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INFLUENCES ON EINSTEIN'S EARLY WORK IN RELATIVITY THEORY

A REPORT BASED ON ARCHIVAL MATERIALS *

1. INTRODUCTION

On March 11, 1952, Albert Einstein wrote to Carl Seeling: "Between the conception of the idea of this special relativity theory and the completion of the corresponding publication, there elapsed five or six weeks. But (he adds rather cryptically) it would be hardly correct to consider this as a birth date, because earlier the arguments and building blocks were being prepared over a period of years, although without bringing about the fundamental decision" [1].

Can we get some idea of what may have happened during those years and what — or who — helped to bring about the "fundamental decision"?

I have been interested in studying the various influences that worked their way into Einstein's early publications on relativity, and conversely, the reception accorded to these publications and ideas. How large or how small was the effect of the work of earlier physicists? Is there some strong influence that has so far been overlooked? In what respects was Einstein's view of the problem different from that of his contemporaries — for example, in the role that experiment results played compared with epistemological considerations? And how do such differences explain the rather hostile early response of the scientific community to his work?

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[1] The literary rights to quotations from the writings of Albert Einstein belong to the Estate of Albert Einstein; permission to use quotations should be secured from the Executor, Dr. Otto Nathan, 55 East Loth Street, New York City.

The style of Einstein's first 1905 paper on relativity was markedly different from what was the accepted as current practice, different, for example, from Lorentz's or even Emil Cohn's (whose theory of electrodynamics of moving bodies was taken quite seriously at the time, *e.g.* by Bucherer and Abraham). Nor are there really sufficient clues in the literature which biographers cite that help us to understand the structure of that paper. You may recall it starts with the question why there should be different equations for dealing with the emf generated in a moving conductor going past a stationary magnet as against a stationary conductor near a moving magnet. Then, without specifically mentioning by name any of the now-so-famous experiments, it goes on to dismiss the conception of absolute motion and of the ether, and attacks the discussion of relativistic kinematics through a fundamental philosophical examination of the concepts of space and time. Only later on comes the treatment of Maxwell's equations and finally, almost as an afterthought, some predictions about electron motion, giving the equations „according to which the electron must move in conformity with the theory presented here". Only Einstein's friend, co-worker at the Patent Office, and former fellow-student, namely Michele Besso, is credited directly by Einstein in this paper with being helpful.

This is a strange and unique way of going about electrodynamics in 1905. Max von Laue, one of the first and foremost partisans for and contributors to relativity theory, nevertheless confessed to Margot Einstein in a letter of October 23, 1959, that he had feld fundamental difficulties for a long time. He wrote that after the publication of Einstein's paper in 1905 *ging mir langsam aber stetig eine neue Welt auf. Ich habe viel Mühe darauf verwenden müssen.... Und ganz besonders waren es erkenntnistheoretische Schwierigkeiten, die mit zu schaffen machten. Aber seit etwa 1950 glaube ich, diese überwunden zu haben....* Professor Infeld, in his book on *Albert Einstein* (1950), similarly writes (p. 23): "The title sounds modest, yet as we read it we notice almost immediately that it is different from other papers. There are no references; no authorities are quoted, and the few footnotes are of an explanatory character. The style is simple, and a great part of this article can be followed without advanced technical knowledge. But its full understanding requires a maturity of mind and taste that is more rare and precious than pedantic knowledge, for Einstein's paper deals with the most basic problems; it analyzes the meaning of concepts that might seem too simple to be scrutinized...".

So in retrospect it is not entirely surprising that it took also a long brooding period for Einstein himself before this remarkable egg was hatched. But where, when, and from whom might Einstein have obtained some of his point of view, his questions, and his method?

2. ARCHIVAL HOLDINGS

One way to begin to find out is, of course, to look at documents. Therefore, after these questions had raised themselves, I began to search for whatever documents might have survived that would be relevant. In the possession of Albert Einstein's Estate, and kept for the time being at the Institute for Advanced Study at Princeton, there are indeed documents — about twenty metal file-drawers full, counting only the most scientific part of Einstein's manuscripts and the largely unpublished correspondence.

Einstein's remarkable and devoted secretary, Miss Helen Dukas, has been putting the documents into systematic order; over the past three years, under my general supervision, the material has been catalogued with the help of some graduate student's labor and by means of my periodic visits from Harvard, plus some work I put in during a sabbatical leave which I spent at the Institute for Advanced Study at Princeton a year ago. Copies of correspondence are continually being received and added, but a preliminary archival calendar is now fairly well finished, and it has begun to be put to use in scholarly work. In this paper itself I shall be drawing on a number of hitherto unpublished documents, and thereby can hope to furnish one example of the many problems that can now be attacked with the aid of the archives.

Letters — originals or copies, often to and from the correspondent, on scientific matters and other professional interests — are available under the following names (to cite only a few of those correspondents who are included in the archives and who are now deceased): Bohr, Besso, Bucherer, Cassirer, Eddington, Ehrenfest, James Franck, Grossmann, Hadamard, Hilbert, Langevin, v. Laue, Lenard, Levi-Civita, Lorentz, D. C. Miller, Mach, Meyerson, Nernst, Pauli, Planck, Reichenbach, Schrödinger, de Sitter, Sommerfeld, Schlick, Swann, Stark, Szilard, Weyl, Wien. All this is quite apart from an immense correspondence on social, political, and other matters; personal correspondence with kings and commoners, with such persons as Freud and Thomas Mann, Weitzmann and Russell, Shaw and Nehru, as well as job seekers and plain people in need of advice. Einstein was apparently untiringly generous in his response whenever there was any good, human cause or interesting idea.

In addition, there are eleven notebooks, starting from Einstein's student days; a few travel diaries; folders upon folders of published manuscripts, many in early draft; and several dozen unpublished manuscripts.

All this survived, more or less by good luck: On returning from a trip to the United States in the winter of 1932-33, Einstein found on reaching Europe that Hitler had taken over in Germany. Einstein never again set foot on German soil, and most of the correspondence was brought out by diplomatic pouch.

One of the first things I looked for was, of course, any manuscript or draft of Einstein's 1905 paper on Relativity Theory. But particularly for the early papers, Einstein destroyed or discarded the manuscripts when they were returned from the printer, if they ever were. Einstein himself had occasion to regret this. For during a war-bond drive in the United States during World War II, he was asked to donate the manuscript, and when he could not oblige in this way, he was persuaded to write the paper out all over again in longhand. Miss Dukas recalls that she dictated it to him from his published paper, and that he kept interrupting saying that there is a simpler or more elegant way of expressing matters. The temptation to write something different, however, was overcome, and the manuscript was "auctioned" off to the person, who promised to buy the largest amount of War Bonds (which turned out to be several million dollars), and who, in turn, deposited the manuscript in the Library of Congress in Washington.[2]

Thus, there is no contemporaneous draft or manuscript from which one might learn something of the genesis of the paper. But there are two notebooks which Einstein kept while still a student at the Polytechnic Institute at Zürich (ETH) during the period 1897 to 1900. Both are sets of lectures notes taken in the physics course given by Heinrich Friedrich Weber whose special field of work was alternating-current technology. One of them is on heat and thermodynamics, the other on technical problems such as liquefaction of gases (with detailed drawings), and electricity from Coulomb's law to induction. But it does not even go to Maxwell's work. And on this hangs part of my tale. For what was left out was exactly what young Einstein was waiting for in his studies. The fact that he was thrown on his own devices had, as we shall see, some interesting consequences in the genesis of relativity theory.

3. READING AT HOME

As Besso wrote (in his notes of August 1946 for Professor Stickelberg's article on Einstein in Switzerland), Einstein came to the Aarau Kanton-school in 1896 "with the (then much-debated) questions concerning the palpability (*Greifbarkeit*) of ether and of atoms" in mind. When he went on to the ETH in Zürich, the lectures of physics made no great impression on Einstein who found his teachers' discussions "self-explanatory". It was indeed Weber who, Besso reports, said once

[2] And so it is both true and false that "Einstein's manuscriptis now in the Library of Congress, Washington" as the *Bibliography of Scientific Papers of Albert Einstein*, compiled by E. Weil (London, 1960), tells us.

to Einstein, "You are a clever fellow! But you have one fault: One can't tell you anything, one can't tell you anything". Clearly Weber could not.[3]

This circumstance is corroborated by Einstein's classmate, Louis Kollros (in Carl Seeling, editor, *Helle Zeit-Dunkle Zeit,* Zürich, 1956):

"There was not very much theoretical physics done at the Poly, which was strong in mathematics... (Weber's) lectures concerning classical physics were lively; but we waited in vain for an exposition of Maxwell's theory. We knew that the theory was founded on the identity of the transmission of electricity and light, and that the work of Hertz concerning electric waves had verified the theory. We would have gladly learned more about it. Above all, it was Einstein who was disappointed (for, as Einstein recalls in his *Autobiographical Notes,* it was «the most fascinating subject at the time»). In order to fill this gap, he undertook to study on his own the works of Helmholtz, Maxwell, Hertz, Boltzmann and Lorentz".

Kollros's list of authors reminds us of the famous passage in Einstein's *Autobiographical Notes* which does seem very relevant to the question of early influences shaping the thought expressed in the 1905 relativity paper. The passage (page 15) concerns the period of 1897-1900:

"I entered the Polytechnic Institute of Zürich as a student of mathematics and physics. There I had excellent teachers (for example, Hurwitz, Minkowski), so that I really could have gotten a sound mathematical education. However, I worked most of the time in the physical laboratory, fascinated by the direct contact with experience. The balance of the time I used in the main in order to study at home the works of Kirchoff, Helmholtz, Hertz, etc."

The really interesting part may well be the study of the last, the *e t c.* Who is hiding behind the phrase *et cetera*? Perhaps somebody who prepared the way Einstein went in presenting his relativity theory. We must, of course, not dismiss Kirchhoff, Helmholtz, and Hertz, or for that matter Boltzmann, Mach, Poincaré, and Lorentz. But someone else is also needed to explain Einstein's 1905 paper. If this neutrino exists, we should be able to find it. As you will see, we have now a good candidate for this honor.

4. MAXWELL, DIRECT AND INDIRECT

First a look at the others. From about 1903 on, Einstein was at the Patent Office in Berne for about six years. According to a manuscript note from Besso in the Archives, the vacancy was specifically advertised as requiring an "intimate acquaintance with Maxwell's

[3] Weber's successor, however, was Pierre Weiss, who brought Einstein back to Zurich from Prague in 1912.

theory." Einstein qualified on this score by the spring of 1905 — of that there is no doubt — and he must have known Maxwell's theory earlier. There are a number of corroborating statements (other than Kollros's and Besso's), for example in a letter to von Laue, sent by Einstein from Princeton on 17 January 1952:

"Dear Laue: I now have received your book concerning special relativity theory and find that it is very good... (But) when one looks over your collection of proofs of the special relativity theory, one becomes of the opinion that Maxwell's theory is unquestionable. But in 1905 I already knew certainly that Maxwell's theory leads to false fluctuations of radiation pressure and, with it, an incorrect Brownian motion of a mirror in a Planckian radiation cavity. In my view, one could not get arround ascribing to radiation an objective atomistic structure which, of course, does not fit into the frame of Maxwell's theory... Unfortunately, the fifty years which have since passed have not brought us closer to an understanding of atomistic structure of radiation".

Granting that Einstein obviously knew Maxwell's theory by 1905, the question is left through which books he learned it. It may have been by direct study of Maxwell's work, although there is no documentary evidence for this. At any rate, direct study would not have been the only or even the most important way.

Maxwell came to most German students of physics first through the works of Helmholtz, Boltzmann, and Hertz. They are in many ways quite different, but they also have at least one element in common: that these presentations of Maxwell's theory are quite un-Maxwellian, that, in different degrees, their style is even further from that of Maxwell than from Einstein's paper. On this point, a brief word must suffice here. For example, to a contemporaneous physicist in England and France, Helmholtz's way of thinking must have looked quite terrifying. Fully half of this introductory volume of the *Lectures on Theoretical Physics* is spent on the following topics: philosophy and science; physical science; critique of the old logic; concepts and their connotations; hypotheses as bases for the laws; the completeness of scientific experience and its practical significance; and so forth. Maxwell's work proper is presented in Volume 5 of the *Lectures on Theoretical Physics,* issued in 1897. The terminology there is one Einstein used to some extent later. What catches our eye is that there is very little attention to experimentation. One cannot, for example, find a reference to the Michelson experiments which, after all, were first tried under the sympathetic eye of Heimholtz himself. Even the section entitled "The Necessary Properties of the Ether" has no reference to experiments. And in the only paper which Helmholtz wrote specifically on the subject of Maxwell's theory, called *Consequences of Maxwell's Theory Concerning the Motion of the Pure Ether*, there is not a single mention of actual experiments. What Einstein might have obtained from studying Helmholtz's version of Maxwell's theory is first of all a reinforcement of a taste for a consciously epistemological approach, and a confirmation that in this area experiments do not count crucially.

Reading Hertz, whose collected works were available by 1895 Einstein will have seen Hertz's first thorough essay on *The Fundamental Equations of Maxwell's Electrodynamics* published in 1884, and the article significantly entitled *Concerning the Fundamental Equations of Electrodynamics for Moving Bodies* of 1890. Even this greatest of experimenters in the field of electromagnetism makes no mention of the "ether" experiments that have recently loomed so large in some discussions of the origins of relativity theory. On the other hand, the main effect the study of Hertz's work might have had upon a reader like Einstein is perhaps best characterized by Hertz's own remark in the *Principles of Mechanics*: "In general, I owe very much to the fine book concerning the development of mechanics by Mach." It was one of very many forces urging young Einstein toward Ernst Mach. As he said later in his *Autobiographical Notes,* Mach's *History of Mechanics* "shook this dogmatic faith" in "mechanics as the final basis of all physical thinking... This book exercised a profound influence upon me in this regard while I was a student... Mach's epistemological position... influenced me very greatly..." [4]

5. MACH

Indeed, it is an ironic circumstance that the state of contemporary research physics during the period when the young Einstein began to work on special relativity was really characterized by such a degree of dogmatic rigidity as the thought. As Stephen Brush has recently pointed out,[5] the mechanistic view of physical reality was then defended by only a "few lonely men such as Boltzmann... The most «advanced» and «sophisticated» theories were those that took a purely phenomenological viewpoint: scientific theories should deal only with the relations of observable quantities, and should strive for economy

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[4] Besso, writing in late 1947 to Einstein, reminds him that during the year of 1897 or 1898, Besso had drawn Einstein's attention to Mach, and he asks whether it is correct "that this introduction (to Mach) fell into a phase of development of the young physicist when the Machian style of thinking pointed decisively at observables — perhaps even, indirectly, to «clocks and meter sticks»"? The correspondence of Einstein abounds with references to the influence of Mach in the formative years of relativity theory. For example, in a letter of August 8, 1942 to A. S. Nash, Einstein wrote: "In the case of Mach, the influence was not only through his philosophy, but also through his critique concerning the fundaments of physics".

[5] *Thermodynamics and History.* "The Graduate Journal", in press.

of thought rather than trying to explain phenomena in terms of unobservable entities..." In short, around 1900 Mach's views were no longer those of an isolated fighter, the role which he rather liked and which he appeared to have in the books that young Einstein read with deep-felt agreement. On the contrary, it was the great H. A. Lorentz and H. Poincaré whose styles were coming to be out of step with the new physics exemplified by the Curies, Rutherford, Einstein himself, and at least at one point, even by Planck.[6]

Something of this flavour comes through in letters in Einstein's correspondence, and a famous passage in Einstein's *Autobiographical Notes* is closely related:

"Reflections of this type made it clear to me as long ago as shortly after 1900, *i.e.* shortly after Planck's trail-blazing work, that neither mechanics nor thermodynamics could (except in limiting cases) claim exact validity. By and by I despaired of the possibility (*Nach und nach verzweifelte ich an der Möglichkeit*) of discovering the true laws by means of constructive efforts based on known facts. The longer and the more despairingly I tried (*Je länger und verzweifelter ich mich bemühte*), the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results".

No matter how some of the younger physicists of the time wrestled with the problems of physics, the use of conceptions developed in nineteenth-century physics seemed to them merely to produce failure and despair. It is not too much to say that the new physics they fashioned was first of all *eine Physik der Verzweiflung.* And

[6] Thus in an unpublished letter from Berlin (1931) from Max Planck to R. W. Wood, kindly made available by Professor Wood's son to the American Institute of Physics Project on the History of Physics, and on deposit at their Archives in New York: "Dear Colleague: You expressed recently, at our nice dinner at Trinity Hall, the wish that I should describe to you more concerning the psychological side of the considerations which led me at the time to postulate the hypothesis of energy quanta. Here I want to accomodate your wish. Briefly put, I can describe the whole effort as an act of desperation, for by nature, I am peaceful and against dubious adventures. But I had been fighting already for six years, from 1894 on, with the problem of equilibrium between radiation and matter without having any success; I knew that this problem is of fundamental significance for physics; I knew the formula which provides the energy distribution in the normal spectrum; a theoretical explanation, therefore, had to be found at all cost, whatever the price. Classical physics was not sufficient, that was clear to me... (Except for the two laws of thermodynamics) I was ready for any sacrifice of my established physical convictions. Now Boltzmann had explained that thermodynamic equilibrium comes about through statistical equilibrium, and when one applies these considerations to the equilibrium betwenn matter and radiation, one finds that one can prevent the deterioration of energy in radiation by means of the supposition that energy is from the beginning forced to remain in certain quanta. This was a purely formal assumption, and I did not really think much about it except just this: No matter what the circumstances, may it cost what it will I had to bring about a positive result". (Translation by G. H.).

here the role of Mach as iconoclast and critic of classical conceptions was particularly important; for whether or not Einstein's assessment of the contemporary scene was right, it is certain that Mach's critical force and courage made a strong impression on him, as on so many others.

We cannot go here into the vast and important topic of the relation between Einstein and Mach. Suffice it to say that the archives help to illuminate each of the five stages in the drama: (1) Mach's place in physics and philosophy of science in the early years of this century; (2) Einstein's early acceptance of the main features of Mach's doctrine; the Einstein-Mach correspondence and meeting; (3) the revelation in 1921 of Mach's sudden and brutal attack on Einstein's relativity theory (occasioned, it appears to me, in good part by the fact that Mach discovered the anti-Machist kernel of Einstein's epistemology even before Einstein did himself); and (4) Einstein's own further development of a philosophy of knowledge in which he rejects many but not all, of his earlier Machist tendencies. Thus at the end of this development Einstein writes (on January 24, 1938, to C. Lanczos): *Vom skeptischen Empirismus etwa Mach'scher Art herkommend hat das Gravitationsproblem mich zu einem gläubigen Rationalisten gemacht, d.h. zu einem, der die einzige zuverlässige Quelle der Wahrheit in der mathematischen Einfachkeit sucht.*

In his letters, as in his *Autobiographical Notes*, Einstein later continued to acknowledge the general but strong influence that Mach had been. Nevertheless, we can well understand that at heart Mach's rejection was very painful, the more so as it was somehow Einstein's tragic fate to have the contribution he most cared about be rejected by the very men whose approval and understanding Einstein would have most gladly had — a situation not unknown in the history of science. In addition to Mach, the list includes these four: Poincaré (who to his death in 1912, only once deigned to mention Einstein's name in print, and then only to register his dissent); Lorentz (who gave Einstein personally every possible encouragement — short of fully accepting this theory of relativity for himself); Planck (whose support of the special theory of relativity was unstinting but who resisted Einstein's ideas on general relativity, not to speak of the quantum theory of radiation); and Michelson, who to the end of his days did not believe in relativity theory, and even once said to Einstein that he was sorry that his own work may have helped to start this "monster" [7].

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[7] R. S. Shankland: "American Journal of Physics" 1963, 31, p. 36.

6. POINCARÉ AND LORENTZ

The influence on and response to Einstein's work on the part of both Poincaré and Lorentz has also been a fascinating problem for the historians of recent science. Although the old myths will not die quickly, they have been pretty well exploded.[8]

To put it briefly, and without intending in the slightest to denigrate Poincaré's enormous accomplishments, we may say that he saw the "crisis" in physics as one primarily revolving about experimental difficulties, and therefore involving neither epistemological nor fundamentally different theoretical reorientation. This is, of course, directly antithetical to Einstein's view of the matter at about the same time: The new experimental findings, such as Michelson-Morley experiment, neither provoked the crisis as Einstein saw it, nor were guides to the new orientation needed. That Einstein's work in 1905 is independent of Poincaré's investigations on electromagnetism in 1904—05 has now been repeatedly and adequately established.

When it comes to the debt of independence of Einstein with respect to Lorentz's work, and the response of Lorentz to Einstein's early papers, the record is also quite clear. Einstein and others repeatedly said that he did not know of Lorentz's 1904 paper on electromagnetic phenomena.[9] On this well-worked ground, perhaps one need only to point out anecdotally how difficult it would have been for an almost unknown Patent Office employee in a Swiss town such as Bern to have had direct access to the *Proceedings of the Amsterdam Academy* in which Lorentz published the 1904 paper. In the Rijksarchiv at the Hague, Holland, I found a letter from M. Laue, writing to Lorentz on November 30, 1905, from Berlin, apparently for the first time, and in his capacity as Assistant at the Institute for Theoretical Physics (therefore, as Planck's assistant):

"Since the *Proceedings of the Amsterdam Academy* are here more difficult to obtain than other journals — they exist only in the Royal Bibliothek, and it lends out recent journals only for a day — I take the liberty of expressing to you the request to send me, if possible, a reprint of your publication, «Electromagnetic Phenomena in a System Moving with Any Velocity Less than that of Light»".[10]

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[8] *e.g.* G. Holton: *On the Origins of the Special Theory of Relativity.* "Am. J. Phys". 28, 1960, p. 627; and G. Holton: *On the Thematic Analysis of Science: The Case of Poincaré and Relativity.* in *Melanges Alexandre Koyré.* I, 1964, p. 257.

[9] For the evidences, see for example Holton: *op. cit.* In his lectures and publications Lorentz repeatedly gave generous credit to the novelty and independence of Einstein's work. See also, for example, Lorentz's remarks quoted in Silberstein's *Theory of Relativity.* p. 117, and Lorentz's footnote addendum on p. 10 of the 1912 edition of his 1904 essay, in H. A. Lorentz, *et al., Das Relativitätsprinzip,* Teubner.

[10] The paper, originally printed as part of the *Proceedings of the Academy meeting of April 23, 1904,* was first published in the June 1904 issue of the

If one had to summarize the difference between Lorentz's and Einstein's relativity physics in a sentence, one might say this: Lorentz's work can be seen somewhat as that of a valiant and extraordinary captain rescuing a patched ship that is being battered against the rocks of experimental fact, whereas Einstein's is the physics of despairing disenchantment with the mode of transportation itself — an escape to a rather different vehicle altogether.

This brings us back for a final assault on the problem of the proper antecedents of Einstein's work. Neither the shape nor the content of Einstein's 1905 paper, *The Electrodynamics of Moving Bodies,* is adequately explained as a sequel to the chain Lorentz-Poincaré, or Maxwell-Helmholtz-Boltzmann, or even Kirchoff-Mach-Hertz. It is, of course, possible that Einstein's 1905 paper was a creation with no direct preparatory antecedent. And not having found any models in the works of the major contributors of the time, we may be tempted to make this assumption, even if reluctantly.

But it turns out that we do not have to do this. Working with the documents in the Princeton Archives, I came across a clue that so far had not raised itself. And this clue may lead us to the possibility of entertaining a quite different and unconventional view of the direct ancestry of Einstein's thought processes leading to his 1905 paper.

7. AN ALMOST FORGOTTEN TEACHER

In one of the thousands of letters there appears, only once and casually, the name of a physicist who has not yet been mentioned here. It is August Föppl.

This name is known to a number of older German scientists and engineers, but to almost nobody else. It sounds very much out of place compared to "Kirchhoff, Helmholtz, Hertz, *etc.*" — so much so that it might well have ended up among the *et ceteras* mentioned in the autobiographical note of Einstein. And indeed, the search for the identity of August Föpl starts very badly: Born in 1854, Föpl was, at the age of 36 a technical high school teacher and administrator in Leipzig when he published his first book, a rather pedestrian little outline of elementary physics. From the first exercise of this *Leitfaden* (how rapidly must a disc spin to throw off a lightly adhering object?) to the last (explain parallel winding in a.-c machines), there is nothing to indicate that this man could ever enter our story.

Two years later, now a civil engineer in Leipzig, Föppl published his first real book, *Das Fachwerk im Raume.* The book works up some

Dutch-language edition of the *Proceedings,* and later in the English-language edition. At that time, incidentally, Einstein probably knew no Dutch and little, if any English. In a letter to Besso, dating no earlier than 1913, Einstein writes: "*Ich lerne English* (by Wohlwand), *langsam aber gründlich*".

previous essays which Föppl has used for his degree candidacy at the University of Leipzig in 1886, and, one supposes in connection with his subsequent task helping to design the Markthalle of Leipzig. Yet, the book is by no means intended as a mere practical manual. On the contrary, Föppl objects to the definition by which *Fachwerk* usually is regarded as a structure made of solid straight rods, to carry loads. "For me it is a purely ideal structure" (page 3). And in defense of this point he plunges into an epistemological digression concerning the process and warrant of introducing concepts such as rigid bodies, or ether, "which by no means in every respect coincide with their *«realen Urbildern»*".

And then, in 1892, Föppl was called to the University of Leipzig to teach, of all things, agricultural machinery. As he later confessed cheerfully, he knew very little about this, so he spent the summer touring factories to find out. His versatile intelligence seems to have helped him to absorb enough in a short time to enable him to teach the course soon thereafter, but the subject was not what his mind reached out for. And so, perhaps largely out of boredom, he began to write a treatise in his spare time, entitled *Maxwell's Theory of Electricity*, published in 1894. Indeed, this was the Ur-ancestor of the book on electricity by Abraham-Föppl, later Abraham-Becker, late Becker--Sauter, *etc.*, *etc.*, — although a book very different from all these! And Föppl's book was widely bought, particularly because of the author's ability to put Maxwell's theory clearly to engineers.

Perhaps as a result of the book, Föppl was called in 1894 to the Technical University at Munich — the very city in which Einstein then was living, still a boy of 15 — and there Föppl stayed and wrote volubly, although as far as I could find out, he never taught from his book on Maxwell's theory, the book which, upon its publication, "aroused in the profession astonishment (*Aufsehen*), for at that time the electrodynamic considerations of the great English physicist, Maxwell, had hardly gained any ground" — to cite the introductory essay of the editors of the *Festschrift* in Föppl's honor on his 70th birthday, 25 January 1924.[11]

Before we look at Föppl's *Maxwell*, we can seize up the particular style that characterized his thinking by considering Föppl's immensely

[11] *Beiträge zur Technischen Mechanik und Technischen Physik*, with essays by some of Föppl's students, including Theodor von Karman, Prandtl, H. Thoma, Timoschenko, and Föppl's two sons, Ludwig and Otto (Berlin, 1924). As this list alone shows, his influence was large, although predominantly in technical mechanics. In 1904, Föppl made a gyroscopic experiment to measure the rate of rotation of the earth, a work "which made him familiar with questions of absolute and relative motion". And in 1914, he wrote an essay *Über Absolute und Relative Bewegung*, a field "in which A. Föppl already, before Einstein, occupied himself with the relativity theory, though not with such remarkable success" — according to the editors' indroduction in the *Festschrift*.

successful next work, the *Vorlesungen über technische Mechanik*, published from 1898 on in several volumes. (The *Festschrift*, page vi, notes that to 1924 nearly 100,000 volumes of this work were sold all over the world.) Föppl himself sent seven editions through the press, and others after him continued this work.

The Foreword of Föppl's *Mechanik*, dated June 1898, tells us something rather revealing about his special talent as a teacher and writer. His students, he confesses, sometimes have complained that he "proceeds too slowly rather than too fast", but he placed very special emphasis on laying the foundations carefully. It is almost as if he had a special eye for a reader who might not also have the benefit of formal lectures on the subject, and who might even have bad holes in this formal background.

After the encouraging *Foreword*, the reader comes up against the first two sentences of the text, typical in their mixture of straight forwardness and discursiveness: "Mechanics is a part of physics. Its teaching rests, as does that of all natural sciences, in the last analysis on experience". And with this, he turns to a discussion of the meaning of the term "experience" (*Erfahrung*). By page 4, he confesses "it is now, of course, no longer a question of mechanics, but a philosophical and epistemological question. Its discussion can, however, not be circumvented in an introduction to mechanics, no matter how, on the basis of earlier unfavorable experiences, one may shy away from touching on philosophical questions in the exact sciences". Föppl announces that his exposition of antimetaphysical and self-conscious empiricism is shared by leading scientists generally, and he specifically calls on three by name, in whose spirit he believes he is proceeding: Kirchhoff, Heinrich Hertz (once at Munich University), and — Ernst Mach. Indeed, the volume on dynamics starts with the section entitled "*Relative Motion*", and in the Preface Föppl says again: "One will notice that the (early part of the volume) is strongly influenced by the work of Mach, which made a persistent impression on me".

8. FÖPPL'S MAXWELL

We are beginning to see some evidences of the kind of aproach to physics which would appeal to a young reader with the kind of background, or lack of background, of Einstein in the late 1890's. This impression is much reinforced when we now return to Föppl's *Introduction into Maxwell's Theory of Electricity* (Leipzig 1894). He writes in his *Foreword* that now not only the professional physicist, the teacher, and the student in physics, but also "the scientifically trained electrotechnical engineer [12] is attempting to make himself acquainted with the foundations of this (Maxwell's) theory in which today one may see with great probability the permanent foundation of every physical research in this domain... With this there is a recent demand for an exposition of Maxwell's theory which is as generally understandable as possible, but also scientifically correct".

Maxwell's original work, Föppl reports, is too difficult, and it has many mistakes or incompletenesses which in the meantime have been removed. Boltzmann, he says, has written such a work, but although nothing better of its kind can be done, Föppl sees need for another, different attempt. What Föppl particularly wants to provide is a "clear understanding of the concepts and considerations of this theory in order to give the reader the ability for his own, unsupervised work (*selbststaendigen Arbeiten*) — in short, just the kind of book an interested student would want if deprived of Maxwell's theory in course lectures...".

One idiosyncrasy of the book that interests us is explained in the following manner:

"In this book I have left out citation of sources as a matter of principle... I wanted to write not a Handbook but a *Lehrbuch* which should as far as possible be cast in one piece. Therefore, I avoided as far as at all possible during the writing going back to publications which I had read earlier in order that I may not be directly influenced by them. I wanted to be led by the developments and results of other authors only insofar as these matters had firmly penetrated into my memory and had become an intimate part of my own views. In this matter I hoped to attain a more unified and coherent exposition of the whole system than would have been possible by going another way".

As a consequence, there is a remarkable paucity of references to actual experimental situations (of course, none is made to the Michelson or other ether-drift experiments; but almost all references to any others are also missing).

In Föppl's book we find six main sections; the first is on vector calculus, the second on fundamental electricity (Gauss's Theorem, Coulomb's law, magnetism, induction, *etc.*), the third and fourth are the usual extensions (ponderomotive forces, vector potential, energy relations in the electromagnetic field between stationary conductors). So far, it is all done competently and patiently, but as if it were merely prelude to something else.

Then we come to the fifth main section, which turns out to be of particular interest to us (pages 307—356). It is entitled *The Electro-*

[12] It is, incidentally, worth noting that Einstein came to the Zürich ETH initially planning to study engineering, and that both Einstein's father and closest uncle were in electrical engineering and manufacturing.

dynamics of Moving Conductors (*Die Elektrodynamik bewegter Leiter*), and the first chapter in it is entitled "Electromotive Force Induced by Movement". The first paragraph in this first chapter is "Relative and Absolute Motion in Space", and starts in an unusual way:

"The discussion of kinematics, namely of general theory of motion, usually rest on the axiom that in the relationship of bodies to one another only relative motion is of importance. There can be no question of an absolute motion in space since there is absent any means to find such a motion if there is no reference object at hand from which the motion can be observed and measured... According to both Maxwell's theory and the theory of optics, empty space in actuality does not exist at all. Even the so-called vacuum is filled with a medium, the ether ... However, were we to accept the notion of completely empty space, it would either be not at all subject to possible experience, or alternatively, we would first have to make a deep-going revision of that conception of space which has been impressed upon human thinking in its long period of development. The decision of this question forms perhaps the most important problem of science of our time".

Föppl continues a few lines later in this way:

"When in the following we make use of the laws of kinematics for relative motion, we must proceed with caution. We must not consider it as a priori settled that is, for example, all the same whether a magnet (moves) in the vicinity of a resting electric circuit or whether it is the latter that moves while the magnet is at rest".

This, we recall, is precisely the way Einstein's paper starts — and Föppl adds a rather familiar kind of *Gedanken*-experiment:

"To decide this question, we can consider a third case". He proposes to think of both magnet and conductor moving together, with no relative motion between them. Experience shows, he says, that in this case the "absolute motion" in itself causes no electric or magnetic force in either body. This thought experiment is then quickly developed to show that in the previous two cases what counts is only relative motion.

Later, Föppl goes on to discuss the interaction of moving magnets and resting conductors (pp. 314—320), and resting magnets and moving conductors (pp. 321—324). The rest of this section, too, may be directed first of all to engineers (unipolar induction, emf for a moving conductor, magnetomotive force, motion of a wire loop in a magnetic field, *etc.*). There is a rather brief last (sixth) part, a summary of the other aspects of Maxwell's work, including electromagnetic waves — again, with hardly a reference to the actual ether experiments. But our attention is most attracted by the fifth section of Föppl's book; there, and in portions of the rest of the book, is the kind of thinking which would indeed have appealed to Einstein, and which in fact is far closer to the structure and style of argument of Einstein's 1905 paper than the work of any of the others whom we have named — far more so than the books of Helmholtz, Boltzmann, Hertz, Runge for example.

9. OTHER REFERENCES TO FÖPPL

But before the parallelism with Föppl carries the day, we must ask for more evidence. After all, earlier we dismissed the suggestion that Einstein built on Poincaré's and Lorentz's work of 1904, even though there are certain parallels.

We therefore must ask, why did not anyone else who knew Einstein intimately vouch in print for the fact that Einstein read Föppl's book. Thus I asked my former teacher and colleague, Einstein's biographer, Philipp Frank, why he had no mention of Föppl in his book *Einstein: His Life and Times* (Alfred Knopf, New York, 1947). Professor Frank replied he thought he had mentioned Föppl, and I showed him my copy of the biography in which it was plain that he had not. This was a considerable surprise to Professor Frank, but after some thought he referred me to the German edition of his book (Paul List Verlag, 1949, p. 38). In the *Foreword,* Professor Frank explained that this, the German edition, is the first complete edition of his manuscript as written in 1939—1941. And there, on page 38, Professor Frank writes that during his years as a student at the Polytechnicum in Zürich, "Einstein threw himself into the work of these classics of theoretical physics (of the late nineteenth century), the lectures of Helmholtz, Kirchhoff, Boltzmann, the electricity theory of J. C. Maxwell and H. Hertz, and their exposition in the textbook of Abraham Föppl. Einstein buried himself with a certain fanaticism day and night in these books from which he learned how one builds up the mathematical framework and then with its help constructs the edifice of physics" [13].

And there is one other guide that leads us to Föppl. There are dozens of biographies of Einstein — most of them written at second or third hand. But here the Archives at Princeton delivered again a surprise. I knew that a certain Anton Reiser had published a biography in English in 1930, when Einstein was still in Berlin. Despite a pleasant foreword by Einstein, at first glance it can hardly be considered reliable: for quite apart from the suspicious circumstance that no German edition was ever brought out, there are also no credentials for the author of the book. No other publications by Reiser exist any-

[13] Since Max Abraham's version of August Föppl's book did not get done until 1904, it would be Föppl's original work which must be meant here; but this slip does remind us that a substantial fraction of several successive generations of physicists were brought upon — and then taught from — Abraham-Föppl and later Abraham-Becker.

where, and a search in the usual sources leads one to suspect that he simply does not exist. Now the material in the Archives shows that the name Anton Reiser was a pseudonym for Rudolph Kayser; and Rudolph Kayser was Einstein's own son-in-law who had proceeded with the biography with Einstein's acquiescence.

We return therefore to Reiser's biography with new respect — and sure enough, there we find Föppl mentioned again as one of the authors Einstein read in his early days. So our missing signal is recovered from the noise level of the "*et cetera*".

In balance we may say the role of Föppl was that he, with Helmholtz on the one hand, and Mach on the other, reinforced the unique aspects that made Einstein's 1905 paper so important and, for his contemporaries, so difficult. As the various contemporaneous treatments of elestrodynamics showed, there was in principle a great diversity of possible roads open to Einstein. What Föppl was capable of providing in helping to shape Einstein's thought processes prior to the fashioning of the relativity theory was, first of all, encouragement to go ahead in a manner so very different from that taught to him at school and presented in all the most respectable books. It helps us to understand better what to this day remains as the most startling part of Einstein's relativity paper: a mixture that contains a good share of youthful philosophizing as a part of doing physics; the recognition that the fundamental problem to be cracked is how to achieve a new point of view on the conceptions of time and space; the attention to *Gedanken*-experiments, and conversely, a quite low interest in the actual detailed experiments which so many of our texts make appear to be the point of departure of relativity theory. And there is also, I believe, some poignancy in the discovery how Föppl may have reached across to Einstein — the book of an "outsider" who never had students in this subject to whom he could teach its contents in lectures, and the student who, also already being regarded by his teachers as an "outsider", was looking to this book for the material and stimulation he could not get in their lectures.

10. PRIORITY OF FUNDAMENTAL THEORY

The Archives at Princeton are full of evidence of the gradual hardening of Einstein against the epistemological priority of experiment, not to speak of sensory experience. Again and again he put the consistency of a simple and convincing theory or a thematic conception higher in importance than the latest news from the laboratory — and again and again, he turned out to be right.[14] A theoretical and episte-

[14] The same point of view is found, of course, also in Einstein's published writings. To give here only one early example, in his 1907 "Jahrbuch" ar-

mological wholeness of the work is the chief criterion in Einstein's mind. (The case is particularly richly documentable with respect to the aether-drift experiments). Again and again one sees his confidence that success of theory is not coupled either at the beginning or at the end to sheer experimental facts alone. Thus he writes to Besso in March 1914, before the first eclipse expedition to test the conclusions of General Relativity: "Now I am fully satisfied, and I do not doubt any more the correctness of the whole system, may the observation of the eclipse succeed or not. The sense of the thing (*Vernunft der Sache*) is too evident". And at a later time Einstein commented on the fact that there remains up to ten percent discrepancy between the measurement of the deviation of light by the sun's field and the calculated effect: "For the expert this thing is not particularly important because the main significance of the theory does not lie in the verification through little effect, but rather in the great simplification of the theoretical basis of physics as a whole" (Seeling, p. 195). Or again Einstein reports in his notes on the origins of the general theory (*Ideas and Opinions*, p. 287) that he "was in the highest degree amazed at its existence (of the law of the equality of inertial and gravitational mass)", but that he "had no serious doubts about its strict validity, even without knowing the results of the admirable experiment of Eötvös...".

Again, writing to Besso in 1921 on the verifications of general relativity theory to this point: "I had not a moment's doubt that it must be this way". And on Christmas Day 1925, he received the following cable from America: "President Miller, American Physical Society, announces discovery of ether drift. Says «my work annuls second postulate Einstein theory.» Please cable collect 200-word opinion

ticle (*Radioactivität und Electrizität*, vol. 4 Issue No 4), Einstein discusses the experiments by W. Kaufmann. On page 436 ff. he cites Kaufmann's paper in the *Annalen der Physik*, 1906, 19, "Concerning the Construction of the Electron", reproduces some of his figures, and says that W. Kaufmann, working "with admirable care", had found the relation of the radii of curvature of the path of fast electrons in pure electric and pure magnetic fields. There is a systematic small difference from Einstein's relativity theory for the results as Kaufmann had pointed out. Einstein says that Kaufmann's calculations are free of error. But "whether there is an unsuspected systematic error or whether the foundations of relativity theory do not correspond with the facts one will be able to decide with certainty only if a great variety of observational material is at hand". The series of electron motion given by Abraham and in Bucherer's 1904 book, Einstein says, do give predictions considerably c l o s e r to the curve. But then he adds: "However, in my opinion both theories have rather a small probability because their fundamental assumptions concerning the mass of moving electrons are not explainable in terms of theoretical systems which embrace a greater complex of phenomena" (p. 439).

As is characteristically the case in an Einsteinian objection, the a d h o c character of a theory is found objectionable, even though the "experimental facts" at that time very clearly seem to favor the theory of Einstein's opponents.

press rates. David Dietz, NEA Service, Inc.". There apparently was no answer, but on the same day Einstein wrote to Besso, "I think that the Miller experiments rest on an error in temperature. I have not taken them seriously for a minute". Again, on the 14th of March 1926, in a letter to Piccard, Einstein says apropos the Miller experiments, "I believe that in the case of Miller the whole spook is caused by temperature influences (air)".[15]

In the end, the epistemological thread running through Einstein's whole work, from the beginning, a thread that connects him with certain aspects of the German school of thought, as particularly exemplified in Föppl's text, is just this particular balance he struck between the demands of theory and of the world of detailed experience. As Einstein put it in a hitherto unpublished addendum to his *Autobiographical Notes*, "Everything conceptualizable is constructive and not derivable in a logical manner from immediate experience. Therefore we are in principle completely free in the choice of those fundamental conceptions upon which we found our rendition of the world. Everything depends only on this: to what extent our construction is suitable for bringing order into the apparent chaos of the world of experience". As the letters and manuscripts show even better than Einstein's published works, he constantly saw his task as being, in large part, the subjugation of the world of mere, immediate experience by means of fundamental thought. We, who have come here to celebrate his achievements, are indeed the beneficiaries of his lonely and grand message.

And while we have looked at some of the documents which Einstein surely did not initially mean to be used for historic research, we can nevertheless be sure that Einstein would have understood and not objected to this purpose. For as he wrote to Besso (30 November 1949): "When I write you something, you can show it to anyone you like. I have long been above making of secrets". And in another unpublished manuscript (No. 17, undated, not before 1931): "Science as an

[15] Despite many entreaties from scientists and the press, Einstein only gave very little public evidence of interest in the Miller experiments. There is one letter from Miller to Einstein (of 20 May 1926) showing that Einstein had written to Miller and that they had met before, also that Einstein had told Miller "a difference of a tenth degree in the temperature of the air along the light path in the arms of the interferometer would produce a displacement of the fringes of the amount observed". Miller was rendering Einstein's query, apparently, but adds that "very elaborate precautions have been taken to eliminate such an effect of temperature". It is ironic that the splendid and elaborate analysis of R. S. Shankland and his colleagues in 1955 traced Miller's observed effects precisely to this source. Indeed, in a letter of 24 February 1963 to Shankland, Miss Dukas reports that the famous remark *Raffiniert ist der Herrgott, aber boshaft ist Er nicht* was made by Einstein at a reception in 1921 after a lecture in Princeton when Einstein was asked his views about the Miller experiments. The remark illustrates Einstein's confidence concerning the kinds of experiences Nature would "allow".

existing, finished (corpus of knowledge) is the most objective, most unpersonal (thing) human beings know, (but) science as something coming into being, as aim, is just as subjective and psychologically conditioned as any other of man's effort...". And that aspect, he went on to say, one should certainly "permit oneself also". Happily, he and his friends and colleagues have done just that. They have left us the record of "science coming into being" and thereby they have enriched our understanding for all time.

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