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EINSTEIN'S ATTITUDE TOWARDS EXPERIMENTS: TESTING RELATIVITY THEORY 1907–1927

1. Introduction

EINSTEIN is well-known to virtually everybody today, and of the theoretical physicists of our century, only Niels Bohr is comparable to him in originality and impact. So it is not surprising that there has been created a popular image of Einstein concerning his attitude towards experiments which portrays him as a highbrow theorist who all-knowingly anticipated essential experimental outcomes. Contrary to this widespread Einstein legend, it is demonstrated that in many cases he was extremely curious about certain experimental results and that he could hardly wait for the moment when tests which he had suggested were actually done by skilled observers. I will show that this was the case whenever these empirically testable effects were closely linked to his newly proposed fundamental principles which still lacked empirical support, focussing on the examples of gravitational redshift (linked to the equivalence principle between gravitational and acceleration fields), of light deflection (linked to mass–energy equivalency and the curvature of space–time), and of interferometric experiments (linked to the two axioms of the special theory of relativity). I will not cover experiments related to statistical mechanics, quantum theory or quantum mechanics, but the following remarks could also fit into this context (see Table 1 below). It is shown here that Einstein eagerly motivated specialists to search for these effects, actively helped to provide them with adequate institutional backing if needed, and kept in close contact with them as long as their empirical results were uncertain. Furthermore, *Einstein regarded these experiments as crucial,* in the sense that they set out to test the predictions derived from certain fundamental principles which marked the cornerstone of his theory of relativity and gravitation. In contrast to other theoretical physicists and philosophers of science of his day, who were willing to carefully modify this theory in the light of apparently contrary evidence (such as St. John's negative redshift result of 1917, Freundlich's anomalous light deflection measurements of 1929, or Miller's allegedly positive evidence

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for ether-drift effects of 1925), Einstein vigorously insisted on the validity of his theories in their original form. Some Einstein articles and interviews in the daily press of the mid-'20s which I recently rediscovered will illustrate that *he was not willing to modify his theories but rather was prepared to give them up completely in the case of irrefutable contrary empirical evidence*. Why did he make these risky, but as we all know, well-placed bets on the unaltered theory instead of searching for small modifications, as many of his contemporary colleagues did whenever experiments appeared to come in conflict with his predictions? As a plausible reason it is proposed here that Einstein relied on a holistic model of physical theories, in which due to the many logical interdependencies of assumptions on all levels of generality, from top axioms down to observational sentences, it is *nearly impossible to modify given theories without destroying their overall consistency*. Although I am aware of a certain global shift of Einstein's epistemological, if not ontological, position, indicated by the contrast between Einstein the late rationalist and Einstein the young empiricist, as described by Holton and others,[1] I nevertheless would like to propose that the attitude towards experiments sketched here is characteristic of Einstein dating from no later than 1907 until at least 1927.

2. The Hunt for Experiments and Experimenters: Epistemological and Sociological Implications

A. Epistemological

One of the most striking characteristics of Einstein is that even in those papers where he worked out the profoundest theoretical principles and theories, such as in the 1905 paper about the special theory of relativity, he did not finish without at least glancing around for possible verifications of their empirical consequences. His 1905 paper on the electrodynamics of moving bodies, for example, ended with remarks about "properties of the motion of electrons within the reach of experiment", and the succeeding paper from late 1905 on inertia and energy content contains the farsighted conjecture that "for matter, whose energy content is variable to a high degree (as, e.g., the radium salts) a test of the theory is feasible".[2] His 1911 paper, in which he gives a generalized argument for the existence of a gravitational redshift of spectral

[1]See Gerald Holton, *Thematic Origins of Scientific Thought* (Cambridge, MA: Harvard University Press, 1973), pp. 219ff. For a critique of Holton see Jim Shelton, 'The Role of Observation and Simplicity in Einstein's Epistemology', *Studies in History and Philosophy of Science* **19** (1988), 103–118, esp. 105–110.

[2]See Albert Einstein, 'Zur Elektrodynamik bewegter Körper', *Annalen der Physik* (4) **17** (1905), 891–921, reprinted in John Stachel *et al.* (eds), *The Collected Papers of Albert Einstein*, Vol. 2 (Princeton: Princeton University Press, 1989), p. 305f., p. 314f.; cf. ibid., p. 465 (our translations).

lines, has a footnote in which he refers to some hints of such a redshift in the spectroscopic work of Charles Fabry and Henri Buisson.[3] The 1911 Prague theory of relativity and gravitation is closely connected with his prediction of light deflection in the sun's gravitational field, and his final general theory of relativity of 1915/16, despite the mathematical difficulties of tensor calculus,[4] is connected right away to the motion of Mercury's perihelion in addition to redshift and light deflection, the prediction for the latter being now twice as large as in his Prague theory.

Let us look in more detail at the example of light deflection. In 1911, as soon as he had derived his first numerical prediction for the change of a star's position when its light ray just passes the Sun's surface tangentially on its way to the Earth, Einstein tried to find astronomers interested in putting this prediction to the test. At that time Einstein was still in Prague, and the local astronomer whom he asked in turn contacted observers at other sites. Only one, Erwin Finlay Freundlich (1885–1964), a young mathematically well-qualified observer at the Royal Observatory (Königliche Sternwarte) in Berlin, took this request seriously, partly because of his mathematical training under Felix Klein in Göttingen, partly because he was fed up with the boring routine work he was supposed to do.[5]

Because Freundlich was a window through which Einstein could reach the astronomical scientific community, his contact with Freundlich became very intensive, as is obvious from the many preserved letters from 1911 on. Einstein's letters to Freundlich reveal his excitement about the fact that finally at least one astronomer was actively interested in the question, as well as his

[3]See Albert Einstein, 'Über den Einfluß der Schwerkraft auf die Ausbreitung des Lichtes', *Annalen der Physik* (4) **35** (1911), 898–908, esp. p 905: "L.F. Jewell . . . and especially Ch. Fabry and H. Boisson[!] have actually found such shifts of fine spectral lines towards the red end of the spectrum in the order of magnitude calculated here, but ascribed them to the effect of pressure in the absorbing layer" (our translation). As I show in Klaus Hentschel, 'Julius und die anomale Dispersion — Facetten der Geschichte eines gescheiterten Forschungsprogrammes', *Studien aus dem Philosophischen Seminar der Universität Hamburg*, Series 3, issue no. 6 (1991), Einstein got this reference from Julius and misspelled both the names Buisson and Lewis E.(!) Jewell and only quoted a review article instead of Jewell's own paper in the *Astrophysical Journal.* For the early work on solar redshift see Klaus Hentschel, 'The Discovery of the Redshift of Solar Spectral Lines by Rowland and Jewell in Baltimore Around 1890', to appear in *Historical Studies in the Physical and Biological Sciences* **23** (1993) issue 2.

[4]See Albert Einstein, 'Erklärung der Perihelbewegung des Merkur aus der allgemeinen Relativitätstheorie', *Sitzungsberichte der Preußischen Akademie der Wissenschaften*, math.-physik. Klasse, 1915, pp. 831–839; cf., e.g., John Earman and Clark Glymour, 'Lost in the Tensors: Einstein's Struggles with Covariance Principles', *Studies in History and Philosophy of Science* **9** (1978), 175–214.

[5]See Eric Gray Forbes, 'Freundlich, Erwin Finlay', in *Dictionary of Scientific Biography*, vol. 5, pp. 181–184, and Klaus Hentschel, *Der Einstein-Turm, E. F. Freundlich und die Relativitätstheorie* (Heidelberg: Spektrum, 1992), chapter 1 for Freundlich's early biography and chapter 3 for an account of the early interaction of Freundlich and Einstein. See also footnote 53.

curiosity about the outcome of Freundlich's research on the subject.[6] Freundlich in turn tried in vain to check exposures made during earlier solar eclipses for the Einstein effect, but these were unsuitable, and two solar eclipse expeditions, one organized by C.D. Perrine in 1912 and Freundlich's own expedition in 1914, both failed, the former because of bad weather and the latter because of the outbreak of World War I.[7] But what is most important for us is that well into the 1920s Einstein took a great interest in all steps taken by Freundlich in his efforts to confirm Einstein's light deflection or redshift prediction.

B. Sociological

Throughout Einstein's Berlin years (1914–1933), when he not only had the will but also the power to do so, he was vital in setting the stage for observers such as Freundlich, who were willing to check some of his theoretical predictions. He did this in his capacity as director of the Kaiser-Wilhelm Institut für Physik (founded in 1917 but until 1937 without its own building, nevertheless an important source of money for staff and instrumentation), as well as in his capacity as committee member of several other institutions such as the Physikalisch–Technische Reichsanstalt. Freundlich, for instance, got on Einstein's initiative the very first and for a long time the only full-time contract with the KWI, after he had lost his position at the Berlin Observatory at Babelsberg. The contract was signed in early 1918 and described Freundlich's only task as "the performance of experimental and theoretical astronomical research for testing the general theory of relativity and connected questions".[8] But the most spectacular example is perhaps the funding and building of the tower telescope, soon informally called the 'Einstein Tower', at the site of the Astrophysikalisches Observatorium, Potsdam, near Berlin. Though it never achieved its original aim and was soon devoted to a much wider spectrum of tasks, its original task was to provide the instrumental and institutional means to confirm the last of the three famous predictions of Einstein's general theory of relativity and gravitation considered to be not yet resolved in 1920:

[6]Cf., e.g., Einstein to Freundlich, 1 Sept. 1911: "I would be very pleased if you would take on this interesting problem"; 8 Jan 1912: "I am extremely glad that you are taking on the question of light deflection with such enthusiasm, and I am very curious what the examination of the available photoplate material will produce" (our translation). All items of the Einstein–Freundlich correspondence up to 1914 will be included with full German text and commentary in Klein *et al.* (eds), *Collected Papers of Albert Einstein*, Vol. 5 (Princeton: Princeton University Press, 1993).

[7]Cf., for instance, Einstein's letter to Sommerfeld, 28 Nov. 1915, where he expressed his deep disappointment: "only the intrigues of miserable people prevent the execution of this last, new, important test of the theory ... [signed] Your infuriated Einstein" (German original in: Armin Hermann (ed.), *Albert Einstein/Arnold Sommerfeld Briefwechsel* (Basel: Schwabe, 1968), p. 36; quoted from the translation by Abraham Pais, *'Subtle is the Lord ... ' The Science and the Life of Albert Einstein* (Oxford: Oxford University Press), p. 304.

[8]See Hentschel, *op cit.*, note 5, chapter 6.

gravitational redshift.[9] In addition, Einstein's active support of such experimenters can also be seen in his relation to the doctoral students Albert Bachem and Leonhard Grebe in Bonn, who tried to find the reasons for earlier failures to find gravitational redshift in the sun's spectrum through photometric means.[10] Einstein's actively seeking funding for experiments testing relativity theory through his institutional connections sheds new light on his role as a science policy maker during his Berlin years.

3. Experiments as Clues

Einstein's surprising emphasis on a subtle empirical effect like light deflection and his institutional backing for experimental inquiries related to it can be explained as follows: the empirical outcome served as a pathfinder for him in a situation where several theoretical alternatives existed, of which one was the most plausible to him. He had thus chosen *one* of these options on theoretical grounds using the criteria (Themata) of simplicity, unification etc. without being able to rule out the other possibilities by reasoning alone. Here is some evidence for this thesis, dating back to 1913:

> Regarding the remaining current theories of gravitation, my view is the following. Abraham's theory, in which light is bent just as in mine, is inconsistent from the standpoint of the theory of invariants. This still leaves the relativity theories of gravitation by Mie and Nordström. The former is fantastical and in my opinion its intrinsic probability is infinitely small. The latter, though, is very reasonable and demonstrates how to manage consistently without the equivalence principle. According to Nordström, like in my theory, a redshift of solar spectral lines exists, *but there is no bending of light rays in a gravitational field.* The observations at the next solar eclipse will have to show, which of these two points of view are consistent with the facts. Nothing can be done here through theoretical means. Next year, you astronomers can do a clearly invaluable service to theoretical physics in this regard. We will get reliable information on whether it is correct to continue to generalize the relativity principle or whether we must stop at the first step [the special principle of relativity].[11]

So, despite the chronic problems of philosophers of science with the concept of 'crucial experiments',[12] the foregoing quote makes it obvious that Einstein considered the testing of light deflection a *crucial experiment* in this sense.

[9]See *ibid.*, chapters 8–9.

[10]Cf. Klaus Hentschel, 'Grebe/Bachems photometrische Analyse der Linienprofile und die Gravitations-Rotverschiebung: 1919–1922', *Annals of Science* **49** (1992), pp. 21–46.

[11]Einstein to Freundlich, mid-August 1913, Collected Papers of Albert Einstein (hereafter CPAE), Sign. 11 203; emphasis German original, quoted from Klein, *op. cit.*, note 6, Doc. 466.

[12]This Baconian concept of 'instances at the crossroads' has been severely criticized by Pierre Duhem, *La théorie physique, son objet et sa structure* (Paris: Rivière, 1906), translated into English by P. Wiener as: *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press, 1954). Cf. Section 6 of this paper.

Whether or not light deflection was detected should decide the question of whether or not to adopt the scalar theory of gravitation proposed by Nordström instead of Einstein's tensor theory, both of which could not be ruled out on purely *a priori* grounds, which was the case with Abraham's and Mie's. Experiments like those on light deflection could thus help to decide matters of choice whenever he reached an impasse not resolvable any more by theoretical reasoning alone. *His groping for theories needed empirical input once in a while, but only at specific theoretical bifurcation points.*

Einstein's letters and publications show that he was mostly concerned with *qualitative* confirmations of his predictions as opposed to precise *quantitative* confirmations, which he (correctly) considered to be beyond the instrumental and experimental possibilities of his contemporaries. With hindsight we now know that all predictions of general relativity had to await the 1960s and decisive technological innovations before they could be tested accurately on a firm *quantitative* basis. All earlier allegedly quantitative confirmations turned out to be unsatisfactory, sometimes even illusory.[13] Here is just one example of Einstein's confidence in qualitative hints of the existence of the effects predicted by his theory of general relativity and gravitation: after Freundlich's expedition to Russia failed to perform the test of light deflection Einstein had considered so crucial, and after he bitterly complained to Sommerfeld about this missed opportunity (cf. the quote in footnote 7), Einstein continued:

> But this is not so painful for me after all, because the theory appears sufficiently secure to me, especially also in consideration of the qualitative confirmation of the shift of the spectral lines.[14]

Thus, Einstein did not trust in the singular outcome of just one experiment, but rather checked the circumstantial evidence provided by a whole field of experiments all related to the same theory. *If* the theory were correct, *then* he ought to find hints of the existence of its effects at least qualitatively in this whole group of experiments. The light quantum hypothesis, for instance, tentatively introduced by Einstein in 1905 as a heuristic device, was instrumental in explaining not only Lenard's experiments, but also Stokes's law of

[13]Cf., e.g., Clifford M. Will, *Theory and Experiment in Gravitational Physics* (Cambridge: Cambridge University Press, 1981); Jean Eisenstaedt, 'The Low-Water Mark of General Relativity, 1925–1955', in Don Howard and John Stachel (eds), *Einstein and the History of General Relativity* (Basel: Birkhäuser, 1989), pp. 277–292.

[14]Einstein to Sommerfeld, 8 Nov. 1915, our translation; German original in Hermann, *op. cit.*, note 7, p. 36.

photoluminescence, photoionization and the Volta effect (inverse photoeffect).[15]

The same attitude towards experiments can also be found in Einstein's published and unpublished statements about gravitational redshift, first proposed by Einstein in 1907 and then again in his papers of 1911 and in the final theory of 1915/16. If Einstein's principle of equivalence between acceleration and force fields were to hold, then a redshift of spectral lines of the amount $\Delta\lambda/\lambda = \Delta\phi/c^2$ (where $\Delta\phi$ is the difference in gravitational potential) is an *inevitable* consequence. That this link is a necessary one and cannot be avoided was shown by Einstein himself—in fact, between 1907 and 1919, he gave several different theoretical derivations of the effect, showing not only his aim of improved generality, but also reflecting his reliance on those effects which were stable against small variations in their derivation.[16] Consistent with this claim of Einstein's tendency to survey whole fields of related experiments is Einstein's search from 1911 on (supported by experts in the field such as Willem Henri Julius, Karl Schwarzschild and Erwin F. Freundlich) for hints of the existence of such a gravitional redshift in older literature.[17] This survey did not lead to a clear answer, and the first tests for the existence or nonexistence of redshift in the solar spectrum performed by Karl Schwarzschild, John Evershed and Charles Edward St. John, yielded confusing, contradictory results.[18] So the situation by 1920 as concerns the gravitational redshift was rather devastating; some colleagues started to propose strategies of how to cope with the possible nonexistence of the effect *within* the general framework of general relativity.[19] In a letter of congratulations to Eddington, written in December 1919 for his successful solar eclipse expedition, Einstein expressed the effect of gravitational redshift in terms of an all-or-nothing affair, despite the contemporary overall negative evidence for it:

[15]See Stachel, *op. cit.*, note 2, pp. 162–166; cf., e.g., Jagdish Mehra and Helmut Rechenberg, *The Historical Development of Quantum Theory* (Berlin: Springer, 1982), vol. 1, pp. 72–83. One of Einstein's early scientific collaborators, Jakob Johann Laub, published a survey of all experiments related to the special theory of relativity: 'Über die experimentellen Grundlagen des Relativitätsprinzips', *Jahrbuch der Radioaktivität und Elektronik* 7 (1910), pp. 405–463.

[16]Cf. Albert Einstein, 'Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen', *Jahrbuch der Radioaktivität und Elektronik* **4** (1907), pp. 411–462, esp. pp. 458–462 (now reprinted in Stachel, *op. cit.*, note 2, pp. 480–484), and Einstein, *op. cit.*, note 3, § 2–3.

[17]See Hentschel, *op. cit.*, note 3, for an account of the discovery of redshifts in the solar spectrum around 1890 in Baltimore.

[18]Cf., e.g., Eric Gray Forbes, 'A History of the Solar Red Shift Problem', *Annals of Science* **17** (1961), 129–164, John Earman and Clark Glymour, 'The Gravitational Redshift as a Test of General Relativity', *Studies in History and Philosophy of Science* **11** (1980), 175–214, and Klaus Hentschel, 'The Conversion of St. John', to appear in *Science in Context* (1993), for St. John's case.

[19]See, e.g., James Hopwood Jeans *et al.*, 'Discussion on the theory of Relativity', *Proceedings of the Royal Society of London* **A97** (1920), 66–79, or the letter by Franz Selety to Einstein, dated 30 July 1923, CPAE 20 484–6; see also Klaus Hentschel, 'Die Gravitations-Rotverschiebung in der Einschätzung einiger theoretischer Physiker', unpublished manuscript, about Larmor, Weyl, Wiechert and others.

Table 1. Overview of links between types of certain experiments (1st column) and theoretical principles of Einstein's theories (3rd column). The middle column gives the date of Einstein's first publication dealing with each issue.

Experiment or Observation	Date of A.E. publication	Theoretical Principle Involved
capillarity	1901	intermolecular force law
energy distribution and radiation pressure, fluorescence, photoelectric effect, ionization of gases	1905	molecular structure of radiation (light quanta)
Brownian motion	1905	atomistic constitution of matter
1st order of v/c	1905	relativity principle
2nd order of v/c	1905	relativity principle $c = \text{constant}$
Doppler shifts in starlight	1905	$c = \text{constant}$
Fizeau's experiment	1905	addition theorem of velocities
gravitational redshift	1907	equivalence principle between force and acceleration fields
equality of inertial and gravitational mass (Eötvös)	1907	equivalence principle between force and acceleration fields
energy and momentum fluctuations	1909	dualistic model of light: undulatory and corpuscular
light deflection	1911	$E = mc^2$
light deflection	1915	$E = mc^2$ and curvature of space-time
observables in correlated systems	1935	completeness of quantum mechanics

> I am convinced that the redshift of spectral lines is an absolutely inevitable consequence of relativity theory. If it were proven that this effect does not exist in nature, the whole theory would have to be abandoned.[20]

He saw no way to avoid this effect in a theory, once it obeyed the principle of equivalence. Einstein's excitement about all viable attempts to confirm this effect is in accordance with this tension between a subjective reliance on the principle and the poor contemporary empirical knowledge.[21]

That this situation is by no means an isolated case is further illustrated by Table 1. It summarizes similar situations (including the contexts of statistical mechanics and quantum theory and quantum mechanics) in which experiments

[20]Einstein to Eddington, 15 Dec. 1919, CPAE, Sign. 9 203–2 (our translation). All quotations from unpublished Einstein letters with permission granted by the Albert Einstein Archive, The Hebrew University of Jerusalem, Israel.

[21]Cf., e.g., Einstein's letters to Besso, 6 Jan. 1920 and 26 July 1920, about Grebe/Bachem's photometric and St. John's venus results, respectively, both published in Pierre Speziali (ed.), *Albert Einstein–Michele Besso correspondance 1903–1955* (Paris: Hermann, 1972), p. 150, p. 152; for quotes from the Einstein–Bachem/Grebe correspondence, see Hentschel, *op. cit.*, note 10; for the St. John case, see Hentschel, *op. cit.*, note 18.

could serve as a clue because of their inevitable connection to profound theoretical principles.

As a last remark in this section, let me add that Einstein's comments on the status of his *theories*, as expressed from the very beginning until well into the 1920s, are consistent with his attitude towards *experiments* as described so far. Here is a typical example of Einstein's insistence on the connection of the theory with *some crucial facts* learnt from experience when he was confronted with a theoretical alternative to his own theory, proposed by the Göttingen mathematician Hermann Weyl (1885–1955) in 1918:

> Redshift will certainly be confirmed already in a few years, so that it cannot be looked upon as an advantage of Weyl's theory if it does not produce this redshift. It is altogether questionable to see in relativity theory something formal, justified quasi *a priori*. It concerns adaptation of the theory to well-determined *facts* (constancy of the velocity of light, equality of inertial and gravitational mass) rather than something preformed and logically predetermined in the human mind; quite different from Weyl's approach; I am almost tempted to call it Hegelianism.[22]

As will be shown further in Section 6, this rather frontal attack on Weyl's attitude towards physical theories is remarkable, since one decade later Einstein himself had changed his mind and advocated a position not very different from Weyl's,[23] who had relied on *mathematical* instead of *physical* heuristics in his efforts to unify electricity and gravitation.

4. Outright Rejection of Theoretical Softening

After the first evidence was found for the crucial effects which Einstein had predicted, Einstein's attitude towards further experiments changed drastically. *Before*, experiments on crucial effects assisted the theorist in making major

[22]Einstein to Zangger, 1921, CPAE, 40 011 (our translation).

[23]On Weyl, see esp. Skuli Sigurdsson, 'Hermann Weyl, Mathematics and Physics, 1900–1927' (Ph.D. diss.: Harvard University, 1991); cf. also Klaus Hentschel, *Interpretationen und Fehlinterpretationen der speziellen und allgemeinen Relativitätstheorie durch Zeitgenossen Albert Einsteins* (Basel: Birkhäuser, 1992), sections 1.5, 4.3.4.

decisions about how to proceed and which of several conflicting fundamental principles to choose. *After* this decision had been taken, the only remaining purpose of experiments concerning these crucial effects was to give the finishing touches so to speak, to widen the network of experimental knowledge; but these experiments were not of much relevance to him anymore as long as they remained consistent with each other. They were thus heuristically irrelevant. Of course, whenever anomalous results were found, as happened several times during Einstein's lifetime, problems came up again.

But, unlike what you would perhaps expect of a theorist deeply immersed in his thoughts on fundamental theories, in such a case Einstein did not immunize his theories against falsification through *ad hoc* modifications, but always insisted that he would have to give them up *if* conclusive evidence against one of their predictions were found. The following is a typical statement to this effect, made soon *after* the first confirmation of his light deflection prediction:

> The great attraction of the theory is its logical consistency. If any deduction from it should prove untenable, it must be given up. A modification of it seems impossible without the destruction of the whole.[24]

And here is what he wrote in September 1911, eight years *before* the first successful test of light deflection in gravitational fields:

> One thing can be stated with certainty: If such a deflection does not exist, then the assumptions of the theory are not correct. One must keep in mind, of course, that these assumptions, though they are suggestive, are quite daring, too.[25]

So, the requirement of 'logical consistency' was ranked consistently above that of full empirical adequacy; rather than tampering with his theories and altering them in small steps,[26] Einstein rejected any theoretical softening of his theories outright.[27]

In situations where other theoreticians tried to convince him to modify his theories for some reason and whenever possible falsifications showed up,

[24]Albert Einstein, 'Time, Space and Gravitation', *The Times*, 28 Nov. 1919, pp. 13–14; see also the summary given in *Nature* **104** (4 Dec. 1919), p. 360.

[25]Einstein to Freundlich, 1 Sept. 1911, CPAE 11 199, our translation; quoted from Klein *et al. op. cit.*, note 6, Doc. 282.

[26]This was Poincaré's heuristics; cf., e.g., Arthur Ian Miller, *Albert Einstein's Special Theory of Relativity* (Reading, MA: Addison-Wesley, 1981), pp. 40ff, Holton, *op. cit.*, note 1, pp. 185ff.

[27]Cf. Section 6 for Duhem's holism as a possible source of this attitude.

Einstein repeatedly made striking statements to the effect that modifications of his theories were out of the question.[28] Perhaps most prominent is the case of interference experiments performed in the 1920s by Dayton Clarence Miller[29] (1866–1941), certainly not a scientific nobody but a well-known expert of acoustics and president of the American Physical Society in the mid-twenties. Miller, who had already repeated the Michelson–Morley experiment together with Edward W. Morley in 1905, decided to repeat once again the famous experiment (originally designed to find the effects of an 'ether-wind' suggested by classical electrodynamics, the so-called FitzGerald–Lorentz contraction of bodies moving in the supposed electromagnetic medium, the ether), at a greater altitude where a larger ether-wind was to be expected, namely on top of Mount Wilson near Pasadena at 1734 metres above sea level. Despite the negative outcomes of all earlier runs of this type of experiment, from 1921 on Miller surprisingly found small but positive interference effects.[30]

While Einstein was in Princeton during his first trip to the U.S.A. in 1921, rumours about Miller's results reached him. Though his first reaction was scepticism ("Subtle is the Lord, but malicious He is not"),[31] he nevertheless decided to visit Miller in Cleveland shortly before his return to Europe to discuss his results with him.[32]

[28]See, e.g., Einstein to Freundlich, Aug. 1913, CPAE, 11 204–2: "If the velocity of light is dependent even to the slightest degree upon the velocity of the light source, then my entire theory of relativity, including the theory of gravitation, is false" (our translation, quoted from Klein *et al. op. cit.*, note 6, Doc. 470).

[29]For biographical information on Miller see, e.g., H.W. Mountcastle in *Science* **93** (1941), 270–272, and Harvey Fletcher in *Biographical Memoirs of the National Academy of Science* **23** (1945), 61–74.

[30]See, e.g., Dayton Clarence Miller, 'Ether-Drift Experiments at Mount Wilson Solar Observatory', *Physical Review* (2) **19** (1922), 407–408, and in *Science* **55** (1922), 496; 'Ether-Drift Experiments at Mount Wilson Observatory', *Proceedings of the National Academy of Science* [Washington] **11** (1925), 617–621; 'Significance of the Ether-Drift Experiments of 1925 at Mount Wilson', *Science* **63** (1926), 433–443; 'The Ether-Drift Experiment and the Determination of the Absolute Motion of the Earth', *Reviews of Modern Physics* **5** (1933), 203–245; as well as the detailed historical analysis of Miller's repetition of the Michelson–Morley experiments in Loyd S. Swenson, Jr, *The Ethereal Aether: A History of the Michelson–Morley–Miller Aether-Drift Experiments, 1880–1930* (Austin: University of Texas Press, 1972), chapter 10.

[31]"Raffiniert ist der Herrgott, aber boshaft ist er nicht", taken as the title for the Einstein biography of Abraham Pais, *op. cit.*, note 7. Cf. Einstein to O. Veblen, 30 April 1930 (CPAE, 17 284, our translation): "But I would like to have you consider that this remark might appear slightly frivolous to the reader who does not know the circumstances under which it was made. The idea could be expressed, for example, as follows: Nature conceals her secret due to her intrinsic sublimity [Erhabenheit ihres Wesens], but not out of cunning [List]."

[32]Cf. Pais, *op. cit.*, note 7, p. 113; Swenson, *op. cit.*, note 30, p. 195, reports that according to Miller, Einstein's visit was most pleasant, and that the great theoretician was "not at all insistent upon the theory of relativity". See also Robert S. Shankland, 'Albert Einstein — in remembrance', *Biography* (Honolulu) **2** (1979), 190–200, esp. pp. 193–194.

According to a publication of new findings by Miller in 1925, an ether-drift of about 10 km/s had definitely been established. Assuming a drag coefficient of about 0.05, Miller claimed that the solar system moves at about 200 km/s relative to the ether towards the head of the constellation Draco. This caused a major upheaval in the scientific community. In 1926 Miller received a $1000 prize from the American Association for the Advancement of Science for one of his papers on the significance of the ether-drift experiments, and news about it was spread by radio and newspaper to the wider public.[33] Many old critics of relativity theory who had never followed Einstein in his rejection of 'good old ether' happily announced the end of the period of relativistic physics,[34] and many journalists tried to get Einstein's comments on the issue.[35] Sometimes Einstein even responded, apparently with the motivation to calm down the exaggerated interest and to make clear that he did not share the excitement of the public at large:

> I don't have a high opinion of the reliability of Miller's results. Tomaschek's experiments[36] have further reduced the probability of the accuracy of Miller's views. But today is not the moment to discuss this issue. The completion of those experiments which are entirely in keeping with Miller's method will have to be awaited.[37]

But scepticism was not Einstein's only message. Even in this highly publicized case of Miller's apparent evidence for an ether-drift, Einstein made statements to the effect that, should the Miller experiments turn out to be correct, he

[33]See Miller's 'Ether Drift Experiment of 1925 at Mount Wilson', broadcast from WCAP, Washington, and printed in *Scientific Monthly* **22**, pp. 352–355. The A.A.A.S. prize was awarded for Miller (1926), *op. cit.*, note 30. The Miller bibliography in Fletcher, *op. cit.*, note 29, lists about 20 papers by Miller about his ether-drift results, appearing in all major scientific journals.

[34]Cf. Swenson, *op. cit.*, note 30, chapter 10 for a description of the discussions which took place around 1925 about Miller. A typical example for the sensational reports released to the press is Paul Kirchberger, 'Die Grundlagen der Relativitätstheorie erschüttert? Aufsehenerregende Versuche eines amerikanischen Physikers', *Berliner Tageblatt*, No. 16, 10 Jan. 1926; or Siegfried Jacoby, 'Erneuter Angriff gegen Einsteins Relativitäts-Theorie', *Jüdisches Familienblatt* (Leipzig) 7 (1926), No. 6, p. 5, who quotes Einstein, *op. cit.*, note 40, verbatim and then continues: "Now, but what do the antisemitic dailies have to say about this? The individual editorial staffs are delighted. The racialists [Völkische] are so delighted that one would think Ludendorff had set out on a new military campaign. All cries against relativity theory are in vain, even if some other news were to arrive from America" (our translation from German original). The scientists' incertitude is exemplified in Hendrik Antoon Lorentz, 'Physics in the New and Old World' (inaugural address, delivered at the American Week at Leiden, 1926), printed in his *Collected Works*, vol. 8 (The Hague, 1935), pp. 404–417, see p. 415.

[35]See, e.g., the cable of David Dietz, NEA Service inc., to Einstein, 25 Dec. 1925, CPAE, No. 17 266; the letter of Ralph E. Zuar, correspondent for *Popular Wireless and Wireless Review*, to Einstein, 18 April 1926; or the letter by Frederick Kuh, head of the Berlin office of United Press of America, to Einstein, 30 April 1926, CPAE, 17 271.

[36]Einstein refers to Rudolf Tomaschek, 'Über Versuche zur Auffindung elektrodynamischer Wirkungen der Erdbewegung in großen Höhen I', *Annalen der Physik* (4) **76** (1925), 743–756, who had repeated Trouton-Noble's experiment with suspended capacitors and got a negative result.

[37]Einstein to Zuar, undated handwritten draft of his reply to Zuar's request of 18 April, 1926, CPAE, No. 17 272-1 (crossed-out parts have not been quoted, own translation); cf. Einstein to O. Tolischus [*Berliner Tageblatt*], 11 January 1926.

Meine Theorie und Millers Versuche

Von

Professor Albert Einstein

Der Berliner Korrespondent der Hearst-Presse O. D. Tolischus kabelt seinen Blättern den folgenden Artikel in Beantwortung der Zweifel an der Relativitäts-Theorie, die durch die Experimente von Professor Dayton C. Miller in Amerika entstanden sind. Wir veröffentlichen nach Vereinbarung mit dem Amerikanischen Konzern den Artikel.

Da die Versuche von D. Miller gegenwärtig im Mittelpunkte des Interesses stehen, ist es wohl angebracht, wenn ich meine Ansicht über die Bedeutung dieser Versuche hier öffentlich mitteile.

Wenn die Ergebnisse der Millerschen Versuche sich bestätigen sollten, so wäre die Relativitätstheorie nicht aufrecht zu erhalten. Denn die Versuche würden dann beweisen, daß mit Bezug auf Koordinatensysteme von geeignetem Bewegungszustande (Erde) die Vakuum-Lichtgeschwindigkeit von der Richtung abhinge. Damit würde das Prinzip der Konstanz der Lichtgeschwindigkeit widerlegt, welches eines der beiden Grundpfeiler bildet, auf welchen die Theorie ruht. Es ist aber nach meiner Ansicht so gut wie gar keine Wahrscheinlichkeit dafür vorhanden, daß Herr Miller recht hat. Seine Ergebnisse sind unregelmäßig und deuten eher auf eine nicht entdeckte Fehlerquelle hin als auf einen systematischen Effekt. Ferner sind Millers Resultate an und für sich wenig glaubhaft, weil sie eine starke Abhängigkeit der Lichtgeschwindigkeit von der Höhe über dem Meeresniveau behaupten. Endlich hat jüngst ein deutscher Physiker (Tomaschek) ebenfalls in beträchtlicher Höhe über dem Meere einen elektrischen Versuch gemacht (Tronton-Noblesches Experiment), dessen Ergebnis insofern gegen Millers Ergebnis spricht, als es für die Abwesenheit eines „Aetherwindes“ in großer Meereshöhe spricht.

Zusammenfassend kann ich sagen: Wenn du, verehrter Leser, diese interessante wissenschaftliche Situation zum Abschließen einer Wette benützest, so wette lieber darauf, daß Millers Versuche sich als fehlerhaft erweisen, bzw. daß seine Ergebnisse nichts mit einem „Aetherwind“ zu schaffen haben. Ich wenigstens wäre gern bereit, eine solche Wette abzuschließen.

Fig. 1. Facsimile of Einstein's statement on Miller's claims, 19 January 1926, Vossische Zeitung. *Reproduced by courtesy of the Universitäts- und Stadtbibliothek Köln.*

would discard his theory. While others again started to elaborate on possible modifications of relativity theory,[38] Einstein stuck to his unmodified theory, not only privately in letters,[39] but also in public as the following facsimile of an article, recently rediscovered by me and not listed in any of the standard Einstein bibliographies,[40] demonstrates (Fig. 1). Very probably, this is the German original of the statement which Edwin E. Slosson, Director of Science Service had requested from Einstein in a letter of late June 1925, and which was referred to in truncated form by the anti-relativist Ludwik Silberstein in his brief statement for 'Science News', published in the *Science* supplement issued 31 July 1925, but apparently never published in English in full.[41]

Notice that Einstein's argument for his rejection of any theoretical modifications is in keeping with his general attitude towards crucial experiments as described above.

> If the results of the Miller experiments were to be confirmed, then relativity theory could not be maintained, since the experiments would then prove that, relative to the coordinate systems of the appropriate state of motion (the Earth), the velocity of light in a vacuum would depend upon the direction of motion. With this, the principle of the constancy of the velocity of light, which forms one of the two foundation pillars on which the theory is based, would be refuted.[42]

[38]See, e.g., Raschevsky to Einstein, 3 Aug. 1925 (CPAE, 17 262); Nathan Rosen to Einstein, 30 March 1939 (CPAE, 20 237); or Hans Reichenbach, 'Über die physikalischen Konsequenzen der relativistichen Axiomatik', *Zeitschrift für Physik* **34** (1925), 32–48, who made a distinction between 'light geometry' and 'matter geometry' ("Körper-Geometrie") and proposed to give up the validity of relativity theory for matter geometry while retaining it in light geometry as the core of relativity theory. Cf. Schlick to Einstein, 27 December 1925 (CPAE, 17 267), cited in Klaus Hentschel, *Zum Verhältnis Philosophie-Physik anhand der Korrespondenz Schlick–Einstein und ergänzender Dokumente* (Universität Hamburg: Magisterarbeit, 1984), p. 143.

[39]Cf., e.g., Einstein's correspondence with Cohn, Piccard, Rosen, Tolischus and many others, all in CPAE.

[40]Albert Einstein, 'Meine Theorie und Millers Versuche', *Vossische Zeitung*, 19 Jan. 1926. This article is listed neither in the Einstein Readex inventory compiled by Nell Boni, Monique Russ and Dan H. Laurence, *A Bibliographical Checklist and Index to the Published Writings of Albert Einstein* (Paterson, New Jersey: Pageant Books, 1960); nor in Paul Arthur Schlipp (ed.), *Albert Einstein: Philosopher-Scientist* (Evanston, Ill.: Library of Living Philosophers, 1949); Carl Seelig, *Albert Einstein. Eine dokumentarische Biographie* (Zurich, 1954), pp. 265–294; E. Weil, *Albert Einstein: A Bibliography of his Scientific Papers* (London, 1960); or Johannes Wickert, *Albert Einstein in Selbstzeugnissen und Bilddokumenten* (Reinbek: Rowohlt, 1972).

[41]See Edwin E. Slosson to Einstein, 26 June 1925, CPAE, 17 259-1; Ludwik Silberstein, 'The Relativity Theory and the Ether Drift', *Science* **n.s. 62** (1925), Suppl. p. viii: "The Einstein theory of relativity must fall or at least require radical modification, if the experiments performed at Mt. Wilson, in California, by Professor Dayton C. Miller, of the Case School of Applied Science, are correct, is the opinion of Professor Albert Einstein himself, expressed in a communication from him to Science Service. 'If Dr. Miller's results should be confirmed', he says, 'then the special relativity theory, and with [it] the general theory in its present form, falls. Experiment is the supreme judge. Only the equivalence of inertia and weight remain, which would lead to an essentially different theory'."

[42]Einstein, *op. cit.*, note 40, emphasis original. This is consistent with Ilse Rosenthal-Schneider's account of a conversation in 'Erinnerungen an Gespräche mit Einstein', typed manuscript, *c.* 1957, CPAE, 20 295, p. 2: "What would happen, if the report that the American physicist [Miller?] really had established the 'absolute stationary ether' were correct? In answer to this, he [Einstein] said: 'Then the entire theory of relativity would just be nonsense.'" See also facsimile in Fig. 1.

But this is *not all*. The very form of theory-holism which led Einstein to reject any theoretical softening of his theories also enabled him to argue against the probability of such a threatening experiment being correct. He continues:

> There is, however, in my opinion, *practically no likelihood* that Mr. Miller is right.[43]

If we inspect in some more detail the arguments which led Einstein to reject Miller's results as very improbable, we will be surprised to find many criteria which his colleagues from experimental physics would have used themselves in evaluating an experiment: checks for accidental and systematic errors and for the inner consistency of results[44] and the reliability of instruments used,[45] and above all, comparison with other recent experiments of the same sophistication in which an ether-wind such as claimed by Miller should also have shown up.

> His [Miller's] results are irregular and point rather to an undiscovered source of error than to a systematic effect. Furthermore, Miller's results are in and of themselves hardly credible, because they assume a strong dependence of the velocity of light upon the height above sea level. Finally, a German physicist (Tomaschek) recently performed an electrical experiment also at a considerable height above the sea (the Trouton–Noble experiment), the result of which speaks against Miller's results insofar as it supports the absence of an 'ether wind' at great altitudes.[46]

Other sources show that Einstein already had a concrete suspicion of what the systematic error causing the small positive results was: thermal effects causing unequal expansion of the rods.[47]

[43]*Ibid.*, emphasis original. That Einstein was by no means alone in this evaluation is shown *inter alia* by the papers of Hans Thirring, 'Kritische Bemerkungen zur Wiederholung des Michelson-Versuchs auf dem Mt. Wilson', *Zeitschrift für Physik* **35** (1926), 723–731; J. Weber, 'Der Michelson-Versuch von D. C. Miller auf dem Mt. Wilson', *Physikalische Zeitschrift* **27** (1926), 5–8; as well as by letters which Einstein received from, e.g., André Metz, 8 Jan. 1926 (CPAE, 17 269), George J. Burns, 5 Feb. 1926 (CPAE, 17 270), George Ellery Hale, 2 Dec. 1926 (CPAE, 12 073).

[44]Violated by Miller because of the "irregularity" of his results, because of an implausible non-meridional component of the direction of the alleged ether-drift (cf. Paul Ehrenfest to Einstein, 16 Sept. 1925, CPAE 17 264) and because of an unplausible angular dependency with a periodicity of 360° instead of 180°.

[45]Cf. Born's account of his visit to Mt. Wilson 1925/26 in Max Born (ed.), *Albert Einstein – Max Born Briefwechsel 1916–1955* (Munich: Nymphenburger, 1969), p. 107, p. 128: he found Miller's larger interferometer "very shaky and unreliable". But see also Karl W. Meissner's statement about Miller's experiments on 2 Oct. 1925 (CPAE, 17 265-2, our translation): "I do think, however, that Miller's results should initially be looked upon as the results of a very careful observer who should not be suspected of primitive errors in the instrumental design."

[46]Einstein, *op. cit.*, note 40. In this quote, Einstein again refers to Tomaschek, *op. cit.*, note 36, who despite his anti-relativist prejudices, had commented sceptically on Miller's results in the last section of his paper (p. 755). It is interesting to note, however, that Einstein does not refer to Tomaschek's 1923 repetition of the Michelson–Morley experiment with fixed starlight and its negative result: see Rudolf Tomaschek, 'Über den Michelsonversuch mit Fixsternlicht', *Astronomische Nachrichten* **219**, No. 5251, col. 301–306.

[47]See, e.g., his letter to Besso 25 Dec. 1925, quoted from Speziali, *op. cit.*, note 21: "I also think that Miller's results are based upon errors through changes in temperature [Temperaturfehler]. I didn't take them seriously for a moment." Cf. Einstein to Hale, 25 Dec. 1926, CPAE, 12 075, and Robert S. Shankland, 'Conversations with Albert Einstein', *American Journal of Physics* **31** (1963), 47–57, see p. 57.

Miller, however, had thought of this possibility since his first indications of positive results on Euclid Heights, Cleveland, in 1905 when Morley and he had got 1/10 of the then 'expected' drift (gauged by the average velocity of the Earth on its path around the Sun), yet Miller repeatedly rejected this possibility.[48]

So we see that Einstein treated experiments and experimental physics as a network of mutually supporting and connected results just as much as he did with theoretical physics, most notably his own theories. His reasons for rejecting anomalous results such as Miller's as utterly improbable were thus by no means idiosyncratic or irrational, but on the contrary perfectly justifiable. *If* Miller was right, *then* Tomaschek (and others who were more than eager to find a positive effect, anyway!), ought to have found some evidence of ether-drag effects as well. *Only if* all these recent experiments of the Michelson–Morley type (such as Kennedy's, Jos's, Piccard's and Stahel's) had indicated a weakness in special relativity, would Einstein have taken these experiments seriously and possibly would have abandoned his theory.[49] It was only in 1955 that a team of experts in Cleveland headed by Robert S. Shankland could finally establish temperature effects as the most plausible reason for the 'Miller-effect'.[50]

[48]See, e.g., Miller (1925), *op. cit.*, note 30, p. 619: "there was a suspicion that this might be due to a temperature effect", and ibid., p. 620: "These experiments proved that under the conditions of actual observation the periodic displacement could not possibly be produced by temperature effects"; cf. Miller (1933), *op. cit.*, note 30, p. 220, and Miller's letter to Einstein, 20 May 1926 (CPAE, 17 274), in reply to Einstein's (lost) letter to Miller in which he had stated that a difference of only 1/10°C in the temperature of air in the light-path in the arms of the interferometer would produce a displacement of the fringes of the amount observed! Miller's reply: "It is this possibility that has made necessary such a long continued series of observations; Professor Morley and I felt that this might have been the cause of our observed effect in 1905. Very elaborate precautions have been taken to eliminate such an effect of temperature. The effects which are obtained are regularly periodic in each half-turn of the interferometer. Usually the apparatus makes one turn in each twenty-five seconds, so that temperature disturbances certainly would vary in time and in azimuth, and thus would be cancelled out in a long series of observations containing several thousand determinations of the periodic function."

[49]Cf. Albert Einstein, 'Neue Experimente über den Einfluß der Erdbewegung auf die Lichtgeschwindigkeit relativ zur Erde', *Forschungen und Fortschritte* **3** (1927), 36, where he again discusses Miller's experiment in the context of other recent repetitions of the Michelson–Morley experiment, all with negative outcomes despite significant improvements in their sensitivity. Einstein here gives the following evaluation: "It was very meritorious of Prof. Miller that through his investigations he prepared the way for a careful verification of Michelson's important experiment. But through Kennedy's and Piccard's experiments, his results must count as disproven" (our translation). See also Einstein to L. Abegg (Wissenschaftliches Korrespondenzbüro Akademia), 5 Dec. 1929 (CPAE, 17 280).

[50]See Robert S. Shankland, S. W. McCuskey, F. C. Leone and G. Kuerti, 'New Analysis of the Interferometer Observations of Dayton C. Miller', *Reviews of Modern Physics* **27** (1955), 167–178; cf. R.S. Shankland to Helen Dukas, 10 June 1963, CPAE, 17 279: "Had I known of this suggestion of Professor Einstein in 1950, it is probable that we would not have made the three other attempts (statistics, method of data reduction, vibration of interferometer) to explain Miller's observations. However, this is the way science goes and I feel pleased indeed that our final conclusions coincide with the insight of Professor Einstein's genius at the very time (1926) these experiments were in progress. When we finally concluded from our studies that temperature effects were the cause, Professor Einstein agreed with us most emphatically, which gave me great satisfaction".

5. Daring Reliance on the 'All-or-Nothing Bet'

Einstein summarized his attitude towards Miller's results of 1925 in terms of a bet:

> In summing up I can say: If you, dear reader, use this interesting scientific situation for placing a bet, then you better wager that Miller's experiments will prove to be faulty, i.e., that his results have nothing to do with an 'ether wind'! I at least would be very ready to make such a bet.[51]

Einstein laid such bets several times in his life, whenever a set of circumstances occurred in which an isolated experimental result became known which threatened to seriously affect his own theory. For example, in an interview made and published in late November 1927 for a major German newspaper of the time (*Deutsche Allgemeine Zeitung*), Einstein commented upon the strange results of the astronomer Leo[pold] Courvoisier (1873–1955),[52] at that time the main observer ('Hauptobservator') at the Universitätssternwarte Berlin-Babelsberg. His results fell exactly into the category of observations just described. Courvoisier had collected evidence of systematic deviances of stellar positions compared over the period of one year. He called this effect "jährliche Refraktion" (annual refraction) and attributed it to the effect of the solar system's absolute motion in the ether of about 700 km/s towards the Charioteer constellation, approximately towards the fixed star Capella. If such an effect were present, then it would be the long-searched-for proof of an ether drift by astronomical means, and it would plainly contradict the special theory of relativity, according to which absoloute motion should not be detectable. Courvoisier's first results were already published in 1905 and 1913,[53] but his results only attracted attention in the 1920s after he had published papers in which he claimed that his 'annual refraction' would also be of relevance in the discussion of the light deflection measurements of 1919.[54] Courvoisier argued that if the Earth were to move relative to the ether, there should also be a classical Lorentz–FitzGerald contraction of the Earth in its direction of

[51]Einstein, *op. cit.*, note 40.

[52]Courvoisier was born in Riehen close to Basel, Switzerland, studied astronomy in Basel and Strassburg between 1891 and 1897, was Assistant at the Heidelberg observatory from 1898, Observer at the Königliche Sternwarte Berlin, later Berlin-Babelsberg, from 1905 (Head Observer from 1914) where he stayed until his retirement in 1938.

[53]See Leo[pold] Courvoisier, 'Über systematische Abweichungen der Sternpositionen im Sinne einer jährlichen Refraktion', *Beobachtungsergebnisse der Königlichen Sternwarte zu Berlin* **15** (1913). It is interesting to note that one of Freundlich's tasks during his time at the Sternwarte Babelsberg was to write a mathematical appendix to Courvoisier's 1913 paper (see *ibid.*, pp. 75–79).

[54]See Leo[pold] Courvoisier, 'Jährliche Refraktion und Sonnenfinsternisaufnahmen 1919', *Astronomische Nachrichten* **211** (1920), No. 5056, col. 305–312; cf. Josef Hopmann, 'Die Deutung der Ergebnisse der amerikanischen Einsteinexpedition', *Physikalische Zeitschrift* **24** (1923), 476–485, Hans Kienle, 'Kosmische Refraktion', *Physikalische Zeitschrift* **25** (1924), 1–6, August Kopff, 'Courvoisier-Effekt und Einstein-Effekt', *Physikalische Zeitschrift* **25** (1924), 95–96, etc.

Einstein und das Berliner Kilogramm

Ein Gespräch mit dem Gelehrten

Professor Einstein sagt: „Zunächst eine Sache der Experimentatoren."

Der berühmte physikalische Denker und Schöpfer der Relativitätstheorie interessiert sich, um es offen zu gestehen, wenig für das, was über sein Gebiet in die Zeitungen kommt, und gewiß ist das recht gut für den Wert von Arbeiten, die aus der Stille und dem ungestörten Nachdenken geboren werden müssen, wenn sie Bestand haben sollen. Sein Forum, das ihm maßgebend ist und sein muß, liegt anderwärts, wo ein Urteil lange Zeit braucht, bis es entsteht und in Worte gefaßt wird, aber dafür auch meist unwiderruflich ist.

Doch entzog sich Professor Einstein nicht einem Gespräch über die Bedeutung der Versuche von Courvoisier in Potsdam, von denen an dieser Stelle die Rede war. In seiner kleinen, auch zur Mittagszeit dämmrigen Arbeitsbibliothek im Dachgeschoß jenes hochstöckigen Mietshauses, wo er auf der Grenze von Schöneberg und Wilmersdorf wohnt, empfing er den Berichterstatter zwanglos, mit dem wallenden Schlafrock angetan, der ihm die Kühle des schlecht geheizten Raumes fernhalten muß; unterbrach das Studium einer anscheinend schwierigen partiellen Differentialgleichung, die ihn gerade beschäftigt hatte und sprach wohl eine Viertelstunde, stehend, an die Bücherei gelehnt, langsam und nachdenklich, aber ohne jegliche Leidenschaft. Deutlich spürt man, es ist nur die Sache, die ihn beschäftigt, nicht die Gegnerschaft eines Fachgenossen.

Natürlich sind ihm Name und Arbeiten des Potsdamer Astronomen bekannt, aber: „Wissen Sie, das Ganze geht mich eigentlich erst in zweiter Linie an, und ich kann mich kaum dazu äußern. Hier stehen ja Experimente zur Diskussion, und ich Theoretiker darf den Herrn Experimentatoren nicht ins Handwerk pfuschen. Wenn die Ergebnisse von Courvoisier stimmen, so könnte das allerdings für die Relativitätstheorie von unangenehmen Folgen sein, denn wenn irgend eine Richtung im Raume bevorzugt ist, so liegt natürlich der Gedanke an eine Bewegung als Ursache am nächsten. Aber so weit sind wir noch lange nicht."

Und er weist darauf hin, daß derartige Versuchsergebnisse erst dann wissenschaftliche Geltung besitzen, wenn sie das Spießrutenlaufen der Nachprüfung durch die Kollegen bestanden haben, ob die Methode einwandfrei ist, ob Fehlerquellen mit Erfolg ausgeschaltet wurden usw. Und solange die messenden und experimentierenden Physiker ein Resultat nicht gut geheißen haben, braucht der Denker sich in seinem Gebäude auch nicht erschüttert zu fühlen. (Zwischen den Worten spürt man, daß Einstein an diese Möglichkeit gar nicht glaube.)

„Wieviel hat er herausgefunden? Eine Tagesschwankung um 6 Milligramm? Das bewegt sich ja in der Größenordnung von einem Millionstel, ist ungeheuer viel nach heutigen physikalischen Begriffen! Die Nachprüfung kann gar nicht so schwierig sein."

Und Einstein verliert sich, den Besucher kaum noch gewahrend, in Nachdenken über mögliche Versuchsanordnungen, entwickelt einige davon im Selbstgespräch und wird dabei sehr physikalisch. So daß wir den nichts Böses ahnenden Leser mit diesen Dingen lieber verschonen wollen!

„Die Intensität der Schwerkraft und ihre Schwankungen sind ja für den Physiker wichtig und interessant genug", sagt er weiter, „um einer fortdauernden Aufmerksamkeit sicher zu sein. Es gibt sehr bedeutende Fachleute, die sich dauernd mit diesem Gegenstand befassen; es ist wenig wahrscheinlich, daß eine so auffällige Tatsache ihnen entgangen wäre. Die ausgearbeiteten Messungsmethoden sind sehr genau. Wirklich, Sie müssen sich an jemand anderen wenden, der mehr als ich zuständig ist — er nennt einen Namen —, Sie werden dann schon sehen . .

Und er lächelt vor sich hin, jungenhaft harmlos und ein wenig spitzbübisch beinahe.

„Es wird wohl nicht anders werden als bei den „Egebnissen von Miller", sagt er auf eine weitere Frage des Besuchers.

Miller, das ist jener astronomische Professor, der in Kalifornien mehrere tausend Meter hoch auf einer Sternwarte haust und mittels Messungen in solcher Höhe den „Michelsonschen Versuch" widerlegt zu haben glaubte. Auf diesem berühmten Versuch ist bekanntlich die Relativitätstheorie aufgebaut. Aber Professor Piccard in Brüssel hat die Sache in den Alpen nachgeprüft, und es war nichts daran. Es ist eben nicht so einfach heute, richtig zu experimentieren.

„Aber ich selbst", sagt er noch einmal zum Schluß, „experimentiere schon seit Jahrzehnten nicht mehr" —, und man kann hinzufügen, die Experimentatoren bauen heute kaum noch Theorien; es ist fast unmöglich, auf beiden Seiten des Gebiets die Methoden vollständig zu beherrschen, wie das etwa Heinrich Hertz noch tat.

Ein paar persönliche Bemerkungen über frühere Zusammentreffen folgen, und Einstein, anscheinend ebenso erleichtert „über das Ende des Fachgesprächs wie der, der mit Ehrfurcht zugehört hat, führt den Besucher noch zum Flügel und greift selbst zur geliebten Geige. Da kann sich der Vom-Blatt-Begleiter bei den alten Meistern der „Hohen Schule" ordentlich blamieren, und zum Schluß bedauert er nur, dem Partner nicht intensiv genug gelauscht, sein Spiel nicht genug genossen zu haben, weil er zu sehr aufpassen mußte. Geschieht ihm ganz recht, warum fragt er Einstein über die Relativitätstheorie aus? Doch tut ers gerne übermorgen wieder.

Dr. Richard Wolf.

Von Jerusalem nach Galiläa

Deutsches Bauerntum im Heiligen Lande

Von unserem Sonderberichterstatter

Nazareth, Ende Oktober

Reisenden, die Palästina nur im Frühling gesehen haben, bleibt der Eindruck eines farbenfrohen, gesegneten Landes. Wer im Herbst durch das Land reist, sieht in erster Linie Dürre und Staub. Zumal die Berge Judäas sind kahl geworden, und nur an wenigen wasserreichen Stellen leuchten ein paar grüne Pflanzen. Erst wenn man nach Samaria kommt, werden die Täler fruchtbarer, wird der Baumbestand weniger spärlich.

Mit einiger Wehmut liest man in alten Reisebeschreibungen, wie romantisch früher, als es noch keine Automobile gab, eine Reise durch Palästina sich vollzog. Da gab es nach jeder Tagesreise einen Brunnen oder eine Zisterne, teils durch historische Erinnerungen geweiht, teils durch einen so romantischen Namen wie Ain El Haramije (Räuberquelle) zu angenehmem Gruseln einladend. Dort schlugen die Reisenden ihre Zelte auf und kamen so mit den Führern der Kamelkarawanen, mit den auf Eseln über das Land reitenden Einheimischen in ständige Berührung. Der moderne Reisende, der im Auto das Land durcheilt, behält nur einen flüchtigeren, oberflächlicheren Eindruck. Und wer sich gar, wie die meisten Amerikaner, dem Büro von Thomas Cook anvertraut, kann in zwei Tagen ganz Palästina „erledigen" — und ist unbedingt davor beschützt, mit der Bevölkerung in allzu nahe Berührung zu kommen und mehr zu sehen, als ein paar durch modernen Betrieb entstellte Weihestätten im Heiligen Lande. Will man aber den aktuellen Problemen Palästinas gerecht werden, so kann man gar nicht oft genug Rast machen und eine Fühlungnahme mit der Bevölkerung suchen; nur dann wird einem bewußt, wie ungeheuer verschieden die sozialen Verhältnisse dieses Landes von denen Europas sind, wie ungemein genügsam der einheimische Fellach lebt, und welch tragische Kluft zwischen seiner Lebenshaltung und dem Lebensminimum selbst des bescheidensten Einwanderers aus Europa klafft.

Kaum eine Autostunde nördlich von Jerusalem liegt auf kahlem Hügel das ärmliche Dörfchen Betin, das alttestamentarische Bethel — „Haus Gottes": die erste Stätte des jüdischen Monotheismus. Wenn nicht schon hier der Altar stand, den Abraham, als er von Kaldäa kam, seinem Gotte baute — so jedenfalls das Gotteshaus Jakobs, der hier die Vision der Himmelsleiter gehabt hatte. Hier stand zeitweilig die Stiftshütte; später aber wurde der Ort Sitz des von Jerobeam eingeführten Bilderdienstes — „das Haus des Lasters", wie ihn die Propheten schelten. Lastet ihre Verwünschung noch heute über diesem unfreundlichen Flecken, von dem sich die historische Phantasie kaum vorzustellen vermag, daß er einst Schauplatz bedeutsamer geistesgeschichtlicher Ereignisse war?

Doch allmählich wird die Landschaft lieblicher und fruchtbar; der Oelbaum und der Feigenbaum wachsen an den sanft geneigten Talwänden. Zweieinhalb Autostunden von Jerusalem haben wir Nablus, das Zentrum von Samaria, erreicht. Südlich von der heutigen Stadt, die mehr als irgendein anderer Ort Palästinas Zerstörungen durch das letzte Erdbeben aufweist, liegen die Ruinen des alten Sichem, um dessen Freilegung sich Professor Sellin, den wir leider nicht persönlich antrafen, hoch verdient gemacht hat.

Die in wasserreichem Tal und an der Kreuzung wichtiger Verkehrswege gelegene Stadt von etwa 35 000 Einwohnern ist der Mittelpunkt des arabischen Nationalismus und hat, wie ganz Samaria, fast keine jüdischen Einwohner. Aber wohl enthält die Stadt eine ethnische und religiöse Kuriosität: die letzten Samaritaner, eine israelitische Gemeinschaft, die nicht wie die Juden in alle Welt zerstreut wurden und doch ihre alten religiösen Gebräuche gleichfalls über mehr als zwei Jahrtausende in die Gegenwart hinübergerettet hat. Die heidnischen Ansiedler, die Sargon II. nach Samaria gebracht hatte,

Fig. 2. Facsimile of Einstein's statement on Courvoisier's results in an interview with Dr Richard Wolf, published in the Deutsche Allgemeine Zeitung, *27 November 1927. Reproduced by courtesy of Universitätsbibliothek Tübingen.*

absolute motion, and this in turn should influence matter density and thus the gravitational force on its surface. Because of the rotation of the earth superimposed on its purported absolute motion through the ether, this contraction should have a 24-hour periodicity, and along with it, the weights of all bodies should also vary with the same periodicity, amounting to a difference of about 6 mg.[55] When a sensational article about Courvoisier's findings appeared in the *Deutsche Allgemeine Zeitung* of 25 November 1927 under the heading 'The Berlin kilogram varies',[56] Einstein was approached by a journalist to comment upon these results, and — to the surprise of all who knew of the aversion Einstein had to the hustle and bustle of the press ("Presserummel") since 1920 — Einstein gave the following interview, full of carefully chosen rhetoric and (sometimes Chaplinesque) self-stylisation (see Fig. 2 for a facsimile).[57]

All statements given in this interview are consistent with Einstein's general attitude towards experiments which he did not consider as 'crucial', as described in Section 5 with reference to the Miller case. We again find his acceptance of the relevance of these experiments for testing the theory *in principle*:

> If Courvoisier's results are correct, then this could certainly have unpleasant consequences for the theory of relativity, since if any one direction in space is preferred, then naturally the idea of motion being the cause comes the closest.[58]

Directly after conceding this, we find his characteristic move to evaluating the probability of a confirmation of this result as very low: "But we are nowhere near coming to that yet."[59] We then see (1) his reliance on the importance of successful replications of experimental effects before they should be considered seriously, (2) his offering physical or even technical arguments *against* the plausibility of such results, as well as (3) his reference to other experimenters who ought to find and have found similar effects in their experiments:

> [1] And he points out that such experimental results gain scientific validity only after they have survived running the gauntlet of verification by colleagues, as to whether the method is sound, whether sources of error had been successfully eliminated, etc. And as long as the measuring and experimenting physicists have not sanctioned a result, the theorist [Denker] does not need to feel his framework is shaky . . .

[55]See, for instance, Courvoisier's review article summarizing all his previous findings: 'Bestimmungsversuche der Erdbewegung relativ zum Lichtäther', *Astronomische Nachrichten* **226** (1926), No. 5416, col. 241–264; **230** (1927), No. 5519, col. 425–432; **234** (1928), No. 5599, col. 137–144.

[56]See J. H. Hoelling, 'Das Berliner Kilogramm schwankt. Das Courvoisiersche Phänomen', *Deutsche Allgemeine Zeitung*, No. 551, 25 Nov. 1927, Beiblatt, based on Courvoisier, *op. cit.*, note 55.

[57]See Richard Wolf, 'Einstein über das Berliner Kilogramm. Der Gelehrte zu den Versuchen Courvoisiers', *Deutsche Allgemeine Zeitung*, 27 Nov. 1927. This interview containing many Einstein quotes is listed in none of the Einstein bibliographies listed in footnote 40.

[58]Einstein in Wolf, *op. cit.*, note 57, col. 2 (cf. facsimile in Fig. 2).

[59]*Ibid.*

[2] 'How much did he detect? A daily fluctuation of around 6 milligrams? Then it lies in the order of magnitude of a millionth, which is extremely much for physical concepts of today! Its verification cannot be so hard at all.'

[3] 'The intensity of gravitation and its variability are, of course, important and interesting enough for the physicist ... to ensure his lasting attention. There are some very prominent experts who constantly concern themselves with this subject; it is very unlikely that such a conspicuous fact would have slipped by them. The developed methods of measurements are very accurate.'[60]

We even see him linking the case of Courvoisier's results in 1926/27 with those of Miller in 1925, which proves the parallel between these two cases was obvious to him:

And he smiled to himself a boyishly innocent and nearly a little impish smile. 'It probably won't be any different to Miller's results.'[61]

Wolf's Einstein-interview is also interesting for yet another reason: it is here that Einstein explicitly discusses the rapid specialization of physicists into theoretical versus experimental physicists. This becomes evident already in the first direct quote of the published version of the interview:

You know, this whole thing is actually of secondary concern to me, and I can hardly comment on it. Experiments are under discussion here, and being a theorist, I may not barge into the gentlemen experimenters' trade.[62]

And later he comes back to this increased specialization among physicists which became so crucial precisely to his generation of scientists:

It's just not that easy today to experiment *properly*. 'Even I myself ... have not performed experiments any more for decades now' — and it can be added, experimenters hardly ever construct theories today; it is almost impossible to completely master the methods on both sides of the field, as Heinrich Hertz, for example, could still do.[63]

'Decades' is a bit exaggerated, since Einstein had performed experiments on the gyromagnetic moment leading to rotation by magnetization together with de Haas as late as 1914/15,[64] but the overall tendency noted by Einstein in this interview is certainly correct. His later reluctance to get involved in discussions about experiments might well have had to do with the fact that he simply felt

[60]Einstein in *ibid.* (again cf. facsimile in Fig. 2).

[61]*Ibid.*

[62]*Ibid.*, col. 1–2. On the issue of professionalization of theoretical physics, see Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* (Chicago: University of Chicago Press, 1986), esp. vol. 2, pp. 120f. about precision measurement.

[63]Einstein, *ibid.*, col. 2–3.

[64]See A. Einstein and W. J. de Haas, 'Experimenteller Nachweis der Ampèreschen Molekularströme', *Verhandlungen der Deutschen Physikalischen Gesellschaft* **17** (1915), 152–170; cf. the discussion by Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987), chapter 2.

more and more incompetent to judge on the internal criteria adopted by experimental physicists in the discussion and evaluation of experimental results.

To complete my discussion of the Courvoisier case, let me mention that the preserved correspondence between Courvoisier and Einstein[65] reveals that Einstein (as with Miller in 1922) discussed Courvoisier's strange results with him, first during a meeting in January 1924, and then again in correspondence up to October 1928. As can be expected, no agreement was reached, and Courvoisier continued to publish his results about experimental findings with alleged evidence for the Lorentz contraction due to absolute motion of the Earth in the ether even after his retirement and up to his death.

6. Invariants and Contingencies: Time Dependence of Einstein's Attitude Towards Experiments?

In Section 4 we saw that the reason Einstein was not able or willing to modify his theories stems from a network characteristic of his theories: to tamper with parts of them would — according to him — destroy the "logical consistency" of the whole. Very probably, this type of statement was much inspired by Einstein's reading of Pierre Duhem (1861–1916) who advocated such a holistic view of scientific theories in his *La théorie physique, son objet et sa structure*, first published as a book in 1906.[66] The first occurrence of such a holistic argument in Einstein's writings appears only one year after the appearance of Duhem's book, that is in 1907, in the course of a debate over Kaufmann's *e/m* experiments on fast moving electrons:

> It remains to be remarked, that Abraham's and Bucherer's theories of the electron's motion lead to predictions which agree considerably better [with Kaufmann's data] than the curve derived from relativity theory. In my opinion, however, these theories have a rather slight probability because their fundamental assumptions concerning the mass of [fast moving] electrons do not suggest themselves in terms of theoretical systems which embrace a larger complex of phenomena.[67]

[65]CPAE, call no. 8 416–8 419.

[66]It is very improbable that Einstein had read the series of Duhem's articles for the *Revue de Philosophie* appearing in 1904, later expanded into Duhem's book, which was soon translated into German (1908 by Friedrich Adler) and later also into English (1954, cf. note 12). See Don Howard, 'Einstein and Duhem', *Synthese* **84** (1990), 363–384 and Klaus Hentschel, 'Einstein, Neokantianismus und Theorienholismus', *Kantstudien* **78** (1987), 459–470, for independent arguments in favor of a strong influence of Duhem on Einstein's epistemology.

[67]See Einstein, *op. cit.*, note 16, p. 461 (our translation); see, for instance, Miller, *op. cit.*, note 26, pp. 341ff. for a detailed discussion of this topic.

Table 2. Overview of instances of Einstein's dismissal of theoretical softening.

Date	*Type of Experiment*	*Experimenter*	*Einstein's rejection argument*
1907	velocity dependency of specific charge *e/m*	Kaufmann	Singular result, "not in harmony with larger theoretical system"
1925f.	Michelson–Morley type interferometer	Miller	Singular result (suspicion of temperature effects)
1926f.	Absolute motion of solar system; Lorentz contraction time dependency of gravitational constant	Courvoisier	Singular result, effect too large not to have been observed by others

Needless to say, Einstein's reliance upon his theory proved correct in this respect when more sensitive experiments were carried out later.[68] So we have reason to suppose that this holistic line of thought was present in Einstein's conception of science since at least 1907, and we find it several times until at least 1927 (in Einstein's statement about Courvoisier; cf. Table 2 for an overview).

His rejection of, e.g., Kaufmann's, Miller's and Courvoisier's empirical claims does at first appear to be a direct consequence of Duhem's critique of the concept of 'crucial experiments', because one of Duhem's claims was that scientific theories can always be stabilized in spite of apparently contrary empirical evidence. But we saw that Einstein did not follow Duhem's strategy of making minute changes or auxiliary hypotheses in the scientific theory under question but rather stuck to its validity in unaltered form. In this sense, the realist Einstein deviated from Duhem's decidedly conventionalist opinions in this matter: for Einstein, 'crucial experiments', in the sense of Section 3 of this paper, existed, while for Duhem there were no such things.

Table 2 gives an overview of some cases where Einstein rejected experimental results which conflicted with the predictions of his own theories. These are all comparable to the pattern described in more detail in Section 5 using the Miller case, because *only isolated experimental results*, and *not whole mutually consistent groups of experiments* threatened the validity of his predictions.

Scholars such as Gerald Holton have interpreted the above quoted passage concerning Kaufmann's experiments in Einstein's 1907 review paper as evidence for a stabilization in Einstein's rejection of the epistemological

[68]See Miller, *ibid.*, for a discussion of later *e/m* experiments.

priority of experiments,[69] and have claimed that in the following years, Einstein ranked criteria like simplicity and logical consistency of theories much higher than the latest news from the laboratory.[70] I would argue that this shift in Einstein's attitude towards experiments happened only in the late 1920s when he came more and more under the influence of Meyerson's 'realistic rationalism',[71] and that before this time, Einstein held the following somewhat two-faced attitude towards experiments: on the one hand, a high interest, an enthusiastic support and a bold confidence in them concerning those effects linked to fundamental principles not yet sufficiently confirmed by independent experiments but made plausible by abstract reasoning or 'Gedankenexperimente',[72] on the other hand, a tepid interest and actually a scepticism of all those experiments which were part of a larger experimental research field but which happened not to confirm other findings.

The following quote illustrates how this attitude changed after the end of the twenties. This is what Einstein wrote to Max Born in 1952 after Born had told him about Freundlich's efforts to come to grips with the small deviations which he had found between Einstein's prediction of light deflection in the Sun's gravitational field and his observations during the solar eclipse of 1929:[73]

> Unfortunately, testing the theory is too hard for me. After all, man is just a poor wretch. Freundlich doesn't affect me one little bit. If there were absolutely no light deflection, no perihelion motion and no redshift, the gravitational equations would still be convincing because they avoid the inertial system. . . . It is really quite strange that humans are usually deaf towards the strongest arguments, while they are constantly inclined to overestimate the accuracy of measurement.[74]

[69]Holton, *op. cit.*, note 1, p. 236: "explicit evidence of a hardening of Einstein against the epistemological priority of experiment, not to speak of sensory experience.".

[70]*Ibid.* In support of his views, Holton cites a letter by Einstein to Besso, March 1914: "Now I am completely satisfied and do not doubt the validity of the whole system, regardless of whether the observations of the solar eclipse succeed or not." Note, however, the occurrence of the word 'system' in his quote.

[71]Cf. A. Einstein's review of Meyerson's *Déduction Relativiste* (Paris: Payot, 1925) in: 'A propos de "La déduction relativiste" de M. Meyerson', *Revue Philosophique de la France et de L'Etranger* **105** (1928), 161–166. See also: Elie Zahar, 'Einstein, Meyerson and the Role of Mathematics in Physical Science', *British Journal for Philosophy of Science* **31** (1980), 1–43, Hentschel, *op. cit.*, note 23, sect. 4.10.

[72]For another example, see Einstein's letter to Besso, 12 Dec. 1919: "The question of whether the cosmological solution applies will maybe be able to be tested again after all by means of fixed star astronomy. I keep tormenting myself with this again and again." Quoted from Speziali, *op. cit.*, note 21, p. 148.

[73]Cf. Hentschel, *op. cit.*, note 19.

[74]Einstein to Born, 12 May 1952, Stiftung Preußischer Kulturbesitz (hereafter SPK) and CPAE; cf. Born to Einstein, 4 May 1952, SPK; quoted from Born, *op. cit.*, note 45, p. 258, our translation.

How different is the Einstein of 1952[75] from the Einstein of 1911, who had pushed Freundlich and others so hard to try everything possible in order to test his predictions of redshift and light deflection![76]

To sum up this part, I propose to distinguish at least *three different periods as concerns Einstein's attitude towards experiments*:

- an early period up to *c.* 1906 with mixed influences;
- the middle period from 1907 up to 1927, characterized by Duhem's epistemology, which as has been shown is present in many of his statements about experiments in this period;
- and a later period from roughly 1928, dominated by Meyerson's realist rationalism, and reflecting the late Einstein's deep-seated belief in the ultimate possibilities of *mere* reasoning and his far-reaching, sometimes arrogant disregard for experiments.

The distinction between the first two periods is perhaps not so clear, but certainly Duhem's influence only starts with the publication of Duhem's *La théorie physique*. It would be interesting to know whether other texts or perhaps certain types of practice such as his work at the patent office or his early practice as a theoretical physicist might have led Einstein to a similar way of evaluating experiments before 1906. Let me also add that it is perhaps no accident that the late Einstein moved away very considerably from his earlier positions concerning experiments: the theory he was working on in this period, unified field theory,[77] posed considerable mathematical difficulties, and unlike with general relativity or special relativity he was not able to 'boil them down' to the point where he could predict any effects which were 'crucial', as were redshift and light deflection for the general theory of relativity, and as were the negative outcomes of Michelson–Morley type interferometer experiments for the special theory of relativity. Also, the whole intellectual atmosphere in the thirties changed more towards a rationalistic climate.[78] This is a convincing reason why Einstein no longer showed any interest in experiments.

[75]See, for instance, his famous letter to Maurice Solovine, 7 May 1952, reproduced in facsimile in A. Einstein, *Lettres à Maurice Solovine* (Paris: Gauthier-Villars, 1956), p. 120, and commented upon by Gerald Holton, *Thematische Analyse der Wissenschaft* (Frankfurt: Suhrkamp, 1981), pp. 372ff., for the most systematic summary of his epistemological views.

[76]Cf. Einstein to Schwarzschild, 9 Jan. 1916: "As concerns Jupiter, I understood that it is a hard task to set before the astronomers. But the importance of the matter justifies in my view only *one* standpoint, and that is: it *must* work!" CPAE, 21 561-4, our translation. Original emphasis is double underlining!

[77]Cf. Pais, *op. cit.*, note 7, chapter 17 for an overview.

[78]Cf., e.g., Helge Kragh, 'Cosmo-Physics in the Thirties', *Historical Studies in the Physical Sciences* **13** (1982), 65–108, esp. p. 91 for a description of the new a-priorism in cosmology.

7. Shattering of a Related Einstein Myth and Closing Remarks

Some of my readers will already have expected me to quote the famous story told by (Anna Marie Felicia) Ilse Rosenthal-Schneider (1891–1990), at that time still Miss Schneider and an advanced student of philosophy and physics at Berlin University,[79] about the time when Einstein had just received the news about the first provisional confirmation of his prediction of the minute light deflection in the Sun's gravitational field tested by Eddington and Crommelin during the solar eclipse of 29 May 1919.[80] She reported that Einstein was allegedly not at all surprised by the confirmation of his prediction:

> Suddenly Einstein interrupted reading and handed me a cable that he took from the window-sill with the words: "This may interest you". It was Eddington's cable with the results of the famous eclipse expedition. Full of enthusiasm I exclaimed: "How wonderful, this is almost the value you calculated". Quite unperturbed, he remarked: "I knew that the theory is correct. Did you doubt it?" I answered: "No, of course not. But what would you have said if there had been no such confirmation?" He replied: "I would have had to pity our dear God. The theory is correct all the same."[81]

Before discussing this quote in more detail, let me just note the following: in some second-hand versions of this story, Einstein actually *receives* the telegram in Schneider's presence, but her own account reveals that *he actually already had it*, which opens the possibility that he may have been jokingly pretending

[79]According to a letter by Oppenheim to Einstein, dated 1919, 27 Nov. (CPAE, 44 626), (Rosenthal-)Schneider had studied with the Neo-Kantian Alois Riehl and had prepared a very good study about relativity theory for his seminar. A later curriculum vitae only mentions that she studied physics, mathematics and philosophy at the Friedrich Wilhelm University of Berlin for 5 years (1910–14), then (during World War I) worked as bacteriologist in the military reserve hospital at Berlin Tiergartenhof and later (1919–20) continued her advanced studies in physics and philosophy (see CPAE, call no. 20 268). Her obituary in the *Wentworth Courier*, 14 Feb. 1990, kindly provided to me by Kenneth E. Smith, University Archivist, University of Sydney, further mentions that she completed a masters degree on the comparison of ancient and modern Greek around 1914, and that she married Hans S.A. Rosenthal (1890–1968) at the age of 31 in 1922. In late 1938, she, her Jewish husband and her daughter Stephanie emigrated to Australia where they arrived in 1939, settling in Vaucluse close to Sydney. She was a Tutor in German at the University of Sydney from 1944 to 1952 and then part-time lecturer in history and philosophy of science from 1953–1955.

[80]Cf. Frank Watson Dyson, Arthur Stanley Eddington and C. Davidson, 'A Determination of the Deflection of Light by the Sun's Gravitational Field, from Observations Made at the Total Eclipse of May 29, 1919', *Philosophical Transactions of the Royal Society of London* **A 220** (1920), 291–334 for the published version of the paper and, for instance, the protocol of the discussions in the scientific community which took place soon after this result became known in the *Monthly Notices of the Royal Astronomical Society* **80** (1919/20), 96ff., in *Observatory* **42** (1919/20), 119–122, and in the *Proceedings of the Royal Society of London* **97** (1920), 72ff. Cf. Donald Moyer, 'Revolutions in Science: The 1919 Eclipse Test of General Relativity', in Arnold Permutter and L.F. Scott (eds), *On the Path of Albert Einstein* (New York: Plenum, 1979), pp. 55–101.

[81]Ilse Rosenthal-Schneider, *Reality and Scientific Truth: Discussions with Einstein, von Laue and Planck* (Michigan: Wayne State University Press, 1980), p. 74; for the German original see Rosenthal-Schneider, *op. cit.*, note 42, p. 2 and her *Begegnungen mit Einstein, von Laue und Planck* (Braunschweig: Vieweg, 1988), p. 60.

to be untouched by the result.[82] I cannot directly refute Rosenthal-Schneider's dramatic description,[83] but there are many reasons to believe it is highly questionable. Here are the ingredients of my circumstantial evidence.

I. The available sources all consistently point towards the contrary. Rosenthal-Schneider speaks of the telegram Einstein had received from *Eddington*. Although no such telegram has survived in the Einstein files, he might have sent one shortly before the announcement of his results in public on 6 November 1919. But *Einstein was already well-informed about the approximate result* of Eddington and his co-workers by a telegram from his colleague and friend Hendrik Antoon Lorentz, received on 27 September 1919, that is more than one month before.

On the same day that Einstein received *Lorentz's* telegram informing him about the provisional result of Eddington's data evaluation pointing to a light deflection for tangential light rays between 0.9″ and 1.8″, he wrote a letter to *Die Naturwissenschaften* in which he *very proudly announced the consistency of these results with his prediction* of 1.75″ to the wider public;[84] would he have done this if he had so much anticipated the result as Rosenthal-Schneider wants us to believe? No, then he would rather have left this job to Eddington or to some of the other participating observers or simply waited till the news spread by other means.

On the same day that he received the telegram, Einstein also wrote several letters, among them one to his mother, all *expressing the deepest satisfaction about this confirmation* and revealing that he was quite impressed, in fact, overwhelmed by this result.[85]

Would he have pressed so hard for the positive result in the years following 1911 when he first proposed such an effect? On the evidence of the Einstein–Freundlich correspondence and other sources in Section 2 of this paper, certainly not! The existing Einstein corresponding with observers and

[82]Cf. Rosenthal-Schneider (1980) *op. cit.*, note 81, p. 90: "he would tease me whenever the opportunity offered itself"; and *ibid.* p. 83 (=[1988], *op. cit.*, note 81, p. 69 or p. 76) for other evidence of Einstein often joking with her.

[83]According to John Norton, Rosenthal-Schneider repeated her account almost verbatim, when Norton visited her in Australia after World War II (personal communication).

[84]See A. Einstein, 'Prüfung der allgemeinen Relativitätstheorie', *Die Naturwissenschaften* **7** (1919), dated 9 Oct. 1919, appearing in the issue of Nov. 1919. That this is by no means a singular case is shown, e.g., by Einstein's rapid announcements of the alleged success of Grebe and Bachem to find evidence for gravitational redshift in early 1920 or of St. John in 1921; see his letter to Eddington, 2 Feb. 1920 (CPAE, 9 271) and 12 Nov. 1921 respectively, and A. Einstein, 'Excerpt of a Letter by Einstein', *Nature* **104** (1920), 565; cf. Hentschel, *op. cit.*, note 10, for Grebe and Bachem, and Hentschel, *op. cit.*, note 18, for St. John.

[85]See Einstein to Pauline Einstein, 27 Sept. 1919: "Dear Mother, joyous news today. H.A. Lorentz telegraphed that the English expeditions have actually demonstrated the deflection of light from the sun" (quoted after Pais, *op. cit.*, note 7, p. 303 where he then comments: "Einstein was excited").

experimenters capable of performing tests of his predictions[86] shows his *eagerness to get the tests done*, his great curiosity in their empirical results (cf. Section 3) and his openness to giving up his theories in the case of well-corroborated experimental evidence against his predictions (cf. Section 4). Einstein very much pressed the observers to perform tests, so that he could then settle on the theoretical options involved.[87]

II. In her 1980 book, Rosenthal-Schneider does not just tell us this anecdote; she has quite *manifest interests* for doing so. In 1921, her dissertation about the 'space–time problem as treated by Kant and Einstein' (evaluated as *magna cum laude*), once ironically referred to by Einstein in a postcard to her as "relativized Kant",[88] had been published by Springer on the recommendation of Max von Laue. In it she tried to reconcile Einstein's theory of relativity with Kant's theory of space and time. As a convinced Kantian philosopher of science, Rosenthal-Schneider later cites this story of her alleged meeting with Einstein in late 1919 as support for her claim that Einstein in fact was a Kantian too, in the sense that he could anticipate the empirical (*a posteriori*) result of Eddington and his co-workers by mere (*a priori*) reasoning.[89] But this claim is simply wrong: *Einstein never showed any inclination towards Kantianism* in that sense. His discussion remark at the Société Française de Philosophie on 6 April 1922 is famous:

> As concerns Kant's philosophy, I think that each philosopher has his own Kant . . . I for my part do not think my theory is completely consistent with the thought of Kant as I see it.[90]

Einstein's scepticism towards Kantianism in general and towards the many contemporary efforts to reconcile his theory of relativity and gravitation with Kant's general philosophical framework is furthermore revealed quite strikingly in a postcard sent to none other than to Ilse (Rosenthal-)Schneider herself on 15 September 1919, that is, roughly two months before Einstein got the news about the light deflection tests (cf. Fig. 3):

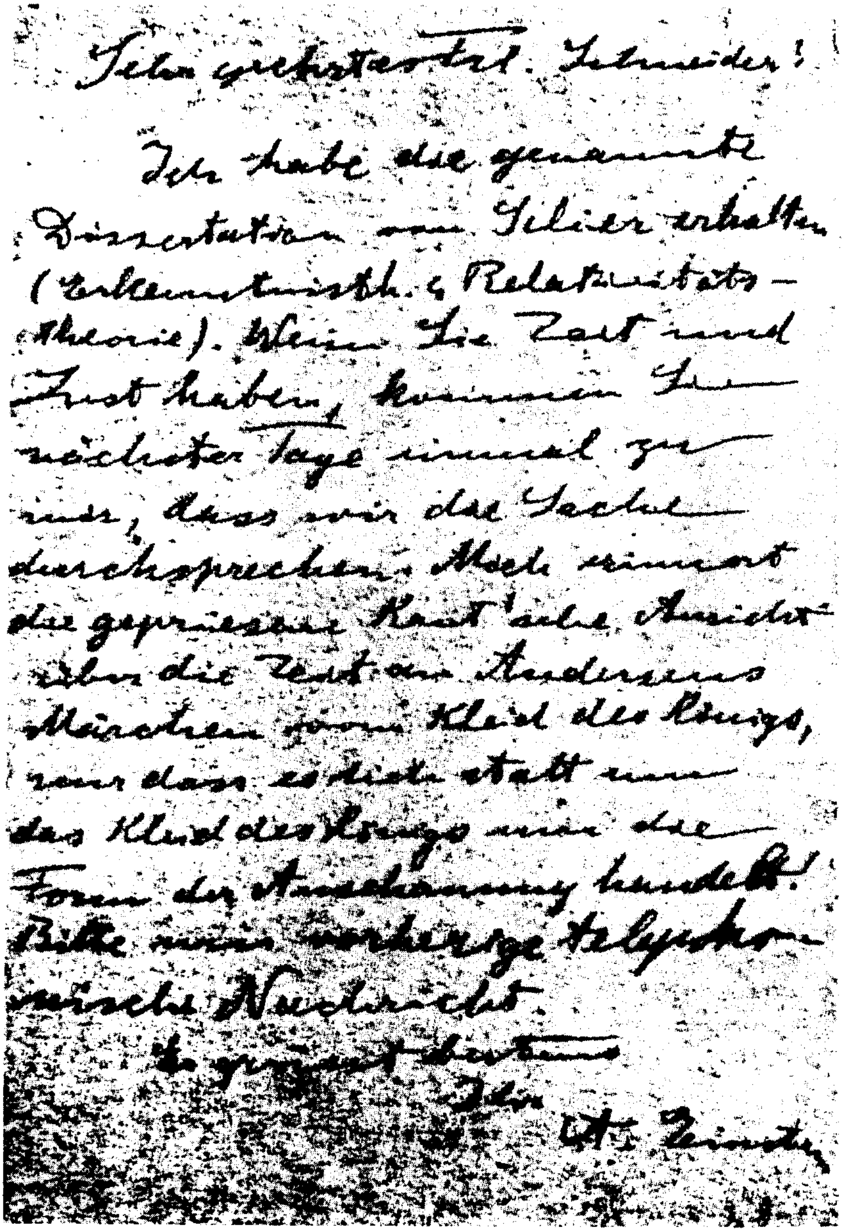
[86]For instance, with Freundlich, Julius (see Klein, *op. cit.*, note 6), later also with Schwarzschild, de Sitter, Bachem and Grebe, St. John and others.

[87]If the light deflection was only half the value 1.75″, then his mass–energy equivalency would be correct but not his general theory of relativity with space–time curvature effects accounting for the other half of the effect; if no light deflection were observed, or if some other value were observed, his general theory of relativity would have to be replaced by some other theory.

[88]See Rosenthal-Schneider (1980), *op. cit.*, note 81, p. 84; cf. Hentschel, *op. cit.*, note 23, section 4.1. about the (neo)Kantian interpretation of the special and general theories of relativity.

[89]Cf., e.g., Ilse Rosenthal-Schneider, 'Albert Einstein: 14 March 1879–18 April 1955', *Australian Journal of Science* **18** (1955), 15–20, see p. 18, and her (1988), *op. cit.*, note 81, p. 60: "He was so sure that his basic assumptions and his calculations were right; that is why he felt its confirmation to be a foregone conclusion"; see also p. 74ff.

[90]A. Einstein, 'Discussion Remarks at the Session of the Société Française de Philosophie on 6 April 1922', *Bulletin de la Société Française de Philosophie*17 (1922), 91–113, see p. 101 and a short English extract in *Nature* **112** (1922), 253 (our translation from French original). See also Rosenthal-Schneider (1980), *op. cit.*, note 81, p. 89.



Sehr geehrtes Frl. Schneider!

Ich habe die genannte Dissertation von Sellien erhalten (Erkenntnisth. u. Relativitäts-theorie). Wenn Sie Zeit und Lust haben, kommen Sie nächster Tage einmal zu mir, dass wir die Sache durchsprechen. Mich erinnert die gepriesene Kant'sche Ansicht über die Zeit an Andersens Märchen vom Kleid des Königs, nur dass es sich statt um das Kleid des Königs um die Form der Anschauung handelt. Bitte um vorherige telephonische Nachricht.

Es grüsst bestens
Ihr
A. Einstein

Fig. 3 Facsimile of Einstein's letter to (Rosenthal-)Schneider, dated 15 September 1919, reproduced from Rosenthal-Schneider (1980), op. cit., *note 81, p. 85 or (1988),* op. cit., *note 81, p. 72, by courtesy of the Estate of Ilse Rosenthal-Schneider.*

Kant's much praised view on Time [in another book on relativity theory just received by the Neo-Kantian philosopher Ewald Sellien] reminds me of Andersen's tale of the Emperor's new clothes, only that instead of the Emperor's clothes [here] we have the form of intuition [Anschauungsform].[91]

The way in which (Rosenthal-)Schneider tries to discuss away this evidence running contrary to her claim that Einstein had strong Kantian inclinations is quite interesting. She introduced the reproduction of this letter in her late book in the following manner:

As an undergraduate I received a postcard from Einstein which may suggest that he did not take the problems of Kant's transcendental aesthetics seriously. But I know he regarded them as important. He just wanted to tease me when he wrote [then follows the above quoted letter].[92]

This is quite a clever way of turning the evidence upside down. From all we know about Einstein's philosophical opinions at that time, this claim reflects nothing but (Rosenthal-)Schneider's personal interpretation of Einstein's views. His book reviews of Elsbach and Winternitz in the 1920s,[93] for instance, as well as other remarks all demonstrate his *rejection of the (Neo-)Kantian approach* towards relativity theory, and, in fact, a certain annoyance whenever someone mentioned Kant at all:

Kant is a sort of highway with lots and lots of milestones. Then all the little doggies come by and each deposits his contribution at the milestones [Dann kommen die kleinen Hunderln, und jeder deponiert das Seinige an den Meilensteinen].[94]

III. In a small handwritten note to (Rosenthal-)Schneider on his visiting card, *shortly after* her publicized meeting with Einstein, namely on 5 January 1920, Einstein expressed his thanks for the *friendly congratulations* (cf. Fig. 4). What congratulations could he have meant but a written note on the positive confirmation by Eddington of his prediction of light deflection? The timing of Einstein's reply to Rosenthal-Schneider's congratulations, which unfortunately are not preserved in the Einstein files, is consistent with this, because the news about Eddington's and his co-workers' results announced on 6 November

[91]Rosenthal-Schneider, *ibid.*, p. 84; cf. the facsimile of Einstein's postcard in *ibid.*, p. 85 or (1988), *op. cit.*, note 81, p. 72.

[92]Rosenthal-Schneider (1980), *op. cit.*, note 81, p. 83 (cf. her (1988), *op. cit.*, note 81, p. 69).

[93]See Einstein, *op. cit.*, note 90, p. 101, A. Einstein, reviews of A.C. Elsbach, *Kant und Einstein* (1924) and of J. Winternitz, *Relativitätstheorie und Erkenntnislehre* (1923), in *Deutsche Literaturzeitung* **45** (=**n.s.** 1) (1924), 20–22, 1685–1692. See also Hentschel, *op. cit.*, note 66.

[94]Einstein according to Rosenthal-Schneider (1980), *op. cit.*, note 81, p. 90; for the German version see her (1957), *op. cit.*, note 42, p. 1 resp. (1988), *op. cit.*, note 81, p. 76.

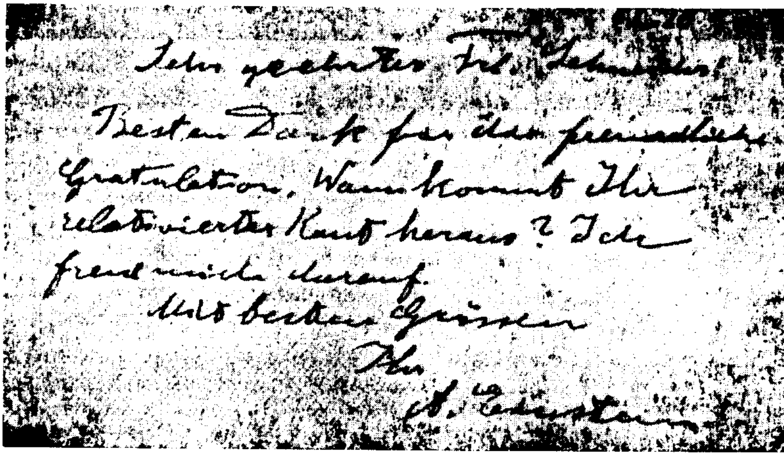

Sehr geehrtes Frl. Schneider!
Besten Dank für die freundliche Gratulation. Wann kommt Ihr relativierter Kant heraus? Ich freue mich darauf.
Mit besten Grüssen
Ihr
A. Einstein

Fig. 4. Facsimile of Einstein's note to (Rosenthal-)Schneider, dated 5 January 1920, reproduced from Rosenthal-Schneider (1980), op. cit., *note 81, p. 87 resp. (1988),* op. cit., *note 81, p. 73 by courtesy of the Estate of Ilse Rosenthal-Schneider.*

1919,[95] did not appear in English newspapers before 7 November 1919,[96] and not before 18 November 1919, in Germany,[97] and it is quite possible that (Rosenthal-)Schneider learnt about the results from the *Vossische Zeitung*, which was, especially among intellectuals, the most widely read newspaper in Berlin. But if she congratulated Einstein on the English expedition results in written form, this implies she had *not* been present when he received the news.

IV. (Rosenthal-)Schneider first told her story two years after Einstein had died,[98] and *not*, e.g., in her contribution to the Schlipp volume of 1949.

All these points make it highly probable that *Rosenthal-Schneider spread this story to illustrate her claim that Einstein, von Laue and Planck were all Kantians of some sort*, a claim which is only true for von Laue. The wide circulation which this story about Einstein has received[99] has helped to create the

[95]Cf. Eddington to Einstein, 1 Dec. 1919, CPAE, 9 261: "Our results were announced on Nov. 6; and you probably know that since then all England has been talking about your theory. It has made a tremendous sensation; and although the popular interest will die down, there is no mistaking the genuine enthusiasm in scientific circles and perhaps more particuarly in this University."

[96]See *Times*, 7 Nov., pp. 11ff; 28 Nov., pp. 13–14; 4 Dec., p. 15; 13 Dec., p. 9, col. 1; *New York Times* **69** (1919), 9 Nov., sect. 1, p. 6; 16 Nov., sect. 3, p. 1, col. 5; 18 Nov., p. 12, col. 5; 29 Nov., p. 11, col. 3; 3 Dec., p. 19, col. 1–2; 5 Dec., p. 14, col. 5; 7 Dec., p. 11, col. 5–6; etc.; cf. Pais, *op. cit.*, note 7, p. 306ff., Lewis Elton, 'Einstein, General Relativity and the German Press', *Isis* **77** (1986), 95–113.

[97]See, e.g., 'Einstein und Newton. Die Ergebnisse der Sonnenfinsternis vom Mai 1919', *Vossische Zeitung*, 18 Nov., evening edition; cf. 'Das Relativitätsprinzip', *ibid.*, 7 Dec. and 14 Dec., and *Neue Zürcher Zeitung* **141** (1920), 8 Jan., 1st and 3rd morning issue, pp. 1–2. Einstein himself had already released information on the earlier Lorentz telegram to Alexander Moszkowski, who published them on 8 Oct. 1919 in the *Berliner Tageblatt*.

[98]In an unpublished memo probably written for Helen Dukas: see (Rosenthal-)Schneider, *op. cit.*, note 42, p. 2.

[99]It is quoted even by Einstein scholars such as Holton, *op cit.*, note 1, pp. 236–237.

misleading myth of Einstein as the all-knowing theoretical physicist who disregarded experiments to a large extent. The existing and mutually consistent documents all point towards a quite contrary conclusion as regards Einstein's attitude towards experiments. As historians of science, part of our responsibility is to debunk unfounded (or at least uncorroborated) myths. Another perhaps more interesting part might be to interpret their meaning. Without being able to fulfil this latter task in this paper, I would at least like to point out the temporal correlation of the faulty portrayal of Einstein as a high-brow theorist and a period of theory-dominated history and philosophy of science.

Considering the wealth of available material and the complexity of the issue including the possible time dependency of Einstein's views about experiments, it might be frowned upon to try to condense what I said before about Einstein's attitude towards experiments, but the following quote from an Einstein letter in contribution to a commemorative session of the Cleveland Physics Society for Michelson's hundredth birthday on 19 December 1952, might serve this purpose. This text has already been referred to by Gerald Holton in his analysis of the degree of importance which the Michelson–Morley experiment had for the development of the special theory of relativity,[100] but its last sentence directly pertains to our problem:

> There is, of course, no logical way leading to the establishment of a theory, but only groping constructive attempts controlled by careful consideration of factual knowledge.[101]

While the first part of the activity of theoretical physicists as portrayed by Einstein as 'groping conceptualization' ("tastendes Konstruieren") is the well-known characteristic of Einstein's epistemology at least in his rationalistic period after 1925, the latter part, 'careful consideration of the established facts', encompasses his attitude towards experiments throughout his life: empirical results are taken into consideration, not simply all-inclusively as naive empiricists would like to have it, but carefully ("sorgfältig"). I hope to have illustrated that in this quote 'careful' is no less than a very condensed expression of his experiments as they fit within an overall experimental context ('experimentelle Gesamtheit'), in which many independent results all have to

[100]See Gerald Holton, 'Einstein and the Crucial Experiment', *American Journal of Physics* **37** (1969), 968–982, reprinted in Holton, *op. cit.*, note 1, pp. 261ff.; cf. Shankland, *op. cit.*, note 47.
[101]As translated by Holton, *op. cit.*, note 1, p. 285.

point towards a certain conclusion which then deserves "*tastendes Konstruieren*".[102]

This is why Einstein overtly rejected certain empirical claims contradicting his theories — not because of his blind reliance on his own capabilities as a theoretician, through arrogance or dogmatism, but because they lacked this consistency with other well-established results, and because they were unconnected to sets of 'crucial experiments' highly regarded by Einstein which provided guidance at theoretical bifurcations and thereby confirmed (or rejected) his own preferences and decisions. Thus, his rejection of the many other experimental results which were not 'crucial' in this sense can also be understood and a unified perspective is gained on Einstein's attitude towards experiments from *c.* 1907 to *c.* 1927.[103]

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[102]Consistent with his claim, right before this quotation, Einstein claimed that it was not the Michelson–Morley experiment alone which convinced him, but rather the famous thought experiment about the asymmetry of conductor and magnet moving relative to each other in the electrodynamics of his day, in conjunction with the result of Fizeau's experiment and Bradley's aberration of fixed starlight. Einstein looked upon these many independent experimental results as one such 'experimentelle Gesamtheit' from which he started to construe the principle of relativity and the postulate of constancy of the velocity of light in vacuo as the two axioms of his special theory of relativity.

[103]So, beside all changes in Einstein's epistemological attitudes around 1920, this seems to me to be another candidate for Holton's list of Einstein *Themata*. See, e.g., Holton, *op. cit.*, note 1.