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Physics in Perspective

Why was Relativity Accepted?

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Historians of science have published many studies of the reception of Einstein's special and general theories of relativity. Based on a review of these studies, and my own research on the role of the light-bending prediction in the reception of general relativity, I discuss the role of three kinds of reasons for accepting relativity: (1) empirical predictions and explanations; (2) social-psychological factors; and (3) aesthetic-mathematical factors. According to the historical studies, acceptance was a three-stage process. First, a few leading scientists adopted the special theory for aesthetic-mathematical reasons. In the second stage, their enthusiastic advocacy persuaded other scientists to work on the theory and apply it to problems currently of interest in atomic physics. The special theory was accepted by many German physicists by 1910 and had begun to attract some interest in other countries. In the third stage, the confirmation of Einstein's light-bending prediction attracted much public attention and forced all physicists to take the general theory of relativity seriously. In addition to light-bending, the explanation of the advance of Mercury's perihelion was considered strong evidence by theoretical physicists. The American astronomers who conducted successful tests of general relativity became defenders of the theory. There is little evidence that relativity was "socially constructed" but its initial acceptance was facilitated by the prestige and resources of its advocates.

Key words: Relativity theory; reception; predictions; social construction; birth order; aesthetic factors; mathematics; astronomy; gravitational light-bending; advance of perihelion of Mercury; Michelson-Morley experiment; Albert Einstein; Arthur Stanley Eddington.

Introduction

How does a new theory or discovery get accepted or rejected by a scientific community? Historians of physics have studied in considerable detail the origins of Galileo's kinematics, Newton's dynamics, Fresnel's optics, Maxwell's electromagnetism, Einstein's relativity, Rutherford's nucleus, and Schrödinger's wave mechanics. With one exception, considerably less attention has been given to the process by which such innovations become part of established knowledge about the physical world. In the absence of historical research, philosophers and sociologists of science

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have been free to postulate rules for testing theories and doctrines about the construction of knowledge. Do those rules and doctrines correspond to scientific practice, past and present?

"Reception studies" form a recognized genre of historical research. Using an electronic database, the *Isis Cumulative Bibliography of the History of Science*, one can easily retrieve several hundred such studies. Most of them deal with one of only three theories: Darwinian evolution, Freudian psychoanalysis, and Einsteinian relativity.* Moreover, many reception studies do not inquire into the reasons why scientists accepted the theory, and they may include a number of philosophers and intellectuals who are not competent to judge the technical validity of the theory. In order to have a reasonably large sample of reception studies that report reasons why scientists accepted a theory, I have concentrated on relativity.

One might object that it is impossible to know the "real" reason why someone accepts a theory, and that the reasons given in published works are less accurate than those in private correspondence or interviews. Such objections must be taken seriously if one wants to determine why a particular scientist accepted a theory. Indeed, some reception theories do deal with only one person. But I am concerned primarily with reception by a community and with reception by individuals only to the extent that they were leaders who influenced others. Letters written by influential individuals do provide useful evidence, but so do published papers and even textbooks, which help to pass on the core of accepted knowledge to the next generation. Published works provide a useful first approximation to the views of the community; letters and interviews may give a second approximation. But what one finds in many works on the history of science is only a *zero*-level approximation: claims are made (*e.g.*, "general relativity was accepted because of the observation of gravitational light bending," or "Maxwell's electromagnetic theory was accepted because of Hertz's experiments on electromagnetic waves") on the basis of published statements of only one scientist, or even none at all. The writer seems to assume that just because an experiment is now seen as confirming a theory, it persuaded scientists to accept that theory when it was performed.

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* The *Isis Cumulative Bibliography*, published annually under the title *Current Bibliography* as a separate issue of *Isis*, is available back to about 1975 through the electronic database RLIN in the HST file. A search for items with the word "reception" in their titles yields more than 100 books and articles; nearly 100 more can be retrieved by searching for words such as "response" and "reaction." Unfortunately the only clue some authors give in their title that they are doing a reception study is the word "in" (*e.g.*, "Darwin in China" or "Einstein in America"). In this sample about 20% of all such studies are devoted to Darwinian evolution, 18% to relativity and 15% to Freudian psychoanalysis; quantum theory comes in a distant fourth at 3%.

A more comprehensive search of humanities and social science literature would probably increase the proportion of Freud and Darwin studies. But note that the term "reception theory" has a specialized meaning in literary criticism, as explained by Holub (ref. 1). It was promoted in Germany by Hans Robert Jauss as a new Kuhnian paradigm in the 1960s, and is now pursued in the U.S. as an alternative version of "reader-response theory." In most cases the theorist analyses the reception of a literary work by a hypothetical or "implied" reader who is a projection of himself or herself. There are some empirical studies using groups of professors, students or other kinds of readers, but Holub considers these studies a "colossal waste of energy and resources" that only prove what the theorist already knows (p. 137) and are "a vestige of superannuated scientism" (p. 145).

For simplicity I will divide the reasons for accepting a theory into three general categories: first, empirical tests; second, social or psychological factors; third, aesthetic or mathematical evaluations of the theory itself. After some brief comments on each of these categories, I review the conclusions of historians, including a summary of my own research comparing the role of gravitational light bending and the advance of Mercury's perihelion in the acceptance of general relativity in the 1920s. (Many historians treat "relativity" as a single theory to be accepted or rejected, but in some cases it is possible to distinguish between the reception of the special and the general theory.)

Empirical Predictions and Explanations

Among the many empirical consequences of relativity the following are often mentioned:

- Advance of perihelion of planet Mercury (Le Verrier, 1859)
- Failure to detect the Earth's motion through space (Michelson and Morley, 1887)
- Mass increase with velocity, by factor $1/\sqrt{(1 - v^2/c^2)}$ (Bucherer, 1908; Neumann, 1914; Guye and Lavanchy, 1916)
- Effect on electron orbits and spectrum in Bohr model of atom as calculated by Sommerfeld (Paschen, 1916)
- Gravitational bending of light (Eddington *et al.*, 1919)
- Existence of positron predicted by Dirac from his wave equation (Anderson, 1932)
- Deviation of atomic weights from Prout's hypothesis, owing to binding energy of nuclear forces (Aston, 1919–1927; Bainbridge 1933); partial mass-energy transformation in nuclear reactions (Cockcroft and Walton, 1932)
- Complete mass-energy transformation in electron-positron creation and annihilation (Blackett and Occhialini 1933, Klemperer 1934)
- Time dilation (Kennedy and Thorndike, 1932; Ives and Stilwell, 1937)
- Gravitational redshift of spectral lines (Pound and Rebka, 1960)

Note that the names and dates given above refer to experiments that were considered confirmations at the time; in some cases (*e.g.*, mass increase with velocity) later work has thrown doubt on their accuracy.

In deciding why scientists accepted relativity, it may be relevant to ask:

- Which of these facts were already known before Einstein proposed his theory, and did he propose relativity in order to explain them?
- Did facts predicted by the theory and later confirmed count more heavily than previously-known facts explained by the theory?
- Which facts were also predicted or explained by a competing theory?
- Which facts became known before relativity was generally accepted, and thus could have influenced its acceptance?

- If a fact is, strictly speaking, evidence only for the special theory of relativity, does it also influence scientists to accept the general theory – and conversely?

These questions are not easy to answer. For example, most people believed for many years that Einstein proposed the special theory to explain the negative result of the Michelson-Morley experiment, and this belief is still found in some physics textbooks and popularizations.[2] But, as a result of substantial historical research by Gerald Holton, it is now clear that while Einstein was aware of the failure to observe the Earth's motion, this problem was not the primary motivation for his research, and had only a small and indirect effect on his early work.[3]

Since the 19th century, philosophers have argued about what reasons *should* persuade scientists to accept a theory. More recently, some have claimed that scientists actually do choose theories according to definite rules, thus reinforcing (or sometimes conflating) a normative (prescriptive) account of scientific procedure with a descriptive account. One of the most popular rules is the "hypothetico-deductive method": one should deduce the consequences of a theoretical hypothesis, and then compare those consequences with observations and controlled experiments. As the philosopher Karl Popper pointed out, this method cannot prove that a theory is correct (since other theories could lead to the same consequences) but it can disprove a theory. So scientists should prefer a theory that has survived many attempts to disprove it over one that has survived only a few or has actually been disproved.

The rule is often summarized by saying: a theory should make testable *predictions*. Popper and some other philosophers argued that the confirmation of a prediction made before an empirical fact was known, especially if that fact is contrary to what might be expected on the basis of accepted theories, is stronger evidence than the explanation of a previously-known fact. Such predictions are now called *novel*, to distinguish them from deductions or explanations (sometimes called "retrodictions") of known facts. Other philosophers say that novelty should not make any difference: if a fact supports a theory, how can it matter, logically, *when* you knew that fact?[4]

According to Popper, a theory that cannot be disproved or "falsified" by testing its novel predictions, but is so flexible that it can explain any empirical fact, should not be considered a scientific theory at all. "Falsifiability" rather than "truth" thus provides a criterion for distinguishing science from pseudoscience. Popper recalled in his autobiography that he was led to his falsifiability criterion because of the spectacular confirmation of Einstein's prediction of the gravitational bending of light by the British eclipse expedition of 1919. This case provided a stark contrast with the excessive flexibility (and hence untestability) of Alfred Adler's psychological theories with which Popper was working at the time.[5]

The falsifiability criterion proved to be unsatisfactory because it stigmatized as pseudoscience several theories that are generally regarded as scientific, while labeling as science some theories generally regarded as unscientific.* While many

* Popper himself declared that Darwinian evolution is not a scientific theory by his criterion, then reversed himself. Conversely, most biologists would argue that creationism is falsifiable and has been falsified, but would not therefore consider it scientific. More generally, any theory that deals with phenomena taking place over such large ranges of time and space that they cannot be brought into the laboratory for controlled experiments (*e.g.*, many theories in geology and astronomy) are unfalsifiable yet scientists consider them scientific. In many such cases one could make novel predictions about future events (evolution of new species, rejoining of drifted continents, collapse of the universe) but the theorist would not live long enough to see the prediction tested. See also Laudan (ref. 6).

philosophers have abandoned the search for a criterion to demarcate science from pseudoscience, scientists increasingly feel the need to explain to the public, Congress, and the courts why their own theories and evidence should be given more credibility than creationism, astrology, and alternative medicine.

The preference for novel predictions is often associated or confused with the dislike of *ad hoc* hypotheses. For example, G. F. FitzGerald explained the negative result of the Michelson-Morley experiment by postulating that "the length of material bodies changes, according as they are moving through the ether or across it," by an amount just sufficient to cancel the expected differences in the times for the light beams to travel the paths along and perpendicular to the earth's motion.[7] A similar assumption was later made by H. A. Lorentz as part of his electron theory. The FitzGerald-Lorentz contraction (FLC) was considered *ad hoc* by physicists because it was not derived from a plausible theory. It is considered *ad hoc* by philosophers of science because it is not independently testable by any experiment other than the one it was invented to explain. Thus many physicists considered that Einstein's theory was preferable to Lorentz's because it explained the FLC by deriving it from general postulates. Some philosophers have argued that it was not *ad hoc* because Lorentz could have derived it from a hypothesis about molecular forces, even though Lorentz invoked molecular forces only retrospectively as a plausibility argument; or because, according to Adolf Grünbaum, the FLC makes a prediction that *was* independently testable (and was in fact refuted) by the Kennedy-Thorndike experiment (whose result can be explained only if one also uses the time-dilation hypothesis), although that experiment was not performed until after most physicists had already accepted the special theory of relativity.[8] However, that both Einstein's and Lorentz's theories were considered to make the same prediction about the increase of electron mass with velocity may explain why the test of that prediction had little effect on the acceptance of the special theory of relativity.

Social-Psychological Factors

During the 1970s and 1980s, several sociologists argued that science should be studied by an approach that came to be known as "social construction." Although they differed among themselves on many points, they generally accepted a principle of *symmetry*: the same causes should be invoked to explain why scientists accepted a theory now considered correct, as those invoked to explain why they accepted a theory now considered wrong. In other words, one should not simply say that a correct theory was adopted *because* it is correct, and use social explanations only to account for the behavior of those scientists who failed to adopt it.

Unfortunately some of these sociologists described their approach in ways that seemed to imply scientific research does not discover, and is not even significantly constrained by, the natural world. To say "scientific knowledge is socially constructed" seemed to debunk science.

Sociologist Stephen Cole argued that while social factors could affect the direction and rate of progress of scientific research, there was no evidence that such factors influenced the substantive content of scientific knowledge. In response to Cole's challenge to social constructionists to produce even one example in which social causation had been demonstrated by the accepted methods of sociological research, David Bloor (one of the founders of social constructionism) pointed to Andrew Warwick's study of the reception of relativity at Cambridge University. Warwick found that mathematicians and physicists understood Einstein's theory in radically different ways, depending on their own disciplinary commitments.[9]

As an example of social construction this is not very convincing, since Warwick's study is limited to the early period 1905–1911. According to Stanley

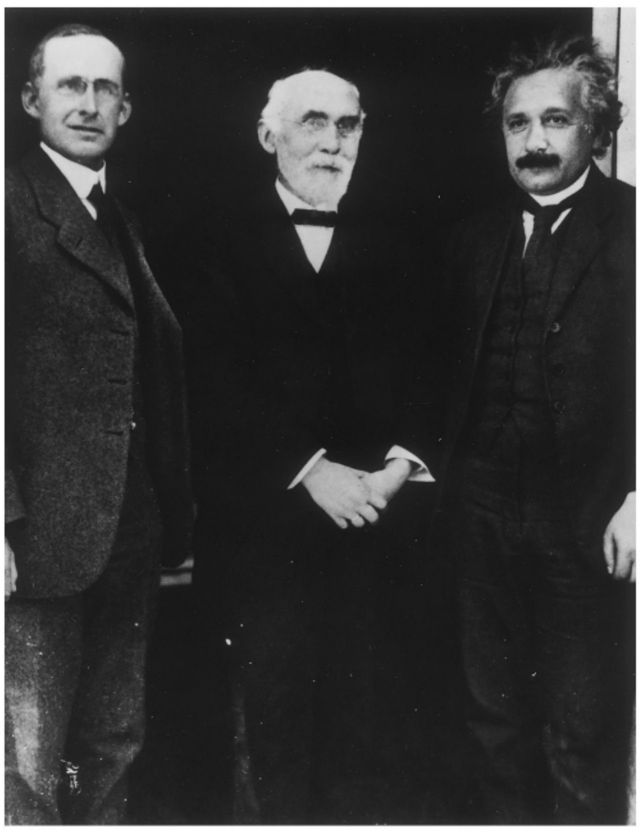

Fig. 1. Arthur Stanley Eddington, Hendrik Antoon Lorentz, and Albert Einstein. Credit: AIP Emilio Segrè Visual Archives.

Goldberg, British physicists did not abandon their strong belief in the ether until much later, so one could not say that relativity theory had really been accepted in this early period; moreover, as Warwick himself points out, before 1911 "there existed no well-defined special theory of relativity" that could be accepted or rejected.[10] At the same time, Warwick's study does show that individual scientists may have been influenced to accept relativity by reasons based more on their own disciplinary affiliation than on the objective merits of the theory itself.

Loren Graham, on the basis of his extensive studies of Russian science, has recently pointed out that social constructionists have up to now failed to make one of the most obvious tests of their thesis: to compare the development of science in radically different cultures such as the Soviet Union and Britain. Graham concludes that social factors – *i.e.*, Marxist ideology as enforced by the bureaucracy of the Soviet state – did contribute significantly to the *rise* of Lysenkoism in biology; but one cannot explain the later *fall* of Lysenkoism without giving a major role to "the obtruding of reality." Although Graham accepts the symmetry principle as a guideline for historical research, this particular example would seem to contradict its accuracy as a description of what actually happened. As for relativity, Graham states that the research of the physicist V. A. Fock is a legitimate example of social construction, and shows the "good" effects of what Western scientists would generally consider a "bad" social influence, dialectical materialism.[11]

Lewis Pyenson described the "relativity revolution" in Germany before 1914 by constructing an elaborate analogy between its participants and the protagonists in the French Revolution. He argued that "physicists in Central Europe were particularly disposed to seeking revolutionary solutions for the scientific problems that they faced" and noted that words meaning "revolution[ary]" were often used by the early defenders of relativity.[12] This would imply that physicists judged the new theory not just on its own merits but also on the basis of their own like or dislike of radical change in general.

Why would a particular physicist tend to accept or reject an idea because it is revolutionary? We might find an answer to this question in Frank Sulloway's study of openness to scientific innovation. Based on analysis of 308 scientists whose positions on relativity before 1930 are known, Sulloway concluded that age is a strong predictor of tendency to accept Einstein's theories, while social attitudes and birth order are moderately good predictors: young, more liberal scientists who were the second or later child in their family were statistically more likely to support relativity.[13]*

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* In a private communication Sulloway gives further details: of the 308 scientists in his population for the relativity revolution, 222 worked in physics, 83 in mathematics and 45 in astronomy (some contributed to more than one field). Their positions on relativity are documented in Glick's volume, *Comparative Reception* (ref. 12). The best predictors for support of special relativity (based on a subset of 135 scientists) are age (correlation -0.44), social attitudes (correlation $+0.39$ with being more liberal), birth order (correlation $+0.30$ with being later-born rather than first-born), and being Jewish (correlation $+0.13$). The results are similar for support of special and general relativity by the larger group of 308 scientists (correlations -0.47, $+0.38$, $+0.19$ and $+0.22$ respectively). Physicists and mathematicians are somewhat more likely than astronomers to support relativity but the differences are not significant. I thank Dr. Sulloway for allowing me to present these results of his analysis.

Aesthetic-Mathematical Factors

Although some historians and sociologists seem to believe that empirical (or "positivist") and social factors are the only alternatives for explaining how scientists choose theories,[14] other students of (and participants in) the scientific enterprise have stressed the importance of a third factor: the belief that a theory must be correct because it is mathematically elegant, aesthetically pleasing, and expresses a necessary truth about nature. Einstein expressed this view in a 1933 lecture:

> Nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover, by means of purely mathematical constructions, those concepts and those lawful connections between them which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed.[15]

In the historiography of science, the best known example of the use of this factor is Alexandre Koyré's interpretation of Galileo as a Platonist who arrived at his discoveries by rational deduction and adduced experiments (which he may not even have performed) to illustrate rather than to demonstrate his conclusions.[16] Koyré's claims were severely criticized by other historians, but more recently Gerald Holton has written extensively, and with careful attention to primary sources, on the role of recurring "themata" that guide scientists in the construction of theories and influence the attractiveness of those theories to other scientists. He sees Einstein's relativity theory as a "return to classical purity" rather than a discontinuous break.[17] Since Einstein began his 1905 paper with an aesthetic question – the problem of symmetry in Maxwell's theory – it would not be surprising if his followers also gave priority to such issues.[18]

Lewis Pyenson argues that physicists and mathematicians in Germany were motivated to accept relativity because it satisfied their desire to believe in a "preestablished harmony between mathematics and physics."[19] A philosopher of science, James W. McAllister, has recently reviewed the role of aesthetic factors in scientists' evaluation of theories; he argues that the frequent invocation of such factors as a reason for accepting relativity shows that this theory is *not* revolutionary.[20]

Thomas Glick, in his study of the reception of relativity in Spain, found that many mathematicians were among the early advocates of Einstein's theory. Mathematics was the strongest discipline among the exact or physical sciences; "identification with relativity enhanced its prestige."[21] Mathematicians were needed to explain the absolute differential calculus to physicists and to resolve conceptual problems in

Fig. 2. Einstein playing the violin, late 1920s. Credit: Ullstein Bilderdienst.

the general theory of relativity. Moreover, they had close connections with Italian mathematicians, who were enthusiastic about relativity.[22]

Of course the three factors – empirical, social, and aesthetic – are not independent. For example, a theory may initially be accepted by prestigious leaders of a scientific community for empirical or aesthetic reasons; other scientists may then be influenced to accept it, not so much because of their own expert evaluation but because they accept the judgment of the leaders and decide that it is in their own interests to jump on the bandwagon. In this case the social factor would amplify the empirical or aesthetic reason – in other words it would be a "factor" in the mathematical sense, multiplying rather than just adding to the other factors. Conversely, the example of Spain suggests that mathematicians may be initially attracted to a theory for social reasons but find it worthy of intense study because of its mathematical-aesthetic virtues.

Early Reception of Relativity

The first comprehensive study of the early reception of the special theory of relativity was conducted by Stanley Goldberg; valuable historical research was also published by Arthur I. Miller and Lewis Pyenson.[23] They found that relativity was widely discussed among leading physicists in Germany soon after the appearance of Einstein's 1905 papers. By 1911 it was considered so well established that Arnold Sommerfeld, who had planned to speak about relativity at the Solvay Congress that year, decided to address instead the more controversial questions about quanta and the nature of light.[24]

The most important advocate of the theory was Max Planck (1858–1947). A later-born, Planck is often credited with starting the quantum revolution* and

* But Kuhn (ref. 25) argues that the *physical* quantum hypothesis was first proposed by Einstein in 1905; Planck used the quantum only as a mathematical convenience in deriving his radiation formula.

assisted at the birth of what some would call the relativity revolution. Planck presented the theory at the physics colloquium in Berlin during the winter semester 1905–6 and published a paper on it in 1906 (the first publication on relativity other than Einstein's). While Planck did not yet believe that the truth of the theory had been demonstrated experimentally, he considered it such a promising approach that it should be further developed and carefully tested. Planck challenged the conclusions of Walter Kaufmann, who claimed that the results of his experiments on the mass increase of accelerated electrons supported Max Abraham's theory rather than the Lorentz-Einstein formula, and showed that Kaufmann's data did not rule out relativity.[26] (Eventually, experiments by A. H. Bucherer and others were believed to have confirmed the Lorentz-Einstein formula.) As a professor at Berlin (at that time one of the major centers of physics) Planck encouraged his students and colleagues to work on relativity theory. As editor of the prestigious journal *Annalen der Physik*, Planck saw to it that any paper on relativity meeting the normal standards would get published.[27]

According to Goldberg, Planck was attracted to relativity theory because of "his philosophical and ethical convictions about the ultimate laws of reality." He liked the "absolute character with which physical law was endowed" by relativity theory, such as that the laws of nature are the same for all observers. "For Planck this represented the supreme objectivity toward which science was striving."[28]

Other German physicists became interested in relativity because it promised a new approach to theoretical puzzles such as the rigidity of the electron. In this way Arnold Sommerfeld, Wilhelm Wien, Max Born, Paul Ehrenfest and others, some of whom were initially skeptical for various reasons, made relativity theory useful and even fashionable among theoretical physicists. Arthur I. Miller and Martin J. Klein concluded that Ehrenfest's analysis of the distortion of moving bodies was an important factor in special relativity's acceptance as a theory different from Lorentz's.[29] Max Laue, who learned about the theory from Planck, was quickly converted to it and eventually published the first definitive monograph on relativity in 1911; he wrote that relativity "has found an ever growing amount of attention" despite its inadequate empirical foundation and puzzling assertions about space and time.[30]

An interesting exception is Bucherer, who was apparently converted to relativity because of his experimental results despite an earlier dislike of the theory.[31]

Pyenson, as mentioned earlier, attributed the acceptance of relativity by German mathematicians and some theoretical physicists to its conformity with their convictions about the harmony of the world; they liked the theory itself even if they did not fully appreciate its physical meaning or experimental predictions. Pyenson quotes the physicists Wilhelm Wien and J. J. Laub, and the mathematicians Hermann Minkowski, Felix Klein, and Adolf Kneser to show their liking for the aesthetic qualities of relativity.[32]

According to Goldberg, the "rate at which the theory diffuses through the culture is culturally dependent": relativity was first adopted in Germany, then in the United States, Britain, and later in France.[33] It was introduced in the United States not by physicists but by two physical chemists, G. N. Lewis and R. C. Tolman. Their joint paper, published in 1909, stressed the empirical aspects of the theory, as did

Tolman's later monographs. The American mathematician R. D. Carmichael, in the first English-language monograph on relativity (1912), also emphasized its experimental basis.[34]

In Britain, relativity theory was introduced in 1907 by Ebenezer Cunningham, but he believed that the theory could be made consistent with the existence of an ether (perhaps a different ether for each frame of reference). Warwick disagrees with Goldberg on the sense in which Cunningham accepted or responded to Einstein's theory, but it seems clear that he saw it as a useful way to supplement or extend the established ether theory. Warwick argues that the response of the ''Cambridge school'' of theorists

> derived from their adherence to the formal school of [Joseph] Larmor's ETM [Electronic Theory of Matter]. They fully accepted the reality of the contraction of moving matter, and routinely applied the Lorentz transformations, in Larmor's sense, when tackling problems in the electrodynamics of moving bodies. We should not, then, be surprised that they did not identify Einstein's work as representing any kind of important break-through or advance in physics, but treated it rather as a comparatively minor philosophical gloss upon one of the important results of the ETM. … Indeed it is perhaps an indication of the disregard with which they treated Einstein's electrodynamics, that they appear not to have taken seriously any claim that the ether did not exist.[35]

José Sánchez-Ron suggests that G. A. Schott and other British physicists were interested in relativity because it offered a way to deal with problems arising from the new atomic physics, in which the magnetic interactions of electrons and their behavior at very high speeds needed to be better understood.[36] Sánchez-Ron also points to the interest of these physicists in symmetry considerations. But the most explicit statement he quotes about the reason for accepting relativity is that of an anonymous reviewer of Ludwik Silberstein's 1914 book: ''without the result of Michelson and Morley's experiment there would be no justification for the theory at all. …[It] will only be when further experimental data of a crucial kind are obtained that the theory will run much chance of becoming definitely accepted as scientific knowledge.''[37]

In France, relativity was ignored or rejected by most physicists until the 1950s, despite the efforts of its major advocate, Paul Langevin. Langevin had been working on electrodynamic theory along the lines initiated by Lorentz and Poincaré; this work ''made him receptive to the influence of relativistic ideas… .'' He was also ''sensitive to the formal perfection of the theory.''[38] His training in the mechanistic tradition taught him that the unification of various physical theories (such as Maxwell had achieved with optics and electromagnetism) was an important goal of science, and Einstein's work promised to continue this tradition.[39] His lectures at the Collège de France, attended by a small elite group of students, presented his own calculations on the mass-equivalent of light in 1905–6, and dealt with Einstein's theory as early as 1910–11. Langevin tried to persuade the Collège de France to invite Einstein to lecture in Paris, and finally succeeded in 1922; the result was a flurry of popular interest in relativity.[40] After Einstein's visit Langevin decided that relativity is supported by experiments such as that of Michelson and Morley.[41]

In Russia, the conditions for the reception of relativity theory were rather favorable because of the strong interest of physicists like P. N. Lebedev, N. A. Umov and A. A. Eichenwald in light pressure and electrodynamics, and of mathematicians like V. F. Kagan in non-Euclidean geometry (continuing the tradition started by Lobachevskii). In 1907, K. K. Baumgart supported the "Lorentz-Einstein theory" because it was compatible with the negative result of the Michelson-Morley experiment. In 1909, O. D. Khvol'son praised relativity theory, in part because it replaced the mechanical with the electrical worldview. It appears that several Russian physicists had already become enthusiastic about the electromagnetic world-view before 1905, and saw Einstein's work as a major contribution to that world-view. The high prestige of German physics also inclined them to take an interest in a theory that was being widely discussed in Germany. Paul Ehrenfest, who resided in St. Petersburg from 1907 to 1912, aroused further interest in relativity with his discussions of the problem of electron rigidity. Mathematicians were attracted to relativity because of the opportunity to investigate new geometrical structures.[42]

Do Scientists give Extra Credit for Novelty? The Case of Gravitational Light-Bending

I now turn to the period after 1916, when a new phase in the history of relativity began with the publication of Einstein's general theory and its confirmation by the calculation of the anomalous motion of Mercury's perihelion.* Then came A. S. Eddington's sensational confirmation of Einstein's prediction that the path of light

* According to philosopher Elie Zahar, it was rational for physicists to pursue Lorentz's research programme until 1915, even though Einstein's programme was "heuristically superior"; the two theories had similar empirical consequences. Einstein's programme superseded Lorentz's empirically by successfully explaining the anomalous motion of Mercury's perihelion. Zahar, following Imre Lakatos, gives great weight to the confirmation of successful novel predictions in the comparison of research programmes; yet the Mercury anomaly had been known for several decades. Zahar therefore had to introduce a new definition of "novel fact": "a fact will be considered novel with respect to a given hypothesis if it did not belong to the problem-situation which governed the construction of the hypothesis." Zahar believed that the Mercury anomaly was a novel fact by this definition. See Zahar (ref. 43). However, when John Earman and Clark Glymour examined Einstein's unpublished correspondence, they found a 1915 letter to Arnold Sommerfeld that strongly suggested Einstein did in fact use the known behavior of Mercury's perihelion in choosing his field equations for general relativity. See Earman and Glymour (ref. 43).Thus even with Zahar's new definition of novelty, or the definitions proposed by other philosophers of science (who argue that a fact may be novel if it is not actually *used* in constructing the hypothesis that predicts it), the Mercury anomaly cannot be considered a novel fact. In any case this question seems irrelevant to the reasons why scientists accepted relativity, since no one at the time except Einstein and Sommerfeld knew that the Mercury anomaly had influenced the construction of general relativity theory.

is deflected by a gravitational force.* How did these two empirical tests affect the acceptance of relativity?

The reception of general relativity theory in the 1920s offers a good opportunity to find out whether scientists are more likely to accept a theory that has made a successful novel prediction than one that has provided a successful explanation of a previously-known fact. To review the situation: Einstein's theory made three testable predictions: the advance of the perihelion of Mercury (hereafter called "Mercury's orbit"), the bending of light by a gravitational field, and the gravitational red-shift of spectral lines from a massive source. The first phenomenon had been known for several decades but not satisfactorily explained within the framework of Newtonian theory; the second was first observed in 1919 for the purpose of testing Einstein's earlier prediction. The status of the third prediction was still in doubt until after World War II.

There is a large amount of scientific and popular literature that mentions the eclipse test of Einstein's theory, and some of it allows us to judge the weight attributed to the prediction of light bending by comparison with the two other tests. So one can inquire whether light bending provided better evidence for Einstein's theory than Mercury's orbit *because* it was a novel prediction.[44]

The comparison between Mercury's orbit and light bending 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.

But there is another problem: the confusing use of words. 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. 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 the "novelty" 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.

The eclipse results created enormous publicity for relativity theory and made Einstein the most famous scientist of the 20th century. 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

* Warwick ("Electrodynamics," ref. 35, Chapter 7) shows how Eddington had already "assumed the role of unofficial champion of General Relativity theory, a role which would lead him in 1917 to defend Einstein's theory against claims by Oliver Lodge that the ETM [Electronic Theory of Matter] could offer a simpler explanation of the advance of the perihelion of Mercury and the gravitational bending of starlight than could the General theory" (p. 262). Before 1919 he was the only "Cambridge physicist fully to adopt an Einsteinian reading of the principle of relativity and to master the forbidding theoretical technology of the General theory of relativity" (p. 263).

person's life."[45] 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.

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 result in advance. But elsewhere these same physicists suggested that the prediction of light bending was not so important after all.[46]

Most of the published comments by physicists during the first two or three years after the 1919 eclipse observation indicated that light bending and Mercury's orbit counted equally strongly in favor of general relativity. If light bending was more important that was not because it had been forecast in advance, but because the data themselves were believed to be more definitive.

For astronomers, especially in the United States, light bending played a more important role, but not (except in one case) because it was a novel prediction. Jeffrey Crelinsten has studied the reception of general relativity among American astronomers in great detail. He finds that while the calculation of Mercury's perihelion "sparked great interest" the primary reason why California astronomers became advocates of relativity was that their own observations, especially those on light bending, turned out to confirm Einstein's predictions. But Crelinsten stresses that this was not a simple case of doing a crucial experiment in order to test a theory, and

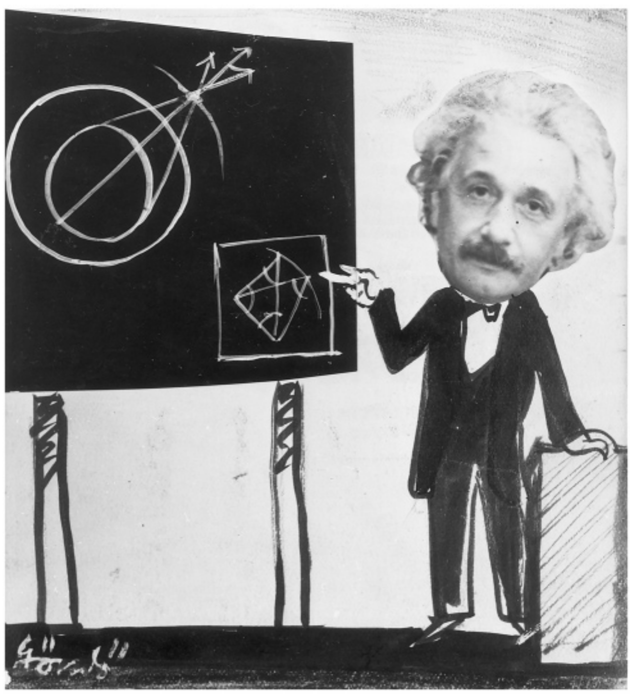

Fig. 3. Caricature of Einstein giving a lecture, 1930. Credit: Ullstein Bilderdienst.

then accepting the theory because the result was favorable. W. W. Campbell, Charles St. John, and Heber D. Curtis did not understand general relativity theory and were initially skeptical of it, but had the necessary equipment and skills to test some of its predictions. Only H. J. Trumpler, trained in Europe, was knowledgeable about the theory.[47] Yet by 1920 American astronomy was widely acknowledged to be the best in the world, especially on the observational side,[48] and Europeans looked to the United States for definitive tests of relativity. After the analysis of the 1922 eclipse results confirmed Einstein, and after St. John concluded that his observations confirmed the gravitational redshift, Campbell, St. John and Trumpler were "forced" to defend their results against the attacks of anti-relativists to protect their own reputations and the prestige of their institutions (Lick and Mt. Wilson observatories).[49] They and other California astronomers such as R. G. Aitken, also an early skeptic, were struck by the reactionary, pathological and anti-Semitic views of the most outspoken critics of Einstein, in America and Europe, and saw a need to make public statements about the strong empirical support for relativity. By the end of the 1920s they were asserting that "the theory had to be right because it had passed all the empirical tests" despite their "'slight antipathy' to its wider implications, and a continuing inability to master its mathematics."[50]

Crelinsten identifies the turning point for American astronomers as Campbell's 1923 announcement that Einstein's light-bending prediction had been confirmed by analysis of the 1922 eclipse observations in Australia. It did not cause astronomers to change their minds about the validity of the theory, but "positions tentatively taken after the 1919 eclipse results came out, were entrenched after Campbell's corroboration" – this was "the seal of approval of American technology." Yet H. D. Curtis and others continued to reject relativity despite its empirical confirmation; "they just did not like relativity in the first place."[51] Crelinsten remarks that the public debate created the misleading impression that the validity of relativity theory depended on its passing all the "crucial [empirical] tests"; this view fails to explain why some scientists adopt a theory because of successful tests "while others continue to search for alternative explanations."[52] A more complete explanation might invoke aesthetic-mathematical or social-psychological factors. Since Crelinsten defines full acceptance of a theory (his "third level" of reception) as requiring "actively working with the theory and elaborating its implications," and since most of the American astronomers who had the ability to do this in the period before 1930 turned out to be opponents of relativity (with the significant exception of Trumpler and Russell), his sample is too small to allow him to explore non-empirical reasons for *accepting* relativity. From my perspective it is desirable to investigate also the reasons why teachers and textbook authors accept a theory, since as Stanley Goldberg points out they can have a major influence on what is considered established knowledge by the next generation of scientists; moreover, textbooks are "one of the few places" where scientists discuss "the meaning of theories and their concepts of how evidence supports theories."[53]

During the next couple of decades it became clear to the experts that Mercury's orbit was stronger evidence for general relativity than light bending.[54] In part this was because the observational data were more accurate – it was very difficult to make good eclipse measurements, even with modern technology – and in part because the Mercury orbit calculation depended on a "deeper" part of the theory itself. 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 of 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.[55] 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. 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, by contrast, had not been previously 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. It was only ten years after the initial report of light bending observations that Einstein's supporters could plausibly assert, as did R. J. Trumpler, 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.[56]

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.[57] It is even asserted by a historian, Jean Eisenstadt, though with no specific citations.[58]

In the post-1923 technical literature I have examined, only one physicist explicitly states that light bending constituted better evidence because it was a novel prediction. R. C. Tolman stated that the verification of Einstein's theory by "the three so-called crucial tests" was

> 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.[59]

I have not searched the astronomy literature on this point; Crelinsten quotes one example that does give credit for novelty. D. H. Menzel remarked, in a 1929 review article, that relativity's success in explaining results such as light bending and the gravitational red shift "is a strong argument for its reality; that it predicted many of them before any study had been made is even more convincing proof of its correctness."[60]

I think Tolman and Menzel represent only a small minority; 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 Mendel Sachs: 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.[61]

So the main value of a successful novel prediction (as compared to a successful deduction of a known fact) is favorable publicity. The prediction itself, whether confirmed or not, 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.[63]

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, and some contemporary physicists who are experts on relativity, have suggested that the astronomers who observed light bending exaggerated the agreement between their

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* An example is Murray Gell-Mann's 1962 prediction of the Ω^- particle, which led physicists to perform an experiment designed to detect a particle with the specific properties deduced from his theory; see Brush (ref. 62).

results and Einstein's prediction because they were already supporters of the theory.* A historian of astronomy, Norris Hetherington, argues that the history of the third classical test of general relativity, the gravitational redshift, illustrates this influence.[65] Conversely, Crelinsten notes that H. D. Curtis, who disliked relativity, found that the results from one plate taken during the 1918 eclipse "were in excellent agreement with the prediction based on general relativity" but chose to average those results with the null results from another plate and concluded that Einstein was wrong.[66]

In effect, the preference for novel predictions 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 below) 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.

Mathematical-Aesthetic Reasons for Accepting Relativity

Some eminent scientists and mathematicians have asserted that none of the empirical tests is as convincing as the coherence and beauty of the theory itself; some go so far as to say that even if all its predictions were falsified, the theory should still be retained.[67] Conversely, some opponents of relativity assert that even if there were perfect agreement between its predictions and the results of observation, it still would not be an acceptable theory.[68]

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." 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." 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."[69]

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. According to S. Chandrasekhar, the project was undertaken, not primarily because Eddington wanted to test relativity, but rather because he was a pacifist; his friends and mentors wanted him to escape the disgrace of refusing to

* According to Everitt (ref. 54) a detailed reading of the reports on the 1919 eclipse observations "leads only to the conclusion that this was a model of how not to do an experiment …. It is impossible to avoid the impression – indeed Eddington virtually says so … that the experimenters approached their work with a determination to prove Einstein right. Only Eddington's disarming way of spinning a yarn could convince anyone that here was a good check of General Relativity. The results of later eclipse expeditions have been equally disappointing …" (pp. 533–534).

serve his country in wartime. The Astronomer Royal, Frank Dyson, was the instigator of the project and arranged for Eddington to be deferred from military service on the condition that he organize the eclipse expedition if the war ended in time.[70]

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. He referred to both Mercury's orbit and light bending as "predictions" of the theory. Reflecting on the status of relativity in 1923, Eddington wrote:

> 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.[71]

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*."[72]

P. A. M. Dirac, who constructed the first successful synthesis of special relativity and quantum mechanics, expressed similar views in his recollections. Dirac stated that he first learned about relativity theory when it was widely publicized in England after World War I by Eddington. His initial interest in the theory was captured by the experimental evidence – Michelson-Morley experiment, Mercury's orbit, and light bending. But he made it clear that this evidence was not the primary source for his subsequent belief in the theory:

> Suppose a discrepancy had appeared, well confirmed and substantiated, between the [general relativity] theory and observations. … Should one then consider the theory to be basically wrong? I would say the answer … is emphatically no. The Einstein theory of gravitation has a character of excellence of its own … a theory with the beauty and elegance of Einstein's theory has to be substantially correct. If a discrepancy should appear in some application of the theory, it must be caused by some secondary feature relating to this application which has not been adequately taken into account, and not by a failure of the general principles of the theory.[73]

Thus, for many scientists, mathematical-aesthetic factors were more important than empirical factors in persuading them to accept relativity.*

* The astronomer E. Findlay Freundlich, who was involved in an earlier (abortive) attempt to detect light bending, "supported the validity of general relativity … without having astronomical evidence to support his case" because it was "intuitively satisfying." Karl Schwarzschild also held this view; see Pyenson, *Goettingen Reception* (ref. 23), p. 184.

Social-Psychological Reasons for Accepting Relativity

Sulloway, in his analysis of the response of first-born and later-born scientists to radical innovations, found that the correlation between birth order and acceptance of relativity was much weaker after 1915 than it had been before the publication of the general theory. He suggests that "the eclipse results of 1919 caused empirical arguments to uncouple from ideological ones."[74] Although relativity still appealed to younger scientists with liberal social attitudes after 1919, and it is well known that opposition to Einstein was part of a strong anti-Semitic movement in German society, it seems to be difficult to find much evidence that physicists accepted relativity because of social or psychological factors.

The case of V. A. Fock, mentioned by Graham as an example of social construction, turns out on closer examination of Graham's own analysis to be not so relevant to our question. Fock (1898–1956) graduated from Petrograd University in 1922 and was working on relativistic quantum mechanics by 1926. He first learned and accepted relativity as a physicist, then later connected it with Marxist ideology.[75] In the introduction to his well-known monograph on gravitational theory (he rejected the label "general theory of relativity") Fock wrote:

> The philosophical side of our views on the theory of space, time and gravitation was formed under the influence of the philosophy of dialectical materialism, in particular under the influence of Lenin's "Materialism and Empiriocriticism." The teachings of dialectical materialism helped us to approach critically Einstein's point of view concerning the theory created by him and to think it out anew. It helped us also to understand correctly, and to interpret, the new results we ourselves obtained. We wish to state this here, although this book does not explicitly touch philosophical questions.[76]

It remains to be determined whether any of his students who started out as committed Marxists were persuaded by Fock's lectures and writings to accept relativity *because* it was compatible with Marxism.

A dissertation by Maxim William Mikulak gives a detailed account of the interactions between relativity theory and Marxist ideology in the USSR. Mikulak concludes that "there is no universally accepted dialectical materialist interpretation of relativity physics in the Soviet Union." There was a lively debate in the 1920s and 1930s about whether relativity is compatible with Marxism; by the 1950s an affirmative consensus had been reached. The major ideological obstacle to accepting the special theory of relativity was the idea that matter is "equivalent" to or can be transformed into energy, since this would seem to undermine materialism. But, Mikulak writes, "What was indeed striking about the special theory of relativity in the hands of Soviet physicists after 1954 was its singular interpretation that emphasized not the dialectical materialist analysis but the substantial empirical data that assured the theory's sound foundation." Thus social factors retarded but did not ultimately prevent the acceptance of the special theory by Soviet physicists.[77]

The situation was different with the general theory, especially in regard to its cosmological applications; a universe limited in space and time was seen as clearly contradictory to Marxist philosophy. Mikulak reports that "most Soviet physicists suspended judgment on [general] relativity theory and simultaneously shunned any Marxist discussions involving Einstein's physics" in the 1920s. Little work was done in relativistic cosmology in the USSR until the 1950s. In 1953 Fock and the mathematician A. D. Aleksandrov rejected Einstein's view that all inertial frames are equivalent and suggested that nature prefers a particular coordinate system; according to Mikulak, they "were groping for a dialectical materialist rendition of relativity theory without disturbing the theory's mathematical edifice."[78] According to Mikulak their view was accepted, so that at least as of 1960 one could argue that the particular version of relativity theory adopted in the USSR was, in part, socially constructed.

One could also argue that, in the West, the enthusiasm of mathematicians for relativity reflects not just an aesthetic judgment about the theory but a self-interested decision to exploit their own skills in order to become valued experts on a popular subject. According to Sánchez-Ron, the general theory of relativity gave them a "feeling of life" not easy to find in other areas of mathematical research. In this sense they, like Ebenezer Cunningham, were motivated to accept relativity by social-psychological factors. Similarly, that theoretical and mathematical physics – disciplines hospitable to relativity – were able to acquire greater institutional acceptance and resources during the early decades of the 20th century, is a significant social factor.[79] But the initial reception of a theory may be influenced by diverse factors – personal, disciplinary, geographical – while its eventual survival as established textbook knowledge probably depends on its meeting more universal criteria.

An adequate social explanation for the acceptance of relativity should tell us why the general theory went into a state of decline after the 1920s and was revived only in the decades after World War II.[80] The empirical reasons are fairly clear: there were no new convincing tests of the theory's predictions until the development of better technology and advances in atomic physics such as the Mössbauer effect made possible the Pound-Rebka verification of the gravitational redshift. In the meantime quantum mechanics offered many more research opportunities for physicists.[81] At the same time the establishment of quantum mechanics and of Dirac's relativistic wave equation made *special* relativity part of the core theories of atomic physics.*

The revival of general relativity began in astronomy, with renewed interest in cosmological models inspired partly by theoretical speculation, partly by developments in nuclear physics, and partly by the microwave technology that allowed the discovery of the cosmic black-body background radiation.[82] The mathematical-aes-

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* Warwick argues that even after 1919 most British physicists did not "accept" relativity in the sense that they could "generate the three crucial predictions from Einstein's field equations"; this is not true even today. Instead, relativity was integrated into the actual practice of physics only because after 1925 it was essential to quantum mechanics; "By the mid 1930s, a working knowledge of quantum theory had become a virtual necessity to *any* physicist working on the physics of atoms or radiation, and this entailed a practical working knowledge of Special relativity theory" (ref. 35, on p. 286).

Table 1.

Country	Author	Pro [convert]	Con	Total
USA	Goldberg	17	4	21
Britain	Sánchez-Ron	6	2	8
Germany	Pyenson	15 [4]	11	26
France	Paty	6 [1]	7	13
Paris*	Biezunski	1	4	5
Italy	Reeves	15 [1]	7	22
Spain	Glick	18	3	21
Russia/USSR	Vizgin & Gorelik	30	13	43
Poland	Sredniawa	27 [1]	2	29
Japan	Kaneko	2	1	3
Total		137 [7]	54	191

* Langevin (pro) and Poincaré (con) have been omitted to avoid duplicating Paty.

thetic factor comes in again to revive Einstein's program for a unified field theory: a single theory should encompass gravity, electromagnetism, strong and weak interactions – "a theory of everything." Social factors seem to be relevant only in the USSR, where pressures to make scientific theories conform to Marxist ideology were strong in the 1930s and 1940s but weaker after the mid-1950s.

A Statistical Summary of Comparative Reception

The accompanying tables present my analysis of the studies of the reception of relativity in nine countries, published in Glick's book.[83] Table 1 shows the number of scientists, mathematicians, and textbook authors in each country who accepted relativity ("pro"), with the number of those who first rejected and later accepted in brackets [convert], and the number who remained opposed ("con"). Table 2 gives the number of supporters for whom the authors mention any reason for supporting relativity; as a whole, this amounts to only about 20% of the supporters. The reasons given for their support (more than one in some cases) are then analyzed and grouped into categories as indicated below. ("G" means the author asserts that scientists generally supported relativity for this reason without attributing it to specific scientists.)

In some cases the authors of reception studies do not make it clear whether the scientists they discuss accepted or rejected the special theory, the general theory, or both. We can get some indication by noting the specific *empirical* results given as reasons: the first six are predictions of the special theory, the last three follow from the general theory.

Table 2. Reasons for accepting relativity (N = number of scientists for which reasons were given; G means general support for this reason).

		Empirical								
Country	N	MiMo	EtDr	ElMa	ElCh	VeLi	BoOr	LiBe	MePe	GrRe
USA	6	4	1		1	1				
Britain	0							G		
Germany	0							G		
France	3	G		1			1	G	1	
Paris	1									1
Italy	4	G,1		1				1	1	
Spain	4		2							
Russia	5		1							
Poland	3	1								
Japan	0									
total	26	2G,6	4	2	1	1	1	3G,1	2	1

		Social-Psychological				
Country	N	NSTh	MoSc	PoSt	CoIC	GePh
USA	6					
Britain	0					
Germany	0					
France	3					
Italy	4					
Spain	4	G,2	G	G	G	
Russia/USSR	5					G
Poland	3					
Japan	0					
total	26	G,2	G	G	G	G

		Aesthetic-Mathematical			
Country	N	Math	Unif	EMWV	NEGe
USA	6				
Britain	0				
Germany	0				
France	3	2	1		
Italy	4	G	3		
Spain	4	1			
Russia,USSR	5	4		1	1
Poland	3		2		
Japan	0				
	26	G,7	6	1	1

Empirical

MiMo negative result of Michelson-Morley experiment
EtDr failure of ether drift experiments
ElMa variation of electron mass with velocity
ElCh variation of electron charge with velocity
VeLi velocity of light from terrestrial and celestial sources is the same
BoOr correction of orbits and energy levels in Bohr model of atom
LiBe light-bending observations
MePe Advance of Mercury Perihelion
GrRe Gravitational redshift of spectral lines

Social-Psychological

NSTh rejection of absolute space and time is consistent with neoscholastic theology
MoSc association with "modern" science
PoSt relativism of political structures, attractive to anit-democratic ideologues
CoIC acquire prestige for comprehending the incomprehensible
GePh respect for authority of German physics

Aesthetic-Mathematical

Math mathematical aspects of the theory
Unif desire for a unified, elegant physical theory
EMWV connection with electromagnetic worldview
NEGe connection with Lobachevskii's non-Euclidean geometry

I have not included other reception studies in this tabulation, either because they have already been discussed earlier, or because they partially duplicate the information given here or do not provide detailed reasons why several scientists in one country accepted relativity.

It is difficult to draw reliable conclusions about national differences from these statistics because the studies were done by different authors using different methods, and in all cases the number of scientists for whom the authors report *reasons* for accepting relativity is small compared to the size of the scientific community. With these qualifications, we can say that according to the articles in Glick's book, scientists accepted relativity (1) because of the negative results of the Michelson-Morley and other ether-drift experiments; (2) their attraction to its mathematical aspects and its promise to unify physics; (3) the confirmation of the light-bending prediction. These authors give practically no evidence for social-psychological reasons except in the case of Spain. Vizgin and Gorelik assert that the attacks on relativity by Soviet Marxist philosophers were "not seriously perceived" by professional physicists in the USSR and were "decisively rebuffed in the sphere of philosophy and scientific publication," implying that they had no impact on the development of relativity theory.[84]

Conclusions

Why was relativity accepted? The historical studies reviewed in this paper can be put together to suggest a three-stage answer. In the first stage, a few leading scientists such as Planck and Eddington adopted the theory because it promised to satisfy their desire for a coherent, mathematically sophisticated, fundamental picture of the universe. In the second stage, their enthusiastic advocacy persuaded other scientists to work on the theory and apply it to problems that were currently of great interest: the behavior of electrons, and Bohr's atomic model. The special theory was accepted by many German physicists by 1910 and had begun to attract some interest in other countries.

In the third stage, the confirmation of Einstein's light-bending prediction attracted so much attention among the general public as well as among scientists that no one could ignore it after 1919. Physicists who had not previously accepted relativity now had to take it seriously, and when they did, they were persuaded of its validity by a combination of factors. Those who insisted on empirical evidence gave priority to the Michelson-Morley experiment, gravitational light bending, and the advance of Mercury's perihelion. For the experts who could appreciate Einstein's deductions, the Mercury orbit test was more convincing even though it was not a novel prediction but an explanation of a long-known discrepancy. Those who demanded a coherent fundamental theory were converted to relativity but still found difficulty in giving up old concepts such as the ether, absolute space and time. Mathematicians, whose commitment to the concepts of Newtonian physics was weaker, were more eager to embrace a theory that demanded (and rewarded) expertise in abstract reasoning. For astronomers, especially those in the United States who were involved in successful tests of general relativity, defending the research of their own institutions (and their own discipline) made them defenders of relativity.

Younger scientists who had more liberal social attitudes and were the second or later child in their family were statistically more likely to accept relativity, especially before 1919. There is little evidence that the *technical content* of relativity was "socially constructed," except for general relativity and cosmology in the USSR; however, the theory's acceptance was facilitated by the prestige and institutional resources of mathematics and theoretical physics.

Although the authors of reception studies rarely address this point directly, I suspect that one result of the confirmations of the *general* theory of relativity was to persuade many more physicists to use the *special* theory in their research and teaching; while they "accepted" the general theory in a vague sense, it was of little use to them. Yet the status of general relativity as the approved way to extend the relativistic approach to problems involving gravity made it the first candidate for consideration when such problems again attracted the interest of scientists.

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7 G. F. FitzGerald, "The Ether and the Earth's Atmosphere," *Science*, **13** (1889), 390. A. M. Bork, "The 'FitzGerald' Contraction," *Isis*, **57** (1966), 199–207. S. G. Brush, "A Note on the History of the FitzGerald-Lorentz Contraction," *Isis*, **58** (1967), 230–232.

8 Elie Zahar, "Why did Einstein's Programme Supersede Lorentz's?" in Colin Howson, ed., *Method and Appraisal in the Physical Sciences* (New York: Cambridge University Press, 1976), pp. 211–275 (reprinted from *British Journal for the Philosophy of Science*, 1973); "Einstein's Debt to Lorentz: A Reply to Feyerabend and Miller," *British Journal for the Philosophy of Science*, **29** (1978), 49–60; *Einstein's Revolution: A Study in Heuristic* (LaSalle, Ill.: Open Court, 1989). Adolf Grünbaum, "The Bearing of Philosophy on the History of Science," *Science*, **143** (1964), 1406–1412; *Philosophical Problems of Space and Time*, 2nd ed. (Boston: Reidel, 1973); "Remarks on Miller's review of *Philosophical Problems of Space and Time*," *Isis*, **68** (1977), 447–448. Arthur I. Miller, "On Lorentz's Methodology," *British Journal for the Philosophy of Science*, **25** (1974), 29–45; review of Grünbaum's *Philosophical Problems*… in *Isis*, **66** (1975), 590–594; reply to Grünbaum's remarks, *Isis*, **68** (1977), 449–450. Detailed accounts of the experimental tests of special relativity are given by Arthur I. Miller, *Albert Einstein's Special Theory of Relativity: Emergence (1905) and Early Interpretation (1905–1911)* (Reading, MA: Addison-Wesley, 1981); see also James A. Coleman, *Relativity for the Layman: A Simplified Account* (New York: Mentor, 1958). Tests of general relativity are reviewed in the works cited in footnote 54, below.

9 Stephen Cole, *Making Science* (Cambridge, MA: Harvard University Press, 1992). Cole, "Voodoo Sociology: Recent Developments in the Sociology of Science," in P. R. Gross, N. Levitt and M. W. Lewis, eds., *The Flight from Science and Reason* (New York: New York Academy of Sciences, 1996), pp. 274–287. David Bloor, "Remember the Strong Program?" *Science*, *Technology and Human Values*, **22** (1997), 373–385. Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905–1911," *Studies in History and Philosophy of Science*, **23** (1992), 625–656; **24** (1993), 1–25.

10 Stanley Goldberg, *Understanding Relativity: Origin and Impact of a Scientific Revolution* (Boston: Birkhäuser, 1984). Andrew Warwick, "International Relativity: The Establishment of a Theoretical Discipline," *Studies in History and Philosophy of Science*, **20** (1989), 139–149, on p. 140.

11 Loren R. Graham, *What Have We Learned About Science and Technology from the Russian Experience*? (Stanford: Stanford University Press, 1998), pp. 24, 7. See also his article "The Reception of Einstein's Ideas: Two Examples from Contrasting Cultures," in G. Holton and Y. Elkana, eds., *Albert Einstein: Historical and Cultural Perspectives* (Princeton: Princeton University Press, 1982), pp. 107–136.

12 Lewis Pyenson, "The Relativity Revolution in Germany," in Thomas F. Glick, ed., *The Comparative Reception of Relativity* (Dordrecht and Boston: Reidel, 1987), pp. 59–111, on p. 60.

13 Frank J. Sulloway, *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives* (New York: Pantheon, 1916), pp. 40, 332.

14 Steven J. Harris, "Introduction: Thinking Locally, Acting Globally," *Configurations: A Journal of Literature, Science, and Technology*, **6** (1998), 131–139. Steven Shapin, in his review of Cole's book (ref. 9), assumed the validity of this dichotomy when he argued that since scientific theories are underdetermined by empirical evidence, they must be, at least in part, socially constructed. Shapin, "Mertonian Concessions," *Science*, **259** (1993), 839–841. According to David Cassidy, "Understanding the History of Special Relativity," *Historical Studies in the Physical Sciences*, **16** (1986), 177–195, on p. 180, Goldberg's view in *Understanding Relativity* (ref. 10) is that since experimental tests could not decide between Einstein's and Lorentz's theories, "alogical and extrascientific factors must at first have guided preference for either of these two theories." But Cassidy includes aesthetics in his category of "alogical factors."

15 Albert Einstein, "The Method of Theoretical Physics," quoted in Holton, *Thematic Origins* (ref. 3), p. 252. Holton cites several other examples in which Einstein and others express similar views. Einstein's views on the role of mathematics in science are discussed by Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, Vol. 2 (Chicago: University of Chicago Press, 1986) pp. 334–347.

16 Alexandre Koyré, *Études Galiléenes* (Paris: Hermann, 1966); *Metaphysics and Measurement* (Cambridge, Mass.: Harvard University Press, 1968).

17 Holton, *Thematic Origins* (ref. 3), p. 195.

18 "That Maxwell's electrodynamics – the way in which it is usually understood – when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena is well known …"; see the translation of Einstein's 1905 paper in Miller, *Albert Einstein's Special Theory* (ref. 8), p. 392. The original version of this and other publications by Einstein, along with correspondence, notes, English translations, extensive annotations and commentary, may be found in the magnificent edition of *The Collected Papers of Albert Einstein*, edited by John Stachel *et al.* (Princeton: Princeton University Press, 1987-).

19 Lewis Pyenson, "Relativity in late Wilhelmian Germany: The Appeal to a Preestablished Harmony between Mathematics and Physics," *Archive for History of Exact Sciences*, **27** (1982), 137–155.

20 James W. McAllister, *Beauty and Revolution in Science* (Ithaca: Cornell University Press, 1996). For an opposing view see Henk W. De Regt, "Explaining the Splendour of Science," *Studies in History and Philosophy of Science*, **29** (1998), 155–165.

21 Thomas F. Glick, *Einstein in Spain: Relativity and the Recovery of Science* (Princeton, N.J.: Princeton University Press, 1988), p. 66; see also José Sánchez-Ron, "The Reception of General Relativity among British physicists and Mathematicians (1915–1920)," in J. Eisenstaedt and A. J. Kox, eds., *Studies in the History of General Relativity* (Boston: Birkhäuser, 1992), pp. 57–88.

22 Judith R. Goodstein, "The Italian Mathematicians of Relativity," *Centaurus*, **26** (1983), 241–261.

23 Stanley Goldberg, *Early Responses to Einstein's Theory of Relativity: A Case Study in National Differences, 1905–1911* (Ph. D. Dissertation, Harvard University, 1968); *Understanding Relativity* (ref. 10). Lewis Robert Pyenson, *The Goettingen Reception of Einstein's General Theory of Relativity* (Ph. D. Dissertation, Johns Hopkins University, 1974); *The Young Einstein: The Advent of Relativity* (Boston: Adam Hilger, 1985). Miller, *Einstein's Special Theory* (ref. 8). See Cassidy's review of these books, "Understanding the History" (ref. 14), which includes an extensive bibliography of recent Einstein scholarship.

24 Goldberg, *Understanding Relativity* (ref. 10), p. 184.

25 T. S. Kuhn, *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (New York: Oxford University Press, 1978).

26 Stanley Goldberg, "Max Planck's Philosophy of Nature and His Elaboration of the Special Theory of Relativity," *Historical Studies in the Physical Sciences*, **7** (1976), 125–160; Miller, *Einstein's Special Theory* (ref. 8), pp. 225–235, 334–352. Gerald Holton, *Science and Anti-Science* (Cambridge, Mass.: Harvard University Press, 1993), pp. 92–103.

27 Lewis Pyenson, "Physical Sense in Relativity: Max Planck Edits the Annalen der Physik, 1906–1918" in E. Schmutzer, ed., *Proceedings of the 9th International Conference on General Relativity and*

Gravitation (Berlin, 1983), pp. 285–302. See also Jungnickel and McCormmach, *Intellectual Mastery* (ref. 15), Vol. 2, pp. 321–323; A. Pais, '*Subtle is the Lord …*' *The Science and the Life of Albert Einstein* (New York: Oxford University Press, 1982), p. 150.

28 Goldberg, *Understanding Relativity* (ref. 10), pp. 189, 191; also Goldberg, "Planck's Philosophy" (ref. 26); Miller, *Einstein's Special Theory* (ref. 8), pp. 254, 361. Similarly, Minkowski saw in Einstein's theory the basis for an "absolute theory of the world"; see Pyenson, *Goettingen Reception* (ref. 23), p. 223.

29 Goldberg, *Understanding Relativity* (ref. 10), pp. 193–201. Miller, *Einstein's Special Theory* (ref. 8), pp. 235–257. Martin J. Klein, *Paul Ehrenfest*, Vol. 1, *The Making of a Theoretical Physicist* (Amsterdam: North-Holland Pub. Co., 1970), pp. 150–154. "Until around 1909, almost all physicists considered Einstein's special theory of relativity to be a contribution to the electron theory"; see Pyenson, *Goettingen Reception* (ref. 23), p. 146. On Born and Herglotz see *ibid*., pp. 234–246, 293–298.

30 Max Laue, *Das Relativitätsprinzip* (Braunschweig: Vieweg, 1911; second edition 1913); later editions published under the title *Die Relativitätstheorie*. Quotation from Goldberg, *Understanding Relativity* (ref. 10), p. 203.

31 Miller, *Einstein's Special Theory* (ref. 8), p. 349.

32 Pyenson, "Wilhelmian Germany" (ref. 19).

33 Goldberg, *Understanding Relativity* (ref. 10), p. 235. Cassidy complains that Goldberg's account is biased by his "commitment … to the overwhelming influence of culture" and reliance on stereotypes of national characteristics; see "Understanding the History" (ref. 14), pp. 181, 183.

34 Stanley Goldberg, "Putting New Wine in Old Bottles: The Assimilation of Relativity in America," in Thomas F. Glick, ed., *Comparative Reception* (ref. 12), pp. 1–26.

35 Andrew Warwick, *The Electrodynamics of Moving Bodies and the Principle of Relativity in British Physics, 1894–1919* (Ph. D. Dissertation, Cambridge University, 1989), p. 155. See also Warwick, "Cambridge Mathematics" (ref. 9); Stanley Goldberg, "In Defense of Ether: The British Response to Einstein's Special Theory of Relativity," *Historical Studies in the Physical Sciences*, **2** (1970), 89–125.

36 José Sánchez-Ron, "The Reception of Special Relativity in Great Britain," in Glick, *Comparative Reception* (ref. 12), pp. 27–58.

37 José Sánchez-Ron, "The Role Played by Symmetries in the Introduction of Relativity in Great Britain," in M. G. Doncel, A. Hermann, L. Michel and A. Pais, eds., *Symmetries in Physics (1600–1980)* (Barcelona: Universitat Autònoma de Barcelona, 1987), pp. 166–193, quotation on p. 170.

38 Michel Paty, "The Scientific Reception of Relativity in France," in Glick, *Comparative Reception* (ref. 12), pp. 113–167, quotation on p. 130. For a detailed account of Langevin's contributions to relativity see Camillo Cuvaj, *A History of Relativity – The Role of Henri Poincaré and Paul Langevin* (Ph. D. Dissertation, Yeshiva University, 1970); "Paul Langevin and the Theory of Relativity," *Japanese Studies in the History of Science*, **10** (1971), 113–142.

39 Michel Biezunski, "Einstein's Reception in Paris in 1922," in Glick, *Comparative Reception* (ref. 12), pp. 169–188, quotation from p. 175.

40 Michel Biezunski, *La Diffusion de la Théorie de la Relativité en France* (Thesis, University of Paris, 1981); "Popularization and Scientific Controversy: The Case of the Theory of Relativity in France," in T. Shinn and R. Whitley, eds., *Expository Science* (Boston: Reidel, 1985), pp. 183–194. Cuvaj, *History of Relativity* (ref. 38).

41 Goldberg, *Understanding Relativity* (ref. 10), p. 217.

42 V. P. Vizgin and G. E. Gorelik, "The Reception of the Theory of Relativity in Russia and the USSR," in Glick, *Comparative Reception* (ref. 12), pp. 265–326.

43 E. Zahar, "Why did Einstein's Programme Supersede Lorentz's?" in C. Howson, ed., *Method and Appraisal in the Physical Sciences* (New York: Cambridge University Press, 1976), pp. 211–275, quotations on pp. 218, 271; first published in the *British Journal for the Philosophy of Science* (1973). See also Zahar, *Einstein's Revolution*:" *A Study in Heuristic* (LaSalle, Ill.: Open Court, 1989); A. I. Miller, "Lorentz's Methodology," *British Journal for the Philosophy of Science*, **25** (1974), 29–45. J. Earman and C. Glymour, "Einstein and Hilbert: Two Months in the History of General Relativity," *Archive for History of Exact Sciences*, **19** (1978), 291–308; see also Michael R. Gardner, "Predicting Novel Facts," *British Journal for the Philosophy of Science*, **33** (1982), 1–15.

44 For details and references see S. G. Brush, "Prediction and Theory Evaluation: The Case of Light Bending," *Science*, **246** (1989), 1124–1129. (The following five paragraphs are based on this article, abstracted or excerpted with permission of the copyright owner, the American Association for the Advancement of Science.) For detailed discussion of the early attempts to test this prediction see John Earman and Clark Glymour, "Relativity and Eclipses: The British Eclipse Expeditions of 1919 and their Predecessors," *Historical Studies in the Physical Sciences*, **11** (1980), 49–85; Jeffrey Crelinsten, *The Reception of Einstein's General Theory of Relativity among American Astronomers, 1910–1930* (Ph. D. Thesis, Université de Montréal, 1981); Crelinsten, "William Wallace Campbell and the 'Einstein Problem': An observational Astronomer Confronts the Theory of Relativity," *Historical Studies in the Physical Sciences*, **14** (1983), 1–91.

45 Marshall Missner, "Why Einstein became famous in America," *Social Studies of Science*, **15** (1985), 267–291, on p. 288.

46 The scientists were J. J. Thomson, P. Langevin, M. Born, Max von Laue, and H. A. Lorentz; for details see Brush, "Light Bending" (ref. 44).

47 Crelinsten, *Reception* (ref. 44), quoted on p. xiv; Crelinsten, "Campbell" (ref. 44). The only other American astronomer whose support for relativity was based on theoretical understanding was H. N. Russell at Princeton.

48 Crelinsten, *Reception*, Chapter 1 and Appendices. Stephen G. Brush, "Looking Up: The Rise of Astronomy in America," *American Studies*, **20**, no. 2 (Fall 1979), 41–67.

49 Crelinsten, *Reception*, p. xv. In the 1920s, Lick and Mt. Wilson astronomers "reaped the rewards [praise from press, funding] of further verifications of empirical results, and adopted more and more the role of defenders of relativity"; "In the empirically-oriented U. S. astronomical community" relativity theory "was accepted in large measure *because* it passed crucial tests." Crelinsten, *Reception*, pp. 285 and 397 (emphasis in original).

50 Crelinsten, *Reception*, pp. xvi, 384–385; Crelinsten, "Campbell" (ref. 44), p. 87.

51 Crelinsten, *Reception*, pp. 264 and 269. Announcements of more complete analyses of these and other tests in 1928 "sealed the case once and for all"; *ibid*., p. 383.

52 Crelinsten, *Reception*, pp. 442 and 443.

53 Goldberg, *Understanding Relativity* (ref. 10), p. 276.

54 L. I. Schiff, "On Experimental Tests of the General Theory of Relativity," *American Journal of Physics*, **28** (1960), 340–343. Ronald Adler, Maurice Bazin and Menahim Schiffer, *Introduction to General Relativity* (New York: McGraw-Hill, 1965), pp. 194–206. C. W. F. Everitt, "Experimental Tests of General Relativity: Past, Present and Future," in Riazuddin, ed., *Physics and Contemporary Needs*, Vol. 4 (New York: Plenum, 1980), pp. 529–555. C. W. Will, *Theory and Experiment in Gravitational Physics* (New York: Cambridge University Press, 1981) and other works cited in Brush, "Light Bending" (ref. 44). Steven Weinberg, *Dreams of a Final Theory* (New York: Pantheon Books, 1992), pp. 96, 288. This and the following two paragraphs are taken from Brush, "Light Bending" (ref. 44) by permission of the American Association for the Advancement of Science.

55 W. De Sitter, *Kosmos: A Course of Lectures on the Development of our Insight into the Structure of the Universe* (Cambridge, Mass.: Harvard University Press, 1932), p. 111.

56 R. J. Trumpler, "The Relativity Deflection of Light," *Journal of the Royal Astronomical Society of Canada*, **23** (1929), 208–218, on p. 218.

57 E. L. Turner, "Gravitational Lenses," *Scientific American*, **259**, no. 1 (July 1988), 54–60, on p. 54, and other references in Brush, "Light Bending" (ref. 44); Morris R. Cohen, "Einstein's Theory of Relativity," *New Republic*, **21** (1920), 228, 341. Fritz Rohrlich, *From Paradox to Reality* (New York: Cambridge University Press, 1987), p. 18.

58 Jean Eisenstaedt, "La Relativité Générale à l'Étiage: 1925–1955," *Archive for History of Exact Sciences*, **35** (1986), 115–185. A more accurate description is given by Donald Franklin Moyer, "Revolution in Science: The 1919 Eclipse Test of General Relativity," in A. Perlmutter and L. F. Scott, eds., *On the Path of Albert Einstein* (New York: Plenum, 1979), pp. 55–101.

59 R. C. Tolman, *Relativity*, *Thermodynamics and Cosmology* (Oxford: Clarendon Press, 1934), p. 213.

60 D. H. Menzel, "Progress of Astronomy," *Publications of the Astronomical Society of the Pacific*, **41** (1929), 224–231, on p. 229.

61 M. Sachs, *Einstein versus Bohr: The Continuing Controversies in Physics* (LaSalle, Ill.: Open Court, 1988), p. 193.

62 S. G. Brush, "Prediction and Theory Evaluation: Subatomic Particles," *Rivista di Storia della Scienza*, Ser. II. **1**, n. 2 (1993), 47–152.
63 Cf. Deborah Mayo, *Error and the Growth of Experimental Knowledge* (Chicago: University of Chicago Press, 1996), p. 288.
64 Everitt, "Experimental tests" (ref. 54), pp. 533–534; see also Earman and Glymour, "Relativity and Eclipses" (ref. 44); Crelinsten, *Reception* (ref. 44), pp. 170–173; Deborah Mayo, *Error* (ref. 63), Chapter 8; Weinberg, *Dreams of a Final Theory* (ref. 54), p. 97. This and the following paragraph are taken from Brush, "Light Bending" (ref. 44) which gives additional references on this point.
65 Norriss Hetherington, *Science and Objectivity: Episodes in the History of Astronomy* (Ames: Iowa State University Press, 1988). Crelinsten remarks that views on the gravitational red shift sometimes seemed to be governed by the adage "I'll see it when I believe it"; *Reception* (ref. 44), p. 208.
66 Crelinsten, *Reception*, pp. 227–229.
67 H. Weyl, E. T. Whittaker, P. G. Bergmann, L. Infeld, R. B. Lindsay and H. Margenau, cited in Brush, "Light Bending" (ref. 44); views of Eddington and Dirac, quoted below.
68 William D. MacMillan, "The Fourth Doctrine of Science and its Limitations," in R. D. Carmichael *et al*., *A Debate on the Theory of Relativity* (Chicago: Open Court, 1927), pp. 117–127.
69 Quoted by Holton, *Thematic Origins* (ref. 3), pp. 255; A. Pais, '*Subtle is the Lord …*' (ref. 27), p. 273.
70 S. Chandrasekhar, "The Richtmyer Memorial Lecture – Some Historical Notes," *American Journal of Physics*, **37** (1969), 577–584; see also the references in Brush, "Light Bending" (ref. 44). Eddington's early work on relativity is discussed by Moyer, "Revolution in Science" (ref. 58); but see Crelinsten, *Reception* (ref. 44), pp. 100–101, 106–108 for a different interpretation.
71 A. S. Eddington, *The Mathematical Theory of Relativity* (London: Cambridge University Press, 1923), preface.
72 A. S. Eddington, *New Pathways in Science* (London: Cambridge University Press, 1934), p. 211.
73 P. A. M. Dirac, "The early Years of Relativity," in Holton and Elkana, eds., *Albert Einstein* (ref. 11), pp. 79–80; "The Excellence of Einstein's Theory of Gravitation," *Impact of Science on Society*, **29** (1979), 11–14, on p. 13.
74 Sulloway, *Born to Rebel* (ref. 13), pp. 40, 345.
75 Loren Graham, *Science and Philosophy in the Soviet Union* (New York: Knopf, 1972), Chapter IV; V. J. Frenkel, "Fok, Vladimir Aleksandrovich," in F. L. Holmes, ed., *Dictionary of Scientific Biography* (New York: Scribner, 1970-), vol. 17, pp. 303–305; Alexei Kojevnikov, private communication.
76 V. A. Fock, *The Theory of Space Time and Gravitation*, translated by N. Kemmer (New York: Pergamon Press, 1959), p. xviii. The same statement appears in the second edition (1964), p. 8.
77 M. W. Mikulak, *Relativity Theory and Soviet Communist Philosophy (1922–1960)* (Ph. D. Dissertation, Columbia University, 1965), quotations on pp. ii, 230.
78 *Ibid*., pp. 238, 240–241. See also Mikulak, "Soviet Cosmology and Communist Ideology," *Scientific Monthly*, **81** (1955), 167–172; "Soviet Philosophic-Cosmological Thought," *Philosophy of Science*, **25** (1958), 35–50. For a different view see V. P. Vizgin and G. E. Gorelik, "The Reception of the Theory of Relativity in Russia and the USSR," in Glick, ed., *Comparative Reception* (ref. 12), pp. 265–326.
79 Sánchez-Ron, "Reception of General Relativity" (ref. 21), p. 73.
80 Jean Eisenstaedt, "The Low Water Mark of General Relativity 1925–1955," in D. Howard and J. Stachel, eds., *Einstein and the History of General Relativity* (Boston: Birkhäuser, 1989), pp. 277–292; see also "Relativité Générale" (ref. 58). Lewis Pyenson, "La Réception de la Relativité generaliseé: Disciplinarité et Institutionalisation en Physique," *Revue d'Histoire des Sciences et de leurs Applications*, **28** (1975), 61–73. Jungnickel and McCormmach, *Intellectual Mastery* (ref. 15). Warwick, "Cambridge Mathematics" (ref. 9). Russell McCormmach, "editor's Foreword," *Historical Studies in the Physical Sciences*, **2** (1970), ix–xx, on xviii. Crelinsten objects that this view is "physics-oriented" and ignores the continuing interest of astronomers in general relativity; see *Reception* (ref. 44), p. 446.
81 In British journals during the 1920s the number of papers on general relativity "was almost insignificant compared with the number of papers on quantum physics"; see Sánchez-Ron, "Reception of General Relativity" (ref. 21), p. 69.

82 J. D. North, *The Measure of the Universe: A History of Modern Cosmology* (New York: Oxford University Press, 1965; reprinted with a new preface, New York: Dover, 1990). M. P. Ryan, Jr. and L. C. Shipley, "Resource Letter RC-1: Cosmology," *American Journal of Physics*, **44** (1976), 223–230.

83 Glick, *Comparative Reception* (ref. 12).

84 Vizgin and Gorelik, "Reception of the theory of Relativity in Russia" (ref. 78), on p. 294.

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Ein Limerick

The following limerick, dating from before World War II, made the rounds:

In a notable family called Stein,
There were Gertrude, and Ep, and then Ein,
Gert's writing was hazy
Ep's statues were crazy
And nobody understood Ein.

Quoted in Alan J. Friedman and Carol C. Donley, *Einstein as Myth and Muse* (Cambridge: Cambridge University Press, 1985), p. 3.