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# Eclipses of the Stars – Mandl, Einstein, and the Early History of Gravitational Lensing

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# ECLIPSES OF THE STARS

## MANDL, EINSTEIN, AND THE EARLY HISTORY OF GRAVITATIONAL LENSING

Jürgen Renn and Tilman Sauer

Aber rühmen wir nicht nur den Weisen
Dessen Namen auf dem Buche prangt!
Denn man muß dem Weisen seine Weisheit erst entreißen.
Darum sei der Zöllner auch bedankt:
Er hat sie ihm abverlangt.

But the honour should not be restricted
To the sage whose name is clearly writ.
For a wise man's wisdom needs to be extracted.
So the customs man deserves his bit.
It was he who called for it.

Bertold Brecht, Legende von der Entstehung des Buches
Taoteking auf dem Weg des Laotse in die Emigration
(Legend of the origin of the book Tao-te-ching
on Lao-tsu's road into exile)

This paper is about an odd but characteristic episode in Einstein's life, presenting him as an egalitarian intellectual, supportive of an outsider to the scientific establishment, unprejudiced and open to good ideas however humble their source may be, some ambivalence notwithstanding. It shows how one such humble idea eventually became a great scientific achievement—after much resistance and reluctance due to elitist attitudes towards science. It is a story about imagination and individual generosity, but also about science as a social enterprise and the role of contingency in its development. The paper continues an investigation begun jointly with John Stachel and is dedicated to him in gratitude for all he has taught us about these issues.

## An Amateur's Idea

Some day in spring 1936, Rudi W. Mandl, a Czech amateur scientist, walked into the building of the National Academy of Sciences in Washington and asked for the offices of the Science Service, an institution devoted to the popularization of science. He came with a new idea of his, concerning a "proposed test for the relativity theory based on observations during eclipses of the stars."[1] He was looking for someone to help him publish his ideas and persuade professional astronomers to take up investigations along his proposal. What to do with him? His intentions seemed sincere, his ideas not so easy to be refuted, and the man himself not easily to be dismissed either. After a while, somebody from the staff of the Science Service suggested to have Mandl discuss his ideas with an undisputed expert on relativity theory. Princeton was not too far away, so he might go there for a day and talk to Professor Einstein himself about his proposed test. They would give him some money for the trip, and if Einstein found his ideas worthwhile he might come back and they would see what they could do for him.

What Mandl was going to present to Einstein was, as we know from correspondence and manuscripts surviving in the Einstein Archives, a queer combination of ideas from general relativity, optics, astrophysics, and evolutionary biology. There is evidence that Mandl rather obsessively attempted to persuade professional scientists of his ideas, among them William Francis Gray Swann, director of a center of cosmic ray studies, the Nobel prize winners Arthur Holly Compton and Robert Andrews Millikan, and V. K. Zworykin, research scientist at the Radio Corporation of America (RCA) and inventor of the first all-electronic television system.[2] Some of them reacted with interest and gave Mandl's ideas some brief consideration, others excused themselves with lack of time or understanding. None of them, in any case, pursued the matter seriously.

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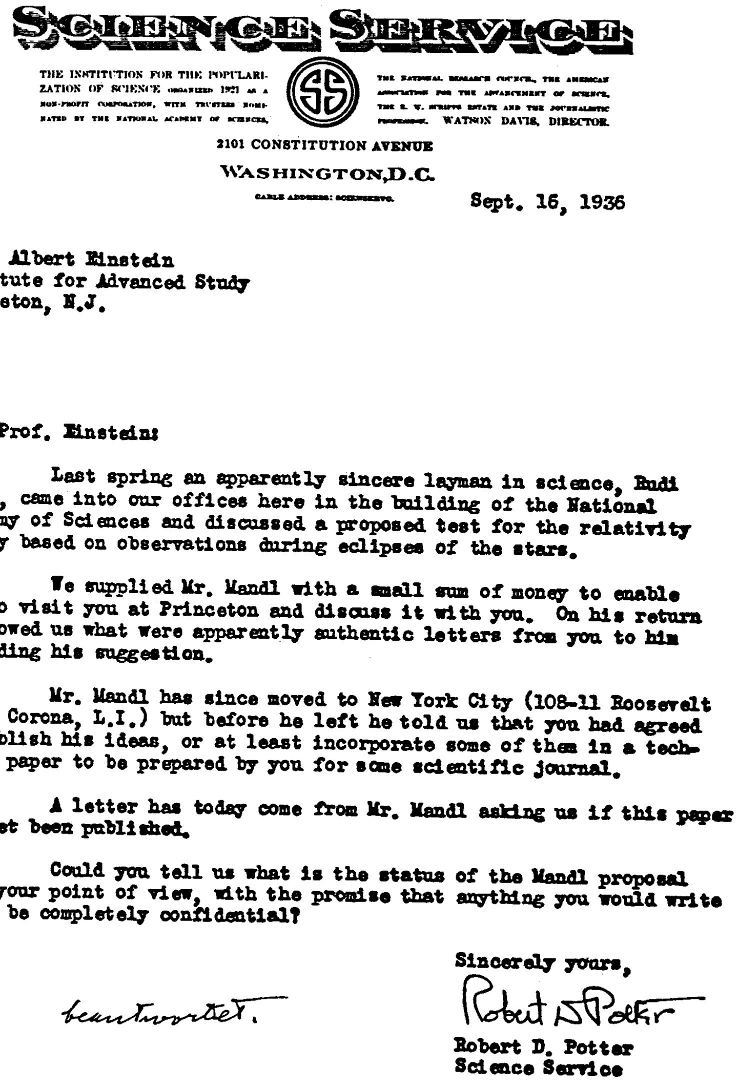
[1] R. D. Potter to A.Einstein, 16 September 1936, EA17039. Cp. Fig. 1.
[2] See Mandl to Einstein, 3 May 1936, EA17031, and *Zwicky 1937a*, p.290.



SCIENCE SERVICE

THE INSTITUTION FOR THE POPULARIZATION OF SCIENCE ORGANIZED 1921 AS A NON-PROFIT CORPORATION, WITH TRUSTEES NOMINATED BY THE NATIONAL ACADEMY OF SCIENCES,

WATSON DAVIS, DIRECTOR.

2101 CONSTITUTION AVENUE
WASHINGTON, D.C.

Sept. 16, 1936

Prof. Albert Einstein
Institute for Advanced Study
Princeton, N.J.

Dear Prof. Einstein:

Last spring an apparently sincere layman in science, Rudi Mandl, came into our offices here in the building of the National Academy of Sciences and discussed a proposed test for the relativity theory based on observations during eclipses of the stars.

We supplied Mr. Mandl with a small sum of money to enable him to visit you at Princeton and discuss it with you. On his return he showed us what were apparently authentic letters from you to him regarding his suggestion.

Mr. Mandl has since moved to New York City (108-11 Roosevelt Ave., Corona, L.I.) but before he left he told us that you had agreed to publish his ideas, or at least incorporate some of them in a technical paper to be prepared by you for some scientific journal.

A letter has today come from Mr. Mandl asking us if this paper has yet been published.

Could you tell us what is the status of the Mandl proposal from your point of view, with the promise that anything you would write would be completely confidential?

Sincerely yours,

beantwortet.

Robert D. Potter
Science Service

**Figure 1:** R. D. Potter to A. Einstein, 16 Sept. 1936, EA 17039, © Einstein Archives, The Hebrew University Jerusalem.

When Mandl visited Einstein in Princeton on April 17, 1936, he found the professor friendly and willing to listen to his ideas in spite of their oddity. The core of Mandl's suggestion was in fact simple, it essentially amounts to the combination of an elementary insight from general relativity, the deflection of light rays by a gravitational field, with the lensing effect familiar from ray optics. Mandl proposed a simple model according to which one star focalizes the light of another star if both are aligned with the earth, thus constituting a gravitational lens and its object. He speculated that the effects of such a focalization might already have been observed, though their origin had remained undisclosed. Among the possible effects that Mandl took into consideration were the recently discovered annular shaped nebulae which he interpreted as gravitational images of distant stars, cosmic radiation which he conjectured to be an effect of the gravitational amplification of the radiation emitted by a distant galaxy, and the sudden extinction of biological species such as dinosaurs, which he attempted to relate to the momentary intensification of such radiation due to what he described as a stellar eclipse.

Though the range of Mandl's ideas was daring, at its core was an insight that would eventually–several decades later–indeed turn into an astrophysical confirmation of relativity theory. From a letter written a day after Mandl's visit to Einstein, we can gather what that idea was. (Consider the "old formula" and the corresponding sketch in the diagram that Mandl included in his letter, depicted in Fig. 2.)

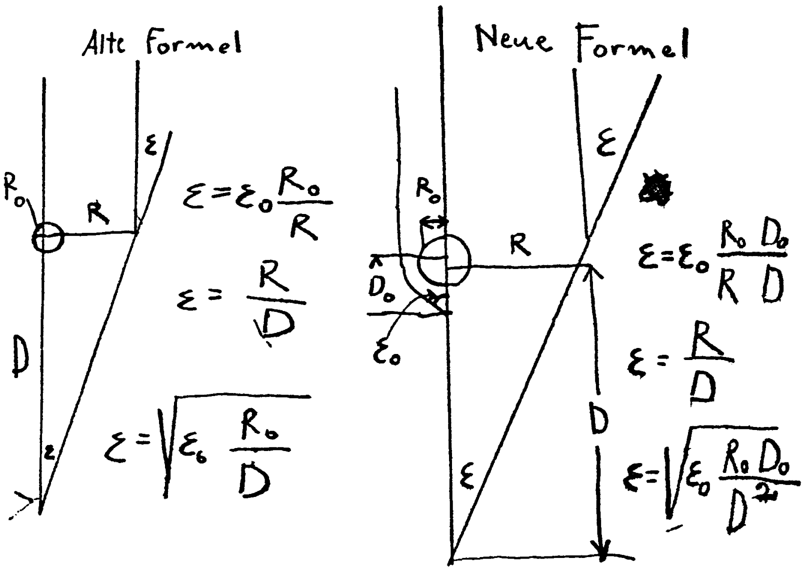



**Figure 2:** Sketch by Mandl. EA 17-028, © Einstein Archives, The Hebrew University Jerusalem

Light coming from an infinitely distant star, located exactly behind a massive, gravitating star of spherical radius $R_0$, located at distance $D$ from a terrestrial observer, passes at a distance $R$ from the center of that gravitating object. The light ray is bent inwards, i.e. towards the line between the observer and the gravitating star by gravitational deflection and is seen under an angle $\varepsilon$ by the terrestrial observer. According to general relativity, the angle of deflection $\varepsilon$ is inversely proportional to the offset $R$, i.e. $\varepsilon \sim \frac{1}{R}$. Let $\varepsilon_0$ be the angle of deflection for light rays just grazing the edge of the deflecting star, i.e. for $R = R_0$.[3] Since $\varepsilon_0 \sim \frac{1}{R_0}$, the angle of deflection in general would be $\varepsilon = \varepsilon_0 \frac{R_0}{R}$. Light rays visible on the earth, on the other hand, can be seen for angles $\varepsilon = \frac{R}{D}$. And from the latter two formulae the relation

[3] Strictly speaking, only a determination of the numerical value of $\varepsilon_0$ would provide a test of General Relativity since the $\frac{1}{R}$-dependence follows already from the equivalence hypothesis alone and was, in fact, already derived in Einstein 1911.

$$\varepsilon = \sqrt{\varepsilon_0 \frac{R_0}{D}} \qquad (1)$$

follows.

Clearly, what Mandl and Einstein had been discussing was some analogue of an optical lens, effected by the light deflection of the gravitating star. The angle $\varepsilon$ then would be the apparent magnitude of the distant star, as seen from the observer. But in contrast to the lensing properties of common optical lenses, gravitational light deflection by a massive star does not collect parallel light rays in one single focal point but rather smeared out along a "focal line" as it were. For an observer at the distance $D$ the condition holds only for light rays passing at a particular distance $R$, resulting in what we would now call an "Einstein ring." In any case, Mandl's idea seems to have been that this lensing effect would result in a considerable magnification of the star's light at the point of the terrestrial observer.

To Mandl the opportunity of a conversation with Einstein must have meant a lot. "Inexcusably" he had forgotten to express his thankfulness for the "friendly reception" and hastened to make up for this mistake by a first letter written a day after his visit. In this letter he also pointed out what he believed was an error in their considerations. He proposed a correction of the usual light deflection formula, now assuming without any further explanation that the angle of deflection would have to be taken in general as $\varepsilon = \frac{R_0 D_0}{RD}$ with $D_0$ denoting some fixed distance of the lensing star.[4]

In the course of his meeting with Mandl Einstein let himself apparently be drawn into the matter by Mandl's enthusiasm and even entertained the possibility of publishing something about the idea. After some days of reflection, however, skepticism and caution prevailed so that Einstein changed his mind and wrote to Mandl:

> The first formula stays. The second one is based on an erroneous consideration. I have come to the conclusion that the phenomenon in question will, after all, not be observable so that I am no longer in favor of publishing anything about it. [5]

Mandl, on the other hand, relentlessly pursued his idea and, in a letter written the same day and crossing that of Einstein, reported about further progress, addressing precisely the issue of Einstein's concern, the difficulty of empirically checking the phenomenon:

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[4] Mandl to Einstein, 18 April 1936, EA 17027/28.

[5] "Es bleibt bei der ersten Formel. Die zweite beruht auf einer falschen Überlegung. Ich habe mir überlegt, dass das fragliche Phänomen doch nicht beobachtbar sein wird, sodass ich nicht mehr dafür bin, etwas darüber zu publizieren." Einstein to Mandl, 23 April 1936, EA 17030.

> Meanwhile I have found a method to measure the intensity increase in the domain of the focal line of a star and to confirm it experimentally. It would be, according to my view, in the interest of science to begin with these experiments as soon as possible. [6]

Mandl also asked for another meeting with Einstein, insisting that his idea would provide a very simple explanation of the origin of cosmic radiation. Ten days later, Mandl wrote once more. He had received Einstein's letter in the meantime and agreed that his second formula was wrong. Mandl emphasized that he did not want to persuade Einstein into some publication against his will but was, at the same time, well aware that he needed the latter's authority for gaining recognition for his idea. He attempted to win Einstein for his publication plan by appealing to the need of defeating pseudo-science:

> The main reason why I bother you in order to gain your e[steemed] collaboration is that immediately after the publication of my investigations all astrologists and similar parasites of science will take possession of the results of my considerations, and it is my conviction that it would be of use for the "average citizen" if, from the very beginning, a man of your rank and fame would emphasize the nonsense of the pseudo-science of these charlatans.[7]

Ironically some of the ideas Mandl expounded in a four-page typescript enclosed with his letter must have sounded to Einstein very much like the pseudo-science Mandl pretended to fight. It contains a sober assessment of the observability of a stellar eclipse but also daring speculations about its possible effects on life on earth. He asked that Einstein read this manuscript and kindly point out fallacies if he would find any.

Mandl's typescript[8] was never published. It started with the computation of some numerical values of the distance $D$ of the "focal point" and of the apparent diameter $\varepsilon$ of the ring image of the distant star as a function of the offset $R$ both for the case of a lensing star of the mass of the sun and for a lensing star of 100 solar masses. In the following three pages, Mandl explained his idea of an "Einstein focal line" ("E. F. L.", Mandl also asked Einstein for permission to give the phenomenon his name) in somewhat more detail. Regarding its observability, he remarked:

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6 "Ich habe in der Zwischenzeit eine Methode gefunden die Intensitaetssteigerung im Bereich der Focuslinie eines Sternes zu messen und experimentell bestaetigen zu koennen.
Es waere meiner Ansicht nach im Interesse der Wissenschaft mit diesen Versuchen alsbald als moeglich zu beginnen." Mandl to Einstein, 23 April 1936, EA 17029.

7 "Der Hauptgrund warum ich Sie um Ihre w. Mitarbeit zu gewinnen belaestige ist dass unmittelbar nach Veroeffentlichung meiner Untersuchungen alle Astrologen und aehnliche Parasiten der Wissenschaft sich der Resultate meiner Ueberlegungen bemaechtigen werden und es ist meine Ueberzeugung dass es von Nutzen fuer den "Average citizen" waere wenn von vornherein ein Mann Ihren Ranges und Rufes denn Unnsinn der Pseudowissenschaft dieser Charlatane untersteichen wuerde." Mandl to Einstein, 3 May 1936, EA 17031.

8 EA 17032.

> Given the extraordinarily small surface which the fixed stars cover in comparison with the surface of the sky, the transition of the earth through an E. F. L. [of a fixed star] is a very rare phenomenon and would be best compared, regarding both time and perhaps also extension, with a solar eclipse, a transition through an E. F. L. [of a galaxy], however, would be something ordinary and should even last over millenia. [9]

Mandl added in parentheses:

> (The relatively large quantity of cosmic radiation would be most easily explained by a transition through an E. F. L. of the milky way or other nebulae.)[10]

With the astronomical details explained, the remainder of the typescript discusses the surmised spectacular terrestrial consequences of this focal effect:

> In the sequel some general observations on Darwinism. The greatest unclearness and uncertainty in the Darwinian doctrine of evolution consists in the insufficiency of the explanation of the cause why evolution proceeds not gradually but by leaps and bounds and that after periods of intensive evolution and mutation a long period of stagnation followed and why all-of-a-sudden entire classes of highly-developed animals went extinct. [11]

With reference to H.J. Muller's 1927 experiments[12] on drosophila mutation after exposure to X-ray radiation, Mandl goes on to conjecture that the supposedly unexplained origin of discontinuous evolutionary steps in Darwin's theory might be due just to the gravitational focussing of distant cosmic light. He concluded:

> The theory of relativity thus delivers a key, as absurd as it may appear in the first moment, to the hitherto dark parts of the evolutionary doctrine.[13]

At the end of his typescript, Mandl disputed any vicinity of his considerations to a pseudo-science such as astrology. As a matter of fact, his final remarks represent, however, just the kind of disclaimer which raises rather than refutes suspicions:

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[9] "Bei der auserordentlich geringen Oberfläche den die Fixsterne im Vergleich zu der Himmelsoberflaeche einnehmen ist der Durchgang der Erde durch eine E. F. L. [eines Fixsterns] ein sehr seltenes Phaenomen und waere im Bezug auf Zeit und vielleicht auch in Ausdehnung am besten mit einer Sonnenfinsternis zu vergleichen, ein Durchgang jedoch durch ein E. F. L. [einer Galaxie] waere etwas alltaegliches und duerfte sogar Jahrtausende andauern." EA 17032.

[10] "(Die verhaeltnismaessig grosse Quantitaet der Cosmischen Strahlung waere am leichtesten durch einen Durchgang durch eine E. F. L. der Milchstrasse und anderer Nebel zu erklaeren.)" EA 17032.

[11] "Nachstehend einige allgemeine Bemerkungen ueber den Darvinismus.
Die groesste Unklarheit und Ungewissheit in der Darvinischer Evolutionslehre besteht in der Unzulaenglichkeit der Erklaerung der Ursach warum die Evolution nich graduell sondern sprunghaft vor sich ging und dass nach Perioden einer intensiven Evolution und Mutation eine lange Periode von Stillstand kam und warum ploetzlich ganze Klassen von hoch entwickelten Tieren ausstarben." EA 17032.

[12] Mandl's memorandum erroneously refers to "Prof. J.P.Mueller von der University of Texas" but this is clearly a slip.

[13] "Die Relativitaets-Theorie liefert also so wiedersinnig es im ersten Mommente erscheinen mag einen Schluessel zu den bisher dunklen Teilen der Evolutionslehre." EA 17032.

> While the above consideration teaches us that the stars though being far away "are responsible" for the evolution or have brought it about, it has to be emphasized at this point that the "science"?? of astrology has nothing in common with the above considerations and that we are unable, even with our astronomical instruments of today, to reconstruct even merely by calculation which stars come into consideration for having influenced our earth in the sense pointed out above. [14]

Reading Mandl's memoir, it does not come as a surprise that, as Mandl reported in his covering letter, other physicists whom he had meanwhile approached about his theories reacted with caution or even disdain. While Swann and Compton excused themselves for not entering into the matter for lack of time, and asked that Mandl send them more details, Millikan had just given him short shrift with three brief "I don't understand."[15]

Although Einstein clearly shared the aversion of his established colleagues to the over-ambitious projects of an amateur-scientist with the air of a crackpot, he did, as we shall see, take the matter less lightly, not the least because Mandl would not let him easily "off the hook." Indeed, before Einstein even had a chance of responding to Mandl's memoir, he received yet another letter by Mandl, dated May 9. In it he argued against Einstein's claim that the phenomenon would be unobservable, pointed out that there would be a numerical error in his table, and expressed his hope to hear from Einstein soon. Three days later Einstein gave in to Mandl's bugging and finally wrote him. He had actually sat down to work on Mandl's ideas:

> Dear Mr. Mandl: I have calculated your intensification effect more precisely. The result is the following.[16]

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[14] "Wenn auch obige Ueberlegung uns lehrt dass die Sterne moegen selbe noch so ferne stehen fuer die Evolution 'verantwortlich sind' oder sie zustande gebracht haben muss an dieser Stelle hervorgehoben werden das die 'Wissenschaft'?? der Astrologie mit obigen Ueberlegungen nichts gemein hat und dass wir selbst mit unseren heutigen Astronomischen Instr nicht im stande sind auch nur zurueckzuberechnen welche Sterne fuer unsere Erde in Betracht kommen diese im oben dargelegten Sinne beeinflust zu haben." EA 17032.

[15] "I found, during the last days, occasion to discuss the consideration with Dr. Compton, Dr. Swann and Dr. Millikan, and the first two were interested in the matter and asked me (because of lack of time) for further information. Dr. Millikan, however, gave me short shrift with three brief 'I don't understand.' ("Ich fand Gelegenheit waehrend der letzten Tage mit Dr. Compton, Dr. Swann und Dr. Millikan die Ueberlegung zu besprechen und die beiden erstgenannten interessierten sich fuer die Sache und ersuchten mich um (des Zeitmangels wegen) naehere Angaben Dr. Millikan jedoch fertigte mich mit drei kurzen 'I dont understand' ab." (Mandl to Einstein, 3 May 1936, EA 17031). The reference to Compton is probably to Arthur Holly Compton, then professor of physics at the University of Chicago, winner of the 1927 Nobel prize for the discovery of the "Compton-effect," who had changed his main research interests from X-rays to cosmic rays in the early 1930s which is probably why Mandl approached him. Mandl may, however, also refer to Arthur's brother Karl Taylor Compton who had been chair of the physics department at Princeton University for many years and was, since 1930, president of the Massachussetts Institute of Technology. In 1935-36 Karl Taylor Compton presided the American Association for the Advancement of Science. William Francis Gray Swann (1884-1962) since 1927 was director of the Bartol Research Foundation of the Franklin Institute, a center of cosmic ray studies, and author of the highly successful book *The Architecture of the Universe* (1934). Robert Andrews Millikan (1868-1953) was director of the Norman Bridge Laboratory at the California Institute of Technology in Pasadena, and had done extensive experimental research on cosmic rays. Believing for many years that cosmic rays were photons originating in processes of nuclear fusion (the "birth cries" of atoms), Millikan had to concede in the early 30's that some percentage of cosmic radiation consisted of charged particles, and by 1935 he also rejected his atom-building hypothesis. Millikan had won the Nobel prize in 1923.

Denoting the distance between an observer and a light-bending star ("ablenkender Stern") $D$, the radius of the bending star $r_0$, the amount of deflection of a light ray grazing the surface of the bending star $\alpha_0$, the (vertical) distance of the observer from the line passing through the centers of the emitting and the light-bending star $x$, and introducing the quantity $l^2 = D\alpha_0 r_0$, Einstein reported the following expression for the intensification $G$:

$$G = \frac{l}{x} \frac{1 + \frac{x^2}{2l^2}}{\sqrt{1 + \frac{x^2}{4l^2}}}. \tag{2}$$

He remarked that a noteworthy intensification would take place if $x$ would be small against $l$, in which case one would have approximately $G = \frac{l}{x}$ and hence infinite intensity on the central line itself. For a nearby fixed star $l$ would be roughly 10 light seconds, and hence the intensification would be restricted to a very small zone. For an intensification of factor 10, that zone would be one light second, or roughly equal to the diameter of the sun. Einstein also pointed out that, strangely, $l$ would grow like $\sqrt{D}$, and hence the effect of a distant star would be larger than that of one close by.

These dazzling results must have been a great satisfaction to Mandl. Regarding the observability of the phenomenon, Einstein now showed himself less pessimistic than in his first letter. Would he agree to some kind of joint publication? But although his calculations had opened up more promising perspectives for the observability of the phenomenon, Einstein, in the end, nevertheless came to a negative assessment and concluded:

> In any case there may well be more chance to occasionally observe this intensification effect than the "halo effect" with which we have dealt earlier. But the probability that we can get so precisely into the connecting line of the centers of two stars at very different distances is rather low, even lower the probability that the phenomenon, lasting in general only a few hours, happens to be observed.[17]

Even more than the problems of observability Einstein must have felt uneasy about the wild speculations Mandl had associated with his idea. But in spite of these hesitations and in contrast to the reactions of other colleagues who had dealt with Mandl's idea, he finally faced the possibility of a "modest publication:"

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16 "Sehr geehrter Herr Mandl: Ich habe Ihren Verstaerkungseffekt genauer ausgerechnet. Folgendes ist das Resultat." Einstein to Mandl, 12 May 1936, EA 17034/35.

17 "Immerhin ist wohl mehr Chance vorhanden, diesen Verstärkungseffekt gelegentlich einmal zu beobachten als den "Hof-Effekt", von dem wir früher gehandelt haben. Aber die Wahrscheinlichkeit, dass wir so genau in die Verbindungslinie der Mittelpunkte zweier sehr verschieden entfernten Sterne hineinkommen ist recht gering, noch geringer die Wahrscheinlichkeit, dass das im Allgemeinen nur wenige Stunden währende Phänomen zur Beobachtung gelangt." Einstein to Mandl, 12 May 1936, EA 17034,35.

> Your fantastic speculations associated with the phenomenon would only make you the laughing-stock of the reasonable astronomers. I warn you in your own interest against such a publication. On the other hand, one cannot object against a modest publication of a derivation of the two characteristic formulas for the "halo effect" and the "intensification effect."[18]

Einstein's letter triggered a series of enthusiastic epistles by Mandl. On May 17, he wrote a lengthy letter, thanked Einstein for his response, and repeated his request "to publish the results of the effects as well as your formulas as I am really lacking any possibility to do so."[19] Two days later, he apologized for having written in a rather confused way but he was sure:

> that it [the letter] will bring you great joy, a joy that will only be smaller by a bit than the joy which our common friend Hitler will have once he finds out that it was again one of those damned Jews who turned the Einstein theory into the Einstein law.[20]

While, for all we know, Hitler could not have cared less, an empirical confirmation of general relativity achieved by an unknown Jewish amateur scientist would not only have surprised German scientists of the period who, like Max Planck on the occasion of his meeting with Hitler in May 1933, distinguished between valuable and less valuable Jews.[21] That a non-professional could contribute to a physical theory of the complexity of general relativity was hardly conceivable also to the American academic establishment. In fact, Mandl's efforts to find a channel of publication for his views remained in vain. Two days after the letter quoted above, on 21 May, he turned once more to Einstein, this time in a rather discouraged vein:

> In the last days I undertook desperate attempts to publish the results of my researches together with your formulas and I heard, without exception, the question: 'Well, if Mr. Einstein appreciates your results why doesn't he publish them himself?' Hence it seems to depend on you to make the results accessible to the scientific world.[22]

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[18] "Ihre an das Phänomen geknüpften phantastischen Spekulationen würden Ihnen nur den Spott der vernünftigen Astronomen eintragen. Ich warne Sie in ihrem eigenen Interesse vor einer derartigen Veröffentlichung. Dagegen ist gegen eine bescheidene Publikation einer Ableitung der beiden charakteristischen Formeln für den 'Hof-Effekt' und den 'Verstärkungs-Effekt' nichts einzuwenden." Einstein to Mandl, 12 May 1936, EA 17034/35.

[19] "die Resultate der Effecte sowie Ihre Formeln zu veroeffentlichen, da mir ja jede Moeglichkeit dazu fehlt." Mandl to Einstein, 17 May 1936, EA 17036.

[20] "das selber [Brief] Ihnen eine grosse Freude machen wird eine Freude die nur um ein klein wenig kleiner wird als die Freude die unser gemeinsamer Freund Hitler haben wird wenn er herausfindet dass es wieder einer der verdammten Juden war der die Einstein Theorie in das Einstein Gesetz verwandelte." Mandl to Einstein, 19 May 1936, EA 17037.

[21] See *Planck 1947*, p. 143, and, for historical discussion, *Albrecht 1993*.

[22] "Ich machte in den letzten Tagen verzweifelte Versuche, die Resultate meiner Forschungen mit Ihren Formeln zu veroeffentlichen und hoerte ausnahmslos die Frage 'Ja wenn Herr Einstein ihre Resultate gut befindet warum veoeffentlicht er sie nicht selber?' So es scheint dass an Ihnen liegt die Resultate der wissenschaftlichen Welt zugaenglich zu machen." Mandl to Einstein, 21 May 1936, EA 17038.

In an environment in which participation in the dissemination of information depended heavily on academic status, an outsider had indeed little chance to place his ideas and needed, like an intellectual in the early modern period, the grace, benevolence, and authority of some patron to be admitted to the official world of learning. The patron in turn, inevitably took some responsibility for the actions of his protégé. When Mandl's last letter confronted Einstein with this choice, he seems to have shied away from it. Apparently, he was reluctant to take over the responsibility even for the modest publication he himself had earlier suggested. All we have is draft notes for a letter to Mandl, dated June 2, 1936, with the derivation of his aforementioned result.[23] The letter itself is lost but it may have contained a vague promise of publishing these results in some form or other. For the time being, in any case, Einstein seems to have laid the matter to rest with that.

Because of the involvement of the Science Service, however, the affair had taken an administrative turn and was no longer just a question between two individuals. This is why three months later, Mandl could appeal to the Science Service asking what had become of Einstein's promise, a question that Robert D. Potter from Science Service duly passed on to Einstein without delay:

> he told us that you had agreed to publish his ideas, or at least incorporate some of them in a technical paper to be prepared by you for some scientific journal. [...] Could you tell us what is the status of the Mandl proposal from your point of view, with the promise that anything you would write would be completely confidential?[24]

Whether such a confidential answer by Einstein to this question was ever written is unclear. Mandl's appeal had, in any case, the desired effect: Einstein complied with his wish and submitted a short note to *Science*. The note is entitled "Lens-like Action of a Star by the Deviation of Light in the Gravitational Field" and was published in the December 4 issue of the journal,[25] cp. Fig. 3. It has since become the classical starting point for the officially recorded history of gravitational lensing research.[26]

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[23] EA 3-011-55; see *Renn, Sauer, Stachel 1997*, p. 186, for a facsimile.
[24] R. D. Potter to A. Einstein, 16 Sept. 1936, EA 17-039, cp. Fig. 1.
[25] *Einstein 1936*.
[26] For more explicit historical accounts of gravitational lensing, see, e.g., *Barnothy 1989, Schneider, Ehlers, and Falco 1992*, pp. sec. 1.1.

## DISCUSSION

### LENS-LIKE ACTION OF A STAR BY THE DEVIATION OF LIGHT IN THE GRAVITATIONAL FIELD

SOME time ago, R. W. Mandl paid me a visit and asked me to publish the results of a little calculation, which I had made at his request. This note complies with his wish.

The light coming from a star $A$ traverses the gravitational field of another star $B$, whose radius is $R_o$. Let there be an observer at a distance $D$ from $B$ and at a distance $x$, small compared with $D$, from the extended central line $\overline{AB}$. According to the general theory of relativity, let $\alpha_o$ be the deviation of the light ray passing the star $B$ at a distance $R_o$ from its center.

For the sake of simplicity, let us assume that $\overline{AB}$ is large, compared with the distance $D$ of the observer from the deviating star $B$. We also neglect the eclipse (geometrical obscuration) by the star $B$, which indeed is negligible in all practically important cases. To permit this, $D$ has to be very large compared to the radius $R_o$ of the deviating star.

It follows from the law of deviation that an observer situated exactly on the extension of the central line $\overline{AB}$ will perceive, instead of a point-like star $A$, a luminous circle of the angular radius $\beta$ around the center of $B$, where

$$\beta = \sqrt{\alpha_o \frac{R_o}{D}}.$$

It should be noted that this angular diameter $\beta$ does not decrease like $1/D$, but like $1/\sqrt{D}$, as the distance $D$ increases.

Of course, there is no hope of observing this phenomenon directly. First, we shall scarcely ever approach closely enough to such a central line. Second, the angle $\beta$ will defy the resolving power of our instruments. For, $\alpha_o$ being of the order of magnitude of one second of arc, the angle $R_o/D$, under which the deviating star $B$ is seen, is much smaller. Therefore, the light coming from the luminous circle can not be distinguished by an observer as geometrically different from that coming from the star $B$, but simply will manifest itself as increased apparent brightness of $B$.

The same will happen, if the observer is situated at a small distance $x$ from the extended central line $\overline{AB}$. But then the observer will see $A$ as two point-like light-sources, which are deviated from the true geometrical position of $A$ by the angle $\beta$, approximately.

The apparent brightness of $A$ will be increased by the lens-like action of the gravitational field of $B$ in the ratio $q$. This $q$ will be considerably larger than unity only if $x$ is so small that the observed positions of $A$ and $B$ coincide, within the resolving power of our instruments. Simple geometric considerations lead to the expression

$$q = \frac{l}{x} \cdot \frac{1 + \frac{x^2}{2l^2}}{\sqrt{1 + \frac{x^2}{4l^2}}},$$

where

$$l = \sqrt{\alpha_o D R_o}.$$

DECEMBER 4, 1936

If we are interested mainly in the case $q \gg 1$, the formula

$$q = \frac{l}{x}$$

is a sufficient approximation, since $\frac{x^2}{l^2}$ may be neglected.

Even in the most favorable cases the length $l$ is only a few light-seconds, and $x$ must be small compared with this, if an appreciable increase of the apparent brightness of $A$ is to be produced by the lens-like action of $B$.

Therefore, there is no great chance of observing this phenomenon, even if dazzling by the light of the much nearer star $B$ is disregarded. This apparent amplification of $q$ by the lens-like action of the star $B$ is a most curious effect, not so much for its becoming infinite, with $x$ vanishing, but since with increasing distance $D$ of the observer not only does it not decrease, but even increases proportionally to $\sqrt{D}$.

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**Figure 3:** Einstein's note in Science, presenting "the results of a little calculation" he had made at Mandl's request.

How much self-conquest it must have taken Einstein to overcome his reluctance and publish this note is evident from its introductory sentence, as well as from a letter Einstein wrote to Professor J. McKeen Cattell, Editor of *Science*, on December 18, two weeks after the publication of Einstein's paper. The opening statement of the published note reads:

> Some time ago, R. W. Mandl paid me a visit and asked me to publish the results of a little calculation which I had made at his request. This note complies with his wish.[27]

This mention of Mandl reads less like giving due credit to the proprietor of a good idea than like a general disclaimer. In his letter to the editor of *Science*, Einstein distanced himself even more definitively from his publication, stressing that it was only written to appease Mandl:

> Let me also thank you for your cooperation with the little publication, which Mr. Mandl squeezed out of me. It is of little value, but it makes the poor guy happy.[28]

Einstein could apparently recognize no value in the theoretical analysis of what must have appeared to him as a "science-fiction effect" because gravitational lensing seemed so far out of the reach of any observational verification. Thus, his note ends with the remark that:

> there is no great chance of observing this phenomenon, even if dazzling by the light of the much nearer star $B$ is disregarded.[29]

Einstein concludes with what reads like an attempt at a justification for yet publishing his "little calculation:"

> This apparent amplification of $q$ by the lens-like action of the star $B$ is a most curious effect, not so much for its becoming infinite, with $x$ vanishing, but since with increasing distance $D$ of the observer not only does it not decrease, but even increases proportionally to $\sqrt{D}$.[30]

Evidently, it was, at least in Einstein's understanding, hardly legitimate for theoretical physics to decouple itself from experimental physics or observational astronomy to this extent and pursue theoretical consequences in a merely speculative way, even if they pointed to what he described as "a most curious effect." In fact, such consequences might be empirically substantiated only in some indefinite future, while there was, on the other hand, the plentitude of known phenomena still awaiting a satisfactory interpretation. Nevertheless, in spite of such hesitations, Einstein did in the end decide to publish his note.

## A Deja Vu

Why did Einstein not simply dismiss Mandl as Prof. Millikan had done? What was it that eventually tipped the scales? Do we have to assume that it was, after all, Einstein's "physical instinct," some subconscious capability of foreseeing the eventual success of Mandl's idea? One

[27] *Einstein 1936*, p. 507.
[28] "Ich danke Ihnen noch sehr für das Entgegenkommen bei der kleinen Publikation, die Herr Mandl aus mir herauspresste. Sie ist wenig wert, aber diese arme Kerl hat seine Freude davon." Einstein to J. McKeen Cattell, 18 December 1936, EA65-603.
[29] *Einstein 1936*, p. 508.
[30] *Einstein 1936*, p. 508.

element of a more plausible answer is found in Einstein's above-mentioned letter to the editor of *Science*, James McKeen Cattell, formerly professor of psychology at Columbia University. This letter shows a political awareness on the part of both correspondents that provides a relevant context also to their support of Mandl:

> I know quite well that you had to leave Columbia. Researchers are here treated as are elsewhere waiters or salesmen in the retail business (The latter also merit, of course, to work under better secured conditions, this expression hides no class arrogance); presently it is particularly bad if somebody is considered to be a 'radical.' The public is much too indifferent with regard to those violations of the freedom of teaching and teachers.[31]

Cattell had also asked Einstein to participate in a political meeting informing the public about the "terrible state of affairs" ("furchtbaren Zustände") in Nazi Germany. Einstein declined for personal reasons; in fact, his wife, Elsa Einstein, was fatally ill and died only two days later. He also wrote that to avoid the suspicion of partiality it would be better if somebody who was not directly suffering from the current "criminal business" ("Verbrecherwirtschaft") in Germany would do the job.

Although Einstein in his letter to Cattell spoke of the "poor guy" Mandl with a somewhat condescending air, he looked upon him not just as a crackpot at the margins of science. In fact, Einstein was well aware that the Czech immigrant was one of the many victims of Nazi imperialism which needed support, a support that he was willing to extend without much ado also to those not in the limelight as outstanding scientists.

But Einstein's tortuous and hesitant decision to give in to Mandl's pestering, as well as the ambivalent character of his support, have deeper roots than his political awareness. This is evident from a circumstance that has so far played no role in our story, although it is intimately related to it. With a portion of luck, this circumstance, documented by an early Einstein notebook, was revealed in the course of joint work with John Stachel. Without this finding, the encounter between Mandl and Einstein would have entered the history of gravitational lensing merely as a fortunate coincidence of two biographies which in fact were worlds apart, that of a lonely amateur and that of a famous scientist. It has turned out, however, that Einstein himself had even earlier than Mandl thought of gravitational lensing, at a time when he was in a position very much like Mandl's.

[31] "Ich weiss sehr wohl, dass Sie Columbia verlassen mussten. Forscher werden hier anderwärts behandelt wie Kellner oder Verkäufer in Detailgeschäften (Letztere verdienten es natürlich auch, unter besser gesicherten Verhältnissen zu arbeiten, es steckt kein Klassenhochmut hinter diesem Ausdruck.); gegenwärtig ist es besonders arg, wenn man von einem findet, er sei ein "Radical". Die Öffentlichkeit ist gegen jene Verstösse gegen die Freiheit der Lehre und der Lehrer viel zu gleichgültig." Einstein to Cattell, 18 December 1936, EA 65-603, the sentence in parenthesis was added as a footnote to the original letter.

In fact, as an analysis of Einstein's scribblings in a notebook used during the years he was a professor in Prague has shown, he not only had considered the very same idea in 1912 but also performed exactly the same calculations that he made in 1936 at the request of Rudi Mandl.[32] When Einstein first conceived the idea of gravitational lensing, he as well was still at the periphery of the academic establishment and desperately searched support for what to many of his colleagues appeared as an outlandish idea, that gravitation might affect the course of light rays. In a paper published in 1911, Einstein had discussed consequences of the *Influence of Gravitation on the Propagation of Light*,[33] an idea quite unheard of since Newton's corpuscular theory of light had been discarded in favour of wave theoretic concepts. And just as Mandl was trying to do in 1936, Einstein also encouraged astronomers to pursue investigations along his ideas:

> It would be urgently desirable that astronomers take up the question broached here, even if the considerations presented above may appear insufficiently founded or even adventurous.[34]

But Einstein's attempts to contact established astronomers remained as unsuccessful as those of Mandl many years later.[35] Only due to the mediation of one of his Prague students, Leo Pollak, Einstein had the opportunity of enlisting the help of a younger astronomer, Erwin Finlay Freundlich, willing to undertake an exploration of the observational consequences of his theory. Freundlich later remembered:

> 25 years ago Einstein, then professor in Prague, gave one of his first lectures on the gen. rel. theory and concluded with the words that he now needed an astronomical collaborator but that the astronomers were too much behind the time to follow the physicists. At that point a young student stood up and declared that, in Berlin, at the observatory which he had visited, he had become acquainted with a young astronomer for which this characterization did not fit.
>
> Einstein immediately wrote to me. From that our joint work and, de facto, my entire scientific life came into being. The student was Pollak, now ordinary professor in Prague.[36]

Freundlich, in turn, encountered similar problems as Einstein had encountered them at the beginning of his career and as did much later Mandl. At the time assistant at the Prussian Royal Observatory, Freundlich could only pursue his plan to support Einstein against the resistance of his superior, the well known astronomer Struve, who regarded the endeavor with skepticism.[37]

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32 *Renn, Sauer, and Stachel 1997*.

33 *Einstein 1911*.

34 "Es wäre dringend zu wünschen, daß sich Astronomen der hier aufgerollten Frage annähmen, auch wenn die im vorigen gegebenen Überlegungen ungenügend fundiert oder gar abenteuerlich erscheinen sollten." *Einstein 1911*, p. 908.

35 See, e.g., Einstein to Hale, 14 October 1913, and Hale to Einstein, 8 November 1913, *The Collected Papers of Albert Einstein*, Vol. 5, Docs. 477, 483. Even though Einstein by that time already was a well-established ETH professor, he still found it useful to have his ETH colleague Julius Maurer add a postscript to his letter thanking Hale in advance for a friendly reply.

It may well have been therefore in an atmosphere of clandestineness that, on the occasion of a visit to Berlin in April 1912, Einstein had met with Freundlich and evidently discussed with him his daring ideas, including the possibility of a gravitational lensing effect. It is, in any case, among notes from this period that his early calculations on gravitational lensing are found.[38]

Einstein was similarly isolated in his academic context, in spite of his rapid academic career. In fact, his project to develop a relativistic theory of gravitation met in the beginning, and certainly during his years in Prague, that is, 1911 and 1912, with the disinterest if not disapproval of most of his established colleagues. The interest, encouragement, and support which he did find came from friends of his student years, in particular from Marcel Grossmann and Michele Besso. Taking into account their contributions, Einstein's unbending search for a relativistic theory of gravitation emerges as the direct continuation of his bohemian rebellion against established academic science for which the mock Olympia Academy, founded in 1902 in Berne together with friends, stands as a symbol. Einstein was then an employee of the Swiss patent office, where some years later, still far from climbing the academic ladder, he conceived the equivalence principle, the first step towards a theory of gravitation based on a generalization of the relativity principle.

After Einstein had become part of the academic establishment, as a member of the Prussian Academy and Director at the Kaiser-Wilhelm-Institute for Physics, he continued to battle for his project. His struggle for securing Freundlich a position allowing him to work on the astronomical implications of the new theory of gravitation lasted for years. As late as 1918, Einstein intervened on behalf of Freundlich to Hugo Andres Krüss, ministerial director for Academy matters in the Prussian ministry of Education, complaining that:

> The disapproving behavior of his director has made it, for seven years, impossible for him to achieve the realization of his work plans directed towards the checking of the theory.[39]

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[36] "Vor 25 Jahren hielt Einstein, damals Professor in Prag, einen seiner ersten Vorträge über die allg. Rel. Theorie und schloss mit den Worten, dass er nunmehr einen astronomischen Mitarbeiter bedürfe, dass die Astronomen aber zu rückständig seien, um den Physikern zu folgen. Da erhob sich ein junger Student und teilte mit, er habe in Berlin an der Sternwarte, die er besucht hatte, einen jungen Astronomen kennen gelernt, auf den diese Charakterisierung nicht stimme. Einstein schrieb sofort an mich. Daraus ist unsere gemeinsame Arbeit und de facto mein wissenschaftliches Leben hervorgegangen. Der Student war Pollak, jetzt Ordinarius in Prag." Freundlich an Bosch, 4. 12. 193?, (from Unternehmensarchiv BASF, Ludwigshafen, Personalarchiv Carl Bosch W 1/folder 9/2; this letter was found by Dieter Hoffmann who generously made it available to us). See also the reference to Pollack's role as a mediator in note 6 to Doc. 278 of *The Collected Papers of Albert Einstein*, Vol. 5, p. 313.

[37] See *Hentschel 1994*, *Hentschel 1997*, and *Renn, Castagnetti and Damerow 1999*.

[38] See *Renn, Sauer, and Stachel 1997*, p. 185.

[39] "Das ablehnende Verhalten seines Direktors hat es ihm jedoch sieben Jahre lang unmöglich gemacht seine auf die Prüfung der Theorie gerichteten Arbeitspläne zur Ausführung zu bringen." Einstein to Hugo A. Krüss, 10 January 1918, *The Collected Papers of Albert Einstein*, Vol. 8, Doc. 435.

Einstein's interventions for Freundlich are remarkably similar to his later dealings with Mandl, also in their ambivalence. In fact, in the same letter to Krüss, he acknowledged Freundlich's lack of qualification as a professional astronomer and emphasized, at the same time, his role as a pioneer in recognizing the astronomical significance of general relativity:

> Incomparably less gifted than Schwarzschild, he has nevertheless recognized several years before the latter the importance of the new gravitation theories for astronomy and has engaged himself with glowing zeal for the checking of the theory along an astronomical or rather astrophysical way. [40]

In contemporary letters to colleagues he was even more explicit about Freundlich's weaknesses[41] but, in the end, regularly came down on his side, not the least because the latter essentially remained his most devoted support among German astronomers.

Together with Freundlich, Einstein had considered a number of possibilities to detect the light-deflecting effect of gravitation predicted by general relativity. In his first letter to Freundlich he lamented that nature did not provide him with a better environment for testing the theory, for instance by way of a planet sufficiently large to make the effect noticeable. In his letter he also stressed the crucial significance of such a test which, in fact, would be capable of distinguishing between his theory and alternative gravitation theories:

> But at least one thing can be stated with certainty: If such a deflection does not exist, then the premises of the theory are not adequate. For one has to keep in mind that, although these premises are plausible, they are nevertheless rather daring. If only we had a truly larger planet than Jupiter! But Nature did not deem it her business to make the identification of her laws comfortable for us.[42]

On the basis of Einstein's knowledge about the dimensions of the universe in the 1910s, which for him and his contemporaries essentially consisted of our own galaxy, the conditions under which gravitational lensing might be detected must have struck him as even more far-fetched than speculations about the observability of light deflection by Jupiter. Indeed, as we know from a letter to his friend Zangger of 15 October 1915, Einstein had given up the idea of finding

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[40] "Ungleich weniger begabt als Schwarzschild hat er doch mehrere Jahre vor diesem die Wichtigkeit der neuen Gravitationstheorien für die Astronomie erkannt und sich mit glühendem Eifer für die Prüfung der Theorie auf astronomischem beziehungsweise astrophysikalischem Wege eingesetzt." Einstein to Hugo A. Krüss, 10 January 1918, *The Collected Papers of Albert Einstein*, Vol. 8, Doc. 435.

[41] See, e.g. Einstein to Schwarzschild, 9 January 1916, *The Collected Papers of Albert Einstein*, Vol. 8, Doc. 181, Einstein to Sommerfeld, 2 February 1916, *The Collected Papers of Albert Einstein*, Vol. 8, Doc. 186, Einstein to Hilbert, 30 March 1916, *The Collected Papers of Albert Einstein*, Vol. 8, Doc. 207.

[42] "Aber eines kann immerhin mit Sicherheit gesagt werden: Existiert keine solche Ablenkung, so sind die Voraussetzungen der Theorie nicht zutreffend. Man muss nämlich im Auge behalten, dass diese Voraussetzungen, wenn sie schon naheliegen, doch recht kühn sind. Wenn wir nur einen ordentlich grösseren Planeten als Jupiter hätten! Aber die Natur hat es sich nicht angelegen sein lassen, uns die Auffindung ihrer Gesetze bequem zu machen." Einstein to Freundlich, 1 September 1911, *The Collected Papers of Albert Einstein*, Vol. 5, Doc. 281.

an observational confirmation of general relativity on the basis of gravitational lensing even before he completed the theory of general relativity little more than a month later. In this letter he rejected an earlier speculative interpretation of nova stars as lensing phenomena:

> Now it has unfortunately dawned on me that the "new stars" have nothing to do with the "lensing effect," that furthermore the latter has to be, in view of the star densities occuring in the sky, such an enormously rare phenomenon that one would probably expect it in vain.[43]

When the deflection of light by the gravitational field of the sun was eventually confirmed by an English expedition during the total solar eclipse of 1919, it was a stroke of luck that the effect was just within the margins of observability.[44] This confirmation of the predictions of general relativity by Eddington and his collaborators made Einstein all of a sudden world-famous and put general relativity at the foundations of modern physics. But in spite of this breakthrough success, Einstein may well have remembered that general relativity had emerged from what to his colleagues once appeared as a crackpot idea. His support for outsiders such as Freundlich and Mandl becomes fully understandable, it seems to us, only on the background of this experience.

## MANDL'S SUCCESS

The significance of gravitational lensing for the history of general relativity and of cosmology makes it natural to ask who had first suggested it and when. But the peculiar story of Einstein's double encounter with the idea of gravitational lensing—in 1912 and in 1936—and of Mandl's role in the second episode show that this question cannot easily be answered. Evidently, Einstein himself neither in 1912 nor in 1936 considered gravitational lensing to be a great idea, let alone a discovery. But the story is even more complicated. Almost immediately after Einstein's "little calculation"—made to comply with Mandl's wish—was eventually published in the "Discussion" section of the December 4 issue of *Science,* a number of other papers were published which further developed the idea, taking it much more seriously than Einstein himself had done. But what is more, various authors now also claimed a fatherhood to what had evidently become, all of a sudden, a respectable child.

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[43] "Es ist mir nun leider klar geworden, dass die "neuen Sterne" nichts mit der "Linsenwirkung" zu thun haben, dass ferner letztere mit Rücksicht auf die am Himmel vorhandenen Sterndichten ein so ungeheuer seltenes Phänomen sein muss, dass man wohl vergebens ein solches erwarten würde." Einstein to Zangger, 15 October 1915, *The Collected Papers of Albert Einstein*, Vol. 8, Doc. 130.

[44] For a historical discussion, see *Earman and Glymour 1980*.

Thus Tikhov, in a publication triggered by Einstein's note, dated 25 June 1937 and entitled "Sur la déviation des rayons lumineux dans le champ de gravitation des étoiles",[45] claimed in the introductory paragraph that he had had the idea as early as summer 1935 and that he had sent a first communication to the Poulkovo observatory by January 1936. In his paper Tikhov then gives a deduction of the lensing formulae both for what he calls the classical and the relativistic case.

Zwicky, in the second of two notes on gravitational lensing triggered by Einstein's publication, pointed out:

> Dr. G. Strömberg of the Mt Wilson Observatory kindly informs me that the idea of stars as gravitational lenses is really an old one. Among others, E. B. Frost, late director of the Yerkes Observatory, as early as 1923 outlined a program for the search of such lens effects among *stars*.[46]

To our knowledge, however, neither Strömberg nor Frost had ever published anything about their ideas and whatever research they had done, it was not given away in publications.

However, there had indeed also been precursors that discussed the idea in published work—but strangely without leaving any mark on the history of the idea. Tikhov in his paper pointed to a publication from the year 1924 by O. Chwolson.[47] As Tikhov observed, it was the only reference he had found on consulting the literature, that would already discuss the idea of a gravitational lens. Chwolson's note discussed both the possibility of observing double stars as well as the possible effect of a ring-shaped image for perfect alignment.

It is not unlikely that even Einstein was familiar with Chwolson's note and yet simply ignored it. Not only was it published in the prestigious *Astronomische Nachrichten,* at the time perhaps the most important astronomical journal in Europe. Einstein himself also published in that same journal. Indeed, a brief response by Einstein to Anderson on the electron gas came to be printed in the very same issue, in fact just below Chwolson's note on the very same page, cp. Fig. 2.

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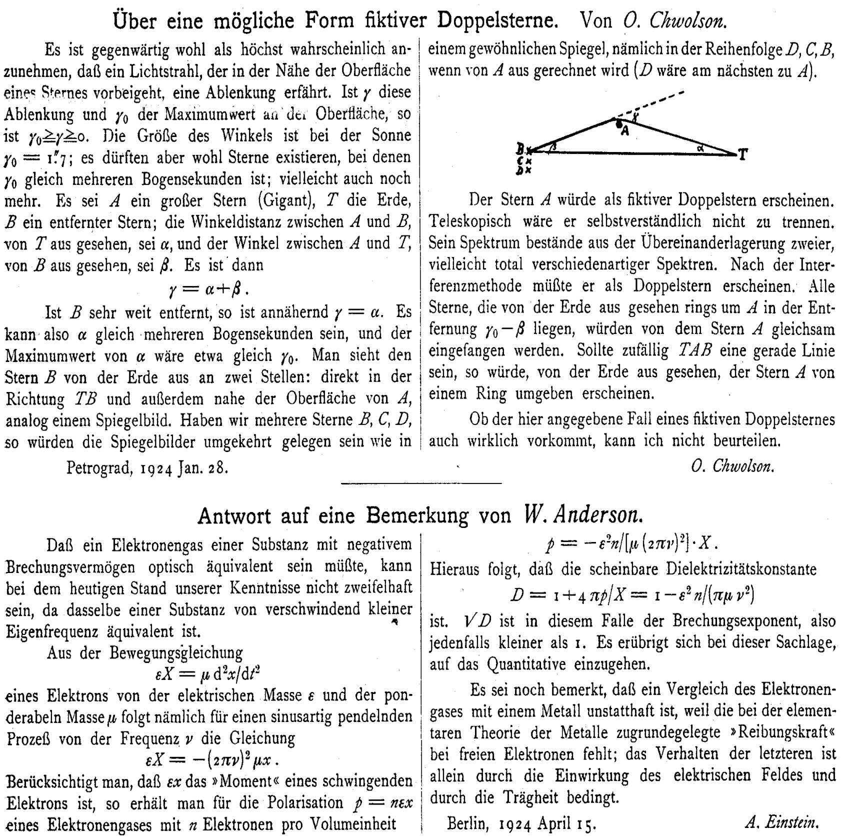
45 *Tikhov 1937*.
46 *Zwicky 1937b*.
47 *Chwolson 1924*.



## Über eine mögliche Form fiktiver Doppelsterne. Von *O. Chwolson.*

Es ist gegenwärtig wohl als höchst wahrscheinlich anzunehmen, daß ein Lichtstrahl, der in der Nähe der Oberfläche eines Sternes vorbeigeht, eine Ablenkung erfährt. Ist $\gamma$ diese Ablenkung und $\gamma_0$ der Maximumwert an der Oberfläche, so ist $\gamma_0 \geqq \gamma \geqq 0$. Die Größe des Winkels ist bei der Sonne $\gamma_0 = 1''.7$; es dürften aber wohl Sterne existieren, bei denen $\gamma_0$ gleich mehreren Bogensekunden ist; vielleicht auch noch mehr. Es sei $A$ ein großer Stern (Gigant), $T$ die Erde, $B$ ein entfernter Stern; die Winkeldistanz zwischen $A$ und $B$, von $T$ aus gesehen, sei $\alpha$, und der Winkel zwischen $A$ und $T$, von $B$ aus gesehen, sei $\beta$. Es ist dann

$$\gamma = \alpha + \beta\,.$$

Ist $B$ sehr weit entfernt, so ist annähernd $\gamma = \alpha$. Es kann also $\alpha$ gleich mehreren Bogensekunden sein, und der Maximumwert von $\alpha$ wäre etwa gleich $\gamma_0$. Man sieht den Stern $B$ von der Erde aus an zwei Stellen: direkt in der Richtung $TB$ und außerdem nahe der Oberfläche von $A$, analog einem Spiegelbild. Haben wir mehrere Sterne $B, C, D$, so würden die Spiegelbilder umgekehrt gelegen sein wie in einem gewöhnlichen Spiegel, nämlich in der Reihenfolge $D, C, B$, wenn von $A$ aus gerechnet wird ($D$ wäre am nächsten zu $A$).

Der Stern $A$ würde als fiktiver Doppelstern erscheinen. Teleskopisch wäre er selbstverständlich nicht zu trennen. Sein Spektrum bestände aus der Übereinanderlagerung zweier, vielleicht total verschiedenartiger Spektren. Nach der Interferenzmethode müßte er als Doppelstern erscheinen. Alle Sterne, die von der Erde aus gesehen rings um $A$ in der Entfernung $\gamma_0 - \beta$ liegen, würden von dem Stern $A$ gleichsam eingefangen werden. Sollte zufällig $TAB$ eine gerade Linie sein, so würde, von der Erde aus gesehen, der Stern $A$ von einem Ring umgeben erscheinen.

Ob der hier angegebene Fall eines fiktiven Doppelsternes auch wirklich vorkommt, kann ich nicht beurteilen.

Petrograd, 1924 Jan. 28.

*O. Chwolson.*

## Antwort auf eine Bemerkung von *W. Anderson.*

Daß ein Elektronengas einer Substanz mit negativem Brechungsvermögen optisch äquivalent sein müßte, kann bei dem heutigen Stand unserer Kenntnisse nicht zweifelhaft sein, da dasselbe einer Substanz von verschwindend kleiner Eigenfrequenz äquivalent ist.

Aus der Bewegungsgleichung

$$\varepsilon X = \mu\, \mathrm{d}^2x/\mathrm{d}t^2$$

eines Elektrons von der elektrischen Masse $\varepsilon$ und der ponderabeln Masse $\mu$ folgt nämlich für einen sinusartig pendelnden Prozeß von der Frequenz $\nu$ die Gleichung

$$\varepsilon X = -(2\pi\nu)^2 \mu x\,.$$

Berücksichtigt man, daß $\varepsilon x$ das »Moment« eines schwingenden Elektrons ist, so erhält man für die Polarisation $p = n\varepsilon x$ eines Elektronengases mit $n$ Elektronen pro Volumeinheit

$$p = -\varepsilon^2 n/[\mu(2\pi\nu)^2] \cdot X\,.$$

Hieraus folgt, daß die scheinbare Dielektrizitätskonstante

$$D = 1 + 4\pi p/X = 1 - \varepsilon^2 n/(\pi\mu\nu^2)$$

ist. $\sqrt{D}$ ist in diesem Falle der Brechungsexponent, also jedenfalls kleiner als 1. Es erübrigt sich bei dieser Sachlage, auf das Quantitative einzugehen.

Es sei noch bemerkt, daß ein Vergleich des Elektronengases mit einem Metall unstatthaft ist, weil die bei der elementaren Theorie der Metalle zugrundegelegte »Reibungskraft« bei freien Elektronen fehlt; das Verhalten der letzteren ist allein durch die Einwirkung des elektrischen Feldes und durch die Trägheit bedingt.

Berlin, 1924 April 15.

*A. Einstein.*

**Figure 4:** A brief note by Einstein published on the same page of the *Astronomische Nachrichten* as Chwolson's 1924 note on the gravitational lensing phenomenon.

Clearly, Chwolson's note, though written by an established physicist in a well-known journal, did not have the least effect on Einstein and, it seems, did not have any effect at all.

Ironically, the more time passed by and the more gravitational lensing became a productive field of research, the more precursors were identified. In 1964, at a time of renewed interest in the effect of gravitational lenses,[48] Liebes went to the efforts of compiling

> the references which have been found in the literature to gravitational lens phenomena, apologizing for those oversights which undoubtedly have been made.[49]

The first reference in Liebes' list is a half-page note by Oliver Lodge on "Gravitation and Light" published in *Nature* in the December 4 issue of 1919. While Lodge qualitatively conceived of the idea of a gravitational lens, he emphasized that

> it is not permissible to say that the solar gravitational field acts like a lens, for it has no focal length.[50]

48 See, e.g., *Darwin 1959*, *Mikhailov 1959*, *Metzner 1963*, *Klimov 1963*, *Refsdal 1964*, and *Schneider, Ehlers, and Falco 1992*, sec. 1.1.

49 *Liebes 1964*, p. B 835.

Liebes then cited Eddington's book *Space, Time and Gravitation*, published in 1920. In his book, Eddington mentioned the possibility of observing a double image due to gravitational lensing in a section on observational tests of general relativity. He also considered the expected intensity of the deflected light rays and concluded that

> it is easily calculated that the increased divergence would so weaken the light as to make it impossible to detect it when it reached us.[51]

Einstein may well have seen Lodge's note, and he certainly was aware of Eddington's book. Nevertheless, for all we know, he did as little respond to any of these authors or mention them in his 1936 paper as he did with Chwolson's note. It was evidently only Mandl's initiative that forced gravitational lensing to enter the historical stage. What was so special about this intervention of an amateur scientist?

The root of Mandl's idea was not a technical problem within a highly specialized scientific discipline but a simple model of gravitational light deflection conceived in analogy to the action of a lens in geometrical optics. What made Mandl so vigorously pursue this idea until he finally turned Einstein's mind was the combination of this mental model with a grand vision of its implications for the understanding of nature on a cosmological scale. However, the simplicity of the mental model and the grandness of his vision, did not fit into the grid of contemporary professional science.

Mandl's characterization as an "amateur scientist" is certainly justified in view of his lack of an adequate professional training and academic status. But it does distract from a crucially important dimension of science, its foundation in a shared knowledge of nature that is not the exclusive property of outstanding individuals or of the institutions of professional science. On the contrary, we believe that it is hardly possible to understand the development of science, and in particular the scientific revolution represented by Einstein's theories of relativity, without taking into account this shared knowledge which, we believe, comprises both specialized theories but also elementary ideas e.g. of space, time, gravity, and light. It is these elementary ideas which by their very nature do not fall under the domain of any specialized domain of physics but cover areas of knowledge ranging from psychology via technological practices to cosmology. Mandl's role in the history of gravitational lensing illustrates that the exploration of this vast territory of knowledge is not necessarily the privilege of a few outstanding scientists such as Einstein. After all, even the most daring and seemingly ridiculous aspects of Mandl's vision turn out to be, under closer inspection, not as "unprofessional" as they might appear at first glance. His expectations about the promise of gravitational lensing to become an observational

[50] *Lodge 1919*, p. 354.
[51] *Eddington 1920*, p. 134.

confirmation of general relativity have obviously been amply confirmed. But also Mandl's, at first sight, far-fetched speculations about a cosmic cause of the disappearance of dinosaurs are strikingly close to Alvarez's theory about a meteorite impact as the possible origin of an ecological catastrophe leading to the extinction of species, a theory that is today widely accepted.

What Mandl achieved, in the end, was to introduce a simple idea into the canon of accepted scientific knowledge, an idea which before was rejected only because it was not deemed observable. Why should astronomers care about an effect that seemed to be inaccessible to observations? Mandl's initiative, together with the fact that Einstein gave prominence to it by his 1936 publication, however, stimulated a broad discussion among astronomers and astrophysicists, even in spite of the absence of any immediate prospects of the observational verification of the lensing effect. And this discussion lasted until the effect was eventually confirmed by observations. Mandl's success was thus, as a matter of fact, also a victory of fantasy. How would a world look in which gravitational lensing would not, as Einstein and Eddington had originally surmised, be a minor, essentially unobservable effect? Einstein's publication stimulated his contemporaries to imagine such a world and thus to take the effect seriously and to explore the conditions under which it might be observable, after all.

One such publication was written by Henry Norris Russell, entitled "A Relativistic Eclipse," and appeared in the February 1937 issue of the *Scientific American*.[52] As the author line tells the reader, Russell was "PhD. Chairman of the Department of Astronomy and Directory of the Observatory at Princeton University. Research Associate of the Mount Wilson Observatory of the Carnegie Institution of Washington. President of the American Astronomical Society." Since both lived in Princeton, Russell and Einstein had probably been talking over the matter personally. The paper, in any case, dated December 2, 1936, acknowledges Einstein's help:

> My hearty thanks are due to Professor Einstein, who permitted me to see the manuscript of his note before its publication.[53]

Russell focused on the issue of observability and agreed that the lensing effect would not be verifiable for terrestrial observers. But he was not disheartened by this and further pursued the idea. The subject of his paper is:

> What Might be Seen from a Planet Conveniently Placed Near the Companion of Sirius. Perfect Tests of General Relativity that are Unavailable.[54]

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[52] *Russell 1937*.
[53] Einstein in turn had also seen Russell's paper prior to publication and had made comments on a previous draft, see H. N. Russell to A. Einstein, dated 27 November 1936, EA 20-067.
[54] *Russell 1937*.

Discussing the orders of magnitude of the lensing effect with a white dwarf, Russell considers taking Sirius's companion as a gravitational lens, and Sirius itself as the light source. He imagines a small planet orbiting around Sirius's companion with just the right distance and considers how Sirius would appear for observers on this planet if distorted by the lensing effect of its companion. The paper gives some sketches of the distorted forms of Sirus's image as seen from the imaginary planet when its lensing companion passes through the line of sight. Paraphrasing the sketches, Russell compares the event to an ordinary solar eclipse (without gravitational deflection). He describes an intermediate state of the eclipse and the appearance of the lensing effect like this:

> a bright crescent has appeared on the *opposite* side of the eclipsing disk. This is produced by light coming from the part of the geometrical disk of Sirius nearest the center of the companion and deflected around the far side of the latter.

The final drawing shows the case of perfect alignment, or the case of the later so-called an "Einstein Ring:"

> ... for central eclipse, it looks like an annular eclipse of a large disk by a small one, instead of the actual total phase. From this point, all the previous phases occur in reverse order.

Russell concluded:

> Our hypothetical space-tourist, therefore, could settle down with his planet in such a place that general relativity would no longer be a matter of the utmost refinement of theory and observation. It would instead be needed to account for the most bizarre and spectacular phenomena of the heavens, as he saw them.

While Russell's world was purely imaginary, his paper contributed much to keep the interest alive and was often cited in the sequel. It also contributed to give a realistic twist to the abstract question of how a world would have to look like to make gravitational lensing an important effect.

The challenge of using gravitational lensing in order to probe cosmic dimensions was taken up in another immediate response to Einstein's paper, which was sent as "Letter to the Editor" of *Physical Review* and which appeared in the February 15 issue of that journal.[55] It was entitled "Nebulae as Gravitational Lenses" and was written by the Swiss astronomer Fritz Zwicky who then worked at the Norman Bridge Laboratory[56] at the California Institute of Technology in Pasadena. Zwicky's concern as well was the observability of the phenomenon. His brief note also started with a reference to Mandl's idea:

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55 *Zwicky 1937a.*
56 The Norman Bridge Laboratory was directed by Millikan, see note 15

> Einstein recently published some calculations concerning a suggestion made by R. W. Mandl, namely, that a star $B$ may act as a "gravitational lens" for light coming from another star $A$ which lies closely enough on the line of sight behind $B$. As Einstein remarks the chance to observe this effect for stars is extremely small.

The next passage makes it clear that also Zwicky had first encountered the problem of gravitational lensing as an indirect consequence of Mandl's persistence:

> Last summer Dr. V. K. Zworykin (to whom the same idea had been suggested by Mr. Mandl) mentioned to me the possibility of an image formation through the action of gravitational fields. As a consequence I made some calculations which show that extragalactic *nebulae* offer a much better chance than *stars* for the observation of gravitational lens effects.

It was in fact the aim of Zwicky's communication to point out that extragalactic nebulae, as a consequence of their masses and apparent dimensions, were much more likely candidates for the observation of gravitational lenses. He argued that the discovery of lensing images "would be of considerable interest" not only since it would provide a test of relativity theory but also because one might find nebulae at greater distance through the lensing effect and also because one might get further information about the masses of those nebulae that act as gravitational lenses. In the final sentence, Zwicky optimistically announced the publication of a "detailed account of the problems sketched here."

Two months later, Zwicky, instead of a detailed account, submitted another letter to the editor of *Physical Review*.[57] It was entitled "On the Probability of Detecting Nebulae Which Act as Gravitational Lenses." Zwicky now argued that:

> the probability that nebulae which act as gravitational lenses will be found becomes practically a certainty. [...]
> Present estimates of masses and diameters of cluster nebulae are such that the observability of gravitational lens effects among the nebulae would seem ensured.

But in spite of his optimism, Zwicky was aware that the search for gravitational lenses would actually be laborious:

> In searching through actual photographs, a number of nebular objects arouse our suspicion. It will, however, be necessary to investigate certain composite objects spectroscopically, since differences in the red shift of the different components will immediately betray the presence of gravitational effects. Until such tests have been made, further discussion of the problem in question may be postponed.

It seems to have been postponed for quite a while, for Zwicky's detailed account, announced for the *Helvetica Physica Acta*, did not appear in that journal for some years.

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[57] *Zwicky 1937b*.

As the papers by Russell, Zwicky, and others testify, gravitational lensing as a subject in its own right had been born with Einstein's 1936 *Science* note. Mandl's role in establishing this subject was crucial since he had helped turn gravitational lensing into a theoretical reality long before it became an observational reality. Only after Einstein's publication of the calculations he had made at Mandl's request, other scientists like Russell, Zwicky, and Tikhov took up the idea and dared to publish their findings. From this point on, the idea of gravitational lensing was kept alive and became part of the theoretical program of general relativity. Henceforth, it was again and again tentatively applied to explain curious astronomical phenomena.[58] Whenever a new phenomenon appeared on the sky, it became part of the routine to ask whether it could be related to gravitational lensing, until one of these curious phenomena turned out to be a perfect embodiment of the idea. But it took almost half a century after Einstein's publication before, in the sequel of the discovery of quasars, the cosmos known to us finally reached the dimensions and astrophysics the technical sophistication to make gravitational lensing a reality.[59]

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[58] For a historical review, see *Barnothy 1989*.

[59] See *Walsh, Carswell, and Weymann 1979* and *Young et al. 1980*. For reviews, see *Refsdahl and Surdej 1994* and *Wambsgans 1998*.

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