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Johann Georg von Soldner and the Gravitational Bending of Light, with an English Translation of His Essay on It Published in 1801

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Following Einstein's prediction of the gravitational bending of light, and in the course of experimental work aimed at its verification, only sporadic and at times misleading references have been made to Johann Georg von Soldner. In a paper published in 1804, Soldner derived the gravitational bending of light on the classical Newtonian basis and calculated its value around the sun with remarkable accuracy. Soldner's paper, inaccessible even in German, is now presented in English translation and put in the perspective of Soldner's life and the science of his day and ours.

In the first year of the nineteenth century there appeared in the widely read *Astronomisches Jahrbuch* an article by Johann Georg Soldner on the gravitational bending of light,[2] a topic which in the twentieth century is routinely taken for a hallmark of what is exceptionally original in modern physics. Throughout the nineteenth and well into the twentieth century Soldner was remembered for other scientific contributions,[3] a fact suggestive

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[2] For its title in the German original, see Ref. 26. The *Astronomisches Jahrbuch* is usually referred to as *Berliner Astronomisches Jahrbuch*, an annual volume founded by J. H. Lambert in 1774, or three years before his death, as *Astronomisches Jahrbuch oder Ephemeriden*. In addition to its ephemeridal part it contained an even longer section described in its subtitle as "a collection of the latest observations, news, comments, and essays relating to the astronomical sciences." Following Lambert's death, his former assistant, Johann Elert Bode, held the editorship for almost half a century, the reason why the *Jahrbuch* is also often referred to as *Bode's Jahrbuch*.

[3] The reference to Soldner's article by Poggendorff[(1)] seems to be its only specific citation in the hundred and twenty years following its publication. Karl Max von Bauernfeind, Soldner's first biographer and an engineer by training, offered only the vague generality

of the irony which is time and again evident in the route followed by scientific progress. Irony was lurking in the background when Einstein declared in 1907 in a tone of unmistakable originality that gravitation must have an influence on the path of light and gave a formula for its deflection per centimeter.(10),4 More irony was in store when in 1911 Einstein calculated the bending of light around the sun, because it occurred to him that the effect could be observed during total solar eclipse. Neither Einstein, nor the many readers of his article in *Annalen der Physik*, realized that a hundred and ten years earlier almost exactly the same value, 0."84 versus 0."83 as given by Einstein,(11) had already been calculated by Soldner.

The first reaction to Einstein's prediction was almost as complete a silence as the total indifference that greeted the publication of Soldner's paper. Although in March 1914 Erwin Freundlich, a young German astronomer, called attention to the opportunities offered by the total eclipse

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that, after joining Bode, as his assistant, Soldner "was able to contribute to the astronomical yearbooks of his teacher several literary works whose significance, together with the remarkable circumstances of his education, soon earned him supporters and friends." This phrase is from Bauernfeind's seven-page account of Soldner's life and achievements,(2) an account based on Bauernfeind's rectoral speech on Soldner delivered on July 27, 1885, at the Technische Hochschule of Munich and published as a brochure,(3) which I was unable to consult. Lack of reference to Soldner's article is understandable in the two pages devoted to Soldner by Amann(4) (I was unable to consult an earlier work of essentially the same scope by von Orff(5)). From what immediately follows in the text of this article it should appear highly ironical that Soldner's article was not referred to in the introduction which J. Frischauf wrote to the reprinting in 1911 of Soldner's *Theorie der Landesvermessung*.(6) The irony was heightened when in 1914 there appeared a 160-page-long dissertation on Soldner by Franz Johann Müller(7) which contained a detailed summary (pp. 46–47) of Soldner's calculation of the bending of light. The dissertation, submitted to the Technische Hochschule in Munich as a partial fulfillment for the degree of doctor in engineering science, was a somewhat enlarged form of Müller's essay published in a journal a year earlier under the same title.(8) It shows something of the slow spread of information about major scientific breakthroughs that neither Müller, a land-surveying official in Augsburg, nor Dr. Max Schmidt and Dr. Sebastian Finsterwalder, professors at the Technische Hochschule, who on October 26, 1914 approved of the dissertation, were aware of the fact that Soldner's calculation anticipated a result obtained by Einstein. In fact, as late as 1922, when another essay by Müller on Soldner was published,(9) he still did not suspect the historic significance of Soldner's paper. He referred to it only by title, although, unlike in his long dissertation, he found place for a brief summary of Soldner's article on the relative motion of stars (see Ref. 28). Müller gave the impression that Soldner's article had to do with the motion of the sun toward the constellation Hercules, a point which Soldner did not even refer to. The partial reprint by Lenard of Soldner's paper in 1921 in the *Annalen der Physik*(26) came, of course, too late to be taken into account by Müller.

[4] Einstein was, of course, highly original inasmuch as he postulated the effect on the basis of the principle of equivalence.

of August 21, 1914, his words failed to elicit interest.[5] A main reason for this might have been the realization that the predicted value was very close to the limits of observational precision. Indeed, the response was markedly different when in 1915 Einstein doubled the predicted value.[6] With the new value, or 1.″7, the feasibility of testing the prediction increased so much as to set in motion a chain of events which within a few years produced scientific news that electrified not only the body scientific but also the public at large.

The first link in that chain was an article published in March 1917 in the *Monthly Notices* in which attention was called to the total eclipse of May 29, 1919, an eclipse occurring against a background of very bright stars.[(16)] In October 1917 government funds were requested by the Royal Society and on March 8, 1919, Eddington and three other British astronomers boarded ship to set up observational stations outside the city of Sobral in northern Brazil and on the island of Principe off the western coast of Africa. The observational results made scientific history when the paper accounting for the expedition and for the observations was presented by Sir Frank W. Dyson, the Astronomer Royal for England, to the Royal Society meeting on November 6, 1919. As the group at Sobral measured a deflection of 1.″98 $\pm$ 0.″12 and the group on Principe 1.″61 $\pm$ 0.″30, the paper's conclusion was that "the results ... can leave little doubt that a deflection of light takes place in the neighbourhood of the Sun and that it is of the amount demanded by Einstein's generalised theory of relativity, as attributable to the sun's gravitational field."[(17)] The meeting was immortalized by Whitehead in a well-known passage of which two details need to be recalled here. One was his remark that "a great adventure in thought [general relativity] had at length come safe to shore," the other was his reference to the portrait of Newton in the background "to remind us that the greatest of scientific

[5] For Freundlich's call, see Ref. 12. For a brief account of the various types of work done in connection with that eclipse, see Ref. 13.

[6] This doubling was part of a paper read by Einstein at the plenary meeting of November 18, 1915, of the Berlin Academy on the explanation of the advance of the perihelion of Mercury according to general relativity. The explanation depended on solving to first and second approximations what Einstein called a "most radical form of relativity theory."[(14)] The solution to second approximation yielded the prediction of an advance of 45″ per century of the position of Mercury's perihelion, whereas the solution to first approximation demanded in the case of the bending of light around the sun a value twice as great as the one calculated in 1911, which, as Einstein now realized, corresponded to the Newtonian case. The difference between the Newtonian and Einsteinian solutions is presented succinctly in the introductory part of an account by Weber in Ref. 15, a presentation which starts with a reference to Soldner's paper. Yet Weber's account of its contents and the diagram given by him to illustrate Soldner's procedure makes one wonder whether Soldner's paper has really been consulted.

generalisations [Newton's theory of gravitation] was now, after more than two centuries, to receive its first modifications."[18]

The true situation was less safe than appraised by Whitehead, and for two reasons: first, for the next half century further measurements of the bending of light during solar eclipses did not appreciably reduce the probable error of the first test. The situation changed for the better only in the early 1970s when measurements with radiotelescopes yielded a result which was 0.96 ± 0.05 times the value predicted by general relativity.[19],[7]

The other reason concerns the portrait of Newton. As an advocate of at least a partially corpuscular theory of light, Newton could have naturally thought of and even calculated the bending of light in a gravitational field. He, of course, had reflection, refraction, and diffraction in mind as he noted in 1704 Query 1 of the *Opticks*: "Do not Bodies act upon Light at a distance, and by their action bend its Rays; and is not this action (*caeteris paribus*) strongest at the least distance?"[22] Yet this Query, which implies an affirmative answer, was phrased, though unwittingly, in such a generality as to accommodate even the idea of the gravitational bending of light. To spell out that idea and to deal with it quantitatively would have been most natural for a Newton who coped with far more intricate problems in celestial dynamics than that of the bending of light. The formulation and solution of that problem came, however, only in 1801 and even then not from a professional scientist but from a largely self-taught man, Soldner, apprentice-assistant of Johann Elert von Bode, Astronomer Royal for Prussia and editor of *Astronomisches Jahrbuch*. But Soldner's article made no ripple in the scientific world. It soon became so forgotten that his biographers, none of them astronomers and cosmologists, could hardly be fascinated by its title and discover its significance. No wonder that Soldner's portrait was not in evidence at that memorable meeting of the Royal Society, although its display would have been as appropriate as that of Newton's portrait.

Astronomers and physicists got a glimpse of Soldner, only to be soon forgotten, through a series of events that was sparked by none other than Eddington, a key figure in the 1919 expedition. In his *Report on the Relativity Theory of Gravitation*, first published in 1918, Eddington pointed out the factors which in Einstein's theory led to a value of the bending of light twice as great as predicted by Newtonian theory.[23] But Eddington did not mention Soldner, nor did E. Lihotzky, a physicist at the famous Leitz optical firm in

[7] It was an uncanny sign of diffidence about the conclusiveness of measurements made with ordinary telescopes that the bending of light received in 1964 only seven lines in an authoritative book-length survey by Dicke[20] of all experimental tests of general relativity. A more favorable evaluation was given in 1955 by Trumpler, with a strong dissent on the part of E. Finlay-Freundlich; see Ref. 21.

Wetzlar, who studied Eddington's *Report* and decided to work out in detail a comparison between the two theories in a paper printed with obvious haste in the January 1, 1921 issue of the *Physikalische Zeitschrift.*[24] Its technical conclusion was that the difference between the Newtonian and the Einsteinian predictions was not a sharp difference between two values differing by 100 %; rather the difference varied as the function of distance from the center of the attracting body and at a certain distance from that center the two values coincided. To this technical conclusion, which in Lihotzky's eyes was damaging, though not fatally, to Einstein's theory, he added a philosophical conclusion which covertly endorsed the charge that relativity rested on complex assumptions bordering on contradictions. The truth of a theory, Lihotzky claimed, ultimately rested on its being free of contradictions: "if it makes a number of existing contradictions to disappear without introducing new ones then we must attribute to it a greater content of truth."[24]

Lihotzky's paper was seized upon by Philipp Lenard, leader of the anti-Einstein crusade waged by several prominent German physicists, as he was writing an article on questions concerned with the speed of light and especially with the bearing on it of the Michelson–Morley experiment.[25] The article was anti-Einsteinian polemics and Lenard found, in the style of polemists, everything to be grist to his mill. Although Lihotzky's paper had nothing to do with the topic of Lenard's paper, Lenard referred to it in a long footnote, using it as another evidence that Einstein's theory not only did not give predictions really different from those of Newton's theory, but was also far less simple than all great theories of physics and was therefore not necessary.

Lenard obviously discussed the contents of his paper with the astronomer Max Wolf, his colleague at the University of Heidelberg, because on April 22, two days after he had sent his article to the *Astronomische Nachrichten,* he received word from Wolf about a piece of information which the latter had just obtained from Martin Näbauer, then professor of geodesy at the Technische Hochschule in Karlsruhe. According to that information, the bending of light had been discussed and computed by Soldner more than one hundred years before Einstein. It should not be difficult to imagine Lenard's elation upon hearing that news about Soldner, news that apparently was now spreading. Indeed, the publication in early June of Lenard's paper prompted Hugo von Seeliger, professor of astronomy at the University of Munich, to write to Lenard about Soldner. That Seeliger learned from Wolf about Soldner, or from Näbauer himself, is a distinct possibility. That Näbauer knew of Soldner's feat can be explained by the fact that while receiving his PhD in geodesy at the University of Munich, Näbauer could easily have developed an interest in the life and work of Soldner, whose memory was alive among Bavarian geodesists and in their organization, the Bayerische Landesvermessungsamt.

On the communication on Soldner by Wolf and Seeliger to Lenard the latter is our source of information in a long footnote of an article of his published on September 27, 1921, in *Annalen der Physik*.[26],[8] That the article saw print so quickly indicates that Lenard worked with all possible speed to make it public knowledge that concerning his prediction of the bending of light, Einstein had a remarkably accurate and original predecessor. Lenard's 11-page-long article had for its title the very title of Soldner's article followed by Soldner's name. This could only give the impression that the subtitle, "With an Introductory Remark by P. Lenard," would not overshadow Soldner's contribution. Actually, the "Introductory Remark" was as lengthy, if not more, as the space reserved for Soldner's article, of which only the first two and hardly most important pages were reproduced verbatim, the rest given in summary. Clearly, Lenard was interested in Soldner only insofar as the latter could be used or rather abused as anti-Einsteinian ammunition. Soldner was at least indirectly abused as Lenard presented him as a precursor of Planck's quantum theory of light, without pointing out that it was under the impact of Einstein's persistent argumentation that Planck at long last accepted the view that not only does the emission of light take place in quanta, but in its propagation, too, light remains quantized. The quantization of light was an idea as removed from Soldner's mind as was the mass–energy equivalence about which Lenard was eager to point out that Hasenöhrl, without using the assumptions of relativity, derived the formula $E = M/c^2$, in 1904, a year before Einstein did. Those assumptions were taken by Lenard as lightly as were the reasons which prompted Einstein in 1915 to revise his calculation of the bending of light as given in 1911. Soldner was clearly abused when Lenard stated with an eye on his work that "either the theory of relativity (1911) is in its content identical with the simple [Newtonian] assumption ... or is contrived and connected only in appearance with the result." The same was true when, after discussing at length the question of the advance of the perihelion of Mercury, Lenard declared that "the introduction for its explanation of a cumbersome theory, such as the theory of relativity, which, as shown, has nowhere a secure ground in experience, can so far appear arbitrary and disturbing" (Ref. 26, pp. 597, 600).

Had Lenard had an unselfish interest in Soldner, he would have put Soldner's startlingly original paper in its context, namely, in the light of

[8] What immediately follows in the text of this article should make it clear that the reference by Whittaker[27] to the "reprinting" of Soldner's article in the *Annalen der Physik* is misleading.

Soldner's first publication, a cosmological paper on the motion of stars in the Milky Way,[(28)] to be discussed later. Worse even, Soldner himself received but perfunctory mention in Lenard's "Introductory Remark." Clearly, he was too prominent an astronomer to be described as a "German mathematician and geodesist." He would have hardly agreed with Lenard's other brief remark about him, that he, "a Bavarian, the son of a peasant, had the advantage of not having attended too many schools."[9] On the contrary, young Soldner longed to go to school, but, apart from a year or two in the elementary school in Feuchtwangen (the village near the farm Georgenhof where he was born on July 15, 1776), he never had formal schooling.[10] He was already sixteen when he received private instruction in Latin and French, and had reached twenty when friends steered him to a scientist in Ansbach. That Soldner was a man of unusual talents was further recognized when an official of the Prussian government in Ansbach obtained for him a yearly pension to work as assistant of Bode in the Berlin Observatory. It was through publications in Bode's *Jahrbuch*, in Zach's *Monatliche Correspondenz*, and in Gilbert's *Annalen* that his name became known. In addition to his papers on the motion of stars in the Milky Way and on the bending of light, he published about that time a paper on the path of the comet newly discovered by Piazzi, a comet which turned out to be the first asteroid. In 1804 there appeared his first paper on land-surveying, a discussion of the Swedish project of measuring the length of a degree, and a year later his proposal for a similar project in equatorial Africa.[(29),11] In the same year he also published a paper on the general law governing the force of expansion of steam.

It was undoubtedly under the impact of these papers that by 1805 he had been asked three times to accept the directorship of the Observatory of the University of Moscow. Friends who wanted to keep his talents for Germany secured for him the appointment in 1805 as director of Prussian land-surveying in the Ansbach district. Three years later he was in the

[9] Reference 26, pp. 593 and 595. While keeping silent about all of Soldner's publications and activities as astronomer and geodesist, Lenard pointedly recalled, for patently chauvinistic reasons, that Soldner was the first to propose the idea of a dew-point hygrometer which later became known under the names of Daniell and Regnault.

[10] For this and other details on Soldner's life, see the easily accessible article of Bauernfeind.[(2)]

[11] As the precision of geodetic measurements depends on the exact amount of the flattening of the earth, equatorial measurements of the length of a degree of an arc are indispensable for accuracy. The coastland of the Congo appeared to Soldner to be an ideal place for carrying out such measurements because it lay almost directly south of central Europe and had just come under the control of European powers. Soldner particularly felt the need for carrying out his project, because measurements of the length of a degree near Philadelphia could not be reconciled with measurements made in Europe.

same capacity in the service of the Bavarian government. His rapid rise earned him the envy of some, to such extent that a year after his appointment to the Munich Academy he resigned in order to clear his name of the charge of plagiarism. The charge concerned the originality of his famous memoir on the reduction of observed azimuths printed in 1813.[12] His name was not only cleared, but his accuser, Felix Seyffer, member of the Academy and director of its observatory, was removed from his post, which was filled by Soldner, who also took on the further task of planning a new observatory.

It was a reflection on Soldner's competence and industry that the new observatory had by 1818 been completed in Bogenhausen near Munich and equipped with the best instruments. These at that time could only come from the workshop of Fraunhofer, who later owed in a large part to Soldner his nomination to the Munich Academy. In 1825, at the urging of the younger Herschel, he was elected a corresponding member of the Royal Astronomical Society in London, and about the same time he was knighted by the King of Bavaria and the King of France. Meanwhile, because of a liver ailment, he was forced to leave to his assistant, Johann Lamont, the observational work, which had much to do with the question of the motion of stars. Germane as these observations were to the topic of his first published paper, Soldner left cosmology untouched in publishing those observations in the same way as he did in his letter written around 1806 to Gilbert, from which the latter published a lengthy section in his *Annalen* in 1811, under the title, "On the Theory of Light, of Heat, and about a Work from Integral Calculus."[(32),13] No sooner had Soldner burst onto the cosmological scene in 1800 and 1801 with two papers, each of which suggested a boldly speculative mind equally bent on observational precision, than he gave himself entirely to mathematical investigations useful for astronomy and geodesy. In that connection his most meritorious work, reprinted many years later in Ostwald's *Klassiker der Naturwissenschaften*, was a method of computing triangles on a flattened spheroid such as the earth.[(6)] Its principal novelty was the method of calculating the length of arcs instead of that of the chords and with an error not greater than 1 cm over several kilometers.

[12] See Ref. 30; the method was further elaborated by Soldner in 1815 in Bode's *Jahrbuch*. The essential point made by Soldner was defended as "fortunate and new" by Delambre.[(31)]

[13] Soldner argued that light could very well be a fourth state of matter, in addition to its solid, liquid, and vaporous states. His letter contained a sharp criticism of the curricula of German schools and universities, especially of their emphasis on philological studies to be pursued by future scientists (Ref. 32, p. 238). However, he did not suggest that he had been fortunate for not having attended too many schools, as claimed by Lenard.

As a self-taught man Soldner was plagued to the end of his life by the envy of incompetent colleagues. Following Soldner's death on May 18, 1833, Franz Gruithuisen, professor of astronomy at the University of Munich, but notably weak in mathematical techniques that were Soldner's forte, was proposed to be his successor at the Bogenhausen observatory, a plan which caused concern even to Schelling, president of the Munich Academy, who, as a chief proponent of Naturphilosophie, was hardly a friend of exactness in the sciences. That at the urging of Gauss and Bessel it was not Gruithuisen but Lamont who succeeded Soldner helped keep alive Soldner's renown as a champion of precision and new mathematical techniques both in astronomy and geodesy. As a highly original author of an essay on the bending of light, a topic with deep cosmological relevance, Soldner is still to be accorded the wide recognition he deserves.

That the bending of light has such relevance hardly needs to be noted since Einstein. But even before him, and as early as in Soldner's time, the question of the bending of light was not without potentially deep relevance for physics and cosmology. Measuring that bending, as calculated by Soldner, could have reinforced the corpuscular theory in the face of the growing popularity of the wave theory, a theory that had been revived by Thomas Young's work in the same year of 1801 that Soldner's paper on the bending of light appeared. For, if light was a wavelike propagation in an imponderable ether, a notion much in vogue especially in the latter half of the nineteenth century, the bending would not take place.

To measure the calculated effect, one first needed a telescope with a resolving power of fractions of a second of an arc. Such telescopes were available by the 1820s through the work of Fraunhofer. His instruments enabled Bessel and Struve to measure for the first time stellar parallaxes which were much smaller (0.″12 for α Lyrae and 0.″29 for 61 Cygni) than the amount predicted by Soldner. In addition, one needed the advent of stellar photography, but sufficient progress was made in this by the closing decades of the century to make feasible an experimental test of Soldner's calculation well before Einstein brought the matter into new focus.

But Soldner's idea about the bending of light was not the only cosmological notion far ahead of his time. Actually, it seems to have been prompted by another cosmological idea equally far ahead of the times but equally timely since the advent of general relativity. The idea is that of a star made invisible by its gravitational field, a notion strikingly resembling the notion of black holes, and proposed in 1796 by Laplace in the first edition of his *Exposition du système du monde*.[(33),14] There Laplace confined himself to

[14] The passage, still present in the second edition (De l'Imprimerie de Crapelet, Paris, An VII [1799], p. 348), was omitted in the third (1808), fourth (1813), and fifth (1824) editions for reasons which Laplace did not specify.

the assertion that the light of a star whose diameter exceeds by 250 times that of the sun and whose density is equal to that of the earth would be trapped in its gravitational field.[15] At the urging of Franz Xavier von Zach, astronomer of Prince Ernest of Saxe-Gotha, Laplace worked out the proof of his assertion in a brief essay which saw print in German in von Zach's *Allgemeine geographische Ephemeriden* in 1799.[16] Laplace's proof, based on the equality of the velocity of light to the escape velocity at the surface of the star specified by him, provoked echo only in a very few. Soldner was one of them.[17]

For Soldner the mathematics of Laplace was unobjectionable but not his metaphysics![18] The latter was wrong because it did not take into account, and here Soldner quoted Kant's *Die metaphysischen Anfangsgründe der Naturwissenschaft* and Newton's *Principia* as authorities, the principle that all material change depends on a material cause external to it.[19] Therefore, Soldner argued, it was faulty to assume with Laplace that the velocity of light was constant, because the processes of light emission were presumably very variable and could even be such as to impart to light corpuscles a relatively small velocity. Thus, depending on the chemical and mechanical condition of its material, a star could even be very small and still trap the light emitted by it. To this remark relating to the problem of a central body

[15] In a paper read at the Royal Society on November 27, 1783 and published in 1784[(34)] John Michell had calculated that "if the semi-diameter of a sphere of the same density with the sun were to exceed that of the sun in the proportion of 500 to 1 [the actual calculation gave a ratio of about 497 to 1] ... all light emitted from such a body would be made to return towards it, by its own proper gravity" (Ref. 34, p. 42). While, of such bodies, "we could have no information from sight; yet, if any other luminous bodies should happen to revolve about them," the disturbances in the motions of these visible bodies might still allow us to "infer the existence of the central ones with some degree of probability" (Ref. 34, p. 50). See Ref. 35 for a discussion of this paper. Further, just as Laplace's calculation prompted a response from Soldner, so Michell's paper seems to have led Henry Cavendish to calculate, in a very brief note found among his unpublished papers, the "bending of a ray of light which passes near the surface of any body by the attraction of that body," yielding a result, like Soldner's, of about one-half that of Einstein's.[(36)]

[16] Available in English translation as an Appendix in Hawking and Ellis.[(37)]

[17] Among those few were von Zach and Wilhelm Olbers. The main point of von Zach's remark was that light was not simply trapped at the surface of such a star but reached various heights depending on the nature of its emission. Olbers then drew the further inference that the aberration of light, a function of its velocity, had to be a variable quantity and proposed a possible observational verification. See Ref. 38.

[18] See his article on the motion of stars, Ref. 28, p. 191.

[19] Reference 28. Soldner obviously had in mind Proposition 3 of Chapter III of the *Anfangsgründe*, which is a paraphrase of the first law of motion as given in Newton's *Principia*. In referring to Kant and Newton, Soldner merely bolstered the ideas of von Zach and Olbers mentioned in note 17.

in the star system Soldner added that the aberration of starlight had also to be considered a variable magnitude because of the variability of the speed of light.

Soldner discussed Laplace's invisible stars in the concluding part of an article which dealt with the problem whether a star system like the Milky Way had a large mass in its center or not. That the observable stars formed a system could not be doubted since Kant, Lambert, and Herschel—so Soldner declared—but whether their motion around a center was governed by a massive body there was another question. Soldner decided against the presence of such a central body on the ground that the motion of stars around it would be too fast not to be noticed. To support this point he calculated the relative angular velocities of stars closer and farther from the center of a homogeneous distribution of stars resembling a highly flattened spheroid. Since he assumed that all visible stars were so close to the center of the Milky Way as to be within one-tenth of its radius, not even their relative angular velocities could differ perceptibly.[20] As a result, Soldner concluded, it was not justified to assume the existence of a central body in the Milky Way, not even in the form of Laplace's invisible stars, which he now discussed in detail as given above.

Soldner's paper has therefore two principal aspects. One was his readiness to subject to the test of mathematical physics the idea of central bodies, a test which was well beyond Kant's amateurism, but certainly within Lambert's and Laplace's abilities. It shows something of Soldner's outstanding talent that he did what famous scientists before him failed to do. The other aspect is his rigorous insistence on the mechanical character of all processes. It was that rigor which made him postulate the variability of the speed of light and its being subject to gravitational attraction, just as any other material phenomenon and entity was subject to it. His pondering of such ideas could naturally raise in his mind the idea of the bending of light in strong gravitational fields and make him search for a method of determining it with accuracy. The result was a classic paper which even today receives but sporadic mention in the literature[21] and therefore deserves

[20] Soldner hoped that future observations of nebulae similar to the Milky Way would permit the calculation of the actual orbital velocities of stars.

[21] That the partial reprint of Soldner's article only took place in 1921 explains the absence of reference to it in the now classic accounts of relativity by H. Weyl (1918), M. Born (1920), A. S. Eddington (1920), W. Pauli (1921), and L. Silberstein (1922). In Einstein's popular accounts of relativity Soldner is never mentioned. Soldner is not referred to in major books on relativity and/or gravitation by G. D. Birkhoff (1923 and 1925), J. Rice (1923), R. C. Tolman (1934), P. G. Bergmann (1946 and 1968), C. Møller (1952), J. Weber (1961), and C. W. Misner, K. S. Thorpe, and J. A. Wheeler (1973). In connection with relativity and gravitation Soldner would today be well remembered had he been mentioned in *The Universe and Dr. Einstein* by Barnett,[39] which through its

to be rendered into English all the more as it is rather inaccessible in the German original.

In translating Soldner's article faithfulness to the original has been the principal aim. Thus, for instance, whereas in connection with Soldner's topic the word bending is almost invariably used today in English, Soldner's somewhat indiscriminate use of the words *Ablenkung*, *Krümmung*, and *Perturbation* has been scrupulously followed in the translation. So was the punctuation of the original. The diagram illustrating the bending of light, which is given here as part of the text, is printed in the original as Fig. 3 together with other diagrams on an end-foldout page. The mathematics in the article involves no advanced calculus and the steps of demonstration are given by Soldner so meticulously as to make clarifying notes unnecessary. Soldner must have, of course, known (and the same holds true of Bode, editor of the *Jahrbuch*) that the gravitational potential is proportional to g and not to $2g$. Therefore one must perhaps assume that behind Soldner's use of $2g$ was his realization that the bending of light around a celestial body would be 2ω, that is, ω of the diagram and its mirror image. At any rate, the essential achievement in Soldner's article is not so much the value of ω obtained by him as his essentially sound treatment of light as being subject to gravitation. The decimal seconds refer to the division of a circle into 400 degrees, a system of angular measurement in vogue for a relatively short time before and after the publication of Soldner's paper. Soldner's reference to Vidal is to Jacques Vidal (1747–1818), director of the Toulouse Observatory from 1791 on, whose observation of Venus was relayed by Lalande to von Zach, who published the news in the July 1800 issue of his *Monatliche Correspondenz*. The O. L. preceding Zach's name in the original stands for *Oberleutnant*, rendered here as Lt. The first volume of Laplace's *Traité de mécanique céleste*, used by Soldner as a source of data, was published in 1799. Soldner was clearly intent on using the latest and the best. The quotation from Lucretius is given in Rouse's prose translation,[44] but, in accordance with the Latin quoted by Soldner, is broken into verse form.

countless reprints as a Mentor paperback has become the principal popularization of Einstein's ideas. The situation could have been remedied, but was not, through the otherwise well-researched book by Clark.[40] In fact, as late as 1975 Soldner's work on the gravitational bending of light failed to be mentioned in the article on him in the *Dictionary of Scientific Biography*.[41] In Soldner's own land mention of his anticipation of a prediction of Einstein is still largely restricted to circles of geodesists. See, for instance, Prof. R. Sigl's memorial address in Georgenhof, Soldner's birthplace, on March 26, 1966,[42] and the commemorative address of the bicentennial of Soldner's birth delivered on February 9, 1976 by Prof. E. Messerschmidt.[43]

ON THE DEVIATION OF A LIGHT RAY FROM ITS MOTION ALONG A STRAIGHT LINE THROUGH THE ATTRACTION OF A CELESTIAL BODY WHICH IT PASSES CLOSE BY

Herr Joh. Soldner

Berlin, March 1801[22]

In the present very imperfect condition of practical astronomy it will be ever more necessary to develop from theory, that is, from the general properties and interaction of matter, all circumstances which may have an influence on the true or median position of a celestial body, so that one may derive from a good observation all the benefit which it is capable to yield.

It is, of course, true that already through observations and otherwise one was aware of considerable deviations from an assumed law; such as was the case with the aberration of light. There can, however, be deviations which are so small that it is difficult to decide whether they are true deviations or errors of observation. There can also be deviations which are considerable but, being combined with magnitudes one has not yet succeeded in clearly identifying, escape the observer.

Of the latter kind may be the deviation of a light ray from straight line when it passes close by a celestial body and is considerably exposed to its attraction. For then one can easily see that this deviation should be the greatest when, seen from the surface of the attracting body, the light ray comes in the horizontal direction, and will be zero when the light ray comes down vertically; thus the magnitude of deviation will be a function of altitude. But as the refration of light is also a function of altitude, these two magnitudes must be combined together; and therefore it may be that the deviation in its maximum would amount to several seconds [of an arc] without its being possible to identify it through observations.

These are roughly the considerations which moved me to reflect further on the perturbation of light rays, which according to my knowledge has so far been investigated by nobody.

Before undertaking the investigation itself, I will make a few more general remarks, through which the calculation will be facilitated. As at first I will determine only the maximum of such bending, I will let the light ray pass horizontally to the surface of the attracting body at the point of observation, or I assume that the star, from which the light ray comes, is apparently caught in its rising. For the sake of facility in the undertaking I assume that the light ray does not come in at the point of observation

[22] *Astronomisches Jahrbuch für das Jahr 1804* (C. F. E. Späthen, Berlin, 1801), pp. 161–172.

but leaves from there. One can easily see that this makes no difference concerning the determination of the figure of its path. Furthermore, when a light ray comes horizontally to a point on the surface of the attracting body and continues its path, which at first is again horizontal, then one will easily notice that it will in this continuation of its advance describe the same curved line which it has already followed. If one also draws a straight line through the point of observation and the center of the attracting body, then this line will become the main axis of the curved line serving for the path of the light ray, insofar as below and above that straight line there will be described two entirely congruent segments of the curved line.

Let now (Fig. 3) C be the center of the attracting body, A a point on its surface. From A let a light ray go forth in the direction AD, or horizontally, with a velocity such that it goes the distance v in a second. The light ray will, however, instead of going on in the direction AD, be forced, because of the attraction of the celestial body, to describe a curved line AMQ whose nature we shall investigate. After a time t, computed from the moment of departure from A, the light ray will find itself at M on that curved line, at a distance CM $= r$ from the center of the attracting body. Let g be the acceleration of gravity at the surface of the body. Further, let CP $= x$, MP $= y$, and the angle MCP $= \varphi$. The force with which the light ray at M will be pulled

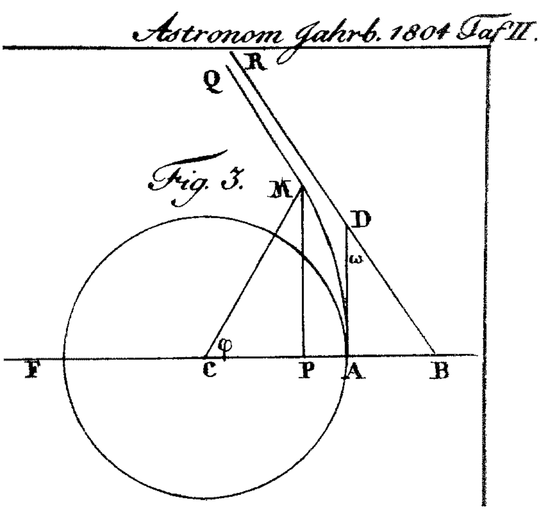

Figure 3 of Soldner's paper. (Reproduced from a photograph of a copy in the Crawford Library of the Royal Observatory of Edinburgh with permission of the Astronomer Royal for Scotland.)

by the body in the direction MC will be $2gr^{-2}$. This force can be decomposed into two others

$$\frac{2g}{r^2}\cos\varphi \qquad \text{and} \qquad \frac{2g}{r^2}\sin\varphi$$

according to the directions x and y; and therefore one obtains the following two equations (see *Traité de mécanique céleste* by Laplace, Vol. I, p. 21)

$$\frac{ddx}{dt^2} = -\frac{2g}{r^2}\cos\varphi \tag{1}$$

$$\frac{ddy}{dt^2} = -\frac{2g}{r^2}\sin\varphi \tag{2}$$

Let one now multiply the first of these equations with $-\sin\varphi$, the second with $\cos\varphi$, and add them, and thus one obtains

$$\frac{ddy\cos\varphi - ddx\sin\varphi}{dt^2} = 0 \tag{3}$$

Now let one multiply the first with $\cos\varphi$, the second with $\sin\varphi$, and add them, and thus one has

$$\frac{ddx\cos\varphi + ddy\sin\varphi}{dt^2} = -\frac{2g}{r^2} \tag{4}$$

In order to diminish in these equations the number of variable magnitudes we want to express x and y through r and φ. One easily sees that

$$x = r\cos\varphi \qquad \text{and} \qquad y = r\sin\varphi$$

One differentiates and therefore one will obtain

$$dx = \cos\varphi\, dr - r\sin\varphi\, d\varphi \qquad \text{and} \qquad dy = \sin\varphi\, dr + r\cos\varphi\, d\varphi$$

And if one once more differentiates,

$$ddx = \cos\varphi\, ddr - 2\sin\varphi\, d\varphi\, dr - r\sin\varphi\, dd\varphi - r\cos\varphi\, d\varphi^2$$

and

$$ddy = \sin\varphi\, ddr + 2\cos\varphi\, d\varphi\, dr + r\cos\varphi\, dd\varphi - r\sin\varphi\, d\varphi^2$$

By substituting these values for ddx and ddy in the above equations, one obtains from (3)

$$\frac{ddy\cos\varphi - ddx\sin\varphi}{dt^2} = \frac{2\, d\varphi\, dr + r dd\,\varphi}{dt^2}$$

One also has

$$\frac{2\, d\varphi\, dr + r dd\varphi}{dt^2} = 0 \tag{5}$$

And further from (4)

$$\frac{ddr - r d\varphi^2}{dt^2} = -\frac{2g}{r^2} \tag{6}$$

In order to turn Eq. (5) into a real differential, let one multiply it with $r\, dt$, so that

$$\frac{2r\, d\varphi\, dr + r^2 dd\varphi}{dt} = 0$$

and when one integrates again, one will obtain

$$r^2\, d\varphi = C\, dt$$

where C is an arbitrary constant. In order to determine this C, let it be noted that $r^2\, d\varphi$ $(= r \cdot r\, d\varphi)$ is equal to the double surface of the small triangle which the radius vector r in time dt has described. Twice the area of the surface of the triangle described in the first second is, however, equal to $\mathrm{AC} \cdot v$; one has $C = \mathrm{AC} \cdot v$. And if one takes the radius AC of the attracting body for unity, as we henceforth will do, then $C = v$. Let this value for C be put in the preceding equation so that

$$r^2\, d\varphi = v\, dt$$

One also has

$$d\varphi = \frac{v\, dt}{r^2} \tag{7}$$

Once this value for $d\varphi$ is put in Eq. (6), one obtains

$$\frac{ddr}{dt^2} - \frac{v^2}{r^3} = -\frac{2g}{r^2}$$

When one multiplies this equation with $2\, dr$, the result is

$$\frac{2\, dr\, ddr}{dt^2} - \frac{2v^2\, dr}{r^3} = -\frac{4g\, dr}{r^2}$$

and, if one again integrates,

$$\frac{dr^2}{dt^2} + \frac{v^2}{r^2} = \frac{4g}{r} + D$$

where D is a constant which depends on constants that are in the equation. From the equation which has just been found the time can be eliminated, because

$$dt = \frac{dr}{\sqrt{[D + (4g/r) - (v^2/r^2)]}}$$

If one puts that value for dt in Eq. (7), one has

$$d\varphi = \frac{v\,dr}{r^2\sqrt{[D + (4g/r) - (v^2/r^2)]}}$$

In order to integrate this equation, let it be brought into the form

$$d\varphi = v\,dr \Big/ \left\{ r^2 \sqrt{\left[D + \frac{4g^2}{v^2} - \left(\frac{v}{r} - \frac{2g}{v} \right)^2 \right]} \right\}$$

Now let one set

$$\frac{v}{r} - \frac{2g}{v} = z$$

so that

$$\frac{v\,dr}{r^2} = -dz$$

If this and z will be put in equation for $d\varphi$ one will have

$$d\varphi = -\frac{dz}{\sqrt{[D + (4g^2/v^2) - z^2]}}$$

From this the integral is

$$\varphi = \arccos \frac{z}{\sqrt{[D + (4g^2/v^2)]}} + \alpha$$

where α is a constant. According to familiar properties one further has

$$\cos(\varphi - \alpha) = \frac{z}{\sqrt{[D + (4g^2/v^2)]}}$$

and when one replaces z with its value

$$\cos(\varphi - \alpha) = \frac{v^2 - 2gr}{r\sqrt{[v^2 D + 4g^2]}}$$

Now $\varphi - \alpha$ is the angle which r makes with the main axis of the curved line to be determined. Since, furthermore, φ is the angle which r makes with the line AF, the axis for the coordinates x and y, α must be the angle which is formed by the main axis and the line AF. Since, however, AF goes through the point of observation and the center of the attracting body, AF must, according to the foregoing equation, be the main axis itself; also $\alpha = 0$, and therefore

$$\cos \varphi = \frac{v^2 - 2gr}{r\sqrt{[v^2 D + 4g^2]}}$$

For $\varphi = 0$, $r = \mathrm{AC} = 1$, and then one obtains from this equation

$$\sqrt{[v^2 D + 4g^2]} = v^2 - 2g$$

One substitutes this in the preceding equation, and thus the still unknown D will be eliminated, and so at the same time the root sign will also be eliminated; and one obtains

$$\cos \varphi = \frac{v^2 - 2gr}{r(v^2 - 2g)}$$

and further from this

$$r + \left[\frac{v^2 - 2g}{2g}\right] r \cos \varphi = \frac{v^2}{2g} \tag{8}$$

From this final equation between r and φ the curved line can be determined. However, to carry this out in a more commodious way we shall again reduce the equation to [rectangular] coordinates. Let (Fig. 3) AP $= x$ and MP $= y$, and one has

$$x = 1 - r \cos \varphi$$
$$y = r \sin \varphi$$
$$r = \sqrt{[(1 - x)^2 + y^2]}$$

If one puts these values into Eq. (8), one finds

$$y^2 = \frac{v^2(v^2 - 4g)}{4g^2}[1 - x]^2 - \frac{v^2(v^2 - 2g)}{2g^2}[1 - x] + \frac{v^2}{4g^2}$$

and, if one develops all that pertains,

$$y^2 = \frac{v^2}{g}x + \frac{v^2(v^2 - 4g)}{4g^2}x^2 \tag{9}$$

As this equation is of the second degree, *the curved line is a conic section*, which now can be more closely investigated.

If p is the parameter and a the half main axis, then, if one measures the abscissa from the vertex, the general equation for all conic sections is

$$y^2 = px + \frac{p}{2a} x^2$$

This equation has the properties of a parabola, if the coefficient of x^2 is zero; an ellipse, if it is negative; and a hyperbola, if it is positive. The latter is obviously the case in our equation (9). Since for all celestial bodies known to us $4g$ is smaller than v^2, the coefficient of x^2 must be positive.

> Thus when a light ray passes by a celestial body, it will, instead of going on in a straight direction, be forced by its attraction to describe a hyperbola whose concave side is directed against the attracting body.

The conditions under which a light ray would describe another conic section now can also easily be determined.

It would describe a parabola, if $4g = v^2$; an ellipse, if $4g$ is greater than v^2; and a circle, if $2g$ were equal to v^2. Since, however, we know of no celestial body whose mass would be so great that it could produce at its surface such an acceleration of gravity, a light ray describes, in the world known to us, always a hyperbola.

It now remains to be investigated how much thereby the light ray will be deviated from a straight line; or how great is the angle of perturbation, as I will call it.

As now the form of the path is known, one can again consider the light ray as coming in. And since at first I will compute only the maximum of the angle of perturbation, I will assume that the light ray comes from an infinitely great distance. The maximum [of deviation] should in this case take place, because the attracting body will work longer on the light ray, if this comes from a greater rather than from a smaller distance. Should now the light ray come here from infinitely far, then its original direction was like the asymptote BR (Fig. 3) of the hyperbola, because at infinite distance the asymptote coincides with the tangent. The light ray, however, comes in the direction DA to the eyes of the observer; thus ADB will be the angle of perturbation. If one calls this angle ω then one has, since the triangle ABD at A is a right triangle,

$$\tan \omega = \frac{\text{AB}}{\text{AD}}$$

From the nature of the hyperbola it is, however, known that AB is the half major axis and AD is the half minor axis. These magnitudes also still have to be determined. If a is the half major axis and b is the half minor axis, the parameter

$$p = \frac{2b^2}{a}$$

One substitutes this value in the general equation of the hyperbola

$$y^2 = px + \frac{p}{2a} x^2$$

which transforms itself into

$$y^2 = \frac{2b^2}{a} x + \frac{b^2}{a^2} x^2$$

If one now compares these coefficients of x and x^2 with those in (9), then one obtains the half major axis

$$a = \frac{2g}{v^2 - 4g} = \mathrm{AB}$$

and the half minor axis

$$b = \frac{v}{\sqrt{(v^2 - 4g)}} = \mathrm{AD}$$

If one puts these values for AB and AD in the expression for $\tan \omega$, then one has

$$\tan \omega = \frac{2g}{v\sqrt{(v^2 - 4g)}}$$

We shall now make of this formula an application for the earth and investigate how much a light ray will be deviated from the straight line if it passes by at the surface of the earth.

On the presupposition that light needs 564″.8 decimal seconds of time to come from the sun to the earth, one finds that it traverses in one-tenth of a second 15.562085 earth radii. Thus $v = 15.562085$. If one takes among the geographical latitudes that whose square of the sine is 1/3 (corresponding to a latitutde of 35° 16′), the earth's radius as 6,369,514 meters, and the acceleration of gravity there as 3.66394 meters (see *Traité de mécanique céleste* by Laplace, Vol. I, p. 118), then expressed in earth radii $g = 0.000000575231$. I make use of this set of units so that without special reductions I may take from the *Traité de mécanique céleste* the newest and most available determinations of the magnitude of the earth radius and of the acceleration of gravity. Thereby nothing will change concerning the final results, for here only the relation of the speed of light to the velocity of a body falling to the earth is concerned. The earth's radius and the acceleration of gravity must therefore be taken at the specified degree of latitude, because the earth-spheroid is, with respect to bodily content, similar to a globe which has for its radius the earth's radius, or 6,369,514 meters.

When one puts these values for v and g into the equation for $\tan \omega$, then one obtains, in sexagesimal seconds, $\omega = 0''.0009798$, or in round figures, $\omega = 0''.001$. As this maximum value is quite unobservable, it would be superfluous to go further; or to determine how this value decreases with height over the horizon; and by how much it becomes smaller when the distance of the star from which the light ray comes is assumed to be finite and corresponding to a given magnitude. Such is a determination that would present no difficulty.

If one were to investigate by means of the given formula how much the moon would deviate a light ray when it goes by the moon and comes to the earth, then one must, after substituting the corresponding magnitudes and taking the radius of the moon for unity, double the value found through the formula, because a light ray, which goes by the moon and comes to the earth describes two arms of a hyperbola. But regardless of this, the maximum still must come to a much smaller value than in the case of the earth, because the mass of the moon, and therefore g, is much smaller. The bending must therefore depend only on the cohesion, on the dispersion of light, and on the atmosphere of the moon; the universal attraction contributes nothing noticeable.

If one substitutes in the formula for $\tan \omega$ the acceleration of gravity on the surface of the sun, and one takes the radius of that body for unity, then one finds $\omega = 0''.84$. If one could observe the fixed stars very close to the sun, then one would have to take this very much into account. But since this is not known to happen, the perturbation caused by the sun can also be neglected. For light rays which come from Venus, a star which Vidal [now] observes only two minutes [of an arc] away from the edge of the sun (see Herr Lt. von Zach's *Monatliche Correspondenz*, Vol. II, p. 87), the perturbation is much smaller, because the distances of Venus and of the earth from the sun cannot be taken to be infinitely great.

Through the combination of several bodies which a light ray could encounter on its way the results would be somewhat larger, but for our observations still certainly unnoticeable.

Therefore it is clear that nothing makes it necessary, at least in the present state of practical astronomy, that one should take into account the perturbation of light rays by attracting celestial bodies.

I must now face still a couple of objections which one can perhaps make to me.

It may be remarked that I have deviated from the usual procedures in that I have, already before making the calculations, assumed some general properties of curved lines, which usually happens only through [first] making them and should have been done here as well. But the computation is much shortened thereby; and why should one calculate where the point to be proven can be made much more evident through a small reflection.

Hopefully, no one would find it objectionable that I treat a light ray as a heavy body. That light rays have all the absolute [basic] properties of matter one can see from the phenomenon of aberration which is possible only because light rays are truly material. And furthermore, one cannot think of a thing which exists and works on our senses that would not have the property of matter.

> Besides, there is nothing which you can call distinct from body and separate from void to be discovered as a kind of third nature.
>
> Lucretius: *On the Nature of Things*, I, 431.

At any rate, I do not believe that there is any need on my part to apologize for having published the present essay just because the result is that all perturbations are unobservable. For it would still be just as important for us to know what is presented by theory, though it has no noticeable influence on praxis, as we are interested in what has in retrospect real influence on it. Our insights would by both be equally enlarged. One also demonstrates, for instance, that the daily aberration, the disturbance of the rotation of the earth, and other similar things are unobservable.

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