Prediction and Theory Evaluation: The Case of Light Bending

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Is a theory that makes successful predictions of new facts better than one that does not? Does a fact provide better evidence for a theory if it was not known before being deduced from the theory? These questions can be answered by analyzing historical cases. Einstein's successful prediction of gravitational light bending from his general theory of relativity has been presented as an important example of how "real" science works (in contrast to alleged pseudosciences like psychoanalysis). But, while this success gained favorable publicity for the theory, most scientists did not give it any more weight than the deduction of the advance of Mercury's perihelion (a phenomenon known for several decades). The fact that scientists often use the word "prediction" to describe the deduction of such previously known facts suggests that novelty may be of little importance in evaluating theories. It may even detract from the evidential value of a fact, until it is clear that competing theories cannot account for the new fact.

IN AUGUST 1989, PLANETARY SCIENTISTS FACED WHAT *Science* called a "final exam" when Voyager 2 tested their predictions about Neptune (*1*). Those predictions had been published in response to a challenge by A. J. Dessler, who had stated that "the classic test of a theory is its ability to predict. Successful predictions are so rare that they are usually regarded as compelling evidence in favor of the underlying theory" (*2*).

It appears that these scientists accept the claim by Karl Popper and other philosophers that prediction is an essential function of scientific theories, and that the confirmation of a prediction made before the empirical fact was known, especially if that fact is contrary to what might be expected on other theories, is stronger evidence for the theory than the explanation of a previously known fact (*3–9*). Other philosophers have disputed this claim, primarily on logical grounds (*10, 11*).

A stronger version of the claim is that the willingness to put a theory "at risk" by making falsifiable predictions is a necessary and sufficient condition for being "scientific." The criterion of "falsifiability" thus provides a line of demarcation between science and pseudoscience. Popper used the criterion to label as "pseudoscience" such theories as Marxism and psychoanalysis (*5*) and to cast doubt on the scientific character of evolutionary theories in biology (*12*).

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These remarks have provoked considerable discussion of the epistemological status of biological and psychological theories, including proposals that such theories should not have to satisfy the same criteria as those in the physical sciences (*13*). Even though Popper later admitted that his criticism of Darwinian evolutionary theory had been mistaken and defended its scientific character (*14*), considerable damage had been done. For example, creationists exploited Popper's statement that evolutionary theory is not scientific because it is not falsifiable; at least one of the legislative proposals (Maryland House of Delegates, Bill No. 1078, p. 3) calling for "equal time" for creationism and evolution argued that since "evolution-science like creation-science cannot be . . . logically falsified," it does not deserve preference in the classroom (*15*). Popper's criterion has also been incorporated into some definitions of "science literacy"—pollsters judge the public's understanding of the "scientific attitude" by whether the respondent can define it as "something like 'the advancement and potential falsification of generalizations and hypotheses' " (*16*, p. 441).

The credibility of psychologists and psychiatrists as scientists has recently come under attack because of their alleged inability to make accurate judgments about "the current, prior, or future state" of individuals subject to confinement or punishment (*17*). Someone's failure to make an accurate prediction of violent behavior in connection with the Massachusetts prison furlough program resulted in political damage to Michael Dukakis in the 1988 presidential election campaign. But in sciences that are on the verge of producing accurate predictions of unpleasant events such as earthquakes and genetic disease, critics suggest that the prediction itself may have undesirable effects on those at risk (*18*); perhaps the emphasis on predictiveness has been too great.

Although philosophers and sociologists have expressed doubts about whether one should try to characterize science by falsifiability or any other criterion (*19*), until quite recently there have been few serious attempts to find out whether scientists themselves give successful predictions the same weight that Popper claims they do (*20*). Thus Meehl asserts (*21*, p. 373) that "every working scientist (in any field!) that I have asked about this" says Popper is right in claiming that a fact found after a prediction counts more than one found before; but he names no specific scientist in support of his assertion. Rosenkrantz, in his contribution to the same volume of *Minnesota Studies*, acknowledges that scientists "never fail to pay lip-service" to this "old chestnut" but also "never fail to disregard it in practice whenever it tends to weaken the evidence for their own theories" (*11*, p. 83).

The first large-scale systematic effort to test this and other methodological claims against evidence from the history of science was planned and carried out by a group of scholars at Virginia Polytechnic Institute and State University (*22*). A striking result of their research project is that predictiveness or falsifiability was not considered important by key scientists in the three cases where this factor was examined: Finocchiaro's study of Galileo's work on the Copernican system, Hofmann's investigation of Ampère's electrodynamics, and Zandvoort's report on the reception of nuclear magnetic resonance (*23*). Independently, Worrall's detailed analysis of the role of the famous "bright spot" prediction in the acceptance of Fresnel's wave theory of light shows that temporal novelty was of no significance in this case (*24*).

I, along with other historians of science, had previously accepted the premise that successful predictions give at least a greater psychological advantage to a hypothesis than explanations of known facts (*25, 26*). However, my recent studies of theories of the origin of the solar system indicate that if a theory is not acceptable to the scientific community, it may not gain any credit at all from successful predictions (*27*).

In considering possible tests of Popper's thesis against historical evidence, it is desirable to keep in mind his distinction between predictions of events of a known kind (for example, eclipses) and predictions of new kinds of events (such as electromagnetic waves or synthesis of new elements) (*28*). Many statements about the predictive capability of theories refer only to the first kind of prediction, yet it is the second kind that seems much more significant in connection with possible differences between the physical and biological sciences. No one has claimed that the predictability of seasonal reproductive cycles (as expressed by Tennyson in "Locksley Hall": "In the spring a young man's fancy lightly turns to thoughts of love") makes biology a predictive science.

Popper reported that he was led to his falsifiability criterion partly because of the spectacular confirmation of Einstein's prediction of the gravitational bending of light (*29*) by the English eclipse expedition of 1919 (*30*). This case provided a stark contrast with the excessive flexibility (and hence untestability) of Adler's psychological theories with which Popper was working at the same time (*5, 31*); therefore, this Einstein prediction is the most appropriate case to examine first.

Testing the Consequences of General Relativity Theory

Light bending turns out to be a rather good example for our purposes. There is a large amount of scientific and popular literature that mentions the eclipse test of Einstein's theory (*32*). Moreover, one can judge the weight attributed to the prediction of light bending by comparison with two other tests that were discussed at the same time: the advance of the perihelion of Mercury and the gravitational redshift of spectral lines. The former, which will be called "Mercury's orbit" for convenience, was a well-known discrepancy that theorists had failed to explain successfully despite several decades of work; Einstein had managed to calculate the observed effect within the observational error without introducing any arbitrary parameters (*33*). The redshift was, like light bending, a prediction from general relativity theory, but its observational confirmation was still in doubt in the 1920s and remained so for several decades (*26, 34–37*). So one can inquire whether light bending provided better evidence for Einstein's theory than Mercury's orbit because it was a prediction.

The comparison is not quite fair because the quality of the data and the fundamental significance of the two effects are not the same. That difficulty is probably characteristic of all attempts to test simple philosophical theses in complex historical situations.

In explaining the origin of his falsifiability criterion, Popper also quoted Einstein's statement that "the general theory of relativity will be untenable" if the redshift prediction is disconfirmed (*38, 39*). But Popper seems not to have noticed that Einstein stuck to his theory even though that prediction was never satisfactorily confirmed during his own lifetime. [The confirmation by the Pound-Rebka experiment was not achieved until after his death (*40*).]

In looking at the technical literature one has to recognize that scientists, especially physicists, frequently use the word "prediction" in a more general sense that includes the deduction of previously known facts (*41*). Thus it is quite common to see references to the "three predictions" of general relativity theory, or to the "prediction" of Mercury's orbit. This usage may itself be partly responsible for creating the impression that scientists consider predictions important in evaluating theories; but once it is understood, it suggests that novelty is not considered an important feature. The mere statement that Einstein's theory gains credibility because it predicted the bending of light does not count as evidence for Popper's thesis, unless it is also stated or at least implied that the gain resulted to some extent from the prediction having come before the observation. When necessary I will use the term "forecast" to indicate this temporal element.

The eclipse results created enormous publicity for relativity theory and made Einstein the most famous scientist of the 20th century (*42, 43*). The public was impressed in part by his ability to forecast a striking new phenomenon; Einstein's fame or notoriety was enhanced by the suggestion that he possessed "secret and mysterious methods to harness enormous power and thus control, and maybe destroy, the ordinary person's life" (*43*, p. 288). But scientists, writing in technical journals and books addressed to other scientists, rarely ascribe such efficacy to any theory, although they often concede that the eclipse results brought Einstein's theory to their attention.

Einstein himself, though pleased by the eclipse results, gave them little weight as evidence for his theory. According to his student, Ilse Rosenthal-Schneider, after showing her a cable he received from Arthur Eddington about the measurements, Einstein remarked "But I knew that the theory is correct" (*44*, p. 255). When she asked what he would have done if the prediction had not been confirmed, he said "Then I would have been sorry for the dear Lord—the theory *is* correct" (*44*, p. 255). Later he wrote: "I do not by any means find the chief significance of the general theory of relativity in the fact that it has predicted a few minute observable facts, but rather in the simplicity of its foundation and in its logical consistency" (*45*, p. 183).

Eddington, the person primarily responsible for carrying out the eclipse observation project, was already convinced of the truth of Einstein's theory before making the observations. His reason for agreeing to undertake the expedition was not primarily to test general relativity but rather to escape the disgrace of refusing to serve his country in wartime. The astronomer royal, Frank Dyson, was the instigator of the project and arranged for Eddington, a pacifist, to be deferred from military service on the condition that he organize the eclipse expedition if the war ended in time (*39, 46*).

In his preliminary discussion of the results, Eddington stated that they confirmed only the "law" of propagation of light in a gravitational field—the mathematical formula for the interval *ds*—but not Einstein's general theory (*47*). He referred to both Mercury's orbit and light bending as "predictions" of the theory (*48*). Reflecting on the status of relativity in 1923, Eddington wrote (*49*, preface):

> The present widespread interest in the theory arose from the verification of certain minute deviations in the theory from Newtonian laws. To those who are still hesitating and reluctant to leave the old faith, these deviations will remain the chief centre of interest; but for those who have caught the spirit of the new ideas the observational predictions form only a minor part of the subject. It is claimed for the theory that it leads to an understanding of the

world of physics clearer and more penetrating than that previously attained.

As Eddington asserted in a famous dictum, one should not "put overmuch confidence in the observational results that are put forward *until they have been confirmed by theory*" (*50*, p. 211; *51*). Indeed, even the so-called "fact" that a star is located in a certain position in the heavens is a consequence of Einstein's *theory* which determines how much light may deviate from a straight line (*52*).

In the initial excitement caused by the announcement of the eclipse results, several scientists made extravagant statements that one might take to imply that general relativity should be considered favorably because Einstein had predicted the results in advance. Among them were five of the most influential physicists in Britain, France, Germany, and Holland. J. J. Thomson proclaimed to the Royal Society of London that "this is the most important result obtained in connection with the theory of gravitation since Newton's day" (*53*, p. 311) and asserted (*53*, p. 316): "Though there are some hundreds of theories of gravitation Einstein's is the only one which has predicted a result which has been verified by experience."

Similarly, Langevin pointed out to the Paris Academy of Sciences (*54*, p. 831):

> This theory is the *only one* that permits one actually to represent all the known experimental facts and that possesses moreover the remarkable power of prediction confirmed in so astonishing a manner by the deviation of light rays and the displacement of spectral lines in the gravitational field of the sun.

Max Born, describing the successful prediction of light bending, wrote the following (*55*, p. 233): "Since this greatest achievement of modern prophesy the Einstein doctrine stands as a more secure possession of science." Max von Laue stated in a somewhat more conditional tone (*56*, p. 12): "If it is ultimately confirmed, Einstein's prediction of light bending will be one of the greatest triumphs of the human spirit."

H. A. Lorentz, discussing the eclipse result and Einstein's theory in a newspaper article, said the observation has "established the conviction that the formulation of this theory is one of the most important steps ever taken in physics" (*57*, p. 264). In another public statement he asserted that light bending is "more remarkable" than the deduction of Mercury's orbit "because it has a bearing upon a phenomenon which formerly could not be imagined" (*58*, p. 46).

But elsewhere these same physicists suggested that the prediction of light bending was not so important after all. Thomson would not abandon his belief in the ether and found general relativity too abstruse to be acceptable; Langevin would not give more weight to light bending than to Mercury's orbit; Born and Laue omitted the above-quoted statements from later editions of their books; and Lorentz did not mention light bending at all when he discussed general relativity in his lectures at the California Institute of Technology (*59*).

Even before the results had been published, some scientists warned that a successful prediction would not immediately confirm Einstein's theory but would instead stimulate a search for other explanations of light bending (*60*). And indeed that is exactly what happened, even though one of them, H. F. Newall, admitted it is a "cheap criticism to make a qualitative suggestion in opposition to a quantitative result" (*61*, pp. 395–396; *62*). Furthermore, a relativity advocate, R. D. Carmichael, reminded an audience that such alternative explanations did not necessarily constitute a "blow" to relativity theory as long as the latter gave the "most convenient and agreeable" account of the facts (*63*).

Although the more trivial suggestions—that bending could be attributed to ordinary refraction in the atmosphere around the sun or Earth—did not survive for long, respectable physicists such as Larmor and Weichert published elaborate attempts to develop electromagnetic explanations of light bending (*64*). Other physicists accepted light bending as a phenomenon but did not see it as confirmatory evidence for the theory from which it had first been predicted (*65, 66*). Einstein himself admitted that light bending observations could not yet distinguish between his theory and a proposed alternative "cosmic refraction" effect (*67*). Even the philosopher-mathematician Alfred North Whitehead, whose first-hand account of the dramatic announcement of the eclipse results is often quoted, was not persuaded to adopt Einstein's space-time curvature ideas (*68*).

Most of the published comments by physicists during the first 2 or 3 years after the 1919 eclipse observation indicated that light bending and Mercury's orbit counted equally strongly in favor of general relativity (*65, 69*). If light bending was more important that was not because it had been forecast in advance, but because the data themselves were more definitive (*50, 70*).

Doubts About the Importance of Light Bending

It later became clear to the experts that the Mercury orbit was stronger evidence for general relativity than light bending. In part this was because the observational data were more accurate—it was very difficult to make good eclipse measurements, even with modern technology (*36, 37, 71*)—and in part because the Mercury orbit calculation depended on a "deeper" part of the theory itself (*36, 72*). The fact that light bending was a forecast whereas the Mercury orbit was not seems to count for little or nothing in these judgments. In fact, one cosmologist, Willem de Sitter, asserted that all the evidence previously found to support Newton's theory of gravitation also supports general relativity in those cases (the vast majority) where they have the same empirical consequences (*73*). This would imply that any such evidence that had been forecast by Newton's theory but not by Einstein's counts no more for the former than for the latter.

But the most significant argument (though it was not often explicitly stated) is that, rather than light bending providing better evidence because it was predicted before the observation, it actually provides less secure evidence for that very reason. This is the case at least in the years immediately following the announcement of the eclipse result, because scientists recognized that any given empirical result might be explained by more than one theory. Because the Mercury orbit discrepancy had been known for several decades, theorists had already had ample opportunity to explain it from Newtonian celestial mechanics and had failed to do so except by making implausible ad hoc assumptions (*33*). This made Einstein's success all the more impressive and made it seem quite unlikely that anyone else would subsequently come up with a better alternative explanation. Light bending, on the other hand, had not previously been discussed theoretically (with rare exceptions), but now that the phenomenon was known to exist one might expect that another equally or more satisfactory explanation would be found (*74*). It was only 10 years after the initial report of light bending observations that Einstein's supporters could plausibly assert, as did R. J. Trumpler (*75*, p. 218), that

> No other theory is at present able to account for the numerical values of the observed displacements. The assumption that there is an actual curvature of space in the immediate surroundings of the Sun, which is implied in Einstein's theory, seems indeed to furnish the only satisfactory explanation why the observed light deflections are twice as large as those predicted on the basis of Newton's theory.

But even that statement may exaggerate the importance of the light bending observation in the reception of general relativity. Many scientists have asserted that none of the empirical tests is as convincing as the coherence and beauty of the theory itself (*76, 77*); some go so far as to say that even if all its predictions were falsified, the theory should still be retained (*78*). Conversely, some opponents of relativity assert that even if there were perfect agreement between its predictions and the results of observation, it would still not be an acceptable theory (*79*).

The view that Eddington's confirmation of the light bending prediction "won wide acceptance for general relativity" continues to be expressed in popular or philosophical writings on science (*77, 80*, p. 54; *81*), though some of the scientists who stress the value of forecasting have weakened their case by the way they treat forecasts in works written for a scientific audience (*82*).

In the post-1923 technical literature I have examined, only one physicist explicitly states that light bending constituted better evidence because it was a forecast. Tolman stated that the verification of Einstein's theory by "the three so-called crucial tests" was (*83*, p. 213)

> all the more significant, since the advance in the perihelion of Mercury was the only one of the three phenomena in question which was actually known at the time when Einstein's theory was developed, and the effects of gravitation both in determining the path and wave-length of light had not even been observed as qualitative phenomena prior to their prediction by the theory of relativity.

Unless several more examples are discovered, I will have to conclude that Tolman represents only a small minority, and that in the case of gravitational light bending most scientists ascribed essentially no weight to the mere circumstance that the phenomenon was predicted before it was observed. The majority view is stated in a book by Sachs (*84*, p. 193): the Mercury orbit test was "not as spectacular . . . because the theoretical result came after the experimental facts were known. But this test was certainly as important as the other two. The timing of experimental confirmation of a theory should have nothing to do with its significance for the scientific truth of that theory." This is in accordance with the views of philosophers who have denied the significance of forecasts (*10, 11*).

So the main value of a successful forecast (as compared to a successful deduction of a known fact) is favorable publicity. The forecast itself, even if refuted, may of course advance science by causing scientists to perform an experiment that might not otherwise have been done until much later. Even those physicists who rejected Einstein's general relativity theory had to admit that his prediction had led to the discovery of an important fact about nature. The confirmation of the light bending prediction certainly did force scientists to give serious consideration to a theory that they might otherwise have ignored or rejected. This is by no means a negligible factor in a situation where many theories compete for attention, and those that seem to violate established ideas about the world can easily be dismissed. The eclipse results put relativity much higher on the scientific agenda and provoked other scientists to try to give plausible alternative explanations. But light bending could not become reliable evidence for Einstein's theory until those alternatives failed, and then its weight was independent of the history of its discovery.

Are Theorists Less Trustworthy Than Observers?

The reason why some philosophers and scientists want to give more credit to forecasts is presumably their suspicion that theorists may be influenced in reaching their conclusions by knowledge of the phenomena to be explained. But is it not just as likely that observers will be influenced in reporting their results by knowledge of theoretical predictions of those results? Some opponents of relativity theory have suggested that the astronomers who observed light bending exaggerated the agreement between their results and Einstein's prediction because they were already supporters of the theory (*66, 85*). Hetherington argues that the history of the third classical test of general relativity, the gravitational redshift, illustrates this influence (*26*). In effect, the preference for forecasting implies a double standard for theorists and observers, based on a discredited empiricist conception of science. In view of the increasing evidence that (as suggested by the Einstein and Eddington statements quoted above) observations are not intrinsically more reliable sources of knowledge than theories, perhaps it would be just as reasonable (or unreasonable) to give more weight to observations performed before rather than after a theoretical prediction (*86*).

Conclusions

The claim is sometimes made that successful prediction gives more credit to a theory than deduction of known facts. But it is difficult to find clear-cut evidence for this claim in the technical writings of scientists. A successful prediction may yield much favorable publicity for a theory (including statements that call attention to the novelty of the phenomenon predicted) and thereby force other scientists to give it serious consideration. But subsequent evaluations of the theory in the technical literature do not seem to give greater weight to the prediction of novel facts than to persuasive deductions of known facts.

The word prediction is used by scientists to mean a deduction of a known fact as well as a forecast of a new fact. This usage itself implies that novelty is not of great significance. In the case of general relativity, the prediction of the known discrepancy in the advance of Mercury's perihelion was almost always considered to be just as good evidence for the theory as the forecast of light bending. There is even some reason to suspect that a successful explanation of a fact that other theories have already failed to explain satisfactorily (for example, the Mercury perihelion) is more convincing than the prediction of a new fact, at least until the competing theories have had their chance (and failed) to explain it.

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38. See (*31*), p. 38. The passage quoted by Popper is in *Relativity: The Special and General Theory* (Crown Publishers, New York, paperback ed., 1960), p. 159. G. Holton points out that this statement appeared in the 1920 edition of Einstein's *Ueber die spezielle und die allgemeine Relativitätstheorie* (Vieweg, Braunschweig, ed. 7, 1920), but not in earlier editions and printings of 1917, 1918, and 1919. In the first 15 printings of the book Einstein "acknowledged that his general relativity theory so far had had only one observable consequence, the precession of the orbit of Mercury, whereas the predicted bending of light and of the redshift of spectral lines owing to the gravitational potential were too small to be then observed." He ended the book with the sentence, "I do not doubt at all that these consequences of the theory will also find their confirmation" [G. Holton, *The Advancement of Science and its Burdens* (Cambridge Univ. Press, New York, 1986), pp. 8–9 and 306].
39. See also the letter from A. Einstein to A. S. Eddington, 15 December 1919, quoted by A. V. Douglas [*The Life of Arthur Stanley Eddington* (Nelson, London, 1956), p. 41]: "If it were proved that this effect does not exist in nature, then the whole theory would have to be abandoned."
40. R. V. Pound and G. A. Rebka, Jr., *Phys. Rev. Lett.* **4**, 337 (1960).
41. H. Margenau states that "the word *prediction*, as used in science, does not mean 'forecast' in a temporal sense. *Pre-* implies 'prior to completed knowledge'; it does not contrast with *post-*, as does *ante*. The counterpart to *prefix* is not postfix but *suffix*. It is therefore unnecessary to coin a new word, *postdiction*, to denote what we should call prediction of the past. The use of this word, though it has been suggested, would seem a bit *prepost*erous" [H. Margenau, *The Nature of Physical Reality* (McGraw-Hill, New York, 1950), p. 105]. For other statements defending this double meaning of prediction, see A. P. French [*Newtonian Mechanics* (Nelson, London, 1971), p. 5], H. Pietschmann [*Found. Phys.* **8**, 905 (1978)], and A. J. Rocke [in *Scrutinizing Science*, A. Donovan *et al.*, Eds. (Kluwer, Boston, 1988), pp. 145–161]. An extended discussion of the usage of the term prediction is given by A. N. Strahler [*Science and Earth History: The Evolution/Creation Controversy* (Prometheus Books, Buffalo, NY, 1987), pp. 15–25]. According to Worrall, the attention paid to novel predictive success goes back to the 19th century [J. Worrall, *Br. J. Philos. Sci.* **39**, 263 (1988)].
42. Responses to relativity theory and to the eclipse test in particular have been surveyed by many historians; for example: S. Goldberg, *Understanding Relativity* (Birkhäuser, Boston, 1984); J. Crelinsten, *Phys. Teacher* **18**, 115, 187 (1980); V. V. Raman, *Indian J. Hist. Sci.* 7, 119 (1972); T. F. Glick, Ed., *The Comparative Reception of Relativity* (Reidel, Boston, 1987); D. F. Moyer, in *On the Path of Albert Einstein*, A. Perlmutter and L. F. Scott, Eds. (Plenum, New York, 1979), pp. 55–101; J. Eisenstaedt, *Arch. Hist. Exact Sci.* **35**, 115 (1986). The philosophical aspects of this case are analyzed by R. Laymon [*PSA 1982* (Philosophy of Science Association, East Lansing, MI, 1982), vol. 1, pp. 107–121]. D. Mayo (in preparation) provides a detailed analysis of the eclipse observation as a "severe test" of general relativity to illustrate a general argument about novelty in testing theories.
43. M. Missner, *Soc. Stud. Sci.* **15**, 267 (1985).
44. I. Rosenthal-Schneider, manuscript dated 23 July 1957, quoted by G. Holton, *Thematic Origins of Scientific Thought* (Harvard Univ. Press, Cambridge, MA, rev. ed., 1988).
45. A. Einstein, *Forum Philosophicum* **1**, 173 (1930); quote from *ibid.* (Engl. transl.), p. 183. A. Paris [*'Subtle is the Lord': The Science and Life of Albert Einstein* (Oxford Univ. Press, New York, 1982), p. 273] gives a slightly different translation. See also B. Hoffmann [*Relativity and Its Roots* (Freeman, New York, 1983), pp. 157–158].
46. S. Chandrasekhar, *Am. J. Phys.* **47**, 212 (1979); J. Earman and C. Glymour, *Hist. Stud. Phys. Sci.* **11**, 49 (1980). On Eddington's knowledge of relativity theory before 1919, see J. Stachel [in *The Prism of Science*, E. Ullmann-Margalit, Ed. (Reidel, Boston, 1986), p. 225–250].
47. A. S. Eddington, *Observatory* **42**, 393 (1919).
48. ———, *Mon. Not. R. Astron. Soc.* **80**, 96 (1919); *Space, Time and Gravitation* (Cambridge Univ. Press, London, 1920), pp. 123–124. The report by Dyson *et al.* (*30*), states that general relativity theory is confirmed by both the Mercury orbit analysis and by light bending but gives no preference to the latter because of its novelty. See also the discussion report in *Proc. Phys. Soc. London* **32**, 245 (1920), in which Eddington mentions the confirmation by light bending and Mercury's orbit (without distinguishing the former as a forecast), and the other discussants do not mention light bending at all.
49. ———, *The Mathematical Theory of Relativity* (Cambridge Univ. Press, London, 1923).
50. ———, *New Pathways in Science* (Cambridge Univ. Press, London, 1934; reprinted by the Univ. of Michigan Press, Ann Arbor, 1959).
51. Another version of this aphorism is attributed to D. K. Bobylev: "No man should ever undertake experimental investigations until and unless they are demanded by a 'solid theory' "; see G. A. Tokaty, *A History and Philosophy of Fluid Mechanics* (Foulis, Henley-on-Thames, England, 1971), p. 126.
52. A. S. Eddington, *The Expanding Universe* (Cambridge Univ. Press, London, 1933), pp. 18–19.
53. J. J. Thomson, *Proc. R. Soc. London* **96**, 311 (1919).
54. "Cette théorie est la *seule* qui permette actuellement de représenter l'ensemble des faits expérimentaux connus et qui possède en outre la remarquable puissance de prévision confirmée de manière si éclatante par la déviation de rayons lumineux et le déplacement des raies spectrales dans le champ de gravitation du Soleil" [P. Langevin, *C. R. Acad. Sci.* **173**, 831 (1921)].
55. "Seit dieser grössten Leistung moderne Prophetie kann die Einsteinsche Lehre als gesicherter Besitz der Wissenschaft gelten" [M. Born, *Die Relativitätstheorie Einsteins, Gemeinverständlich Dargestellt* (Springer, Berlin, 1920)].
56. "Bestätigt sie sich engültig, so stellt Einsteins Voraussage der Lichtablenkung einen der grössten Triumphe des menschlichen Geistes dar" [M. von Laue, *Die Relativität-*

stheorie (Vieweg, Braunschweig, ed. 2, 1923)].
57. H. A. Lorentz, *Nieuwe Rotterdamsche Courant*, 13 November 1919; reprinted in his *Collected Papers* [P. Zeeman and A. D. Fokker, Eds. (Nijhoff, The Hague, 1935–1939), vol. 9, pp. 264–275] (quotation translated by J. M. H. Levelt Sengers).
58. ——, *The Einstein Theory of Relativity: A Concise Statement* (Brentano, New York, 1920).
59. J. J. Thomson, *Recollections and Reflections* (Bell, London, 1936), pp. 432–433. On the reception of relativity by Thomson and other British physicists, see A. Warwick [*Ideas Production* 7, 82 (1987)]; P. Langevin, paper presented at the meeting of the Societé Française de Physique, 6 February 1920; in *Bibliotheque de Synthèse Scientifique*, E. Chiron, Ed. (Paris, 1922); *Oeuvres Scientifiques* (Centre National de la Recherche Scientifique, Paris, 1950), pp. 434–435 and 464–465. M. Born, *Einstein's Theory of Relativity* (reprinted by Dover, New York, ed. 3, 1962), pp. 359–360; M. von Laue, *Relativitätstheorie* (Vieweg, Braunschweig, ed. 3, 1953); H. A. Lorentz, *Problems of Modern Physics* (Ginn, Boston, 1927; reprinted by Dover, New York, 1967).
60. R. Jonckheere, *Observatory* **41**, 215 (1918); O. Lodge, *Nature* **104**, 15, 82 (1919).
61. H. F. Newall, *Observatory* **42**, 395 (1919).
62. H. Thirring praised Einstein's "successful prophecy" and discounted other attempts to explain light bending: "Of course it is always possible to explain an established natural fact by some hypothesis invented *ad hoc*, but Einstein's original explanation, based as it is on the compulsion of profound thought, will be given preference" [H. Thirring, *Die Idee der Relativitätstheorie, Gemeinverständlich Dargestellt* (Springer-Verlag, Vienna, ed. 3, 1948), pp. 112 and 114; quotation from *The Ideas of Einstein's Theory: The Theory of Relativity in Simple Language*, R. A. B. Russell, transl. (Methuen, London, 1921), pp. 107–108. B. Cabrera also criticized those who tried to explain away the result; see T. F. Glick, *Einstein in Spain* (Princeton Univ. Press, Princeton, NJ, 1988), p. 43. For discussion of other proposed explanations, see E. Freundlich [*Naturwissenschaften* **8**, 667 (1920)], and L. Silberstein [*Mon. Not. R. Astron. Soc.* **80**, 630 (1919–1920)].
63. R. D. Carmichael *et al.*, *A Debate on the Theory of Relativity* (Open Court, Chicago, 1927), pp. 128–148. None of the participants in this debate (R. D. Carmichael, H. T. Davis, W. D. MacMillan, and M. E. Hufford) suggested that light bending is better evidence for relativity just because it is a forecast.
64. E. Wiechert, *Ann. Phys.* **63** (ser. 4), 301 (1920); J. Larmor, *Philos. Mag.* **45** (ser. 6), 243 (1923); *Nature* **119** (suppl.), p. 49 (1927).
65. J. H. Jeans, *Proc. R. Soc. London* **A97**, 66 (1920); L. T. More, *Philos. Mag.* **42** (ser. 6), 841 (1921); L. Silberstein, *Observatory* **42**, 396 (1919); F. A. Lindemann, *Mon. Not. R. Astron. Soc.* **80**, 114 (1919); P. Painleve, *C. R. Acad. Sci.* **173**, 677 (1921); *ibid.*, p. 873; *ibid.* **174**, 1137 (1922); L. Le Cornu, *ibid.*, p. 337; H. Malet, *ibid.* **188**, 443 (1929); A. B. Bäcklund, *Ark. Mat. Astron. Fys.* **15** (no. 14), 20 (1921); O. Meissner, *Phys. Z.* **22**, 183 (1921); J. Hopmann, *ibid.* **24**, 476 (1923); J. Kunz, *ibid.* **31**, 83 (1930); E. Gehrcke, *Z. Techn. Phys.* **1**, 123 (1920); H. F. Newall, *Mon. Not. R. Astron. Soc.* **80**, 22 (1919); S. Mohorovicic, *Z. Phys.* **18**, 34 (1923); G. v. Gleich, *ibid.* **25**, 230 (1924); R. Hiecke, *ibid.* **24**, 117 (1924).
66. M. E. Hufford, in (*63*), pp. 64–116.
67. A. Einstein, *Phys. Z.* **24**, 484 (1923). For arguments against this effect, see H. Kienle [*ibid.* **25**, 1 (1924)] and A. Kopff (*ibid.*, p. 95).
68. A. N. Whitehead, *Science and the Modern World* (Macmillan, New York, 1925; reprinted by Mentor, New York, 1948), pp. 11 and 124–125; *The Principle of Relativity* (Cambridge Univ. Press, London, 1922), p. 10.
69. L. Bolton, *An Introduction to the Theory of Relativity* (Dutton, New York, 1921), p. 155; R. D. Carmichael, *The Theory of Relativity* (Wiley, New York, ed. 2, 1920), pp. 103–105; C. P. Steinmetz, *Four Lectures on Relativity and Space* (McGraw-Hill, New York, 1923), pp. 12–13 and 60–61; H. Reichenbach, *Axiomatization of the Theory of Relativity*, M. Reichenbach, Transl. and Ed. (Univ. of California Press, Berkeley, 1969), pp. 168–170 (first published in German, 1924).
70. W. Pauli, Jr., *Encyclopädie der Mathematischen Wissenschaften mit Einschluss ihrer Anwendungen*, A. Sommerfeld, Ed. (Teubner, Leipzig, 1921), vol. 5, part 2, pp. 539–775; L. Rougier, *La Matière et l'Énergie selon la Theorie de la Relativité et la Theorie des Quanta* (Gauthier-Villars, Paris, ed. 2, 1921), pp. 76–77.
71. H. von Klüber, *Vistas Astron.* **3**, 47 (1966).
72. P. G. Bergmann, *Introduction to the Theory of Relativity* (Prentice-Hall, Englewood Cliffs, NJ, 1942), pp. 217–221; H.-Y. Chiu and W. F. Hoffmann, *Gravitation and Relativity* (Benjamin, New York, 1964), pp. xxiv–xxvi; W. J. Kaufmann III, *The Cosmic Frontiers of General Relativity* (Little, Brown, Boston, 1977), pp. 70–74; S. Weinberg, *Gravitation and Cosmology* (Wiley, New York, 1972), p. 198.
73. W. de Sitter, *Kosmos: A Course of Six Lectures on the Development of Our Insight into the Structure of the Universe* (Harvard Univ. Press, Cambridge, MA, 1932), p. 111.
74. E. Cunningham, *Nature* **104**, 354, 374, 394 (1919); P. Langevin, *Oeuvres Scientifiques* (*58*, p. 464); H. W. Carr, *The General Principle of Relativity in its Philosophical and Historical Aspects* (Macmillan, London, 1920), pp. 9 and 136–137. Max Planck declared that "the success of a new theory in physics cannot be decided according to its logical consistency with accepted notions, but rather by the test whether or not it explains and co-ordinates certain facts already ascertained, but which cannot be explained on any other grounds except that of the new hypothesis" [*Where is Science Going*?, J. Murphy, Transl. and Ed. (Allen and Unwin, London, 1933; reprinted by Ox Bow, Woodbridge, CT, 1981), p. 178]. H. Frankel [in *Scrutinizing Science* (*23*), pp. 269–287] stated that scientists behaved similarly in cases he has studied in modern geophysics: the real test is whether rival theories can explain the new phenomenon, not "who got there first" with the prediction.
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