[107] As chairman, Campbell was asked to speak first, and he presented Curtis' results on the Einstein effect. After summarizing the main elements of the work, including the troubles that had beset the enterprise from the beginning, he reported that Curtis had arranged the corrected differences of position for each star in order of distance from the sun, and that he "was not able to say that there was anything systematic about these differences, which showed no change of the order required by Einstein's second hypothesis." Campbell noted that the error was "regrettably large," and that a telescope of long focal length would have been a great help. "For the one we used," he explained, "the stars were too faint and in the long exposure required we suffered from the increased extent of coronal structure." He then described Curtis' method of dividing the stars into an inner and outer group, and noted that Curtis obtained a differential displacement between the two groups of 0″.05, whereas it "should have been 0″.08 or 0″.15, according to which of Einstein's hypotheses was adopted." In discussing Curtis' examination of the 40-foot Lick plates from the 1900 eclipse, Campbell

105. Campbell to Curtis, 22 Jun 1919; Curtis to Campbell, 23 and 24 Jun 1919 (LO).
106. "Record of meetings of the delegation to Brussels," p. 2 (NA, International Astronomy Union, American Section).
107. Joel Stebbins, "Report on the organization of the International Astronomical Union," 8pp. (AAS).

noted that it would be useless to take a duplicate photograph now "owing to uncertainty in the values of the proper motions." He reported that reference had been made to the Paris plates in the *Carte du ciel,* "but Curtis was unable to say from the comparison that the innermost star showed a displacement due to the Einstein effect." Campbell concluded: "It is my own opinion that Dr. Curtis' results preclude the larger Einstein effect, but not the smaller amount expected according to the original Einstein hypothesis."[108] The Americans voted no to Einstein.

After Campbell had spoken, the president of the RAS, Alfred Fowler, asked the Astronomer Royal, Frank Dyson, to relate news from Eddington's eclipse expedition. "Prof. Campbell could not have chosen a more interesting subject just now than the problem of relativity," Dyson remarked. "It is an extremely difficult question to settle." He had received a letter from Eddington two days earlier, expressing great disappointment over his results at the recent eclipse. Out of 16 photographs secured, only the last six showed any stars on them, the first ten being ruined by cloud. No more than five images appeared on any of the six plates, and on none were the images well distributed. Nonetheless, Eddington was hoping "to get good enough measures to determine the displacement definitely....From his best plate, however, he has some evidence of deflection in the Einstein sense, but the plate errors have yet to be fully determined." Dyson added that the sky was clear ten minutes after totality. Meanwhile, back on the mountain, Curtis was pressing on with remeasurement of the plates and revision of his Pasadena paper. Early in July he was handed a letter addressed to Campbell from T. J. J. See, whose piece in the *Observatory* had elicited a comment by Curtis in the Pacific Astronomical Society's *Publications*. Knowing that Campbell had looked for the Einstein effect, See asked him "whether the Einstein calculations point to a refraction of the ray which throws the star apparently farther from the sun than it actually is at the time of observation? That is, is the Einstein effect like that of a slight atmosphere about the sun?"[109] See claimed that "in my own researches I have reached some very remarkable results, and I may be able to throw light on this problem. Hence I wish to be sure of the Einstein conclusion." Curtis replied for Campbell, mentioning Einstein's two values, referring the earlier to relativity and gravity, the other to "a four-dimensional time-space manifold." He did not hint why or how the mechanism for the bending changed from the earlier to the later relativistic prediction. Neither in any case agreed with Curtis'

108. "Meeting of the Royal Astronomical Society, Friday, July 11, 1919," *Observatory, 42* (1919), 298–299.
109. See to Campbell, 3 Jul 1919 (LO).

observations: "So far as I have gone in the work of measurement, the conclusion seems very definite that there is no marked deflection of the light ray when passing through a strong gravitational field, and that the Einstein effect is non-existent. I do not believe that the proposed further working over of the Lick Observatory material will change this conclusion. I can, however, speak more definitely on this point a month from now, by which time I hope to have additional measures made."[110]

In the summer of 1919, Eddington was almost single-handedly trying to educate English-speaking astronomers to the beauty and plausibility of general relativity. In certain ways he misled more than enlightened. His insistence that the eclipse expeditions were setting out to "weigh light" gave a Newtonian ring to his expositions of the theory. Though he sometimes distinguished analogies, directed to correlating the new ideas with the old ones, from more precise formulations of the new conceptions, more often than not the Newtonian analogies were retained by those relying on his writing for an understanding of Einstein's theory. And Eddington faced an uphill struggle against the data. In the theoretical vacuum that predominated among American astronomers, the matter was being arbitrated with observations, and both Lick and Mt. Wilson had not found the relativity effects that they had set out to measure. In the case of the gravitational redshift, the British researcher in the same field, John Evershed, concurred with the prestigious Mt. Wilson group. In the case of the bending of light, the highly-regarded Lick observers, although recognizing some imperfection in their data, thought it weighty enough to crush Einstein.

## The tide turns: Lick goes underground and the British verify relativity

In Europe Campbell continued to discuss Curtis' results with various people and became increasingly concerned about the "regrettably large" probable errors in Curtis' measurements. The Goldendale errors were of the order of 0".5, about an order of magnitude larger than routine parallax measurements. Five days after he had spoken at the Royal Astronomical Society, he sent a cable to Lick: "Both Curtis Einstein Results Small Weight Errors Large Use Cautiously."[111] Curtis wrote immediately to ask Robert Aitken, editor of the *Publications* of the Astronomical Society of the Pacific, to withhold his Pasadena paper from the coming issue. He told Aitken about Campbell's cable:

110. Curtis to See, 5 Jul 1919 (LO, See file).
111. Campbell to "Astronomer," 16 Jul 1919 (LO).

> This coincides with my own view on the matter, although I think an effect of the size of the value predicted by Einstein would have shown up. Then the 40-foot plates and Burckhalter's depend still on the Carte du Ciel in the one case, and a combination of Boss and Paris in the other. These two must wait for comparison plates taken with the same lenses, to be as certain as we can make them, and these plates can not be taken till August 15.

In case the printer had already set the type, making withdrawal impossible, Curtis would have provided a final paragraph "calling attention to the fact that, while the plates, as treated, indicated no marked expansion, still the probable error is large, and final decision must await the taking of additional comparison plates for the 40-foot and Burckhalter plates."[112]

Curtis still believed that he had ruled out the Einstein effect. He had already made some improvements in the measuring engine since the first measures, and he told Aitken that the probable errors had been reduced, though they were "still rather large." The six closest and easiest stars to measure for the Goldendale eclipse "show no expansion, but a contraction of 0".9." So the results still seemed to be negative. In the end, there was time to withdraw the paper, and a substitute never appeared. Another cable arrived, this time from Brussels. It read: "Delay Publishing Einstein Results. Campbell."[113]

Campbell sailed from Europe in the middle of August and arrived at Mt. Hamilton at the beginning of September.[114] A little more than a month later, at a joint meeting of the Royal Society and the Royal Astronomical Society, the announcement was made that the British eclipse had found the full deflection of stellar rays by the gravitational field of the sun, as predicted by Einstein's general theory of relativity. The startling news was picked up by the newspapers, from which See among others learned of the British claim. He found room for doubt: "Just how they could be sure of 0".87 [!] on plates covering several degrees of space, and thus being a quantity of the order 150000 I do not see, but not wishing to form hasty opinions I have written to the Astronomer Royal for light." See asked Campbell if he could send any further data on the Lick effort. "My *New Theory of the Aether* is now being arranged for the printer," he announced, "and I have so many new results that I am drawing no conclusions as to what may exist. My

112. Curtis to Aitken, 16 Jul 1919 (LO). "Paris" referred to the *Carte du ciel* and "Boss" to a catalogue of proper motions published by Lewis Boss of Dudley Observatory in 1904. Cf. Waterfield (ref. 10), 120.

113. Campbell to "Astronomer," 21 Jul 1919 (LO).

114. Campbell, telegrams of 13 and 27 Aug 1919; Campbell to Cunard Steamship Company, 4 Sep 1919 (LO).

work throws definite light on the field about the sun, just as my work on the Lunar Fluctuations illuminated that subject."[115]

Having received no reply by early December, See wrote Campbell again:

> I want the Lick view, independently of that sent out from London, of which I am somewhat sceptical, especially as Curtis wrote in June that there was no trace of Einstein Effect. So far as I can judge the quantity sought (0".87) is likely to be one part in 10000 of angular space, and thus I am aware of the difficulty of experimental detection, even by the most perfect superposition of plates. But I am not judging hastily, as I have proof of a new law of Density of the Aether about the Sun.

Campbell answered promptly this time, to the effect that Curtis was still measuring and reducing the data.[116]

4. THE POSTWAR PERIOD: 1920–1923

In the heady climate following announcement of the results of the British eclipse expeditions, astronomers' words and deeds acquired a larger import than they might have carried in a strictly professional context. Many of them were caught in the awkward position of trying to explain to an interested public a theory that they neither understood nor approved. Attitudes toward relativity polarized as journalists and others tried to extract a definitive statement from their scientific fellow citizens. Campbell tried to stay clear of the theoretical debate raging around him, and concentrated on developing the observational techniques to the highest possible level of refinement, accuracy, and dependability.

### Reactions to the British eclipse results

Frank Dyson at Greenwich Observatory, the prime mover of the British eclipse expeditions, accepted the results as verification of Einstein's prediction. Writing to Hale toward the end of December

115. "Revolution in science," *London Times,* 7 Nov 1919, p. 12; "Eclipse showed gravity variation," *New York Times,* 9 Nov 1919, p. 4; Jeffrey Crelinsten, "Einstein, relativity, and the press: The myth of incomprehensibility," *The physics teacher, 18* (1980), 115–122; See to Campbell, 16 Nov 1919 (LO); cf. "Joint eclipse meeting of the Royal Society and the Royal Astronomical Society," *Observatory, 42* (1919), 389–398; 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, *Transactions, 220* (1920), 291–333, reprinted in RAS, *Memoirs, Supplement* (1920), 1–43.

116. See to Campbell, 6 Dec 1919; Campbell to See, 8 Dec 1919 (LO).

1919, he announced that at Greenwich they "were very satisfied with the eclipse results, as they seemed definitive." He admitted that he personally had been "a sceptic" and had expected a different outcome. "Now I am trying to understand the principle of relativity," he related, "& am gradually getting to think I do."[117]

Dyson was aware that many astronomers might question the results of the British eclipse observers, and he sent Hale a print of one of the Sobral photos. "Naturally our opinion depends on whether the observational material was good," he explained, "& so I am going to distribute a few similar copies to show the goodness of the images." He also sent a print to Campbell, "so that you can see what the star images look like," and to Frank Schlesinger of Allegheny Observatory.[118]

Hale agreed that the star images were good and admitted that his understanding of Einstein was bad.[119]

> I congratulate you again on the splendid results you have obtained, though I confess that the complications of the theory of relativity are altogether too much for my comprehension. If I were a good mathematician I might have some hope of forming a feeble conception of the principle, but as it is I fear it will always remain beyond my grasp.... However this does not decrease my interest in the problem, to which we will try to contribute to the best of our ability."

Hale advised Dyson that it was not clear whether St. John's results could be applied as a test, but promised that his colleagues would work toward "leaving no doubt whatever regarding the position of the solar lines."

Hale's old friend Hugh Frank Newall, director of the Solar Physics Observatory at Cambridge, tried to develop a theory of an extended atmosphere around the sun, with refractive properties capable of deflecting the light from the stars to the sun's vicinity. Newall articulated his position at meetings of the RAS and in print for months after the eclipse results were announced, and had the pain of being systematically refuted by "relativists" like Dyson and Frederick A. Lindemann.[120] When Newall shared his doubts about relativity with Hale,

117. Dyson to Hale, 19 Dec 1919 (HM).

118. Dyson to Campbell, 29 Dec 1919 (LO); Schesinger to Dyson, 16 Feb 1920 (SM).

119. Hale to Dyson, 9 Feb 1920 (HM).

120. Joint eclipse meeting (ref. 115), 395–397; "Meeting of the Royal Astronomical Society," *Observatory, 42* (1919), 421–430, on 428–429; H. F. Newall, "Note on the physical aspect of the Einstein prediction," *MNRAS, 80* (1919), 22–25; "Proceedings at meeting of Royal Astronomical Society," *Observatory, 43* (1920), 145–148. Dyson and Lindemann pointed out that comets do not appear to slow down near the sun, as they ought to do should a significantly refractive gas exist there. Cf. "The revolution in science, astronomers' discussion," *London Times* (15 Nov 1919), 14; Crelinsten (ref. 115), 120–122.

he could not extract informed sympathy. "I cannot pretend... to have the smallest comprehension of the theory of relativity [Hale replied], and probably I shall never reach a much higher state."[121]

As a specialist in stellar photography, Frank Schlesinger was ready to accept provisionally that "the photograph [sent by Dyson] leaves little doubt that the deflection is close to the total amount," although he wanted the test repeated. "I trust you are planning to observe the 1922 eclipse in the same way," he wrote, "as I am sure you will be among the first to agree that results of such importance should be thoroughly confirmed before we accept them as establishing Einstein's theory." On the theoretical side, however, Schlesinger hesitated. "I have a feeling that some explanation for the deflection will be found without resorting to non-Euclidean space."[122]

Dyson's reply indicates that he had worked a little more carefully with the details of the theory. Emphasizing that "I put a great deal of reliance on St. John," he explained that "for myself am only prepared to say that the law of gravitation is $\delta\int ds$ is stationary when $ds^2 = (1-\frac{2m}{r})dr^2 - r^2d\theta^2 - r^2\sin^2\theta dr^2 + (1-\frac{2m}{r})dt^2$ with suitable units."[123] Dyson also mentioned future expeditions: one to Christmas Island in 1922, and possibly another to the Maldives. He hoped that an American expedition would be sent "in view of the importance of having the point thoroughly settled.... I have tried to understand the Relativity business, & it is certainly very *comprehensive*, though elusive and difficult."

Hale's and Schlesinger's attitudes exemplify professional opinion immediately following announcement of the British eclipse results. The scepticism and ignorance helped keep up public interest in a theory that appeared to elude the grasp of most scientists. The physicist Ernest Rutherford wrote to Hale from Cambridge:[124]

> The interest of the general public in this work is most remarkable and almost unexampled. I think it is due to the fact that no one can give an intelligible explanation of the same to the average man and this excites his curiosity. While I personally have not much doubt about the accuracy of Einstein's conclusions and consider it a great piece of work, I am a little afraid it will have the tendency to ruin many scientific men in drawing them away from the field of experiment to the broad road of metaphysical conceptions. We already have plenty of that type in this country and we do not want to have more if Science is to go ahead.

121. Hale to Newall, 20 May 1920 (HM).
122. Schesinger to Dyson, 16 Feb 1920 (SM).
123. Dyson to Schlesinger, 18 Mar 1920 (SM).
124. Rutherford to Hale, 13 Jan 1920 (HM).

Another aspect of the publicity that worried some astronomers was its quantity. The press was so enthusiastic in England that Americans unfriendly to relativity believed mistakenly that the British had launched a publicity campaign to bolster the controversial and difficult theory. "I suppose we have all been worrying lately over the 'Einstein effect,'" complained William Hammond Wright of Lick Observatory to Hale, "chiefly as a result of the publicity which has been accorded to its so-called confirmation by the English Eclipse observations."[125] One not bothered by metaphysics or publicity was William F. Meggers, spectroscopist at the Bureau of Standards in Washington. In 1920 he proposed to Schlesinger a plan to measure the displacement of red stars near the sun during the daytime. Schlesinger thought that although the chances for success were probably less than half, the scheme was worth trying.[126] Meggers discussed the problem with two astronomers who had served at the Bureau of Standards during the war: two specialists in spectroscopy, Keivin Burns and Paul Merrill. After the war, Burns had gone to Lick temporarily, and Merrill had joined the staff at Mt. Wilson. Burns thus responded to Meggers' suggestion:[127]

> I haven't tried to photograph the sun in the long waves, nor stars in the day time, mainly for lack of a proper screen. I don't think much would come of it. Of course no one at Lick believes in the Einstein effect, it being contrary to philosophy, judgement and horse sense. But since so much is being said on the subject it is necessary to be interested. It may take a long while to show the error of the ways of the English astronomers.

Burns admitted that the general problem of daylight photography of the sun should be explored, and if a suitable screen were found, it would be well worth trying at Lick or Mt. Wilson. But, he thought, "the Einstein effect is the result of some medium sized minds trying to lift their intelligence over a mental obstacle by their intellectual bootstraps. It corresponds to nothing objective."

Merrill took a similar line:[128]

> Nobody here is very enthusiastic about the Einstein stuff. I have heard it referred to as an "accursed theory." Some of the coolest Englishmen even have no use for it. There is not much violent opposition to it here—the attitude is one of watchful waiting. There are dozens of other things which are more worth working on.

125. Brown to Schlesinger, 7 Nov 1920 (SM); Wright to Hale, 27 Feb 1920 (HM).

126. Meggers to Schlesinger, 9 Jan 1920, and Schlesinger to Meggers, 17 Jan 1920 (MC).

127. Meggers to Burns, 22 Jan 1920, and Burns to Meggers, 6 Feb 1920 (MC).

128. Merrill to Meggers, 1 Mar 1920 (MC).

Like Burns, Merrill was intrigued with the technical problem. "A first class wide angle lens would be the thing," he advised. "This might be used in fundamental star places, or in other problems such as that of disposing of the Einstein foolishness.... If I can get hold of a suitable screen I might get one of the direct photography men to shoot at stars in the daytime, but don't stop any experiments that occur to you and that you have the opportunity of making."

Meggers dropped plans for the project. About a year later Orley H. Truman, then at the Lowell Observatory in Flagstaff, asked him for information about the infrared procedure. He replied that he was glad to hear of this interest: "Nearly a year ago, we suggested that either the Lick or Mt. Wilson Observatories might undertake this experiment, but they seem to be prejudiced against the Einstein doctrine and so far as we know have done nothing in testing the deflection by daylight photography of stars near the sun."[129]

## Personnel changes at Lick

Campbell had decided to suppress publication of Curtis' results from the 1918 eclipse until his measures and calculations could be thoroughly checked. During the first months of 1920 Curtis worked steadily at this problem, but he did not complete the task. Despite the pleasant life at Lick, the offer of the directorship of the observatory at Allegheny as replacement for Schlesinger who was going to Yale, and a salary of $6000, drew him East. Curtis tendered his official resignation on April 16, setting July as his departure from Lick.[130] Curtis' move to Allegheny came at an awkward time for Campbell, who was left holding the bag of the Einstein problem. He soon found someone to hand it to, Robert Trumpler, who (to complete the circle) had recently come to Lick from the Allegheny Observatory.

Born in Zurich, Trumpler had studied there and in Göttingen, where he obtained his PhD in 1910 under the astronomer Leopold Ambronn. His arrival in Göttingen coincided with Hermann Minkowski's presentation of his four-dimensional formulation of relativity. Trumpler came to America in 1915, as an assistant at the

129. Meggers to O. H. Truman, 8 Feb 1921 (MC). Truman apparently did not follow up the idea, which had been suggested by Lindemann during the war. A. F. and F. A. Lindemann, "Daylight photography of stars as a means of testing the equivalence postulate in the theory of relativity," *MNRAS, 77* (1916), 140.

130. Correspondence of Curtis and Schlesinger, 15 Jan–20 Mar 1920 (SM); Campbell to Merriam, 23 Jan and 23 Mar 1920, Curtis to Campbell, 16 Apr 1920, Campbell to David P. Barrows, 20 Apr 1920 (LO); Curtis to Family, 1 Feb and 15 Mar 1920 (LO copy from MA); Campbell, "Resignation of Dr. Curtis," *PASP, 32* (1920), 201–202.

Allegheny Observatory. His work there was primarily observational, and his own research was a detailed study of the Pleiades star cluster.[131]

Trumpler went to Lick as a Martin Kellog Fellow for the academic year 1919–20. He intended to finish the Pleiades work as quickly as possible, and to return to Switzerland if he were offered a good position there. When Campbell learned that Curtis would be leaving, he offered Trumpler a position as assistant astronomer, at a salary of $1800.[132] Trumpler stayed at Lick for fifteen years. His training in precise stellar photography with the Pleiades cluster was perfectly suited to measuring the star displacements on the eclipse plates. And there was an added bonus. Trumpler emerged as the only astronomer in America capable of treating the theoretical aspects of relativity. Shortly after the news of the British eclipse results had come out, the astronomer Paul Biefeld wrote to Trumpler at Lick, "I know you have the theory [of relativity] so well in hand that you could give me the essentials." Five years later the Lick astronomer Williams Wright boasted: "We have here at the Observatory a specialist on Relativity (Dr. Trumpler)."[133] In 1920, however, Campbell had not yet realized Trumpler's potential in this regard, and he regretted Curtis' departure quite keenly.

Before Curtis left, Campbell discovered that Curtis' procedure for the measurement of the eclipse plates was the source of the large probable errors in his work. As a remedy, Campbell devised an intermediate plate with pairs of short diamond scratches ruled at right angles, intersecting at points corresponding to the positions of the star images on the day and night plates. In his revised measuring procedure, the eclipse and intermediate plates were put face to face with the emulsion side of the eclipse plate in contact with the ruled surface of the intermediate plate. The microscope with micrometer was readily moved over each star image, and the intervals between star and the intersection point of the two rulings quickly and accurately measured. The night comparison plates were measured in the same way, relative to the intermediate plate. "Curtis blessed me for hitting upon this simple and accurate device for comparing the night and day Einstein plates differentially and accurately [Campbell wrote Schlesinger]. He immediately ruled the intermediate plates in this manner and made complete sets of measures of the two 1918 eclipse plates and the corresponding night plates in June of that year 1920, before going to Allegheny. The results were a vast improvement upon his plan of absolute

131. Trumpler, biographical sketch (LO).

132. A. G. Marshall to Trumpler, 18 May 1919; Trumpler to "Herr Kollege," 28 Apr 1919; and R. G. Sproul to Trumpler, 9 Jun 1920 (LO).

133. Biefeld to Trümmler [*sic*], 13 Dec 1919, and W. H. Wright to Tracey R. Kelley, 28 May 1924 (LO).

measurement."[134]

The improved measures were mentioned publicly at a joint symposium on relativity for the Mathematical Society, Physical Society, and the Astronomical Society of the Pacific. It is worth recording that the efforts of the organizer, Samuel L. Boothroyd, did not receive enthusiastic approval. A member of the program committee, Vesto Melvin Slipher, wrote:

> My guess is that at this time the average Pacific coast astronomer is not as enthusiastic over the astronomical standing of the theory as are our English friends, and naturally, too. However, this is my personal feeling on the subject, so you should not let it weigh against such opinion as you will get from Lick and Mount Wilson men for these men have, I am sure, had opportunity for wider discussion of the subject and so should have a less personal view than mine is.

Robert Grant Aitken of Lick, chariman of the program committee, also advised against having the relativity symposium; but Boothroyd went ahead with his plan and lined up Charles St. John of Mt. Wilson to talk on "The Astronomical Bearing for the Theory of Generalized Relativty."[135]

The meeting took place in June 1920. Campbell's emissary, Joseph Haines Moore, thus reported the strange turn of events there:[136]

> Dr. St. John was unfortunately not present and Professor [Edward P.] Lewis and I to our horror were called upon with a few minutes notice to try and fill out his end of it. Fortunately I had familiarized myself with the English astronomers work, taking of course the opportunity of expressing my admiration for their fine piece of work, and a statement of their results. In this connection I spoke of the L.O. [Lick Observatory] expedition, explaining about the difficulties under which we had worked, on account of our apparatus not arriving in time from Russia, and the delay of the measurement and discussion of the plates on account of the war, with a statement that the plates were being remeasured by a method which we believed possessed considerable advantage over the method which had been used in the previous measures of our plates, the results of which had been presented by Curtis last year at Pasadena.

Moore hinted that the measurements made by the new technique were incompatible with Einstein's theory.

134. Campbell to Schlesinger, 9 Jun 1922 (SM).

135. Boothroyd to Slipher, 8 Mar 1920; Slipher to Boothroyd, 20 Mar 1920; Aitken to Slipher, 19 Mar 1920; Boothroyd to Slipher, 26 Apr 1920 (LA); and *PASP, 32* (1920), 191.

136. Moore to Campbell, 18 Jun 1920 (LO).

### Errors and difficulties delay publication

Curtis left Lick before definitive results were obtained by the new technique. He soon received as the first results from the measurement some computations done by Adelaide Hobe (table 5).

TABLE 5

| | | | |
|---|---|---|---|
| Plate 2: | in Dec. | $+0''.11$ | $\pm$ $0''.16$ |
| | in R.A. | $-0''.35$ | $\pm$ $0''.27$ |
| Plate 3: | in Dec. | $+0''.40$ | $\pm$ $0''.16$ |
| | in R.A. | $-0''.18$ | $\pm$ $0''.45$ |

Because the probable errors were still large, Campbell wrote that he was "provisionally thinking of asking Dr. Trumpler to look over the computations with me in the next two or three days, to see if we can find any chance for improvement."[137] This remark was the first indication that Trumpler might become involved in the Einstein work.

It turned out that Curtis' haste in his last weeks at Lick had contributed to the large probable error. "Am sorry that the results seem indeterminate," Curtis apologized to Campbell, "and have larger p.e.'s than my former solutions." Among other difficulties, the intermediate plate had obscured one star image that he had used in his previous "final" solution. To compensate he had added four others. "Perhaps this was a mistake", he admitted, "and it might pay to run through the solution with the same stars as I used before, cutting out the four I added, and of course, the one I could not get." Curtis was beginning to suspect, however, that little more could be squeezed from the data, and he hoped results would soon be available for announcement at a coming astronomical meeting in September.[138]

Campbell responded quickly, asking Curtis to keep the Einstein results confidential for awhile, as potentially serious errors were showing up in the current work.[139] Curtis acknowledged the likelihood that he had made mistakes in measurement, transcription, and computation. Certain pencil marks on the eclipse plates were wrong "evidently my error, made in the dark room at Goldendale." Curtis recommended that Trumpler go over everything, and he backed off from his efforts to get Campbell to prepare material for a formal announcement of results. "No need for the lantern slides for a long time, I can see."[140] Campbell himself remeasured the better of the two Goldendale plates using the

137. Campbell to Curtis, 15 Jul 1920 (LO).
138. Curtis to Campbell, 22 Jul 1920 (LO).
139. Campbell to Curtis, 26 Jul 1920 (LO).
140. Curtis to Campbell, 2 and 18 Aug 1920 (LO).

intermediate plate method; but when he came to do the comparison plates, "I gave it up when I saw how much the chart stars are elongated in declination. I had not realized that the wind had played such havoc with the image."[141] He decided to take a new set of comparison plates.

> Just recently Dr. Moore gave me the valuable suggestion that we mount the Chabot lenses in a suitable wooden tube on the side of the Crossley reflector and use that instrument and its excellent clock for guiding, with all its advantages. This I decided within ten seconds to do. I wonder why nobody thought of this before. Hoover will be making the camera tube when the first storm comes, and we shall plan to get the chart plates in the first week of November.

But Curtis was now convinced that further refinement could not improve the outcome. "It appears to me that the limit is here set by the character of the eclipse plates."[142]

The first opportunity to get new chart plates came in November between storms and under poor conditions. The plates were exposed, or rather over exposed, for three minutes.[143] The trouble with the previous comparison plates had not only been the wind, as Campbell had assumed. Aberration from the lenses had elongated the star images away from the center of the field. Campbell decided to try again, with one-minute exposures, but he now doubted that the Chabot lenses could "answer the Einstein question either positively or negatively." If the new exposures corroborated this judgment, he would be ready to abandon the project as hopeless.

"Looks like a year of work gone to [hell]," Curtis commiserated.[144] He agreed that a definite disclosure should be made promptly, describing all the troubles incurred in the 1918 eclipse and, if possible, mentioning some positive results as well.

> 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 results 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.

141. Campbell to Curtis, 4 Oct 1920 (LO).
142. Curtis to Campbell, 11 Oct 1920 (LO).
143. Campbell to Curtis, 20 Dec 1920 (CP). Campbell and Moore had determined on three-minute exposure from consideration of the faintness of the Goldendale images. Several months later they realized that the eclipse star images had been doubled by an unwanted movement of the apparatus. Their judgment of faintness, based upon only part of the image, had led to overcompensation.
144. Curtis to Campbell, 29 Dec 1920 (CP).

Curtis sent along a table including values of the deflection calculated for the two Goldendale plates and the six Chabot Observatory plates (1900 eclipse), with the number of stars for each measure and their relative reliability, the Goldendale having the least merit. As a conclusion he suggested giving the mean result for gravitational deflection at the sun's limb as 0".87, but emphasizing:

> 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.

Yet Campbell decided to continue to work on the problem. Moore helped him "in the struggle to obtain with the Crossley reflector the best possible photograph of the 1918 field," and by the end of March 1921, still another measurement of the two Goldendale plates was well under way.[145] But as we know, the eclipse plates had a "very inferior" definition, which looked all the worse in comparison with the latest check plates, and Campbell was "not very hopeful" that the probable errors would be reduced in the eventual solution. Sparing no effort, he had invented another way to measure the plates, by illuminating "the images and diamond rulings" by an electric lamp placed several feet behind the star image to be measured and lined up with the axis of the micrometer telescope. "Not only are the star images on the eclipse plate much better defined, but the diamond scratches are perfect."

Still the definitive announcement was delayed. "We are applying a few more checks before daring to make any announcements of results," Campbell wrote to Curtis concerning the one Goldendale plate that had already been completely measured. As for its companion, "Dr. Moore has finished the measures of the other eclipse plate and the night plate, and measures are half way through on that pair. We have no idea what

145. Campbell to Curtis, 17 Jan 1921 (CP); Campbell to Curtis, 31 Mar 1921 (LO): "We had a long struggle with the weather to get satisfactory plates for comparison with your two Einstein plates of 1918. Good weather finally came in the middle of February after a wait of two and a half months, and we found that the correct exposure times were twenty seconds."

the least squares solutions may reveal as to them." And further to extend this agonizing reappraisal, Campbell now wanted to rework the 1900 Georgia eclipse plates that Curtis had borrowed more than a year earlier from Charles Burckhalter of the Chabot Observatory in Oakland. As he plunged into this latest task, Campbell confided to some colleagues that the work had been giving him "serious anxieties and regrets" and "has caused me to lose a good deal of sleep" for over a year.[146]

About the middle of May, Campbell wrote Curtis that Trumpler had just begun "what I think is the last section of computational work."[147] He wanted Curtis' opinion on the factors that might have caused the peculiar double form of the images on the Goldendale plates. Curtis agreed with Campbell's opinion that irregularities in the clock could explain only a part of the appearance of the Goldendale images. The wooden mounting and tubes had not been sufficiently rigid, he now believed, and could "give" a little as the instrument took different hour angles. Curtis calculated that a deflection of 5 inches on the plate would mean only about 1/250 of an inch movement for the tubes as a whole, "and this is not a great deal for a wooden framework." He went on to consider the rigidity requirement for the mounting for the new quadruple lenses that were being made by Brashear for the Lick expedition to Australia. He described a lattice design for the tube, which he thought could be made rigid enough to give no deflection as great as 0.001 inch between zenith and horizontal. To be sure of getting "perfect, round images...under the temporary conditions of an eclipse camp" he emphasized the need "to go the limit in rigidity of tubes and axis and excellence of clock."[148]

As cautious as Campbell was about presenting the fruits of his labors, he did make an announcement at the astronomical session of the Pacific Division of the AAAS, held in Berkeley in August 1921. The only printed report of his negative conclusion appeared in the *New York American* title "Einstein's Theory is Not Proved."[149] The article ran:

> A striking feature of the recent session at Berkeley was a paper, illustrated by lantern slides, in which Astronomer William Wallace Campbell gave his observations of the sun at instant of total eclipse at Goldendale, Wash. station, June 8, 1918. Months passed in the work [of

146. Campbell, correspondence with Curtis and Burckhalter, 31 Mar–6 Apr 1921 (LO); Campbell to Hale, 13 Apr 1921 (HM); Campbell to Stebbins, 12 Apr 1921 (AAS).

147. Campbell to Curtis, 19 May 1921 (LO).

148. Curtis to Campbell, 24 May 1921 (LO).

149. E. L. Larkin, "Einstein's theory is not proved," *N. Y. American* (1921), Lick newsclippings, 197 (LO).

> measurement]. The final result of all this toil is that the Lick Observatory expedition secured measures of bending of rays of light from the immensely distant star of one second of arc, slightly more than half of the deflection predicted by Einstein.... At the conclusion of this valuable paper of Dr. Campbell, he said that this bending of light had not settled the question.... [Then] Astronomer and Astrophysicist George Ellery Hale, planner of the largest observatory in the world, inventor of that powerful instrument, the spectroheliographic telescope, used before the mighty 100-inch mirror was mounted at Mt. Wilson, arose and made a speech commenting on Campbell's now historic work, and said in conclusion: "I still hold Einstein's theory in abeyance."

It appears that Campbell intended to follow his announcement at Berkeley with the long-awaited definitive report. We know his plans and distress about the Einstein business as of November 1921, from a letter to Dyson:[150]

> I have been very unhappy the past two years and more over the results of Dr. Curtis's efforts to test the Einstein hypothesis at the eclipse of 1918. Following Dr. Curtis's departure, I have done considerable work on the 1918 and the 1900 plates myself, in company with Dr. Moore, and I plan to publish the indecisive results very soon. The fact is that we should not have attempted any observations on that subject with the imperfect and untested lenses which we borrowed only one month before the date of the 1918 eclipse, when it became apparent that our own eclipse equipment, already nine months out from Russia on its home journey, would not arrive in time. Having put our hand to the plough, and certain preliminary announcement of results having been made, I am going to see the thing through to the final publication, letting astronomers judge pretty much for themselves as to the weights and values of our conclusions.

No publication appeared.

Why did Campbell decide in the end not to publish his results? No doubt he hoped to get much better data from the Australia eclipse of 1922 and could afford to discard the troublesome material of 1918. This course of action probably was clinched by an error-ridden account of Campbell's results in the *New York Times* for December 19, 1921. The article derived from information supplied by the Princeton astronomer Henry Norris Russell, who had examined a print of the 1919 eclipse, studied the observations and reduction procedures carefully, and published a detailed analysis of the causes for non-radiality of the displacements on one of the Sobral sets of eclipse plates. In the spring of 1921 he gave a special lecture on relativity in which he downplayed the significance of the "still uncertain" question of the gravitational

150. Campbell to Dyson, 23 Nov 1921 (RGO).

redshift for judging Einstein's theory.[151] In his conviction and inclining toward Einstein's views, Russell interpreted Campbell's finding of a deflection greater than one second of arc, as announced at the Berkeley meeting of August 1921, as confirmation of relativistic light bending. This was the message Russell sent the *Times*. The resultant article, "Einstein Theory Again is Verified," carried the subtitle, "Prof. Campbell of Lick Observatory Confirms Calculations on Sun's Curvature of Light."[152] It emphasized that Campbell's "confirmation of the correctness of Einstein's calculations... is made doubly strong by the fact that it is based on observations extending over a number of years, instead of on one eclipse as in the case of the 1919 expeditions."

Russell's statement to the press had been prompted by reports that in 1801 a German astronomer, Johann Georg von Soldner, had predicted that the sun's gravitational field would bend light. The *New York Times* reported: "Soldner's work had been forgotten until recently it was discovered by German scientists, who have been using it to show the great advances made in astronomical science in the last century." That was not the purpose of resurrecting Soldner. The resurrector, the experimental physicist and Nobel Laureate Phillipp Lenard, who had published a fragmentary reprint of von Soldner's paper with extensive comments of his own, belonged to the German nationalist organization called "Study Group of German Natural Philosophers." He trumpeted von Soldner as the precursor of Einstein to support his organization's claim that whatever was valuable in Einstein's work had previously been discoverd by "Aryans."[153] In defending Einstein Russell mentioned that general relativity predicted a result different from von Soldner's and from Einstein's estimate of 1911. He pointed to Campbell's work as corroborating the general relativistic result, and the *Times* seized upon this story for its headline.

151. H. N. Russell, "Note on the Sobral eclipse photographs," *MNRAS, 81* (1920), 154–164, and "Modifying our ideas of nature: The Einstein theory of relativity," Smithsonian Institution, *Annual report,* 1921, 197–211. Departure from radiality had been used as an argument against the conclusion that the British observations had confirmed Einstein's prediction. Russell was able to trace the departure to heating of the coelostat mirrors.

152. "Einstein's theory again is verified," *New York Times* (19 Dec 1921), clipping enclosed in Edwin E. Slosson to Campbell, 19 Dec 1921 (LO).

153. J. von Soldner, "Ueber die Ablenkung eines Lichtstrals von seiner geradlinigen Bewegung, durch die Attraktion eines Weltkörper, an welchem er nahe vorbei geht," *Berliner Astronomisches Jahrbuch,* 1804, 161–172; Lenard's reprint appeared under the same title in *Annalen der Physik, 65* (1921), 593–604. In a footnote he described von Soldner as a farmer's son who had enjoyed "the advantage of not having attended too much school." Cf. Clark (ref. 1), 254–264; Armin Hermann, "Der Kampf um die Relativitäts Theorie," *Bild der Wissenschaft, 14* (1977), 108–116; Bernstein (ref. 1), 123.

Campbell was annoyed, and declined various offers from journalists to enter a discussion or present his results.[154] He shut up even tighter. That in turn annoyed Curtis, still waiting for some statement of the outcome of the labor poured into the eclipse projects. The following from Campbell gave him opportunity to express his feelings:[155]

> Moore and I discovered, from looking over your measures, and especially the computations, for the Einstein effect, that computing is not your strong point. Examination of your computations made me extremely sorry that I had not insisted upon your following my advice, twice offered, that Miss Hobe check your computations. The sheets contained so many errors 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!

To which the reply:[156]

> 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 them 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.

Perhaps the most important legacy from the efforts that had brought so much annoyance, and disappointment, and misunderstanding was awareness of the many precautions and the high standards that would be required to collect eclipse data suitable for testing Einstein. Unhappy experience with the Chabot lenses, the clock drive, and the telescope mounting, taught Campbell and his colleagues how to prepare for the few minutes of measuring that would be theirs in Australia.

### Preparations for the Australian eclipse

On March 12, 1920 Arthur Hinks informed the RAS of the path of the eclipse of 1922. It would begin on the East coast of Africa, he said, and cross the Indian Ocean, passing the Maldives and Christmas Island reaching Australia in the early afternoon. Its landfall was not enticing.

154. J. P. McSorley to W. W. Campbell, 1 Jan 1922; Campbell to McSorley, 3 Jan 1922; Science Service (Slosson) to Campbell, 17 Mar 1922 (LO).
155. Campbell to Curtis, 12 Jun and 3 Jul 1922 (LO).
156. Curtis to Campbell, 11 Jul 1922 (LO).

"The eclipse track reaches Australia at Ninety-Mile Beach, a hopeless part of the coast, and strikes into the great desert. There are no facilities for landing.... The desert is inaccessible, except to camels. There are no railways within hundreds of miles, and motor cars are out of the question."[157] Hinks judged that the first feasible Australian site was Cunnamulla in South Queensland, the terminus of the railway from Brisbane, and that the eclipse would arrive there around 4 p.m. Hinks also recommended two island sites. Dyson decided on Christmas Island, where a British company had built serviceable facilities to work phosphate deposits, and sent an expedition under Harold Spencer Jones in charge.

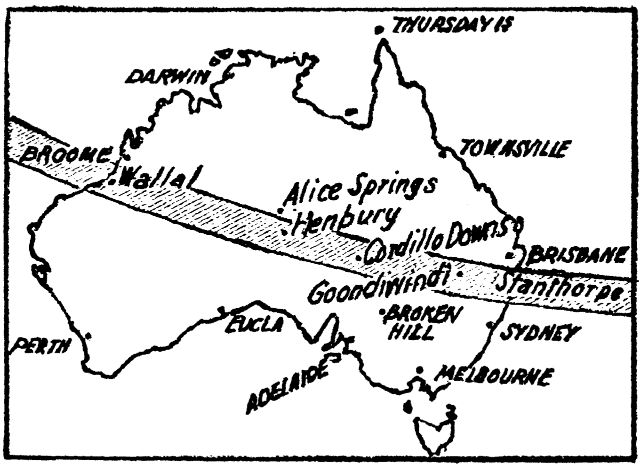


FIG. 6 Map of eclipse path across Australia. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

Campbell collected additional information about Ninety-Mile Beach from the New Zealand astronomer Charles Edward Adams, a former Lick fellow. Curtis judged from the material Adams sent that the site "appears to beat them all, if only one could get to it."[158] The eclipse path would enter Australia close to a combined post and telegraph station called Wallal. The sun would be quite high (58°), and the eclipse would last $5^{m}18^{s}$, the longest at any of the possible sites. The dryness of the desert region insured little chance of rain. Campbell asked Adams whether "the Australian Government would be disposed to dispatch a small Government steamer from Perth or Port Darwin at the proper time to carry an eclipse expedition to the vicinity of Wollal [*sic*]

157. *Observatory, 43* (1920), 143–144.
158. Campbell to Adams, 8 Jun 1920, and Curtis to Campbell, 24 Aug 1920 (LO).

and back to the point of starting."[159]

Campbell had already started work on ordering equipment. He asked Curtis' advice about optical specifications that should be met by the Brashear Company in making two quadruple eclipse lenses, aperture 5 inches, focal length 15 feet. He wanted to be sure that the star images would be good over a large field, and he wanted time to practice: "If I order the lenses I want them soon, so that Trumpler or someone else here may set the completed apparatus up at least a year before the eclipse date and determine by actual experiment what we may expect in the way of exposure times, driving clock control, probable errors of measured star positions, etc."[160]

Curtis took on the responsibility of supervising and testing the lenses authorized by Crocker from Brashear, whose plant was near Curtis in Pittsburgh. Although he was disappointed not to be able to go himself, he generously helped with Campbell's plans. This time the thing would be done right. "During the next ten years we are going to have available, with luck, only about eighteen minutes for further tests on the deflection effect, and it would seem as though the Australian eclipse should be well observed, as it is one of the best of the lot."[161]

By the end of 1920 Campbell had matured his plan for observing the eclipse in Australia. Two parallel cameras would be erected on one equatorial mounting, each with quadruplet objective lenses 5 inches in aperture and 15 feet in focal length. The British had employed coelostats, flat mirrors driven by a clock, to track the sun and to reflect the solar image into the main telescopes, in order to avoid building clockwork mounts for their telescopes. Campbell had always used such mountings at eclipses and saw no reason to change his ways since the coelostats had a habit of heating up and troubling the images.[162] He decided to put Trumpler in charge of the observations, despite Trumpler's junior position on the Lick staff.[163]

> The Einstein problem, in my opinion, provides the limit of difficulty in the determination of accurate star positions. It would not be fair to assign this work to Dr. Moore or other astronomers whose training has been exclusively in spectroscopy: it would be the plunge of a non-

159. W. W. Campbell, "The total eclipse of September 21, 1922," *PASP, 33* (1921), 254–255; Campbell to Adams, 17 Nov 1920 (LO).

160. Campbell to Curtis, 12 Sep 1920 (LO).

161. Correspondence of Campbell and Curtis, 19 Sep–23 Dec 1920 (LO); Campbell to Curtis, 20 Dec, Curtis to Campbell, 29 Dec, and Curtis to Eddington, 23 Dec 1920 (CP).

162. Campbell to Clarence Augustus Chant, 3 Dec 1920, and Chant to Campbell, 26 Nov 1920 (LO). See also the Chant-Curtis correspondence in the Robarts Library, University of Toronto.

163. Campbell to Curtis, 17 Jan 1921 (CP).

> swimmer into deep water without previous trial in shallow water. The work should be done by someone who is thoroughly informed and experienced in the photographic star position problem. Dr. Trumpler's experience conforms to these requirements.... After the lenses reach us and the size of the workable field has been estimated, we shall want to construct the mounting as rapidly as good workmanship will permit and then test the actual performance of the lenses by means of photographs of some well-known region, say the Pleiades group, to determine what may be expected from the lenses, and to estimate the unforeseen difficulties and get rid of them as far as possible. The actual tests here may, and probably will, consume three solid months. I think Trumpler is the man to make them.

By early February Curtis had tested the first lens from Brashear. "It is so far ahead of a two piece lens that there is no comparison." Campbell and Trumpler agreed, and the lens was shipped to Lick in March.[164] Several months later Curtis had a chance to see the British 1919 plates. "They are certainly good plates [he wrote Campbell]. The farthest star shows a little coma on one or two, but the rest are fine images. But still I am not converted. I think that no two-piece lens is adequate for this problem, and feel that your four-piece lenses will settle it."[165]

As the new lenses for the Australian eclipse were being made and tested, a new feature of the program emerged. Campbell's original program was to use two cameras with the Brashear quadruplet lenses of 15-foot focal length each to photograph the immediate surroundings of the sun, a region approximately 5° x 5°, providing a large scale on the photographic plate (45 seconds of arc in the sky to one mm on the plate). To get a larger field around the sun, a lens with shorter focal length would be needed; but that would result in a smaller scale on the plate and probably also distortion near the plate edges from lens aberration. The design of the Brashear lenses was the best compromise between large linear scale and field of view. But in the spring Campbell learned from Frank E. Ross of Eastman Kodak about a new short-focus lens that made possible photographing a large region around the sun without distortion.[166] Ross urged that one of his new lenses be tried on the Einstein problem, and on another related problem, named after Leo Courvoisier of the Berlin Observatory. The Courvoisier effect was a yearly refraction cycle reported by Courvoisier in 1913 from daylight observations with the meridian circle.[167] No one had succeeded in

164. Curtis to Campbell, 7 Feb 1921, and Campbell to Curtis, 2 Mar 1921 (LO).
165. Curtis to Campbell, 1 Jun 1922 (LO).
166. Campbell to Curtis, 4 May 1921 (LO).
167. L. Courvoisier, "Systematische Abweichungen der Sternpositionen im Sinne einer Jährliche Refraktion," Königliche Sternwarte, Berlin, *Beobachtungs-Ergebnisse, 15* (1913),

verifying the effect, which, however, had been mentioned as a possible explanation of the 1919 British eclipse results. Campbell decided that it would be very desirable to use a pair of Ross 60-inch (5-foot) focus lenses in addition to the pair of 15-foot focus quadruplets, and he ordered them from Brashear.[168] The Lick program was expanded to include two pairs of cameras, one pair of long focus (15-foot) to get a magnified view of the immediate solar vicinity; and one pair of short focus (5-foot) to get a larger field of stars, which would be useful for studying the detailed decrease of stellar displacement for increasing angular distance from the sun.

In the fall of 1921 Campbell received notification that the Australian Navy would provide transport for eclipse observers directly between the Wallal site and Fremantle, cutting out the necessity of travelling to Broome by commercial steamer. Australian generosity allowed Campbell to increase the size of the Lick expedition, which until then was to consist solely of Trumpler and himself. Moore and Campbell's wife were added to the party. Campbell told Chant of the University of Toronto the welcome news and suggested that he might also avail himself of Navy transport to Wallal. "The more the merrier." Campbell also urged Dyson to send a British party there.[169] The expedition was promising to become an outing, but still a prolonged stay on inhospitable 90-mile beach did not appear attractive; so Campbell invented a more pleasant way to take the comparison night-plates.[170]

> We propose to send our Einstein instruments and observer to the island of Tahiti, which is nearly the same latitude as Wallal in north-west Australia, three months in advance of the eclipse, and secure the night plates at Tahiti, under conditions as nearly as possible the same as those expected at the eclipse station. We also plan to select another region of the night sky which can be observed at both Tahiti and Wallal; at Tahiti on the same plates as the critical eclipse region, and at Wallal on the same plates with the eclipse exposures. We plan for only two exposures with each camera, exposing the first one of the two to the auxiliary night field the night before the eclipse and exposing the second eclipse plate to the auxiliary night region on the night following the eclipse. This is a rather ambitious program and the observers will have to pay strict attention to meeting all requirements.

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cited in W. W. Campbell and R. J. Trumpler, "Observations made with a pair of five-foot cameras on the light-deflections in the sun's gravitational field at the total solar eclipse of September 21, 1922," *LOB, 397* (1928), 155. See also L. Courvoisier, "Jährliche Refraktion und Sonnenfinsternisaufnahmen 1919," *AN, 211* (1920), 305–312.

168. W. W. Campbell, "The total eclipse of the sun, September 21, 1922," *PASP, 25* (1923), 11–44, on 15.

169. Campbell to Chant, 11 Oct 1921 (LO); Dyson to Campbell, 8 Nov 1921 (RGO).

170. Campbell to Chant, 11 Oct 1921 (LO).

The technique of using the auxiliary night region was to correct for any differences between the Tahiti and eclipse stations. It was designed to avoid moving the telescope during the eclipse.[171]

Trumpler left for Tahiti on March 30, 1922, planning to arrive on the 10th of April. Reaching his destination on schedule, he established "a splendid observing site" in the "garden of an American resident, with valued advantages of work rooms and other conveniences immediately on hand." In June he wrote that all the comparison plates had been successfully taken, as well as one long-exposure plate to use for the intermediate plate in the measuring process. Campbell and the rest of the Lick party were to sail from San Francisco in the middle of July, meeting Trumpler in Perth, where he would have previously begun to measure the positions of the stars on the night comparison plates relative to the intermediate plate.[172] Campbell had worked out a plan so that measures of one of the eclipse plates would be made while in Australia, allowing him to make a preliminary announcement of the results. In this way he hoped to avoid the pressure that would attend a delayed announcement. He had learned from Goldendale. "I have not found any way of shifting the responsibility for securing workable designs with minimum chances of something going wrong. If I could only have done the same thing for the eclipse of 1918 I would be very much happier that I am today."[173]

171. The same technique had been used by the British in Principe in 1919. Comparison plates had been taken at Oxford before the eclipse, and to ensure some control over differences between conditions at the two sites, a check field had been photographed as well. Dyson, Eddington and Davidson (ref. 45), 317–319. A further refinement of this technique had been suggested by Berkeley astronomer Charles Donald Shane, who proposed obtaining the check field during the eclipse itself, thus providing an independent set of data for calculating effects of scale and plate orientation without using the eclipse-field stars. Campbell rejected Shane's innovation of obtaining the check field during the eclipse because he wished to get as many stars as possible on each exposure and to change plate holders as infrequently as possible to ensure steadiness of the apparatus. The British heard of Shane's proposal through Curtis and decided to try it, planning many short exposures of the eclipse field (10, 20, and 30 seconds) between exposures of the check field. They allowed 15 seconds for movement of the telescope between eclipse and check field, and 12 seconds for each plate change. Trumpler's two exposures took two minutes each, and he allowed 50 seconds to change the plate holders and steady the telescope from the resulting vibrations. Crelinsten (ref. 3), 243–245.

172. Campbell to Trumpler, 4 Jul 1922, and other documents (LO, 1922 eclipse box); cf. Campbell (ref. 166), 21–22.

173. Campbell to Mitchell, 14 Feb 1922 (LO).

## The 1922 eclipse: All eyes on Lick

Seven separate attempts were made to measure the light-bending effect around the eclipsed sun of 1922. Three represented serious competition to Campbell in a race for definitive results pertaining to the Einstein theory. Spencer Jones' expedition, sent by Greenwich Observatory, was the British sequel to the 1919 expeditions, and offered Greenwich astronomers the chance to duplicate the previous measures, vindicating or disproving their original dramatic results. It went to Christmas Island as originally planned, since the alternative at Wallal did not become practicable until preparations had gone too far to be altered for Australia.[174] Near the British camped a German-Dutch expedition headed by Erwin Freundlich. This would be Freundlich's first shot at the Einstein problem since his thwarted attempt in 1914. A group under John Evershed, sent out under the auspices of the Indian government, intended to set up in the Maldives, but encountered transportation problems and went instead to Wallal.[175] Dyson had arranged for Evershed to borrow a 16-inch coelostat from the British JPEC to use with his Einstein camera and to vindicate the method against the criticism of Campbell and others.

The Adelaide Observatory sent a party to Cordillo Downs, in the extreme north-eastern corner of South Australia. Curtis loaned the Australians a quadruplet Einstein camera, and Campbell provided the polar axis mounting, driving clock, and driving arm, as well as a duplicate 40-foot coronal camera to his own so that the Adelaide and Lick parties might detect changes in the solar corona by comparing observations made at the two sites. The Sydney Observatory went to Goondiwindi, near the southern border of Queensland, to look into the Einstein problem. Campbell helped this group too. He had Trumpler ship equipment to Sydney after completion of the Tahiti phase of the project. "It is our desire [he wrote Trumpler] to cooperate and assist the Australian astronomers in their eclipse plans, in so far as this is practically possible." The Canadian party under Chant located with Campbell at Wallal. Curtis supervised the construction of Chant's lens by the Brashear Company, and Campbell arranged to have Trumpler take the comparison plates for the Toronto expedition and bring them on with him to Australia. "I suppose you have your hands full advising some of the weak sisters who are going to observe the eclipse," Curtis remarked

174. Dyson to Campbell, 8 Nov 1921 (LO).

175. J. Evershed, "Report of the Indian eclipse expedition to Wallal, West Australia," Kodaikanal Observatory, *Bulletin, 72* (1923), 45.

wryly to Campbell during the long months of consultation and preparation. "I have sent small volumes to one or two of them myself."[176]

By preparing his expedition far in advance, Campbell was able to perform experiments on Mt. Hamilton to test important features of his eclipse program. For instance, to determine the maximum exposure possible for photographing stars illuminated by solar corona without fogging the plates, Campbell used the Crossley reflector to photograph star fields near the full moon with varying exposure times. He ascertained that the two-minute exposure he had planned for his program for the 15-foot camera lenses would not fog the plates. Campbell also experimented with developers and lengths of time in the developing process to ensure optimum results in the darkroom stage of the work, which would be carried out in Australia.[177] None of his competitors had prepared as thoroughly as he had. None felt, as he did, anxiety to brighten a reputation that in his opinion had been tarnished by his "mistake of having reported, though guardedly, on Curtis's results at the meeting of the Royal Astronomical Society [in 1919]." Curtis sensed the drama as sailing time approached and admitted that it was "one of the big disappointments of my life that I am not 'in' on this particular eclipse."[178]

Nature rewarded the careful preparation of her inquisitor by storming over his chief competition and smiling sunnily at him. The British were greeted at their landing site by a storm that prevented their steamer from unloading equipment for ten days. All the expeditions on Christmas Island were plagued by bad weather. The British could not do their preliminary photometric work, and at eclipse time, both the British and German-Dutch parties were clouded out.[179] Campbell had perfect weather. That was not enough for success for Evershed, who was plagued by instrumental difficulties. During the eclipse he took five exposures; when developed, "all were found to have failed for one

176. Various documents in LO, 1922 eclipse box; Campbell to Mitchell, 28 Mar 1922 (LO); G. F. Dodwell and C. R. Davidson, "Determination of the deflection of light by the sun's gravitational field from observations made at Cordillo Downs, South Australia, during the total eclipse of 1922 September 21," *MNRAS, 84* (1924), 150–162, on 150–151; Campbell to Trumpler, 23 Jan and 25 Feb 1922, Trumpler to Campbell, 25 Jan 1922, and Chant to Campbell, 16 Feb 1922; Curtis to Campbell, 1 Jun 1922 (LO).

177. W. W. Campbell and R. Trumpler, "Observations on the deflection of light in passing through the sun's gravitational field, made during the total solar eclipse of Sept. 21, 1922," *LOB, 346* (1923), 41–54, on 42–45.

178. Campbell to Schlesinger, 9 Jun 1922 (SM); Curtis to Campbell, 11 Jul 1922 (LO).

179. *Observatory, 45* (1922), 61, 142–144, 317–318; Trumpler to Campbell, 31 Oct 1922 (LO): "The first few seconds of the eclipse the sky was still clear at Christmas Island, so that the second contact could be observed, but 6 or 7 seconds after the beginning of totality the sky became overcast and no successful photographs were obtained."

FIG. 7 Campbell's Einstein cameras set up at Wallal to observe the 1922 Australian eclipse. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

reason or another." Much of Evershed's problem had to do with a faulty driving screw, and in his report he remarked angrily that "if British manufacturers could be induced to abandon the old methods and apply ball bearings to all moving parts in astronomical instruments, as should have been done thirty years ago, an enormous gain would result in the uniformity of movement so essential in this research." He reported "with envy" that the American installation was "fitted with ball or roller bearings, and with a simple and most effective method of driving without the use of any gearing whatever."[180]

Of the seven expeditions that were to try the Einstein problem, four obtained useful observations: the Lick and Canadian observers at Wallal, the Sydney astronomers at Goodiwindi, and the Adelaide party. Only three obtained useful results. By February 1923 the Sydney astronomer in charge of the Einstein test, William Ernest Cooke, had to announce that he could conclude nothing from the eight plates he had taken under excellent conditions. His equipment, like Curtis' in 1918, was not adequate. Cooke expressed the opinion that "the first satisfactory results would be those of Dr. Campbell, of the Lick Observatory." He predicted that the public would have to remain in a state of suspense for probably several years, "because the most minute calculations and measurements would require to be carried out before any announcement on the subject could be accepted as being correct."[181]

Astronomers had to wait almost a year before they heard anything from any of the other three expeditions. Knowing of the immense interest, Campbell had hoped to get out a preliminary statement of results before leaving Australia. He had planned that Trumpler would go to Perth Observatory after the Tahiti phase and spend about five weeks measuring the brighter stars on the Tahiti plates. Campbell and Moore were to reach Perth in August, leaving a week for pre-eclipse measurements and calculations before the voyage to Wallal. Trumpler had left Tahiti on schedule, but shipping delays forced him to start setting up his measuring apparatus at Perth weeks later than intended. The travel plans to Wallal were also altered at the last minute. Other expeditions decided to take advantage of the services of the Australian navy, and the rendez-vous point was changed from Fremantle to Broome. This meant that Campbell's party would now have to take the commercial steamer from Fremantle to Broome, and that the Lick measuring apparatus had to be sent off earlier than expected. Trumpler had to take down the equipment for shipping before he could begin his

180. Evershed (ref. 175), 51; Crelinsten (ref. 3), 249–251.

181. "Relativity. Eclipse Observations. Unsatisfactory results," *Sydney Morning Herald* (22 Mar 1923); "Einstein theory. Negative results locally," *Sydney Daily Telegraph* (3 Mar 1923),Lick newsclippings, 69, (LO), quote in former.

measurements, and the Tahiti stars were not measured before the eclipse.[182]

The new travel arrangements meant a delay of more than a week at Broome on the return journey to wait for the commercial steamer to Fremantle. Unaware that Trumpler had not yet started on the Tahiti measures, Campbell wrote him from Sydney early in August, suggesting that they measure the Einstein eclipse plates at Broome while waiting for the steamer:[183]

> I should prefer that you measure on the Tahiti plates at Perth, or extract from your complete measures of the Tahiti plates, the data for from twelve to twenty selected stars, and set up the equations of condition from them, so that the corresponding stars on the eclipse plates may be measured in five or six days at Broome, and these data be entered in the equations for solution at Broome or on our southbound steamer. I advise strongly against your remaining behind at Broome. If further measures seem to be desirable before making announcement, Perth is, in my opinion, the place for you to make them. In other words, the Broome program of measurement should not include too many stars.

Unfortunately, Trumpler had already decided to return home via the northern route to Switzerland, where after a visit with family he would return to the United States.[184] The only time for measurement would be in Broome.

Trumpler and Campbell devoted eighteen hours a day to measuring one of the Einstein plates and the corresponding Tahiti comparison plate; but there was only time for measures in one direction, and the usual procedure of repeating the process in the reverse direction could not be carried out. Nonetheless, some numerical results were obtained from these rushed and incomplete measures and they indicated a definite light deflection, larger than the Newtonian value, but smaller than the Einstein deflection. Trumpler carried out a full reduction of the measurements and left a hurried note for Campbell with the final figures.[185] The check region was used to determine the second-order terms, the first-order terms being obtained from 19 stars in the eclipse field, located two to three degrees from the center of the plate. Trumpler excluded one "faint discordant star" and found the deflection of the sun's limb by least squares solution using the radial displacements of all stars, giving the faint ones half weight. He reduced Campbell's and his own measures separately and secured the limb deflections shown in table 6. "Check region reduced exactly same way

182. Campbell (ref. 168), 23–26.
183. Campbell to Trumpler, 5 Aug 1922 (LO, 1922 eclipse box).
184. Campbell (ref. 168), 40.
185. Trumpler to Campbell, undated (LO).

TABLE 6

| | |
|---|---|
| Trumpler: | 1".38 (79 stars) |
| Campbell: | 1".17 (72 stars) |
| Mean: | 1".28 |
| Probable error: | 0".18 |

shows no appreciable deflection," Trumpler recorded; "calculation gives minus thirteen [−0".13]." He concluded: "Some light deflection beyond doubt, but amount smaller than predicted." Campbell, however, chose not to announce these preliminary results.

Campbell found himself under immense pressure from colleagues and the press. A telegram from Science Service awaited him at Lick asking for "first announcement of Einstein Eclipse results." He cabled that the Einstein eclipse negatives were on the steamer, due to reach Mt. Hamilton about December 10. "Probably 2 or 3 months measurement with powerful microscope necessary," he added, "and considerable computation before results available."[186] Campbell sent Trumpler an urgent telegram: "Intense public and academic pressure for Einstein results. Please make Swiss visit minimum possible. Write estimate when return." Trumpler replied the earliest he could reach home was the 6th or 7th of February. "I am sorry that we did not measure the four plates obtained with the fifteen-foot cameras before leaving Australia, as it had originally been planned," he added; "my visit in Switzerland as well as the trip would have been more pleasant. You may however be assured that I am doing my best to follow your wishes with respect to my return trip."[187]

The shipment of equipment and plates reached Mt. Hamilton on December 16. A week later one of the Einstein plates sat on the measuring microscope ready for adjustment. "Of course," Campbell wrote Trumpler, "I wish you were here to proceed with the adjustments and measures. We apparently need your more extended notes before we can undertake the adjustments without considerable loss of time."[188]

The delay caused many people to suspend important plans. Samuel Alfred Mitchell, writing a book on eclipses, intended to include a large section on the Einstein theory. "When I saw Dyson last," he told Campbell, "he said that he would not be in the least surprised if the 1922 photographs did not confirm the Einstein effect. He thought that

186. Science Service to Campbell, 22 Nov 1922, and Campbell to Science Service, 25 Nov 1922 (LO).

187. Campbell to Trumpler, 24 Nov 1922, and Trumpler to Campbell, 29 Dec 1922 (LO).

188. Campbell to Trumpler, 29 Dec 1922 (LO).

possibly they in England had stressed Einstein a little too much." Mitchell wanted to include an account of the Lick trip and results in his book. As for Dyson, he had an entire eclipse expedition weighing in the balance. The next eclipse, which would be visible from Mexico and lower California, was to take place in eight months, which did not leave much time to make arrangements should Campbell's results of 1922 not confirm the British results of 1919. "If there is disagreement we must regard the point as unsettled and every endeavour must be made to test the matter again next September. In that case it will be desirable for us to send an expedition, but it will hardly be necessary if you confirm the 1919 results in which case the question may be regarded as settled." Campbell told Dyson that it would be at least six weeks before he could cable any results. "We have some indications of the outcome," he remarked, "but any statement whatsoever at the present time would be scientifically unjustified."[189]

The press also clamoured for results. The editor of the *Michigan chimes,* for example, hunted Campbell for a brief article he had promised about the Australian expedition. In response to pressure from a San Francisco newsreel firm, Campbell declared himself adverse to further publicity. "Astronomers and other scientific men do not want to appear before the public in relation to 'what they are going to do,' and before they do it. I am sure you will see this principle which, throughout the high-class scientific world, is followed as closely as possible."[190]

Added to the fuss over the eclipse expedition came an invitation to accept the presidency of the University of California. Campbell's acceptance was conditioned on his retaining the directorship of the Lick. The condition was met, and Campbell planned to assume his new duties on July 1. "In the meantime the indirect consequences are bearing heavily upon my time," he told Dyson, "which I am trying to save for devotion to the astronomical problems which surround me."[191]

Trumpler reached Mt. Hamilton in early February. He gave an extensive interview to a San Jose newspaper, relating details of the Tahiti and Australian expeditions, explaining why interest in the pending results abounded, and predicting that it would be "at least two months before we can give out anything on it."[192] Then he and

189. Mitchell to Campbell, 30 Nov 1922; Dyson to Campbell, 9 Jan 1922; Campbell to Dyson, 2 Feb 1922 (LO).

190. Campbell to Mr. E. C. McCobb (*Michigan chimes*), 16 Jan 1923; Campbell to Mr. J. B. Gum, International Newsreel Corp., 5 Jan 1923 (LO, 1922 eclipse box).

191. Campbell to Dyson, 2 Feb 1922 (LO). Cf. C. J. Struble to Campbell, 29 Jan 1923, and Campbell to Board of Regents, 2 Feb 1923 (LO).

192. Trumpler to Campbell, 2 Feb 1923 (LO); Edith McConn, "Lick astronomer tells of eclipse expedition," *The Evening News* (San Jose, Cal., 12 Feb 1923), Lick newsclippings (LO).


FIG. 8 Robert Trumpler measuring Einstein plates at the Lick Observatory, as depicted in the *San Francisco Chronicle,* 30 January 1924. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

Campbell plunged into the work of measuring the plates and reducing the measures.

While Campbell measured, two prestigious organizations vied to get him to present his results at their annual meetings in the coming spring. Russell had been asked by the president of the American Philosophical Society to get a commitment from Campbell for a public evening lecture in Philadelphia on the eclipse results. Charles Greeley Abbot, Home Secretary of the National Academy of Sciences, tried to get him in Washington. Campbell rejected Russell's invitation and agreed to lecture at the Academy.[193] Abbot relayed the news to Science

193. Correspondence with Russell and Abbot, 6 Dec 1922–9 Mar 1923 (LO).

Service, which wrote Campbell asking for first news of the results. Its director asked Campbell to write a small book on relativity. "Since you have in your hands the latest and most authoritative evidence on the subject," he wrote, "you would be best qualified to write such a book." Campbell, reacting strongly to the implication that he had already arrived at results and was sitting on them until the April meeting, refused.[194]

Meanwhile, Chant had been busy reducing his own measures taken at Wallal. On April 6 the Associated Press quoted "Prof. C. A. Chant of the University of Toronto as commenting upon as 'distinctly favorable' to the Einstein theory observations made at Wallal." On April 11 Herbert Hall Turner at Oxford, having written Campbell a note on other business, penned a postscript: "We have had no hint of results of your measures up to now, tho' the Canadians have voted for Einstein." The same day, however, news releases went from Lick with the preliminary 15-foot camera results, and the next day Campbell cabled Einstein in Berlin, announcing confirmation of his prediction.

> Three pairs Australia Tahiti eclipse plates measured by Campbell Trumpler, sixty-two to eighty four stars each, five of six measurements completely calculated give Einstein deflection between one point fifty-nine and one point eighty-six seconds arc, mean value one point seventy-four seconds, Campbell.

Similar news went to Dyson by cable (fig. 9) and flashed across the United States by the press. From Campbell's press release, the following appeared in the *New York Times*:[195]

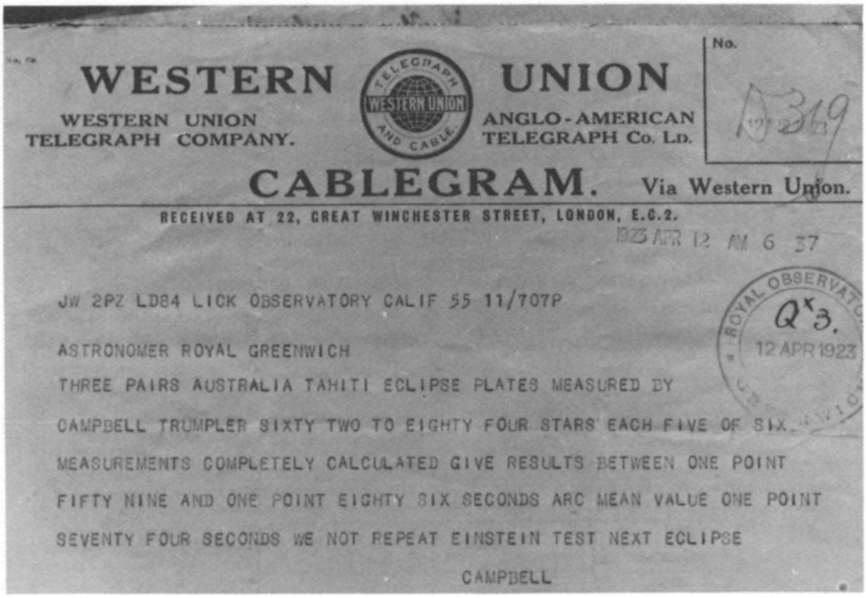
> The agreement with Einstein's prediction from the theory of relativity... is as close as the most ardent proponent of that theory could hope for. In fact the agreement of our observed value with the predicted value is so satisfactory that the Lick Observatory does not plan to repeat the Einstein test at the total solar eclipse due to occur in extreme Southwestern California and in Mexico on September 10, 1923.

At the Academy of Sciences meeting in Washington, Campbell gave suitable details.[196] There had been four plates for the 15-foot camera

194. Correspondence with Science Service, 13–31 Mar 1923 (LO); Campbell to Slosson, 7 Apr 1923 (ibid.).

195. J. R. Brokenshire to Campbell, 9 Apr 1923, and Campbell to Einstein, 12 Apr 1923 (LO, 1922 eclipse box); Turner to Campbell, 11 Apr 1923, and Dyson to Campbell, 15 Apr 1923 (LO); Campbell to Dyson, 12 Apr 1923 (RGO); W. W. Campbell, "Sun eclipse pictures prove Einstein theory; Lick Observatory finds star light is bent," *New York Times* (12 Apr 1923), Lick newsclippings, 125 (LO).

196. R. Trumpler, "A test of Einstein's predicted light deflection near the sun at the total solar eclipse of Sept. 21, 1922. Report to W. W. Campbell for Washington lecture, April 1923 (not published)" (LO).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ASTRONOMER ROYAL GREENWICH

THREE PAIRS AUSTRALIA TAHITI ECLIPSE PLATES MEASURED BY

CAMPBELL TRUMPLER SIXTY TWO TO EIGHTY FOUR STARS EACH FIVE OF SIX

MEASUREMENTS COMPLETELY CALCULATED GIVE RESULTS BETWEEN ONE POINT

FIFTY NINE AND ONE POINT EIGHTY SIX SECONDS ARC MEAN VALUE ONE POINT

SEVENTY FOUR SECONDS WE NOT REPEAT EINSTEIN TEST NEXT ECLIPSE

CAMPBELL

ROYAL OBSERVATORY 12 APR 1923

FIG. 9 Cable from Campbell to Dyson 12 April 1923, announcing preliminary results in favor of general relativity. (Courtesy of the Royal Greenwich Observatory, Herstmonceux, England.)

observations, two for each camera. Three plates had been completely measured by Trumpler, and two by Campbell. Though the star images on all the plates were round, symmetrical and well defined for the brighter stars, they were fuzzy and diffuse for the fainter stars, especially near the edge of the plate. In order to be able to use as many stars as possible, Campbell and Trumpler devised a system to account for the variability in image quality. Each assigned weights independently for their own measures using a method similar to one used at Allegheny Observatory for parallax photographs. The eclipse and comparison plates were each measured relative to the intermediate plate four times for each observer: direct and reverse for differences in right ascension, and direct and reverse for differences in declination. They used as many stars as possible in each eclipse field and, for comparison, 37 stars of the check field "well distributed over the plates and of suitable brightness." As a precaution against changes in the setup during measurement, two to four of the eclipse stars nearest to the sun, "which are of the greatest importance to the present problem," were measured at the beginning, in the middle and at the end of each series.

Campbell and Trumpler determined the mean eclipse-comparison differences for the two directions (right ascension and declination) by averaging the direct and reverse measures, corrected for proper-motion and parallax of the stars, and used the check field of stars (90° from the eclipse field) to control for differential refraction and aberration, and inclination of the plate to the optical axis. They included terms of the first and second order using equations of the form:

$$\Delta x\,(\text{eclipse} - \text{comparison in R.A.}) = a + bx + cy + dx^2 + exy + fy^2$$
$$\Delta y\,(\text{eclipse} - \text{comparison in Dec.}) = g + hx + iy + jx^2 + kxy + ly^2 . \qquad (3)$$

The coefficients came from least squares solutions using the check field stars with certain simplifying assumptions that reduced the number of constants to be determined for each plate. The constants were then applied to correct the measured displacements of the eclipse field stars.

They then redetermined the linear plate constants (zero point, scale and orientation) using from 28 to 38 eclipse stars (depending upon the plate), all more distant from the center than 2°, where any deflection according to Einstein's theory would be small. This innovation assured that the plate constants would be reasonably independent of any light-bending law that existed in the neighborhood of the eclipsed sun. Only the scale value thus determined would be slightly in error if any light deflection existed, and would be revised later when establishing the actual deflection law. This technique was only possible because Campbell had chosen to make long exposures to ensure that a large number of stars out to fairly large angular distances from the sun would be photographed. The linear plate constants were determined by least squares solution of the standard formula (equation 2), using the so-called "reference stars" 2° or more away from the center.

The corrections thus obtained were applied to the measured displacements, yielding the so-called "residuals" in $x$ and $y$. The radial component of these residuals represented the displacement of the stars owing to any light deflection. Assuming a linear law of radial displacement of the form

$$\Delta r = ad + b\frac{1}{d} , \qquad (4)$$

where $d$ is the angular distance of the star from the sun's center, $b$ is the deflection at the sun's limb, and $a$ is the correction of the scale value, Campbell and Trumpler found $a$ and $b$ for each set of measures by a least-squares solution using all the eclipse stars. The deflections at the sun's limb ($b$) came out as shown in table 7. The mean probable error, determined for a star of weight 1 (good), was $\pm 0\overset{''}{.}18$ for Trumpler and $\pm 0\overset{''}{.}20$ for Campbell. These values represented probable errors of the difference of two measured star images. To get p.e. of one star image, they assumed the errors of the intermediate plate to be eliminated, and divided the mean errors by $\sqrt{2}$ getting $\pm 0\overset{''}{.}125$ for

TABLE 7

| PLATE | TRUMPLER | CAMPBELL | MEAN |
|---|---|---|---|
| CD22-CD15 | 1″.84 ± 0″.28 | 1″.70 ± 0″.13 | 1″.77 |
| CD23-CD17 | 1.59 ± 0.22 | | |
| AB18-AB12 | 1.86 ± 0.20 | 1.71 ± 0.22 | 1.78 |
| Mean | 1.76 ± 0.13 | 1.71 ± 0.18 | |
| Mean of five sets of measures: | | | 1″.74 |
| Deflection predicted by Einstein: | | | 1″.75 |

Trumpler and ±0″.140 for Campbell. The report concluded that the accuracy of the data "compares well with other photographic measures; the Paris Zone of the Astrographic Catalogue for instance has a probable error of ±0″.13 for the mean of two images." The final report for the 15-foot camera observations, completed in May and issued in July, gave the results for the four plates (see table 8).[197] As can be seen by comparing this data with the table presented by Campbell in Washington (table 7), the mean results for the more complete measures was now 1″.72 instead of 1″.74 for the limb deflection, still in excellent agreement with Einstein's prediction.

## 5. REACTIONS TO THE LICK RESULTS

In Britain, the eclipse problem was viewed as settled—an empirical verification of a theoretical prediction, performed by at least three independent teams of observers from Britain (1919), America and Canada (1922). Dyson, upon receiving Campbell's cable, replied: "I don't think there is 'any possible probable shadow of doubt' about the correctness of Einstein's prediction of the deflection of light, whatever difficulties may be found with the rest of his theory. It is hardly likely that anyone will be coming from this side for the eclipse in California."[198] Harold Spencer Jones, disappointed at the 1922 eclipse, agreed that the nature of the debate over the correctness of Einstein's theory had changed.[199]

> The opponents of the theory must now take up the position that, although it has successfully accounted for the motion of the perihelion of Mercury, and although it has *predicted* the correct amount of the deflection of rays of light in passing through the field of gravitation of the Sun, it is not correct. It is no longer open to them to say that the

197. Campbell and Trumpler (ref. 177).
198. Dyson to Campbell, 15 Apr 1923 (LO).
199. "The total solar eclipse of 1922 September 21," *Observatory, 46* (1923), 164–165.

TABLE 8

**Light deflection at the sun's limb**

| PLATES | CAMPBELL | NO. OF STARS | TRUMPLER | NO. OF STARS | PLATE MEAN |
|---|---|---|---|---|---|
| CD22-CD15 | *1".72 ± ".32 | 62 | *1".88 ± ".27 | 69 | 1".80 |
| CD13-CD17 | 1.35 ± .22 | 77 | *1.62 ± .22 | 81 | 1.48 |
| AD18-AB12 | *1.78 ± .22 | 80 | *1.91 ± .19 | 84 | 1.85 |
| AB17-AB10/9 | | | 1.76 ± .22 | 85 | 1.76 (weight 0.9) |
| Mean for each observer | 1".60 ± ".14 | | 1".78 ± .11 | | |
| Mean from four plates | | | | | 1".72 ± ".11 |
| Einstein's predicted value | | | | | 1".745 |

* These five values, very slightly modified, were the only ones available when the preliminary announcement of our results for the Einstein eclipse problem was made through the press associations and otherwise, on April 11, 1923. At that date we had also determined from measures of the check-region star images, that any corrections suggested or demanded by existing small plate errors could not operate to diminish any of the five values of the Einstein coefficient.

> prediction of the amount of the deflection has not been fully confirmed.

Turner commented on the implications of the event in his regular column in the *Observatory*:[200]

> It is of no disparagement to the Canadian observers, who had already announced a similar result, to say that we were all hanging on the utterance which should come from the Lick Observatory; and now that it has come we feel more than ever how much was at stake. The English observers were resolute to go on even if the American verdict had been against them; there are no Courts of Appeal or Houses of Lords in Astronomy for getting verdicts reversed—the only recourse is to hammer on as before with renewed vigilance for possible flaws in the evidence but for this they were quite prepared. It is, however, a considerable relief to find that the necessity for this further campaign is now removed, and if any English observers are able to visit America this year it will no doubt be merely as a return to their old love, the Corona.

In the United States reactions were mixed. Russell was of course delighted. "Just what I expected! *Now* where are the scoffers? I don't believe it will ever be necessary to do the thing again."[201] Charles St. John agreed. "Campbell's results look very definite as to the reality of the bending to the proper amount." St. John was just beginning to find evidence for a relativity shift in the solar spectrum though it would be some months before he presented any details publicly.[202] Mitchell changed from "betting on your [Campbell] confirming the half-deflection and not the full Einstein amount" to asserting in his now up-to-date book: "The consequences of relativity have been so thoroughly substantiated by observations that he who has a scientific reputation at stake must indeed be very rash to state that physicists must all be mistaken and that the whole theory of relativity is 'tommyrot.'"[203] Yet he was not fully persuaded: "no doubt it will be many years in the future before astronomers and physicists are agreed on the exact status of the theory of relativity." Some of Campbell's colleagues had stronger reservations than Mitchell's. C. D. Perrine found that he could not accept relativity "notwithstanding the great weight of the observations and the

200. "From an Oxford note-book," *Observatory, 46* (1932), 171. The Australians took comparison plates in March 1923, and, because of faulty measuring equipment at Adelaide, did the reductions at Greeenwich. Their results, published in January 1924, yielded a limb deflection of $1''.77$ with a rather high probable error of $0''.3$. Dodwell and Davidson (ref. 176), 156, 162.

201. Russell to R. G. Aitken, 11 Apr 1923 (Center for History of Physics, AIP, New York. Microfilm copy of Henry Norris Russell Collection, Princeton University archives).

202. St. John to Hale, 13 May 1928 (HM); Crelinsten (ref. 4), 212–213.

203. Mitchell to Campbell, 7 Apr 1923 (LO); S. A. Mitchell, *Eclipses of the sun* (New York, 1923), 370–419.

excellent agreement with theory." He could not accept the conceptual framework of the relativity explanation of gravitation.[204]

> The whole relativity business has seemed to me unreal and so purely philosophical that to accept it is to upset our previously carefully constructed and very material systems. Indeed it seems to me like a "near" return to the era of "inductive reasoning" and an undermining of the material foundations of our science particularly. If it is true that our foundations are faulty the sooner we find it out the better. I am open minded but conservative in this matter.

Though he had told Campbell that the quadruplet lenses would "settle it," Curtis found that he could not accept Campbell's and Chant's confirmation of the British measurements. "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."[205] After the *Lick bulletin* came out in July, another former Lick astronomer, George F. Paddock, wrote to Curtis expressing doubts whether Campbell's and Trumpler's published results confirmed a limb deflection of 1".7. Curtis answered:

> No, I can't say that I regard... Campbell's paper as giving any too strong a support to the theory of a deflection of 1".7 at the sun's limb. It does seem to me, however, that his results pretty well establish the existence of a deflection, due to some cause or other, and larger than the predicted deflection of 0".87 predicted on the Newtonian Theory. Just what causes it, no one can tell.

He told Paddock that Charles Lane Poor, "another one of the irreconcileables [*sic*]," thought that it was "some sort of a refraction effect" and that he himself had tried to test this suggestion out at the recent Mexican eclipse using equipment that Poor had supplied.[206]

The basis of Curtis' heresy was the belief that Einstein's fictions had trespassed the limits allowed in science:[207]

> I have never been able to accept Einstein's theory. This in spite of the fact that many eminent mathematicians regard it as the greatest advance since Newton's time. I regard it as a beautifully worked out alternative "reference frame", apparently adequate, but by no means essential and by no means necessarily the correct system of reference. I regard it as like non-Euclidean geometry; we can form not one but many systems of geometry based on curved or hyperbolic space; we can explain every

204. Perrine to Campbell, 30 Jan and 2 Jun 1923 (LO).
205. Chant to Curtis, 15 Apr 1923, and Curtis to Chant, 18 Apr 1923 (CP).
206. Paddock to Curtis, 17 Aug 1923, and Curtis to Paddock, 29 Sep 1923 (CP). For Curtis' work at the ecjipse of September 1923 and after, see Crelinsten (ref. 3), 286–300, 325–328, 349–354, 388–396.
207. Curtis to Dr. Vogtherr, 19 Sep 1923 (CP).

geometrical theorem by such geometries as well as we can by the Euclidean; they are alternatives simply. We do not force ourselves to accept non-Euclidean geometry simply because it seems to "fit." Perhaps I am wrong, but it does not seem to me at present that I shall ever be willing to accept Einstein's theory, beautiful but bizarre,—clever but not a true representation of the physical universe.

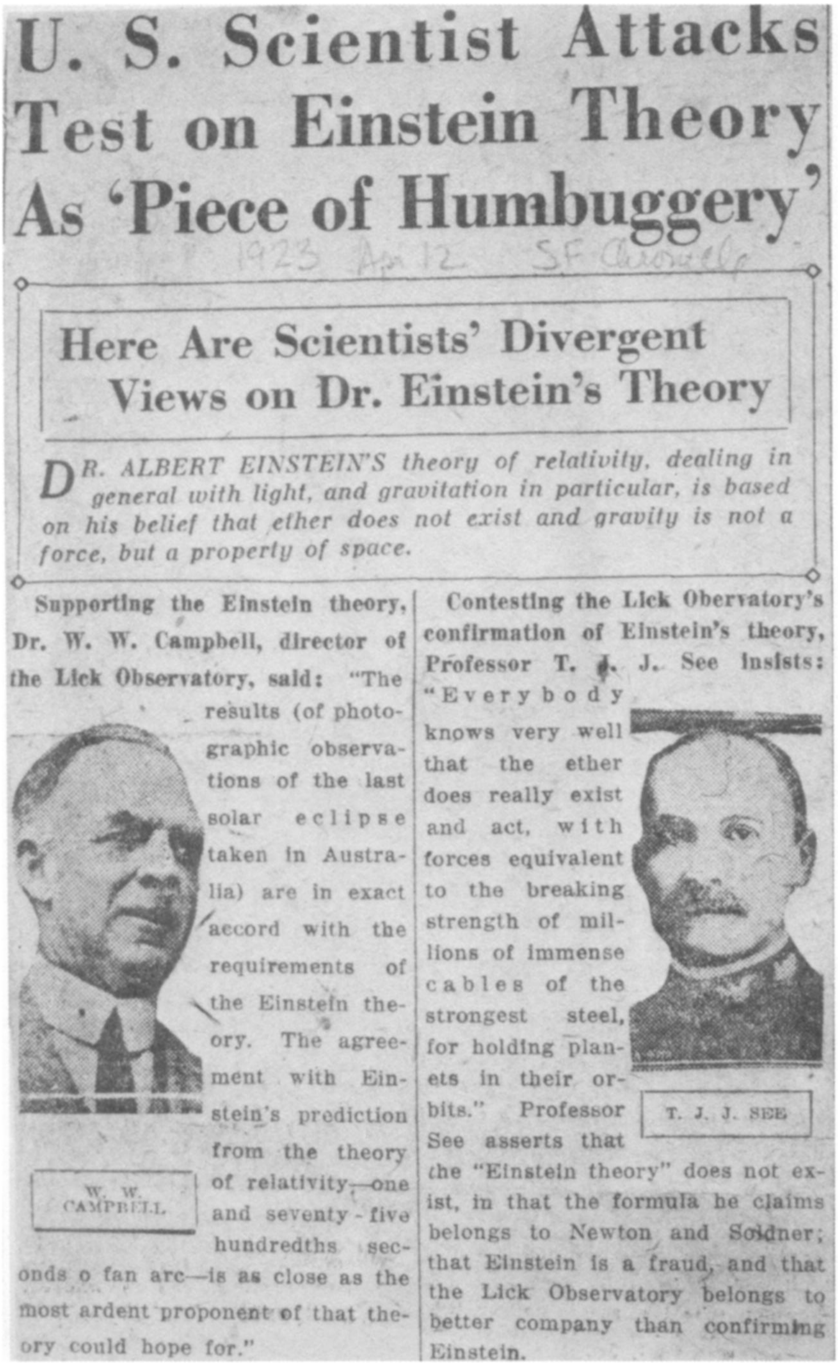
## T. J. J. See versus the Lick Observatory

In 1922 See had published his "New theory of the Aether" in *Astronomische Nacrichten* and in monographs printed in France, the United States, and England.[208] Believing that the eclipse results might be utilized to bring attention to his own work, he attacked Campbell's April 1923 press release favoring Einstein's theory and accused Einstein of trickery and plagiarism. In Philadelphia a news headline blared: "Government Scientist Exposes Einstein Trick." Campbell's announcement was only briefly mentioned, and most of the column discussed the fact that it had been "contested vigorously" by "Captain T. J. J. See, Government astronomer at Mare Island Navy Yard." The *San Francisco Chronicle* ran the headline, "U. S. Scientist Attacks Test on Einstein Theory as 'Piece of Humbuggery.'"[209] Photos of the two astronomers appeared side by side, with captions taken from their respective press releases (see figure 10). Most of the article was devoted to See's views. See's assault was inspired in part by the anti-Einstein propaganda of Philip Lenard and Ernst Gehrcke, the only two prominent physicists who had joined the "Study Group of German Natural Philosophers." They capitalized upon the numerical coincidence that Einstein's general relativistic prediction of a 1".74 light bending at the limb of the sun was exactly twice that of his prediction of 1911. They claimed that Einstein had copied von Soldner's prediction of 1801, which had coincidentally contained a mathematical error of a factor of 2, and then changed the numbers to cover up the mistake. See repeated at face value all that the Germans said, implying that because of their high reputations as physicists, they should be believed.

In blindly following Gehrcke and Lenard, See was either a fool or a cheat. The same may be said for his allegation that in October 1921, the Paris Academy of Sciences "came out with conspicuous proclamations by professors Picard and Painlevé against Einsteinism, and in favor of Newtonian mechanics." Again, there was a larger context to

208. T. J. J. See, "New theory of the aether," *AN, 217* (1922), 193–284. See also Maurice Lecat, *Bibliographie de la relativité* (Brussels, 1924), 115, item 3.

209. *Philadelphia Journal* (12 Apr 1923), and *San Francisco Chronicle* (12 Apr 1923), Lick newsclippings, 131, 174 (LO).



# U. S. Scientist Attacks Test on Einstein Theory As 'Piece of Humbuggery'

1923 Apr 12 SF Chronicle

## Here Are Scientists' Divergent Views on Dr. Einstein's Theory

*DR. ALBERT EINSTEIN'S theory of relativity, dealing in general with light, and gravitation in particular, is based on his belief that ether does not exist and gravity is not a force, but a property of space.*

**Supporting the Einstein theory, Dr. W. W. Campbell, director of the Lick Observatory, said:** "The results (of photographic observations of the last solar eclipse taken in Australia) are in exact accord with the requirements of the Einstein theory. The agreement with Einstein's prediction from the theory of relativity—one and seventy-five hundredths seconds o fan arc—is as close as the most ardent proponent of that theory could hope for."

W. W. CAMPBELL

**Contesting the Lick Obervatory's confirmation of Einstein's theory, Professor T. J. J. See insists:** "Everybody knows very well that the ether does really exist and act, with forces equivalent to the breaking strength of millions of immense cables of the strongest steel, for holding planets in their orbits." Professor See asserts that the "Einstein theory" does not exist, in that the formula he claims belongs to Newton and Soldner; that Einstein is a fraud, and that the Lick Observatory belongs to better company than confirming Einstein.

T. J. J. SEE

FIG. 10 The battle between Campbell and See as fought out in the *San Francisco Chronicle,* 12 April 1923. (Courtesy of the Mary Lea Shane Archives of Lick Observatory.)

this piece of information. Picard had been instrumental in refusing to admit astronomers from the former central powers or neutral countries into the newly created International Astronomical Union. His judgments on scientific matters were colored by intense anti-Germanism created by the experience of 1914–1918.[210] It was years before Einstein was elected a foreign associate to the Paris Academy, and even in England, a move to award the 1920 Gold Medal of the Royal Astronomical Society to Einstein had been blocked by astronomers who were leery of honoring a German, especially one with such a controversial theory.[211]

While Campbell was still in the East, See stormed again at news from Europe that Einstein was about to publish a "new discovery" concerning "the connection between the earth's power of attraction and terrestrial magnetism."[212] See had put forward his ether theory as the final connection between the earth's gravitation and magnetism. In a letter to the *London Times* later reprinted by U.S. newspapers, See cried out that he was being robbed. The press approached Lick Observatory for comment upon See's claims. In Campbell's absence, Aitken told reporters "that Professor See is at liberty to have his own opinions.... We don't wish to enter into any controversy."

See would not allow the Lick to stay silent. During April 1923, the *San Francisco Journal* printed a series of attacks by See on Einstein, relativity, and the Lick Observatory. After the series had completed its run in the newspaper, See presented an eight-page pasteup copy to the Library of Lick Observatory, and one to the Lowell Observatory in Flagstaff, Arizona.[213] Campbell was doubly concerned as director of the Lick and president at the University. See had written:

> Let us Californians watch the publications of Lick Observatory, which is a public institution, supported by the state of California, to see if they will bear witness to the truth of history, or attempt to cover up Einstein's appropriation of Von Soldner's work of 1801.

Campbell chose to point out See's errors about the Soldner formula,

210. Cf. H. A. Bumstead to Hale, 23 Jul 1918 (HM). Daniel J. Kevles, "Into hostile political camps. The reorganization of international science in World War I," *Isis, 62* (1970), 47–60; Brigitte Schroeder-Gudehus, "Challenge to transnational loyalties: International scientific organizations after the first world war," *Science studies, 3* (1973), 93–118.

211. Hoffmann and Dukas (ref. 1), 135; McCrea (ref. 2), 257–259; Newall to Dyson, 4 Jan 1920 (RGO).

212. "Professor See again attacks Einstein test," unidentified clipping, Lick newsclippings, 135 (LO). Cf. Einstein, "Zur affinen Feldtheorie," Preussichen Akademie der Wissenschaften, *Sitzungsberichte* (1923), 137–140.

213. T. J. J. See, "Einstein a second Dr. Cook?" 7 Jun 1923 (LO); "Einstein a trickster. Noted European scientists, charging plagiarism, reject theory of relativity," 15 Jun 1923 (LA).

and Trumpler was clearly the man for the job. Campbell asked him to "write down for me the supposed relations of Einstein and Soldner, from the point of view which you recently expressed to me." Campbell arranged with Aitken to have an article by Trumpler appear in the *Publications* of the ASP, and to be reprinted in *Science*.[214]

"A series of articles recently published by Professor T. J. J. See, U.S. Navy," Trumpler began, "gives a quite incorrect impression of the relation of J. Soldner's and of Einstein's work in connection with the deflection of light in the Sun's gravitational field." Trumpler detailed the methods by which Soldner and Einstein (in 1911) had arrived at their results, and showed that the approaches and goals were completely different. Referring to Einstein's value of 1916 Trumpler remarked: "The increase of this value over that in Einstein's 1911 paper is not due to any mistake in calculation in the earlier paper but is an effect of the difference between Einstein's and Newton's law of gravitation." Trumpler concluded that his comparison sufficiently showed the independence of Einstein's work "even if he knew about Soldner's paper, which is not likely, as Soldner's results had fallen into oblivion following the rejection of the corpuscular theory of light on which it is based." He explained See's misconception as arising from Lenard's version of the story.

> Professor See, accusing Einstein of plagiarism, clearly has not read Soldner's original paper and has been misled by a fragmentary reprint of it published in 1921 together with comments by a German physicist, P. Lenard. In these comments Lenard transforms Soldner's formula into a notation and form similar to those employed by Einstein. Professor See mistakes Lenard's transformed formula for Soldner's and bases his unfounded accusations upon its similarity to Einstein's result.

See's extensive public attack on Einstein and his theory forced Campbell into the general debates over relativity. "Our philosophy throughout the entire campaign," he had explained to a sceptical correspondent a few weeks before the preliminary results from the Australian plates were obtained, "has been that of Alexander Pope—'Whatever is, is right.'"[215] Now he was being cast in the role of a defender of the theory itself, and being drawn into theoretical issues such as competing ways of deriving the light-bending effect. Fortunately in Trumpler, Campbell had not only a skilled observer, but a competent relativist and controversialist. The incident that See

214. Campbell to Trumpler, 22 Jun 1923, and to Aitken, 29 Jun 1923; Aitken to Campbell, 3 Jul 1923 (LO), R. Trumpler, "Historical note on the problem of light deflection in the sun's gravitational field," *PASP, 35,* (1923), 185-188; also in *Science, 58* (1923), 161–163.

215. Campbell to Poor, 5 Apr 1923 (LO).

provoked caused Trumpler to study the details of the Soldner episode and to begin systematically to monitor the literature for new developments regarding relativity.[216]

Toward the end of 1923 another brush with See convinced Campbell and his colleagues that dissimulating a neutral stand in the public debates would be useless. See was apparently to give a public address in San Francisco, and he wrote to Robert Aitken (who was then associate director of Lick) that he wanted a statement of his position on relativity. He claimed that Aitken "took refuge under the authority of Eddington" when asked to defend relativity and that "leading men in San Francisco" agreed with him that such conduct was "the surrender of Americanism to low and discredited foreigners." He threatened public denunciation at his forthcoming lecture, should a statement not be forthcoming from Lick that such allegations were untrue.[217]

See's remarks resonated with antagonism in certain circles toward the influence of European science and culture on American institutions. The postwar period saw a great influx of Europeans into academic and research positions, and some Americans did not like it. When Campbell informed Curtis in the fall of 1921 that the sudden resignation of one of the junior astronomers had left a vacancy at Lick, Curtis urged Campbell to hire an American.

> Yes,—I know that science is international and all that, and also that every time we have disagreed in the past twenty years it has turned out that you were in the right of it. But it seems that the personnel list of Lick and Mt. Wilson are getting to read too much like a page of a Swedish directory. There are plenty of good youngsters who would give their eyes for a job at Lick or Mt. Wilson.

Curtis claimed that if he ever got enough money to hire another staff member, "they are going to be American or Canadian citizens, *born in* the U.S. or Canada, or England." By all means "a foreigner" should be brought in once in a while to prevent "inbreeding," he allowed,

> but the actual bread-and-butter jobs at the only two places on earth where it's really worth while to be an astronomer ought to go by preference to our own sort. If we can't wallop our brethren across the water with native born talent, let's acknowledge our inferiority frankly.

Some of Curtis' spontaneous remarks echoed See's harsher words. Campbell had been obliged to answer Curtis that although he sympathized strongly with his comments, it was hard to find an American to hire. The problem continued well into the twenties. Five years later

216. Trumpler, "Einstein's theory of relativity, 1922–24," handwritten notes (LO).
217. See to Aitken, 6 and 8 Nov 1923 (LO).

Hale told Campbell that he had been asked to suggest a professor of physics for a research post at M.I.T. and had mentioned several leading British men, all of whom had declined. "You of course know the difficulty of finding an American of the necessary calibre."[218]

By the end of the year, Campbell was ready officially to endorse Einstein's theory in face of the commotion created by See. Newspapers received the statement from "the dean of western astronomers" that "criticisms of Professor Einstein's laws are based on prejudice with which I have no sympathy."[219] The *San Francisco Journal,* which had published See's complete manifesto months earlier, refused to look on the matter as settled. Instead the editors announced "Einstein Again Supported," and boasted that California could claim the distinction of furnishing both his attacker and defender: "It is a tribute to science in California that we should furnish to the world the scientific ammunition for both sides of the controversy over this epoch-making discovery."[220]

The controversy over relativity and its observational tests continued with diminishing intensity throughout the decade. Others tried to test light bending at subsequent eclipses, and spectroscopists looked for definitive measures of the displacement of solar and stellar spectral lines. When Charles St. John at Mt. Wilson announced preliminary results in favor of relativity a few months after Campbell and Trumpler had thrown their observations behind Einstein, he encountered resistance to his conclusions similar to what Campbell had experienced. The California observers found themselves defending their observations against attacks inspired by conservatism and hostility toward theory. Yet in a milieu where observation was king, the weight of their evidence decided the issue.[221]

## 6. CONCLUSIONS

The episode involving Campbell and the "Einstein problem" illustrates several larger aspects of the reception of relativity in the United States. We find here a reliance of American observers upon European theoreticians manifested in various forms—the emphasis on the observations by most of the American astronomers, the lack of any attempt to work with the general theory itself on the part of the proponents or the critics, the general reliance on Eddington's writings for theoretical

218. Campbell to Curtis, 21 Sep and 4 Oct 1921; Curtis to Campbell, 28 Sep 1921 (LO); Hale to Campbell, 26 Nov 1926 (HM).

219. "Prof. Campbell endorses Einstein," 6 Dec 1923; "Campbell sanctions theory of relativity," Lick newsclippings, 132, 135 (LO).

220. "Einstein again supported," *San Francisco Journal* (9 Dec 1923), ibid., 136.

221. Cf. Crelinsten (ref. 3), 286–432.

support and clarification on the part of those who wanted to go deeper into the theory. We also have the corollary that the serious attempts to contribute to relativity in America were by observation. Not only was it the eclipse test to which the Lick astronomers devoted their attention, but all the issues explored and debated surrounded specific empirical results. The great weight attached to Lick's announcement further attests to the high reputation held by American observations.

Much of the postwar argument prevailed over nonscientific issues. A large measure of these arose from the war and echoed controversies about Einstein and his theory that were raging in Europe. Furthermore, nationalist concerns at home involving American education and research fueled much of the rhetoric directed at the theory of relativity. Whichever side of the fence various scientists were on with regard to relativity, there was much at stake, and the ubiquity of news reporters prompted polarization of their positions. There was much posturing, from See's self-aggrandizing and promoting of his own theories, to California journalists' glorifying the local scientific community.

As the decade progressed, these tendencies grew stronger. The Lick and Mt. Wilson Observatories reaped the rewards of further verifications of empirical predictions and adopted the role of defenders of relativity in face of continued attacks from critics. Those who fought against the theory found it easier to get funding to repeat observational tests than to pursue theoretical analyses. Those who tried to make any theoretical headway only tended to repeat the more trivial attempts to discredit Einstein's calculations. They made the mistakes that others like See had perpetrated, and gradually lost credibility with their colleagues. Eddington was still able to score theoretical triumphs, and European scientists continued to come to the major research institutions. They were not only sought out in America for their theoretical knowledge, but they were also anxious to come to get a chance to work with the advanced technology available. In turn, the international reputation of American astronomy benefited greatly during this period, in particular at Lick and Mt. Wilson. Through it all, the press continued to monitor events and highlight the conflicts, debates and elements of human drama and national prestige. To a great extent, the appearance of Einstein's theory of relativity crystallized many facets of the national character of the scientific enterprise, and focused the attention of disciplinary leaders upon the strengths and weaknesses of their professional community.

When we come to assess Campbell's overall participation in the reception of Einstein's theory of relativity, this larger national context is quite illuminating. Both his motivations for carrying on the work and the influence that it exerted are understandable in terms of the internationally acknowledged prowess that his institution exhibited in the field of eclipse observations. Campbell adopted the Einstein problem into his eclipse program before it attracted wide attention among astronomers; and when it became a high-profile research problem, it was natural for him to vigorously pursue his initial lead. When the British success eclipsed his own ambiguous result, it became a matter of prestige to prosecute the research to a definite conclusion.

Campbell's lack of interest or facility in the theoretical side of the issue reflected an empirical philosophy toward his science that played a critical role in his overall participation in the reception of relativity. His concern with the Einstein problem began in a period of international cooperation among astronomers. Ruptured by the First World War, this spirit of international science gave way to one of national competition. While Germany was being excluded from multinational endeavours after the war, Einstein's theory became enmeshed in postwar attitudes toward reconciliation or continued retribution. Campbell's strictly empirical view of scientific progress acted as an antidote to the extra-scientific prejudices that were directing a large share of scientific and public opinion in the relativity debate. His maxim, "whatever is, is right," allowed him to stay clear of some of the murkier issues being brought into the discussion, and concentrate on the work that would bring credit to him, his colleagues, his institution, and to his science. His reticence to publish the earlier inconclusive results, while at the same time keeping influential colleagues at home and abroad in touch with each new development, was in part an attempt to circumvent the enthusiastic sensationalism being generated around relativity by the press. It also underscored the fact that the "Einstein problem" was not a simple one, and that the Lick astronomers were ensuring that it would be done properly or not at all.

When the 1922 eclipse expedition finally took place it was clear that Campbell had indeed refined the observational techniques greatly. After his results were published, the same philosophy that had protected him from taking a stand on the theoretical side of the issue, now acted in the other direction. Having obtained conclusive proof that light was bent in the sun's gravitational field by the amount predicted by Einstein, Campbell not only became the focus for antirelativity attacks, but he also found himself in the position of defending his observations, his own reputation and that of his colleagues and institution, and by association, the theory of relativity. In a similar manner to Dyson, who, after the British expeditions, began to send photographs around to colleagues and to try to understand the theoretical issue a bit more, Campbell now took on the responsibility of countering theoretical attacks (via Trumpler) and making public statements in defense of the Lick observations, Einstein, and relativity.

Looking back at the standard account of astronomers' role in the reception of general relativity, we must conclude that the Lick 1922 results have been undervalued. The 1919 British eclipse results can certainly be linked with public fascination that the relativity debate generated and surely helped to launch Einstein's personal world-fame as well as the myth that relativity is incomprehensible. Yet the scientific community demanded that the results be repeated before a final decision could be made. The Lick determination of the light-bending prediction three and a half years later influenced scientific opinion as much as the earlier British results had affected public opinion. To a great extent, the reception of general relativity was influenced by American observers almost totally ignorant of the theory, who defended their sophisticated observations from a strong position of leadership that characterized American science and technology of the interwar period.

## ABBREVIATIONS

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| AAAS | American Association for the Advancement of Science |
| AAS | American Astronomical Society |
| ASP | Astronomical Society of the Pacific |
| CIW | Carnegie Institution of Washington |
| CP | Heber Doust Curtis Papers, University of Pittsburgh |
| HA | Harvard University Archives, Pickering Collection |
| HM | Papers of George Ellery Hale, Robert A. Millikan Library, Caltech |
| IAU | International Astronomical Union |
| JPEC | Joint Permanent Eclipse Committee |
| LA | Lowell Observatory Archives |
| LC | Willem de Sitter Collection, Leiden Observatory |
| LO | Mary Lea Shane Archives, McHenry Library, University of California, Santa Cruz |
| MA | Heber Doust Curtis Collection, archives of the University of Michigan |
| MC | William F. Meggers Collection, Neils Bohr Library, American Institute of Physics |
| NA | National Academy of Sciences, Washington, D.C. |
| NAS | National Academy of Sciences |
| RAS | Royal Astronomical Society |
| RGO | Royal Observatory, Herstmonceux, England |
| RS | Royal Society |
| SA | Smithsonian Institution, Washington, D.C. |
| SM | Frank Schlesinger Collection, University of Pittsburgh |
| UC | University College, London |
| US | United States National Archives, Washington, D.C. |
| USNO | United States Naval Observatory. |

The archives of the AAS are at the Niels Bohr Library, American Institute of Physics, New York; I've used the microfilm copy of HM and SM preserved there.

Journal abbreviations are:

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| *AN* | *Astronomische Nachrichten* |
| *ApJ* | *Astrophysical journal* |
| *BMNAS* | NAS, *Biographical memoirs* |
| *DSB* | *Dictionary of scientific biography* |
| *JHA* | *Journal for history of astronomy* |
| *LOB* | Lick Observatory, *Bulletin* |
| *MNRAS* | RAS, *Monthly notices* |
| *PASP* | ASP, *Publications.* |